Do State Tax Breaks for Land Conservation Work?

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Abstract

Private decisions about land conservation are crucial for preservation of endangered species as 80% of their habitat are on private land. I study the efficacy of state tax breaks to promote private land conservation. I use the Protected Area Dataset of United States and construct a county-year level panel of the flow of undeveloped land protected per year. I use fixed effects panel estimations combined with optimal full matching to improve balance on observable covariates between treated and control counties. Results show that, on average, counties in a state with a tax break more than double the yearly flow of conservation after the incentive is in place. These findings suggest that state tax breaks are an effective incentive to promote land conservation.

Keywords: Land Conservation, Difference-in-Difference, Matching, Tax Incentives

JEL Classifications: Q24, Q58, H23

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1 Introduction

In recent years, many states have tried to increase incentives for private land conservation. This makes sense considering that more than half of the species listed under the Endangered Species Act have at least 80% of their habitat on private property (USFWS, 1997) (Parkhurst, 2002). Some states have implemented income tax breaks for land conservation on the premises that this incentive can influence people's behavior. However, the loss of tax revenue presents a trade-off of these conservation policies. The question that seems to follow is: do these tax incentives affect the private land use decision and translate into more acres conserved?

Two aspects of the land conservation scenario are useful for this study. First, conservation tax incentives are becoming more popular, yet only sixteen states have adopted these extra incentives. This presents an opportunity to provide an estimation of the effect of these policies by using impact evaluation techniques. Second, two categories of conservation are possible: fee simple and easement. This distinction provides more information for the analysis. Given that some tax breaks only apply when land is conserved through an easement, it is expected that an effective tax break will translate in more acres conserved in that category. However, if the total amount of acres conserved remains the same, the tax break just decreased conservation through fee simple in favor of conservation through easement.

To show how state income tax breaks affect the amount of acres donated for conservation in different states, I first estimate a panel fixed effect model. I use fixed effects by county and state to clean idiosyncratic county and state characteristics that remain unchanged through time, and year fixed effects to account for specific shocks common to all states. I assume that treatment and control groups are comparable except for unobservable characteristics that are invariant through time. I find that, on eastern states, a tax incentive increases the flow of conservation per year per county. Using similar counties as a control shows what conservation would have been like in the absence of the tax incentive. The key aspect is for these two groups to be comparable in some characteristics that affect conservation, such as geography, climate, urban development. I concentrate the analysis on a county-year level balanced panel between 1990 and 2010, for the eastern region of US.

A second approach improves the estimation by using observable characteristics to reassure treatment and control groups are comparable. Characteristics like land value, proportion of land cover in forest, and population density can determine the proportion of land available for conservation. I use optimal full matching (Rosenbaum, 1991) to create treatment and control groups that are balanced on certain covariates of interest. This type of matching generates matched sets by optimally minimizing the distance between covariates. The number of treated and control observations in each matched set is determined by the full matching algorithm. Panel estimations after matching also show that states with tax breaks have increased the amount of acres protected.

I am able to study the effect of these policies due to a new dataset (PADUS Version 1.3). Recently, there have been advances in collecting data with information regarding date of conservation for parcels. Knowing if a certain parcel was conserved before or after the implementation of the tax break policy is key to measure the impact of the incentive. The process of collecting information about date of conservation is still in progress, but it is a start point to open the discussion on how to influence private land conservation.

Some studies analyze policy effects on land conservation, such as, Anderson and King (2004), Anderson (2005), Polyakov and Zhang (2008), Parker and Thurman (2011), Sundberg (2014), Suter et al. (2014). In particular, Parker and Thurman working paper (2015) concentrates on the effect of state tax incentives. They develop an income tax calculator to quantify the after tax price of donating an easement and use a state-level panel data to analyze how acres donated through easements grow as a result. However, there is no study that analyzes the global effect of state tax breaks on the amount of acres protected.

I use the percentage of acres protected per year and county to measure land conservation. Counties' boundaries, although subject to some change, are more permanent in time than parcel boundaries. This makes it easier to analyze the change in acres protected in a particular area, before and after a tax break. Another aspect to consider is that counties' size greatly differ between states. To account for this issue I use percentage of acres protected instead of total acreage under conservation per county. I consider the flow of conservation, which represents the increment of acres that are protected each year, that add to the total amount of acres permanently protected.

Finally, I generate placebo laws to test the results in two different ways. First, I test how the different estimations perform under randomly generated placebo treatment. I find that fixed effect panel estimations using either raw data or a matched sample show no effect of treatment. Second, I run a Monte Carlo simulation to check robustness of standard errors clustering. I find that clustering standard errors by state reduces the rejection rate of the null hypothesis of no effect to what one expects, at a given significance level. As an extra alternative to standard error correction, I collapse data to two effective periods, before and after a tax break, and find that tax breaks increase the amount of acres protected in counties with the incentive.

2 A Brief Review on Conservation Tax Incentives

Land conservation have exponentially grown in the past years making government incentives much more frequent and worth of analysis. Federal incentives have existed for almost fifty years and are now reaching to the state level. The fact that only some of the states start implementing tax breaks for conservation in recent years allows the use of impact evaluation techniques to study the effect of these policies.

Individuals have different ways of setting land aside for conservation: fee simple or easement. Under fee simple, the landowner sells or donates the land to a Land Trust who then owns all the rights on that particular parcel. A conservation easements is a legal agreement between a landlord and a land trust or government agency to protect the conservation value of the land by limiting its use permanently. This legal restriction generally allows the normal use of the land, in agriculture for example, and the construction of new structure related to that use, but forbids any kind of development. The landlord can still sell the land or pass it on to heirs, but the new owners will still be bounded by the easement.

The distinction between fee simple and easement is necessary because some incentives only allow for tax credit when the conservation is through an easement. Federal tax breaks apply only to easements, whereas some state tax incentives apply to both, easement and fee simple. When the tax deduction only applies to parcels with a conservation easement, considering just the increment in the amount of acres protected through easement can be misleading. Analyzing both categories show if total conservation is actually increasing or just changing from one to another.

Two federal incentives promote land conservation under easement. An income tax incentive allows landlords to deduct the market fair price of their land, up to 50% of their adjusted gross income (100% for farmers and ranchers), for as long as 16 years. This incentive started in 1964 when the government allowed as a charitable deduction the value of certain wooded area with a scenic view near to a federal highway. It was in 1969 when the Tax Reform Act ruled about those types of charitable donation deductions related to conservation (Internal Revenue Code, Section 170 (f)). The law has suffered several adjustments since then until the last reform in 2006, where it was greatly expanded reaching the benefits known today. Estate taxes arise as another way to promote conservation. In 1997, a law established an estate tax exclusion of up to 40% of the value of land where an easement for conservation have been placed, up to $ 500,000 (Internal Revenue Code, Section 2031(c)).

The increasing interest in land conservation has encouraged many states to also offer some incentives. State tax breaks arise as one of these strategies. Even though each state has some specific features, the incentive usually consists of a state income tax deduction of part of the donated land value. Sixteen states have adopted these extra incentives between 1983 and 2011. The list of states includes: Arkansas, California, Colorado, Connecticut, Delaware, Florida, Georgia, Iowa, Maryland, Massachusetts, Mississippi, New Mexico, New York, North Carolina[1], South Carolina, and Virginia. I present date of implementation and highlights of each state tax incentive on Table 1.

3 Data

I concentrate the study on the eastern region of continental United States. I combine several datasets and construct a county-year panel with the amount of acres protected between 1990 and 2010. I include land cover characteristics, agriculture and population census variables.

The basic conservation dataset is the Protected Area Database of the United States (PADUS), Version 1.3, developed by US Geological Survey Gap Analysis Program. This dataset includes maritime and terrestrial protected areas in continental US, Alaska, Hawaii, and Puerto Rico. The key aspect of this dataset is that it includes the date each parcel was protected. This specific feature allows me to reshape the data into a panel to study conservation trends and policy effects.

PAD-US is a parcel/area level dataset with information on 30 attributes for 734,515 protected areas. Attributes can be grouped in two sets. The first set of attributes provides identification information for each area. It includes name of the organization that owns and manages the land, name of the area, source of information and other identification features. The second set of attributes refers to some characteristics of the protected area. *Category* refers to the way the land is conveyed for conservation. Areas can be owned by Fee Simple or an Easement can be created to restrict development and enforce land conservation. *GIS_Acres* represents the size of the protected area, in acres, obtained from the geometry tool in arcGIS software. Other attributes are the level of public access

[1] North Carolina have eliminated the tax credit program, effective January 2014

permitted in the protected area: Open, Restricted or Closed (*Access*), and the level of intervention permitted for biodiversity conservation purposes (*GAP_Sts*). This Level of allowed intervention is coded from 1 to 4, from minimal intervention to no restrictions. The last attribute in this set is *Date_Est*, which records the date the area was protected. This is a new feature incorporated in the last version of PADUS, and it is what makes the analysis on this paper possible.

