Ethanol Plant Investment and Government Policy: A Dynamic Structural Econometric Model*

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Abstract

Ethanol has attracted considerable policy attention both for its use as a gasoline substitute, and as a way to enhance profits in rural areas. In this paper, we analyze the effects of government policy on the decisions of ethanol-producing firms to invest in building new ethanol plants in the Midwestern United States during the second US ethanol boom. To do so, we develop and estimate a dynamic structural econometric model of the ethanol plant investment timing game, and then use the estimated structural parameters to simulate counterfactual policy scenarios to explore the policy factors driving industry growth and location, and to disentangle the impacts of state and national policies on the timing and location of investment in the industry. According to our results, government policies, the intensity of corn production, and private information shocks all have significant effects on ethanol investment payoffs and decisions. Of the policies analyzed, the policies that led to most of the ethanol plant investment during this time period were the ban on the use of the oxygenate MTBE as a gasoline additive, and the 2007 Renewable Fuel Standard (RFS2).

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1 Introduction

Ethanol has attracted considerable policy attention as an environmentally-friendly alternative to imported oil; as a substitute, additive, oxygenate, and/or octane booster for gasoline (Irwin and Good, 2017); and as a way to boost farm profits and improve rural livelihoods. In the United States, several government policies that actively promote ethanol production have coincided with a boom in the construction of corn-ethanol plants, known as the second US ethanol boom, that began in the mid-1990s and hit full-stride by the early 2000s.[1] First, the Clean Air Act Amendments of 1990 mandates the use of oxygenates, which include ethanol, in gasoline. The subsequent phase out and ban of the oxygenate methyl tertiary-butyl ether (MTBE) as a gasoline additive beginning in the late 1990s further increased the demand for ethanol. Second, the Renewable Fuel Standard (RFS) mandates a minimum volume of ethanol be blended into gasoline; the initial RFS (RFS1) was created under the Energy Policy Act of 2005, and a more stringent version (RFS2) was created under the Energy Independence and Security Act of 2007 (EPA, 2021; Lade and Lin Lawell, 2021). Third, many states have offered tax credits to ethanol producers (Cotti and Skidmore, 2010). These federal and state policies have coincided with increases in petroleum prices that made ethanol more competitive as an energy substitute for gasoline (Gallagher, 2009).

In this paper, we analyze the effects of government policy on the decisions of ethanol-producing firms to invest in building new ethanol plants in the Midwestern United States during the second US ethanol boom. The decision to invest in building an ethanol plant is a dynamic decision that may be affected by economic factors and government policies. In a static model of investment, the statically optimal investment rule is to invest if the payoff from investing is positive. When investments are irreversible and there is uncertainty over the future payoff from investment, however, the statically optimal investment rule is not dynamically optimal. In particular, when the payoff from investing in building a new ethanol plant depends on uncertain market conditions and government policies that vary stochastically over time, a potential investor holds an option to invest that is lost when the irreversible investment is made. In order to make a dynamically optimal decision, a potential investor would therefore need to account for the option value to waiting before making this irreversible investment (Dixit and Pindyck, 1994).

Potential investors in ethanol plants face uncertain market conditions and government policies. The payoff from investing in building a new ethanol plant depends on market conditions such as the feedstock price that vary stochastically over time. Commodity markets occasionally exhibit broadly based massive booms and busts; at the core of these cycles is a set of contemporaneous supply and demand surprises that coincide with low inventories and that are magnified by macroeconomic shocks and policy responses (Carter, Rausser and Smith, 2011). Market volatility can induce periods of boom and bust in the ethanol industry, causing episodes of bankruptcy and reduced capital investment (Hochman, Sexton and Zilberman, 2008). In addition, potential investors

[1] The first US ethanol boom stemmed from the desire for more energy self-sufficiency in the aftermath of the oil embargoes in 1973 and 1979, and led to the construction of 153 new plants by 1985 (DOE, 2008). For a more detailed discussion of the first and second US ethanol boom, see Lin Lawell (2017).

in ethanol plants face policy uncertainty as well, including uncertainty regarding the possibility, timing, longevity, credibility, and/or extent of government policies that may support or promote ethanol (Miao, Hennessy and Babcock, 2012; Lade, Lin Lawell and Smith, 2018a; Clancy and Moschini, 2018; Markel, Sims and English, 2018; Lade, Lin Lawell and Smith, 2018b). Owing to uncertain market conditions and government policies, there is an option value to waiting before investing in building an ethanol plant that makes the decision dynamic rather than static (Dixit and Pindyck, 1994).

The dynamic decision-making problem faced by a potential ethanol investor is even more complicated when the investment payoff may be affected not only by market conditions and government policies, but also by the existence of nearby plants. There are two main channels through which existing ethanol plants may affect ethanol plant investment decisions. The first is a negative competition effect: if there is more than one ethanol plant located in the same region, these plants may compete in the local feedstock input market and/or in the local fuel ethanol output market. The second is a positive agglomeration effect: existing plants in a region may have developed transportation and marketing infrastructure and/or an educated work force from which entering plants can benefit (Lambert et al., 2008; Lin Lawell, 2017; Thome and Lin Lawell, 2022).

Due to potential competition effects and agglomeration effects, the presence of existing ethanol plants may affect the payoff from investing in an ethanol plant. Because the investment decisions of other potential investors affect the future values of state variables and the future payoff from investing in a new ethanol plant, potential ethanol investors must anticipate the investment strategies of other potential investors in order to make a dynamically optimal decision. As a consequence, a potential ethanol investor's investment decision depends on its conjecture about competitors' behavior. Uncertainty over whether a plant might be constructed and start production nearby is another reason there is an option value to waiting before investing that makes the decision dynamic rather than static (Dixit and Pindyck, 1994).

To analyze the effects of government policy on the decision to invest in building a new ethanol plant, we estimate a structural econometric model of the ethanol plant investment timing game. We use the estimated parameters from the structural model to simulate counterfactual policy scenarios to explore the policy factors driving industry growth and location, and to disentangle the impacts of state and national policies on the timing and location of investment in the industry.

A better understanding of the effects of government policy on the decisions of ethanol-producing firms to invest in building new ethanol plants is important for two main reasons. First, the promotion of expanded ethanol production is an objective of several federal and state policies in the US, and, particularly for a nascent renewable energy industry such as the ethanol industry at the onset of the second US ethanol boom, expanding production generally entails investment in new plants. A second reason why the timing and location of investment in new ethanol plants matters is that there are high transportation costs in both the feedstock and ethanol markets (Thome and Lin Lawell, 2022). Feedstock is approximately 70% of the cost of producing corn-ethanol, and transportation costs for the bulky grains constitute a significant share (Whittington, 2006). Fuel ethanol transportation is more difficult, and thus is more expensive, than gasoline transportation because ethanol can easily absorb water during the transportation process, ethanol has corrosive properties, ethanol vapor is flammable at a wider range of concentrations than gasoline, and ethanol fires cannot be put out with water (Jaehne, 2008; Truant, 2011). The number and spatial distribution of new ethanol plants therefore has important implications for the development of the ethanol industry.

There are several advantages to using a structural approach to analyzing the decision to invest in building a new ethanol plant. First, our structural model explicitly models the dynamic investment decision, including the continuation value to waiting. A potential investor invests if the payoff from investment, which we define as the expected present discounted value of the entire stream of net benefits from investing in an ethanol plant, exceeds the discounted continuation value to waiting, which captures the option value to waiting. The option value to waiting before investing in building an ethanol plant arises from uncertainty regarding market conditions, government policies, and whether another plant might be constructed nearby. A second advantage of our structural model is that we are able to estimate the effect of each state variable on the payoff from investing in an ethanol plant. While the parameters in reduced-form models are confounded by continuation values, we model the structural relationship between the continuation value to waiting and the payoff from investment, which enables us to estimate parameters in the payoff from investing in building an new ethanol plant, including parameters measuring the effects of government policy.[2] A third advantage of a structural model is that the parameter estimates from the structural model can be used to simulate counterfactual scenarios. We use the estimated parameters from the structural model to run counterfactual simulations to explore the policy factors driving industry growth and location, and to disentangle the impacts of state and national policies on the timing and location of investment in the industry.

In our dynamic structural model, government policies affect the decision-making problem faced by a potential investor through several channels. First, government policies affect the payoff from investing in an ethanol plant. Second, expectations and uncertainty about future values of

[2]The entry and location determinants identified in previous reduced-form models of ethanol plant entry and location decisions (Goetz, 1997; Sarmiento, Wilson and Dahl, 2012; Lambert et al., 2008; Haddad, Taylor and Owusu, 2010; Cotti and Skidmore, 2010; Thome and Lin Lawell, 2022) provide a starting point for our analysis as far as identifying potentially important state variables to include in our structural model. The previous literature on ethanol investment also includes studies that analyze the viability of ethanol plants (Whims, 2002; Gallagher et al., 2006; Eidman, 2007; Ellinger, 2007; Richardson et al., 2007; Richardson, Lemmer and Outlaw, 2007; Gallagher, Shapouri and Brubaker, 2007; Dal-Mas et al., 2011; Jouvet, Le Cadre and Orset, 2012; Markel, Sims and English, 2018), the most profitable plant size under different market conditions (Gallagher, Brubaker and Shapouri, 2005; Gallagher, Shapouri and Brubaker, 2007; Khoshnoud, 2012), ethanol plant investment option values (Schmit, Luo and Tauer, 2009; Gonzalez, Karali and Wetzstein, 2012), and the effects of government policies (Babcock, 2011; Babcock, 2013; Herath Mudiyanselage, Lin and Yi, 2013; Bielen, Newell and Pizer, 2018; Ghoddusi, 2017; Yi and Lin Lawell, 2025a; Yi and Lin Lawell, 2025b; Yi, Lin Lawell and Thome, 2025). A related literature examines the Renewable Fuel Standard (de Gorter and Just, 2009; Lapan and Moschini, 2012; Holland et al., 2014; Chen et al., 2014; Lade and Lin Lawell, 2015; Wu and Langpap, 2015; Skolrud et al., 2016; Lemoine, 2016; Moschini, Lapan and Kim, 2017; Just, 2017; Skolrud and Galinato, 2017; Korting and Just, 2017; Lade, Lin Lawell and Smith, 2018a; Korting, de Gorter and Just, 2019; Lade, Lin Lawell and Smith, 2018b; Irwin, McCormack and Stock, 2020; Landry and Bento, 2020; Afkhami and Ghoddusi, 2020; Lade and Lin Lawell, 2021).

government policies affect a potential investor's decision because they affect the expected payoffs from investing in the future and therefore the option value to waiting; this channel is captured in the continuation value to waiting. Third, government policies affect the evolution of economic factors, which include the ethanol price and the availability and cost of corn, and therefore the expected payoffs from investing in the future; this channel is captured in the transition densities for the economic factors and affects the continuation value to waiting. Fourth, government policies affect the decisions of other potential investors, which affect the expected payoffs from investing in the future and are again captured in the continuation value to waiting.

According to our results, government policies, particularly the ban on the use of the oxygenate MTBE as a gasoline additive at the state level, and the 2007 Renewable Fuel Standard (RFS2) at the federal level, have significant effects on ethanol investment payoffs and decisions. The intensity of corn production at the county level and private information shocks have significant effects on ethanol investment payoffs and decisions as well. We use the estimated structural parameters to simulate counterfactual policy scenarios to disentangle the impacts of state and national policies on the timing and location of investment in the industry. We find that, of the policies analyzed, the MTBE ban and the RFS2 led to most of the ethanol plant investment during this time period.

The balance of our paper proceeds as follows. We present our structural econometric model in Section 2. We describe our data in Section 3. We present our results in Section 4. We run our counterfactual simulations in Section 5. Section 6 discusses our results and concludes.

2 Dynamic Structural Econometric Model

2.1 Ethanol Plant Investment Timing Game

We model the dynamic and strategic decision faced by a potential investor (or entrant)[3] $i \in \{1, ..., n_{kt}\}$ of whether to invest in building an ethanol plant in county k in year t. I_{ikt} is an indicator of whether potential investor i invests in building a new ethanol plant in county k in year t. Investment in an ethanol plant is irreversible and, in each year t and each county k, all investment decisions by all n_{kt} potential investors in county k in year t are made simultaneously.

The publicly observable state of county k in year t is given by $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$, a vector of discrete and finite-valued state variables that are observed by all the potential investors in county k as well as by the econometrician. N_{kt} is a dummy variable for whether there is an existing plant in the county; G_{kt} describes the policy environment; and X_{kt} are economic factors. The state variables $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$ describe the current environment and summarize the direct effect of the past on the current environment.

