Role of Ethanol Plants in Dakotas Land Use Change: Incorporating Flexible Trends in the Difference-in-Difference Framework with Remotely-Sensed Data

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Abstract:

Rapid land-use changes in North and South Dakota over the past decade are mainly characterized by conversions of grasslands to crop production, especially corn and soybeans. Approximately 271,000 hectares of grasslands were lost to corn and soy production in 2006-2011 period, almost seven times the losses in 1989-2003. The implications of these changing land-uses range from reduced biodiversity and loss of habitat for waterfowl species to low agricultural productivity on these drought-sensitive marginal lands. While progress has been made in *characterizing* regional land-use changes, formal analyses establishing *causal relationships* at the local level are lacking. We construct a spatially delineated dataset for the Dakotas and utilize a Difference-in-Difference (DID) model in conjugation with Propensity Score Matching to estimate the impact of a corn-based ethanol plant on nearby corn-acres. We hold the advent of an ethanol plant to be a treatment that influences land-use on surrounding agricultural plots. Based on the Parallel Paths assumption of the DID, we find that the effect of ethanol plants on corn production varies by plants and a single point estimate for all ethanol plants in a region, as usually provided in the literature, can be highly misleading. Surprisingly, we find both positive as well as negative effects of ethanol plants on corn-acres that may be statistically insignificant. Negative estimates are irreconcilable to the economic incentives due to these corn-based ethanol plants. We find intensified corn production and reduced soybeans due to the ethanol plants. Our analysis also reflects a difference in opportunity of converting from wheat to corn and from grass to corn. We use placebo tests and pre-treatment trends in corn acres to validate the Parallel Paths assumption that identifies these DID estimates. We find that this assumption fails to hold and incorporate differentiated trends into the DID framework through more flexible assumptions. Our earlier finding of differentiated conversion opportunity from wheat to corn and from grass to corn is consistent for this updated DID framework as well. To validate the flexible assumptions due to differentiated trends, we implement a spatial placebo and find that estimating identified localized treatment effects, in this study, is challenging. The estimated treatment effects are only identified for two out of the four ND ethanol plants. The identified treatment effects due to these two ethanol plants are still found to be positive as well as negative. The negative treatment effects instead of economic incentives due to ethanol plants are puzzling.

An important contribution of this paper is that it presents a unique research design that uses quasi-experimental techniques to evaluate the impact of a change/policy upon availability of spatially delineated datasets. Through this design we showcase the implementation of a mechanism that evaluates a policy/change in the event when the identifying Parallel Paths assumption of the standard DID model does not hold.

Background and Motivation

Characterizing the Dakotan Land Use Change

Recent research findings suggest rapid land use changes in North and South Dakota, where grasslands have been lost to corn and soybean cultivation. Wright and Wimberly (2013) characterize conversion rates from grass to corn and soybean in the U.S. Western Corn Belt (WCB) from 2006 to 2011. The authors attribute expanding biofuels production and increased crop prices as potential factors driving higher production of these crops and therefore, such land use changes. The WCB spans five states: North Dakota, South Dakota, Nebraska, Iowa and Minnesota. A total of 271,000 hectares of net grassland losses in the Dakots out of 528,000 hectares in all of the WCB's five states imply that conversions during this period were predominantly in the Dakotas. Spatial characterization of land use changes in these two states, using U.S. Department of Agriculture (USDA) Cropland Data Layer (CDL), finds westward expansion of the Corn Belt in regions east of Missouri River that intersect with the Prairie Pothole Region (PPR).

Johnston (2014) provides a longer-term perspective on cropland expansion in the Dakotas, utilizing USDA National Agricultural Statistical Service (NASS) state-level cropping acres from 1980 to 2011, along with USDA CDL spatial imagery from 2006 to 2012. She reports that land attributed to corn or soybean production almost tripled between 1980 and 2011, where in 1980 it accounted for only 5% of the total area in the two states. Author also characterizes land use transitions among various categories such that probability of corn/soy being re-planted to corn/soy increased from 68% in 2006-07 to 80% in 2011-12. On the other hand, such probability for grasslands decreased from 81% in 2006-07 to 74% in 2011-12. In addition, corn and soybeans replaced multiple land uses such as wheat and other small grains that were historically predominant in this region due to their climatic tolerance. Technological advancements yielding drought and cold resistant corn and soybean varieties are reported to be potentially driving such land use conversions.

One other study by Stephens (2008) estimates the probabilities of grassland conversion conditional on amounts of surrounding grasslands, slope and soil productivity. Their annual estimate of the probability of grassland conversion was 0.004 for the Dakotas from 1989 to 2003, amounting to 36,450 hectares of grassland conversion for the period of study. However, they find that probability of conversion is not uniform across all lands of high biological value. Thus, conservation policies for such lands should be prioritized based on the probabilities of conversion, conditional on their location and other land attributes. A 2015 study by Lark, Salmon and Gibbs evaluates the types, amounts and locations of converted lands for cultivation in the conterminous U.S from 2008 to 2012. North and South Dakota are found to have experienced greatest increase in new cultivated land around all U.S. states during this period, predominantly east of the Missouri river. However, northwestern and southeastern North Dakota experienced contraction of croplands in 2008-2012 period. To evaluate conversion rates on native prairies they utilize long-term trend analyses from U.S. Geological Survey spanning 1972-2002. For the Dakotas, they report 14-25 acres of previously native prairies converted per 10,000 acres of land on the east of Missouri river and 10-14 acres converted west of the river. Overall, the Dakotas stood out with highest conversion rates on lands previously attributed to native grasses. Soybeans were found to be the first crop planted upon conversion during 2008-2012 period on east of the Missouri river, whereas west of the river spring and winter wheat were the first crop planted upon conversion in North and South Dakota respectively.

Although Dakotas' native grasslands are a natural resource of national importance, most is under private ownership. Hence, the observed land use changes reported in the recent literature are an aggregate outcome of private decisions by individual landowners. These decisions could be a result of change in many factors including climatic conditions, technology, the local business environment, infrastructure, commodity prices, government payments towards conservation and crop insurance etc. For instance, Claassen *et al.* (2011) provide evidence that federal crop insurance subsidies have intensified cropping practices by reducing related risks. They conclude that the 2008 Sodsaver provision that restricts such subsidies could reduce grassland conversions by up to 9% in the PPR. These land use decisions have not only permanently or temporarily change the overall landscape of these states, but would also have long term impacts on the welfare of local farmers in the Dakotas.

Related Concerns and Policy Implications

Land use changes in the Dakotas raise many ecological, agronomic, environmental and economic concerns and related policy implications. The aforementioned study by Wright and Wimberly (2013) acknowledges the threat to existing wetlands and supported biodiversity from rapid agricultural conversions in the PPR, since wetlands are critical nesting and habitat sites for regional waterfowl species. Increased corn and soybean acres on originally native grassland imply loss of ecosystem services. Reduced populations of game species, when such conversions are in close proximity to the wetlands in the area, augment these losses (Wright and Wimberly, 2013; Johnston, 2014; Stephens *et al.* 2005). Another finding of Wright and Wimberly (2013) that raises concerns as well as interests to policymakers is that, in the Dakotas, corn/soybeans has replaced pasture and hay for livestock production on high quality lands (Land Capability Class II, explained hereafter in the *Data* section). First, higher production of corn and soybeans means fewer opportunities for livestock production. This may be due to an imbalance in incentives towards intensive cropping through reduced risks with insured crops and investments into developing tolerant genetically-engineered seed varieties. Second, rapid increase in corn and soybeans in the region would tailor the socio-economic structure of the region towards more crop-based infrastructure, thereby making crops even more attractive to farmers.

Agronomic issues arising from grassland conversions relate to reduced soil quality and increased soil erosion. Shifts from grass-based agriculture to crop-based agriculture reduce the water holding capacity of the soils, reduce soil ecosystem functions and decrease soil carbon thereby reducing soil productivity. Erosion due to intensified row cropping practices, especially corn, degrades soil quality and pollutes water streams in the region (Wright and Wimberly, 2013; Johnston, 2014). Degraded soils ultimately affect land productivity due to elevated vulnerability to drought due to less suitable climates of this region (Wright and Wimberly, 2013). Further intensification of agricultural activity and prolonged periods of extreme weather events like droughts in this region are considered serious threat to mostly ephemeral wetlands. Further, loss of stored carbon from uprooting the native grasses accounts towards environmental impacts of conversion (Johnston, 2014).

Among the policy suggestions, Johnston (2014) calls for policies that incentivize farmer behavior towards sustainable agricultural practices in light of detrimental environmental and soil-quality implications of intensive corn/soy production on these marginal lands. Further, whereas Stephens (2008) suggests conservation policies to prioritize land with higher chances of conversion based on their location and attributes, Wright and Wimberly (2014) suggest regulating location of biorefineries, deemed responsible for higher corn production in their study. Lark et al. (2015), while recognizing the broad economic and environmental impacts of land use conversion, point to the need for reformed policies aimed towards conserving natural ecosystems. Even though the new Renewable Fuel Standards program (RFS2) mandated procurement of grains for ethanol production only from lands under cultivation prior to December 2007, their study finds substantial increase in croplands in the United States. Further, the authors recognize the importance of the new Sodsaver provision in the 2014 U.S. Farm Bill. This provision, applicable in the PPR states including the Dakotas, dis-incentivizes conversion of native sod for agriculture after January 2014 through reduced crop insurance subsidies. Based on their analysis, the authors recommend a nationwide Sodsaver provision that covers forests and native ecosystems other than grasslands.

Our Contribution: Moving from Characterization towards Explaining Land Use Changes

The above studies characterize the rate and extent of land use conversions in the Dakotas at various spatial and temporal scales. They also speculate on potential factors that driver these land use changes in the region. However, detailed analyses to identify various phenomena that drive land use changes in Dakotas are lacking. We take a first step in understanding this phenomenon by evaluating the impact of ethanol plants on land use changes for these states. All Dakotas' ethanol plants are corn-based. Hence, we ask how the advent of an ethanol plant affects corn plantings in its proximity. There are 19 ethanol plants in Dakotas (four in ND and fifteen in SD) with a combined capacity of 1,386 million gallons per year (mgy, 363 mgy in ND and 1,023 mgy in SD). Together, the Dakotas provide for about 9% of the total U.S. ethanol production capacity, currently at 15,198 mgy. Fourteen (out of the nineteen plants in all) started operations in 2006-2008 period, i.e. after the first RFS program was launched under the Energy Policy Act of 2005 and when rapid land-use conversion rates are found by the pertinent literature, discussed above.

To motivate the economic incentives from ethanol plants, we compare trends in county-level corn basis, before 2006 and after 2008, for counties that house these 14 ethanol plants (see figure 1). An increase in corn basis implies an increase in local corn prices relative to the corn futures price. Such an increase in corn basis could be tied to the incentives from the ethanol plants to land owners with farms in the plants' proximity. It is possible for the ethanol plants to provide such incentives to the farmers who supply them corn from near-by areas, since it saves transportation costs for both supplier and the plant. Figure 1 shows a steeper basis trend for corn in post-2008 periods compared to the pre-2006 period. Therefore, we conjecture a positive and statistically significant impact of ethanol plants on local corn acreage. We also extend our models to analyze the effect of ethanol plants on corn-soybean rotations. We do this by separately analyzing evolution combined acreages of corn and soybeans in relation to the advent of an ethanol plant, and then compare these with that of corn acreage. If the effect of an ethanol plant on corn acreage is higher than on the combined acreage of corn and soybeans, then the implication is intensified corn cropping has occurred through reduced corn-soy rotations due to the ethanol plant.

This paper is subdivided into the various sections. First, a literature review section discusses the relevant findings of the impacts of ethanol plants from studies in the past. Second is a data section that discusses how we constructed a spatially delineated dataset for this analysis and provides a detailed explanation of the relevant variables. Third, the methodology section presents our research design and the Differences-in-Difference model in conjugation with Propensity Score Matching. Fourth is a section for estimation results for each ethanol plant. Lastly, we include discussions and conclusions in another section.

Literature Review

Earlier attempts in this direction involved evaluating indirect impact of ethanol plants on land use change by way of analyzing impacts on local corn prices and farmland values. In the more recent years studies have considered direct impact of ethanol plants on corn acres as measure of land use change. We provide a detailed review of the analyses of impacts on land acreage because these are of direct relevance to this article. We also provide a brief review of analyses involving grain prices and farmland values followed by direct impacts literature.

Direct Impacts: Corn Acreage

Miao (2013) has evaluated the proportion of corn acreage for the Iowa counties in response to location, capacity and ownership capacity of ethanol plants. He utilized a county-level panel data set from 1997 through 2009 and the Arellano-Bond generalized method-of-moments estimator to estimate the effect of ethanol plants on land use shares in the region. The specialized estimator attempts to controls for the endogeneity of ethanol plants and allows controlling for corn-soybean rotations by including lagged dependent variable (that is, proportion of corn acreage). He found a positive and significant impact of ethanol plants on intensity of corn production in Iowa. He further found that, all else equal, locally owned ethanol plants have twice as strong an effect on local corn acreage as their non-locally owned counterparts.

Motamed and McPhail (2011) used remotely sensed data to estimate a non-linear response of proximity to ethanol plants on corn acreage for 12 U.S. Midwestern states: ND, SD, NE, MN, WI, IA, KS, OK, MI, IL, IN, OH. They utilized a panel regression model with corn acreage on each of 10 km X 10 km land parcels from 2006 to 2010 as dependent variable. Their explanatory variables include capacity of the nearest ethanol plant, distance to the nearest ethanol plant and grain elevators, cash bids at the nearest grain elevator and a soil productivity index for these parcels. To incorporate non-linearity of response, their regression model includes logarithmic values of dependent and explanatory variables. They recognize that land parcels' corn acreage and their distance from the nearest ethanol plants are endogenous and use an instrumental variable approach as a corrective measure. They instrument each parcel's distance from the nearest ethanol plant on local transportation infrastructure, specifically distance from the nearest interstate ramp, primary/secondary roads and water ports. This analysis finds that upon moving one percent closer to an ethanol plant corn acreage increased by 0.64% within their region of study.

Turnquist *et al.* (2008) measure the impact of ethanol plants on farmland acreage for the state of Wisconsin between years 2000 and 2006. Although Wisconsin was reported to be losing its farmland to other uses during this period, fallow or undeveloped acres were found to increase. This indicated that factors other than development pressures were driving land use in Wisconsin. In addition, given that increases in fallow land are reversible to agricultural production, evaluating the impact of ethanol plants is interesting. The authors use land use data for municipalities in the state and define zones of 2, 10 and 50 miles around 4 operational ethanol plants during 2000-2006 period. The statistical differences between percentage change in agricultural acreage between 2000 and 2006, within- and outside each of these zones, evaluate the impact of ethanol plants in Wisconsin. Impact of ethanol plants on each of 3 zones' agricultural acreage is found to be statistically insignificant.

Mueller and Copenhaver (2009) analyzed the impact of two Illinois ethanol plants (Illinois River Energy Center (IRE) and Patriot Renewable Fuels (PRF)) on surrounding land use, as part of a larger study to deduce the impact of these plants on greenhouse gas emissions. They used satellite imagery and observe land use in corn supply regions for each plant in 2006, 2007, and 2008 to evaluate its impact. Defining these corn supply regions involved corn growers' surveys and inquiries from ethanol plants to adjudge the spatial extent of their corn suppliers. A 43-mile circle around IRE and 23-mile circle around PRF are respective corn supply regions. The study concluded a weak influence of ethanol plants on direct land use change in their vicinity, and inferred that increasing yields supported increasing exports as well as higher ethanol production.

Brown *et al.* (2014) utilized a spatial econometric regression framework to assess the land use decisions of farmers due to proximity to ethanol plants in Kansas. Using satellite imagery, they separately evaluate conversions from other cropland and non-cropland uses in 2007 to corn production in 2008 and 2009 on 5-acre parcels. The authors find that reducing parcel's distance to nearest refinery by 1% significantly increased non-cropland (other cropland) conversion to corn acres by 5% (4%) in a county 25 miles away from the refinery and by 15% (11%) in a county 75 miles from it. However, the authors recognize that their estimates may be biased due to endogenous ethanol plant locations.