PADUS dataset has many advantages worth noticing, but at least one important weakness for the purpose of this study. On the advantages side, it is the first comprehensive collection of protected areas in US. It includes Fee Simple and Easements for lands held by national, state, and some local governments and non-profit conservation organizations. The completeness of the dataset allows to study the effect of conservation incentives on the total amount of conservation, not just Fee Simple or Easements. Other protected areas datasets include only one of these categories, concentrate only on some types of ownership, or limit the analysis to one state. The main weakness of PADUS dataset is the coverage of the date of conservation attribute. As this is a new feature, its completeness is still in progress. Datasets' coordinators concentrate their effort on first gathering date of establishment for areas with minimal and moderate management intervention, classified as GAP Status codes 1 and 2. They plan to extend this coverage in future versions.

I restrict the analysis to continental US areas where date of conservation has been recorded. Problems in data collection result in overlaps of some areas that need to be addressed manually and exceed the scope of this study. After combining Fee Simple and Easement layers, and discarding areas with no date of conservation, I have information on $171,017$ protected areas. I do not include maritime protected areas.

The effect of a tax break is easier to interpret if the unit of observation is relatively permanent. Boundaries of parcels and protected areas usually change over time, and tracking the same protected parcel over the years is a hard task. A better approach considers a more permanent unit of observation where conservation in a particular area can be compared at different points in time. County boundaries, although still subject to some change, tend to be more stable. I overlay the US county shapefile on the combined fee and easement PADUS layers. Using arcGIS intersect tool, I assign each protected area to a county and calculate protected acreage (geometry tool) per parcel/area. Finally, I reshape the data to get a county-year total of acres protected. I get a balanced panel between 1990 and 2010.

Several land features and socio-economic characteristics can affect conservation. Population density and urbanization can determine how much land is available to be protected. Areas near highly developed regions would probably have a high development value, and restricting its development with a conservation easement will be less appealing. Farm areas with higher agriculture value are more attractive for conservation, specially since easements generally allow this type of land use. Primary type of vegetation in the area can also be an important factor to consider. I combine land cover data, population census and agriculture census data to create a set of covariates that are useful as controls variables .

Land Cover data is in raster format with a spatial resolution of 30 meters, that shows a 21-class scheme grouped in eight categories. I use 1992 land cover data, developed by Multi-Resolution Land characteristics Consortium (partnership of US Geological Survey). The eight mentioned categories refer to distinctive types of coverage: water, development, barren, forest, shrubland, non-natural woody, herbaceous upland, planted/cultivated, and wetlands. I extract the information from the raster and create a new dataset where the unit of observation is a county. Each county has information on the proportion of acres in each of the 21 types of coverage.

I also combine county level data from population census, agriculture census and presidential election results. Population census data includes yearly information on unemployment rate (1990-2010) and poverty rate (1997-2009), and decennial information on total population (1980-2010), urban and rural population (1980-2000), population per square mile (1980-2010), total housing units (1980-2010) and median household income measured at the end of 1979,1989,1999, and 2009. Next, I use the last four agriculture census: 1997, 2002, 2007, and 2012. They include information regarding average farm size, total number of farms, total amount of acres on farms per county, and average value of land and buildings per farm and per acre. Finally, I also include percentage of democratic and republican votes cast for president for every election between 1980 and 2008 to account for political views on conservation that can influence conservation trends.

4 Methodology

I use optimal matching and a panel fixed effect model to measure the effect of state tax break policies. Optimal matching helps in making treatment and control groups comparables on observables. Panel estimation allows to study tax breaks that took place at different times for the period under analysis.

In the past years, empirical research on the causal effects of certain programs or policies have grown considerably. Basically, these studies are interested in measuring the change on some outcome of interest, on subjects that have been exposed to the program or policy. The well known problem is that the same subject can only have one outcome: the subject is either exposed to the program or not. This poses some questions on how to find a reliable control group that mimics the one treated so that the difference in outcomes between both groups can be the consequence of the program.

I am interested in measuring the effect of a state tax break for conservation on the flow of acres conserved per year and county. Difference-in-difference comes as the most commonly used method in this type of studies. The key assumption is that what differentiate treatment and control groups are time-invariant characteristics. In that sense, by comparing acres conserved in states with and without a tax break, before and after the tax break was implemented, one is able to isolate the actual effect of the policy. In other words, this method is using a double difference: first, it calculates the difference within each group, before and after the tax break, and second, takes the difference between those differences. This removes permanent differences between both groups as well as time trend differences not related to treatment. Formally:

$$\delta_{DID} = (E[Y_i|S_i = 1, T_i = 1] - E[Y_i|S_i = 1, T_i = 0]) \\ - (E[Y_i|S_i = 0, T_i = 1] - E[Y_i|S_i = 0, T_i = 0]),$$

where Y_i are acres conserved in county i, $S_i = 1$ if the county is in a state with a tax break and 0 otherwise, and T_i refers to the time period: before or after the tax break.

I extend this model to a panel setting and estimate a two-way fixed effect panel model. Panel estimation allows me to control for unobservable characteristics that are invariant through time. I include fixed effects per county, state, and year. County and state fixed effects control for idiosyncratic characteristics that do not change through time. This helps with the concern that tax break incentive may occur in states that are different from the ones without a tax break. Year fixed effects control for specific shocks that affect all counties and states. Finally, I control for some observable characteristics that may affect land conservation, such as population density, proportion of forest, median income.

Formally:

$$Y_{it} = \alpha + \kappa_c + \gamma_s + \lambda_t + \delta D_{it} + \epsilon_{it}, \tag{1}$$

where Y_{it} is acres conserved in county i, at time t, κ, γ_s and λ_t are county, state and year fixed effects, and D_{it} takes the value 1 to indicate that the county i is in a state that has a tax break, after the tax break is in place.

Difference in Difference is a good approach when treatment and control groups are somehow similar on observable characteristics. To improve comparability, I use matching methods to create two groups that are similar on the observables of interest. Matching achieves this by comparing and matching treated and control observations on specific covariates before any treatment takes place. This step eliminates the differences on observables between groups and produce unbiased estimators of the effect of the policy on the treated group.

Propensity score matching is probably the most common approach for matching on observables (Rosenbaum and Rubin, 1983). This method uses the covariates to estimate the probability of treatment for each observation (logit estimation), and then matches observations with similar probabilities. Matching methods assume unconfoundedness and common support of covariates. Unconfoundedness states that given observable characteristics, potential outcomes are independent of treatment assignment. Common support states that treatment and control observations have similar covariates distributions that allow to find a match.

A problem that arise when matching directly on covariates is how to handle many of them and produce a multivariate matching. Propensity score addresses this issue by reducing many covariates to one number that shows the probability of receiving treatment. In this particular case, one can think of propensity score as a way to relate county covariates to a certain characteristics that those counties have because they are in a particular state. In other words, it measures the distance between counties by projecting them onto the state they are in. Another approach is using Mahalanobis distance matrices. These distance matrices follow the same idea of reducing many covariates to one number, but without considering relationship to treatment or assuming any functional form. Basically, a Mahalanobis distance matrix measures similarities between covariate by calculating the distance of each covariate in units of standard deviations. Formally (Rosenbaum 2010):

$$(X_k - X_l)^T \hat{\Sigma}^{-1} (X_k - X_l)$$

where X_k and X_l are covariate matrices for treatment and control, and $\hat{\Sigma}$ is the covariance matrix of X.

As Rosenbaum (2010) pointed out, Mahalanobis distance matrix works best when data is normally distributed. Because it used standard deviation as a measure of distance, this is not the best approach when the data has outliers or a long-tailed distribution. In those cases, the standard deviation will be inflated given less weight to covariates with those distributions. This is an actual problem in my dataset, where counties may have huge differences in covariates. As an example, a county in Illinois may greatly differ on the amount of forest cover with respect to a county in Maryland. This will give that particular covariate a smaller weight when calculating the distance matrix, and a mismatch on that covariate will be less penalized compared to a mismatch on other covariates.

A rank-based Mahalanobis distance is a plausible solution to this problem. The key aspect of this distance matrix is that it uses the ranking of the values of each covariate and an adjusted covariance matrix [2] of these ranks to calculate the Mahalanobis distance matrix. This method solves the problem of extreme outliers and long-tailed distributions. I use Propensity Score and Rank-based Mahalanobis to calculate distance matrices that are used for matching.

The second decision to make is the type of matching that better adapts to the problem under analysis. I use optimal full matching which allows the matching of one treated unit to multiple controls, as well as one control to multiply treated units. Full matching produces matching sets that are as close as those produced by pair matching or matching with variable number of controls, and often closer matches come out. It is the optimal matching method that minimizes the weighted average distance (Rosenbaum, 1991). Applications of this method can be found in Hansen (2004), Hansen and Klopfer (2006), Stuart and Green (2008), and Heller et al (2009), to name a few.

