

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:

The focus of this study is the Dakotas' recent land use transitions from grass to corn and soybean cultivation. Recent literature has extensively characterized these land use changes and related concerns. However, formal analyses to understand the factors underlying these conversions are lacking. We study the role of Dakotas' ethanol plants in these land use changes. We construct a spatially delineated dataset and implement a Difference-in-Difference (DID) model in conjunction 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 the treatment and estimate the treatment effects for each ethanol plant based on the parallel paths assumption that is standard for the DID methods. We find that effects vary by ethanol plants and so we view as inappropriate the single point estimates for all ethanol plants in a region that are usually provided in the literature. Surprisingly, we find insignificant positive, and significant but negative ethanol plant impacts on local corn-acres. Negative estimates are hard to reconcile with the economic incentives due to ethanol plants. We also find intensified corn production and reduced corn-soy rotations due to the ethanol plants. Furthermore, based on placebo tests and pre-treatment trends in corn acres, we find that the identifying parallel paths assumption of the standard DID model does not hold. We incorporate differentiated trends into the DID framework through more flexible assumptions. 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 identified for only two out of the four ethanol plants in North Dakota. The identified treatment effects on local corn acreage are found to be positive for one plant and negative for the other. In light of economic incentives provided by the establishment of an ethanol plant, the negative treatment effect is puzzling.

Introduction and Motivation

Recent research suggests significant land use transitions in North and South Dakota, where grasslands have been lost to corn and soybean cultivation. We analyze the role of ethanol plants in the growth of the Dakotas' corn/soy acreage over the past decade. The U.S. ethanol industry boomed after the introduction of the Renewable Fuels Standard in the Energy Policy Act of 2005. In 2015, about 215 ethanol plants were operational in the country. Existing economic analyses have established *regional* impacts of ethanol plants on farmland values, local corn prices and land use. We investigate *localized* impacts of ethanol plants on the Dakotas' land use changes. Our view is that these plants would acquire corn locally to reduce transportation costs towards ethanol production, and would encourage local corn production by offering higher per bushel prices to nearby growers.

The eastern Dakotas contain a major portion of the U.S. Prairie Pothole Region (PPR), which encompasses most of the country's remaining native grasslands. The prairies support the region's wetlands that provide nesting habitat for waterfowl and other avian species. The grasslands also store excess atmospheric carbon and reduce soil erosion. Dakotas' soils are dry, erosive, and prone to highly variable biomass outputs. Historically, grasses have sustained livestock production on these marginal soils. Traditionally, wheat has been the predominant crop due to its tolerance towards these marginal soils. The recent land use changes in the Dakotas towards intensified crop production raise many ecological, environmental, agronomic, and economic concerns.

The ecological concerns arise due to loss of native prairie and drying up of regional wetlands that threaten the local waterfowl population. Intensified cropping raises agronomic concerns of reduced soil quality due to increased erosion, reduced water holding capacity of the

soils and lower productivity. Erosion due to intensified row cropping practices, especially corn, also pollutes regional water streams. Loss of stored carbon from uprooting native grasses adds to environmental impacts of these conversions. The economic concerns are tied to the reduced ecosystem services through loss of native prairie and game species, and frequent crop failures due to the region's erosive soils. Further, fewer opportunities for livestock production remain as row cropping intensifies on more productive soils. Also, higher corn and soybean cultivation would tailor the socio-economic structure of the region towards more crop-based infrastructure, thereby making crops even more attractive to farmers.

Many studies have analyzed the spatial and temporal extent of cropland expansions that displaced grasslands, including the Dakotas' native prairies (discussed hereinafter). The Dakotas have added the most new cultivated land in the United States after 2006 with significant grassland conversions. Relevant studies also point towards the potential role of various physical and market-related conversion factors, along with the potential role of agricultural and environmental policy. Although the Dakotas' land use changes are well characterized, a formal causal analysis to understand what drives these changes is absent. We extend this literature by formally establishing the causal impacts of ethanol plants on local land use changes in these states. All of the Dakotas' ethanol plants are corn-based. Hence, we ask how the advent of an ethanol plant affects corn plantings in its proximity.

Understanding the role of ethanol plants towards grassland conversion is relevant since these grasslands are a public resource largely under private ownership. Therefore, the observed land use changes are essentially an aggregate outcome of the localized private decisions by individual landowners. The private land use decisions are potentially driven by the changing climate, evolving technology, the local business environment, infrastructure, commodity prices,

and government payments towards conservation and crop insurance. For example, Claassen et al. (2011) suggest that federal crop insurance subsidies have intensified cropping practices by reducing related financial risks. Ethanol plants, the focus of this study, also reduce production risks as they enhance corn demand in their locality that potentially incentivizes grassland conversions towards corn cultivation.

There are 19 ethanol plants in the 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), accounting for about 9% of the total U.S. ethanol production capacity. Most of the Dakotas' ethanol plants started operations during 2006–08, which coincides with the observed rapid land use conversions outlined in the pertinent literature. We expect the ethanol plants to influence localized land use changes and hence modelling those rather than aggregate, regional-level decisions is more relevant. We present a unique research design that utilizes spatially-delineated data and implements a quasi-experimental setting to evaluate the impact of ethanol plants on local corn acreage. We now provide a brief summary of the many land use change studies that have characterized the recent grassland conversion in this region.

Wright and Wimberly (2013) used the U.S. Department of Agriculture (USDA) Cropland Data Layer (CDL) database to summarize spatial conversions from grass to corn and soybean between 2006 and 2011 in the U.S. Western Corn Belt (WCB), spanning North and South Dakota, Nebraska, Iowa, and Minnesota. The Dakotas experienced the most grassland conversions with 271,000 hectares lost to cropping out of the 528,000 hectares in all of WCB. Higher commodity prices and increased biofuels production were attributed as potential drivers for such land use changes. The spatial characterization of land use changes in these two states revealed a westward expansion of the Corn Belt toward the Missouri River. Lark et al. (2015)

asserted that the Dakotas added the most new cultivated land in the United States during 2008–12, predominantly east of the Missouri River. However, northwestern and southeastern North Dakota experienced contraction of croplands during this period. Lark et al. present a long-term trend analysis using the U.S. Geological Survey (1972–2002) to evaluate conversions on native grasslands. The Dakotas stood out with the highest conversion rates on lands previously attributed to native grasses. In addition, soybeans (wheat) was found to be the first crop planted upon conversion east (west) of the Missouri River.

Johnston (2014) provided a longer-term perspective on cropland expansion in the Dakotas, utilizing USDA National Agricultural Statistical Service (NASS)'s state-level crop acreage data (1980–2011) and the CDL data (2006–12). The corn/soy acreage almost tripled between 1980 and 2011, where these crops accounted for only 5% of the Dakotas' agricultural acreage in 1980. The probability of corn/soy being re-planted to corn/soy increased from 68% in 2006–07 to 80% in 2011–12. The corresponding probability for grasslands decreased from 81% in 2006–07 to 74% in 2011–12. Corn and soybeans were also found to replace wheat and small grain crops that were historically dominant due to their tolerance for the local climate. Johnston attributed technological advancements (i.e., drought/cold-resistant corn and soybean varieties) as potential drivers of such land use transitions.

A study by Stephens et al. (2008) estimated the probability of grassland conversion conditional on amounts of surrounding grasslands, slope, and soil productivity. The annualized grassland loss in the Dakotas' Missouri Coteau region was estimated to be 0.4%, which amounts to 36,450 hectares during 1989–2003. However, they found that the probability of conversion varied across the lands of high biological value (amenable to waterfowl breeding). Stephens et al.

recommended that conservation policies should be targeted specifically to the lands with higher conversion probability, conditional on their location and soil quality attributes.

This paper is subdivided into several sections. We first motivate the economic incentives that theoretical considerations suggest should motivate land use conversion in the proximity of ethanol plants. A literature review of the relevant findings on the impacts of ethanol plants from earlier studies is then discussed. Our data section discusses how we constructed a spatially delineated dataset for this analysis and provides a detailed explanation of the relevant variables. The methodology section presents our research design, the Differences-in-Difference (DID) model in conjunction with Propensity Score Matching and an extension of the DID to include flexible trends. Section 4 provides estimation results for each ethanol plant and lastly we conclude with some discussions.

Economic Motivation

Consider a representative farmer's dual profit function, $\pi(p - t(x))$, that depends on the difference between the market price of corn and its transportation cost $t(x)$. The transportation cost is a function of the distance between a representative farmer and the demand terminal for corn (x). To motivate the economic incentives due to proximity of these ethanol plants, we compare the pre- and post-ethanol plant trends in corn basis for counties that house these plants in North and South Dakota (see figure 1). Basis is the difference between the local price and the futures price of a commodity. Basis accounts for the transportation costs, and thus a higher corn basis in the post-ethanol plant years should be tied to the reduced transportation costs in the plant's proximity. Figure 1 shows a steeper basis trend after 2008 when compared to before 2006 (i.e., corn basis was higher in the post-plant years in those counties that housed ethanol plants).

Therefore, we conjecture a positive and statistically significant impact of ethanol plants on local corn acreage.

Literature Review

Earlier attempts to assess the impacts of ethanol plants involved an indirect evaluation of land use change by way of analyzing impacts on local corn prices and farmland values. In more recent years, studies have considered the direct impact of ethanol plants on corn acres as a measure of land use change. We provide a brief review of analyses involving grain prices and farmland values, followed by a detailed review of the analyses of impacts on land acreage because these are of direct relevance to our inquiry.

Miao (2013) has evaluated the proportion of corn acreage for the Iowa counties in response to the location, capacity, and ownership of ethanol plants. He utilized a county-level panel data set from 1997 through 2009, and applied 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 control for the endogeneity of ethanol plants and for corn-soybean rotations by including a 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 also 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 et al. (2016) used a grid-level spatially-delineated dataset to estimate a non-linear response of the refining capacity of ethanol plants in each grid-cell's neighborhood on its corn acreage in the U.S. Midwestern states: ND, SD, NE, MN, WI, IA, KS, OK, MI, IL, IN, OH. They utilized a panel regression model where the dependent variable is corn acreage on 10km X 10km land parcels during 2006–10. They corrected for the endogenous ethanol plant locations

and neighboring land use by utilizing the length of railroads within each grid cell as an instrument for refining capacity. They found a significant increase in corn acres in grid cells with higher ethanol refining capacity in their neighborhood, but the effect dampened over the years. Motamed et al. (2016) built upon an earlier study by Motamed and McPhail (2011) that models regional corn acreage on the proximity to nearest ethanol plants. In the 2011 study, the covariates were distance to the nearest grain elevators and ethanol plant, the plant's capacity, cash bids at the nearest grain elevator and a soil productivity index. The instruments for each parcel's distance from the nearest ethanol plant were the distance from the nearest interstate ramp, primary/secondary roads and water ports. This analysis estimated 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) measured the impact of ethanol plants on farmland acreage for the state of Wisconsin between 2000 and 2006. Although Wisconsin was reported to be losing farmland to other uses during this period, fallow or undeveloped acres were found to increase. The authors investigated the possibility that the fallow lands were reverted as croplands in proximity of the ethanol plants. The authors used municipality-level land use data and allocated 2-mile, 10-mile and 50-mile zones around the four operational ethanol plants during 2000–06. The differences between percentage change in agricultural acreage (2000–06) across these zones evaluated the ethanol plant impacts in Wisconsin. The impact of ethanol plants on each of three zones' agricultural acreage was 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 observed 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 judge the spatial extent of their corn suppliers. A 43-mile and a 23-mile circle was placed around IRE & PRF, respectively. The study concluded that ethanol plants had a weak influence on direct land use change in their vicinity, and inferred that higher yields supported increased exports and increased 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 evaluated conversions from other cropland and non-cropland uses in 2007 to corn production in 2008 and 2009 on 5-acre parcels. The authors found that reducing parcel's distance to the 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, their estimates may be biased due to likely endogeneity of ethanol plant locations. Stevens (2015) also utilized a spatially-explicit field-level dataset for IA, IN, IL and NE to estimate the change in probability of planting corn with proximity of the nearest ethanol plant between 2002 and 2014. He found a positive impact of the presence of an ethanol refinery only within its 30-mile radius, although not controlling for the endogeneity of plants' locations.

The literature lacks a consensus regarding impacts of ethanol plants on local grain prices and agricultural land values (Miao 2013), which can provide indirect evidence 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 DID estimation on matched parcels to find increased farmland values in the vicinity of the ethanol

plants at a time that witnessed a sharp dip in residential values. The study by Henderson and Gloy (2008) 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 in plant locations. Du et al. (2007), on the other hand, rejected the hypothesis that ethanol plants significantly affect Iowa farmland cash rental rates. In the context of local grain prices, Katchova (2009), O'Brien (2009), and McNew and Griffith (2005) found a positive and significant impact of ethanol plants on local grain prices, whereas Lewis (2010) found that these positive impacts vary spatially. The author found significant impacts for MI and KS, and an insignificant impacts for IA and IN.