Indirect Impacts: Local Corn Prices and Farmland Values

Miao (2013) also recognized that the literature lacks a consensus about impacts of ethanol plants on local grain prices and agricultural land values, which can be accounted as indirect effects of ethanol plants on land use change. Examples in the context of farmland values are Zhang *et al.* (2012), Henderson and Gloy (2008) and Du *et al.* (2007). Zhang *et al.* (2012) used disaggregated parcel-level data for Western Ohio to evaluate the impact of increased biofuels demand. They conducted difference-in-difference estimation on matched parcels to find increased farmland values in the vicinity of the ethanol plants, at a time that witnessed sharp dip in residential values. The study by Henderson and Gloy (2008) have used a hedonic framework to find a positive impact of ethanol plants on agricultural land values in 2007. Zhang *et al.* (2012) have, however, criticized the hedonic framework due to its inability to correct for selection bias of plant locations. Du *et al.* (2007), on the other hand, reject the hypothesis that ethanol plants significantly affect the cash rentals form farmlands in Iowa. In the context of local grain prices, Katchova (2009), O'Brien (2009), and McNew and Griffith (2005) found a positive significant impact of ethanol plants on local grain prices, whereas Lewis (2010) found that these positive impacts vary spatially. The author found a positive significant impact for MI and KS, and an insignificant impact for IA and IN.

The above review suggests disagreement on the direct and indirect impacts of ethanol plants on local land uses in the literature. Moreover, most studies utilize aggregated county-level datasets. An issue with such aggregated datasets for a location-based analysis is worth considering. Including an indicator (or dummy) variable for the existence of ethanol plants as a regressor assumes its location to be central to its home county when this variable equals 1. It, thereby, assumes that the corresponding ethanol plant will not impact the counties neighboring its home county. If the location ethanol plant is at the center of mass for each home county, we may treat the above as an assumption as reasonable. However, as in the Dakotas, an ethanol plant is often located near the shared boundaries of two or three counties. Consequently, it is appropriate to use spatially delineated data as some studies do. However, these studies ignore the issue of endogeneity that arises in these situations and provide biased estimates of the impacts of ethanol plants.

We make an extensive use of remote sensing tools that provide spatially delineated data with micro-resolutions of the researcher's choice. This article presents estimates of impact of ethanol plants using 500-acre plots as representative decision-making units.[1] This enables the evaluation of the effects of ethanol plants on a plant-by-plant basis, rather than by pooling county-level data for ethanol plants in an entire state or all of Midwestern United States. Adopting a methodology that allows for analyzing impacts of individual plants enables fine-detail scrutiny of local conversion effects. This provides an alternative approach to validate the estimates of the impacts of ethanol plants on corn acreage arrived at from more aggregate methods.

Data

We use remotely sensed data in the form of satellite imagery for the Dakotas from two main sources: land-use from the 'CropScape' portal of USDA-National Agricultural Statistical

[1] We conducted our initial analyses at a much finer resolution (up to 160-acre plots). Aggregating the data up to 500 acres did not change our results significantly. However, higher aggregations suppress measurement errors from satellite imagery.

Service's Cropland Data Layer (CDL) Program and soil quality data from the Web Soil Systems portal of USDA-National Resource Conservation Service (NRCS).

USDA-Cropland Data Layer

CDL satellite imagery for South Dakota are available from 2006 to 2013 and for North Dakota from 1997 to 2013. CDL provides raster (pixel) data for all contiguous US states with different spatial resolutions, 56 m pixels for 2006-2009 and 30 m pixels for other years. To be able to compare land-use statistics across different years, we use remote sensing tools, namely ERDAS Imagine and ArcGIS, to bring each year's imagery to a uniform spatial resolution of 500 acres. To achieve this, each year's raster image was first converted to vector form (pixels to polygons), and then overlaid onto a grid-plot with 500 acre-polygons. Each polygon, which is our representative decision-making plot of land with a unique identifier is observed for every time point thus, facilitating this analysis. Overall, we end up with approximately 104,000 land parcels for North Dakota and 99,000 parcels for South Dakota.

USDA National Resource Conservation Service - Web Soil Systems

We retrieve tabular data for Land Capability Classification (LCC) and representative slope data from the satellite imagery for both states using the Soil Data Viewer application developed by NRCS. Soil Data Viewer provides detailed definitions for both these variables. Briefly, LCC groups soils into eight broad classes each representing degree of limitations for cropping, with higher class codes assigned to greater limitations. LCC classes I and II are well-suited for cropping, whereas LCC classes III and IV require some special conservation practices for cropping, often restricting their use to pasture, rangeland or forests; and LCC V and above have severe limitations that make them impractical for crop cultivation. Representative slope simply measures the rise per unit run. The tabular data combines these soil attributes to geographically delineated and uniquely identified soil map units. See supplementary material for more information on data integration.

Ethanol Plants' Spatial Coordinates

The spatial coordinates of ethanol plants, ultimately used to determine treatment and control groups, were acquired by using the Google Earth application in conjugation with online maps locating plants made available on *Ethanol Producer Magazine*'s website. Overall, there are 4 ethanol plants in North Dakota and 15 ethanol plants in South Dakota. We conduct our analysis using 8 ethanol plants (4 in each state), listed in table 1 with spatial locations in figure 2. Choice of ethanol plants is driven by our methodology and land-use data availability in South Dakota (2006-2013), discussed hereafter under 'Estimation Results'.

Methodology

The objective is to quantify how the emergence of an ethanol plant affects local land-use change. The detailed micro-level panel dataset for the Dakotas allows us to implement a quasi-experimental design to evaluate the effects of ethanol plants on land-use patterns in their neighborhood. In this sense, we interpret the emergence of an ethanol plant as treatment where pre-and post-treatment year outcome levels are observed land-use patterns before and after its emergence, respectively. In order to implement a quasi-experimental setting where emergence of an ethanol plant is treatment, we first need to define treatment and control groups. The argument that a plant's location is potentially influenced by the opportunity for growing corn in its vicinity relates to minimizing costs of acquiring corn for ethanol production. If an ethanol plant procures most of its annually required corn from near-by areas, it saves on transportation and related logistics costs, and so is willing to compensate local suppliers. Therefore, in order to define our treatment and control groups, we assume that these transportation costs are monotonic in the Euclidean distances of a land parcel from the ethanol plant and that the ethanol plant bears at least some of these costs. In this scenario, a representative supplier/land owner located nearer to the ethanol plant has higher incentive to grow corn than the one farther away, *all else equal*. Consequently, we choose to designate samples that lie closer to the ethanol plant as treatment samples and ones farther away as control (or untreated) samples.

How Significant are Transportation Costs? Empirical Evidence

To support our argument that transportation costs, and thus Euclidean distances, are sensible treatment and control parameters, we present our back of the envelope calculations. Consider transportation trucks with carrying capacity of 1 ton (=39.4 bushels[2]) corn and mileage of 134 ton-miles per gallon. According to the U.S. Energy Information Administration, the annual average diesel price in U.S. ranged from $2.4 - $4 post 2005. At such per gallon rates for diesel, the fuel cost of transporting 1 bushel of corn for 1 mile would range from 0.05 to 0.07 cents. O'Brien (2009) estimates the total transportation cost to be approximately 4 times the fuel cost. Therefore, the maximum willingness to pay for the owner of an ethanol plant to incentivize a farmer located 50 miles closer from the plant (than her counterpart) to grow corn would range from 10 to 14 cents per bushel. On the other hand, cash rents for croplands ranged between $39-$46.5 in ND and $53-$71.5 in SD from 2006-10 (USDA NASS Land Values Summary, 2006-10). Given the corn yields of 111-132 bushels/acre in ND and 97-151 bushels/acre in SD (USDA NASS Quick Stats, 2012), the average cropland rents for the Dakotas were between 30-73 cents per bushel of corn. As the transportation costs are 14%-47% of the total cropland rental values, these should generate strong pressure for proximate landowners to engage in corn production.

Designating Treatment and Control Groups

An aspect of our research design that differentiates it from most other quasi-experimental studies is a non-centrally administered or a non-exogenous treatment. We designate the advent of an ethanol plants as treatment, which itself is a market outcome that must be bridging the supply-demand gap in commodities and biofuels markets in the Dakotas and even beyond. The implication of this endogenous intervention is that we do not have exogenous control groups. Rather, our treatment and control groups follow the 'rule of thumb' that treated parcels are located nearer to the ethanol plant than their untreated counterparts. This allows innumerable possibilities of treatment and control groups near each ethanol plant's location and practically inexhaustible combinations that can be included for this study. It is, however, important to conduct robustness checks to seek the sensitivity of our treatment effects' estimates among different combinations of treatment and control groups. We accomplish that by designating two

[2] Bushel/Ton Converter. www.agriculture.alberta.ca

treatment groups and two control groups for each ethanol plant. The control groups are kept apart to ensure independence in robustness checks for each treatment group (see Figure 3)[3]. Based on our definition that parcels farther away from the ethanol plants are controls when compared with the treated, we conjecture that treatment effects using the nearest treatment and the farthest control groups will be larger in size and more significant than the other three combinations. We present the regression results for this particular combination and compare with others as a robustness strategy.

DID in conjugation with PSM

Given pre- and post-treatment periods, as well as treatment and control groups for each ethanol plant, we use the Difference-in-Difference (DID) estimation strategy in conjugation with propensity score matching (PSM) to evaluate their role towards land use conversion. Using the DID approach is reasonable since the location of an ethanol plant is endogenous to land-use trends in its locality. The issue of endogeneity arises because Dakotas' ethanol plants are corn-based facilities and their location decisions could place them in regions with high corn production in pre-plant years or with high potential for corn production in the post-plant years. DID controls for such endogeneity by estimating causal impacts as difference between average temporal trends of land-use acres across treated and untreated groups, assuming that in the absence of the ethanol plant land-use in both these groups would evolve equivalently. This assumption of parallel trends requires constituents of treated and untreated groups (land plots, here) to be alike, except for the land-use patterns potentially affected due to their distance from these ethanol plants. That is, estimated treatment effects are unbiased if these land parcels are randomly assigned to the treatment group and we control for any other within-group or across-group dissimilarity among them. We seek to ensure random assignment of land parcels to treatment group by utilizing the PSM strategy, thereby conditioning their treatment selection on the observed soil quality variables. The soil quality variables are central to land-use decisions, and thus potentially influence ethanol plants' location choice to regions with land attributes favoring corn production. Local infrastructure such as road and railway connectivity also potentially affects ethanol plants' location choice. We tend to choose, at least for some ethanol plants, our treatment and control groups along or parallel to an interstate highway so that the Euclidean distances differentiate access to infrastructure across land parcels. It is noteworthy that while PSM controls for selection on observables, the DID estimation approach controls for selection on unobservables through individual and trend fixed-effects in the regression framework (List *et al.* 2003).

Identifying treatment effects from the DID model

The Parallel Paths Assumption (PPA) is fundamental to identifying the treatment effects that are estimated by the DID model. To illustrate this briefly, consider a representative land parcel i

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[3] It is infeasible to have all of the treatment and control groups to be non-overlapping due to spatial constraints. This is because having non-overlapping groups would require more planar space, which in turn would bring our groups closer to the near-by ethanol plants. So, we stick to non-overlapping control groups for robustness checks.

with $C_{i,t}$ as its corn acreage at time period t. We introduce binary variables d_i and d_t to designate treatment/control groups and pre-/post-treatment periods respectively. So, $d_i = 1$ for treated parcels and equals 0 otherwise, while $d_t = 1$ for time periods after the advent of an ethanol plant and equals 0 otherwise. Further, denote $t^+(t^-)$ as the set of post-treatment (pre-treatment) time periods with t_0 as the treatment year[4]. Intuitively, to evaluate a treatment effect for treated parcel i's corn acreage we would compare the outcome levels with and without ethanol plant in the post treatment era, that is $C_{i,t}$ [5] with $t \in t^+$. Consequently, the average treatment effect for the treated (ATT) equals $E[C^T_{i,t^+} - C^U_{i,t^+} | d_i = 1]$, where superscript T(U) denote presence (absence) of the plant. The issue, though, is that the outcome levels in absence of an ethanol plant (, the treatment) in the post-treatment years are unobserved. The DID approach overcomes this issue by assuming that treated and control parcels would follow *parallel land use trends* if the ethanol plant had not emerged at t. This assumption is the PPA, and can be mathematically expressed as

(1) $$E[C^U_{i,t^+} - C^U_{i,t^-} | Z, d_i = 1] = E[C^U_{i,t^+} - C^U_{i,t^-} | Z, d_i = 0],$$

In equation (1) the superscript U signifies no treatment (both groups stay untreated) and Z is the set of observable covariates for each land parcel. If (1) holds then the ATT is computed as

(2) $$ATT = E[C_{i,t^+} - C_{i,t^-} | Z_i, d_i = 1] - E[C_{i,t^+} - C_{i,t^-} | Z_i, d_i = 0]$$

Thus, the PPA is key to identify the estimates of treatment effects because in the event that this assumption fails our estimates of ATT are meaningless. To ensure that PPA holds, we restrict our sample for estimating treatment effects to one where estimated conditional probability of treatment (or propensity score, PS) for each untreated parcel is close 'enough' to its treated counterpart - usually known as PS matching.

PS Matching

To estimate a conditional probability of treatment for each land parcel in treatment and control groups of an ethanol plant, we utilize a logistic regression. The probability of treatment is regressed upon the area-weighted soil quality variables, *WLCC* and *WSLP*, in their quadratic form. That is,

(3) $$P(d_i = 1) = \frac{\exp(\alpha_0 + \alpha_1 WLCC + \alpha_2 WLCC^2 + \alpha_3 WSLP + \alpha_4 WSLP^2)}{1 + \exp(\alpha_0 + \alpha_1 WLCC + \alpha_2 WLCC^2 + \alpha_3 WSLP + \alpha_4 WSLP^2)},$$ where

$\alpha_0, \alpha_1, \alpha_2, \alpha_3$ and α_4 are regression coefficients. The justification of a quadratic functional form lies in minimizing the *Akaike Information Criterion* (or maximizing the *log-likelihood*) relative

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[4] Example: For the Red Trail Energy ethanol plant that came up in 2007, $t^+ = \{1997, 1998, ..., 2006\}$ and $t^- = \{2008, 2009, ..., 2013\}$.

[5] We present the model for corn acreage. An extension for combined corn and soy acreage follows by changing the notation from $C_{i,t}$ to $CS_{i,t}$.

to linear and cubic forms. The estimated probability of treatment, $\hat{P}(d_i = 1 | X_i^a)$ with $X_i^a = \{WLCC, WLCC^2, WSLP, WSLP^2\}$, is then used for matching treatment and control groups. The PS estimation results are summarized in table 4. We find these soil quality variables to significantly explain the probability of treatment. However, the logistic regressions estimating the PS find that land parcels in vicinity of ethanol plants may have higher LCC and/or be steeply sloped, not particularly suitable for corn production. Higher treatment probabilities for parcels with relatively poor soil quality reflect that the ethanol plants consider factors like lower land values and/or the nearby infrastructure (near a highway or a rail line) to reduce initial and operating costs too matter for ethanol plant locations. However, it is not possible to differentiate land values and infrastructure among different land parcels at the fine spatial resolution used in this study. Therefore, we investigate the spread of estimated PS to see whether our model specification explains the treatment probability reasonably well between 0 and 1 (Figures 4-9). It is also noteworthy that both WLCC and WSLP exhibit decreasing marginal returns in all cases.

We implement a one-to-one nearest-neighbor propensity score matching algorithm and include only those treated parcels for which there exists an untreated parcel whose PS lies within a pre-assigned radius (absolute difference between PSs) of each corresponding treated parcel's score. The choice of this radius involves trade-off between bias and efficiency of treatment effects. A smaller radius will yield more similar land parcels in both groups reducing bias in estimated treatment effects but at the same time a smaller sample which entails higher variance.[6] Post-matching heterogeneity in the distribution of soil quality variables among treated and untreated groups may potentially bias our treatment effects' estimates (Heckman *et al.* 1997). We report treatment effects calculated using samples from a pre-assigned radius or caliper ranging in [0.0001, 0.01]. The assigned calipers vary for by ethanol plants (Table **5**) and are chosen such that the post-matching samples are balanced while maximizing number of observations in each case. The term balancing refers to ensuring homogeneous distribution of these covariates across treatment and control groups. We find that reducing the pre-assigned radius yields higher balance across the two groups used for estimating treatment effects.

To examine whether or not the post-matching samples are balanced and assess the matching quality, we follow Caliendo and Kopeinig (2008). We conduct t- and F-statistics to test equivalence of mean and variance of WLCC and WSLP across matched treated and untreated samples for each ethanol plant (Rosenbaum and Rubin 1985). Further, we test the joint-significance of WLCC and WSLP, in quadratic form, in estimating $P(d_i = 1)$ on the matched samples. This test rejects the joint-significance of these covariates indicating no systematic differences in their distribution across treatment and control groups that could explain underlying

[6] We implement the PSM algorithm developed by Fraeman (2010), which optimizes the sample size in two steps. First, it searches for all possible matches to each treated sample within the pre-assigned radius and then while assigning matches to these treated parcels it prioritizes those with the least number of matches from the first step. The SAS code that implements this algorithm is published in Fraeman (2010).

variations in propensity scores (Dehejia and Wahba, 1999). Table **5** presents the performance of our matching strategy that yields balanced samples for all ethanol plants.