Full matching minimizes the distance between all pairs within each matched sets and across all data. The number of treated and control units in each matched set will depend on covariates' similarities, number of treated and control units, and the full matching

[2] The adjusted covariance matrix consists of pre and postmultiplying the rank covariance matrix by a diagonal matrix where the diagonal elements are the ratio between the standard deviation of tied ranks and the standard deviation of untied ranks (Rosenbaum, 2010).

algorithm. [3] Following Rosenbaum (1991) and Hansen (2004), for each pair $\{c, t\}$, with $c \in C$ and $t \in T$, let $d_{ij} \in [0, \infty]$ be the corresponding distance for the ij observation in the distance matrix, the full matching minimizes:

$$\sum_{i \in T, S(i)>0} \sum_{j \in C, S(i)=S(j)} d_{ij}$$

where $S(i)$ is a mapping that defines the matched sets. An algorithm that explains how optimal full matching works can be found in Hansen (2004) Appendix.

After matching, I estimate the treatment effect of state tax break policies by estimating equation 1 plus a fixed effect for matched sets. Formally:

$$Y_{it} = \alpha + \kappa_i + \gamma_s + \lambda_t + \delta D_{it} + \Delta_{S(i)} + \beta X_{it} + \epsilon_{it} \tag{2}$$

where $\Delta_{S(i)}$ is a matched-set fixed effect and, as before, δ measures the average treatment effect. The treatment effect δ is the result of taking an average of matched-set mean differences, weighted by the number of observations and the ratio between treated and controls on each matched set.

Standard errors that come as a result of estimating equations 1 and 2 may be misleading. The error terms ϵ_{it} in a county-year panel may be serially correlated, affecting the efficiency of the estimator. Bertrand et al. (2004) show how this problem affect difference-in-difference estimations and suggest different ways to correct it. I adopt an arbitrary covariance matrix and cluster standard errors at the state level (Liang and Zeger, 1986, Arellano 1987, Bertrand et al., 2004). As a robustness check, I used another approach and show the results after collapsing the data to two effective periods: before and after tax breaks (Appendix).

[3] The minimum or maximum number of controls used in a matched set can also be specify, to avoid dropping observations or matching with many controls.

5 Results

I concentrate my analysis on the East Region of United States. I present results from a fixed effect panel estimation using both raw data and a matched sample. All approaches show a positive and significant effect of the implementation of a tax break policy. Matching estimation also improves balance between treated and control groups. Finally, I include some other specifications to account for anticipatory effects and also decompose the effect of tax breaks in future years.

I focus on the East Region of the continental US because most of the states with tax breaks are located in this region. I work with a widely used comprehensive definition of eastern states that also includes the first tear of states west to the Mississippi river (Figure 1). Twelve states implemented a tax break between 1990 and 2010: Arkansas (2009), Connecticut (1999), Delaware (2000), Florida (2009), Georgia (2006), Iowa (2008), Maryland (2001), Mississippi (2003), New York (2007), North Carolina (1983), South Carolina (2001), and Virginia (2000). Massachusetts put into effect a tax break in 2011 and it is treated as a control state in this study. North Carolina had a tax break between 1983 and 2014, so it is always considered a treated state. I use a balanced panel from 1990 to 2010, with a county-year unit of observation.

I present summary statistics per state for the outcome variable and some covariates (Table 2). I only include: *percentage of undeveloped acres protected, agriculture value per acre, population density* and *proportion of forest*, to get a rough idea of some key values. The average percentage of undeveloped acres protected double for some states after treatment, but decrease for others. All other variables greatly differ between particular states with a tax break and particular states without it. This is not necessarily a problem. The crucial point is for treatment and control groups to be comparable as a whole.

5.1 Matching and Panel Estimation

Difference-in-Difference based the estimation on the assumption that both groups, treatment and control, are comparable except for unobservable characteristics that are invariant through time. I first present results for a fixed effect panel estimation. Then, I adjust comparability between groups using full matching and re-estimate the panel model. I also include some specifications to consider lagged and anticipatory effects of tax breaks.

Land Conservation for treated and control groups show similar trends before tax breaks start (Figure 2). Both trends show the same slope before 1999 and even during the first few years after that. By 2001, five states have already implemented tax breaks. A bigger effect starts showing up around 2002 and the difference between both groups grows around 2006. The last five tax breaks take place between 2006 and 2009 .

Similarities in conservation trends show that treatment and control groups are comparable. One possible concern is endogeneity of the tax break policy. States may have different reasons that make them more inclined to pass a tax incentive bill. A state with higher conservation rates may pressure the local and state government to expand fiscal incentives for land conservation. The opposite scenario is also possible. A state with very low conservation rates, can try to persuade legislators to pass bill incentives to increase conservation in the area. What helped the state level decision of adopting a tax break incentive may depend on unobservable characteristics. Conservation trends that move at a similar pace reinforce the assumption that lack or excess of conservation in a particular state is not driving the implementation of a tax break policy.

Matching on observable characteristics can improve comparability between treated and control groups. Even though comparability a priori is good, there is some room for improvement. I want to make sure that the outcome of the control group mimics the potential outcome of the treated group, would it have not been treated.

The decision of which specific variable to use for matching can considerable affect balance between groups. There is a trade-off between balance and final number of matched sets that will depend on the variables used for matching and the distance matrix. Depending on the treated and control samples, matching on some covariates can significantly reduce the number of matched sets. Variables that differ significantly between groups are difficult to match. However, if those variables are important for conservation, they cannot be left out.

I use a set of covariates to create two distance matrices using propensity score and rank-based mahalanobis distance, respectively. I consider all variables at the same point in time, the year 1998, before any tax break started. The set of covariates are: average rate of undeveloped land protected per year between 1990 and 1998, cumulative percentage of undeveloped land protected, percentage of farms, agriculture value per acre, unemployment rate, median household income, population density, percentage of votes cast for democrats in presidential election, and proportion of land covered in forest[4]. Forest areas are generally the focus for conservation, providing clean air and wild habitat for different species.

The chosen covariates for matching influence conservation in different ways. One would expect that counties with more acres in forest will also have more acres protected. Similarly, landowners can place an easement on their farms and conserve that area, restricting development but with the possibility of keeping the agriculture use of the area. The opposite will probably be true for highly populated areas. Including the cumulative percentage of undeveloped land protected is a good baseline and helps matching counties that were alike in the amount of protection before treatment. However, it also assumes that counties with similar percentage of land protected are also similar in their future conservation trends. Matching on the average rate of undeveloped land protected can minimize this potential problem. I use proportion (or percentage) of areas (farms, forest, acres protected) to account for county size.

I use full matching using a propensity score distance matrix with a caliper of 0.1

[4]Proportion of forest correspond to the year 1992

standard deviation. From 421 treated and 775 control counties, this matching construct 165 matched pairs. It drops 1 treated and 9 control counties. The dropped treated county is Pickens (South Carolina), home of Table Rock State Park. Forest cover approximately 77% of the county area. Dropped control counties are from Illinois (3), Kentucky (2), Louisiana (1), Minnesota (1), New Hampshire (1), and Vermont (1). They show an extremely low or extremely high rate of conservation between 1990 and 1998. They are also outliers in terms of percentage of farms and proportion of forest.

Overall balance after matching gets significantly better. In particular, balance improves on percentage of farms, proportion of land covered in forest and population density. (Figure 3). The first two variables directly affect availability of land for conservation, and population density is a good measure of how much developed there is in a county. The standardized differences that remain between means in treatment and control groups are not significant (Hansen and Bowers, 2008). After matching, the new control group is a better representation of the treatment group (Table 3).

Matching using rank-based mahalanobis distance matrix and a caliper of 3 standard deviations also improves balance with respect to the original sample. Overall balance improves but it still shows some imbalance on certain covariates (Figure 4). Specifically, percentage of farms and proportion of forest show significant standardized differences in means.

This matching constructs 156 pairs, similar to the 165 pairs for propensity score, but drops many more observations. It does not find a match for 133 treated and 327 control counties. Summary statistics show that dropped treated counties have higher population density and lower agriculture value per acre. Control counties are also in the lower range of agriculture value and show a low average rate of conservation per year (Table 4).

After improving balance between treatment and control groups, I estimate the treatment effect. The first specification follows equation 1. I use fixed effects by county, states and year to clean the effect of county and state idiosyncratic characteristics that are constant through time, as well as any time shocks that are common to all states.

The second and third specifications follow equation 2. These also include fixed effects for each matched set and an treatment term that averages the effect for the whole sample. The matrix of covariates includes: *percentage of farms, average farm size, agriculture value per acres, unemployment, median household income, population density and percentage of votes cast for democrats in presidential elections.* The dependent variable is the percentage of undeveloped land protected per year (Table 5).