The above review suggests disagreement in the literature on the direct and indirect impacts of ethanol plants on local land uses. 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. 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, the endogeneity due the ethanol plant's location was ignored by most of the earlier studies and may provide biased estimates of the impacts of ethanol plants.

We extensively utilize remote sensing tools that generate 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 the 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 for land use and soil quality in the Dakotas from two main sources: the ‘CropScape’ portal of the USDA-National Agricultural Statistical Service’s Cropland Data Layer (CDL) Program, and 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 (pixelated) data for all contiguous U.S. 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 employ remote sensing tools, namely ERDAS Imagine and ArcGIS, and 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. The grid polygons are designated as representative decision-making land parcels with a unique identifier

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.

that are observed every year. Overall, our study sample includes approximately 104,000 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 from 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 impediments for cropping, with higher class codes assigned to more serious impediments. LCC classes I and II are well-suited for cropping, whereas LCC classes III and IV require additional management practices to be suitable for cropping, often restricting their use to pasture, rangeland or forests. LCC level V and worse have severe limitations that make them impractical for crop cultivation. Representative slope simply measures the average rise per unit run. The tabular data combines these soil attributes to geographically delineated and uniquely identified soil map units. To attribute soil quality for each of our representative land parcels, we calculate area-weighted LCC (*WLCC*) and slope (*WSLP*) variables. The area-weights are calculated as the proportion of each soil map unit's area within the 500-acre land parcels. See supplementary information 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 conjunction with online maps of these plants made available on *Ethanol Producer Magazine's* website. We conduct our analysis on all four ethanol plants in North Dakota and four out of 15 ethanol plants in South Dakota, 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), to be discussed hereafter under ‘Estimation Results’.

Methodology

Our 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 impact of ethanol plants on land use patterns in their neighborhood. In this sense, we interpret the advent of an ethanol plant as the treatment where pre-and post-treatment year outcome levels are the observed land use patterns before and after it started operations, respectively.

To implement a quasi-experimental setting with ethanol plant as 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. An ethanol plant that procures most of its annually required corn from nearby areas saves on transportation and related logistical costs, and so is willing to compensate local suppliers. Therefore, in order to define our treatment and control groups, we assume that the related transportation costs are monotonic in the Euclidean distances between a land parcel and the ethanol plant, and that the grower bears at least some of these costs. In this scenario, a supplier/landowner located nearer to the ethanol plant has higher incentive to grow corn than 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? Back-of-the-Envelope-Calculations

We support transportation costs, and thus Euclidean distances, as sensible treatment and control parameters with some empirical evidence. Consider transport-trucks with the carrying capacity of 1 ton (=39.4 bushels²) corn and a mileage of 134 ton-miles per gallon. The annual average cost of diesel was \$2.4–\$4 after 2005 (U.S. Energy Information Administration). O’Brien (2009) estimated the total transportation cost to be approximately four times the fuel cost, which is 0.20–0.28 cents as the fuel cost of transporting one corn bushel for one mile was 0.05–0.07 cents in the U.S. Hence, the maximum willingness to pay in order to incentivize a farmer located 50 miles closer to an ethanol plant would range between 10–14 cents per bushel of corn.

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. Since the transportation costs are 14%–47% of the total cropland rental values, these should generate strong incentives for proximate landowners to engage in corn production.

Designating Treatment and Control Groups

An aspect of our research design that differentiates it from many other quasi-experimental studies is that our treatment is not exogenous. We designate the advent of an ethanol plant as treatment, which itself is a market outcome. 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

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

counterpart.³ This admits innumerable possibilities for treatment and control groups near each ethanol plant's location and practically inexhaustible combinations that can be included for this study. It is, therefore, 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 treatment groups and two control groups for each ethanol plant (see table 2 for the schematics). The control groups are kept apart to ensure independence in robustness checks for each treatment group (see figure 3).⁴

Among the combinations of treatment and control groups, we conjecture that the treatment effects from the nearest treatment group and the farthest control group combination will be larger in size and more significant than the other comparisons. We present the regression results for this particular combination and compare it with others as a robustness strategy.

DID in conjunction with PSM

Given pre- and post-treatment periods, as well as treatment and control groups for each ethanol plant, we use the DID estimation strategy in conjunction with propensity score matching (PSM) to evaluate their role in 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 is intended to control for such

³ In some cases, we have two or more ethanol plants competing for corn from common land parcels. To analyze treatment effects for an ethanol plant in such cases we exclude parcels that are closer to other ethanol plants, irrespective of the parcels' designated group.

⁴ Due to spatial constraints, it is infeasible for all of the treatment and control groups to be non-overlapping. This is because having non-overlapping groups would require more space, which in turn would bring our groups closer to other nearby ethanol plants.

endogeneity by estimating causal impacts as the 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 treated and untreated land parcels to be alike, except for their proximity to the ethanol plant. That is, estimated treatment effects are unbiased if these land parcels are randomly assigned to the treatment and control groups, and we control for any within-group or across-group dissimilarity among them (other than the advent of an ethanol plant).

We seek to ensure random assignment of land parcels to each group by utilizing the PSM strategy, thereby conditioning treatment selection on the observed soil quality. Soil quality is central to the land use decisions, and would potentially influence ethanol plants' location choice toward regions with land attributes favoring corn production. Local infrastructure such as road and rail 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 from ethanol plants appropriately 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 in DID models. To illustrate this point briefly, consider a representative land parcel i with $C_{i,t}$ as its corn acreage at time period t . We introduce binary variables d_i and δ_t to designate treatment/control groups and pre-/post-treatment periods respectively. So $d_i = 1$ for treated parcels and equals 0 otherwise while $\delta_t = 1$ for time periods after the advent of an ethanol

plant and equals 0 otherwise. Further, denote $t^- (t^+)$ as the set of pre-treatment (post-treatment) time periods with t_0 as the treatment year.⁵ 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 $C_{i,t}$ with $t \in t^+$.⁶ Consequently, the average treatment effect for the treated (ATT) equals $E[C_{i,t^+}^T - C_{i,t^+}^U | d_i = 1]$, where superscript T(U) denote presence (absence) of the plant. The issue, though, is that the outcome levels absent an ethanol plant (i.e., the treatment) in the post-treatment years are unobserved. The DID approach seeks to overcome 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 PPA assumption is expressed as

$$(1) \quad E[C_{i,t^+}^U - C_{i,t^-}^U | Z, d_i = 1] = E[C_{i,t^+}^U - C_{i,t^-}^U | Z, d_i = 0],$$

In equation (1) 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 ATT is computed as

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

Thus, the PPA is key to identifying the estimates of treatment effects and in the event that this assumption fails the estimates of ATT are meaningless. In order to provide comparisons such that PPA is most likely to hold, we restrict our sample for estimating treatment effects to one where the conditional probability of treatment (or propensity score, PS) for each untreated parcel is close 'enough' to its treated counterpart. This method is usually known as PS matching.

⁵ For example, the Red Trail Energy ethanol plant that was established in 2007, so $t^- = \{1997, 1998, \dots, 2006\}$ and $t^+ = \{2008, 2009, \dots, 2013\}$.

⁶ 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 $CS_{i,t}$.

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,

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

$\alpha_0, \alpha_1, \alpha_2, \alpha_3$ and α_4 are regression coefficients. The justification for a quadratic functional form lies in minimizing the *Akaike Information Criterion* (or maximizing the *log-likelihood*) relative to the 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 3. We find these soil quality based models to significantly explain the probability of treatment in each case.

The logistic regressions that estimate the PS find that land parcels in the vicinity of ethanol plants may have higher LCC and/or be steeply sloped, not particularly suitable for corn production. Both *WLCC* and *WSLP* exhibit decreasing marginal returns in all cases. Higher treatment probability for parcels with relatively poor soil quality suggests that the ethanol plants may consider factors like lower land values and/or access to infrastructure (near a highway or a rail line) towards their location decisions. However, we cannot differentiate land values and infrastructure across land parcels at the fine spatial resolution of this study. The spread of estimated PS between 0 and 1 (figures 4–9) can measure whether our model specification explains the treatment probability reasonably well. A massing of estimated probabilities at extreme values (e.g., panels A and C in figures 4–9) indicates more variables are needed to

reasonably explain PS in those cases. A constrained availability of variables that estimate the PS recommends caution while interpreting our treatment estimates.

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 a 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 that entails higher variance.⁷ 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 of range [0.0001, 0.01]. The assigned calipers vary by ethanol plants and are chosen such that the post-matching samples are balanced while maximizing the number of observations in each case. The term "balanced" refers to ensuring a 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.

We follow Caliendo and Kopeinig (2008) to examine whether or not post-matching samples are balanced and to assess the matching quality. We conduct t- and F-statistics to test for equivalence of *WLCC* and *WSLP* means and variances across matched treated and untreated samples for each ethanol plant (Rosenbaum and Rubin 1985). Further, we test the joint-

⁷ 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).

significance of *WLCC* and *WSLP*, in quadratic form, when 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 variations in propensity scores (Dehejia and Wahba, 1999). The matching performance based on the mean and variance of the soil quality parameters across matched treatment and control groups and their corresponding calipers is presented in Table 4.

Standard DID estimation summary and moving towards flexible trends in DID

In the DID regression framework using matched samples, we further control for pre-treatment land use decisions as an opportunity to convert to corn. To illustrate, if a land plot was entirely in corn during pre-treatment years, it will not reveal any treatment effect due to the lack of scope for conversion. In addition, even if the land 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. See supplementary information for detailed estimation results of the standard DID model in conjunction with PSM.

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 hard to reconcile with the empirical evidence of incentives for corn production on land parcels in the 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, shown in Table 5, 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.

An implication of imperfect matching is visualized in Figure 11, where we find non-parallel pre-treatment trends for matched treatment and control groups in the case of North Dakota plants. This means that corn acres were not evolving equivalently among treatment and control groups even when the treatment was absent. Non-parallel trends during pre-treatment periods contradict the PPA, and thus the ATT estimates of a standard DID model do not represent the treatment effects due to ethanol plants. We follow Mora and Reggio (2012) to incorporate these differentiated trends between treatment and control groups into a fully-flexible DID model. We also formally test and reject the PPA using this fully-flexible DID model below.

Incorporating Flexible-Trends into the standard DID framework:

The differentiated or non-parallel pre-treatment trends across treatment and control groups in Figure 11 invalidate the PPA. We incorporate such trends into the standard DID framework through more flexible assumptions. To illustrate, we develop a special case of a fully-flexible DID model in an appendix. This special case is based on the non-parallel trends in corn acreage across treatment and control groups in the pre-treatment years (see figure 12(a), in green).⁸ However, the corn acres could potentially vary for each pre-/post-treatment period as found earlier in Figure 11. A generalized version of the differentiated corn trends is visualized in Figure 12(b) and such trends are incorporated into a fully-flexible DID model.

Reber (2005) assesses the impact of court-ordered desegregation plans for schools on school enrollments in the U.S. through a flexible DID framework.

⁸ The special case is hoped to facilitate a smooth transition for readers from the standard DID model with failed PPA to a fully-flexible DID model.

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

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

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, which in turn can potentially allow for 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 (2012), 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 intended to control for the differentiated opportunity cost of growing corn on lands that were attributed towards soybean, wheat, and grass in the previous period. The treatment effects estimator from equation (4), denoted as $ATT'(s, n | Z_i)$, is given as

$$(5) \quad ATT'(s, n | Z_i) = \Delta^{n-1} ATT(s | Z_i) = \Delta_s \Delta^{n-1} \beta_{t^*+s}^d \quad ^9$$

The term s refers to the 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 $ATT'(s, n | Z_i)$.

$ATT'(s, n | Z_i)$ is defined as the n^{th} -order treatment effect s periods ahead of the last pre-treatment period (t^*). It is evaluated by comparing the $(n-1)^{th}$ -order difference (Δ^{n-1}) in outcomes at period s relative to its counterpart at t^* across treatment and control groups. As

⁹ See Theorem 3 in Mora and Reggio (2012)

discussed in the appendix, the parallel (n^{th} -order)-differences assumption can be written mathematically as:

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

For $n = 1$ equation (6) reduces to a parallel paths assumption. For $n = 2$ equation (6) reduces to a parallel (1st-order) differences or parallel growth assumption. Note that the parallel growth assumption is specific to each s^{th} post-treatment year. The parallel growth requires that the difference between corn acres in $s - 1$ and s post-treatment years must be equal among treatment and control groups in the absence of a treatment. Also, $ATT'(s, 2 | Z_i)$ 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 on to higher order differences. For example, $n = 3$ implies a $\Delta^2 (= (1 - L) - (L - L^2))$ operator on s -periods ahead outcome variable in equations (5) and (6). It is clear that we require at least three 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 hence the higher order generalizations ($n > 2$) could not be implemented for the South Dakota plants due to data inavailability. It is interesting to note that the treatment effects can differ in size, sign, and interpretation based on the choice of n^{th} -order identifying assumption. However, these assumptions can be tested for equivalence using the coefficient estimates of the fully-flexible model. Testing the equivalence between parallel (n^{th} -order) and parallel ($(n - 1)^{\text{th}}$ -order) difference assumptions is similar to testing for the null hypothesis: $\Delta^{n-1} \beta_{t^*}^d = 0$ such that $n < T(l) - t^*$.