The state variables in G_{kt} describe the policy environment faced by the corn-ethanol industry. State and federal policies can affect the expected payoff from investing in building a new

[3]Because we are modeling the decision to invest in building a new ethanol plant, we use the terms 'investor' and 'entrant' interchangeably.

ethanol plant through the cost of inputs, expected revenues, and building costs. At the federal level, we include indicators for the two versions of the Renewable Fuel Standard (RFS1 and RFS2), which are implemented as blending mandates. At the state level, we include the year the MTBE ban was implemented; MTBE was a popular oxygenate used to meet environmental regulations and also to boost octane level, and ethanol is a substitute for MTBE. We also include state-level tax credits for ethanol producers.

From the perspective of potential investors, the evolution of these government policies over time and their exact timing were uncertain and could not have been perfectly anticipated. We therefore model future values of these policies as uncertain from the point of view of potential investors in any given year of our period of study in our dynamic structural model. In particular, we assume that these government policies evolve as a finite state first-order Markov process $G_{k,t+1} \overset{iid}{\sim} F_G(\cdot|\Omega_{kt})$, and that a potential investor's expectations of future values of these government policies depend on current values of these policies and on current values of other state variables, including economic factors X_{kt}. We use empirical probabilities to estimate a potential investor's (conditional) expectation of future values of these policies, conditional on current values of these policies and on current values of other state variables.

While we allow for uncertainty in the evolution and exact timing of government policies, since we use empirical probabilities to estimate the transition density for government policies, our empirical transition density rules out some policy scenarios that we never see in the data. For example, for policies such as the MTBE ban and the RFS2 that, once implemented, are never subsequently removed in any future year during the time period of our analysis, our empirical transition density will show that, once that policy is in place, the probability of that policy being in place again in the next year is 1, since in the data the empirical probability of that policy being in place again the following year is 1. Similarly, since the RFS2 is never implemented prior to the RFS1 in the data, our empirical transition density will show that the probability of the RFS2 being in place next year when the RFS1 is not in place this year is 0. Likewise, since the RFS1 and the RFS2 are never in place at the same time in the data, our empirical transition density will show that the probability of the RFS1 and the RFS2 being in place at the same time is 0.

The state variables in X_{kt} include economic factors that affect the payoffs from investing in building an ethanol plant. On the revenue side, we include ethanol price; gasoline price; and proximity to cattle, which is a proxy for sales price of distillers' dried grains with solubles (DDGS), a co-product of corn-ethanol production that is used for animal feed.[4] The gasoline price could have a positive or negative impact on investment depending on whether ethanol is viewed as an energy substitute for gasoline or as a gasoline additive (oxygenate and/or octane booster), respectively.

The vector X_{kt} of economic factors also includes state variables describing the cost of ethanol production. One important factor is the availability and cost of corn, the primary feedstock in the

[4] The co-product market is becoming more significant due to lower prices for ethanol (Dhuyvetter, Kastens and Boland, 2005). There is significant variability in participation in co-product markets (Perrin, Fretes and Sesmero, 2009). Participation is driven by mill type and plant age; wet mills (corn syrup) and dry mills (DDGS) produce different co-products (DOE, 2008).

region of focus and the largest variable cost in ethanol production (Kwiatkowski et al., 2006; Perrin, Fretes and Sesmero, 2009); local availability is important because transportation is costly (USDA, 2007). We include the natural gas price because natural gas is a major energy source for milling corn. We include a metro area indicator in order to capture proximity to market and transportation costs.[5]

The vector X_{kt} of economic factors also includes soy price and whether there is existing biodiesel production capacity in county k at the start of year t because biodiesel and ethanol plants may compete indirectly in the feedstock market: while biodiesel production uses soy as a feedstock, much of the Midwest can be planted to soy or corn.[6]

We model the future values of economic factors as uncertain from the point of view of potential investors. In particular, we assume that the economic factors evolve as a finite state first-order Markov process $X_{k,t+1} \overset{iid}{\sim} F_X(\cdot|\Omega_{kt})$, and that a potential investor's expectations of future values of economic factors depend on the current market conditions and on current values of other state variables, including government policies G_{kt}. We use empirical probabilities to estimate a potential investor's (conditional) expectation of future values of economic factors, conditional on current values of economic factors and on current values of other state variables.

As explained in more detail in Section 3, all of the government policies G_{kt} and almost all of the economic factors X_{kt} in our data are measured at aggregate levels that include many counties k and potential investors i. For example, all of the government policies G_{kt} are at either the state or federal level. Similarly, corn prices and natural gas prices are at the state level. The only economic factor measured at the county level in our data set that we use in our preferred specification is corn intensity; in their reduced-form analysis of country-level ethanol plant entry decisions during the second US ethanol boom, however, Thome and Lin Lawell (2022) cannot reject the exogeneity of county-level corn intensity in any specification. We therefore assume that the government policies G_{kt} and economic factors X_{kt} are exogenous from the point of view of an individual potential investors and that an individual potential investor i's investment decision in an individual county k does not impact government policies G_{kt} or economic factors X_{kt}.

Although we assume that an individual potential investor's investment decisions do not impact government policies or economic factors, we allow government policies to affect the evolution of economic factors, and economic factors to affect the evolution of government policy. In particular, we assume that the economic factors evolve as a finite state first-order Markov process $X_{k,t+1} \overset{iid}{\sim} F_X(\cdot|\Omega_{kt})$, and that a potential investor's expectations of future values of economic factors depend on the current values of economic factors and on current values of other state variables, including government policies. We similarly assume that the government policies evolve as a finite state first-order Markov process $G_{k,t+1} \overset{iid}{\sim} F_G(\cdot|\Omega_{kt})$, and that a potential investor's expectations of future

[5] The modeling of transportation infrastructure investment decisions, which affect transportation costs and may be endogenous at the county level, and which have been studied elsewhere (Fatal et al., 2012), is beyond the scope of this paper. In lieu of explicitly modeling transportation costs, we include a metro area indicator in order to capture proximity to market and transportation costs.

[6] We describe our data in more detail in Section 3.

values of the government policies depend on current values of these policies and on current values of other state variables, including economic factors. We use empirical probabilities to estimate a potential investor's (conditional) expectation of future values of government policies and future values of economic factors, conditional on current values of state values, including government policy and economic factors.

The dynamic decision-making problem faced by a potential investor is even more complicated when the investment payoff is affected not only by market conditions and government policies, but also by the existence of nearby plants. Due to potential competition effects and agglomeration effects (Lin Lawell, 2017; Thome and Lin Lawell, 2022), the presence of existing ethanol plants may affect the payoff from investing in building a new ethanol plant. As a consequence, a potential investor's investment decision depends on its conjecture about competitors' behavior. In particular, potential investors may condition their investment decisions on both whether there is an existing ethanol plant in the county N_{kt} as well as their expectations on what the future values of N_{kt} may be. Future values of N_{kt} may be different from current values if other potential investors invest in building a new ethanol plant in a given year.

Since N_{kt} is a dummy variable for whether there is an existing ethanol plant in county k at the start of period t, and is therefore pre-determined before the time-t investment decision is made, it is not endogenous. Because of the time necessary to construct a plant, a potential investor necessarily observes any previously existing plants before deciding whether to invest.[7]

As explained in more detail in Section 3, almost all of the economic factors X_{kt} in our data are measured at aggregate levels that include many counties k. Because we do not have local variation in ethanol, gasoline, or natural gas prices, local competition in the ethanol and gasoline output markets and in the gasoline and natural gas input markets are captured by the dummy variable N_{kt} for whether there is an existing ethanol plant in the county.

We model the future values of whether there is an existing ethanol plant in the county as uncertain from the point of view of potential investors. In particular, we assume that whether there is an existing ethanol plant in the county evolves as a finite state first-order controlled Markov process, and that a potential investor's expectations $N_{k,t+1} \overset{iid}{\sim} F_N(\cdot|\Omega_{kt}, I_{ikt} = 0)$ of future values of whether there is an existing ethanol plant in the county, conditional on the investor not investing this period, depend on whether there is currently an existing ethanol plant in the county and on current values of other state variables, including government policies G_{kt} and economic factors X_{kt}. We use empirical probabilities to estimate a potential investor's (conditional) expectation of future values of whether there is an existing ethanol plant in the county, conditional on whether there is currently an existing ethanol plant in the county, on current values of other state variables, and on the potential investor not investing this period.

[7] As explained in more detail in Section 3, the dummy for existing plants N_{kt} in the county is a dummy variable for whether there is an operational plant in that county on January 1 of year t, and is therefore observable to any potential investor making a decision in year t. We do not distinguish between whether there are 1 or 2 incumbent plants for state space considerations, and because very few counties had 2 or more ethanol plants. Only 1 county had 3 ethanol plants in 2008, the final year of our analysis.

We use counties to delineate the set of existing ethanol plants and potential investors that may strategically interact in our ethanol plant investment timing game because the county delineation yields markets with geographical areas commensurate with the extent of local competition. Owing to high transportation costs in both the feedstock and ethanol markets, competition among neighboring plants is localized (McNew and Griffith, 2005; Lambert et al., 2008; Sarmiento, Wilson and Dahl, 2012; Zhang and Irwin, 2007; Thome and Lin Lawell, 2022), and the competition effect decays with distance (Sarmiento, Wilson and Dahl, 2012; Thome and Lin Lawell, 2022). In their reduced-form analysis of ethanol plant entry during the second US ethanol boom, Thome and Lin Lawell (2022) find that existing plants have a significant negative effect on the probability of entry in a given county, but that existing plants in neighboring counties do not. Thus, the geographical extent of local competition in the feedstock input market and the ethanol output market is unlikely to be larger than the size of markets defined at the county level.

In addition to the observable state variables $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$, the decision of a potential investor i of whether to invest in building an ethanol plant in county k in year t also depends on a shock ε_{ikt}, which is private information to potential investor i and unobserved by either other potential investors or by the econometrician. Such private information may include, for example, a shock to the cost of building an ethanol plant. We assume the error term is independently and identically distributed exponentially with mean σ, which is among the parameters to be estimated.

The equilibrium concept used in the model is that of a Markov perfect equilibrium. Each potential investor is assumed to play a Markov "state-space" strategy: the past influences current play only through its effect on the state variables. A potential investor's dynamically optimal investment policy is then the Markov strategy that it plays in the Markov perfect equilibrium, which is a profile of Markov strategies that yields a Nash equilibrium in every proper subgame (Fudenberg and Tirole, 1998).

While each potential investor's time-t investment decision depends on both the publicly observable state variables Ω_{kt} as well as the potential investor's own private information ε_{ikt}, its perception of other potential investors' time-t investment decisions depend only on the publicly observable state variables Ω_{kt}. This is because, owing to the above assumptions on the observable state variables and on the unobservable shocks, potential investors can take expectations over their competitors' private information.[8] In equilibrium, potential investors' perceptions of their competitors' investment probabilities should be consistent with those that are actually realized (Starr and Ho, 1969).

The model has at least one Markov perfect equilibrium, and each equilibrium generates a finite state Markov chain in Ω_{kt} tuples (Pakes, Ostrovsky and Berry, 2007).[9] Because we estimate our model using data that is pooled across all counties, we assume that the data are generated

[8] While each potential investor plays a pure strategy, from the point of view of their competitors, they appear to play mixed strategies. Thus, as with Harsanyi's (1973) purification theorem, a mixed distribution over actions is the result of unobserved payoff perturbations that sometimes lead potential investors to have a strict preference for one action, and sometimes a strict preference for another.

[9] A Markov chain is a Markov process on a finite state space (Stokey, Lucas and Prescott, 1989).

by a single Markov perfect equilibrium, and therefore that the same equilibrium is played in each county. If a mixed strategy equilibrium is played, then we assume that the same mixed strategy equilibrium is played in each county. Under these assumptions, for large enough samples, the data will pick out the correct equilibrium and the estimators for the parameters in the model will be consistent (Pakes, Ostrovsky and Berry, 2007). When observed games are drawn from a population that is culturally or geographically close and shares similar norms and conventions, as perhaps can be argued is the case for agricultural counties in the Midwestern United States, one would expect that it is adequate to assume that the same equilibrium is played across games (de Paula, 2013). Moreover, following Bresnahan and Reiss (1990, 1991), to reduce the number of potential equilibria and thereby further mitigate the issue of multiple equilibria, we assume that all the firms are symmetric conditional on the state variables and that they produce a homogeneous good, and therefore that a potential investor's investment decision depends on whether there is an existing ethanol plant in the county N_{kt}, rather than the identity of the existing plants.[10]

The payoff $\pi(\Omega_{kt}, \varepsilon_{ikt}; \theta)$ from investing in an ethanol plant in county k in year t, which represents the expected present discounted value of the entire stream of net benefits from investing in an ethanol plant in county k in year t, can be separated into a deterministic component and a stochastic component as follows:

$$\pi(\Omega_{kt}, \varepsilon_{ikt}; \theta) = \pi_0(\Omega_{kt}; \theta) + \varepsilon_{ikt}, \tag{1}$$

where the deterministic component $\pi_0(\cdot)$ is linear in the state variables:

$$\pi_0(\Omega_{kt}; \theta) = N'_{kt}\gamma_N + G'_{kt}\gamma_G + X'_{kt}\gamma_X, \tag{2}$$

and where $\theta = (\gamma_N, \gamma_G, \gamma_X, \sigma)$ denotes the parameters to be estimated. The coefficients γ_N, γ_G, and γ_X measure the effects of the state variables N_{kt}, G_{kt}, and X_{kt}, respectively, on the payoff to investing in building a new ethanol plant.