Results are similar for all three specifications. Panel estimation without matching show that tax breaks have a positive and significant results on the amount of acres protected. Tax breaks increase the percentage of undeveloped land protected in 0.0708 percentage points (Column 1) in counties with tax break. Both matching estimations slightly increase the coefficient of interest. Rank-based mahalanobis shows the highest effect of a tax break, but reduces the amount of observations in almost one third. Also, the balance between treatment and control was not that good. Propensity score improves balance of the sample without dropping too many observations. It estimates an effect of a tax break of 0.0712 percentage points for treated counties (Column 2). A treated county protects on average at a rate of 0.06% undeveloped land per year. All panel estimations suggest that after a tax break a treated county will more than doubled the rate at which it protects undeveloped land, reaching between 0.13% and 0.14% per year.

Anticipatory Effects

Effects of a tax break policy may affect conservation rates differently at different points in time. Landowners may react to a tax break a couple of years before it starts, and may have a dynamic response a few years after its implementation. I present the results with this new specification and explain the effects in terms of the amount of acres protected.

I consider both anticipatory and lagged effects to pinpoint differences in conservation rates per year before and after tax breaks. On the one hand, the announcement of a tax break policy can hold conservation until the policy is in effect. One can expect to see a negative effect one or two years before a tax break. On the other hand, the implementation of the policy can affect the consequent years in different ways. It may take a few years for landowners to understand the new incentives, which may delay the actual effect of the policy. Or the effect could be higher in the first few years, compensating an anticipatory negative effect, and slowly fading out in future years. Formally:

$$Y_{it} = \alpha + \kappa_c + \gamma_s + \lambda_t + \sum_{\tau=0}^{m} \delta_{-\tau} D_{i,t-\tau} + \sum_{\tau=1}^{q} \delta_{+\tau} D_{i,t+\tau} + \epsilon_{it}, \tag{3}$$

where $\sum_{\tau=1}^{q} \delta_{+\tau} D_{i,t+\tau}$ captures anticipatory effects, assuming the tax break have occurred τ years sooner, and $\sum_{\tau=0}^{m} \delta_{-\tau} D_{i,t-\tau}$ captures lagged effects, as if the tax break occurred τ years later.

I estimate both effects using the second model from Table 5, column 2: full matching with a propensity score distance matrix, and fixed effect panel estimation. I show two new specifications: one with only anticipatory effects and the other one with both, anticipatory and lagged effects.

The first specification (Table 6, column 2) shows a significant effect for the year of the tax break and a year before. As expected, the rate of conservation for the previous year of a tax break decreases. Compared to the conservation rate in treated counties, conservation decreases in 0.0295 percentage points. Once the tax break is in place, treated counties protect 0.0755 percentage points more of undeveloped land than before. This increment represents the effect of tax breaks, net of the anticipatory effect.

The second specification includes anticipatory effects and also estimates how tax breaks affect conservation in future years. Significant effects appear at the year of implementation and later in time, four and seven years after. The effect for the year of the tax break is higher than previous specifications (0.0921 percentage points). The reason for this difference is that, by decomposing the lagged effect, one can see that an increase in land conservation happens primarily during the first year of a tax break. The next significant increases happen at years four and seven, 0.0243 and 0.0759 percentage points, respectively. This reinforces the hypothesis that, after the first response to a tax break. it may take a while for landowners to learn about the new incentive and decide to protect their land.

The question that follows is how much more land is protected as a result of a tax break incentive. Percentage points show that the average conservation rate more than doubles after the tax break. But what does this mean in terms of acres protected? I show conservation rates and the amount of acres protected on an average treated county (Table 7).

Estimations show how tax breaks affect conservation on treated counties. An average treated county's undeveloped area represents almost 94% of its total area. This translates in approximately 420,000 acres. Counties with a tax break protect, on average, 0.0654% of their undeveloped land the year before a tax break (approximately $27,500$ acres). The rate of conservation for the year before a tax break is calculated as an average for the calendar year before a treated county puts in place a tax break incentive. The first estimation in table 6 show an increase of 0.0709 percentage points that translate in a new conservation rate of 0.1363% per year (Table 7, row 1). In terms of the amount of acres protected, this means that tax breaks will result in $57,000$ acres protected every year on an average treated counties. This estimation does not consider any anticipatory or lagged effects

Including anticipatory effects show how information on upcoming law changes can affect land conservation decisions. Two years before tax breaks incentives start, treated counties protect land at a rate of 0.0937%. This average is the rate of conservation at $t-2$. At this time, treated counties protected on average $39,000$ acres per year. The second specification in Table 6 shows that the year before a tax break, i.e. $t-1$, the rate of land conservation decreases in -0.295 percentage points. One can observed on average $27,000$ acres protected that year, $12,000$ acres less compared to a year before. The yearly rate of conservation reaches 0.14% for the first year of a tax break, and the amount of acres protected is on average $59,000$ (at t). This shows the net effect for the first year of a tax break, probably higher compensating for some reduction due to the anticipatory effect. After the decrease of conservation the year before, once the tax break is in place, landowners who were waiting move forward and donate their land.

Lagged treatment dummies help clean the effect of treatment for the first year and decompose the effect for future years after a tax break implementation. The third specification shows almost the same anticipatory effects as before. The first year of a tax break, the effect is 0.092 percentage points (Table 7, row 5). This translates in a new estimated conservation rate of almost 0.16% for the first year, an average of $66,000$ acres. Lagged effects appear four years after a tax break started, with an increase in conservation rate of 0.0243 percentage points. At $t+2$, i.e., the third year after a tax break, treated counties protect on average 0.10% of undeveloped land. The estimation forecast an increase in that rate, reaching almost 0.13%. The amount of acres protected per year at this point is almost $54,000$. At year seven ($t+6$), the conservation rate increases again and reaches almost 0.23% (0.0759 percentage points more than the conservation rate at $t+5$). Treated counties protect on average $95,800$ acres that year.

A back of the envelope calculation can help to quantify how much States give up in loss fiscal revenue. States with a tax break allow a deduction from the state income tax that ranges between 25% and 50% of the fair market value of the land donated. Land donors can input the dollar value of the donation as a tax credit, i.e. the deduction is subtracted directly from the tax owed.

Two different scenarios consider a lower and upper bound for this calculation. I use the specification of no anticipatory or lagged effects which conservatively estimates an average of $57,000$ acres protected in an average treated county the year of a tax break. The average agriculture value per acre for the year of a tax break is approximately $4,000$. I do not have the actual prices for land donated, but the agriculture value per acre is a good approximation of the land market value. The first scenario considers the lower bound deduction of 25% of the value of the land. This means that states with a tax break get $\$1,000$ less per acres protected $(4,000 \times 0.25)$. The second scenario considers an upper bound, with a deduction of 50% of the land value. These states get on average $\$2,000$ less per acres protected $(4,000 \times 0.50)$.

Since in an average treated county landowners donate $57,000$ acres the year of a tax break, the average deduction can range between $57 and $114 millions for the State $(57,000 \times 4,000 \times 0.25$ and $57,000 \times 4,000 \times 0.5)$. Because some states have a tax credit limits per year, this deduction is sometimes carried over several years. However, the total amount deducted in the end will still be the total amount presented here.

5.2 Risk Set Matching

A second approach to matching uses the full panel instead of only data before any treatment for matching.This is known as risk-set matching and it refers to matching observations that are "at risk of receiving treatment", before they are treated (Rosenbaum, 2010). The theory of this type of matching is explained in Li et al. (2001), and some applications include Wu et al. (2008), Rosenbaum and Silber (2009), Silber et al. (2009), Nieuwbeerta et al. (2009).

I match treated county-year observations with control county-year observations. Control counties are all observations that were never treated and if they were, I use only all years before treatment. Treated counties are the ones with a tax break, but only the first year of treatment [5]. This reduces the matching set to $22,161$ observations, 421 treated and $21,740$ controls. Before dropping treated observations after the first year of treatment, I calculate the average percentage of undeveloped land protected per year five years in the future. I use this later as the dependent variable to measure the effect of the tax break incentive.

I use the same two methods to build two different distance matrices: propensity score and rank-based mahalanobis. The set of covariates to calculate distances for both

[5] After a tax break is in place, the treated county is always treated. Once that county-year observation is matched with a control, it cannot be used anymore

methods includes: *cumulative percentage of undeveloped land protected the year before a tax break, agriculture value per acre, population density* and *proportion of forest.* Three other distance matrices work as penalties to avoid certain types of matching. The first penalty matrix avoids matching counties with itself. The second penalty matrix avoids matching a treated county with a control one from a later year. The third penalty matrix does not allow matching a treated county with a control county that will become treated in the next five years. The final two distance matrices include the base propensity score or rank-based mahalanobis distance matrix, with the three penalty matrices.

Optimal full matching based on the propensity score results in better balanced treatment and control groups. Some of the covariates show less standardized mean differences compared to the raw sample. There are still some significant standardize differences in agriculture value per acres and proportion of forest, although their values are not that high (Figure 5). Overall balance for the whole sample improves.

Using a rank-based mahalanobis distance matrix improves balance relative to propensity score distance matrix. The standardize difference in mean for agriculture value per acres is still significant, but the other three covariates do not show significant standardize differences (Figure 6). Overall balance is better compared to the raw sample and similar to the one achieved with propensity score.