As mentioned earlier, the PPA can be formally tested using coefficient estimates of the fully-flexible DID model. This is because the standard DID is a special case of the fully-flexible

model ($n = 1$) and so the PPA is also a special case of the family of identifying assumptions in equation (6). To test whether the PPA holds we can simply test the null $H_0: \beta_t^d = 0 \forall t \leq t^*$. This null hypothesis requires that the treatment effect in each pre-treatment year be zero. In the event that we have perfectly matched treatment and control groups, the PPA is equivalent to the above null hypothesis in the pre-treatment years. The H_0 is rejected for each North Dakota ethanol plant (see Table 6) as indicated by the non-parallel pre-treatment trends earlier.

Multiple pre-treatment years are available for the four North Dakota ethanol plants. So the fully-flexible DID model can be implemented for these plants. However, an opportunity to implement multiple assumptions and estimating corresponding treatment effects for each case comes with the challenge of choosing among these estimates. We restrict our analysis to $n = 2$ as it is the least complex model that does not impose the PPA. That is, we compare difference in growth of corn acres between treatment and control groups rather than the difference in absolute acres, to assess treatment effects. We will conduct a spatial placebo to validate our treatment estimates, to be discussed later.

Estimation Results: The Fully-Flexible DID Model

Estimation

The econometric considerations when estimating equation (4) are discussed here. First, we include lagged variables for the three major transitioning land use types other than corn (i.e., wheat ($W_{i,t-1}$), soy ($S_{i,t-1}$), and grass ($G_{i,t-1}$)). Since the lagged variables may impact the evolution of corn acres alongside the ethanol plants, excluding them may confound the treatment estimates through omitted variable bias. The coefficient estimates to these variables would also

capture differentiated costs of conversion to corn from three different land use types.¹⁰ Second, we compute heteroscedasticity-consistent standard errors by stratifying our panel by designating each land parcel as an individual cluster. This transforms the variance-covariance matrix into a block-diagonal with each block corresponding to an individual land parcel.

A point estimate of the average treatment effect on the treated, based on the parallel growths assumption (i.e., $n = 2$), at each post-treatment period $t^* + s$ can be written as

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

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

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

For each of the four ethanol plants in North Dakota we present the coefficient estimates from the regression equation (4) in Table 6 and $ATT'(s, 2 | Z_i)$ in Table 7.

Treatment Estimates

The fully-flexible DID model estimates year-specific treatment and time effects unlike the standard DID, which estimates a single treatment and trend effects between aggregated pre- and post-treatment years (see supplementary information). However, we find differentiated opportunities for growing corn on land parcels previously planted with wheat and grass, in line with the standard DID estimation. We also include lagged soy acres and find its coefficient in

¹⁰ Although the lags primarily control for the opportunity to grow corn in these rural states, they also capture a negative correlation among $C_{i,t}$ and $C_{i,t-1}$ since corn, soy, wheat and grass are the major land uses under transition.

Table 6 to be always positive, although significant for TE and HRE, reflecting the usual cropping pattern of corn-soy rotations. The negative and significant coefficients for $G_{i,t-1}$ in all cases likely reveal high initial costs of land preparation to convert from grass to corn. Lagged wheat acres, on the other hand, are found to be positive and insignificant for BF and RTE as well as negative and significant for TE and HRE. The opportunity cost of converting from wheat to corn is lower than grass to corn, as reflected by the respective coefficients in all but one case. This is likely due to significant differences in cost of conversion.

The year-specific time dummies are interestingly higher in the post-treatment years than the pre-treatment years. This implies that the role of trend-related effects alone in driving increased corn acres in the vicinity of the North Dakota ethanol plants has been significant, irrespective of the treatment or control groups. Finally, turning to the year-specific treatment estimates, through interaction between time dummies and the treatment dummy, we still find negative (but insignificant) coefficients for BF that are hard to reconcile with economic incentives arising from transportation costs and increased local corn basis. Since the assumption of parallel paths is formally rejected, i.e., $\beta_i^d \neq 0 \forall t \leq t^*$, 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 infer that point estimates of impact across many ethanol plants in a region, as usually reported in the literature, is problematic.

As discussed earlier, we estimate the impact of ethanol plants as $ATT'(s, 2 | Z_i)$, which compares the growth of corn acres among treatment control groups over time. While the PPA based treatment estimates (although not identified) found declining absolute corn acres for three

out of four ND ethanol plants, the parallel-growth-assumption-based estimates find increased growth in corn acres for two ethanol plants: RTE and TE. Whereas HRE was found in earlier estimates to increase the level of absolute corn acres locally, its presence is found to significantly decrease growth in local corn acres. BF is found to decrease absolute corn acres as well as growth in corn acres. Negative treatment effects, whether based on the PPA or the parallel growths assumption, are not supported by the economic incentives due to their presence.

In order to contrast our results with the existing regional-level analyses we pool the data for all four cases in North Dakota. We designate 2006–08 as treatment period so that the last pre-treatment year (t^*) is 2005 and the post-treatment years are 2009–13. We cannot discern a significant uniform impact due to the North Dakota ethanol plants, as opposed to the positive impact for all of the U.S. Midwest ethanol plants by Motamed et al. (2016). Motamed et al. (2016) do recognize the potential differences due to plant-level impacts but estimate a uniform impact for the region. We disagree with this single, regional-level point estimate as our plant-by-plant analysis suggests positive and negative impacts that are not reflected in the ‘pooled’ case.

Placebo test

We need to validate the parallel growths assumption so that the new $ATT'(s, 2 | Z_i)$ estimates can be trusted. Unlike the PPA, the flexible parallel (n^{th} -order)-assumptions are specific to each post-treatment period, s . This feature allows these assumption to hold only for a subset of post-treatment periods. In this scenario, however, we can trust the treatment estimates only for the post-treatment periods where the corresponding assumption is valid.

We utilize a spatial placebo instead of the temporal placebos to validate the parallel (n^{th} -order)-assumption that is specific to every s^{th} year ahead of the t^* . Since the temporal placebos are specified for a subset of years (utilized in case of standard DID, Figure 10) they

cannot validate the new assumptions for all post-treatment years. In case of the standard DID, we aggregated pre- and post-treatment years and thus the PPA was not specific to any post-treatment year. This allowed allocating specific time periods as falsified treatment years (i.e., the temporal placebos) before or after the advent of an ethanol plant. We designate a spatial placebo (S.P.) that is a dummy ethanol plant (a point coordinate) in north-eastern North Dakota.

We locate our S.P. in north-eastern North Dakota (Figure 2) for three reasons. First, to avoid competition in demand for corn from other ethanol plants. The nearest to our S.P. is Tharaldson Ethanol which is approximately 300 km away. Second, we did not locate our placebo in north-western ND so as to avoid competition for rails/roads infrastructure by the region's Bakken Shale industry. Third, we locate our S.P. such that it sits on ND State Highway 18, following the ethanol plants in our study that are usually situated on a major highway/railroad.

We designate treatment and control groups for our S.P. with 735 land parcels in all. We then match these constituent land parcels by estimating a treatment probability for each of these from equation (3) and utilize the nearest-neighbor matching algorithm as discussed earlier. We find that area-weighted LCC and slope, in a quadratic functional form, are jointly significant in estimating the propensity of treatment from a logistic regression. Lower LCC and higher slopes are found to increase a representative parcel's treatment probability. A matching caliper of 0.01 is found to yield a balanced panel with 180 land parcels and 17 years (1997–2013). This balanced sample is then used to estimate equation (4) separately for years 2006, 2007, and 2008 as treatment year designates. We run three separate regressions for each treatment year designate due to the time period-specific identifying assumptions of the fully-flexible model. So, placebo treatment estimates will correspond to TE for 2006; to RTE & BF for 2007; and to HRE for

2008. 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.

The estimation results for placebo regressions and corresponding $ATT'(s,2)$ are presented in Tables 8 and 9, respectively. We find that $ATT'(s,2)$ remains unidentified for HRE and TE, but identified for RTE and BF (except for post-treatment years 2011 and 2013). This finding suggests that identifying localized treatment effects is challenging. Nevertheless, we can still infer upon the effects of ethanol plants on local land use using the regression estimates for RTE and BF. Note that even the placebo regressions find differentiated conversion opportunity costs from soy to corn, wheat to corn, and grass to corn.

Since the treatment effects remain unidentified for HRE and TE, we test the equivalence between the parallel (3rd-order) and (2nd-order) differences assumptions, and between the parallel (4th-order) and (3rd-order) differences assumptions. The results are presented in Table 10. We find that parallel (3rd-order) and (2nd-order) differences assumptions are not equivalent for HRE and TE. We evaluate $ATT'(s,3|Z_i)$ for these two ethanol plants and seek differences from $ATT'(s,2|Z_i)$, if any (see table 11). $ATT'(s,3|Z_i)$ and its variance are expressed as under:

$$(9) \quad ATT'(s,3|Z_i) = (\beta_{t^*+s}^d - 2\beta_{t^*+s-1}^d + \beta_{t^*+s-2}^d) - (\beta_{t^*}^d - 2\beta_{t^*-1}^d + \beta_{t^*-2}^d).$$

$$(10) \quad \begin{aligned} Var(ATT'(s,3|Z_i)) = & Var(\beta_{t^*+s}^d) + 4Var(\beta_{t^*+s-1}^d) + Var(\beta_{t^*+s-2}^d) \\ & + Var(\beta_{t^*}^d) + 4Var(\beta_{t^*-1}^d) + Var(\beta_{t^*-2}^d) \\ & - 4 \cdot Cov(\beta_{t^*+s}^d, \beta_{t^*+s-1}^d) + 2Cov(\beta_{t^*+s}^d, \beta_{t^*+s-2}^d) - 4Cov(\beta_{t^*+s-1}^d, \beta_{t^*+s-2}^d) \\ & - 4Cov(\beta_{t^*}^d, \beta_{t^*-1}^d) + 2Cov(\beta_{t^*}^d, \beta_{t^*-2}^d) - 4Cov(\beta_{t^*-1}^d, \beta_{t^*-2}^d) \\ & - 2Cov(\beta_{t^*+s}^d, \beta_{t^*}^d) + 4Cov(\beta_{t^*+s}^d, \beta_{t^*-1}^d) - 2Cov(\beta_{t^*+s}^d, \beta_{t^*-2}^d) \\ & + 4Cov(\beta_{t^*+s-1}^d, \beta_{t^*}^d) - 8Cov(\beta_{t^*+s-1}^d, \beta_{t^*-1}^d) + 4Cov(\beta_{t^*+s-1}^d, \beta_{t^*-2}^d) \\ & - 2Cov(\beta_{t^*+s-2}^d, \beta_{t^*}^d) + 4Cov(\beta_{t^*+s-2}^d, \beta_{t^*-1}^d) - 2Cov(\beta_{t^*+s-2}^d, \beta_{t^*-2}^d). \end{aligned}$$

Observe that the sign of the higher-order treatment effects for TE and HRE is the same as earlier. These higher-order treatment effects ($n = 3$) are interpreted as change in rate of growth in corn acres due to the presence of an ethanol plant. However, the spatial placebo invalidates the identifying parallel (3rd-order) difference assumption. Hence, we now rely solely on HRE and BF to infer on the role of ethanol plants in North Dakota.

The treatment estimates for corn acres due to HRE and BF do indicate a potential shift in agricultural systems due to these ethanol plants, but are not conclusive on the direction of this shift. While HRE has caused a positive, insignificant growth in corn acres, BF is found to affect corn growth in a significantly negative manner. The negative growth in corn acres due to BF is not supported by the aforementioned economic incentives for corn production in its vicinity. We further investigate the negative treatment effects due to BF below.

To investigate the negative impact of Blue Flint on growth of corn acres in its locality, we designate alternative treatment and control groups to the east of BF and on the east of the Missouri River. Conducting this analysis with these newly designated treatment & control groups will also gauge the sensitivity and robustness of our treatment estimates. The originally designated treatment & control groups lie south of BF, but on the other side of the river than BF.¹¹ The alternative treatment and control groups are designated to the east of BF because a new ethanol plant, Dakota Spirit AgEnergy (administered by the Midwest Ag Energy Group, also the owner of BF), was established in June, 2015.¹² 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

¹¹ Both treated and control parcels to south of the BF need to cross a river bridge to reach the plant that leads Euclidean distance to be effectively much shorter than the actual distance (see table in Appendix). Therefore, the treatment effects from the alternative T & C groups should be weaker than their southern counterpart.