The dynamic optimization problem faced by a potential investor i is to choose the investment strategy to maximize the investor's expected present discounted value. The value function $V(\Omega_{kt}, \varepsilon_{ikt}; \theta)$ for a potential investor i in county k in year t, which is the expected present discounted value from following the dynamically optimal investment strategy, can be written as:

$$V(\Omega_{kt}, \varepsilon_{ikt}; \theta) = \max\{\pi(\Omega_{kt}, \varepsilon_{ikt}; \theta), \beta V^c(\Omega_{kt}; \theta)\}, \tag{3}$$

[10] If a long panel is available, it may sometimes be possible to estimate the policy functions separately for each market (Bajari, Benkard and Levin, 2007). Since our study involves a large number of markets (870) and a small number of time periods (13), however, and due to the scarcity of long panel data, we instead assume that the pooled data are generated by the same equilibrium. Otsu, Pesendorfer and Takahashi (2016) propose statistical tests for finite state Markov games to examine whether data from distinct markets can be pooled. Unfortunately, their test is not applicable to our context since their test performs well for moderate values of the number of markets (e.g., 20 or 40), while our study involves a large number of markets (870) and a small number of time periods (13). As seen in their Monte Carlo results when multiple equilibria are possible with non-zero probability in their Tables 3 and 4, their test does not perform well in their simulations in which the number of markets is closest to that in our study (i.e., 640) and the number of time periods is closest to that in our study (i.e., 10).

where β is the discount factor and $V^c(\cdot)$ is the continuation value. The continuation value $V^c(\cdot)$ is the expected value of the next period's value function, conditional on not building an ethanol plant in the current period, and is given by:

$$V^c(\Omega_{kt}; \theta) = E[V(\Omega_{k,t+1}, \varepsilon_{ik,t+1}; \theta) | \Omega_{kt}, I_{ikt} = 0], \tag{4}$$

where the expectation is taken over the values of the state variables $\Omega_{k,t+1}$ next period and the private information $\varepsilon_{ik,t+1}$ next period, conditional on the state variables Ω_{kt} this period, and conditional on not investing this period ($I_{ikt} = 0$). The distribution of state variables $\Omega_{k,t+1}$ next period, conditional on the state variables Ω_{kt} this period, and conditional on not investing this period ($I_{ikt} = 0$), is given by:

$$F_\Omega(\Omega_{k,t+1} | \Omega_{kt}, I_{ikt} = 0) = F_G(G_{k,t+1} | \Omega_{kt}) F_X(X_{k,t+1} | \Omega_{kt}) F_N(N_{k,t+1} | \Omega_{kt}, I_{ikt} = 0). \tag{5}$$

In a static model of investment, the statically optimal investment rule is to invest if the payoff $\pi(\cdot)$ from investing is greater than 0. When investments are irreversible and there is uncertainty over the future payoff from investment, however, the statically optimal investment rule is not dynamically optimal. In particular, since the state variables Ω_{kt} (which describe economic factors, the policy environment, and whether there is an existing ethanol plant in the county) evolve stochastically over time, it is possible that the state variables may take on values in the future that yield a payoff $\pi(\cdot)$ that is high enough that the potential investor would do better in expected present discounted value to wait rather than make the investment now, even if the payoff $\pi(\cdot)$ now is positive. A potential investor who hopes to make a dynamically optimal decision would therefore need to account for the option value to waiting before making this irreversible investment (Dixit and Pindyck, 1994).

As seen in Equation (3) for the value function $V(\Omega_{kt}, \varepsilon_{ikt}; \theta)$ for a potential investor, the dynamically optimal investment policy is for the potential investor to invest in building an ethanol plant in year t if and only if the payoff $\pi(\cdot)$ from investing exceeds the discount factor β times the continuation value $V^c(\cdot)$ to waiting. Because the continuation value to waiting $V^c(\cdot)$ is positive, the dynamically optimal investment rule, which accounts for the option value to waiting, has a higher threshold $\beta V^c(\cdot)$ for the payoff from investment to exceed before an investment is made compared to the static investment rule, whose threshold is 0. The option value to waiting before investing in building an ethanol plant arises from uncertainty regarding market conditions, government policies, and whether another plant might be constructed nearby, as captured by the stochastic evolution of the state variables Ω_{kt}. Thus, our structural model, which is dynamic, is more appropriate than a reduced-form model, which does not explicitly model either the continuation values or the option value to waiting.

Government policies G_{kt} affect the decision-making problem faced by a potential investor through several channels. First, as seen in Equation (1), government policies G_{kt} in year t affect the payoff $\pi(\Omega_{kt}, \varepsilon_{ikt}; \theta)$ from investing in an ethanol plant in county k in year t. Second, expectations and uncertainty about future values of government policies affect a potential investor's decision via the transition density $F_G(G_{k,t+1}|\Omega_{kt})$, which, as seen in Equations (4) and (5), affects the continuation value to waiting $V^c(\cdot)$. Third, government policies G_{kt} affect the evolution of economic factors X_{kt}, which include the ethanol price and the availability and cost of corn, through the transition density $F_X(X_{k,t+1}|\Omega_{kt})$, which, as seen in Equations (4) and (5), in turn affects the continuation value to waiting $V^c(\cdot)$. Fourth, government policies G_{kt} affect the decisions of other potential investors, and therefore the evolution of future values of whether there is an existing ethanol plant in the county N_{kt}, as captured by the transition density $F_N(N_{k,t+1}|\Omega_{kt}, I_{ikt} = 0)$, which, as seen in Equations (4) and (5), again affects the continuation value to waiting $V^c(\cdot)$.

Let $g(\Omega_{kt};\theta)$ denote the probability of investing in an ethanol plant at time t, conditional on the publicly observable information Ω_{kt} at time t, but not on the private information ε_{ikt}. The investment choice probability $g(\Omega_{kt};\theta)$ is then given by:

$$g(\Omega_{kt};\theta) \equiv \Pr\left(I_{ikt} = 1|\Omega_{kt};\theta\right) = \Pr\left(\varepsilon_{ikt} : \pi(\Omega_{kt}, \varepsilon_{ikt};\theta) > \beta V^c(\Omega_{kt};\theta)\right). \tag{6}$$

Evaluating Equation (4) for the continuation value $V^c(\cdot)$ using the exponential distribution for ε_{ikt} and Equation (1) for the investment payoff $\pi(\cdot)$, we obtain (Pakes, Ostrovsky and Berry 2007; Lin, 2013):

$$V^c(\Omega_{kt};\theta) = E[\beta V(\Omega_{k,t+1}, \varepsilon_{ik,t+1};\theta) + \sigma g(\Omega_{k,t+1};\theta)|\Omega_{kt}, I_{ikt} = 0]. \tag{7}$$

Similarly, evaluating Equation (6) for the investment choice probability $g(\cdot)$ using the exponential distribution for ε_{ikt} and Equation (1) for the investment payoff $\pi(\cdot)$, we obtain (Pakes, Ostrovsky and Berry 2007; Lin, 2013):

$$g(\Omega_{kt};\theta) \equiv \Pr\left(I_{ikt} = 1|\Omega_{kt};\theta\right) = exp\left(-\frac{\beta V^c(\Omega_{kt};\theta) - \pi_0(\Omega_{kt};\theta)}{\sigma}\right). \tag{8}$$

For a potential investor i in county k who decides to invest in building an ethanol plant in year t, we define the welfare $w_e(\cdot)$ of that investor (entrant) as the expected current-value payoff that i receives from investing, where the expectation is taken over the private information ε_{ikt}, as follows:

$$w_e(\Omega_{kt};\theta) = E[\pi(\Omega_{kt}, \varepsilon_{ikt};\theta)|\Omega_{kt}] = N'_{kt}\gamma_N + G'_{kt}\gamma_G + X'_{kt}\gamma_X + \sigma. \tag{9}$$

The expression for entrant welfare $w_e(\cdot)$ incorporates both the deterministic part of the payoff from investing, $\pi_0(\Omega_{kt};\theta) = N'_{kt}\gamma_N + G'_{kt}\gamma_G + X'_{kt}\gamma_X$, as well the mean of the private shock $E[\varepsilon_{ikt}] = \sigma$. As the focus of our paper is on ethanol investment, our definition for entrant welfare $w_e(\cdot)$ focuses on the welfare to the entrant, and therefore does not include consumer surplus, environmental benefits, or other possible components of social welfare.

2.2 Econometric Estimation

Among the advantages of estimating a dynamic structural econometric model is that doing so enables one to explicitly model the dynamic investment decision, including the continuation value to waiting; to estimate the effect of each state variable on the payoff from investing in an ethanol plant; and to run counterfactual policy simulations.[11] We use a structural econometric model of a dynamic game developed by Pakes, Ostrovsky and Berry (2007), which has been applied to analyze the multi-stage investment timing game in offshore petroleum production (Lin, 2013) and peer effects in health promotion programs in developing countries (Ma et al., 2025).[12] We employ a two-step semi-parametric estimation procedure.

Let n_{tuple} denote the number of combinations (or tuples) of state variables. In other words, n_{tuple} is the number of different values of the vector $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$ of discrete and finite-valued state variables.

In the first step in the estimation, we estimate a transition matrix M to capture potential investors' expectations $F_{\Omega}(\Omega_{k,t+1}|\Omega_{kt}, I_{ikt} = 0)$ about the evolution of the state variables $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$ over time, conditional on not investing. In particular, the transition matrix M is an n_{tuple} by n_{tuple} matrix that gives, for each combination of state variables in year t, the probability of transitioning to each combination of state variables in year *t+1* conditional on not investing in year t. The element in each row r, column c of the transition matrix M is given by: $M_{rc} = \Pr(\Omega_{k,t+1} = c|\Omega_{kt} = r, I_{ikt} = 0)$. We estimate M non-parametrically using empirical averages. We therefore assume rational expectations on the part of potential ethanol investors, namely that their expectations about the evolution of state variables over the time period of our data set were consistent with the actual evolution realized.

Let $\bar{g}$ be the vectorized investment policy function, which is a vector whose length is the number of combinations n_{tuple} of state variables and whose value $\bar{g}(\Omega_{kt})$ at each component is the investment policy function $g(\cdot)$ evaluated at the respective combination Ω_{kt} of state variables. Each component $\bar{g}(\Omega_{kt})$ of the vectorized investment policy function $\bar{g}$ gives the probability of investment

[11] Dynamic structural econometric models, including those developed by Rust (1987, 1988) and Hotz et al. (1994) have been adapted for many applications, including related applications to energy (Rapson, 2014; Cullen, 2015; Cullen et al., 2017; Cook and Lin Lawell, 2020; Feger et al., 2020; Langer and Lemoine, 2022; Weber, 2022; Bradt, 2024; Butters, Dorsey and Gowrisankaran, 2025), transportation (Donna, 2021; Li, Liu and Wei, 2022; Gillingham et al., 2022), agriculture (Scott, 2013; Carroll et al., 2019; Yeh, Gómez and Lin Lawell, 2025; Carroll et al., 2025b; Meneses et al., 2025; Sambucci, Lin Lawell and Lybbert, 2025; Carroll et al., 2025a), resource management (Timmins, 2002; Aguirregabiria and Luengo, 2016; Reeling, Verdier and Lupi, 2020), environmental regulations (Blundell, Gowrisankaran and Langer, 2020; Toyama, 2024), and forestry (Oliva et al., 2020; Araujo, Costa and M. Sant'Anna, 2021; Wu et al., 2025).

[12] Other structural econometric models of dynamic games include those developed by Aguirregabiria and Mira (2007), Bajari, Benkard and Levin (2007), de Paula (2009), Bajari et al. (2015), Pesendorfer and Schmidt-Dengler (2008), Srisuma and Linton (2012), Egesdal, Lai and Su (2015), Adusumilli and Eckardt (2020), and Dearing and Blevins (2025). Related applications of structural econometric models of dynamic games include applications to environmental policy (Ryan, 2012; Fowlie, Reguant and Ryan, 2016; Yi, Lin Lawell and Thome, 2025; Zakerinia and Lin Lawell, 2025), energy (Lim and Yurukoglu, 2018; Gerarden, 2023; Jha, 2023; Gowrisankaran, Langer and Reguant, 2025; Kheiravar, Lin Lawell and Jaffe, 2025), government policy (Barwick, Kalouptsidi and Zahur, 2025), resource management (Huang and Smith, 2014; Sears, Lin Lawell and Walter, 2025; Sears et al., 2025a; Sears et al., 2025b), and development (Rojas Valdés, Lin Lawell and Taylor, 2025).

in a new ethanol plant for the respective observed state of the world Ω_{kt}. We estimate $\bar{g}$ using empirical averages.