Standard DID estimation summary and moving towards flexible trends in DID

The treatment effects estimated using the DID regression framework using matched samples can be represented as $ATT^m = E[C_{i,t^+} - C_{i,t^-} \mid P(X_i^a), X_i^b, d_i = 1] - E[C_{i,t^+} - C_{i,t^-} \mid P(X_i^a), X_i^b, d_i = 0]$. Note that Z_i , above in equation (2) is replaced by X_i^b and the sample data used for this post-matching estimation will be a subset of its counterpart in (2). Consequently, if β_3^m were to estimate of our new ATT, then it can be retrieved estimating the following regression equation, $C_{i,t} = \beta_0^m + \beta_1^m d_t + \beta_2^m d_i + \beta_3^m d_i d_t + \beta_4^m X_i^b + \beta_5^m d_t X_i^b + \varepsilon_{i,t}$.

In the DID regression framework using matched samples, we further control for pre-treatment land-use decisions as an opportunity to convert to corn. Illustratively, if a land plot was entirely attributed to corn in pre-treatment years it will not reveal any treatment effect even though the impact of ethanol plant were to be positive, since there is no scope of conversion. In addition, even if it was predominantly under wheat (or grass) in the pre-treatment year, the opportunity to convert comes with switching or conversion costs, respectively. Further, in recognition of the fact that farmers usually grow corn and soybean in rotation, we evaluate treatment effects for corn as well as the combined acreage of corn and soy as our dependent variables. (Detailed estimation results of the standard DID model in conjugation with PSM are ‘Supplementary Information’ to this paper. These are available upon request.)

In summary, we find positive, negative as well as statistically insignificant treatment effects on corn acres due to ethanol plants. The negative treatment effects are both surprising as well as irreconcilable to the empirical evidence of incentives for corn production on land parcels in vicinity of these ethanol plants. To further investigate the validity of such treatment estimates, we designate temporal placebos, per figure 10, and estimate ATT for these falsified treatments. Ideally, a false treatment should yield zero treatment effects but our estimates, in Table 3, show that the standard DID framework yields non-zero treatment effects even though there was no treatment. Such placebo tests point towards an imperfect matching strategy or an inability to control for all the factors that affect growth of corn acres in our regressions. To reconcile these non-zero placebo tests, we consider the pre-treatment trends for treatment and control groups for the North Dakota ethanol plants to validate the Parallel Paths assumption of DID estimation strategy (see equation 1). Figure 11 shows that the Parallel Paths assumption has failed and that we need to incorporate differentiated trends between pre- and post-treatment periods and between treatment and control groups. We follow Ricardo and Mora (2012) to incorporate flexible trends into the standard DID model. We discuss the working of this model below.

Incorporating the Flexible-Trends into standard DID framework:

The differentiated or non-parallel pre-treatment trends across treatment and control groups (as seen in figure 11) invalidate the PPA and require incorporating such trends into the standard DID framework. We incorporate variable trends, visualized in figure 12(b), into the above setting according to Mora and Reggio’s (2013) fully flexible DID model. As an illustration, we develop a special case of this fully flexible model per figure 12(a) in an appendix. The green lines in figure 12(a) depict non-parallel trend lines that are time-invariant within pre-treatment and post-treatment periods, where trends need not be constant between any two time periods in figure 12(b), irrespective of the time window. The special case is hoped to facilitate our readers' smooth transition from the standard DID model with failed PPA to the fully-flexible DID model. An early application of the fully-flexible DID model in equation (4) can be found in Reber (2005) to assess impact of court-ordered desegregation plans for schools in 108 U.S. districts on school enrolments.

A fully-flexible DID model by Mora and Reggio (2012) is as follows:

$$(4) \qquad C_{i,t} = \beta_0 + \sum_{\tau=T(i)+1}^{T(l)} \beta_\tau I_{[t=\tau]} + \beta^d d_i + \sum_{\tau=T(i)+1}^{T(l)} \beta_\tau^d \times I_{[t=\tau]} \times d_i + Z_{i,t}{}' \beta_z + \varepsilon_{i,t},$$

Where, $T(i)$ is the first pre-treatment period and $T(l)$ is the last post-treatment period. The model in equation (4) captures flexible time-trends for pre- and post-treatment periods and allows them to differ between treatment and control groups, thus capturing a fully-flexible situation, as in figure 12(b.). The model's advantage is that it calculates time-varying treatment effects' estimates, which in turn can potentially allow differentiating between short-run and long-run impacts of the advent of an ethanol plant on the near-by corn acreage. Note that, unlike Mora and Reggio (2013), we include a vector of controls $Z_{i,t}$ in our regression equation (4). $Z_{i,t}$ consists of lagged soybean ($S_{i,t-1}$), wheat ($W_{i,t-1}$) and grass ($G_{i,t-1}$) acreage at time t for each parcel i. The variables are included in regression (4) to capture the difference in the opportunity or costs of converting different types of land use types to corn. The treatment effects estimator from equation (4), denoted as $ATT'(s,n \mid Z_i)$, is given as

$$(5) \qquad ATT'(s,n \mid Z_i) = \Delta^{n-1} ATT(s \mid Z_i) = \Delta_s \Delta^{n-1} \beta_{t^*+s}^d$$ [7]

The term s refers to s^{th} year after the last pre-treatment year t^* and the term n refers to a parallel (n^{th}-order)-differences assumption that identifies $ATT'(s,n \mid Z_i)$. As discussed in the appendix, the parallel (n^{th}-order)-differences assumption can be mathematically written as:

$$(6) \qquad E[\Delta_s \Delta^{n-1} C_{i,t^*+s}^U \mid Z_i, d_i = 1] = E[\Delta_s \Delta^{n-1} C_{i,t^*+s}^U \mid Z_i, d_i = 0] \ \forall \ s \in [1, \ T(l) - t^* - 1].$$

See that, for $n = 1$ equation (6) reduces to a parallel paths assumption. For $n = 2$ equation (6) reduces to a parallel (1^{st}-order)differences or parallel growth assumption. The parallel growth assumption requires that the growth in corn acres between any two consecutive post-treatment years *minus* the growth between t^* and t^*-1 must equal among treatment and control groups, in the event of no treatment. Also, $ATT'(s,2 \mid Z_i)$ is similar to the Differences-in-Differences-in-Differences (DDD) estimator since we are comparing two-period differences in corn acres, rather than absolute acres, to compute treatment effects. For $n > 2$, we move onto higher order

[7] See Theorem 3 in Mora and Reggio (2013)

differences. For example, $n = 3$ implies a Δ^2 $(=(1-\mathrm{L})-(\mathrm{L}-\mathrm{L}^2))$ operator on s-periods ahead outcome variable in equations (5) and (6). It is clear that we require at least 3 pre-treatment years to estimate $ATT'(s,3|Z_i)$. In this sense, parallel (n^{th}-order) differences would require at least n pre-treatment periods and higher order generalizations ($n > 2$ cases) are only applicable to the North Dakota ethanol plants due to data availability. It is interesting to note that the treatment effects estimated using an exactly same model in equation (4) can be very different in size, sign and interpretation for different identifying assumptions. However, these assumptions can be tested for equivalence using the fully-flexible model discussed next. Testing the equivalence of parallel (n^{th}-order) differences assumption to a parallel ($n-1$ th-order))-differences assumption is similar to testing for the null hypothesis: $\Delta^{n-1}\beta^d_{t*} = 0$ such that $n < T(l) - t*$. To test whether the PPA holds we can simply test the hypothesis: $\beta^d_t = 0 \ \forall \ t \le t*$.

Availability of multiple pre-treatment years for the four North Dakota ethanol plants makes the fully-flexible DID model applicable to our study. However, an opportunity to implement multiple assumptions and estimating corresponding treatment effects for each case comes with a challenge of choosing among these estimates. We restrict our analysis to $n = 2$ for its amenable interpretation of comparing differences in growth of corn acres across treatment and control groups, rather than differences in absolute acres, to obtain $ATT'(s,2|Z_i)$. We will conduct a spatial placebo to validate our treatment estimates, discussed later.

Estimation Results

Estimation

The econometric considerations to estimate equation (4), and eventually $ATT'(s,2|Z_i)$, are discussed here. First, since North and South Dakotas are rural state, including lagged versions of the three major transitioning land use types other than corn: wheat, soy and grass potentially controls for autocorrelation in corn acres. This is because we assume that transitioning acres, due to rotations from soy to corn or conversions from wheat/grass to corn, remain approximately the same across time, i.e. $C_{i,t} + S_{i,t} + W_{i,t} + G_{i,t} \approx \text{a constant}$. Simultaneously, including lags of soy, wheat and grass, rather than that of corn, allows capturing differentiated costs of conversions, as pointed out earlier. Furthermore, including time dummies in the regression also potentially capture autocorrelation, at least partially, through covariances of respective coefficients. Second is the issue of heteroskedasticity. To compute heteroskedasticity-consistent standard errors across land parcels, we simply stratify our panel by choosing each land parcel as an individual cluster. This transforms the variance-covariance matrix into a block-diagonal with each clock referring to an individual land parcel i.

Next, for $n = 2$, the point estimate for the average treatment effects for the treated at each post-treatment period $t*+s$, based on the parallel growths assumption across treatment and control groups, is given as

$$
\begin{aligned}
ATT'(s,2\,|\,Z_i) = \Delta ATT(s\,|\,Z_i) &= \Delta_s(\beta^d_{t^*+s} - \beta^d_{t^*+s-1}) \\
&= (1-L^s)(\beta^d_{t^*+s} - \beta^d_{t^*+s-1}) \qquad (7) \\
&= (\beta^d_{t^*+s} - \beta^d_{t^*+s-1}) - (\beta^d_{t^*} - \beta^d_{t^*-1}).
\end{aligned}
$$

And, a sample estimate of the variance of this point estimate can be computed as follows:

$$
\begin{aligned}
Var(ATT'(s,2\,|\,Z_i)) &= Var((\beta^d_{t^*+s} - \beta^d_{t^*+s-1}) - (\beta^d_{t^*} - \beta^d_{t^*-1})) \\
&= Var(\beta^d_{t^*+s}) + Var(\beta^d_{t^*+s-1}) + Var(\beta^d_{t^*}) + Var(\beta^d_{t^*-1}) \qquad (8) \\
&\quad - 2\cdot Cov(\beta^d_{t^*+s}, \beta^d_{t^*+s-1}) - 2\cdot Cov(\beta^d_{t^*}, \beta^d_{t^*-1}) - 2\cdot Cov(\beta^d_{t^*+s}, \beta^d_{t^*}) \\
&\quad + 2\cdot Cov(\beta^d_{t^*+s}, \beta^d_{t^*-1}) + 2\cdot Cov(\beta^d_{t^*+s-1}, \beta^d_{t^*}) - 2\cdot Cov(\beta^d_{t^*+s-1}, \beta^d_{t^*-1}).
\end{aligned}
$$

We present the estimation results of equation (4) for each of the four ethanol plants in North Dakota (Table 6), along with that of $ATT'(s,2\,|\,Z_i)$ for all four ethanol plants (Table 7).

Results

As pointed out earlier, estimating of a fully-flexible DID model of equation (4) provides year-specific trend estimates for corn and year-specific treatment estimates unlike their counterparts based on aggregated pre- and post-treatment years under the PPA. However, in line with the results of the PPA (Tables L, N, P and R) the fully flexible model reveals differentiated opportunities for growing corn on land parcels previously planted with wheat and grass. We also include lagged soybean acres since these, along with lagged wheat and lagged grass acres potentially capture the temporal auto-correlation for corn acreage (the dependent variable). The estimates for these lagged variable in table 6 find the estimate for soybeans to be always positive, although significant for TE and HRE only, reflecting that corn and soy are often grown in rotations in the region. The negative and significant coefficients for $G_{i,t-1}$ in all cases suggests that grass acres inhibit conversions to corn, mainly due to high initial costs of land preparation from grass to agriculture. Lagged wheat acres, on the other hand, are found to be positive (insignificant) for BF and RTE as well as negative (significant) for TE and HRE. The opportunity of converting from wheat to corn is greater than grass to corn, as reflected by the respective coefficients in all but one case, due to significant differences in cost of conversion. While wheat presents an opportunity for conversion in terms of available acres in areas of little or no corn acres in the initial years (RTE and BF), it could also be costly venture in areas with relatively substantial initial corn acres (TE and HRE) due to local infrastructure-related reasons. The year-specific dummies are interestingly higher in the post-treatment years than the pre-treatment years. This implies that the role of trend-related effects alone on driving increased corn acres in areas proximate to the North Dakota ethanol plants has been significant. Finally, turning to the year-specific treatment estimates, through interaction of time dummies with the treatment dummy, we still find negative (but insignificant) coefficients for BF that are irreconcilable to economic incentives due to transportation costs and increased local corn basis. Since the assumption of parallel paths is formally rejected, i.e. $\beta^d_t \neq 0 \;\forall\; t \leq t^*$, the year-specific coefficients on our time dummies interacted with treatment do not identify the ATT. However, comparing the size, sign and significance of the time-specific coefficients, with and without interacting with the treatment dummy, across the four ethanol plants, it is clear from Table 6 that we are dealing with four different dynamic systems. Based on these findings, we still disagree with estimating a single point effect for many ethanol plants in a region, as usually reported in the literature.

To estimate the impact of ethanol plants, we compute $ATT'(s,2|Z_i)$ by comparing growth of corn acres among treatment control groups over time based on the parallel (2nd-order) difference assumption. It is interesting to note the while the treatment effects (, although not identified) based absolute acres were found be negative for three out of four ethanol plants in North Dakota, we find RTE and TE to increase growth in corn acres. It is further interesting to note that while HRE is found to be the only ethanol plant that caused the absolute corn acres in its locality to increase, its presence is now found to significantly decrease growth in corn acres, along with BF). Decrease in the growth of corn acres is too not supported by the economic incentives mentioned earlier. Therefore, we move onto testing the validity of these effects through a spatial placebo, discussed next.

A Spatial Placebo

Unlike the PPA, the flexible parallel $(n^{th}\text{-order})$-assumptions are designated for each post-treatment period, s. Note that this feature adds flexibility to the validity decision on the new DID estimates, in the sense that the assumption could hold only for a subset of these post-treatment periods and the treatment estimates for these cases will be fully valid. However, this same feature disallows utilizing using temporal placebos to test the validity of these estimates, as in the case of PPA. PPA allowed aggregating pre- and post-treatment years around any year when the treatment did not actually occur since this assumption is not specified to single time periods, but to the aggregated differences across time periods. Therefore, utilizing temporal placebos is unyielding in the case of a fully-flexible DID model since the conclusion of the validity of its assumption cannot be generalized to multiple time periods. For this reason we designate a spatial placebo (S.P.) that is a point coordinate in North Dakota and designate it as a false ethanol plant (or falsified treatment). After designating a spatial placebo, we repeat this study's procedure on this virtual ethanol plant designate by defining its treatment and control groups; matching their constituent 500-acre parcels based on the estimated propensity scores; and estimating equation (4) to eventually compute the new ATTs.

However, the choice of our S.P. is not completely arbitrary. We choose to locate our S.P. in Northeastern ND (figure 2) due to three major considerations. First, to avoid a competition in demand for corn from other ethanol plants, the nearest is Tharaldson Ethanol (approx. 300 km away). Second, we did not locate it in Northwestern ND, even though the region has no ethanol plant, to avoid competition for rails/roads infrastructure by the Bakken Shale industry in that region. Third, we choose the point coordinates such that our S.P. sits on ND State Highway 18, in line with the other ethanol plants that are usually situated on a major highway/railroad. After designating treatment and control groups for these ethanol plants we match them by estimating a probability of treatment for each constituent parcel by equation (3) and performing the nearest-neighbor matching algorithm by Fraeman (2010), discussed earlier. We find that weighted-LCC and weighted slope, in a quadratic functional form, are jointly significant in estimating the propensity score with lower LCC and higher slopes favoring treatment. A caliper of 0.01 yields a matched (and balanced) panel dataset, of 90 parcels in each treated and control category, with 180 observations and 17 years (1997-2013). The unmatched sample had 735 observations. We utilize this matched sample for S.P. and estimate equation (4) three times: separately for years 2006, 2007 and 2008 as treatment year designates. We estimate three separate models for these years due to the time period-specific identifying assumptions of the fully-flexible DID model (see equation (6)). So the estimates from year 2006 will correspond to TE as a placebo test; 2007 for RTE & BF; and 2008 for HRE. The corresponding estimation results, along with the $ATT'(s,2)$, are presented in Tables 10 and 11, respectively. Since a placebo is a false treatment, we expect a zero impact on corn acres due to S.P. Non-zero estimates will invalidate the identifying assumption of the new ATT's.