¹² See <http://www.midwestagenergygroup.com/dakota-spirit-agenergy>

existence of a supply-demand gap to the east of BF that led to the emergence of a new plant to bridge this gap. Our treatment effects will capture whether BF prompted an increase in corn acres among eastern land parcels. We estimate $ATT'(s, 2 | Z_i)$ for the alternative groups and present the estimation results in Tables 12 and 13.

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

Discussion and Conclusions

The Dakotas' grasslands are a valuable natural resource as they sustain livestock production and support a waterfowl breeding habitat on existing wetlands. However, the regional agricultural production significantly increased over the past decade and intensified cropping has displaced these grasslands. Alongside, most new corn-based ethanol plants started operations in the Dakotas between 2006 and 2008. This study seeks to understand the role of new ethanol plants on local corn acreage. We argue that the economic incentives due to ethanol plants are generated as reduced transportation costs and are more relevant at a local level. We utilize a unique research design to evaluate localized land use impacts for *each* ethanol plant rather than a uniform regional impact for all ethanol plants, as usually found in the literature.

We implement a quasi-experimental setting and utilize the DID estimation strategy to evaluate an ethanol plant's impact on local corn acreage, controlling for the endogeneity due to its location. The treated and untreated parcels are first matched on soil quality in order to ensure that the impact of soils on land use does not confound our DID treatment estimates. Use of DID and/or PSM for impact analyses of change/policy is rare in economic analyses of natural

resources, primarily due to unavailability of spatially explicit datasets. On a plant-by-plant basis, we find that treatment effects vary across plants and are different from a single point estimate for all ethanol plants in the region. The state-level ethanol plant impact is found to be negative, insignificant and is not consistent with the differentiated plant-level impacts.

Further, the standard parallel paths assumption of the DID fails to hold in this study. We adapt the standard DID model to a more general framework that incorporates flexible trends differentiated across groups. The updated DID model requires multiple pre-treatment periods and so we restrict our analysis to the North Dakota plants. We estimate the new treatment effects by comparing *growth* of corn acres due to the presence of ethanol plants, rather than comparing *absolute* corn acres as in case of the standard DID. The updated framework finds both positive and negative ethanol plant impacts that may be insignificant. Negative treatment effects are surprising, and difficult to reconcile with the higher incentives to grow corn in treated parcels. A spatial placebo analysis indicates that the treatment effects are identified for only two out of four ethanol plants in North Dakota.

We conclude that, although our research framework allows for a local level analysis, identifying the localized impacts is challenging. Even though we do not find definitive ethanol plant impacts, strong incremental trends in corn acres are evident across all land parcels after the 2006–08 period. Therefore, failure to detect a local effect is not inconsistent with the existence of a national-level effect of ethanol policies resulting from higher national commodity prices.

Our novel research design incorporates remotely sensed data into an applied economic analysis with quasi-experimental setting. We point towards the shortcomings of our approach. First, the Euclidean distances may not be a good representation of the ‘actual’ distances of land parcels from ethanol plants. Future analyses may consider a ‘*Nearest Facility Analysis*’ - GIS tool

to utilize an actual road network. Second, we use ad-hoc treatment and control groups with an imperfect matching strategy. In some cases the average pre-treatment trends in corn acres were weaker for the treated parcels for than the controls, which further raises concerns on our understanding of the ethanol plants' location decisions. Access to public infrastructure, grain elevators and/or other market terminals may better explain the plants' location choice. We lack such data but these factors may impact the land use decisions along with the plant locations. Overall, our results warrant further investigation into the location decisions of ethanol plants and other potential drivers of land use.

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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
<u>North Dakota</u>				
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
<u>South Dakota</u>				
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**. The North Dakota State boundary is located 15 km south of this plant and the N-S dimensions accommodate this.
- Cluster (POET and NuGen): **25 km E-W X 40 km N-S**. The rectangles excluded a circle of radius 2.5 km from NuGen, i.e. permanent development (township).
- Cluster (ABE and GLE): **25 km E-W X 50 km N-S**. The rectangles exclude a circle of radius 7 km from GLE to avoid a large water pond in land use characterization.
- Spatial Placebo: **25 km N-S X 30 km E-W**

Table 3: 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 4: 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
Spatial Placebo	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 5: 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**

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

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

Variable	RTE	BF	TE	HRE	POOLED
Intercept	8.62 (3.18)***	33.05 (5.20)***	-24.60 (5.86)***	77.38 (10.48)***	61.60 (5.33)***
$W_{i,t-1}$	0.003 (0.01)	-0.003 (0.02)	-0.02 (0.02)	-0.36 (0.04)***	-0.20 (0.02)***
$S_{i,t-1}$	0.41 (0.26)	0.09 (0.06)	0.17 (0.02)***	0.25 (0.04)***	0.17 (0.02)***
$G_{i,t-1}$	-0.02 (0.01)***	-0.07 (0.01)***	-0.09 (0.02)***	-0.27 (0.03)***	-0.19 (0.01)***
d_i	-0.20 (1.98)	-7.14 (1.56)***	11.84 (4.42)***	-28.52 (8.76)***	-9.45 (3.17)***
$I_{[t=1998]} \times d_i$	-0.07 (1.96)	12.75 (2.46)***	-9.48 (5.72)	10.50 (10.57)	17.54 (4.16)***
$I_{[t=1999]} \times d_i$	1.65 (2.15)	12.77 (2.32)***	-7.76 (6.18)	44.58 (9.91)***	21.61 (3.57)***
$I_{[t=2000]} \times d_i$	1.36 (2.17)	-0.88 (1.70)	-33.15 (5.84)***	-26.55 (9.63)***	-12.19 (3.39)***
$I_{[t=2001]} \times d_i$	-2.42 (1.90)	12.40 (2.21)***	-34.66 (7.07)***	6.32 (10.59)	4.95 (3.67)
$I_{[t=2002]} \times d_i$	-4.32 (2.01)***	3.13 (2.05)	-33.29 (7.86)***	10.78 (9.71)	4.50 (3.49)
$I_{[t=2003]} \times d_i$	-0.58 (1.97)	8.67 (1.80)***	-33.10 (6.38)***	-29.81 (11.72)***	-5.05 (3.83)
$I_{[t=2004]} \times d_i$	-5.38 (3.86)	4.23 (1.70)***	38.58 (8.60)***	30.58 (11.21)***	18.39 (3.92)***
$I_{[t=2005]} \times d_i$	-0.19 (1.90)	6.90 (2.04)***	1.23 (8.56)	68.96 (9.35)***	22.19 (3.44)***
$I_{[t=2006]} \times d_i$	0.66 (2.50)	12.89 (2.59)***	--	2.41 (9.70)	--
$I_{[t=2007]} \times d_i$	--	--	24.26 (11.18)**	23.60 (9.80)**	--

$I_{[t=2008]} \times d_i$	-2.21 (2.19)	2.53 (2.63)	-1.42 (11.95)	--	--
$I_{[t=2009]} \times d_i$	0.54 (4.29)	3.78 (2.64)	29.33 (10.22)***	41.69 (9.93)***	19.29 (3.82)***
$I_{[t=2010]} \times d_i$	3.64 (4.18)	1.71 (2.72)	22.71 (12.06)*	26.83 (10.22)***	14.60 (4.15)***
$I_{[t=2011]} \times d_i$	8.93 (4.62)*	-1.81 (3.40)	20.85 (11.85)*	14.32 (10.53)	9.90 (4.24)**
$I_{[t=2012]} \times d_i$	14.01 (12.26)	-1.80 (4.55)	46.06 (14.65)***	22.11 (11.06)**	17.87 (5.36)***
$I_{[t=2013]} \times d_i$	29.87 (8.35)***	-5.93 (4.57)	56.18 (12.84)***	27.10 (11.52)***	19.09 (5.29)***
$I_{[t=1998]}$	-4.28 (1.80)**	-12.78 (1.57)***	42.06 (4.58)***	90.67 (8.04)***	20.09 (2.93)***
$I_{[t=1999]}$	-3.03 (1.22)***	-10.55 (1.54)***	37.40 (4.44)***	37.48 (7.30)***	10.76 (2.43)***
$I_{[t=2000]}$	0.80 (1.17)	-3.23 (1.38)**	49.31 (5.21)***	81.09 (7.65)***	27.77 (2.63)***
$I_{[t=2001]}$	-1.82 (1.19)	-11.34 (1.44)***	51.44 (5.10)***	43.44 (8.36)***	15.12 (2.72)***
$I_{[t=2002]}$	-0.47 (1.41)	-3.70 (1.49)***	44.04 (5.25)***	24.51 (7.04)***	11.27 (2.46)***
$I_{[t=2003]}$	-4.53 (1.41)***	-18.59 (2.09)***	40.26 (4.38)***	53.81 (9.40)***	6.76 (3.10)**
$I_{[t=2004]}$	5.72 (2.82)**	-1.03 (1.25)	38.03 (5.08)***	60.47 (7.95)***	25.75 (2.55)***
$I_{[t=2005]}$	-2.86 (1.15)***	-4.23 (1.38)***	34.12 (5.54)***	1.76 (7.43)	4.13 (2.34)*
$I_{[t=2006]}$	3.33 (1.62)	-3.19 (1.40)**	--	51.65 (7.59)***	--
$I_{[t=2007]}$	--	--	84.25 (7.51)***	80.39 (7.54)***	--
$I_{[t=2008]}$	3.05 (1.48)**	8.45 (2.01)***	98.26 (7.88)***	--	--
$I_{[t=2009]}$	6.45 (2.81)**	8.95 (2.05)***	62.18 (6.70)***	44.99 (7.34)***	31.40 (2.56)***
$I_{[t=2010]}$	2.30 (1.92)	6.99 (1.87)***	68.43 (7.71)***	29.51 (7.49)***	23.49 (2.78)***
$I_{[t=2011]}$	3.43 (1.55)**	16.93 (2.35)***	55.92 (6.91)***	84.62 (8.33)***	41.53 (2.83)***
$I_{[t=2012]}$	20.43 (6.28)***	27.17 (2.87)	113.28 (8.93)***	80.72 (8.20)***	56.33 (3.38)***
$I_{[t=2013]}$	6.11 (3.49)*	31.02 (3.08)	111.97 (8.48)***	89.84 (8.16)***	58.94 (3.39)***
R^2	0.16	0.20	0.41	0.32	0.38

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$; -- signifies advent of the ethanol plants. S.E.s in parentheses.

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)	POOLED (2006-'08)
2007	-	-	60.38***	-	-
2008	-3.73	-16.35***	11.67	-	-
2009	1.91	-4.74	68.10***	-3.09	-6.70
2010	2.25	-8.06***	30.73*	-36.05***	-8.50
2011	4.44	-9.51***	35.50*	-33.70***	-8.50
2012	4.23	-5.97	62.56**	-13.39	4.17
2013	15.01	-10.12**	47.46**	-16.19	-2.58

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

Table 8: Estimation of the Fully-flexible DID Model for our Spatial Placebo. Dependent Var. $C_{i,t}$

Variable	TE '2006'	RTE/BF '2007'	HRE '2008'	POOLED '2006-'08'
Intercept	10.57 (4.50)**	11.69 (4.26)**	10.33 (4.33)**	11.17 (4.27)***
$W_{i,t-1}$	-0.05 (0.02)**	-0.05 (0.02)***	-0.04 (0.02)**	-0.04 (0.02)***
$S_{i,t-1}$	0.11 (0.03)***	0.09 (0.03)***	0.10 (0.03)***	0.09 (0.03)***
$G_{i,t-1}$	-0.10 (0.02)***	-0.09 (0.02)***	-0.09 (0.02)***	-0.08 (0.02)***
d_i	-2.70 (3.38)	-3.07 (3.30)	-3.13 (3.26)	-3.47 (3.19)
$I_{[t=1998]} \times d_i$	7.88 (5.70)	7.35 (5.64)	7.72 (5.71)	7.41 (5.68)
$I_{[t=1999]} \times d_i$	1.40 (5.07)	0.07 (4.88)	0.81 (4.85)	-0.04 (4.76)
$I_{[t=2000]} \times d_i$	-1.10 (4.23)	-1.21 (4.13)	-1.17 (4.19)	-1.26 (4.11)
$I_{[t=2001]} \times d_i$	21.31 (5.51)***	18.50 (5.21)***	19.83 (5.26)***	17.92 (5.05)***
$I_{[t=2002]} \times d_i$	11.10 (4.10)**	10.56 (4.01)**	10.77 (4.04)***	10.43 (3.97)***
$I_{[t=2003]} \times d_i$	-9.03 (5.28)*	-9.34 (5.02)*	-8.41 (5.07)*	-8.70 (5.01)*
$I_{[t=2004]} \times d_i$	-46.05 (8.10)***	-44.85 (7.98)***	-44.05 (7.88)***	-43.25 (7.67)***
$I_{[t=2005]} \times d_i$	23.47 (6.54)***	22.43 (6.35)***	23.32 (6.41)***	22.52 (6.27)***
$I_{[t=2006]} \times d_i$	--	-0.39 (6.57)	0.23 (6.62)	--
$I_{[t=2007]} \times d_i$	-48.70 (10.59)***	--	-48.58 (10.55)***	--