From Equation (7), the vectorized continuation value $\bar{V}^c$, which is a vector whose length is the number of combinations n_{tuple} of state variables and whose value at each component is the continuation value $V^c(\cdot)$ evaluated at the respective combination Ω_{kt} of state variables, can be specified in vector form as:

$$\bar{V}^c = M(\beta \bar{V}^c + \sigma \bar{g}), \tag{10}$$

where M is the empirical transition matrix, β is the discount factor, and $\bar{g}$ is the vector of empirical investment probabilities. Because this is an infinite horizon problem, we estimate the continuation value by solving Equation (10) for the fixed point $\hat{V}^c$, which, from Blackwell's Theorem, is unique.

We then apply the estimate $\hat{V}^c$ to Equation (8) to form the vectorized predicted probability of investment in an ethanol plant, $\hat{g}$, which, given the parameters θ, is a vector whose length is the number of combinations n_{tuple} of state variables and whose value $\hat{g}(\Omega_{kt};\theta)$ at each component is the investment policy function $g(\cdot)$ evaluated at the respective combination Ω_{kt} of state variables and at the vector of parameters θ. Each component $\hat{g}(\Omega_{kt};\theta)$ of the vectorized predicted probability of investment $\hat{g}$ gives the probability of investment in a new ethanol plant for the respective observed state of the world Ω_{kt} given the parameters θ. From Equation (8), $\hat{g}$ can be specified in vector form as:

$$\hat{g} = exp\left(-\frac{\beta \hat{V}^c - N'_{kt}\gamma_N - G'_{kt}\gamma_G - X'_{kt}\gamma_X}{\sigma}\right). \tag{11}$$

In the second step of the estimation procedure, we estimate the parameters $\theta = (\gamma_N, \gamma_G, \gamma_X, \sigma)$ by finding the parameters that best match the investment probability predicted by our model with the respective empirical investment probabilities in the data using generalized method of moments (GMM). We use the following moment function:

$$\psi = (\hat{g}(\Omega_{kt};\theta) - \bar{g}(\Omega_{kt}))n(\Omega_{kt}|I_{ikt-1} = 0), \tag{12}$$

where $n(\Omega_{kt}|I_{ikt-1} = 0)$ counts the number of times each state $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$ occurs where there is a potential investor. Additional moments are constructed by interacting the above moments ψ with the state variables Ω_{kt}. The GMM estimator $\hat{\theta}$ is the solution to the problem:

$$\min_{\theta}\left(\frac{1}{obs}\sum\psi\right)W_n^{-1}\left(\frac{1}{obs}\sum\psi\right), \tag{13}$$

where obs is the number of potential investor-county-year observations. Because the system is exactly identified, we use an identity matrix as the weight matrix W_n.[13]

Identification of the parameter σ governing the distribution of private information ε_{ikt} is similar to the identification of the entry parameter in Pakes, Ostrovsky and Berry (2007): it comes

[13] One challenge is determining whether the model has converged at a global or local minimum. We experimented with several combinations of starting values to initialize the parameters to be estimated. We found the model is robust to the starting value.

from the realized investment frequencies, and in particular the moments that match the predicted investment probabilities with the actual probabilities in the data. Identification of the parameters $\gamma = (\gamma_N, \gamma_G, \gamma_X)$ in the payoff from investing in an ethanol plant comes from variation in the state variables $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$ and investment decisions I_{ikt} across county-years, and in particular the moments that match the predicted and actual investment probabilities when these probabilities are interacted with the state variables. As explained below, since our dynamic discrete choice model only identifies relative values of the coefficients in the investment payoff relative to the mean σ of the private shock, and does not separately identify the magnitudes of the coefficients in the investment payoff and the mean σ of the private shock, we focus on interpreting the signs, statistical significance, and relative magnitudes of the parameters, rather than their absolute magnitudes. Moreover, since our dynamic discrete choice model only identifies relative welfare values, not absolute values, and since welfare is therefore unitless, we normalize welfare so that the mean welfare per entrant of the No Policy scenario is equal to 1.

We form standard errors by a nonparametric bootstrap. We randomly draw counties from the data with replacement to generate 250 independent panels of size equal to the actual sample size. The structural econometric model is run on each of the new panels. The standard error is then formed by taking the standard deviation of the estimates from each of the random samples.[14]

3 Data

We focus on investments in corn-ethanol plants[15] in the Midwestern United States, where the majority of corn (and ethanol from corn) is produced, over the period 1996 to 2008.[16] We focus in particular on the following ten states: Iowa, Illinois, Indiana, Kansas, Minnesota, Missouri, Nebraska, Ohio, South Dakota, and Wisconsin. For our econometric analysis, we eliminate completely

[14]The problem of spatially correlated unobservables can be addressed by interpreting the investment payoff in the model as expected investment payoff conditional on observables, where the expectation is taken over the correlated unobservables. The model is still able to separately identify the (expected) strategic interaction from the correlated unobservables. In the online Appendix to Lin (2013), Lin (2013) conducts Monte Carlo experiments to analyze the effect of a state variable that is observed by firms when they make their decisions but unobservable to the econometrician (i.e., a common shock), and shows that the bias introduced by spatially correlated unobservables is small. This is consistent with Pakes, Ostrovsky and Berry (2007), who find that the bias from serially correlated common shocks is small.

[15]While ethanol is produced throughout the United States using various feedstocks, 95% of the ethanol produced in this time frame is produced from corn. Focusing on corn-ethanol plants eliminates the need to consider feedstock choice in the model. For structural econometric models of feedstock choice, see Yi and Lin Lawell (2025b), who model ethanol investment and feedstock choice in Europe; and Yi and Lin Lawell (2025a), who model ethanol investment and feedstock choice in Canada.

[16]We focus on the time period 1996 to 2008, which corresponds to the second ethanol boom in the US. This time period is narrow enough to allow us to use one set of policy variables, as well as ensure similarity in plant technology. Starting the analysis earlier would also be difficult because plant startup and closure information is not readily available before this date. Including the entrants during 2009 and 2010 would require accounting for plant closure due to the market crash and implosion of Verasun, a large producer. Many plants stopped production in late 2008 or early 2009 following Verasun's bankruptcy declaration on October 31, 2008. Operations were normal the rest of the year, and many of the shuttered plants have since restarted under new ownership. Prior to 2008, there was only one permanent closure (exit) in the sample; others closures were the result of accidents or buyouts, and the plants returned to normal operations. The exit phenomenon in a subject of ongoing work and is outside the scope of this model.

non-agricultural counties within the ten states (e.g., northern Minnesota), as well as those with missing data on agricultural production.

We use counties to delineate the set of existing ethanol plants and potential investors that may strategically interact in our ethanol plant investment timing game because the county delineation yields markets with geographical areas commensurate with the extent of local competition. Owing to high transportation costs in both the feedstock and ethanol markets, competition among neighboring plants is localized (McNew and Griffith, 2005; Lambert et al., 2008; Sarmiento, Wilson and Dahl, 2012; Zhang and Irwin, 2007; Thome and Lin Lawell, 2022), and the competition effect decays with distance (Sarmiento, Wilson and Dahl, 2012; Thome and Lin Lawell, 2022). In their reduced-form analysis of ethanol plant entry during the second US ethanol boom, Thome and Lin Lawell (2022) find that existing plants have a significant negative effect on the probability of entry in a given county, but that existing plants in neighboring counties do not. Thus, the geographical extent of local competition in the feedstock input market and the ethanol output market is unlikely to be larger than the size of markets defined at the county level.

3.1 Plant Variables

Our ethanol plant data set includes information about start-up date of new entrants, and nameplate capacity and ownership type for new and existing plants. The original list of operational plants was obtained online from the Renewable Fuels Association and Ethanol Producer magazine, including historical lists from the Renewable Fuels Association.[17] The sample begins with 22 operational plants at the start of 1996, and ends with 149 operational plants with a total capacity of almost 10 billion gallons per year in 2008.

The investment variable I_{ikt} is an indicator of whether potential investor i invests in building a new ethanol plant in county k in year t.[18] The maximum number of ethanol plants in any county in our data set during the time period of our data set is three. Thus, for the number of potential investors n_{kt}, we allow for up to 3 potential ethanol investors per county-year. The investment variable I_{ikt} is equal to 1 if the plant enters in a given calendar year.[19] Once a potential investor i invests, it is no longer a potential investor and therefore exits the sample. The dummy for existing plants N_{kt} in the county is a dummy variable for whether there is an operational plant in that county on January 1 of year t, and is therefore observable to any potential investor making a decision in year t. We do not distinguish between whether there are 1 or 2 incumbent plants for state space considerations, and because very few counties had 2 or more ethanol plants. Only 1

[17]These lists do not match perfectly. We were able to rectify inconsistencies between the two lists as well as collect additional information on plant owners by searching through plant websites, newspaper articles, and SEC filings.

[18]Though the start-up month for new plants is available, we use annual observations for three reasons. First, the feedstock of focus, corn, has one growing season in the US. Second, construction of an ethanol plant takes significantly longer than a month, but usually less than a year, from the start of physical construction activities. There was a production bottleneck in 2007, when plants took 18-24 months to build (Koplow, 2007). We do not consider announcements of new plants, as other studies did, because many announced projects were never completed as investors fell through before construction began. Finally, much of the data on other variables are publicly available at an annual level.

[19]Entry is the date of the first grind of corn, which is the first step in corn-ethanol production.

county had 3 ethanol plants in 2008, the final year of our analysis.

3.2 Policy Variables

We include two state-level policy variables and two federal-level policy variables. The first state-level policy variable we use is an indicator of whether the state banned MTBE at any point in a given year. The first states in our sample banned MTBE as early as 2000. All the Midwestern states in our sample implemented MTBE bans by 2005, before the nationwide ban took effect in 2006.

The second state-level policy variable represents the state producer tax credits.[20] Defining the state producer tax credit variable is complicated because each state places different contingencies on receiving these funds. For example, some states support only large-capacity plants, others only small or community-owned plants. Thus, even in states with tax credits, not all entering or incumbent plants qualify. In addition, some of the credits are available for a specified number or years, while others expire on a date unrelated to time of plant entry. Because of these differences, we represent these policies with a binary variable indicating if producer tax credit benefits were offered to plants that entered in that year.[21]

At the federal level, we include indicators for the two versions of the Renewable Fuel Standard (RFS1 and RFS2). The RFS was created under the Energy Policy Act of 2005 with the goal of accelerating the use of fuels derived from renewable sources (EPA, 2021). This initial RFS (RFS1) mandated that a minimum of 4 billion gallons of ethanol be blended into gasoline in 2006, rising to 7.5 billion gallons by 2012. Two years later, the Energy Independence and Security Act of 2007 greatly expanded the biofuel mandate volumes, creating the RFS2. The RFS2 requires steadily increasing volumes of biofuel to be blended into the nation's fuel supply, reaching 37 billion gallons a year by 2022. We model RFS1 with an indicator for the years 2005 and 2006 and RFS2 as an indicator for the years 2007 and onwards.[22]

3.3 Economic Variables

Corn and soy prices are available annually from the National Agricultural Statistics Service of the USDA (NASS) at the state level. Corn production and acreage are available annually by county

[20] The American Coalition for Ethanol (2007) provides detailed description and review of the policies. Cotti and Skidmore (2010) study state-level impacts of these policies.

[21] We hope in future work to quantify the stringency and extent of various state tax credit policies in order to further examine the effects of government policies on ethanol plant investment decisions.

[22] We do not include other federal-level policy variables such as the tax credit or the small producer subsidy in the analysis because they do not vary enough in the time period to identify the effects. We hope in future work to quantify the stringency and extent of various state tax credit policies and combine the various state tax credit policies with the federal ethanol tax credit in order to further examine the effects of government policies on ethanol plant investment decisions. In our ongoing work in Yi, Lin Lawell, and Thome (2025), for example, we empirically analyze how government subsidies and the Renewable Fuel Standard (a form of a fuel mandate) affect ethanol production, investment, entry, and exit by estimating a structural econometric model of a dynamic game. We use the estimated parameters to evaluate three different types of subsidy – a production subsidy, an investment subsidy, and an entry subsidy – each with and without the Renewable Fuel Standard.

from NASS. Because counties are different areas, we construct a county corn intensity variable, defined as the corn acreage divided by the total area of the county, to capture area-independent acreage using county acreage from the US Census. Because corn price data are not publicly available at a county level, the local competition in the corn feedstock market is captured both by the county-level corn intensity variable and by the dummy variable N_{kt} for whether there is an existing ethanol plant in the county.