After full matching both approaches yield similar results. Propensity score drops 33 treated county-year observations and 1663 control county-year observations. It matches only 4 one-to-one pairs. Rank-based mahalanobis drops 32 treated county-year observations and 1,682 control county-year observations. It forms 25 one-to-one pairs. The final number of observations is similar for both, 20,465 and 20,447 respectively. Finally, I estimate a modified version of equation 2, where the dependent variable is the average percentage of undeveloped acres protected per year, during the five years that follow a tax break incentive.

Results of risk set matching with panel estimation are similar for both, propensity score and rank-based mahalanobis (Table 8) . Using rank-based mahalanobis distance matrix shows a higher effect of tax breaks compared to propensity score. Including a a matrix of covariates improves the adjustment of the model and slightly reduces the coefficient of interest. Results are comparable to the other matching approach. Using rank-based mahalanobis distance matrix seem to be the better approach for risk set matching since it improves balance and it does not drop many observations.

5.3 Robustness checks

I expand the analysis to make sure results are robust to misspecifications. First, I estimate the effect of tax breaks with randomly generated placebo laws. Second, I test robustness of standard errors correction using two different approaches: a Monte Carlo simulation for placebo laws and collapsing data into pre and post treatment periods (Bertrand et at., 2004).

Placebo Laws

Placebo laws can be useful to check how the model performs. Randomly generated placebo tax breaks should not show any effect on land conservation. I generate placebo laws for two samples and estimate equations 1 and 2. I find no effect of this placebo laws on the yearly rate of conservation.

One way to check robustness of results is to randomly assign treatment to treated and/or control states and see how the model performs. Under this assumption, there is no reason to believe that this placebo treatment will show any effect on land conservation. One would expect that previous estimations show the actual effect of tax break laws, and that the increase in conservation is not just randomly explained by the data itself. I choose two different sets of states and assign random treatment to check this hypothesis.

The first approach takes a sample of all the states on the east region that never had a tax break. This sample consists of 18 states with no tax break between 1990 and 2010. I then randomly select five of those states and assign a tax break law for a specific year.

The year is also randomly selected between 1995 and 2005, to ensure enough observations available after the placebo tax break. I replicate Table 5 and estimate a panel fixed effect model without matching (column 1) and with matching using propensity score and rank-based mahalanobis distance matrices (columns 2 and 3, respectively). Treated and control observations are matched before the first tax break.

Results show no significant effects of placebo laws for the period 1990-2010 in control states (Table 9). Panel estimation after matching using rank-based mahalanobis distance matrix shows a negative effect, significant at 10%. A possible explanations for this significant result is that Rank-mahalanobis does not achieve a good balance in the cumulative percentage of acres protected before a tax break. This seriously compromise comparability between treatment and control groups. Also, it uses almost half the number of observations, resulting in less trustable coefficients. Results from the other two estimations are not significant strengthening the arguments in favor of matching with propensity score distance matrix as the preferred model for this study.

The second approach uses all states, treated and controls, but before any tax break starts. This sample consists of a balanced panel of 30 states between 1990 and 1998. I exclude North Carolina from the sample because its tax break started in 1983. Following the same steps mentioned before, I randomly assign placebo laws to 10 states, for a specific year randomly selected between 1992 and 1995. I estimate the same three specifications.

Results are similar, showing no significant effect of placebo tax break laws (Table 10). Rank-based mahalanobis estimation shows again a slightly significant effect. The same reasons apply here and the aforementioned conclusions continue to be true.

Randomly assigned placebo laws show no effect on land conservation and support the selected model specification. Matching using propensity score distance matrix is the preferred estimation to measure the effect of these tax breaks. The next step is to check if standard errors are robust and if coefficients are as significant as expected.

Robustness of Standard Errors

First Approach: Monte Carlo and Placebo Laws

A Monte Carlo exercise with placebo laws can help to support the method selected for correcting serially correlated errors. I follow Bertrand et al. (2004) and randomly select states with no tax break and assign them treatment at random years. One expect to reject the null hypothesis of no effect around 5% of the times, at a significant level of 5%.

I use the same two samples as before for this exercise. First I use a sample of all states that never had a tax break (18 states, for the period 1990-2010). Then, I use a second sample of all states before any tax break started (30 states, for the period 1990-1998). I follow the same steps as before and randomly assign placebo laws for specific years. For each sample, I run a Monte Carlo experiment of 200 simulations, where I estimate equation 1 for 300 different placebo laws assignment.

Results are different depending on how standard errors are calculated. I report the average rejection rate where the absolute value of the t-statistics is bigger than 1.96 (Table 11). The first t-value corresponds to *not corrected* standard errors, ignoring serial correlation. It shows that 26% of the times the model finds an effect of tax break laws, where in fact no such effect exists. After clustering standard errors by state, the rejection rate drops to almost 7% (first row of Table 11). The second sample shows similar results. When errors are not corrected, the rejection rate climbs to a 21%, while clustering standard errors by state shows that 6.8% of the times placebo laws show no effect.

This approach shows that in this particular problem, clustering standard errors by state helps correct the serial correlation. Rejection rates of the null hypothesis of no effect drop to their expected values when standard errors are clustered by state. This exercise reinforces the assumption that errors are serially correlated. The variance-covariance matrix appears to be block diagonal by state.

Second Approach: Collapsing Data, Pre and Post Treatment

Collapsing data to two effective periods is straightforward when treatment happens at one point in time for all treated observations. However, when treatment takes place at different times, this method may be not possible for all the sample. In this particular case I reduce the sample and use as treated only states that passed a tax break law between 1999 and 2001. Connecticut started the tax break incentive in 1999, Delaware and Virginia in 2000 and finally Maryland and South Carolina in 2001. I dropped observations on eastern states that implemented a tax break after 2001 but before 2010: Mississippi (2003), Georgia (2006), New York (2007), Iowa (2008), Arkansas (2009) and Florida (2009). North Carolina has had a tax break since 1983, so it was also dropped from the analysis. All other states in the eastern region are considered control states.

I estimate two specifications. First I estimate a simple before and after difference-in-difference model. I define acres protected before and after the tax break as follows: *acres protected before* is the average of undeveloped land protected per year between 1991 and 1998, *acres protected after* corresponds to the average of undeveloped land protected per year between 2002 and 2009. I use the same set of covariates as before, at a specific point in time: 1998 and 2009. Second, I estimate the same model with a matched sample and a fixed effect for matched groups. I match on observable characteristics before any tax break is in effect (1998).

Results show a positive and significant effect of tax breaks. The coefficient of interest is different than the one estimated with a panel model. This is reasonable because both samples are actually different. In the difference-in-difference approach some states are left out. Furthermore, the dependent variable is a difference in average before and after treatment. This removes the effect of an increasing conservation trend and increases the difference before and after treatment. However, results are comparable to panel estimations with standard errors clustered by state. The specific equations and tables can be found in the Appendix.

6 Concluding Remarks

This study is a general approach to measure the effect of State Tax Breaks on the average percentage of acres protected per year. It concentrates on a extended region, a comprehensive definition of eastern states of United States, which provides a big picture of the incentive's effect. One should interpret the results presented here as a first approach to quantify the overall effect of these policies. Other studies have analyzed the effect of a state tax incentive at a smaller scale, but this exceeds the scope of this paper.

State tax breaks for conservation have a positive and significant effect when analyzing their effect on the eastern states. All estimations show that implementing a tax break increases the rate at which counties protect undeveloped area. An average treated county protects undeveloped land at a rate of almost 0.14% after a tax break starts. This suggests that the rate of conservation more than doubles for counties with a tax break. In terms of acreage, this new rate represents an average area of approximately $57,000$ acres protected every year.

Effects of a tax break are not uniform in all years. Considering anticipatory and lagged effects show how these incentives have a different effect at different points in time. A year before a tax break, the rate of conservation decreases in 0.0292 percentage points, an average of $12,000$ acres less protected. The first year of a tax break, the rate has a peak increasing 0.0921 percentage points and treated counties protect on average $66,000$ acres that year. Tax breaks have no significant effect for the next two years, showing again a positive and significant effect four and seven years after they started.

Optimal full matching improves balance between covariates. Looking at raw data, treated and control counties conservation trends are similar. Nevertheless, matching on observables refine balance between both groups, making them more comparable. Propensity Score distance matrix shows better balance and does not drop many observations, making it the preferred approach. Its results are robust when tested under randomly generated placebo laws.

Although tax incentives lead to increase in conservation, an important trade-off also emerges. Further analysis needs to explore how big these tax credits are in terms of less tax revenue for the states implementing the deduction. A back of the envelope calculation shows the loss in revenue for states with a tax break is between \$1000 and \$2000 per acre protected. This becomes an important issue when many of these states are facing unbalanced budgets and increasing fiscal deficits. The efficient use of public resources is generating more debate now. It is important to study if these policies actually accomplish the goal for which they were design, and to measure the real effect they have. This will help policy makers quantify the effect of the use of state resources, and help decide how to target government spending according to their needs.