Estimates of our spatial placebos reveal that $ATT'(s,2)$ remains unidentified for treatment in *'2006'* and *'2008'*, but identified for treatment in *'2007'* (except for post-treatment years 2011 and 2013). Hence, we trust the identified treatment estimates for RTE and BF, but not for TE and HRE. Note that even the placebo estimates reveal a differentiated conversion opportunity from soy to corn, wheat to corn and grass to corn.

The unidentified estimates for $ATT'(s,2\,|\,Z_i)$ prompt testing for equivalence of parallel (3rd-order) differences assumption to parallel (2nd-order) differences assumption, and the equivalence of parallel (4th-order) differences assumption to parallel (3rd-order) differences assumption. The results are presented in Table 8. For the cases where parallel (3rd-order) differences assumption is not found to be equivalent to parallel (2nd-order) differences assumption, i.e. the TE and HRE ethanol plants, we present $ATT'(s,3\,|\,Z_i)$ to seek any differences in results (Table 9). $ATT'(s,3\,|\,Z_i)$ and its variance can be written as a function of the estimated coefficients of equation (4) as well, similar to that for $ATT'(s,2\,|\,Z_i)$:

(9) $$ATT'(s,3\,|\,Z_i) = (\beta^d_{t^*+s} - 2\cdot\beta^d_{t^*+s-1} + \beta^d_{t^*+s-2}) - (\beta^d_{t^*} - 2\cdot\beta^d_{t^*-1} + \beta^d_{t^*-2}).$$

(10)

$$\begin{aligned}
Var(ATT'(s,3\,|\,Z_i)) = {} & Var(\beta^d_{t^*+s}) + 4\cdot Var(\beta^d_{t^*+s-1}) + Var(\beta^d_{t^*+s-2}) \\
& + Var(\beta^d_{t^*}) + 4\cdot Var(\beta^d_{t^*-1}) + Var(\beta^d_{t^*-2}) \\
& - 4\cdot Cov(\beta^d_{t^*+s},\beta^d_{t^*+s-1}) + 2\cdot Cov(\beta^d_{t^*+s},\beta^d_{t^*+s-2}) - 4\cdot Cov(\beta^d_{t^*+s-1},\beta^d_{t^*+s-2}) \\
& - 4\cdot Cov(\beta^d_{t^*},\beta^d_{t^*-1}) + 2\cdot Cov(\beta^d_{t^*},\beta^d_{t^*-2}) - 4\cdot Cov(\beta^d_{t^*-1},\beta^d_{t^*-2}) \\
& - 2\cdot Cov(\beta^d_{t^*+s},\beta^d_{t^*}) + 4\cdot Cov(\beta^d_{t^*+s},\beta^d_{t^*-1}) - 2\cdot Cov(\beta^d_{t^*+s},\beta^d_{t^*-2}) \\
& + 4\cdot Cov(\beta^d_{t^*+s-1},\beta^d_{t^*}) - 8\cdot Cov(\beta^d_{t^*+s-1},\beta^d_{t^*-1}) + 4\cdot Cov(\beta^d_{t^*+s-1},\beta^d_{t^*-2}) \\
& - 2\cdot Cov(\beta^d_{t^*+s-2},\beta^d_{t^*}) + 4\cdot Cov(\beta^d_{t^*+s-2},\beta^d_{t^*-1}) - 2\cdot Cov(\beta^d_{t^*+s-2},\beta^d_{t^*-2}).
\end{aligned}$$

See that evaluating higher-order treatment effects for TE and HRE does not change the sign of estimates, however their interpretation changes to *rate of growth* in corn acres (and not *growth* in corn acres). However, our spatial placebo again invalidates the identifying parallel (3rd-order) difference assumption. This restricts us to rely on only RTE and BF to conclude about the role of ethanol plants in North Dakota. The growth statistics for corn acres due to HRE and BF do indicate a potential shift in agricultural systems due to these ethanol plants, but are in disagreement on the direction of shift. While HRE has caused a positive, insignificant growth in corn acres, BF is found to affect corn growth in a significantly negative manner. Negative growth in corn acres is still not supported by the economic incentives due to ethanol plants in North Dakota.

To investigate the negative impact of Blue Flint on growth of corn acres in its locality, we designate new treatment and control groups for this ethanol plants. The new treatment and control groups are designated in the east of the plant and on the east of the Missouri River. Re-conducting our analysis for BF with newly designated treatment & control groups also captures the sensitivity and robustness of our previous treatment estimates to treatment and control groups. The originally designated treatment & control groups lie southwards, to the west of the river providing access points for treatment and control groups in a uniform manner. Since the plant is located close to the river but slightly on its east, chances are that the closer proximity of the treatment groups may not be reflected by transportation costs, eventually rendering negative treatment effects. One more motivation for conducting this analysis on the east of the plant is that a new ethanol plant, Dakota Spirit AgEnergy (operationalized by the owner of BF, i.e. Midwest Ag Energy Group), began operations in June 2015 (see *http://www.midwestagenergygroup.com/dakota-spirit-agenergy*). This new ethanol plant is located approx. 200 km east of BF and 100 km west of TE. A linear city model of supply would suggest existence of a supply-demand gap on the east of BF, at least in the later years, and emergence of this new plant is a market outcome to bridge this gap. Out treatment effects will capture whether BF prompted an increase in corn acres, in its east. We estimate equation (4) for matched BF sample and the results are listed in Table 12 below.

The treatment estimates from the alternative treatment and control groups for the BF are in agreement with the treatment effects estimated earlier. Although, corn acreage on the east side of BF increased from 2008-2013, accelerating in 2012 and 2013, BF seems to have played a counter-productive role as far as corn acreage is concerned.

Discussion and Conclusions

This study attempts at identifying the role of ethanol plants in Dakotas' land use change. The rapid continual loss of original-mixed prairie poses many environmental, agronomic and economic concerns, as discussed in earlier sections. By evaluating the role of ethanol plants, we intended at contributing towards explaining recent land use changes in the Dakotas. We also seek policy implications from spatially-differentiated impacts of ethanol plants, in the sense whether the impact of ethanol plants vary by virtue of their spatial locations. We develop a unique research design that uses a quasi-experimental framework to evaluate ethanol plant impacts on local land use utilizing a spatially delineated dataset. We extensively use remote-sensing tools to construct such a dataset for the purpose of this study.

This analysis treats ethanol plants as treatments and uses DID in conjugation with propensity score matching to evaluate treatment effects due to these ethanol plants. The feature of matching land parcels based on their soil characteristics brings strength to our analysis, and differentiates it from other studies, by seeking to eliminate any differences among treated and untreated parcels other than the treatment itself. Our analysis progresses upon the preliminary results of the standard DID model that found negative treatment effects for ethanol plants. This result is both surprising and irreconcilable to the economic incentives due to transportation costs and local corn basis calculated for this analysis. Investigating further, we invalidate the parallel paths assumption of the DID. That is, the estimates of the standard DID model are not identified for this study.

To tackle this, we incorporate flexible-trends into the standard DID model following a previous working paper that developed theoretical econometric properties of the estimators of this model. This model allows computing treatment effects in various forms along with change in absolute outcome levels, like change in growth of corn acres, change in rate of growth of corn acres, so on. Also, the treatment effects are estimated for each post-treatment year that allows evaluating temporally-differentiated impacts for each ethanol plant. Our application of this model yields negative treatment effects for Blue Flint ethanol plant in North Dakota. The treatment estimates are also not identified for all ethanol plants either. Upon utilizing a carefully designated placebo and various other robustness checks, we find identifying treatment effects at a very local level challenging and remain inconclusive about the role of ethanol plants in land use changes across Dakotas. However, our analysis disagrees with a single point estimate for impact of ethanol plants at a regional level. Our analysis does indicate that ethanol plants located in different environments (for example, corn intensive vs. non-intensive regions) can affect land use in a different manner (for example, dominating or dominated by local trends). Our analysis also reflects differentiated opportunity costs of conversion from wheat to corn and grass to corn.

Finally, this study contributes by developing and applying a mechanism that tests the basis identifying assumption of the DID and paves a path for analysis even if that assumption fails.

Ideas for future research in this area

This article provides a novel research design that incorporates remotely sensed data into applied economic analyses, especially those under the ambit of quasi-experimental studies. However, our design has its own shortcomings that provide opportunities for future research. First, we use Euclidean distances rather than 'actual' distances of land parcels from ethanol plants. These 'actual' distances using local road networks provided by the state Departments of Transportation can be incorporated using the 'Nearest Facility Analysis' tool on ArcGIS. Second, our research design uses ad-hoc treatment and control groups and our placebo tests suggest that our matching strategies were not perfect. In some cases, the treated parcels had lower rates of growth in corn acres than their untreated parcels. This observation leads to a question that why would the ethanol plants, in the first place, locate in regions where growth in corn production is initially lower. This is possible due to multiple reasons. The location of ethanol plants would definitely depend on ex-ante land use patterns in its proximity, but that is not the only factor that it considers. These plants would also want to consider areas with good public infrastructure, easy access to grain elevators and other market terminals while making location decisions. In any case, such results can provide a springboard for researchers whose interest lies with understanding the effects of ethanol plants on the socio-economic environment in its proximity.

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TABLES

Table 1: List of Ethanol Plants in North Dakota and South Dakota for our analysis

S. No.	Ethanol Plant	Year Established	Capacity (Million gallons per year)	Location
North Dakota				
1	Red Trail Energy	2007	50	Richardton, Stark County
2	Blue Flint Ethanol	2007	65	Underwood, McLean County
3	Tharaldson Ethanol LLC	2006	153	Casselton, Cass County
4	Hankinson Renewable Energy	2008	145	Hankinson, Richland County
South Dakota				
1	POET Bio refinery (POET)	2008	110	Chancellor, Turner County
2	NuGen Energy (NuGen)	2008	100	Marion, Turner County
3	Advanced Bio Energy (ABE)	2008	53	Aberdeen, Brown County
4	Glacial Lakes Energy (GLE)	2008	100	Mina, Edmunds County

Table 2: Schematics of the treatment and control groups of ethanol plants analyzed in this study.

Ethanol Plant	**T1**	**T2**	**C1**	**C2**
RTE	5km-30km South	15km-40km South	50km-74km South	76km-100km South
BF	5km-30km South	15km-40km South	50km-74km South	76km-100km South
TE	5km-30km West	15km-40km West	50km-74km West	76km-100km West
HRE	5km-30km West	15km-40km West	50km-74km West	76km-100km West
POET & NuGen	5km-30km West of POET*	30km-55km West of POET*	70km-94km West of POET*	96km-120km West of POET*
ABE & GLE	5km-30km West of ABE*	30km-55km West of ABE*	70km-94km West of ABE*	96km-120km West of ABE*
Spatial Placebo	5km-30km South	15km-40km South	50km-74km South	76km-100km South

* GLE lies ~30 km west of ABE – the location of T & C groups can be visualized accordingly.

Notes on Planar Dimensions of our Treatment and Control Rectangles (Part of Table 2):

- Red Trail Energy & Blue Flint Ethanol: **25 km N-S X 50 km E-W**.
- Tharaldson Ethanol: **25 km E-W X 50 km N-S**.
- Hankinson Renewable Energy: **25 km E-W X 40 km N-S**. North Dakota State Boundary is located 15 km south of this ethanol plant. So the N-S dimensions are chosen to be: 30 km N to 10 km S of the ethanol plant, resulting in length of one side of the rectangles be 40 km (N-S).
- Cluster (POET and NuGen): 25 km E-W X 40 km N-S RECTANGLES (25 km N + 15 km S). The rectangles included exclude a circle of radius 2.5 km from NuGen, to avoid permanent development in land use characterization.
- Cluster (ABE and GLE): 25 km E-W X 50 km N-S RECTANGLES. The rectangles here exclude a circle of radius 7 km from GLE to avoid a big water pond in land use characterization.
- Spatial Placebo: **25 km N-S X 30 km E-W**

Table 3: Placebo Estimates with 'Logarithm of CS' as dependent variable

	Red Trail Energy	Blue Flint Ethanol	Tharaldson Ethanol	Hankinson Renewable Energy
F.T. – 1 (2000)	*-1.63***	*1.09****	*-1.27****	*-0.29****
ACTUAL TREATMENT	***-0.28***	***-0.50*****	***-0.54******	***0.09***
F.T. – 2 (2011)	*0.21*	*0.32*	*-0.14****	*-0.46***

* $p<0.1$; ** $p<0.05$; *** $p<0.0$

Table 4: Propensity Score Estimation using Logit regressions. Dependent Variable: $P(d_i = 1)$.

Variable	RTE	BF	TE	HRE	ABGL	PBNE	S.P.
Intercept	24.42** (3.52)	1.60** (0.48)	14.48 (9.64)	9.99** (1.70)	-59.32** (5.41)	11.66** (1.09)	56.41*** (10.51)
WLCC	-40.18** (3.10)	0.63* (0.35)	-12.25 (7.76)	-2.61** (1.05)	11.65** (1.70)	-5.33** (0.98)	-44.23*** (8.44)
$WLCC^2$	7.53** (0.61)	-0.11** (0.04)	2.38 (1.60)	0.33* (0.18)	-2.01** (0.31)	0.79** (0.23)	-8.60*** (1.70)
WSLP	6.52** (0.48)	-0.31** (0.10)	6.20** (2.39)	-2.77** (0.31)	30.71** (4.23)	-2.63** (0.44)	2.26 (3.00)
$WSLP^2$	-0.40** (0.03)	0.01** (0.005)	-1.95** (0.43)	2.88** (0.38)	-5.29** (0.76)	0.33** (0.05)	-1.50* (0.80)
AIC	946	1222	709	1211	991	977	582
SC	972	1246	734	1235	1016	1002	605
-2 Log L	936	1212	699	1201	981	967	572

** means significant at 95% C.I. * means significant at 90% C.I. Standard error in parentheses.

Table 5: Matching Performance.

H_o^1**:** Means of variable X_i^a are statistically equal across groups (t-test).

H_o^2**:** Variances of variable X_i^a are statistically equal across groups (F-test).