To represent the potential market for distillers' grains (DDGS), a co-product of corn-ethanol production that is used for animal feed, we construct a district-level cow density variable using the number of cows per district-acre, where the number of cows is the count of 'all cattle', available from NASS, and districts are defined by the USDA.[23] The potential DDGS market also includes hogs, but data is not available at the district level for all states. Nevertheless, because cattle use DDGS more efficiently than hogs, they represent the larger market for co-products (NASS, 2007).

The ethanol price is the free on board price in Omaha, and is published by the Nebraska Energy Office. We use state-level total gasoline rack prices from the Energy Information Administration. We do not include an E85 price in this analysis because the price series began much more recently than our time frame, and it lacks spatial variation. Natural gas (city gate) price and electricity price to industry are available annually from the EIA, also at state level.[24] We use the average urban CPI to deflate all the prices. For the indicator for metropolitan areas, we use the US Census definition of counties in metropolitan statistical areas. Data on biodiesel are from the National Biodiesel Board and Biodiesel Magazine. We construct a dummy variable *existing biodiesel* for whether there is existing biodiesel production capacity in county k at the start of year t.

Because we do not have local variation in ethanol, gasoline, natural gas, or electricity prices, local competition in the ethanol and gasoline output markets and in the gasoline, natural gas and electricity input markets are captured by the dummy variable N_{kt} for whether there is an existing ethanol plant in the county.

3.4 State Variables

We discretize each of the continuous variables in our data into discrete and finite-valued state variables, as detailed in Table A.3 in Appendix A. For our base specification, we discretize the continuous variables into two bins each. In some cases we aim for equally-sized bins (*natural gas price*, *electricity price*, *gasoline price*, *ethanol price*, *corn intensity*). For other variables, owing in part to their skewed distribution, we create bins that put higher weight on the lower (*corn price*) or higher (*cow density*) part of the continuous variable. We also construct alternate bins to test the robustness of our model to different break points, including discretizing the continuous variables into three instead of two bins. Summary statistics for the discretized state variables used in our

[23] A district is made of up to 120 counties and there are usually 6-8 districts per state.

[24] We use city gate natural gas price instead of price to industry because the complete series is available; these two price series trend together within a given state.

structural model are in Table A.4 in Appendix A.

Each state of the county $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$ is represented by a combination (or tuple) of discretized state variables. The number of potential states n_{tuple} in each county is the product of the number of bins of each state variable. Dimensionality is an important consideration for the simulations we perform using the structural estimates. For example, when we simulate removing a policy, we must observe the rest of the variables describing the state of the world Ω_{kt} with and without the policy. Thus, our preferred specification has fewer bins and covariates, thus fewer potential states of the world Ω_{kt} that we must identify and observe to conduct simulations.

Because the main objectives of this paper are to learn about the effects of government policy on investment, we are most concerned with the other state variables to the extent that they can fully describe the state of input and output markets. The indicator variables we construct for output and energy input prices allow us to control for prices in the state of the world, while freeing up dimensions to focus on and identify different policies in our simulations. The variable *energy input price* is an indicator that is one when both the electricity and natural gas prices are high. The variable *output price* indicator is one when both the gasoline and ethanol price is high.

4 Results of Structural Model

4.1 Structural Parameters

The results from the structural estimation of the parameters are reported in Table 1 and Table A.5 in Appendix A. Since our dynamic discrete choice model only identifies relative values of the coefficients in the investment payoff relative to the mean σ of the private shock, and does not separately identify the magnitudes of the coefficients in the investment payoff and the mean σ of the private shock, we focus on interpreting the signs, statistical significance, and relative magnitudes of the parameters, rather than their absolute magnitudes. Our preferred specification, which we use for the counterfactual policy simulations, is specification (i) in Table 1.

The additional specifications (ii)-(vi) in Table 1 show the robustness of the model to different price specifications. As seen in these alternative price specifications, the coefficient on *electricity price* is not significant and the coefficients on the *input price indicator* are not robust across specifications. This is consistent with the reduced-form analysis in Thome and Lin Lawell (2022), which indicates that *electricity price* does not have a significant impacts on the probability of entry. Our preferred specification (i) therefore includes only *natural gas price* and not *electricity price* or the *input price indicator*. Also as seen in these alternative price specifications, the significant parameters in our preferred specification (i) are all robust and significant across all the alternative price specifications (ii)-(iv).

All of the policy variables have positive impacts on the payoff from investment in an ethanol plant, and two, the state-level *MTBE Ban* and the federal-level *RFS2*, are significant. Because both the MTBE ban and the Renewable Fuel Standard can function as implicit blending mandates (de Gorter and Just, 2010; Anderson and Elzinga, 2014), the similar magnitude of the coefficients suggests similar implicit state blending levels. Further, the coefficient on *RFS1* is much smaller and is not statistically significant, which would suggest that the first version of the RFS was not big enough to induce investment.

On the input (cost) side, county-level *corn intensity* has a positive impact on the payoff from investment, while *corn price* is not significant. This result is similar to the reduced-form literature on plant location, which finds that physical access to feedstock is a significant location determinant, but more aggregate feedstock prices are not important (e.g. Cotti and Skidmore, 2010).

On the revenue side, the coefficient on *output price indicator* is negative; this means when both ethanol and gasoline prices are high, there is a negative impact on the payoff from investing. In the alternate price specifications (iii) and (iv) in Table 1, we show that high ethanol and gasoline prices have negative impacts on the payoff from investment when modeled individually, though the effects are insignificant.

The constant and the mean σ of the private shock are both significant determinants of the payoff from investing. Since our dynamic discrete choice model only identifies relative values of the coefficients in the investment payoff relative to the mean σ of the private shock, and does not separately identify the magnitudes of the coefficients in the investment payoff and the mean σ of the private shock, we focus on interpreting the signs, statistical significance, and relative magnitudes of the parameters, rather than their absolute magnitudes. The estimate of σ is similar in magnitude to the coefficients on *MTBE Ban* and *RFS2*, indicating that this private information shock can be as important as the policies in determining investment payoff. The constant is large and negative, indicating there are significant fixed costs to investing in an ethanol plant.

Specifications (v)-(vi) in Table 1 show the robustness of the model to various specifications of the input price variables, none of which have significant impacts on the payoffs from investing in an ethanol plant. Specification (vii) in Table A.5 in Appendix A builds on the base specification by adding the additional covariates *metro area* and *existing biodiesel*. These variables have insignificant effects on the expected payoff from investing in an ethanol plant, and their inclusion does not lead to noticeable differences in the other estimates. Consequently, we do not include these covariates in our preferred specification. Specifications (viii) and (ix) in Table A.5 in Appendix A show the results of structural estimation with alternate bins and more covariates than our preferred specification (specification (i)).[25] Since dimensionality is an important consideration for the simulations we perform using the structural estimates, however, our preferred specification (i) has fewer bins and covariates, thus fewer potential states of the world Ω_{kt} that we must identify and observe to conduct simulations. Once again, the significant parameters in our preferred specification (i) are robust and significant across the alternative specifications (vii)-(ix).

Across our different specifications, we find the robust result that the dummy for existing plants N_{kt} does not have a significant net effect on the payoff from investment.[26] Thus, our

[25]The bins we use in the base and alternative specifications are described and discussed in Section 3.4.

[26]One possible explanation for why our results show that existing ethanol plants in the county do not have a significant net effect on the payoff from investment is that our dummy for existing plants N_{kt} only captures variation

results show that the uncertainty in the ethanol investment timing decision arises primarily from uncertainty in economic factors X_{kt} and government policy G_{kt}, rather than also on uncertainty in what other potential investors are doing and therefore what the future values of the dummy for existing plants N_{kt} may be.

4.2 Goodness of Fit

To assess the goodness of fit of our model, we conduct a replication exercise in which we use our estimated structural parameters $\hat{\theta} = (\hat{\gamma}_N, \hat{\gamma}_G, \hat{\gamma}_X, \hat{\sigma})$ from our preferred specification (i) of Table 1 and the observed exogenous state variables to simulate (or predict) the data. We call the model predicted results our Base scenario.[27] Table A.6 in Appendix A compares the observed statistics (E, E_t, W, $\bar{w}_e$, s_e) in the data with their model simulated values under the Base scenario. The Base scenario does a good job of replicating the observed number of entrants and their welfare: the simulated number of entrants in the Base scenario has a mean of 136, versus 132 in the data. The data and the Base scenario also have similar values for the mean welfare per entrant $\bar{w}_e$ and for total welfare E. Our model therefore does a fairly good job matching the statistics based on actual data.

5 Counterfactual Policy Scenarios

We use the estimated structural parameters $\hat{\theta} = (\hat{\gamma}_N, \hat{\gamma}_G, \hat{\gamma}_X, \hat{\sigma})$ from our preferred specification (i) of Table 1 to simulate counterfactual policy scenarios to explore the policy factors driving industry growth and location, and to disentangle the impacts of state and national policies on the timing and location of investment in the industry. The counterfactual scenarios we run to disentangle the impacts of state and national policies on the timing and location of investment in the industry include the No RFS1, No RFS2, No Tax Credit, No MTBE Ban, and No Policy scenarios. The No RFS1, No RFS2, No Tax Credit, and No MTBE Ban counterfactual scenarios involve removing each respective policy individually. In the No Policy scenario, we remove all the policies (*MTBE Ban*, *RFS1*, *RFS2*, and *Tax Credit*) that might promote investment in ethanol plants. These counterfactual scenarios are summarized in Table A.1 in Appendix A. We describe our methodology for the counterfactual simulations in more detail in Appendix B.2.

Our counterfactual simulations capture several channels through which counterfactual government policies may affect the decision-making problem faced by a potential investor. First, since

in whether there there is an existing ethanol plant in the county. In their reduced-form analysis of ethanol plant entry during the second US ethanol boom, Thome and Lin Lawell (2022) use variation in the number of existing ethanol plants as well as in the plant ownership type of existing plants, and find evidence for both local competition among ethanol plants within counties, as well as possible agglomeration benefits from existing conglomerates and large ethanol producing firms in neighboring counties. Owing to state-space considerations, we are not able to allow for any more than binary variation in our variable for existing ethanol plants, but focus instead on analyzing the effects of government policies on ethanol investment. We hope to further evaluate strategic considerations in future work.

[27] We describe our methodology for the model fit simulations in Appendix B.1.

government policies affect the payoff from investing in an ethanol plant, the counterfactual removal of one or more government policies affects the payoff from investing and therefore the decision to invest in an ethanol plant. Second, since government policies affect the evolution of other government policies, the counterfactual removal of one government policy affects expectations about future values of other government policies, which in turn affect the expected payoffs from investing in the future and therefore the option value to waiting. Third, since government policies affect the evolution of economic factors, the counterfactual removal of one or more government policies affects expectations about future values of economic factors, including the ethanol price and the availability and cost of corn, and therefore the expected payoffs from investing in the future. Fourth, since government policies affect the decisions of other potential investors, the counterfactual removal of one or more government policies affects the decisions of other potential investors, which in turn affect the expected payoffs from investing in the future.

Table 2 presents the results of counterfactual policy scenarios that were run over the full period of our data set (1996-2008). As seen in the results in Table 2, removing the RFS2 significantly decreases the number of entrants compared to the Base scenario, while removing RFS1 and the state tax credit have smaller but noticeable affects on the number of entrants as well. The removal of RFS2 also decreases the mean welfare per entrant $\bar{w}_e$ relative to the Base scenario more than does either the removal of RFS1 or the removal of the state tax credit.

As seen in Table 2, there are two striking results that arise from comparing entrants and welfare in the Base and No Policy scenarios. First, the mean number of entrants in the No Policy scenario is 37, which is significantly lower than the mean number of entrants of 136 in the Base scenario. Together, the four policies led to most of the investment in plants over the 13 years of the simulation. The second important takeaway is that the mean welfare per entrant, $\bar{w}_e$, is significantly lower in the No Policy scenario than it is under the Base replication scenario. There is less entry because expected payoff from investment in an ethanol plant is much lower without the policies. The standard deviation of welfare per entrant under both the Base and No Policy scenarios is large though; policy changes account for some, but not all, of the differences in profitability across space and time.