$I_{[t=2008]} \times d_i$	-36.38 (10.69)***	-36.51 (10.68)***	--	--
$I_{[t=2009]} \times d_i$	-46.32 (9.71)***	-46.40 (9.69)***	-46.27 (9.73)***	-46.26 (9.73)***
$I_{[t=2010]} \times d_i$	-59.14 (9.74)***	-59.50 (9.74)***	-59.45 (9.76)***	-59.63 (9.76)***
$I_{[t=2011]} \times d_i$	-42.83 (10.09)***	-43.20 (10.06)***	-43.11 (10.10)***	-43.38 (10.07)***
$I_{[t=2012]} \times d_i$	-81.92 (12.37)***	-82.24 (12.43)***	-81.78 (12.35)***	-81.99 (12.35)***
$I_{[t=2013]} \times d_i$	-52.30 (10.62)***	-52.52 (10.53)***	-52.27 (10.58)***	-52.46 (10.53)***
$I_{[t=1998]}$	20.53 (4.61)***	19.89 (4.55)***	19.54 (4.61)***	19.02 (4.49)***
$I_{[t=1999]}$	11.12 (4.87)**	10.32 (4.67)**	9.87 (4.56)**	9.27 (4.50)**
$I_{[t=2000]}$	13.91 (4.58)***	12.25 (4.44)**	12.22 (4.31)***	11.03 (4.17)***
$I_{[t=2001]}$	-4.54 (5.16)	-4.22 (4.81)	-5.31 (4.91)	-5.11 (4.71)
$I_{[t=2002]}$	5.18 (4.07)	4.43 (3.97)	4.21 (3.84)	3.59 (3.75)
$I_{[t=2003]}$	10.87 (5.14)**	10.36 (4.92)**	9.55 (4.88)*	9.16 (4.81)*
$I_{[t=2004]}$	37.67 (7.33)***	37.05 (7.20)***	36.09 (7.24)***	35.76 (7.04)***
$I_{[t=2005]}$	6.78 (4.77)	7.46 (4.44)*	6.32 (4.57)	6.88 (4.41)
$I_{[t=2006]}$	--	24.91 (5.28)***	24.15 (5.30)***	--
$I_{[t=2007]}$	86.02 (10.69)***	--	84.73 (10.46)***	--
$I_{[t=2008]}$	79.38 (9.56)***	78.43 (9.52)***	--	--
$I_{[t=2009]}$	84.32 (8.43)***	83.63 (8.39)***	83.46 (8.42)***	82.95 (8.42)***
$I_{[t=2010]}$	76.53 (9.32)***	76.28 (9.32)***	76.13 (9.29)***	75.92 (9.27)***
$I_{[t=2011]}$	65.38 (9.41)***	64.74 (9.39)***	64.45 (9.37)***	64.03 (9.44)***
$I_{[t=2012]}$	104.64 (11.45)***	105.19 (11.50)***	104.45 (11.39)***	104.83 (11.40)***
$I_{[t=2013]}$	64.21 (10.01)***	64.51 (9.92)***	64.14 (9.97)***	64.38 (9.93)***
R^2	0.32	0.31	0.32	0.33

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$; -- signifies advent of the ethanol plants. S.E.s in parentheses.

Table 9: Estimate of $ATT(s,2|Z_i)$ for our Spatial Placebo.

Ethanol Plant (Year Established)	TE '2006'	RTE/BF '2007'	HRE '2008'	POOLED '2006-'08'
2007	-141.69***	-	-	-
2008	-57.21***	-13.31	-	-
2009	-79.46***	12.93	51.12***	-134.54***
2010	-82.35***	9.72	35.63**	-79.14***
2011	-53.21***	39.11***	65.16***	-49.51***
2012	-108.62***	-16.22	10.14	-104.39***
2013	-39.90***	52.54***	78.32***	-36.22***

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

Table 10: T-statistic: Testing the Equivalence of n^{th} -order and $(n-1)^{\text{th}}$ -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 11: Estimate of $ATT(s,3|Z_i)$ where Equivalence Assumptions Failed (Table 10).

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 12: Estimation of the Fully-flexible DID Model for Eastern Treatment & Control Groups of the BF. Dependent Variable $C_{i,t}$. T1: 15km-40km East & C2: 85km-110km East.

Variable	BF (2007)
Intercept	1.82 (2.95)
$W_{i,t-1}$	0.04 (0.01)***
$S_{i,t-1}$	0.12 (0.03)***
$G_{i,t-1}$	-0.03 (0.01)***
d_i	1.00 (1.60)
$I_{[t=1998]} \times d_i$	2.62 (2.00)
$I_{[t=1999]} \times d_i$	-0.32 (2.06)
$I_{[t=2000]} \times d_i$	-2.63 (1.72)
$I_{[t=2001]} \times d_i$	-2.66 (2.48)
$I_{[t=2002]} \times d_i$	-3.49 (2.32)
$I_{[t=2003]} \times d_i$	-4.04 (1.93)**
$I_{[t=2004]} \times d_i$	4.10 (2.35)*
$I_{[t=2005]} \times d_i$	-6.73 (3.61)*
$I_{[t=2006]} \times d_i$	-0.17 (2.79)
$I_{[t=2007]} \times d_i$	--
$I_{[t=2008]} \times d_i$	-8.97 (4.69)*
$I_{[t=2009]} \times d_i$	-11.92 (4.64)***
$I_{[t=2010]} \times d_i$	-2.69 (4.44)
$I_{[t=2011]} \times d_i$	-3.85 (4.87)
$I_{[t=2012]} \times d_i$	-30.47 (7.84)***
$I_{[t=2013]} \times d_i$	-9.99 (7.34)
$I_{[t=1998]}$	-0.96 (1.69)

$I_{[t=1999]}$	3.05 (1.79)*
$I_{[t=2000]}$	4.23 (1.72)**
$I_{[t=2001]}$	4.45 (2.40)*
$I_{[t=2002]}$	4.65 (2.05)**
$I_{[t=2003]}$	1.81 (1.96)
$I_{[t=2004]}$	1.76 (1.75)
$I_{[t=2005]}$	9.07 (3.62)**
$I_{[t=2006]}$	4.48 (2.38)*
$I_{[t=2007]}$	--
$I_{[t=2008]}$	18.75 (4.31)***
$I_{[t=2009]}$	18.91 (4.56)***
$I_{[t=2010]}$	9.22 (3.78)**
$I_{[t=2011]}$	12.89 (4.48)***
$I_{[t=2012]}$	43.01 (7.42)***
$I_{[t=2013]}$	32.19 (5.94)***
R^2	0.20

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$; -- signifies advent of the ethanol plant. S.E.s in parentheses.

Table 13: Treatment Estimates for the **Eastern Treatment & Control Groups** of the BF.

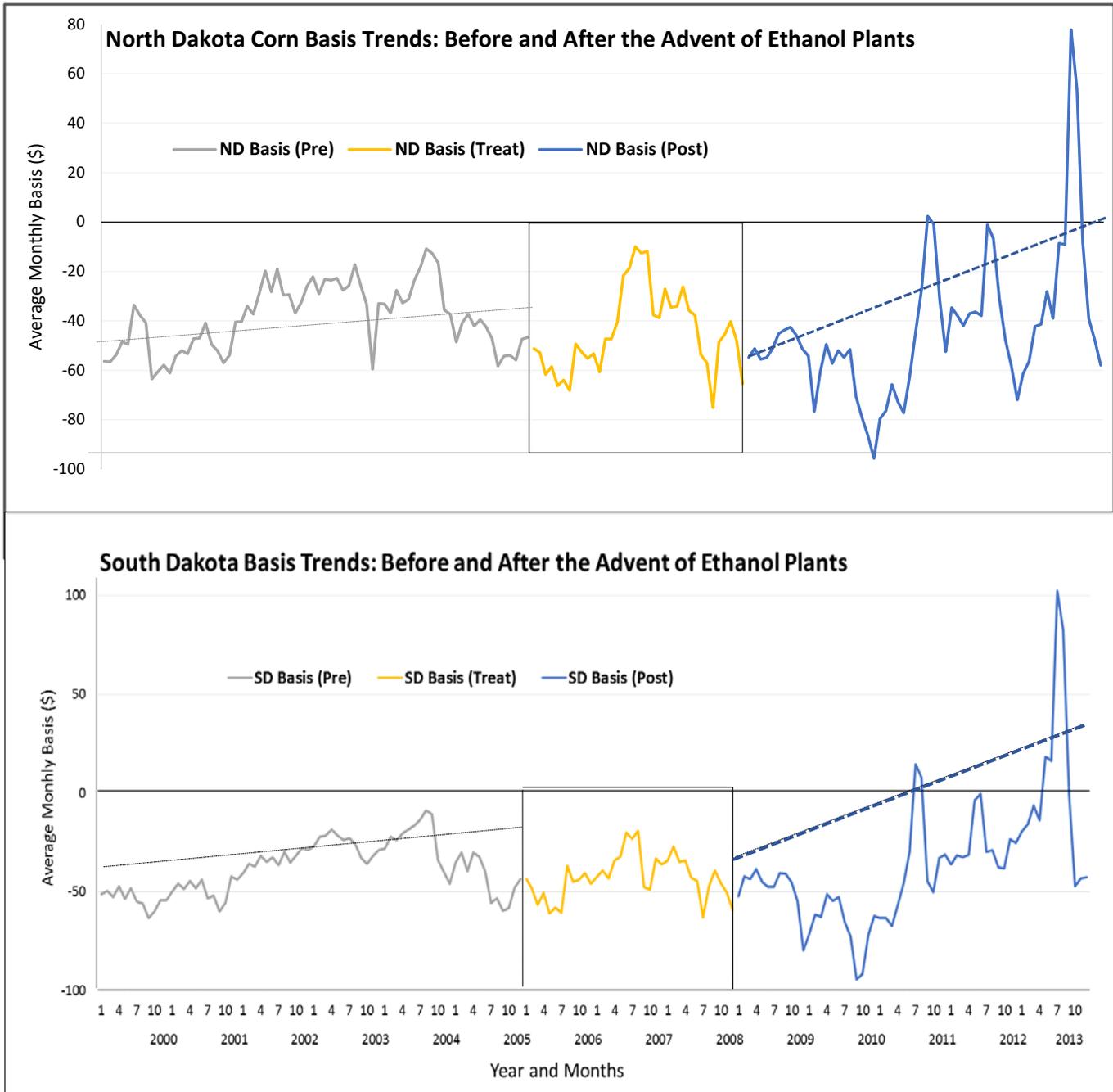
Ethanol Plant (Year Established)	$ATT'(s, 2 Z_i)$	$ATT'(s, 3 Z_i)$
2008	-15.35***	-32.73***
2009	-9.51	-11.54
2010	2.68	-5.20
2011	-7.72	-27.78*
2012	-33.17***	-42.83***
2013	13.92	29.71