Ethanol Plant	Sample Size		Caliper	X_i^a	Mean		H_o^1 p-value	Variance		H_o^2 p-value
	Pre-Match	Post-Match			T	C		T	C	
RTE	1224	130	0.0004	*WLCC*	2.36	2.30	0.42	0.15	0.14	0.66
				WSLP	7.92	7.46	0.11	2.72	2.62	0.87
BF	1012	548	0.01	*WLCC*	3.77	3.68	0.57	2.82	3.20	0.28
				WSLP	9.77	9.73	0.93	16.97	18.84	0.42
TE	1155	240	0.01	*WLCC*	2.09	2.07	0.48	0.05	0.04	0.21
				WSLP	2.83	2.83	0.98	0.02	0.03	0.06
HRE	980	322	0.005	*WLCC*	2.97	2.88	0.34	0.69	0.72	0.77

				WSLP	3.03	3.14	0.39	1.21	1.32	0.54
ABGL	1118	200	0.0005	*WLCC*	2.04	2.06	0.57	0.12	0.08	0.09
				WSLP	3.17	3.24	0.20	0.18	0.14	0.28
PBNE	971	314	0.005	*WLCC*	2.04	2.06	0.57	0.12	0.08	0.10
				WSLP	3.17	3.24	0.20	0.18	0.14	0.27
S.P.	735	180	0.01	*WLCC*	2.22	2.23	0.85	0.14	0.13	0.81
				WSLP	1.92	1.89	0.62	0.13	0.15	0.60

Table 6: Estimates of the fully-flexible DID model. Dependent Variable: $C_{i,t}$

Variable	RTE	BF	TE	HRE
Intercept	8.62***	33.05***	-24.60***	77.38***
$W_{i,t-1}$	0.00	0.00	-0.02	-0.36***
$S_{i,t-1}$	0.41	0.09	0.17***	0.25***
$G_{i,t-1}$	-0.02***	-0.07***	-0.09***	-0.27***
d_i	-0.20	-7.14***	11.84***	-28.52***
$I_{[t=1998]} \times d_i$	-0.07	12.75***	-9.48	10.50
$I_{[t=1999]} \times d_i$	1.65	12.77***	-7.76	44.58***
$I_{[t=2000]} \times d_i$	1.36	-0.88	-33.15***	-26.55***
$I_{[t=2001]} \times d_i$	-2.42	12.40***	-34.66***	6.32
$I_{[t=2002]} \times d_i$	-4.32**	3.13	-33.29***	10.78
$I_{[t=2003]} \times d_i$	-0.58	8.67***	-33.10***	-29.81***
$I_{[t=2004]} \times d_i$	-5.38	4.23***	38.58***	30.58***
$I_{[t=2005]} \times d_i$	-0.19	6.90***	1.23	68.96***
$I_{[t=2006]} \times d_i$	0.66	12.89***	--	2.41
$I_{[t=2007]} \times d_i$	--	--	24.26**	23.60**
$I_{[t=2008]} \times d_i$	-2.21	2.53	-1.42	--
$I_{[t=2009]} \times d_i$	0.54	3.78	29.33***	41.69***
$I_{[t=2010]} \times d_i$	3.64	1.71	22.71*	26.83***
$I_{[t=2011]} \times d_i$	8.93*	-1.81	20.85*	14.32
$I_{[t=2012]} \times d_i$	14.01	-1.80	46.06***	22.11**
$I_{[t=2013]} \times d_i$	29.87***	-5.93	56.18***	27.10**
$I_{[t=1998]}$	-4.28**	-12.78***	42.06***	90.67***
$I_{[t=1999]}$	-3.03***	-10.55***	37.40***	37.48***
$I_{[t=2000]}$	0.80	-3.23**	49.31***	81.09***
$I_{[t=2001]}$	-1.82	-11.34***	51.44***	43.44***
$I_{[t=2002]}$	-0.47	-3.70***	44.04***	24.51***
$I_{[t=2003]}$	-4.53***	-18.59***	40.26***	53.81***

$I_{[t=2004]}$	5.72**	-1.03	38.03***	60.47***
$I_{[t=2005]}$	-2.86***	-4.23***	34.12***	1.76
$I_{[t=2006]}$	3.33**	-3.19**	--	51.65***
$I_{[t=2007]}$	--	--	84.25***	80.39***
$I_{[t=2008]}$	3.05**	8.45***	98.26***	--
$I_{[t=2009]}$	6.45**	8.95***	62.18***	44.99***
$I_{[t=2010]}$	2.30	6.99***	68.43***	29.51***
$I_{[t=2011]}$	3.43**	16.93***	55.92***	84.62***
$I_{[t=2012]}$	20.43***	27.17***	113.28***	80.72***
$I_{[t=2013]}$	6.11*	31.02***	111.97***	89.84***
R^2	0.16	0.20	0.41	0.32

* p<0.1; ** p<0.05; *** p<0.01; -- signifies year of emergence for respective ethanol plant.

Table 7: $ATT'(s,2|Z_i)$ for the four ND Ethanol Plants.

Ethanol Plant (Year Established)	Red Trail E. (2007)	Blue Flint (2007)	Tharaldson E. (2006)	Hankinson E. (2008)
2007	-	-	60.38***	-
2008	-3.73	-16.35***	11.67	-
2009	1.91	-4.74	68.10***	-3.09
2010	2.25	-8.06***	30.73*	-36.05***
2011	4.44	-9.51***	35.50*	-33.70***
2012	4.23	-5.97	62.56**	-13.39
2013	15.01	-10.12**	47.46**	-16.19

* p<0.1; ** p<0.05; *** p<0.01

Table 8: T-statistic: Testing the equivalence of n^{th}-order and $(n-1)^{th}$-order assumptions.

n	H_0	Red Trail E. (2007)	Blue Flint (2007)	Tharaldson E. (2006)	Hankinson E. (2008)
3	$\Delta^2\beta_{t*}^d = 0$	-4.35	3.32	-109.03***	87.74***
4	$\Delta^3\beta_{t*}^d = 0$	-14	-3.79	-180.51***	192.68***
5	$\Delta^4\beta_{t*}^d = 0$			-253.168***	275.61***

* p<0.1; ** p<0.05; *** p<0.01

Table 9: Estimate of $ATT'(s,3|Z_i)$ where equivalence assumptions failed (Table 8).

Ethanol Plant (Year Established)	Tharaldson E. (2006)	Hankinson E. (2008)
2007	169.40***	-
2008	60.32***	-
2009	165.46***	-90.83***
2010	71.65***	-120.70***
2011	113.80***	-85.39***
2012	136.09***	-67.43***
2013	93.93***	-90.54***

* p<0.1; ** p<0.05; *** p<0.01

Table 10: Estimation of the fully-flexible DID model for our spatial placebo. Dependent Var. $C_{i,t}$.

Variable	Treatment = '*2006*'	Treatment = '*2007*'	Treatment = '*2008*'
Intercept	10.57**	11.69**	10.33**
$W_{i,t-1}$	-0.05**	-0.05***	-0.04**
$S_{i,t-1}$	0.11***	0.09***	0.10***
$G_{i,t-1}$	-0.10***	-0.09***	-0.09***
d_i	-2.70	-3.07	-3.13
$I_{[t=1998]} \times d_i$	7.88	7.35	7.72
$I_{[t=1999]} \times d_i$	1.40	0.07	0.81
$I_{[t=2000]} \times d_i$	-1.10	-1.21	-1.17
$I_{[t=2001]} \times d_i$	21.31***	18.50***	19.83***
$I_{[t=2002]} \times d_i$	11.10**	10.56**	10.77**
$I_{[t=2003]} \times d_i$	-9.03*	-9.34*	-8.41
$I_{[t=2004]} \times d_i$	-46.05***	-44.85***	-44.05***
$I_{[t=2005]} \times d_i$	23.47***	22.43***	23.32***
$I_{[t=2006]} \times d_i$	--	-0.39	0.23
$I_{[t=2007]} \times d_i$	-48.70***	--	-48.58***
$I_{[t=2008]} \times d_i$	-36.38***	-36.51***	--
$I_{[t=2009]} \times d_i$	-46.32***	-46.40***	-46.27***
$I_{[t=2010]} \times d_i$	-59.14***	-59.50***	-59.45***
$I_{[t=2011]} \times d_i$	-42.83***	-43.20***	-43.11***
$I_{[t=2012]} \times d_i$	-81.92***	-82.24***	-81.78***

$I_{[t=2013]} \times d_i$	-52.30***	-52.52***	-52.27***
$I_{[t=1998]}$	20.53***	19.89***	19.54***
$I_{[t=1999]}$	11.12**	10.32**	9.87**
$I_{[t=2000]}$	13.91***	12.25**	12.22**
$I_{[t=2001]}$	-4.54	-4.22	-5.31
$I_{[t=2002]}$	5.18	4.43	4.21
$I_{[t=2003]}$	10.87**	10.36**	9.55*
$I_{[t=2004]}$	37.67	37.05***	36.09***
$I_{[t=2005]}$	6.78***	7.46*	6.32
$I_{[t=2006]}$	--	24.91***	24.15***
$I_{[t=2007]}$	86.02***	--	84.73***
$I_{[t=2008]}$	79.38***	78.43***	--
$I_{[t=2009]}$	84.32***	83.63***	83.46***
$I_{[t=2010]}$	76.53***	76.28***	76.13***
$I_{[t=2011]}$	65.38***	64.74***	64.45***
$I_{[t=2012]}$	104.64***	105.19***	104.45***
$I_{[t=2013]}$	64.21***	64.51***	64.14***
R^2	0.32	0.31	0.32

* p<0.1; ** p<0.05; *** p<0.01; -- signifies year of emergence for respective ethanol plant.

Table 11: Estimate of $ATT'(s,2)$ for our spatial placebo.

Ethanol Plant (Year Established)	Treatment = *'2006'*	Treatment = *'2007'*	Treatment = *'2008'*
2007	-141.69***	-	-
2008	-57.21***	-13.31	-
2009	-79.46***	12.93	51.12***
2010	-82.35***	9.72	35.63**
2011	-53.21***	39.11***	65.16***
2012	-108.62***	-16.22	10.14
2013	-39.90***	52.54***	78.32***

* p<0.1; ** p<0.05; *** p<0.01

Table 12: Estimation of the fully-flexible DID model for ***eastern treatment & control groups*** of the BF. Dependent Variable $C_{i,t}$.

Variable	BF (2007)
Intercept	1.82
$W_{i,t-1}$	0.04***
$S_{i,t-1}$	0.12***
$G_{i,t-1}$	-0.03***
d_i	1.00
$I_{[t=1998]} \times d_i$	2.62
$I_{[t=1999]} \times d_i$	-0.32
$I_{[t=2000]} \times d_i$	-2.63
$I_{[t=2001]} \times d_i$	-2.66
$I_{[t=2002]} \times d_i$	-3.49
$I_{[t=2003]} \times d_i$	-4.04**
$I_{[t=2004]} \times d_i$	4.10*
$I_{[t=2005]} \times d_i$	-6.73*
$I_{[t=2006]} \times d_i$	-0.17
$I_{[t=2007]} \times d_i$	--
$I_{[t=2008]} \times d_i$	-8.97*
$I_{[t=2009]} \times d_i$	-11.92**
$I_{[t=2010]} \times d_i$	-2.69
$I_{[t=2011]} \times d_i$	-3.85
$I_{[t=2012]} \times d_i$	-30.47***
$I_{[t=2013]} \times d_i$	-9.99
$I_{[t=1998]}$	-0.96
$I_{[t=1999]}$	3.05*
$I_{[t=2000]}$	4.23**
$I_{[t=2001]}$	4.45*
$I_{[t=2002]}$	4.65**
$I_{[t=2003]}$	1.81
$I_{[t=2004]}$	1.76
$I_{[t=2005]}$	9.07**

$I_{[t=2006]}$	4.48*
$I_{[t=2007]}$	--
$I_{[t=2008]}$	18.75***
$I_{[t=2009]}$	18.91***
$I_{[t=2010]}$	9.22**
$I_{[t=2011]}$	12.89***
$I_{[t=2012]}$	43.01***
$I_{[t=2013]}$	32.19***
R^2	0.20

* p<0.1; ** p<0.05; *** p<0.01; -- signifies year of emergence for respective ethanol plant.

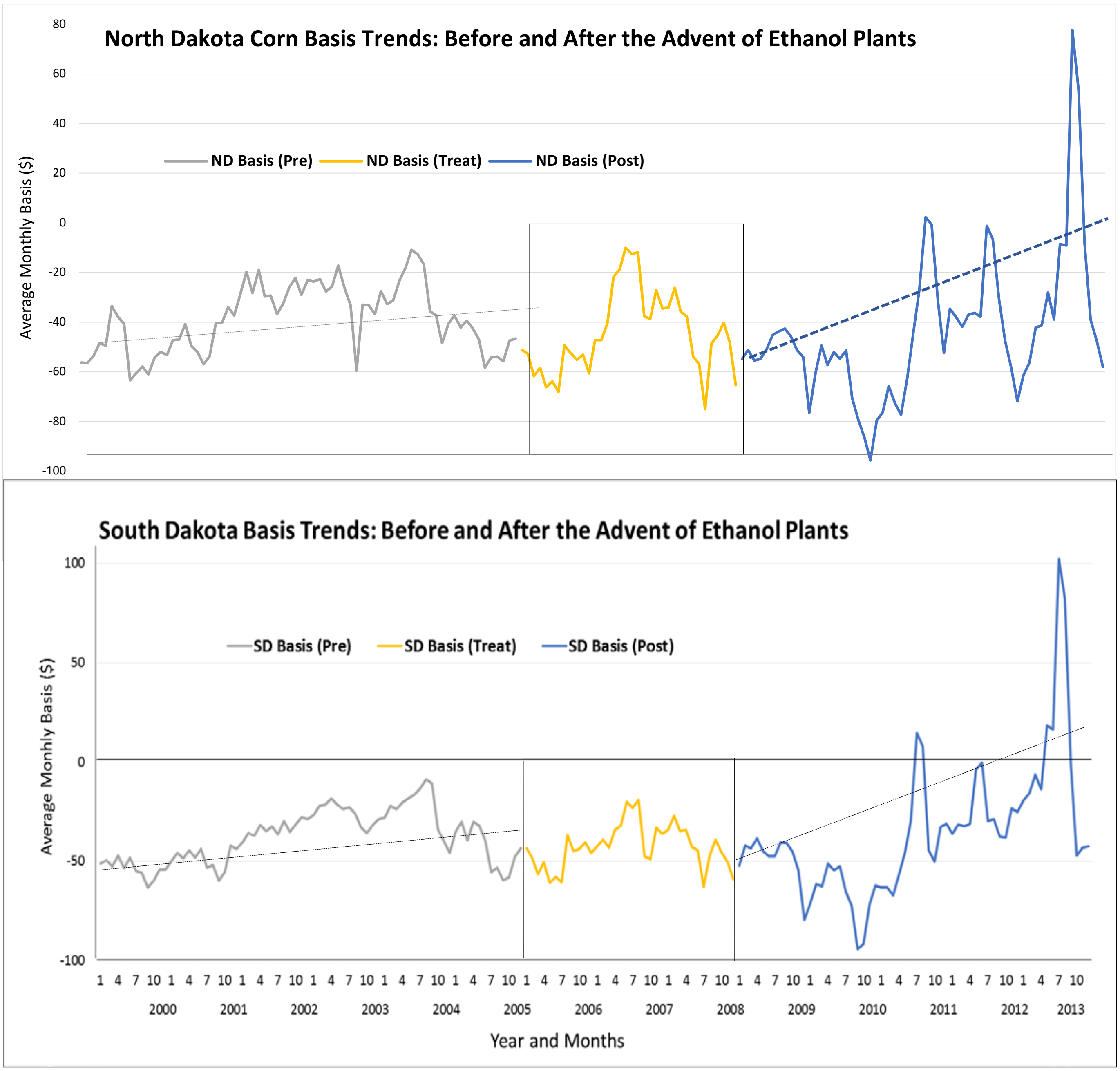
Table 13: Treatment estimates for the ***eastern treatment & control groups*** of the BF.

Ethanol Plant (Year Established)	$ATT'(s,2)$	$ATT'(s,3)$
2008	-15.35***	-32.73***
2009	-9.51	-11.54
2010	2.68	-5.20
2011	-7.72	-27.78*
2012	-33.17***	-42.83***
2013	13.92	29.71

* p<0.1; ** p<0.05; *** p<0.01

FIGURES

Figure1: Comparative Corn Basis Trends for Counties that House Dakotas' Ethanol Plants that started operations in the 2006-2008 period. The acronym 'treat' denotes the period when these ethanol plants started operations, 'pre' ('post') means years prior to (after) the 2006-2008 period.