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Table 1: State Tax Incentives

State	Year	Type of Conservation
Arkansas	2009	Donation of conservation easements in wetland and riparian zones
California	2001*	Donation of land, easement or water rights
Colorado	2007	Donation of a conservation easement
Connecticut	1999	Donation of land or conservation easement (corporate state tax)
Delaware	2000	Donation of land or easement
Florida	2009	Conservation easement
Georgia	2006	Donation of land or conservation easement
Iowa	2008	Donations of land or conservation easements
Maryland	2001	Conservation easement
Massachusetts	2011	Donation of land or conservation easement
Mississippi	2003	Conservation easement
New Mexico	2008	Donation of land or conservation easement
New York	2007	Conservation easement
North Carolina**	1983	Donation of land or conservation easement
South Carolina	2001	Donation of land for conservation
Virginia	2000	Donation of land or conservation easement

*Not in effect between 2002 and 2005
**Tax break suspended since 2014

Table 2: Summary Statistics

	% Undeveloped Protected				Ag. Value per Acre				Population Density				Proportion Forest			
	Control		Treated		Control		Treated		Control		Treated		Control		Treated	
State	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd
AL	0.06	0.36			2092.95	621.71			124.26	103.06			0.64	0.17		
AR			0.03	0.29			1564.67	607.91			41.05	36.92			0.40	0.30
CT			0.04	0.17			10121.48	4654.86			688.82	496.83			0.54	0.13
DE			0.02	0.07			5483.37	3206.75			517.28	462.13			0.20	0.05
FL			0.07	0.42			3913.99	3785.18			208.55	242.06			0.24	0.16
GA			0.03	0.29			2963.55	2051.39			251.76	515.42			0.57	0.21
IA			0.01	0.08			2182.37	792.58			52.80	55.09			0.10	0.09
IL	0.04	0.33			2657.44	1123.20			196.72	641.95			0.15	0.12		
IN	0.03	0.36			2709.58	864.86			186.03	301.98			0.22	0.19		
KY	0.03	0.21			2129.16	1066.05			128.12	250.51			0.50	0.23		
LA	0.12	1.04			2202.70	3365.17			152.40	392.21			0.28	0.28		
MA	0.01	0.06			11590.26	7481.76			702.42	562.76			0.45	0.23		
MD			0.14	0.23			4567.58	2493.15			774.62	1636.18			0.33	0.17
ME	0.26	1.35			2045.21	987.41			86.75	88.48			0.67	0.16		
MI	0.02	0.14			2361.37	1252.89			185.75	425.25			0.29	0.19		
MN	0.03	0.15			1752.18	985.87			77.04	231.43			0.14	0.14		
MO	0.02	0.12			1597.32	728.31			200.37	756.11			0.33	0.24		
MS			0.08	0.46			1314.19	547.08			52.02	37.49			0.29	0.24
NC			0.01	0.08			3816.23	2164.77			249.77	316.21			0.54	0.28
NH	0.34	0.97			3514.79	1780.48			170.24	143.33			0.77	0.09		
NJ	0.02	0.16			17694.39	23237.47			1879.78	2896.19			0.37	0.16		
NY			0.07	0.41			6682.84	20031.88			508.85	1208.97			0.55	0.22
OH	0.04	0.52			3723.10	3260.63			518.68	722.84			0.33	0.20		
PA	0.04	0.14			4192.39	4162.17			573.35	1649.82			0.61	0.21		
RI	0.04	0.16			11648.49	6238.36			1135.98	575.19			0.37	0.21		
SC			0.07	0.41			2384.89	1179.10			160.54	131.12			0.49	0.14
TN	0.02	0.14			2601.74	1084.68			148.75	236.35			0.59	0.26		
VA			0.15	0.50			2617.03	3735.87			640.92	1353.71			0.56	0.21
VT	0.14	0.71			2216.21	757.00			68.96	58.85			0.70	0.19		
WI	0.02	0.18			2222.14	1187.35			205.38	552.46			0.31	0.21		
WV	0.04	0.43			1597.23	937.82			116.26	128.92			0.76	0.12		
All Region	0.0456	0.419	0.0817	0.388	3008.54	5011.46	3446.79	7785.98	254.34	812.83	382.64	975.23	0.36	0.27	0.45	0.26