While we use data from the entire time period of our data set to estimate the effect of the MTBE Ban on the payoff from investing in an ethanol plant, we are only able to run our counterfactual policy simulations for the No MTBE Ban scenario, which analyzes the counterfactual scenario in which the MTBE Ban was removed in all states in all years, for the pre-RFS period only (1996-2004). All the Midwestern states in our sample implemented MTBE bans by 2005, when the Renewable Fuel Standard was first implemented. Thus, the MTBE ban was implemented in all states in our sample in each of the two years that the federal RFS1 was in place in all states (2005 and 2006); and similarly the MTBE ban was implemented in all states in our sample in each of the years in our data set that the federal RFS2 was in place in all states (2007 onwards). As a consequence, in our data whenever we observe one of the RFS standards in place in any county in any year, we also observe the MTBE Ban in place in that county and year. In contrast, we never have any county-year observations in which one of the RFS standards is in place, but the MTBE ban is not. This means that it would therefore be impossible to identify a counterfactual state of the world in which one of the RFS standards in place, but the MTBE ban is not, since we never observe this counterfactual state of the world in the data. We therefore conducted the counterfactual simulations for the No MTBE Ban scenario for the pre-RFS period only (1996-2004), and compare their results with those from similarly running the Base, No Policy, and No Tax Credit scenarios through 2004 instead of through 2008.

Table 3 shows the results of the No MTBE Ban scenario, as well as the Base, No Policy, and No Tax Credit scenarios for the pre-RFS period (1996-2004). In this pre-RFS period only (1996-2004), there were 48 entrants in the Base replication (46 in the data: see Table A.6), and 29 entrants in the scenario with No MTBE Ban; this large difference is statistically significant. In this same time frame, there were 26 entrants in the No Policy scenario, and the difference between the No Policy scenario and the No MTBE Ban scenario is only marginally statistically significant. During this pre-RFS time period, the No Tax Credit scenario leads to fewer entrants than the Base replication, but this number is still more than the No MTBE Ban scenario. In aggregate, these results indicate that the MTBE Ban had a bigger effect on entry than the state tax credits in the pre-RFS era during which the effects of the two policies can be identified and compared.

We disaggregate these results by year in Appendix C to further explore the interactions among the policy effects. Viewing the simulated entrants by year is useful to begin to disentangle the effects of the MTBE Ban and the RFS. As described in more detail in Appendix C, welfare per entrant was lower in the pre-RFS era, which is why there were fewer entrants. During the period 2000-2004, which represents the period during which there were some MTBE bans but no RFS1 or RFS2, the MTBE ban accounted for 54% of the entrants in the period. Without the ban, there would have been 16 new plants instead of the 35 that entered in the Base scenario. The RFS2 had a larger impact in percentage and real terms. Nevertheless, both the No MTBE Ban and No RFS2 scenarios led to significantly lower welfare for entrants compared to the Base scenario in respective the years when the MTBE ban and the RFS2 were in effect. In the No Policy scenario, entry was slow and relatively constant over time, ranging from 1.6 to 4.1 new plants each year.

We disaggregate the results by each of the 10 Midwestern states in Table A.7 in Appendix A. States differ in their local market conditions, when they implemented the MTBE ban, and whether and when they offered tax credits. Figure 1 shows how entry compares across states and policy scenarios. Each bar in the graph shows the number of entrants in the pre-RFS period (1996-2004) in black, and the number of entrants in the post-RFS period (2005-2008) in grey, for each state and each policy scenario. Figure A.1 in Appendix A presents the mean welfare per entrant for each scenario by state, for the full period (left panel) and for the pre-RFS period (right panel).

There are noticeable differences across states in the total number of entrants, in the timing of the entrants, and in the relative impact of the different policy scenarios on entry. First, some states attract much more entry of ethanol plants than others under all scenarios. In particular, Iowa and Nebraska have the most entrants. The total number of entrants does not exactly correspond with the mean welfare per entrant, however (Figure C.2). The mean welfare per entrant is high in these two states, but overall, entrants had higher welfare from entry in Indiana and South Dakota in the Base scenario; South Dakota had fewer entrants because only part of the state is suitable for ethanol production.

The second important difference across states is that some states had relatively more entrants in the pre-RFS era than others. Nebraska, for example, had over half of its plants enter before 2005. Minnesota also experienced more entry in the pre-RFS era. Both these states implemented MTBE bans early (in 2000), and both also had state tax credits for plants that gave them more favorable conditions for entrants.

Different policies had different impacts on different states. The number of pre-RFS entrants in the Base and No MTBE Ban scenarios is directly proportional to the number of years the MTBE ban was in effect in each state, indicating that this policy made a large contribution to industry growth in the region. Likewise, the No RFS2 scenario led to fewer entrants in all states, indicating that the RFS2 was a driver of industry growth in the last two years of our analysis. The No Tax Credit scenario had more mixed results. All the states except Ohio, Iowa, and Illinois had tax credits available to entrants at some point during the analysis, though the year these policies were in effect varied across states.

6 Discussion and Conclusion

In this paper, we develop and estimate a dynamic structural econometric model of the ethanol plant investment timing game to analyze the effects of government policy on the decisions of ethanol-producing firms to invest in building new ethanol plants in the Midwestern United States during the second US ethanol boom. According to our results, government policies, particularly the ban on the use of the oxygenate MTBE as a gasoline additive at the state level, and the 2007 Renewable Fuel Standard (RFS2) at the federal level, have significant effects on ethanol investment payoffs and decisions. The intensity of corn production at the county level and private information shocks have significant effects on ethanol investment payoffs and decisions as well.

We use the estimated structural parameters to simulate counterfactual policy scenarios to explore the policy factors driving industry growth and location, and to disentangle the impacts of state and national policies on the timing and location of investment in the industry. We find that, of the policies analyzed, the MTBE ban and the RFS2 led to most of the ethanol plant investment during this time period. There are noticeable differences across states in the total number of entrants, in the timing of the entrants, and in the relative impact of the different policy scenarios on entry.

One possible reason the MTBE ban was effective in inducing investment in building ethanol plants is that it increased the demand for ethanol as an oxygenate in place of MTBE. Similarly, one possible reason the RFS2 was effective in inducing investment in building ethanol plants is that it increased demand for ethanol by mandating an expansion in ethanol consumption. Previous studies have shown that the demand for ethanol is highly elastic (Irwin and Good, 2017; Yi, Lin Lawell and Thome, 2025). Our results suggest that policies that increase the demand for ethanol have the potential for inducing investment in building ethanol plants.

Both the MTBE ban and the Renewable Fuel Standard can function as implicit blending mandates (de Gorter and Just, 2010; Anderson and Elzinga, 2014). Whenever unpriced emissions are the sole market failure, however, a carbon tax or cap and trade program is more likely to achieve the first-best (Pigou, 1920; Coase, 1960), while fuel mandates are unable to replicate the first-best solution (Helfand, 1992; Holland, Knittel and Hughes, 2009; Lapan and Moschini, 2012). Lade and Lin Lawell (2021) show that when renewable fuel mandates are combined with a cost containment mechanism such as a credit window price, the efficiency of the mandate can increase substantially. Thus, while the MTBE ban and the Renewable Fuel Standard were effective in inducing investment in building ethanol plants, it is possible to increase their efficiency by combining them with cost containment mechanisms or by using a market-based instrument instead. We hope to explore these possibilities in future work.

In this paper, we have defined welfare as the payoff to entrants of entry (investment). One reason ethanol has attracted policy attention are the possible environmental benefits of blending ethanol with gasoline as a source transportation fuel in place of fueling cars with exclusively gasoline. As the environmental costs and benefits of ethanol has been a subject of much debate in the literature (Searchinger et al., 2008; Witcover, Yeh and Sperling, 2013; Treesilvattanakul, Taheripour and Tyner, 2014; Lade and Lin Lawell, 2015), and therefore require a full and thorough treatment to address well, we do not include environmental costs and benefits in this paper, but instead focus on ethanol investment and the payoffs to investment. We hope to incorporate environmental costs and benefits in future work.

Another set of factors that may affect the costs and benefits of ethanol, and that would also require a full and thorough treatment to address well, regards the food versus fuel debate. Because the feedstocks used for the production of ethanol can also be used for food, there is a concern that ethanol policies might affect the relationship between food and fuel markets (Chen and Khanna, 2012), and, in particular, have potential adverse effects on the price of basic food prices for the world's poor (Rajagopal et al., 2007; Abbott, Hurt and Tyner, 2011; Zhang and Wetzstein, 2011; Poudel et al., 2012; de Gorter, Drabik and Just, 2013; de Gorter et al., 2013; Wright, 2014; Hao et al., 2017; Si et al., 2023). We do not include costs and benefits regarding food versus fuel in this paper, but instead focus on ethanol investment and the payoffs to investment. We hope to incorporate the food versus fuel issue in future work.

In analyzing the short-run effects of each counterfactual policy scenario, we assume that the counterfactual policy change we simulate is one that potential entrants do not anticipate, and that the counterfactual scenario does not change which equilibrium is played. Adapting the policy invariance assumption and approach of Benkard, Bodoh-Creed and Lazarev (2019), we therefore assume that the policy functions (as functions of state variables), transition densities of unaffected state variables (as functions of lagged state and action variables), and structural parameters we estimate themselves do not change under the different counterfactual policy changes. Since our model includes many state variables, the state of the game has a large dimension, which makes solving for the counterfactual equilibrium under each counterfactual scenario computationally impossible. In future work, we hope to develop techniques for analyzing counterfactual scenarios that might change the equilibrium being played, for example by building on and applying oblivious equilibrium models, in which knowledge of the state space is restricted for players in their decision-making process (Weintraub, Benkard and Van Roy, 2008; Benkard, Jeziorski and Weintraub, 2015); moment-based Markov equilibrium (MME) models, in which knowledge of the state space is limited to the private state and the distribution of the states of all other players (Ifrach and Weintraub, 2017); and recent empirical applications of oblivious equilibrium and moment-based Markov equilibrium (MME) models (Corbae and D'Erasmo, 2021; Jeon, 2022; Gerarden, 2023; Gowrisankaran, Langer and Zhang, 2025; Sears et al., 2025b).

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Table 1: Results of structural model

	Base Model	Alternate price specifications				
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Coefficients in the investment payoff on:						
Tax Credit	0.209	0.206	0.179	0.16	0.216	0.26
	(0.147)	(0.147)	(0.157)	(0.154)	(0.154)	(0.178)
MTBE Ban	0.814**	1.022***	0.837**	0.936**	0.907*	0.956*
	(0.293)	(0.303)	(0.305)	(0.299)	(0.372)	(0.323)
RFS 1	0.085	0.05	0.168	0.181	0.166	0.142
	(0.242)	(0.214)	(0.283)	(0.313)	(0.26)	(0.279)
RFS 2	0.727**	0.658**	0.786*	0.946**	0.816**	0.965***
	(0.256)	(0.231)	(0.32)	(0.338)	(0.309)	(0.27)
Cow Density	0.189	0.184	0.206	0.28‡	0.22†	0.229
	(0.149)	(0.136)	(0.155)	(0.16)	(0.129)	(0.162)
Corn Intensity	1.012***	0.976***	0.962***	1.193***	0.986***	1.217***
	(0.181)	(0.163)	(0.201)	(0.198)	(0.213)	(0.22)
Energy Output Indicator	−0.423‡	−0.573‡			-0.542	-0.429
	(0.246)	(0.307)			(0.348)	(0.334)
Ethanol Price				-0.376		
				(0.364)		
Gasoline price			-0.289	-0.096		
			(0.286)	(0.245)		
Corn Price	-0.074	-0.071	-0.08	-0.085	-0.167	-0.183
	(0.265)	(0.197)	(0.239)	(0.205)	(0.259)	(0.231)
Energy Input Indicator		0.753*	0.517		0.67	
		(0.354)	(0.41)		(0.444)	
Natural Gas Price	0.374			0.436		0.383
	(0.275)			(0.404)		(0.349)
Electricity Price				0.036		
				(0.179)		
Existing Plant	0.034	0.021	-0.237	0.042	-0.129	0.039
	(0.279)	(0.286)	(0.29)	(0.311)	(0.268)	(0.307)
Constant	-4.97***	-5.164***	-5.087***	-6.108***	-5.042***	-5.962***
	(0.411)	(0.372)	(0.512)	(0.403)	(0.413)	(0.506)
σ	0.648***	0.612***	0.61***	0.786***	0.606***	0.776***
	(0.042)	(0.039)	(0.048)	(0.043)	(0.073)	(0.051)
Number of Observations	33,307	33,307	33,307	33,307	33,307	33,307
Number of Counties	870	870	870	870	870	870

Notes: Standard errors in parentheses. Significance codes: *** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.01

Table 2: Number of entrants and welfare under counterfactual policy scenarios: Full Period