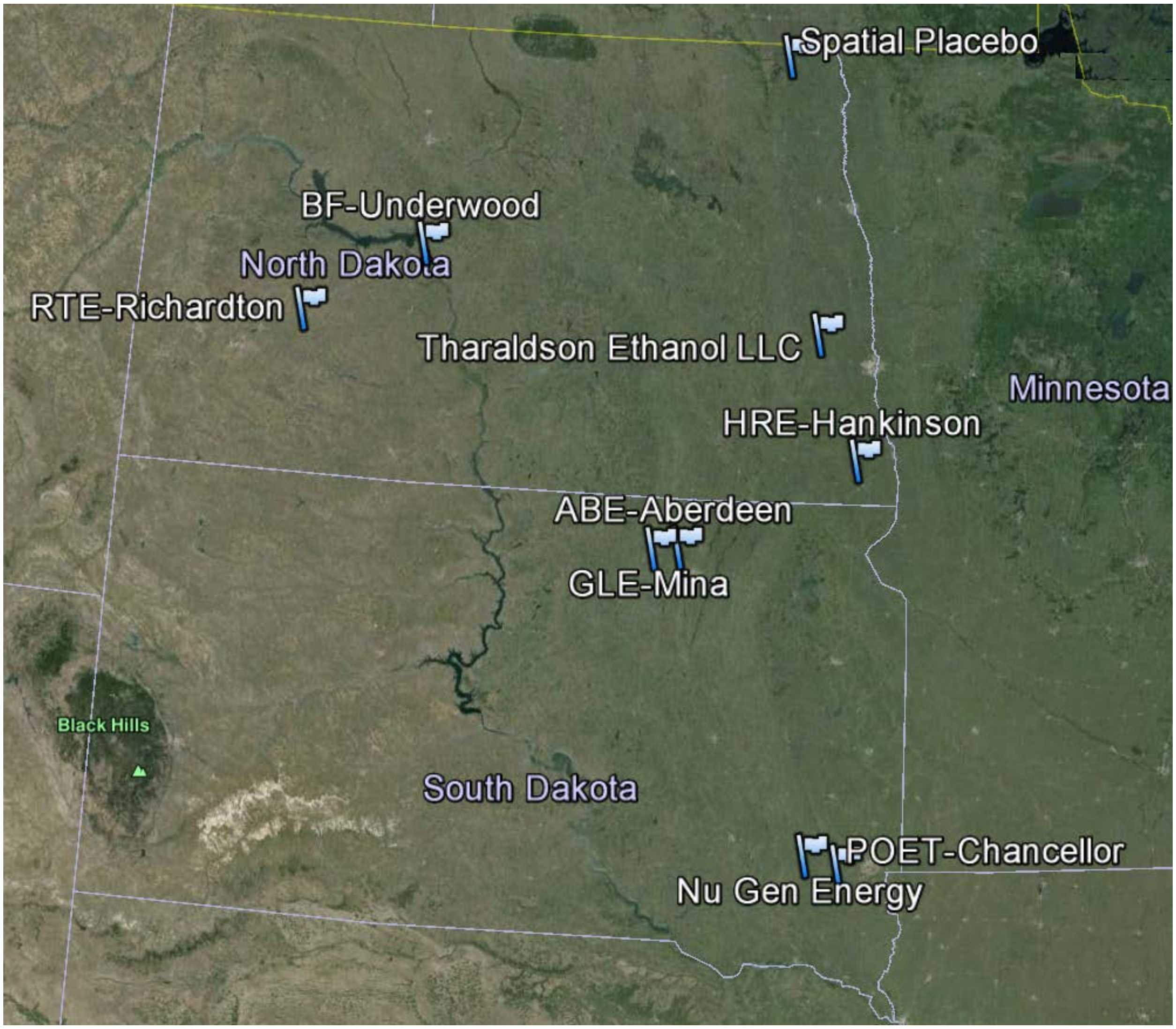
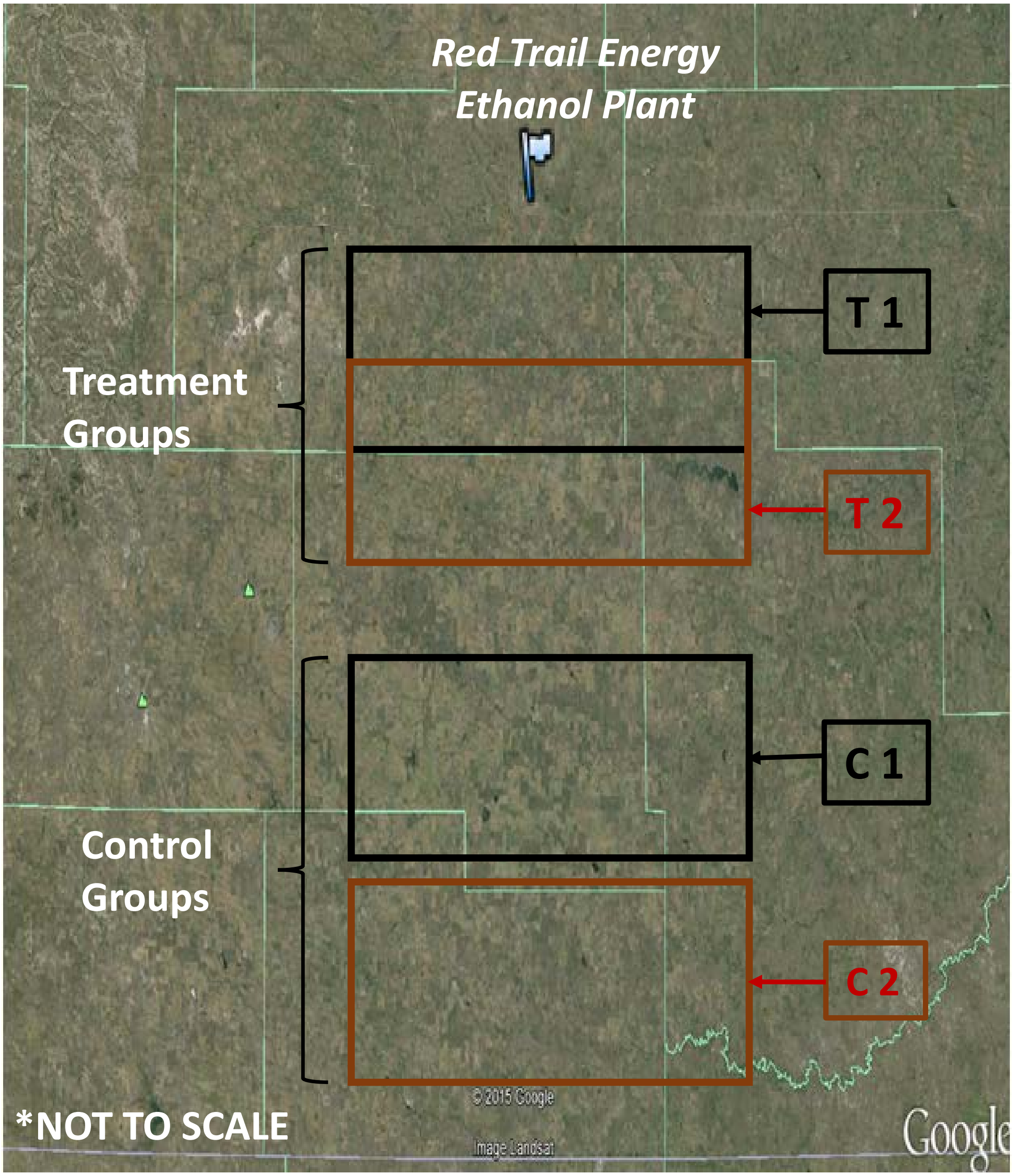


Figure 2: Spatial locations of the 8 ethanol plants included in this analysis

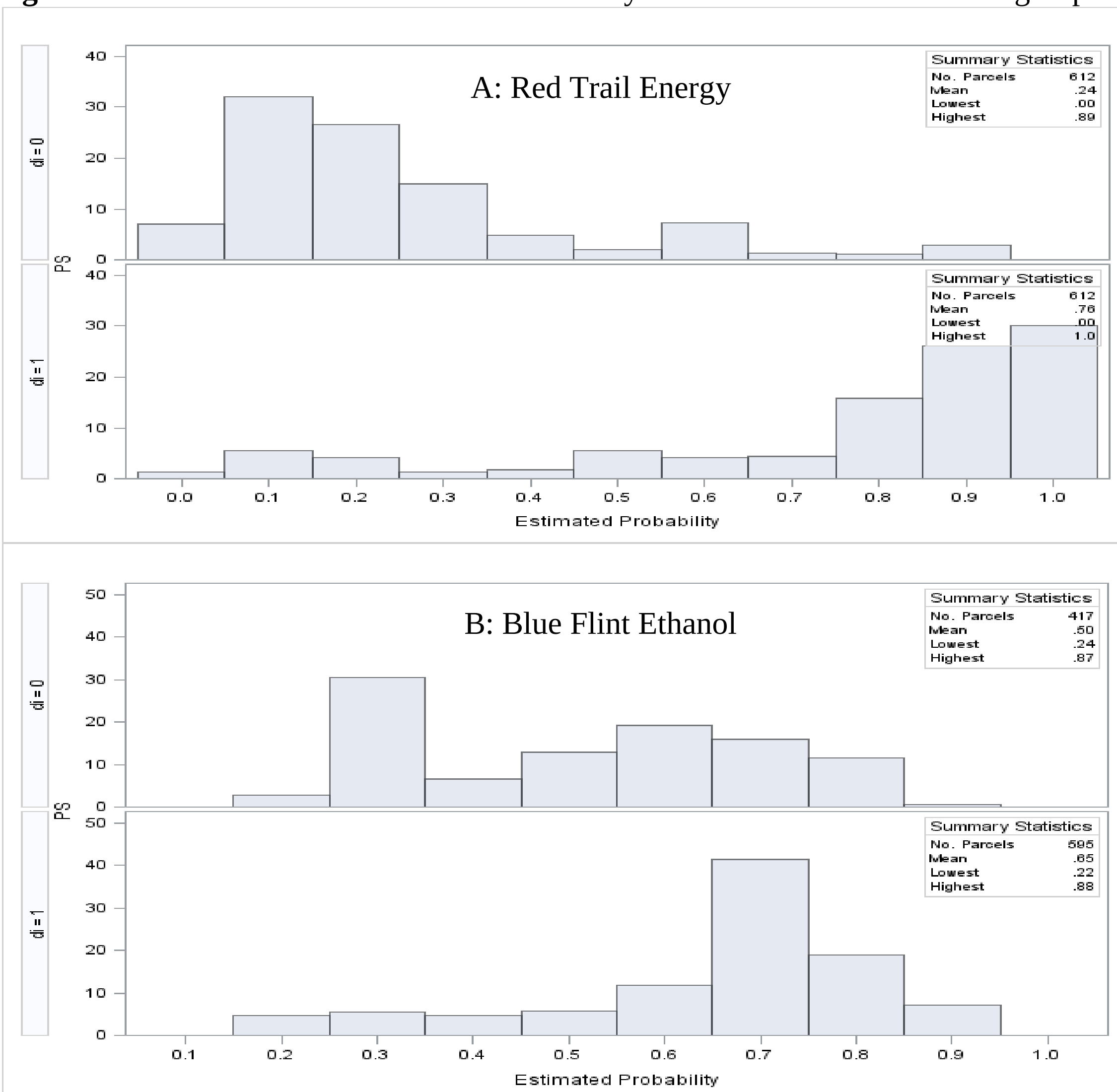
Source: "North and South Dakota." 5122554.70 m N and 393724.99 m E. **Google Earth.** April 9, 2013. August 8, 2015.

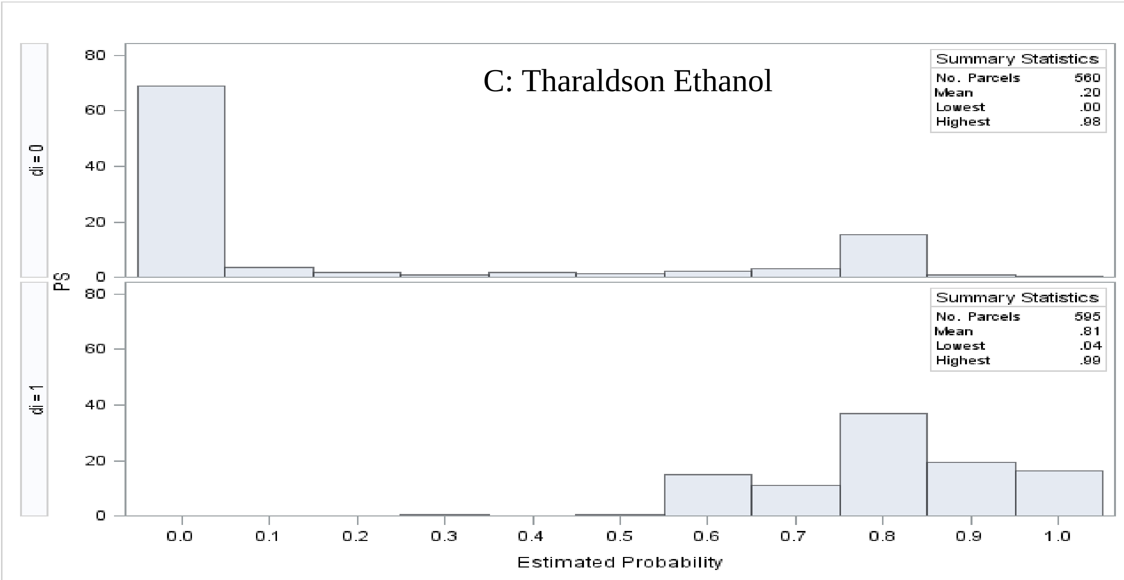
Figure 3: Schematics of treatment and control group: An Example

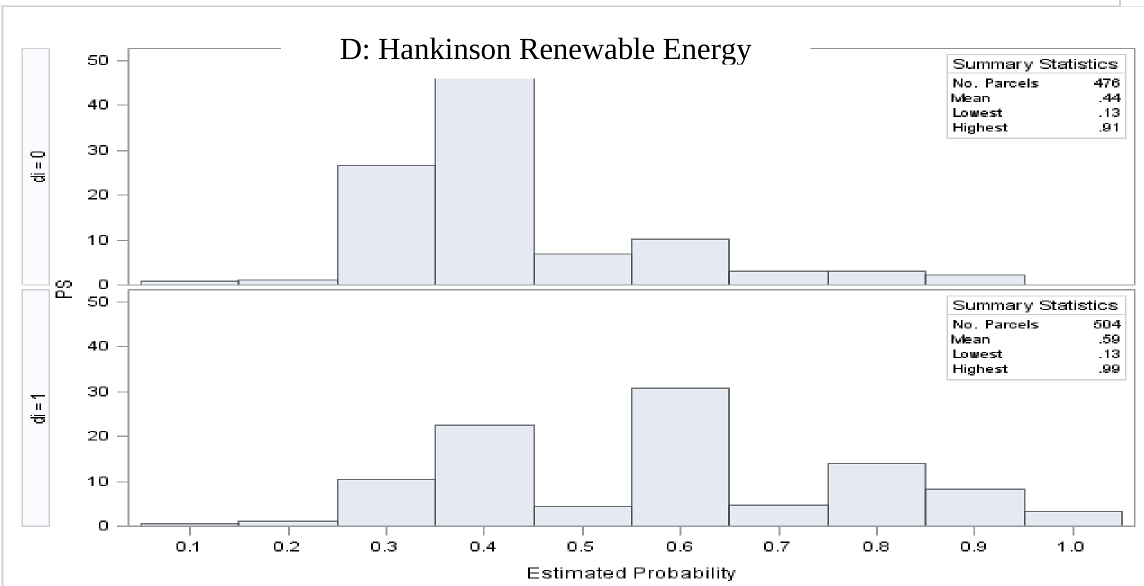


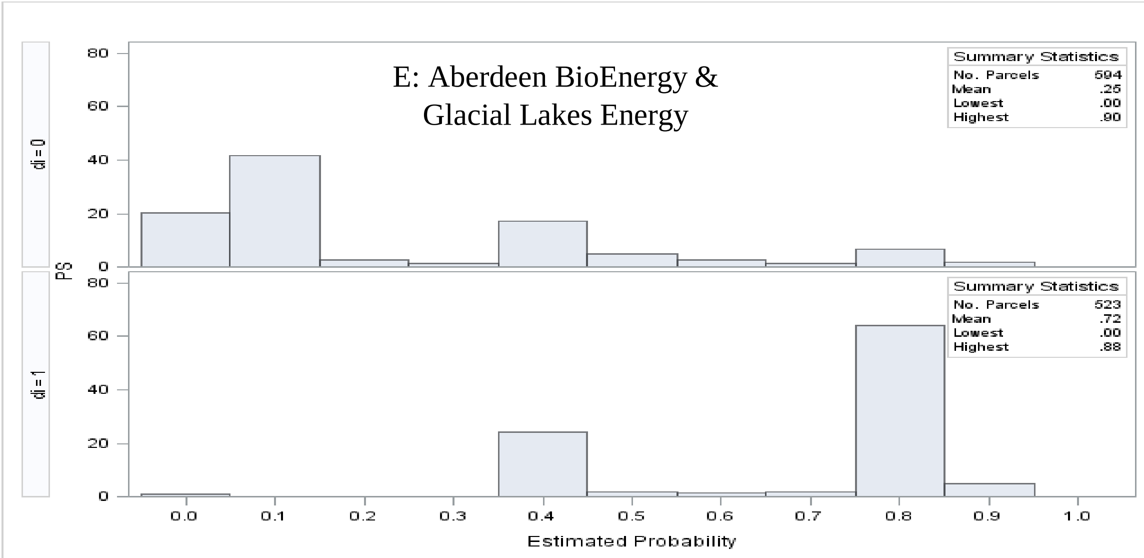
Source: “North Dakota” 33704.21m E, 5249274.59m N. Google Earth. April 9, 2014. October 20, 2014.