Figure 1: Treated and Control States - Eastern Region

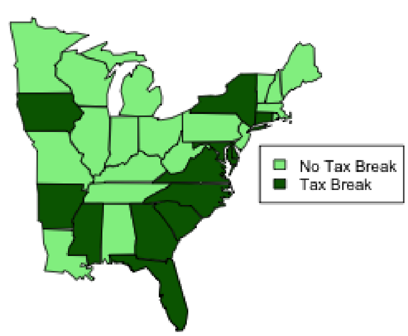

Figure 2: Land Conservation Trends

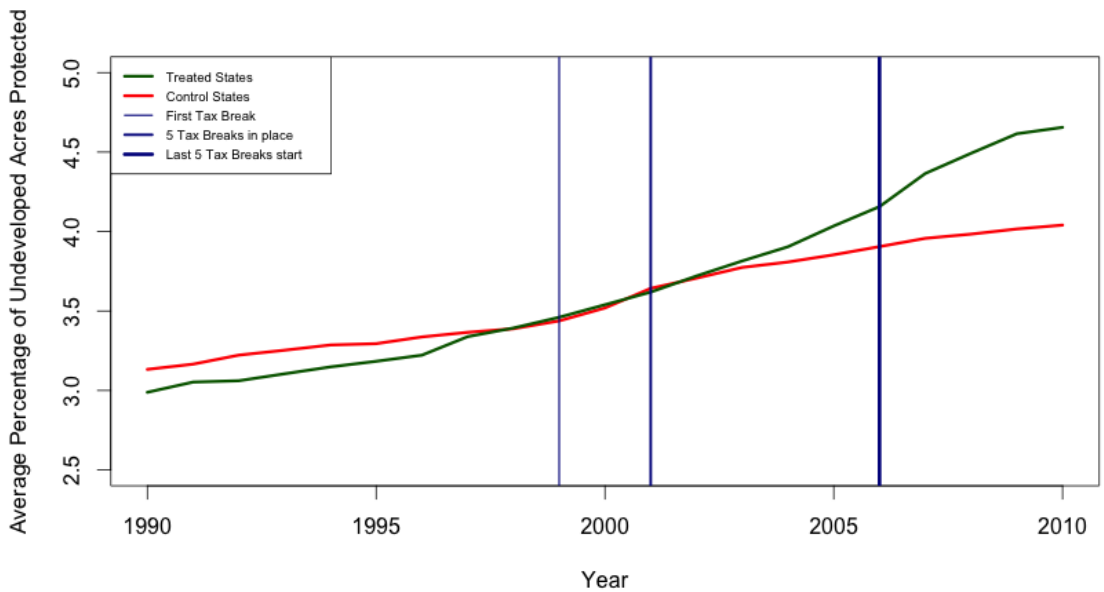

Figure 3: Balance for matching using Propensity Score distance matrix

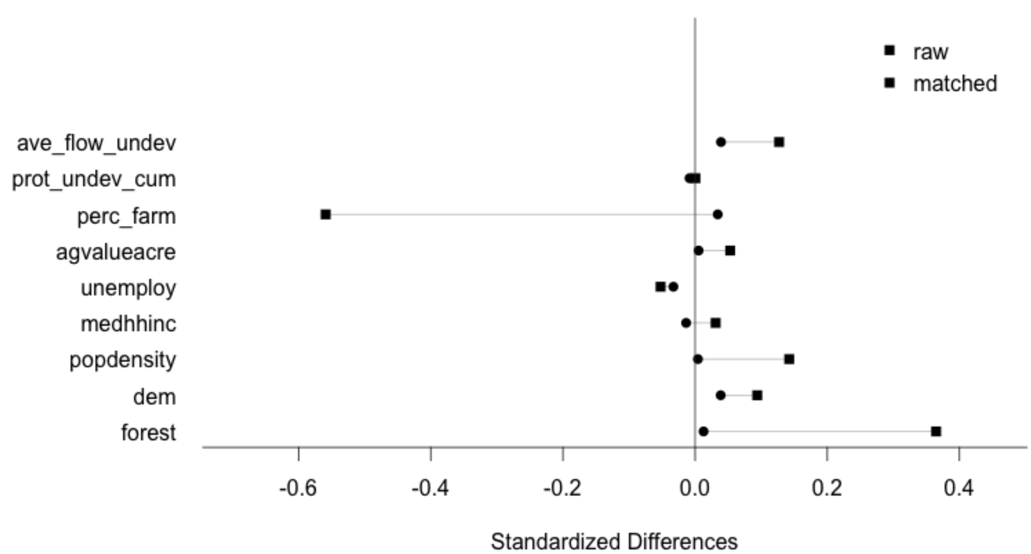

Figure 4: Balance for matching using Rank-based Mahalanobis distance matrix

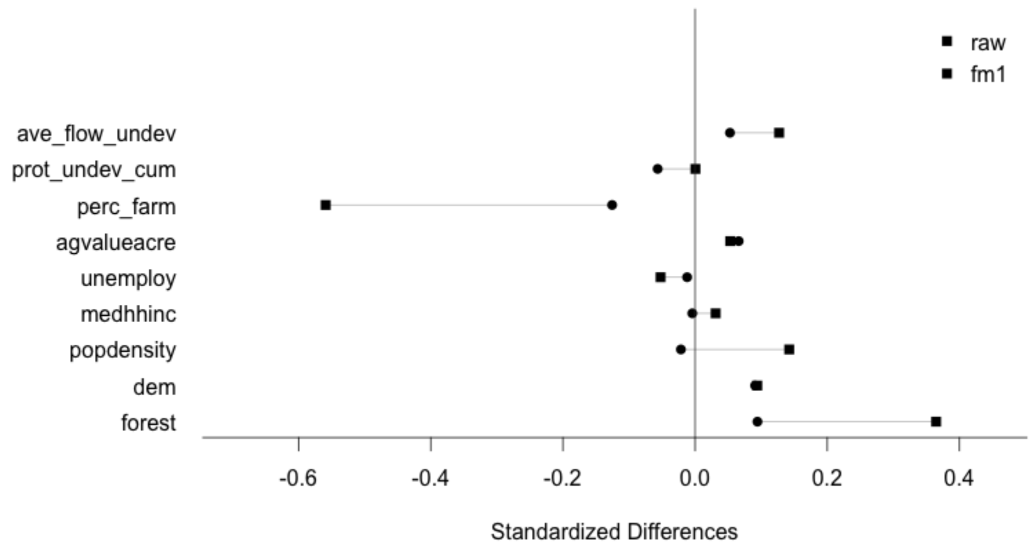

Figure 5: Balance after Risk Set Matching using Propensity Score distance matrix

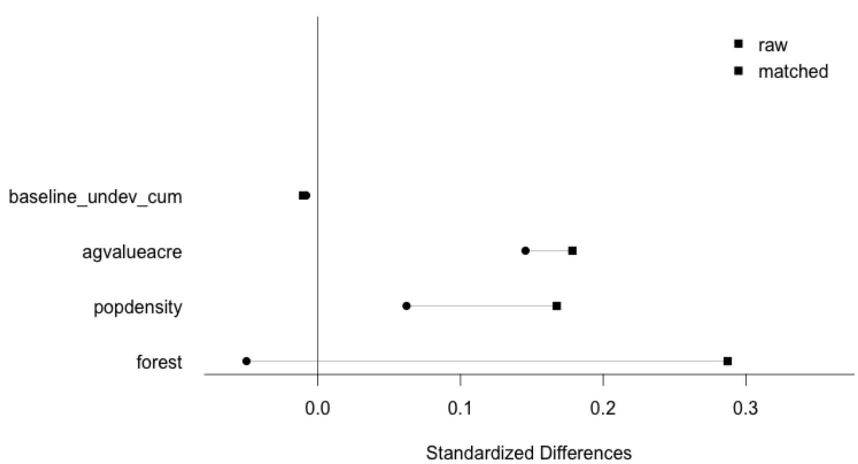

Table 3: Standardized Differences between Treatment and Control Groups after PS matching

	taxbreak=0	taxbreak=1	std.diff	z	p-values
% undev. protected	0.0353	0.0404	0.0392	0.6471	0.518
cum. % undev. protected	3.5772	3.5056	-0.0084	-0.1274	0.899
% area in farms	34.7825	35.7276	0.0343	1.2985	0.194
ag. value per acre	2715.7565	2744.4064	0.0052	0.0778	0.938
unemployment	5.1865	5.1029	-0.0328	-0.5271	0.598
median HH income	35344.5432	35217.6800	-0.0136	-0.2038	0.839
pop. density	338.9842	342.7020	0.0043	0.0588	0.953
% votes democrats	47.5235	47.8591	0.0388	0.5733	0.566
proportion forest	0.4183	0.4217	0.0128	0.2320	0.817

Table 4: Summary Statistics for dropped counties (Rank-Based Mahalanobis)

	Control Counties		Treated Counties	
	Mean	St. Dev	Mean	St. Dev
% undev. protected	0.0383	0.1607	0.0736	0.1980
cum. % undev. protected	4.6515	10.3429	6.2017	12.6157
% area in farms	47.3179	32.0176	21.9858	20.3905
ag. value per acre	2066.1676	2367.9407	1821.4782	1260.3562
unemployment	5.1899	2.6989	4.4910	2.5216
median HH income	34509.9058	7170.2785	33817.5143	7316.8940
pop. density	225.4311	987.7626	581.0174	1404.7501
% votes democrats	46.3752	8.1331	44.8820	9.8948
proportion forest	0.3193	0.2620	0.4544	0.2662

Table 5: Panel Estimation Results

	Dependent variable:		
	Percentage of Undeveloped Acres Protected		
	No Matching	Propensity Score	Rank-Mahalanobis
treatment	0.07080**	0.07120***	0.08730***
	(0.02900)	(0.02730)	(0.03170)
% area in farms	−0.00085	−0.00069	−0.00083
	(0.00086)	(0.00085)	(0.00110)
ave. farm size	0.00025*	0.00024*	0.00014
	(0.00014)	(0.00013)	(0.00017)
ag. value per acre	−0.0000004	−0.000001	−0.000001
	(0.0000005)	(0.000001)	(0.000001)
unemployment	0.00035	0.00027	0.00107
	(0.00265)	(0.00268)	(0.00312)
median HH inc	−0.0000001	0.0000002	0.000001
	(0.000002)	(0.000002)	(0.000002)
pop. density	0.00001	−0.00006	−0.00003
	(0.00009)	(0.00005)	(0.00007)
% votes democ	0.00159**	0.00185**	0.00106
	(0.00077)	(0.00075)	(0.00076)
Observations	25,116	24,906	15,456
R^2	0.08800	0.08890	0.09420

Note: *p<0.1; **p<0.05; ***p<0.01
Clustering standard errors by county or year does not change results

Figure 6: Balance after Risk Set Matching using Rank-Based Mahalanobis distance matrix

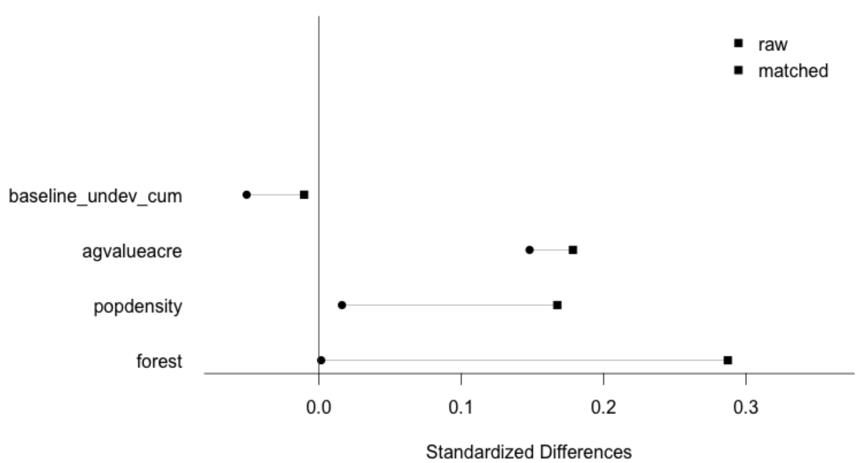

Table 6: Anticipatory and Lagged Effects

	Dependent variable:		
	Percentage of Undeveloped Acres Protected		
	No Anticipatory or Lagged Effects	Only Anticipatory Effects	Both Effects
treatment	0.07120***	0.07550**	0.09210*
	(0.02730)	(0.03650)	(0.05540)
% area in farms	−0.00069	−0.00058	−0.00072
	(0.00085)	(0.00082)	(0.00093)
ave. farm size	0.00024*	0.00025*	0.00024*
	(0.00013)	(0.00013)	(0.00013)
ag. value per acre	−0.000001	−0.000001	−0.000001
	(0.000001)	(0.000001)	(0.0000005)
unemployment	0.00027	0.00045	0.00076
	(0.00268)	(0.00265)	(0.00264)
median HH inc	0.0000002	0.0000004	−0.0000001
	(0.000002)	(0.000002)	(0.000002)
pop. density	−0.00006	−0.00006	−0.00007
	(0.00005)	(0.00005)	(0.00005)
% votes democ	0.00185**	0.00186**	0.00173*
	(0.00075)	(0.00078)	(0.00091)
$t-1$		−0.02950*	−0.02910*
		(0.01600)	(0.01490)
$t-2$		−0.03010	−0.02950
		(0.06180)	(0.06210)
$t-3$		0.05090	0.05330
		(0.06220)	(0.06380)
$t-4$		0.01860	0.01930
		(0.02000)	(0.02110)
$t+1$			−0.05540
			(0.04090)
$t+2$			−0.00745
			(0.04700)
$t+3$			0.02430*
			(0.01290)
$t+4$			0.02210
			(0.02280)
$t+5$			−0.01150
			(0.02910)
$t+6$			0.07590**
			(0.03790)
$t+7$			−0.02430
			(0.01990)
Observations	24,906	24,906	24,906
R^2	0.08890	0.08940	0.09020
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 7: Estimation of Acres Protected including Anticipatory and Lagged Effects