	Base Scenario	No RFS1	No RFS2	No Tax Credit	No Policy
Number of Entrants	135.9	131.6	91.8	123.2	36.6
	(15.0)	(17.1)	(17.8)	(15.7)	(17.8)
Total Welfare of All Entrants	278.2	267.8	157.0	246.6	36.6
	(31.6)	(35.4)	(36.7)	(34.0)	(42.6)
Mean of Welfare per Entrant	2.05	2.03	1.71	2.00	1.00
	(0.15)	(0.14)	(0.14)	(0.15)	(0.18)
Std. Dev. of Welfare per Entrant	0.70	0.72	0.65	0.72	0.60
	(0.04)	(0.04)	(0.04)	(0.04)	(0.07)
Difference between this scenario and Base scenario					
Number of Entrants		-4.3**	-44.1***	-12.7***	-99.3***
Total Welfare of All Entrants		-10.4	-121.2***	-31.6***	-241.6***
Mean of Welfare per Entrant		-0.02	-0.34***	-0.05***	-1.05***
Std. Dev. of Welfare per Entrant		0.02**	-0.05***	0.02**	-0.10***
Difference between this scenario and No Policy scenario					
Number of Entrants	99.3***	95.0***	55.2***	86.6***	
Total Welfare of All Entrants	241.6***	231.2***	120.4***	210.0***	
Mean of Welfare per Entrant	1.05***	1.03***	0.71***	1.00***	
Std. Dev. of Welfare per Entrant	0.10***	0.12***	0.05***	0.12***	

Notes: For each scenario, the reported statistics are averages over 50 simulations. We normalize welfare so that the mean welfare per entrant of the No Policy scenario is equal to 1. Standard errors are in parentheses, and are calculated from using the parameter estimates from each of the 250 bootstrap samples. For each of the 250 bootstrap samples, 50 simulations are run using the parameter estimates from that bootstrap sample. Standard errors for a statistic is the standard deviation of the respective statistics over all 250 bootstrap samples. Significance codes for two-sample t-tests of difference between scenarios: *** p<0.001, ** p<0.01, * p<0.05

Table 3: Number of entrants and welfare under counterfactual policy scenarios: Pre-RFS period (1996-2004)

	Base Scenario (to 2005)	No Tax Credit	No MTBE Ban	No Policy
Number of Entrants	47.6	43.0	28.9	26.1
	(14.4)	(14.4)	(16.0)	(15.6)
Total Welfare of All Entrants	64.0	54.6	24.6	20.3
	(32.4)	(33.5)	(35.7)	(36.2)
Mean of Welfare per Entrant	1.34	1.27	0.85	0.78
	(0.15)	(0.15)	(0.21)	(0.19)
Std. Dev. of Welfare per Entrant	0.70	0.69	0.56	0.55
	(0.06)	(0.07)	(0.06)	(0.06)
Difference between this scenario and Base scenario				
Number of Entrants		-4.6***	-18.7***	-21.5***
Total Welfare of All Entrants		-9.4*	-39.4***	-43.7***
Mean of Welfare per Entrant		-0.07***	-0.49***	-0.56***
Std. Dev. of Welfare per Entrant		-0.01	-0.14***	-0.15***
Difference between this scenario and No Policy scenario				
Entrants	21.5***	16.9***	2.8*	
Total Welfare of All Entrants	43.7***	34.3***	4.3†	
Mean of Welfare per Entrant	0.56***	0.49***	0.07***	
Std. Dev. of Welfare per Entrant	0.15***	0.14***	0.01	

Notes: For each scenario, the reported statistics are averages over 50 simulations. We normalize welfare so that the mean welfare per entrant of the No Policy scenario is equal to 1. Standard errors are in parentheses, and are calculated from using the parameter estimates from each of the 250 bootstrap samples. For each of the 250 bootstrap samples, 50 simulations are run using the parameter estimates from that bootstrap sample. Standard errors for a statistic is the standard deviation of the respective statistics over all 250 bootstrap samples. Significance codes for two-sample t-tests of difference between scenarios: ***p<0.001, **p<0.01, *p<0.05

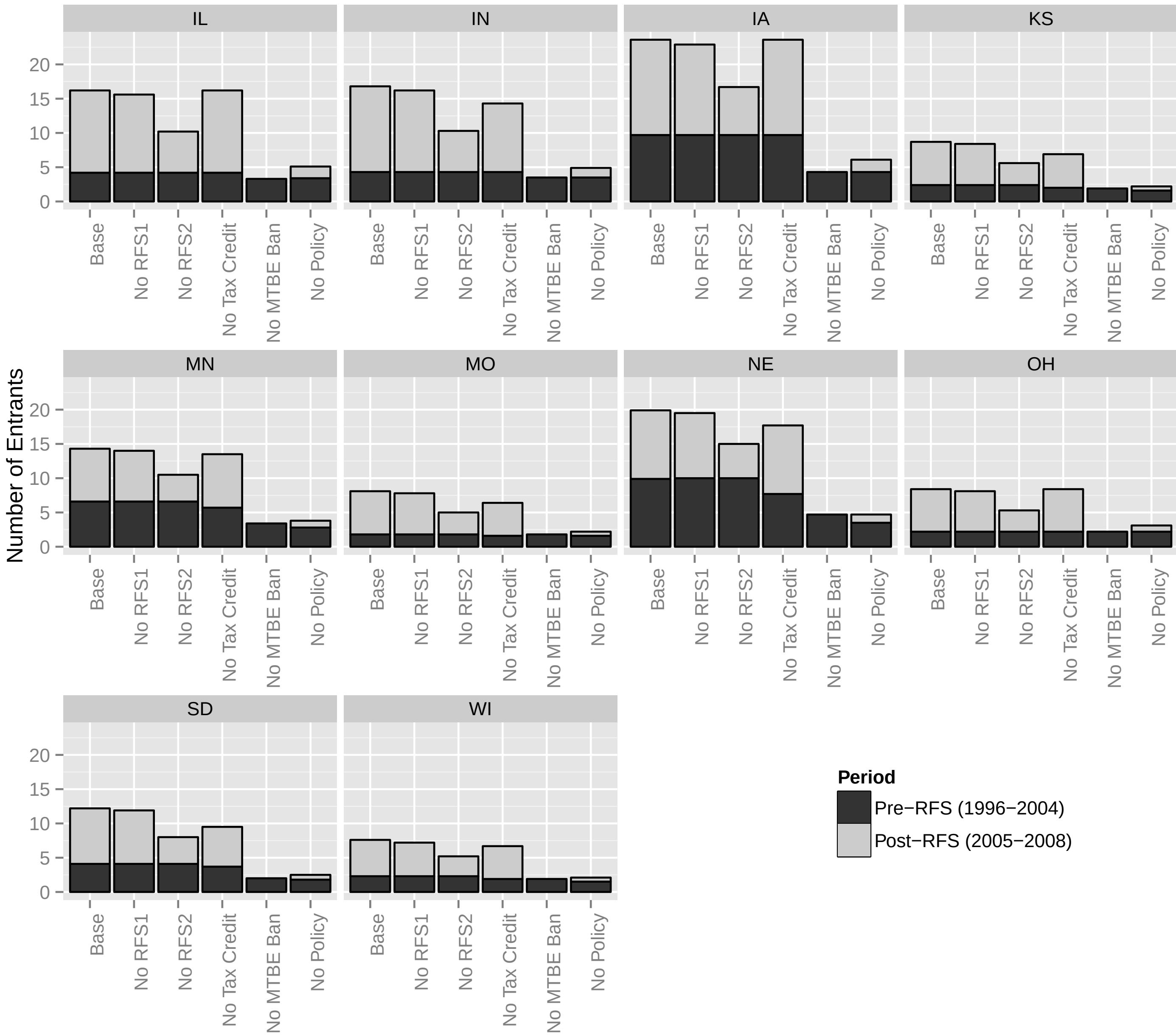

Figure 1: Number of entrants by state under different policy scenarios

Appendix

A Supplementary Tables and Figures

Table A.1: Counterfactual Scenarios

Counterfactual Scenario	**Description**
Base Scenario	Replication with observed data
No RFS1	Remove RFS1 (set *RFS1* to 0)
No RFS2	Remove RFS1 (set *RFS2* to 0)
No Tax Credit	Remove state tax credit (set *Tax Credit* to 0)
No MTBE Ban	Remove MTBE ban (set *MTBE Ban* to 0) [Pre-RFS (1996-2004) only]
No Policy	Remove all policies (set all G_{kt} variables to 0)

Table A.2: Replacement rules followed in counterfactual simulations for missing states of the world Ω_{kt}

		Replacement Rule Followed:		
Counterfactual Scenario	Number Missing	Set *Existing plant*=0	Match policy and significant state variables	Use annual mean $\bar{g}_t$ for entry probability $\hat{g}(\cdot)$
Base Scenario	48.5	48.4	0.1	0.0
No RFS1	66.3	65.5	0.8	0.0
No RFS2	101.2	99.5	1.7	0.0
No Tax Credit	827.3	88.7	738.5	0.0
No MTBE Ban (1996-2004)	380.9	168.1	212.8	0.0
No Policy	4209.1	427.7	3781.2	0.2

Notes: The replacement rules are used for the simulated states of the world Ω_{kt} that we do not observe in the data.

Table A.3: Bin design of variables for structural estimation

	Base Bins		**Alternate Bins**		
	Bin Design	Break	Bin Design	Break 1	Break 2
Cow Density (head/acre)	Bottom two thirds and top third	0.103	Middle Bin is 1.5 Std. Dev. around Mean	0.048	0.124
Corn Intensity	Equal sizes	0.175	Middle Bin is 1.5 Std. Dev. around Mean	0.078	0.191
Ethanol Price ($/gal)	Equal sizes	1.630	Middle Bin is middle 5 years	1.51	1.91
Gasoline Price ($/gal)	Equal sizes	1.110	Bottom third and top two thirds	1	
Output Price Indicator	High if both ethanol and gasoline prices are high				
Alternate Corn Price ($/bushel)	Equal sizes	3.010	Middle Bin is 1.5 Std. Dev. around Mean	2.317	3.32
Corn Price ($/bushel)	Bottom third and top two thirds	2.340	Middle Bin is 2 Std. Dev. around Mean	5.48	8.88
Natural Gas Price ($/1000ft3)	Equal sizes	6.810	Middle Bin is 2 Std. Dev. around Mean	4.519	8.349
Electricity Price (cents/KwH)	Equal sizes	5.130	Middle Bin is 2 Std. Dev. around Mean	4.702	5.741
Energy Input Price Indicator	High if both electricity and natural gas prices are high				

Note: Corn intensity is defined as the corn acreage divided by the total area of the county.

Table A.4: Summary statistics for discretized variables used in structural estimation

	Base Bins		**Alternate Bins**		**Spatial Resolution**
	Mean	Std. Dev.	Mean	Std. Dev.	
New Plant	0.004	0.063	0.004	0.063	county
Tax Credit	0.341	0.474	0.341	0.474	state policy
MTBE Ban	0.476	0.499	0.475	0.499	state policy
RFS I	0.153	0.360	0.153	0.360	national policy
RFS II	0.151	0.358	0.151	0.358	national policy
Cow Density	0.330	0.470	0.943	0.760	district (USDA definition)
Corn Intensity	0.494	0.500	0.917	0.669	county
Corn Price	0.677	0.468	0.918	0.712	state
Alternate Corn Price	0.513	0.500			state
Soy Price			1.093	0.596	state
Output Price Indicator	0.648	0.478			state
Ethanol Price	0.535	0.499	0.917	0.728	national
Gasoline Price	0.493	0.500	0.380	0.485	state
Energy Input Price Indicator	0.797	0.402			state
Natural Gas Price	0.492	0.500	0.945	0.649	state
Electricity Price	0.499	0.500	0.971	0.545	state
Metro Area	0.283	0.450	0.010	0.099	county
Existing Plant	0.036	0.185	0.037	0.188	county
Existing Biodiesel	0.010	0.100	0.285	0.452	county
Number of Observations	33,307		33,307		
Number of Counties	870		870		

Table A.5: Results of structural model with alternate variable and bin specifications