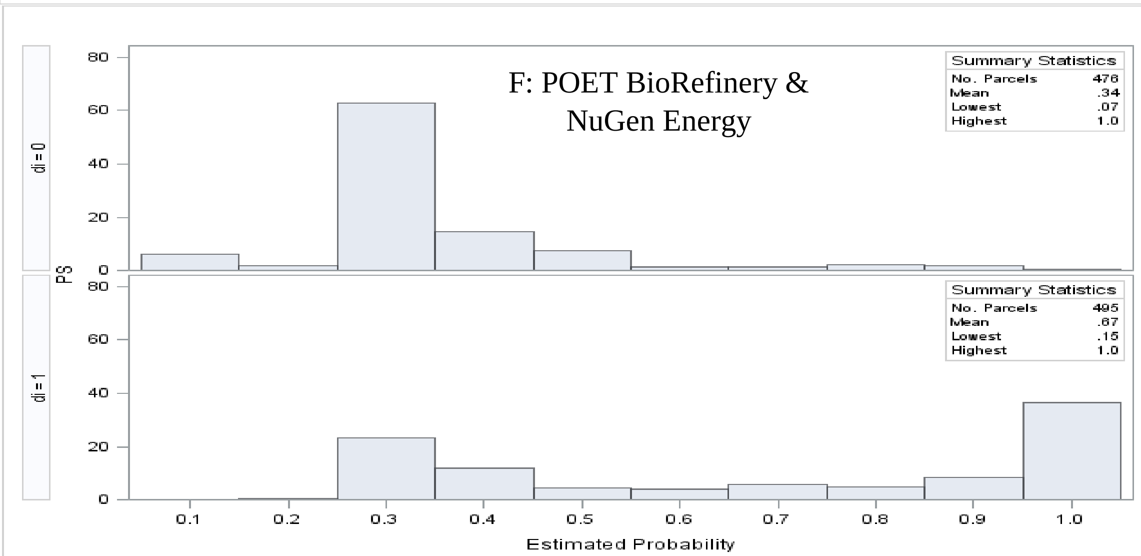
Figures 4-9: Distribution of Treatment Probability across treatment and control groups.




C: Tharaldson Ethanol
Summary Statistics
No. Parcels 560
Mean .20
Lowest .00
Highest .98
Summary Statistics
No. Parcels 595
Mean .81
Lowest .04
Highest .99
di = 0
di = 1
PS
Estimated Probability


D: Hankinson Renewable Energy
Summary Statistics
No. Parcels 476
Mean .44
Lowest .13
Highest .91
Summary Statistics
No. Parcels 504
Mean .59
Lowest .13
Highest .99
di = 0
di = 1
PS
Estimated Probability


E: Aberdeen BioEnergy &
Glacial Lakes Energy
Summary Statistics
No. Parcels 594
Mean .25
Lowest .00
Highest .90
Summary Statistics
No. Parcels 523
Mean .72
Lowest .00
Highest .88
di = 0
di = 1
PS
Estimated Probability


F: POET BioRefinery &
NuGen Energy
Summary Statistics
No. Parcels 476
Mean .34
Lowest .07
Highest 1.0
Summary Statistics
No. Parcels 495
Mean .67
Lowest .15
Highest 1.0
di = 0
di = 1
PS
Estimated Probability

Intentionally Left Blank.

Figure 10: Temporal Placebo Schematics: Validating the estimates from the standard DID model.

Moving Away from the Parallel Paths Assumption

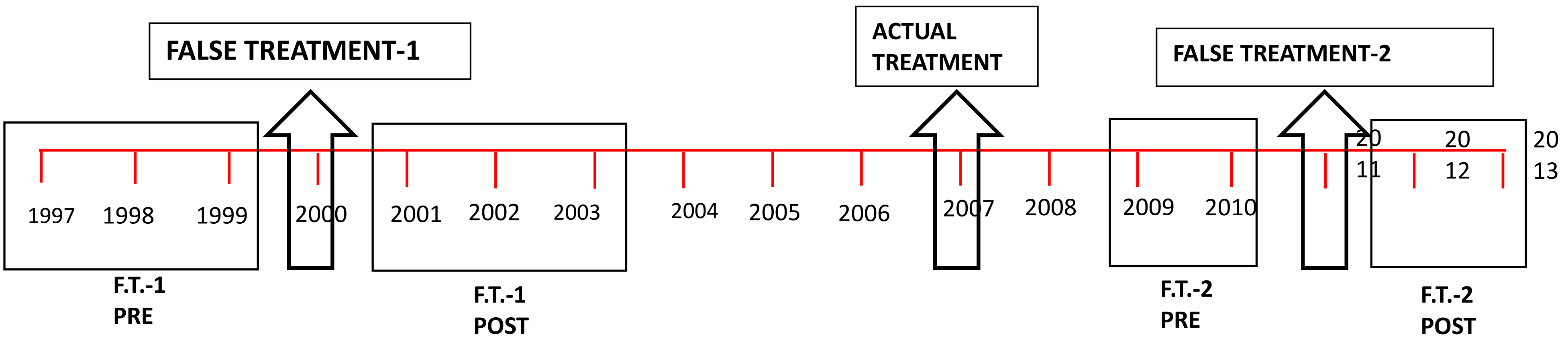


Figure 11: Pre-Treatment Trends for North Dakota Ethanol Plants

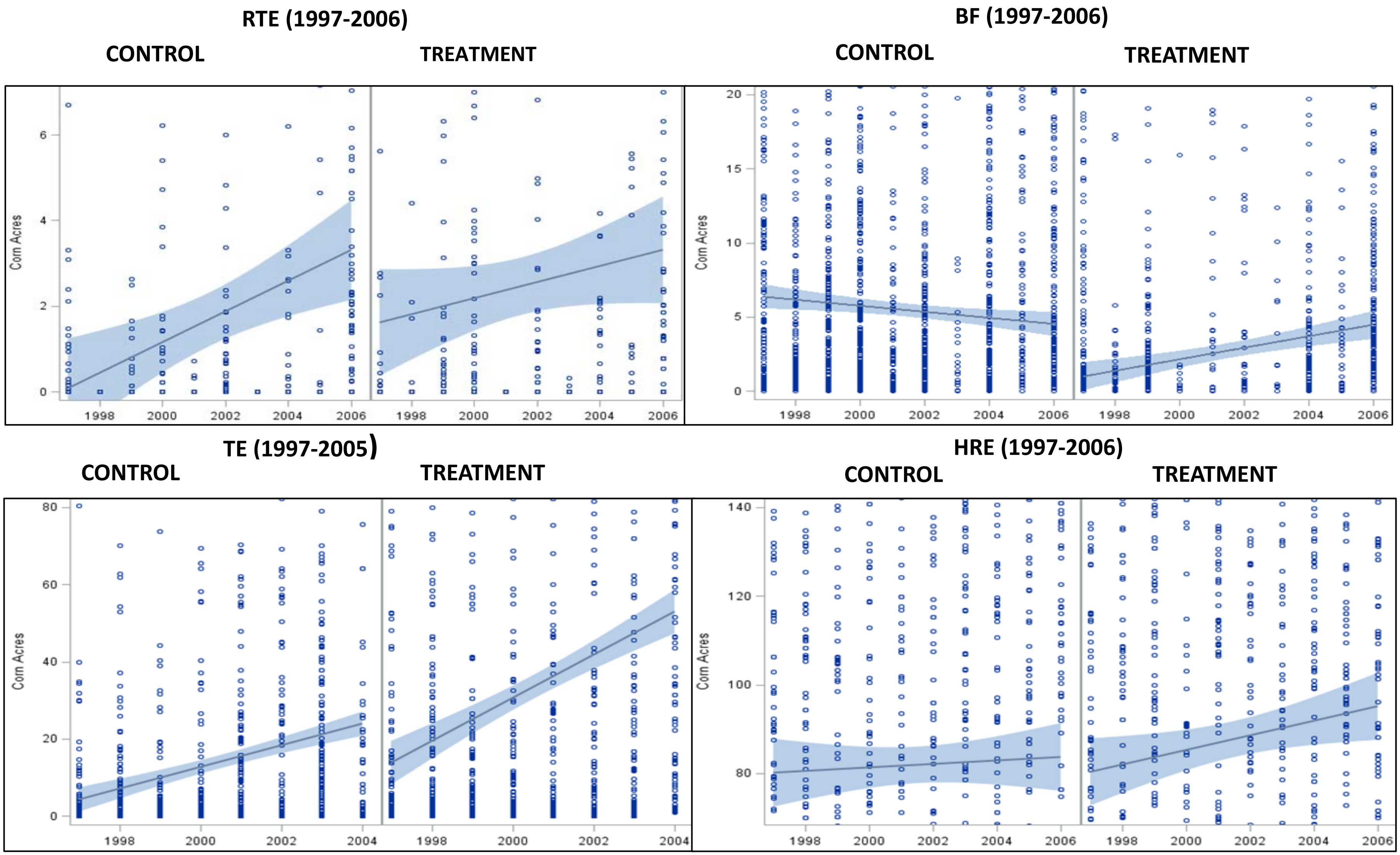
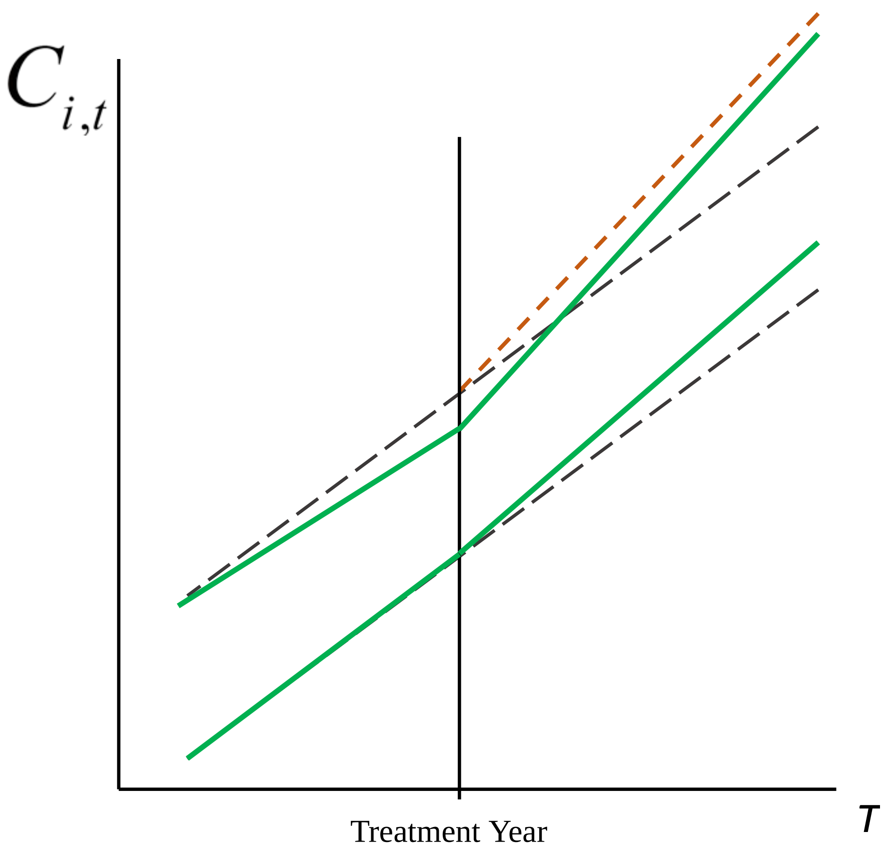
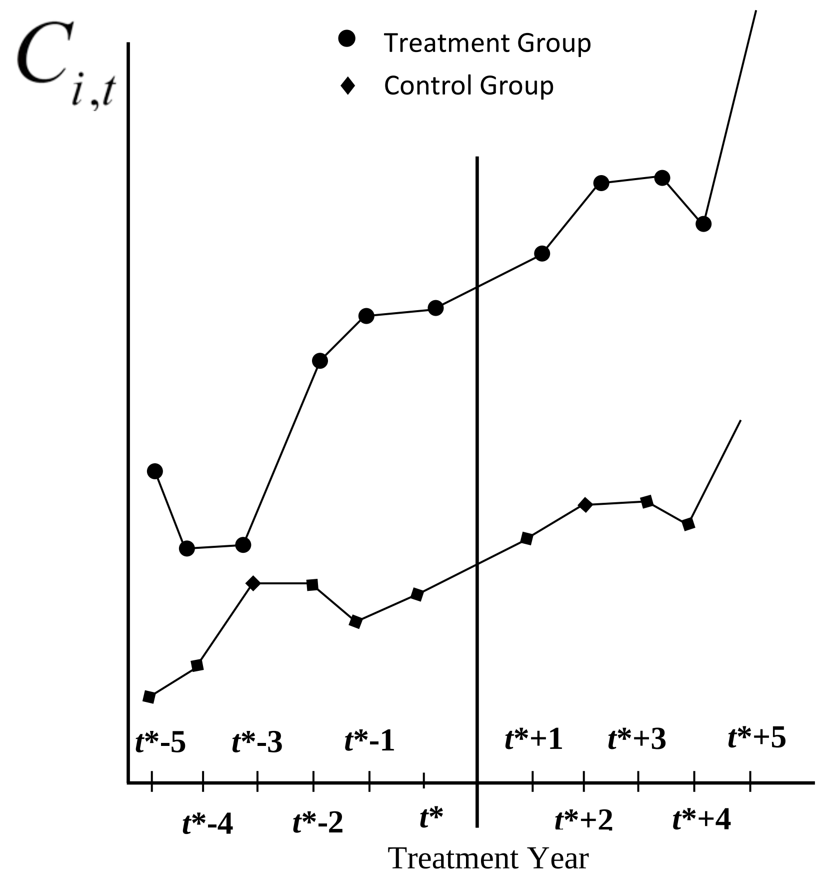


Figure 12 (a, b): The issue of Non-parallel trends among treatment and control groups.





APPENDIX

Modelling Differentiated Trends into Our DID Framework

In this section we develop the DID framework to incorporate differentiated trends among treatment and control groups as well as between pre- and post-treatment periods. In the process, we will exploit the variations in corn acres in multiple periods before and after the advent of an ethanol plant. Capturing trends, by interacting trend variables with corresponding group and time-fixed effects of the original DID model, alters the interpretation of regression coefficients that estimate treatment effects along with the identification strategies (Mora and Reggio, 2012). We will first explain the implications of a failed PPA for pre-treatment years (figure 11) and then layout a 'fully-flexible' model, originally developed by Mora and Reggio (2012), to capture trends that could vary between different years and among groups. We also discuss a family of identifying assumptions tied to estimating treatment effects under a fully-flexible model. As stated before, this section will serve as the direction our analysis will take in future.

The standard DID framework and the role of Parallel Paths Assumption:

Reconsider our equation(1), that is $C_{i,t} = \beta_0 + \beta_1 d_t + \beta_2 d_i + \beta_3 d_i d_t + \beta_{4,t} Z_i + \varepsilon_{i,t}$, where the definitions of these variables and parameters are same as in the 'Methodology' section above. Equation (2) suggests that $ATT = E[C_{i,t^+} - C_{i,t^-} \mid Z_i, d_i = 1] - E[C_{i,t^+} - C_{i,t^-} \mid Z_i, d_i = 0]$ and so mechanics of computing the treatment effects using regression equation (1) are as under:

$d_i = 1;\ d_t = 1 \rightarrow E[C_{i,t^+} \mid Z_i] = \beta_0 + \beta_1 + \beta_2 + \beta_3 + \beta_{4,t} \overline{Z_{i|d_i=1}}$,

$d_i = 1;\ d_t = 0 \rightarrow E[C_{i,t^-} \mid Z_i] = \beta_0 + \beta_2 + \beta_{4,t} \overline{Z_{i|d_i=1}}$,

$d_i = 0;\ d_t = 1 \rightarrow E[C_{i,t^+} \mid Z_i] = \beta_0 + \beta_1 + \beta_{4,t} \overline{Z_{i|d_i=0}}$,

$d_i = 0;\ d_t = 0 \rightarrow E[C_{i,t^-} \mid Z_i] = \beta_0 + \beta_{4,t} \overline{Z_{i|d_i=0}}$. Note that $\overline{Z_{i|d_i}}$ is an unconditional average.

Hence, $ATT = \beta_3$

It is, however, critical to note that by definition the ATT equals $E[C^T_{i,t^+} - C^U_{i,t^+} \mid d_i = 1]$ (where superscripts T (U) represent corn acres in the presence (absence) of ethanol plant in $t \in t^+$) and needs the parallel paths assumption to hold for β_3 to estimate the impact of ethanol plants on corn acres. Figure 12 provides a visualization of the implications when the parallel paths assumption fails. Basically, this assumption ensures that the treatment and control groups grow in a parallel fashion (grey-dashed lines) and any difference in their trends (orange- versus grey-dashed lines after the treatment year) after the advent of an ethanol plant is purely due to its existence. This difference is then captured by our estimate of β_3. However, in reality it seems that the process that we need to model is better depicted by green-solid lines in figure 12. That is, we are dealing with potentially different pre- and post-treatment trends, and also treatment and control group-specific trends. We incorporate these differences in trends in the standard DID model below.