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

FIGURES

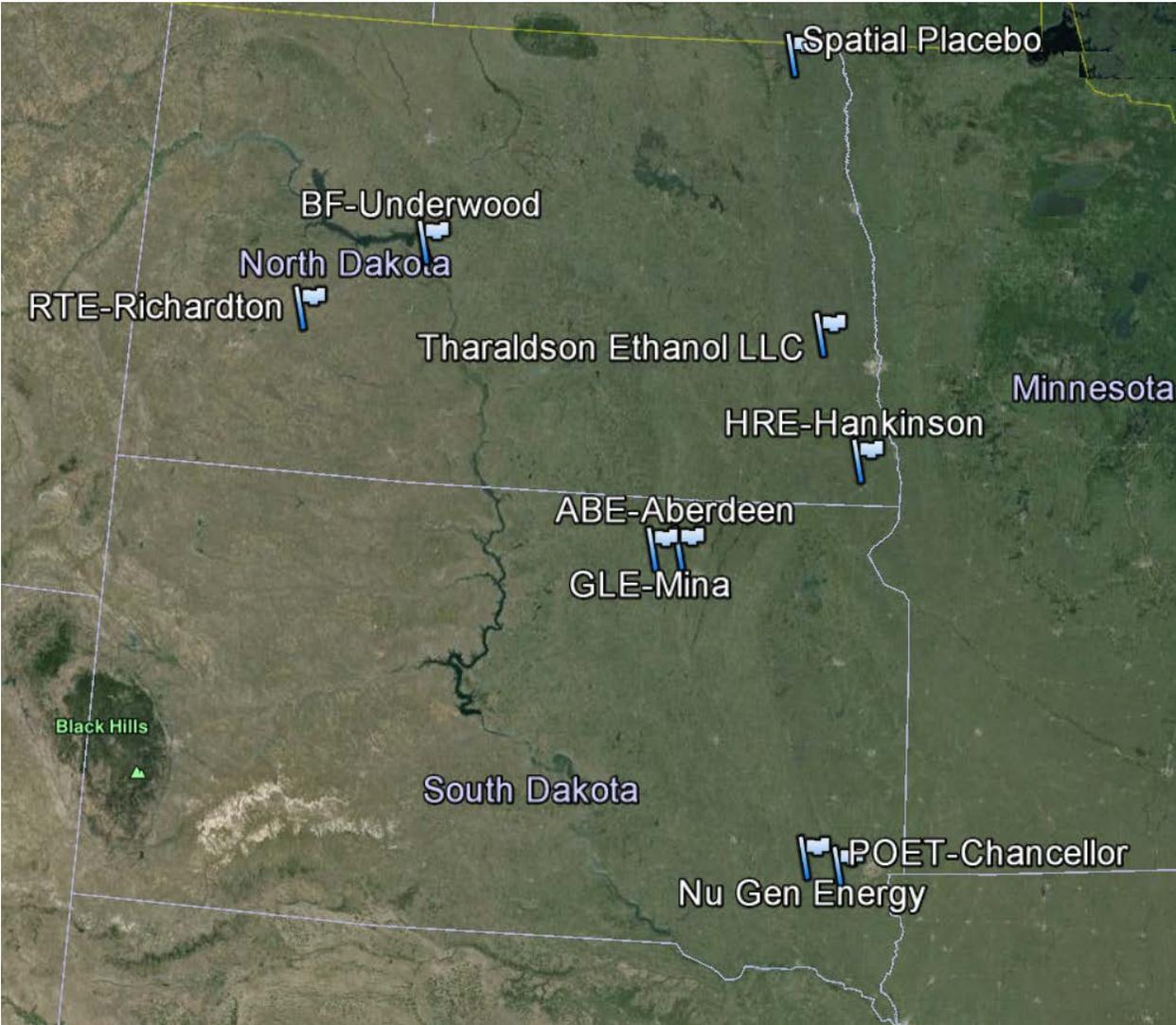
Figure 1: Comparative corn basis trends for counties that house Dakota ethanol plants that started operations in the 2006–2008 period.

*The acronym ‘treat’ denotes the period when these ethanol plants started operations, ‘pre’ (‘post’) means years prior to (after) the 2006–2008 period.



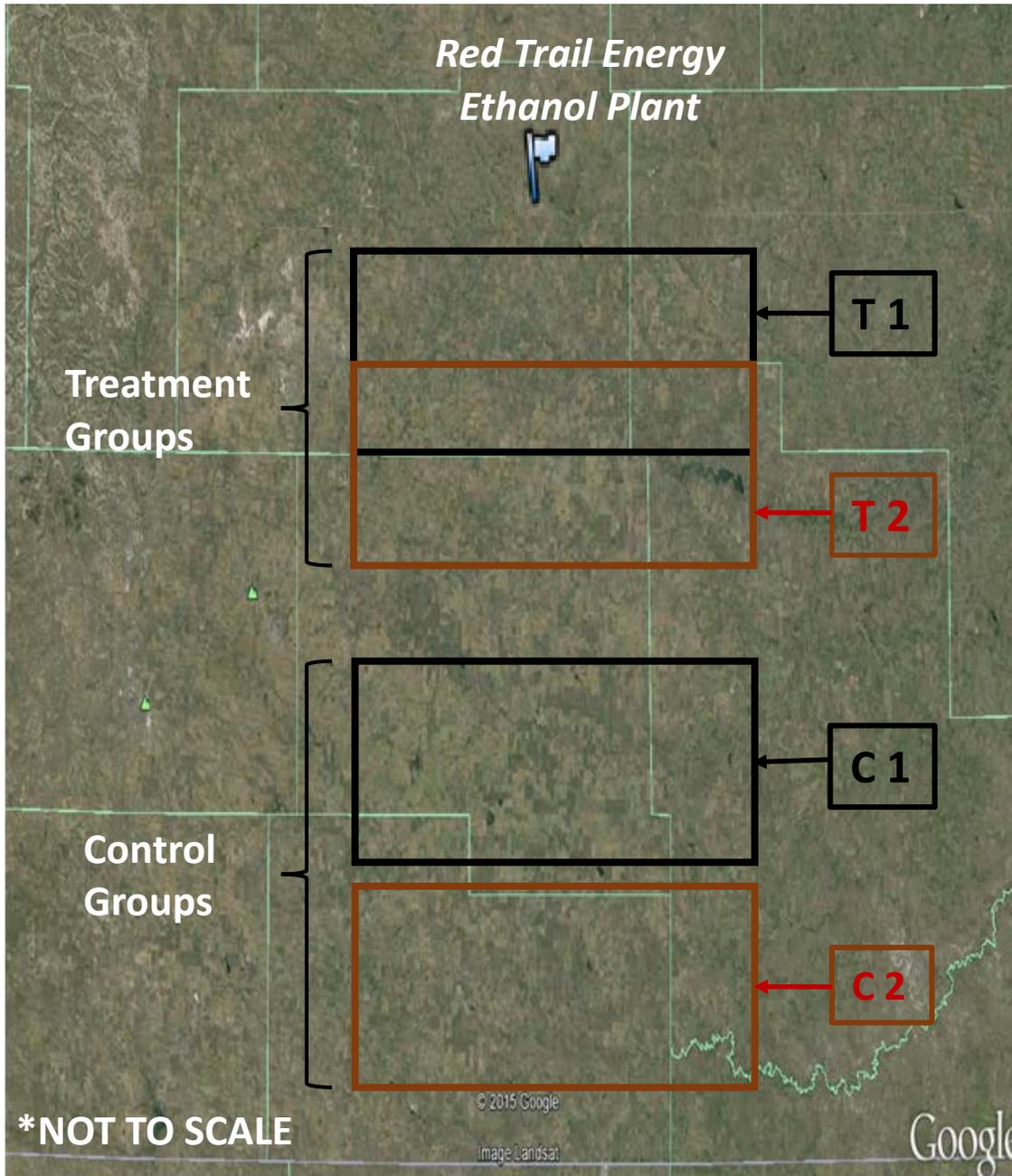
Data Source: Geo Grain

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



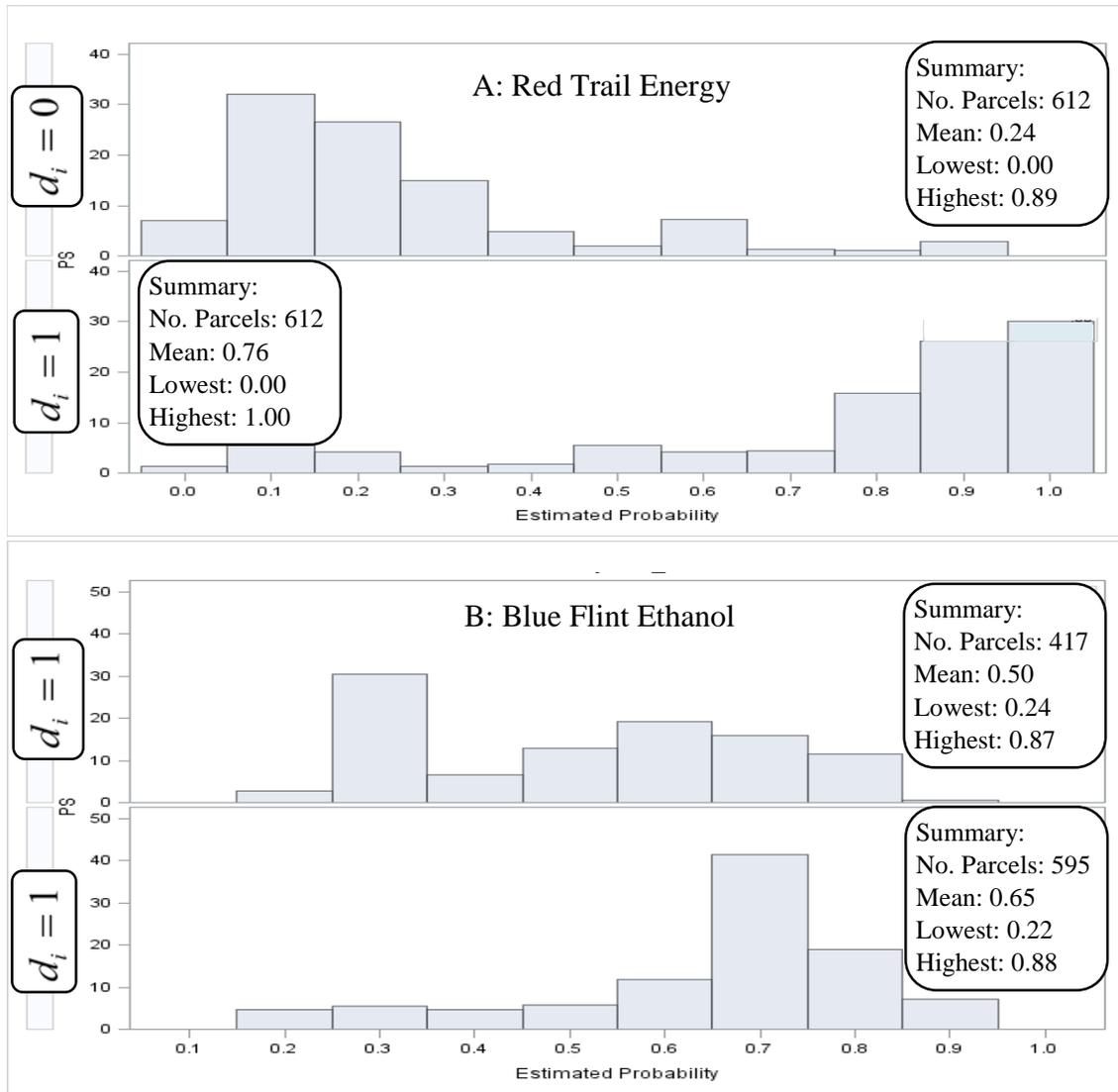
Source: “North and South Dakota.” 5122554.70 m N and 393724.99 m E. **Google Earth**. April 9, 2013. August 8, 2015.

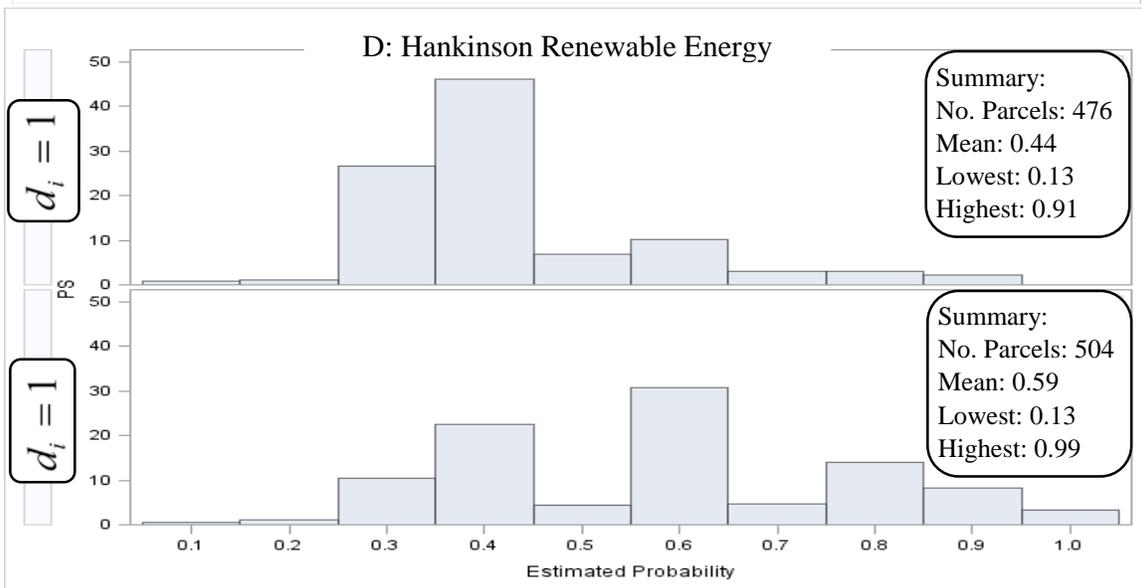
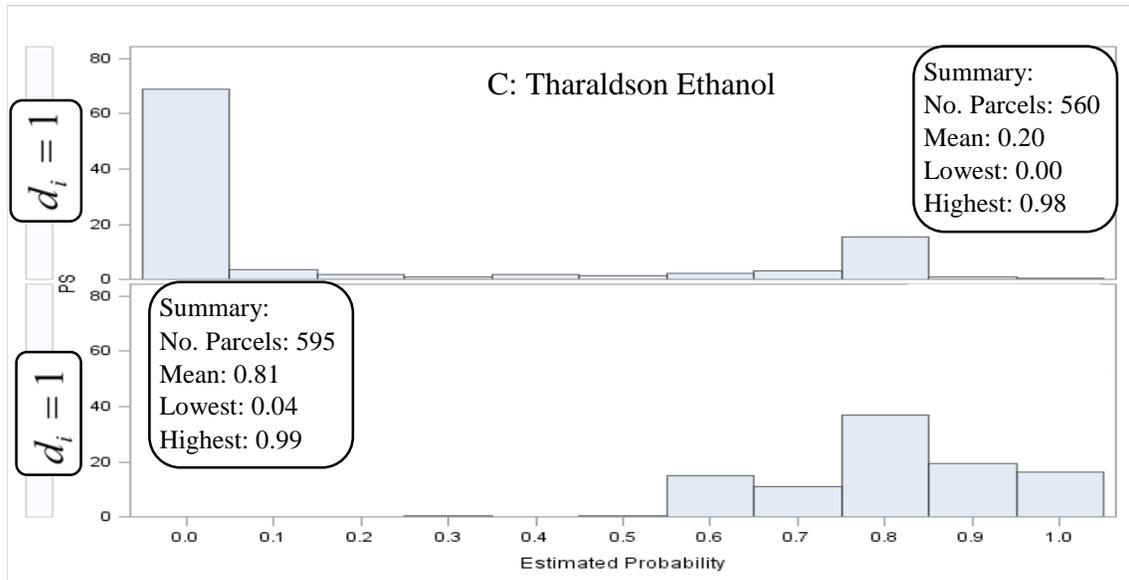
Figure 3: Schematics of treatment and control group: an example.