	Mean Undeveloped Acres	Conservation Rate		Mean Acres Protected Before Tax Break	Estimated Coefficient		New Conservation Rate	Mean Acres Protected After Tax Break
		Year	Rate		Year	Coeff.		
No anticipatory or lagged effects	419610.72	t-1	0.0654	27438.93	t	0.0709	0.1362	57168.70
Only anticipatory effects	419610.72	t-2	0.0937	39303.55	t-1	-0.0295	0.0642	26929.82
		t-1	0.0654	27438.93	t	0.0755	0.1409	59108.18
Both anticipatory and lagged effects	419610.72	t-2	0.0937	39303.55	t-1	-0.0291	0.0645	27075.55
		t-1	0.0654	27438.93	t	0.0921	0.1575	66079.15
		t+2	0.1036	43476.74	t+3	0.0243	0.1279	53686.26
		t+5	0.1526	64037.95	t+6	0.0759	0.2285	95868.12

Table 8: Risk Set Matching and Panel Estimation

	Dependent variable:			
	Average % of Undeveloped Acres Protected during the next 5 years after Tax Breaks			
	Propensity Score		Rank-Based Mahalanobis	
	(1)	(2)	(3)	(4)
treatment	0.062^{**}	0.061^{**}	0.073^{***}	0.071^{***}
	(0.025)	(0.024)	(0.024)	(0.025)
% area in farms		-0.0002		-0.001
		(0.001)		(0.001)
ave. farm size		0.0003^{*}		0.0004^{**}
		(0.0002)		(0.0002)
ag. value per acre		-0.0001^{***}		-0.00000
		(0.00001)		(0.00000)
median HH inc		-0.00000		-0.00000
		(0.00000)		(0.00000)
pop. density		-0.0003^{**}		0.0001
		(0.0001)		(0.0001)
unemployment		-0.001		0.0002
		(0.002)		(0.002)
% votes democ		0.003^{***}		0.003^{**}
		(0.001)		(0.001)
Observations	18,919	18,919	18,887	18,887
R^2	0.381	0.407	0.380	0.384
Note:			*p<0.1; **p<0.05; ***p<0.01	

Table 9: Estimation with Placebo Laws - Sample of states with no tax break (control states)

	Dependent variable:		
	Percentage of Undeveloped Acres Protected		
	No Matching	PS Matching	RM Matching
placebo treatment	−0.00432	−0.01730	−0.02440*
	(0.01530)	(0.01390)	(0.01450)
% area in farms	0.00009	−0.00046	0.00012
	(0.00066)	(0.00072)	(0.00114)
ave. farm size	0.00057***	0.00072***	0.00084
	(0.00021)	(0.00025)	(0.00069)
ag. value per acre	−0.0000004	0.000001	−0.000001
	(0.000001)	(0.000002)	(0.000002)
unemployment	0.00484	0.00264	0.00655
	(0.00304)	(0.00296)	(0.00495)
median HH inc	−0.000003	−0.000003	−0.000003
	(0.000003)	(0.000003)	(0.000003)
pop. density	0.00012	−0.00006	0.00015
	(0.00025)	(0.00011)	(0.00016)
% votes democ	0.00212	0.00234*	0.00222**
	(0.00131)	(0.00124)	(0.00099)
Observations	16,275	15,771	7,623
R^2	0.07250	0.07740	0.07250
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 10: Estimation with Placebo Laws - Sample of all states before any tax break

	Dependent variable:		
	Percentage of Undeveloped Acres Protected		
	No Matching	PS Matching	RM Matching
placebo treatment	0.02820	0.02870	0.04370*
	(0.02610)	(0.02630)	(0.02500)
% area in farms	−0.00655	−0.00690	−0.00992*
	(0.01290)	(0.01300)	(0.00516)
ave. farm size	0.00101*	0.00107*	0.00085
	(0.00058)	(0.00061)	(0.00091)
ag. value per acre	−0.00003	−0.00003	0.00001*
	(0.00002)	(0.00002)	(0.00001)
unemployment	−0.00248	−0.00262	0.00041
	(0.00417)	(0.00420)	(0.00441)
median HH inc	0.000003	0.000003	0.0000001
	(0.000004)	(0.000004)	(0.000004)
pop. density	0.00005	0.00005	0.00007
	(0.00009)	(0.00009)	(0.00008)
% votes democ	−0.00209	−0.00209	0.00039
	(0.00153)	(0.00154)	(0.00160)
Observations	10,476	10,368	5,886
R^2	0.13200	0.13200	0.14700
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 11: Monte Carlo Simulation
Rejection Rate for Placebo Laws

	Non-corrected St. Errors	St. Errors clustered by state
States that never had Tax Break	0.2576	0.0730
	(0.0262)	(0.0161)
All states before first Tax Break	0.2142	0.0682
	(0.0228)	(0.0140)

7 Appendix

Collapsing data to two effective periods avoid the problem of serially correlated standard errors. I use a sample of the original data and estimate the effect of five tax breaks that happen at a specific period in time. I present the equations and some tables for difference-in-difference estimation.

Before and after difference-in-difference uses a double difference between treated and control groups, before and after the treatment. I use a yearly average of acres protected per county, before and after the period 1999-2001, where five states implemented a tax break incentive[6]. I define acres before as the average percentage of acres protected in each county per year, during a period of 8 years between 1991 and 1998, and acres after is the corresponding percentage of acres protected per year during a same-length period of time between 2002 and 2009. Some counties do not show any conservation before the tax break, others do not show conservation after it. In order to avoid positive or negative biases, I kept all observations: 178 treated and 775 control counties. I present summary statistics for both samples on Table 12.

Table 12: Summary Statistics - Two Period Data

Group	undev_prot	Prop_farm	Ave_farm	agvalue	unemploy	medhhinc	popdens	dem
Control								
Mean	0.039	0.461	176723	3311	7.55	40052	259	46.30
St. Dev.	(0.141)	(0.294)	(136589)	(5413)	(3.62)	(11096)	(819)	(9.54)
Treated								
Mean	0.142	0.245	81131	3693	6.34	44995	604	46.63
St. Dev.	(0.225)	(0.161)	(64806)	(4259)	(3.32)	(15858)	(1294)	(11.19)

I estimate the following equation for a simple difference-in-difference model (Ashen-

[6]The five states with a tax break in this period are: Connecticut (1999), Delaware (2000), Virginia (2000), Maryland (2001), and South Carolina (2001). I dropped North Carolina (tax break in 1983), and states with a tax break after 2001 but before 2010: Mississippi (2003), Georgia (2006), New York (2007), Iowa (2008), Arkansas (2009) and Florida (2009)

felter, 1978, Imbens and Wooldridge 2009)

$$Y_{it} = \alpha + \gamma S_s + \lambda T_t + \tau(S_s T_t) + \beta X_{it} + \epsilon_{it}$$

where S_s and T_t are group and time dummies, and $(S_s T_t)$ is the post interaction term.

The second estimation uses a matched sample. I follow the same methods and use propensity score and mahalanobis distance matrices. I matched on the following observable characteristics before treatment (1998): proportion of acres in farms, agriculture value per acres, unemployment rate, median household income, population density, percentage of votes for democrats in presidential election, proportion of land covered in forest (1992), percentage of undeveloped land protected until 1998, and average percentage of undeveloped land protected per year between 1991-1998. The estimated equation is:

$$Y_i = \alpha + \tau D_i + \Delta_{S(i)} + \epsilon_i$$

where Y_i is the difference in % acres protected before and after treatment, in county i, and $\Delta_{S(i)}$ is a matched-set fixed effect

All specifications indicate that a tax break has a positive and significant effect on acres protected per county (Table 13). A tax break incentive increases the percentage of undeveloped land protected per county by year in 0.103% (Propensity Score Matching).

Table 13: Two-Period Difference-in-Difference Results

	Dependent variable:		
	Difference in % of Undeveloped Acres Protected		
	DID	PS	RM
treatment	0.106***	0.103***	0.102***
	(0.018)	(0.019)	(0.016)
Constant	0.054*	−0.058	0.074
	(0.028)	(0.055)	(0.090)
Observations	953	934	318
R^2	0.099	0.410	0.508
Note:			*p<0.1; **p<0.05; ***p<0.01