The DID framework with Differentiated Trends:

We utilize this subsection to motivate the implication of incorporating trends into the standard DID model through a specialized example. We will discuss the mechanics involved in estimating the treatment effects within a new framework, including the underlying identifying assumptions, and show how these are different from the standard case. We will ultimately move towards a generalized model proposed by Mora and Reggio's (2012) working paper, discussing its applicability for our analysis.

To incorporate the differences in trends as depicted by figure 12 a., consider the following econometric model.

(A.1) $$C_{i,t} = \beta_0 + \beta_0' t + \beta_1 d_t + \beta_1' t d_t + \beta_2 d_i + \beta_2' t d_i + \beta_3 d_i d_t + \beta_3' t d_i d_t + \beta_{4,t} Z_i + \varepsilon_{i,t},$$

Where variable t represents time trends such that $t = 1$ for year =1997 (2006) for North (South) Dakota ethanol plants, and it increases by one for each subsequent year. While the standard DID model in equation (1) allows distinct intercepts for treatment/control groups and pre-/post-treatment periods, the updated model in equation (A.1) allows for distinct linear trends (slopes), as well as intercepts, for these groups and periods. Repeating our exercise of the previous subsection for computing treatment effects from equation (A.1), we get

$d_i = 1;\ d_t = 1 \rightarrow E[C_{i,t^+} \mid Z_i] = \beta_0 + \beta_0' t + \beta_1 + \beta_1' t + \beta_2 + \beta_2' t + \beta_3 + \beta_3' t + \beta_{4,t} \overline{Z_{i|d_i=1}}$,

$d_i = 1;\ d_t = 0 \rightarrow E[C_{i,t^-} \mid Z_i] = \beta_0 + \beta_0' t + \beta_2 + \beta_2' t + \beta_{4,t} \overline{Z_{i|d_i=1}}$,

$d_i = 0;\ d_t = 1 \rightarrow E[C_{i,t^+} \mid Z_i] = \beta_0 + \beta_0' t + \beta_1 + \beta_1' t + \beta_{4,t} \overline{Z_{i|d_i=0}}$,

$d_i = 0;\ d_t = 0 \rightarrow E[C_{i,t^-} \mid Z_i] = \beta_0 + \beta_0' t + \beta_{4,t} \overline{Z_{i|d_i=0}}$. And again, $\overline{Z_{i|d_i}}$ is an unconditional average.

So, $E[C_{i,t^+} - C_{i,t^-} \mid Z_i, d_i = 1] - E[C_{i,t^+} - C_{i,t^-} \mid Z_i, d_i = 0] = \beta_3 + \beta_3' t,$ which notably changes with *t*.

However, we already know that $\beta_3 + \beta_3' t$ does not identify the ATT due to advent of an ethanol plant. Now see that, if we subtract equation (A.1) from its one-period lagged counterpart, we have

(A.2) $$\Delta C_{i,t} = \beta_0' + \beta_1' d_t + \beta_2' d_i + \beta_3' d_i d_t + \Delta\beta_{4,t} Z_i + \Delta\varepsilon_{i,t},$$

where $\Delta C_{i,t} = C_{i,t} - C_{i,t-1},\ \Delta\beta_{4,t} = \beta_{4,t} - \beta_{4,t-1}$ and $\Delta\varepsilon_{i,t} = \varepsilon_{i,t} - \varepsilon_{i,t-1}$.

It is evident that the mechanics of equation (A.2) to compute the treatment effects due to the advent of an ethanol plant are similar to those of equation (1), with pertinent differences in notations of variables and parameters. So, our 'new' average treatment effect for the treated (ATT') is given as:

(A.3)
$$ATT' = E[\Delta C_{i,t} - \Delta C_{i,t'} \mid Z_i, d_i = 1] - E[\Delta C_{i,t} - \Delta C_{i,t'} \mid Z_i, d_i = 0] = \beta_3' \;\forall\; t \in t^+,\; t' \in t^- \;\&\; t > t'.$$

Here, it is important to realize that the interpretation of ATT' is not same as our standard ATT. Expanding the mathematical expression of ATT' from equation (A.3) gives

(A.4)
$$\begin{aligned} ATT' = \{&E[C_{i,t} - C_{i,t'} \mid Z_i, d_i = 1] - E[C_{i,t} - C_{i,t'} \mid Z_i, d_i = 0]\} - \\ &\{E[C_{i,t-1} - \Delta C_{i,t'-1} \mid Z_i, d_i = 1] - E[C_{i,t-1} - C_{i,t'-1} \mid Z_i, d_i = 0]\} \;\forall\; t \in t^+,\; t' \in t^- \;\&\; t > t' \end{aligned}$$

We can now re-write our 'new' average treatment effect for the treated as a function of ATT,

$ATT'(t,t' \mid Z) = ATT(t,t' \mid Z) - ATT(t-1, t'-1 \mid Z) \equiv \Delta ATT(t,t' \mid Z) \;\forall\; t \in t^+,\; t' \in t^- \;\&\; t > t'$, which in turn suggests that ATT' measures the impact of treatment as change in the standard treatment effects (ATT) between a specific post-treatment period and a specific pre-treatment period. In the context of ethanol plants, ATT' would measure a one-period change in corn acres from a post-treatment year relative to a one-period counterpart from a pre-treatment year.

One other dimension of our updated DID framework to incorporate trends is an identification assumption. The identification issue with ATT' would, however, remain consistent with the one in the standard DID model. That is, by definition, ATT' equals $E[\Delta C_{i,t}^T - \Delta C_{i,t}^U \mid d_i = 1, Z_i]$, where superscripts T (U) represent corn acres in presence (absence) of ethanol plant in $t \in t^+$. As with the standard DID model, since $\Delta C_{i,t}^U$ is not observed for the post-treatment years, we would need an identification assumption to be able to estimate ATT' as an estimate of β_3' in equation (A.2) above. This identification assumption for ATT' is a modified version of equation (1) above,

(A.5)
$$E[\Delta C_{i,t}^U - \Delta C_{i,t'}^U \mid Z_i, d_i = 1] = E[\Delta C_{i,t}^U - \Delta C_{i,t'}^U \mid Z_i, d_i = 0] \;\forall\; t \in t^+ \;\&\; t' \in t^-.$$

Note that the new identifying assumption compares first-differences in outcome levels among treatment and control groups, as opposed to the outcome levels as in the identifying assumption for the standard ATT (see equation (1)). Based on equations (9) and (11) we can term our new estimator as a difference-in-first-difference estimator (following Mora and Reggio, 2012).

An aspect of the updated model and its identifying assumption is that it allows estimating a (change in) treatment effects for each of the multiple post-treatment periods, i.e. for every $t \in t^+$. Alongside, it also allows using multiple pre-treatment years, i.e. each $t' \in t^-$. However, it would suffice to estimate the impact of treatment from the last pre-treatment period, say t^*. To see this, consider $ATT'(s \mid Z_i)$ defined s periods ahead of t^* such that that $t = t' + s$ and $t' = t^*$. Hence, the identifying assumption and $ATT'(s \mid Z_i)$ are given by equations (A.6) and (A.7) respectively.

(A.6)
$$E[\Delta C_{i,t^*+s}^U - \Delta C_{i,t^*}^U \mid Z_i, d_i = 1] = E[\Delta C_{i,t^*+s}^U - \Delta C_{i,t^*}^U \mid Z_i, d_i = 0].$$

(A.7)
$$ATT'(s \mid Z_i) = E[\Delta C_{i,t^*+s} - \Delta C_{i,t^*} \mid Z_i, d_i = 1] - E[\Delta C_{i,t^*+s} - \Delta C_{i,t^*} \mid Z_i, d_i = 0]$$

We can write $ATT'(s \mid Z_i)$ as a function of the original

$$ATT'(s \mid Z_i) = \{E[C_{i,t^*+s} - C_{i,t^*} \mid Z_i, d_i = 1] - E[C_{i,t^*+s} - C_{i,t^*} \mid Z_i, d_i = 0]\} -$$

(A.8) $$\{E[C_{i,t^*+s-1} - C_{i,t^*-1} \mid Z_i, d_i = 1] - E[C_{i,t^*+s-1} - C_{i,t^*-1} \mid Z_i, d_i = 0]\}$$

$$\therefore \quad ATT'(s \mid Z_i) = ATT(s \mid Z_i) - ATT(s-1 \mid Z_i)$$

Now, to evaluate the impact of ethanol plants our primary interest still lies in estimating ATT from the standard model. Since $ATT'(s \mid Z_i) = \beta_3'$, independent of s, the ATT can be recursively calculated for each post-treatment year as s increases by 1. That is, $ATT(s+1 \mid Z_i) = ATT(s \mid Z_i) + \beta_3'$ for $s \geq 2$. For $s = 1$, first see that $ATT(0 \mid Z_i) = 0$ because $E[C_{i,t^*}^T - C_{i,t^*}^U \mid d_i = 1, Z_i] = 0$ [8], which in turn yields that $ATT'(1 \mid Z_i) = ATT(1 \mid Z_i)$. Since $ATT'(1 \mid Z_i)$ is identified by (12) and $ATT(1 \mid Z_i)$ is not, we compute $ATT'(1 \mid Z_i)$ below.

We know that,

$$ATT'(1 \mid Z_i) = \{E[C_{i,t^*+1} - C_{i,t^*} \mid Z_i, d_i = 1] - E[C_{i,t^*+1} - C_{i,t^*} \mid Z_i, d_i = 0]\} -$$
$$\{E[C_{i,t^*} - C_{i,t^*-1} \mid Z_i, d_i = 1] - E[C_{i,t^*} - C_{i,t^*-1} \mid Z_i, d_i = 0]\}$$

We explicitly write-out the expressions for C_{i,t^*+1}, C_{i,t^*} and C_{i,t^*-1} below because $d_t = 1$ only for t^*+1.

$$C_{i,t^*+1} = \beta_0 + \beta_0'(t^*+1) + \beta_1 + \beta_1'(t^*+1) + \beta_2 d_i + \beta_2'(t^*+1).d_i + \beta_3 d_i + \beta_3'(t^*+1).d_i + \beta_{4,t^*+1} Z_i + \varepsilon_{i,t^*+1}$$
$$C_{i,t^*} = \beta_0 + \beta_0'(t^*) + \beta_2 d_i + \beta_2'(t^*).d_i + \beta_{4,t^*} Z_i + \varepsilon_{i,t^*}$$
$$C_{i,t^*-1} = \beta_0 + \beta_0'(t^*-1) + \beta_2 d_i + \beta_2'(t^*-1).d_i + \beta_{4,t^*-1} Z_i + \varepsilon_{i,t^*-1}$$

It can now easily be shown that $ATT(1 \mid Z_i) = ATT'(1 \mid Z_i) = \beta_3 + \beta_3'(t^*+1)$. The way $ATT(1 \mid Z_i)$ depends on t^* also justifies the use of last pre-treatment period as sufficient to compute $ATTs$ for all post-treatment periods. If we were to use the penultimate pre-treatments period instead of the last pre-treatment period, only (t^*+1) would be replaced by (t^*+2) in the expression for $ATT(1 \mid Z_i)$ as the base period has changed. However, doing this would require at least 3 pre-treatment years which may not be practically available (as is the case of South Dakota for this article).

Hence, the recursive solution to estimate treatment effects, using a DID framework that incorporates differentiated trends, by estimating equation (A.2) is given as:

(A.9) $$ATT(s \mid Z_i) = \beta_3 + \beta_3'(t^*+s) \;\; \forall s \geq 1.$$

Now that we have motivated the idea of incorporating trends into the standard DID framework, we address two further issues addressed by Mora and Reggio (2012). First, that the parallel first-difference assumption that identifies our ‘new’ average treatment effects for the treated can be

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[8] $E[\Delta C_{i,t'}^T - \Delta C_{i,t'}^U \mid d_i = 1, Z_i] = E[C_{i,t'}^T - C_{i,t'}^U \mid d_i = 1, Z_i] = 0 \;\forall\; t' \leq t^*$. This is one of the reasons why it would suffice to consider only the last pre-treatment period to evaluate the treatment effects. Given a recursive formulation to compute ATT for each subsequent post-treatment period, the periods prior to t^* would not matter.

generalized into a family of parallel n-differences assumptions. The formulation and interpretation of the average treatment effects in those cases would, however, differ. Second, the authors provide a 'fully-flexible DID model' by incorporating trends through indicator variables for each time period. This model has its two advantages, when compared to our linear-trends model here: (A.) it incorporates flexible trends visualized in figure 12(b.), and (B.) it allows testing for equivalence between the parallel n-differences assumptions. The linear-trends DID model that we have developed in this sub-section is essentially a special case of the fully-flexible DID model' presented hereafter. A more intuitive way to incorporate trends into our model that vary each period for both groups is introducing non-linear functional forms for the trend-variable (for example, quadratic trends). Since the fully-flexible version includes dummy variables for each time-period these non-linear trends are only special cases of Mora and Reggio (2012)'s model.

Before presenting the mechanics of a fully-flexible DID model we will motivate the specifics of the family of generalized parallel n-differences assumption using our updated DID model in equation (A.1). Consider the parallel first-difference assumption in equation (A.6) that identifies $ATT'(s \mid Z_i)$, s periods ahead of the last pre-treatment period t^*, and re-write it as follows:

(A.10) $$E[\Delta_s \Delta C^U_{i,t^*+s} \mid Z_i, d_i = 1] = E[\Delta_s \Delta C^U_{i,t^*+s} \mid Z_i, d_i = 0] \ ,$$

Where, U represents the case of no treatment (or no ethanol plant) and $\Delta_s \equiv (1-L^s)$ so that we compute the treatment effect s periods ahead of t^* relative to the first difference in outcome levels at t^*. A generalized parallel n-differences assumption including higher-order differences of outcome levels to identify ATT' for all post-treatment periods similar to that in equation (A.10). A parallel n-differences assumption, notated as parallel (n-s) assumption by Mora and Reggio (2013) is given as:

(A.11) $$E[\Delta_s \Delta^{n-1} C^U_{i,t^*+s} \mid Z_i, d_i = 1] = E[\Delta_s \Delta^{n-1} C^U_{i,t^*+s} \mid Z_i, d_i = 0]$$

See that for $n = 1$ equation (A.11) reduces to a parallel paths assumption and for $n = 2$ it is the parallel first-difference assumption. For $n > 2$, however, we move towards higher order differences. For example, $n = 3$ implies a $\Delta^2 [= (1-\mathrm{L}) - (\mathrm{L} - \mathrm{L}^2)]$ operator on the s period ahead outcome variable. We will require at least 3 pre-treatment years in our dataset to exploit such an operator due to the parallel double-differences assumption. Thus, the generalizations introduced by $n > 2$ cases are only applicable to the cases of North Dakota ethanol plants. The generalized average treatment effects from parallel n-differences assumption is given as[9]

(A.12)
$$ATT'(s,n \mid Z_i) = \Delta^{n-1} ATT(s \mid Z_i) = E[\Delta_s \Delta^{n-1} C^U_{i,t^*+s} \mid Z_i, d_i = 1] - E[\Delta_s \Delta^{n-1} C^U_{i,t^*+s} \mid Z_i, d_i = 0]$$

For the $n = 3$ case of our linear-trends model, $ATT'(s,3 \mid Z_i) = \Delta^2 ATT(s \mid Z_i) = ATT(s \mid Z_i) - 2ATT(s-1 \mid Z_i) + ATT(s-2 \mid Z_i)$, which will recursively identify $ATT(s \mid Z_i) = ATT'(s,3 \mid Z_i) + 2ATT(s-1 \mid Z_i) - ATT(s-2 \mid Z_i)$. Similar to the $n = 2$ case, for $s = 1,\ 2$ we will have $ATT(s \mid Z_i) = ATT'(s,3 \mid Z_i)$. It is quite evident here that

[9] See Theorem 1 in Mora and Reggio (2013).

the treatment effects estimated under parallel double-differences assumption will not equal those under parallel first-difference or parallel paths assumptions. It is, however, interesting to note that the treatment effects estimated using an exactly same model in equation (A.5) can be very different in magnitude as well as interpretation depending on the identifying assumption used.

Note that these updated assumptions for incorporating trends into DID cannot be validated since they are defined as nth-order difference in outcome variable including the post-treatment periods. However, these assumptions can be tested for equivalence using the fully-flexible model discussed next. A parallel n-differences assumption is equivalent to a parallel (n-1)-differences assumption (OR $ATT'(s,n \mid Z_i) = ATT'(s,n-1 \mid Z_i)\ \forall\ s$) if and only if $E[\Delta^{n-1} C^U_{i,t*} \mid Z_i, d_i = 1] - E[\Delta^{n-1} C^U_{i,t*} \mid Z_i, d_i = 0]$ [10].

[10] See Theorem 2 in Mora and Reggio (2012).