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.





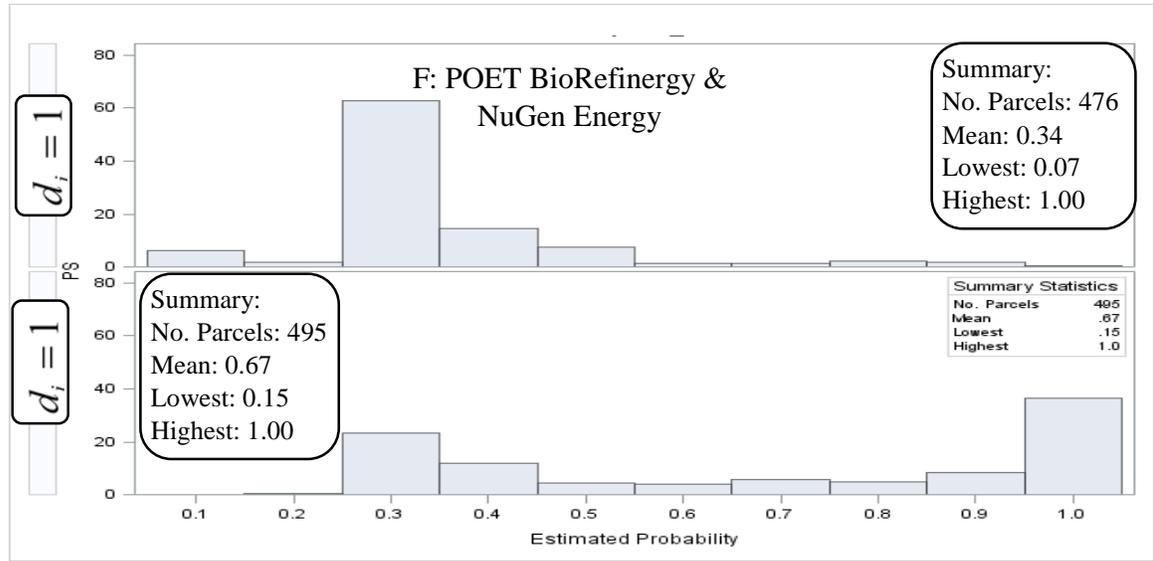
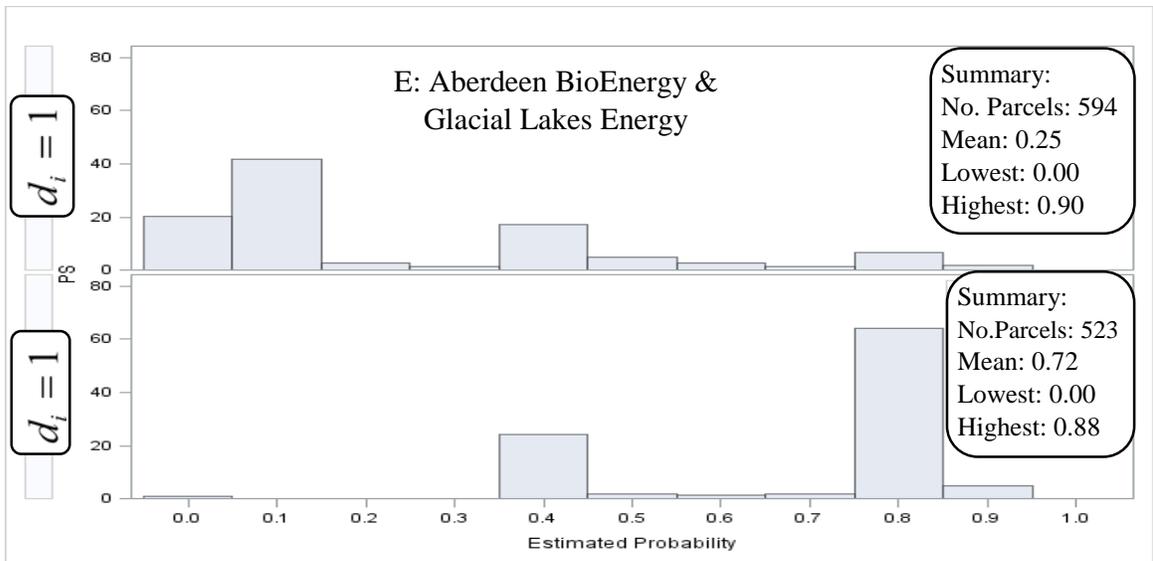


Figure 10: Temporal placebo schematics: validating the estimates from the standard DID model.

Moving Away from the Parallel Paths Assumption

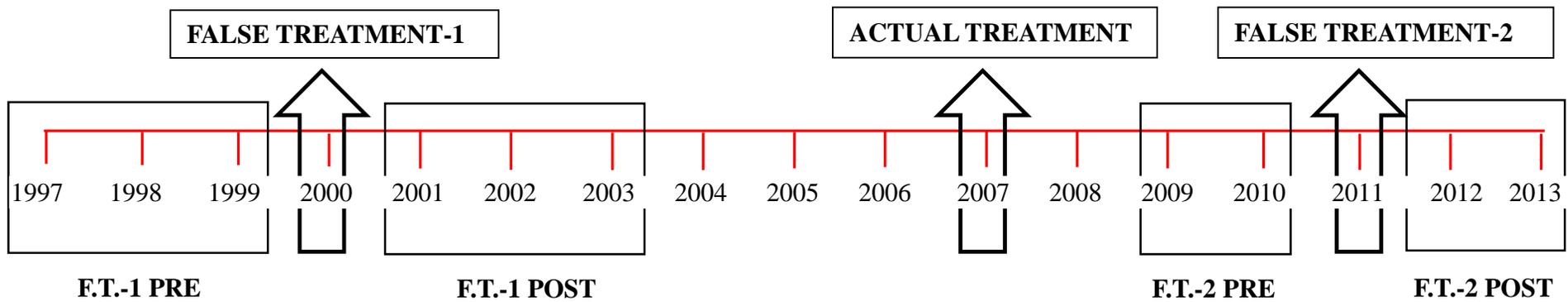


Figure 11: Average corn acre-trends for treated & control groups of the North Dakota ethanol plants. Focus: Pre-treatment trends.

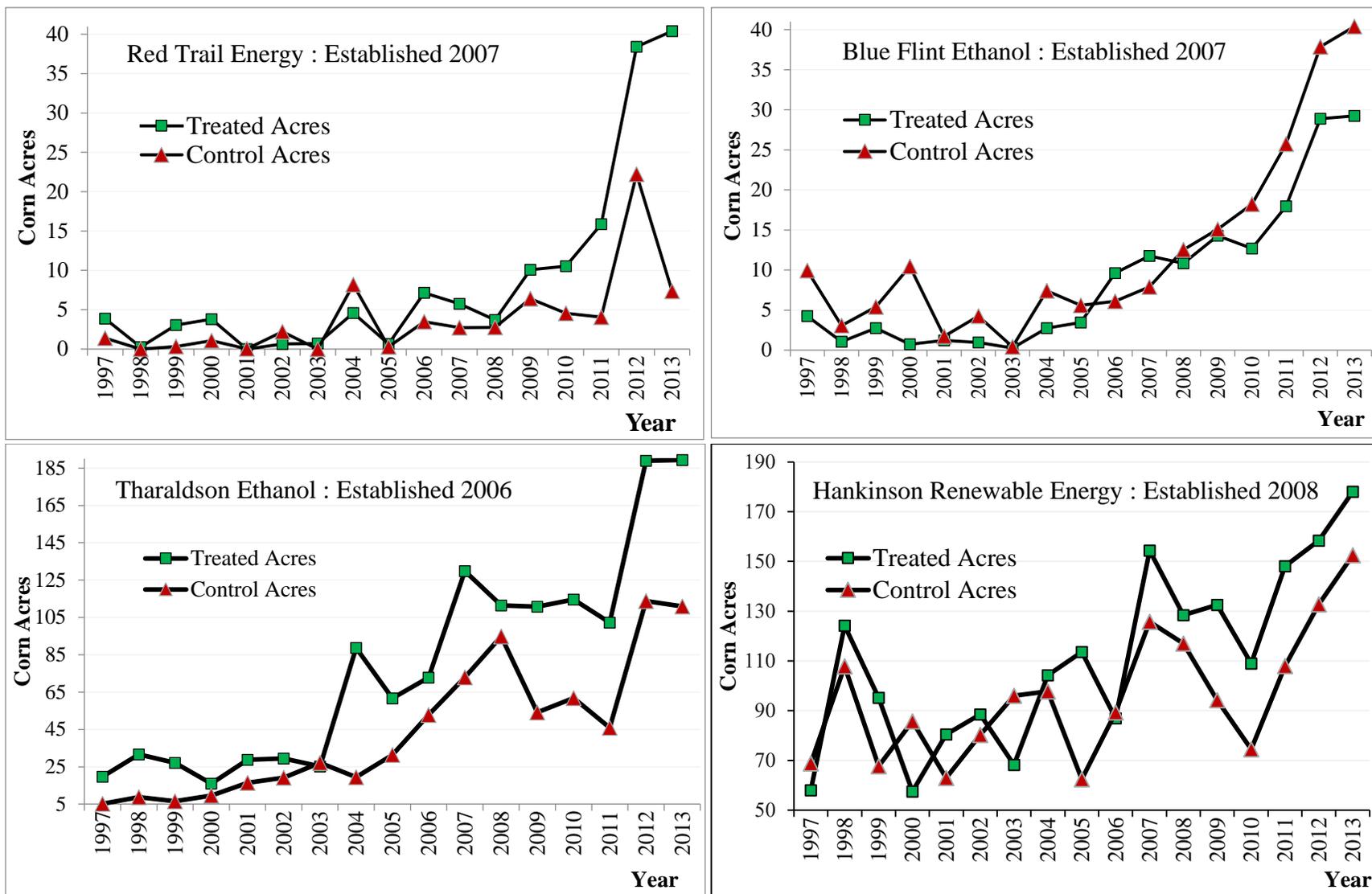
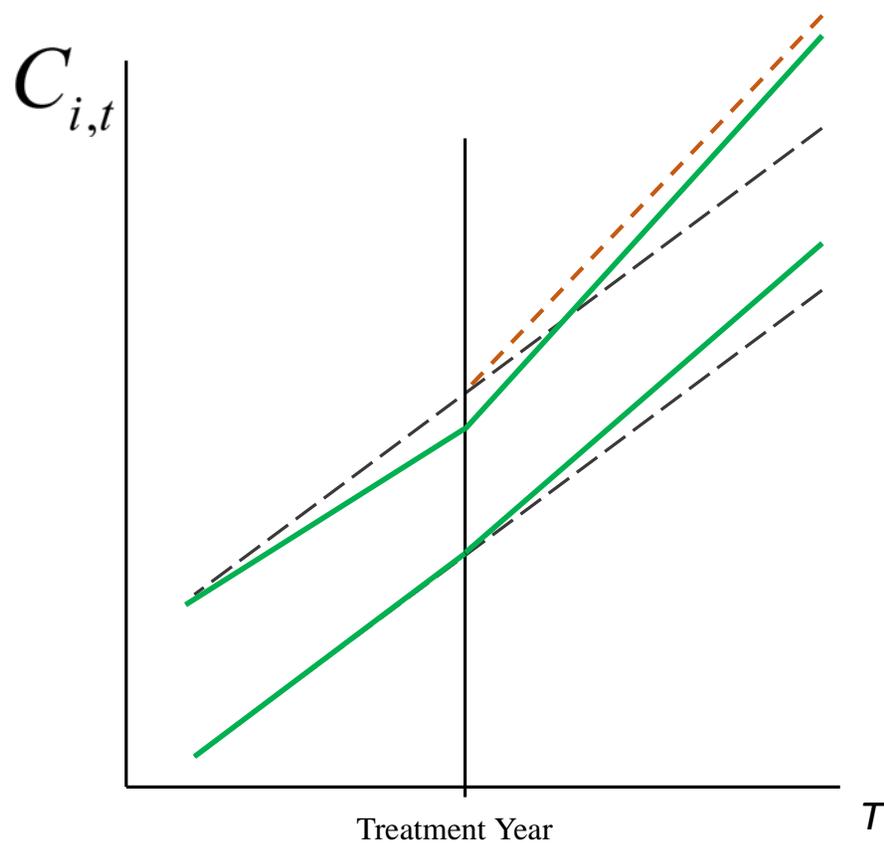
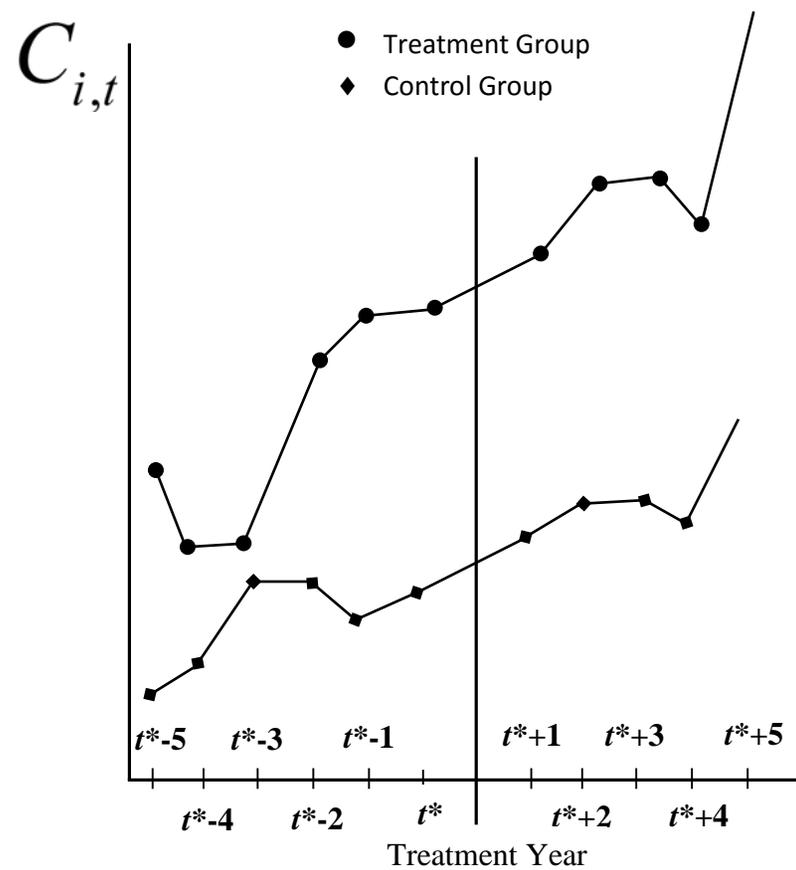


Figure 12 (a, b): The issue of non-parallel trends among treatment and control groups.



a



b

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 this process, we will exploit the variations in corn acres in multiple periods before and after the advent of an ethanol plant. Capturing differentiated trends across groups 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, 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, this section is meant to enable a smooth transition from the standard DID to the fully-flexible DID model for our readers.

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

Reconsider our equation(1), that is $C_{i,t} = \beta_0 + \beta_1\delta_t + \beta_2d_i + \beta_3d_i\delta_t + \beta_{4,t}Z_i + \varepsilon_{i,t}$, where the definitions of these variables and parameters are same as in the 'Methodology' section above.

Equation (2) implies that $ATT = E[C_{i,t^+} - C_{i,t^-} | Z_i, d_i = 1] - E[C_{i,t^+} - C_{i,t^-} | Z_i, d_i = 0]$ and so mechanics of computing the treatment effects using regression equation (1) are as under:

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

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

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

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

Hence, $ATT = \beta_3$

It is, however, critical to note that by definition the ATT equals $E[C_{i,t^+}^T - C_{i,t^+}^U | d_i = 1]$ (where superscripts $T(U)$ represent corn acres in the presence (absence) of ethanol plant in $t \in t^+$) and the parallel paths assumption must hold for β_3 to represent the impact of ethanol plants on corn acres. Figure 12 provides a visualization of the underlying implications when the parallel paths assumption fails. Basically, this assumption ensures that the treatment and control groups evolve in a parallel fashion (grey-dashed lines) and any difference in their post-treatment trends (orange- vs. grey-dashed lines) is purely due to the advent of the ethanol plant (or the treatment). This difference is β_3 . However, the process depicted by green-solid lines in Figure 12 seems more realistic in the event that the parallel paths assumptions fails to hold. That is, we are potentially dealing with the group-specific pre- and post-treatment trends. We incorporate such differentiated trends into the standard DID model below.

The DID framework with Differentiated Trends:

We motivate the implications of incorporating differentiated trends into the standard DID model through a specialized example here. 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 then move towards a generalized model proposed by Mora and Reggio's (2012) and its applicability for our analysis.

To incorporate the group-specific trends, consider the following econometric model.

$$(A.1) \quad C_{i,t} = \beta_0 + \beta_0't + \beta_1\delta_t + \beta_1't\delta_t + \beta_2d_i + \beta_2'td_i + \beta_3d_i\delta_t + \beta_3'td_i\delta_t + \beta_{4,t}Z_i + \varepsilon_{i,t},$$

Where variable t represents trends such that $t = 1$ for year =1997 (2006) for North (South) Dakota ethanol plants, which 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 earlier exercise to compute treatment effects from equation (A.1), we get

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

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

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

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

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

However, we already know that the ATT ($= \beta_3 + \beta_3't$, here) remains unidentified. Now, subtracting equation (A.1) from its one-period lagged counterpart, we have

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

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

Evidently, the mechanics of computing the treatment effects for regression equation (A.2) are similar to those of equation (1), with pertinent differences in notations of the outcome variable and the parameters. So, our 'new' average treatment effect for the treated (ATT') is given as:

$$(A.3)$$

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

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

$$(A.4) \quad ATT' = \{E[C_{i,t} - C_{i,t'} | Z_i, d_i = 1] - E[C_{i,t} - C_{i,t'} | Z_i, d_i = 0]\} - \\ \{E[C_{i,t-1} - C_{i,t'-1} | Z_i, d_i = 1] - E[C_{i,t-1} - C_{i,t'-1} | Z_i, d_i = 0]\} \quad \forall t \in t^+, t' \in t^- \text{ \& } t > t'$$

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

$ATT'(t, t' | Z) = ATT(t, t' | Z) - ATT(t-1, t'-1 | Z) \triangleq \Delta ATT(t, t' | Z) \quad \forall t \in t^+, t' \in t^- \text{ \& } t > t'$, which in turn suggests that ATT' measures the impact of treatment as a 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.

The identification of ATT' is consistent with that of the standard DID. That is, by definition, ATT' equals $E[\Delta C_{i,t}^T - \Delta C_{i,t}^U | d_i = 1, Z_i]$, where superscripts T (U) represent corn acres in presence (absence) of ethanol plant in $t \in t^+$. As with the standard DID, since $\Delta C_{i,t}^U$ is not observed for the post-treatment years, we would need an identification assumption to be able to compute ATT' as β'_3 in equation (A.2). Hence, the identification assumption for ATT' is

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

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)). The new estimator is termed 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 $ATT'(s | Z_i)$ defined s periods ahead of t^* such that that $t = t' + s$ and $t' = t^*$. Hence, the identifying assumption and $ATT'(s | Z_i)$ are given by equations (A.6) and (A.7) respectively.

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

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

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

$$\begin{aligned}
& ATT'(s | Z_i) = \{E[C_{i,t^*+s} - C_{i,t^*} | Z_i, d_i = 1] - E[C_{i,t^*+s} - C_{i,t^*} | Z_i, d_i = 0]\} - \\
(A.8) \quad & \{E[C_{i,t^*+s-1} - C_{i,t^*-1} | Z_i, d_i = 1] - E[C_{i,t^*+s-1} - C_{i,t^*-1} | Z_i, d_i = 0]\} \\
\therefore & ATT'(s | Z_i) = ATT(s | Z_i) - ATT(s-1 | Z_i)
\end{aligned}$$

Now, to evaluate the impact of ethanol plants our primary interest still lies in estimating ATT from the standard model. Since $ATT'(s | Z_i) = \beta'_3$, independent of s , the ATT can be recursively calculated for each post-treatment year as s increases by 1. That is, $ATT(s+1 | Z_i) = ATT(s | Z_i) + \beta'_3$ for $s \geq 2$. For $s = 1$, first see that $ATT(0 | Z_i) = 0$ because $E[C_{i,t^*}^T - C_{i,t^*}^U | d_i = 1, Z_i] = 0$ ¹³, which in turn yields that $ATT'(1 | Z_i) = ATT(1 | Z_i)$. Since $ATT'(1 | Z_i)$ is identified by (12) and $ATT(1 | Z_i)$ is not, we compute $ATT'(1 | Z_i)$ below.

We know that,

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

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

$d_i = 1$ 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 $ATT(1 | Z_i) = ATT'(1 | Z_i) = \beta_3 + \beta'_3(t^*+1)$. The way $ATT(1 | Z_i)$ 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 $ATT(1 | Z_i)$ as the base period has changed. However, doing this would require at least three 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:

¹³ $E[\Delta C_{i,t'}^T - \Delta C_{i,t'}^U | d_i = 1, Z_i] = E[C_{i,t'}^T - C_{i,t'}^U | d_i = 1, Z_i] = 0 \forall t' \leq t^*$. 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.

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

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 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 two advantages, when compared to the linear-trends model: (a) it incorporates flexible trends visualized in Figure 12, 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. An alternative way to incorporate flexible trends into the standard DID model would be to introduce non-linear functional forms for trends (e.g., quadratic trends). Since the fully-flexible version includes a dummy variable for each time-period, different functional forms for the non-linear trends are only special cases.

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). The parallel first-difference assumption of (A.6) that identifies $ATT'(s | Z_i)$ is re-written as follows:

$$(A.10) \quad E[\Delta_s \Delta C_{i,t^*+s}^U | Z_i, d_i = 1] = E[\Delta_s \Delta C_{i,t^*+s}^U | Z_i, d_i = 0] ,$$

Where, U represents the case of no treatment (or no ethanol plant) and $\Delta_s \triangleq (1 - L^s)$ 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 (2012) is given as:

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

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 $\Delta^2 [= (1 - L) - (L - L^2)]$ operator on the s period ahead outcome variable. We will require at least three 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 as¹⁴

(A.12)

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

For the $n = 3$ case of our linear-trends model,

$ATT'(s, 3 | Z_i) = \Delta^2 ATT(s | Z_i) = ATT(s | Z_i) - 2ATT(s-1 | Z_i) + ATT(s-2 | Z_i)$, which will recursively identify $ATT(s | Z_i) = ATT'(s, 3 | Z_i) + 2ATT(s-1 | Z_i) - ATT(s-2 | Z_i)$. Similar to the $n = 2$ case, for $s = 1, 2$ we will have $ATT(s | Z_i) = ATT'(s, 3 | Z_i)$. It is quite evident here that 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 $ATT'(s, n | Z_i) = ATT'(s, n-1 | Z_i) \forall s$) if and only if

$$E[\Delta^{n-1} C_{i,t^*}^U | Z_i, d_i = 1] - E[\Delta^{n-1} C_{i,t^*}^U | Z_i, d_i = 0] \text{ }^{15}.$$

TABLE (Appendix): Actual “Google Map” Distances of the Nearest Treatment Groups and the Farthest Control Groups from respective ethanol plants.

Ethanol Plant	Nearest Treated - T1 (km)	Farthest Control – C2 (km)	Difference (km)
RTE (South)	18.8	91.4	72.6
BF (South)	33.2	124.2	91.0
BF (East)	53.3	129.9	76.6
TE (West)	23.5	94.8	71.3
HRE (West)	18.3	97.7	79.4
POET (West)	17.2	111.2	94.0
ABE (West)	17.7	111.7	94.0

Notes: See Table 2 in the main text for schematics of the treatment and control groups.

¹⁴ See Theorem 1 in Mora and Reggio (2012).

¹⁵ See Theorem 2 in Mora and Reggio (2012).