	Base Model	Additional Covariates	Alternate (More) Bins	
	(i)	(vii)	(viii)	(ix)
Coefficients in the investment payoff on:				
Tax Credit	0.209	0.123	0.109	0.394
	(0.147)	(0.135)	(0.247)	(0.398)
MTBE Ban	0.814**	1.044***	0.502‡	1.014***
	(0.293)	(0.296)	(0.268)	(0.284)
RFS 1	0.085	0.044	1.287***	1.674***
	(0.242)	(0.209)	(0.295)	(0.403)
RFS 2	0.727**	0.651*	2.343***	1.869***
	(0.256)	(0.268)	(0.322)	(0.266)
Cow Density	0.189	0.225‡	0.708***	0.812***
	(0.149)	(0.131)	(0.159)	(0.13)
Corn Intensity	1.012***	0.965***	0.209	0.315*
	(0.181)	(0.168)	(0.173)	(0.131)
Energy Output Price Indicator	-0.423†	-0.586*		
	(0.246)	(0.281)		
Ethanol Price			-0.518	-1.916**
			(0.646)	(0.636)
Gasoline Price			2.168***	2.546***
			(0.551)	(0.613)
Corn Price	-0.074	-0.071	-0.439	0.089
	(0.265)	(0.216)	(0.34)	(0.266)
Soy Price			-0.493	0.67
			(0.59)	(0.758)
Energy Input Price Indicator		0.792*		
		(0.382)		
Natural Gas Price	0.374		-1.549*	-1.104*
	(0.275)		(0.69)	(0.474)
Electricity Price			-0.179	
			(0.253)	
Metro Area		-0.244	-0.564	-0.369
		(0.2)	(0.569)	(0.589)
Existing Plant	0.034	-0.123	0.135	-0.017
	(0.279)	(0.26)	(0.363)	(0.347)
Existing Biodiesel		-0.06	0.033	0.023
		(0.48)	(0.084)	(0.074)
Constant	-4.97***	-5.08***	-5.591***	-6.583***
	(0.411)	(0.287)	(0.607)	(0.587)
σ	0.648***	0.609***	0.997***	0.77***
	(0.042)	(0.046)	(0.083)	(0.092)
Number of Observations	33,307	33,307	33,307	33,307
Number of Counties	870	870	870	870

Notes: Standard errors in parentheses. Significance codes: *** p<0.001, ** p<0.01, *p<0.05, ‡ p<0.01

Table A.6: Number of entrants and welfare in data and Base scenario

Full Period			Welfare per Entrant	
	Number of Entrants	Total Welfare of Entrants	Mean	Std. Dev.
Data	132	273.28	2.07	0.64
Base Scenario	135.92	278.21	2.05	0.704
	(14.97)	(31.62)	(0.15)	(0.04)
1996-2004			Welfare per Entrant	
	Number of Entrants	Total Welfare of Entrants	Mean	Std. Dev.
Data	46	65.68	1.43	0.596
Base Scenario	47.60	64.01	1.3449	0.6958
	(14.38)	(32.41)	(0.154)	(0.0649)

Notes: For the Base scenario, the reported statistics are averages over 50 simulations. We normalize welfare so that the mean welfare per entrant of the No Policy scenario is equal to 1. Standard errors are in parentheses, and are calculated from using the parameter estimates from each of the 250 bootstrap samples. For each of the 250 bootstrap samples, 50 simulations are run using the parameter estimates from that bootstrap sample. Standard errors for a statistic is the standard deviation of the respective statistics over all 250 bootstrap samples.

Table A.7: Number of entrants and mean welfare per entrant by state in full and pre-RFS periods

Number of Entrants		IL	IN	IA	KS	MN	MO	NE	OH	SD	WI
Full Period	Base	16.2	16.8	23.6	8.7	14.3	8.1	19.9	8.4	12.2	7.6
		(2.7)	(3.3)	(4.2)	(1.8)	(2.8)	(1.6)	(3.4)	(1.6)	(2.9)	(1.7)
	No RFS1	15.6	16.2	22.9	8.4	14.0	7.8	19.5	8.1	11.9	7.2
		(2.9)	(3.2)	(4.5)	(1.9)	(2.9)	(1.5)	(3.7)	(1.6)	(3.0)	(1.7)
	No RFS2	10.2	10.3	16.7	5.6	10.5	5.0	15.0	5.3	8.0	5.2
		(2.4)	(2.5)	(3.9)	(1.7)	(2.8)	(1.4)	(3.2)	(1.3)	(2.9)	(1.5)
	No Tax Credit	16.2	14.3	23.6	6.9	13.5	6.4	17.7	8.4	9.5	6.7
		(2.7)	(2.6)	(4.2)	(1.8)	(2.6)	(1.5)	(3.4)	(1.6)	(2.7)	(1.6)
	No Policy	5.1	4.9	6.1	2.2	3.8	2.2	4.7	3.1	2.5	2.1
		(1.8)	(2.3)	(2.6)	(1.7)	(2.6)	(1.4)	(2.0)	(1.1)	(2.7)	(1.2)
Pre-RFS (1996-2004)	Base	4.2	4.3	9.7	2.4	6.6	1.8	9.9	2.2	4.1	2.3
		(1.3)	(2.1)	(2.6)	(1.3)	(2.5)	(1.0)	(2.5)	(0.8)	(2.4)	(1.1)
	No Tax Credit	4.2	4.3	9.7	2.0	5.7	1.6	7.7	2.2	3.7	1.9
		(1.4)	(2.0)	(2.6)	(1.4)	(2.3)	(1.2)	(2.0)	(0.8)	(2.5)	(1.0)
	No MTBE Ban	3.3	3.5	4.3	1.9	3.4	1.8	4.7	2.2	2.0	1.9
		(1.3)	(2.1)	(2.2)	(1.4)	(2.7)	(1.0)	(2.1)	(0.9)	(2.6)	(1.1)
	No Policy	3.4	3.5	4.3	1.6	2.8	1.6	3.5	2.2	1.8	1.5
		(1.3)	(2.1)	(2.1)	(1.4)	(2.4)	(1.2)	(1.6)	(0.8)	(2.6)	(1.1)
Mean Welfare per Entrant		IL	IN	IA	KS	MN	MO	NE	OH	SD	WI
Full Period	Base	2.05	2.15	2.11	1.80	1.99	1.89	2.05	1.89	2.19	2.02
		(0.19)	(0.18)	(0.14)	(0.17)	(0.13)	(0.21)	(0.15)	(0.21)	(0.17)	(0.17)
	No RFS1	2.03	2.14	2.10	1.79	1.97	1.88	2.05	1.87	2.18	1.99
		(0.19)	(0.17)	(0.14)	(0.16)	(0.13)	(0.21)	(0.14)	(0.20)	(0.17)	(0.16)
	No RFS2	1.64	1.74	1.81	1.41	1.70	1.45	1.81	1.46	1.84	1.69
		(0.18)	(0.16)	(0.13)	(0.17)	(0.13)	(0.20)	(0.15)	(0.18)	(0.17)	(0.19)
	No Tax Credit	2.05	2.03	2.11	1.67	2.00	1.75	2.04	1.89	2.03	1.99
		(0.20)	(0.15)	(0.14)	(0.17)	(0.13)	(0.20)	(0.16)	(0.21)	(0.15)	(0.18)
	No Policy	1.01	1.09	1.12	0.60	1.04	0.70	1.08	0.94	0.98	0.93
		(0.18)	(0.17)	(0.18)	(0.17)	(0.20)	(0.19)	(0.20)	(0.19)	(0.23)	(0.21)
Pre-RFS (1996-2004)	Base	1.02	1.14	1.53	0.81	1.46	0.62	1.64	0.74	1.50	1.12
		(0.15)	(0.16)	(0.15)	(0.19)	(0.16)	(0.24)	(0.18)	(0.20)	(0.19)	(0.21)
	No Tax Credit	1.01	1.14	1.53	0.66	1.40	0.46	1.49	0.74	1.40	0.97
		(0.15)	(0.16)	(0.15)	(0.18)	(0.15)	(0.19)	(0.18)	(0.19)	(0.18)	(0.19)
	No MTBE Ban	0.72	0.88	0.91	0.48	0.92	0.62	1.04	0.74	0.84	0.85
		(0.17)	(0.20)	(0.20)	(0.22)	(0.26)	(0.24)	(0.26)	(0.21)	(0.28)	(0.26)
	No Policy	0.71	0.88	0.90	0.35	0.82	0.46	0.89	0.74	0.73	0.70
		(0.17)	(0.20)	(0.20)	(0.20)	(0.23)	(0.19)	(0.22)	(0.19)	(0.25)	(0.22)

Notes: For each scenario, the reported statistics are averages over 50 simulations. We normalize welfare so that the mean welfare per entrant of the No Policy scenario is equal to 1. Standard errors are in parentheses, and are calculated from using the parameter estimates from each of the 250 bootstrap samples. For each of the 250 bootstrap samples, 50 simulations are run using the parameter estimates from that bootstrap sample. Standard errors for a statistic is the standard deviation of the respective statistics over all 250 bootstrap samples.

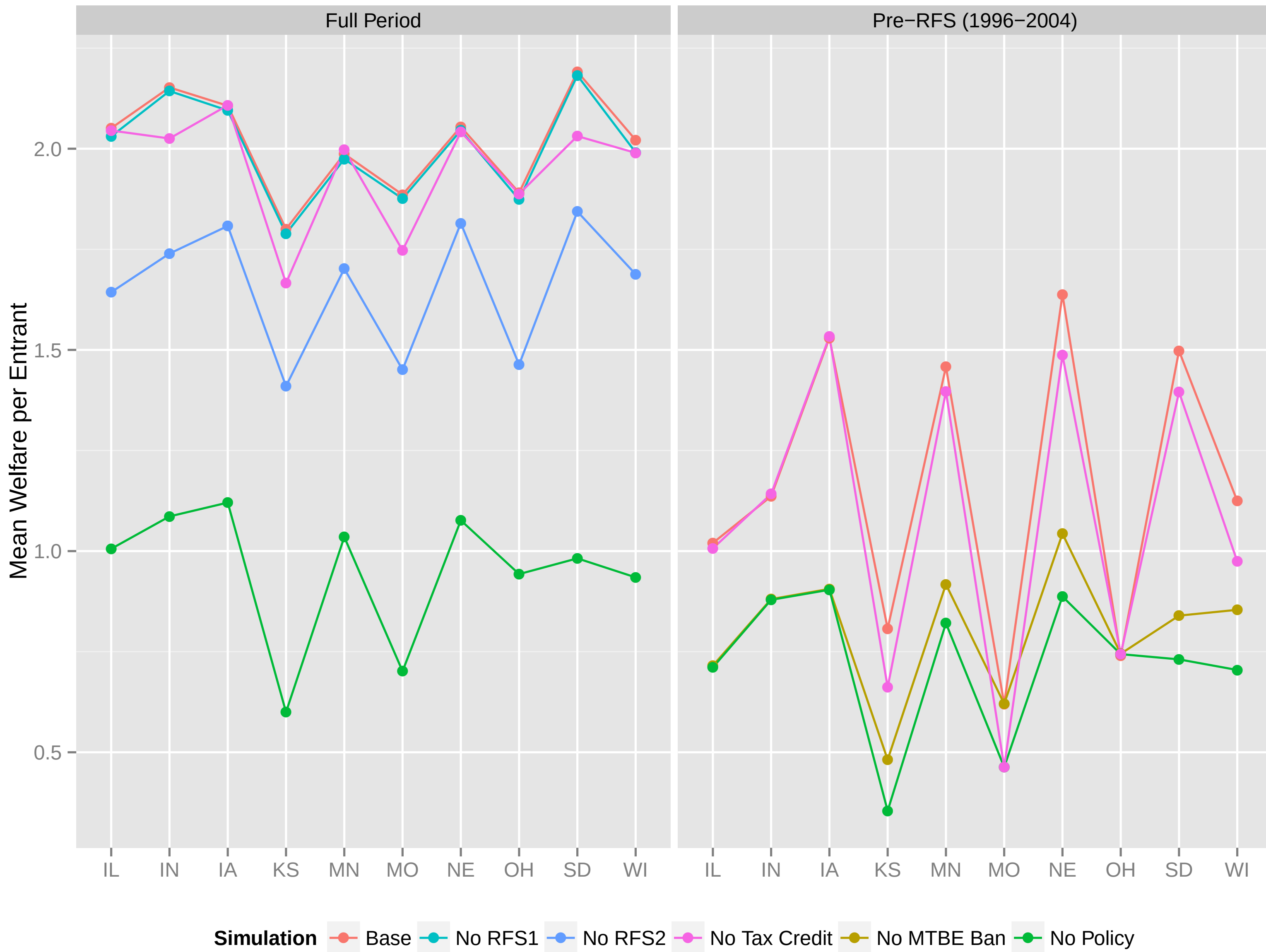

Figure A.1: Mean welfare per entrant by state under different policy scenarios

B Methodology for Simulations

B.1 Model Fit Simulations

We use our estimated structural parameters $\hat{\theta} = (\hat{\gamma}_N, \hat{\gamma}_G, \hat{\gamma}_X, \hat{\sigma})$ to run simulations to assess goodness of fit and to analyze counterfactual scenarios. To assess the goodness of fit of our model, we conduct a replication exercise in which we use our estimated model applied to the observed exogenous state variables to simulate (or predict) the data. We call the model predicted results our Base scenario.

We use our estimated model to simulate 50 trajectories of play, each for 13 years representing the years 1996-2008. For each simulation, we use the observed state variables for the initial values of Ω_{kt} at *t=1*, which corresponds to 1996, our first year of data. For each year t of a given simulation, we evaluate the estimated investment policy function $\hat{g}(\Omega_{kt}; \hat{\theta})$ at the state of the world $\Omega_{kt} = (N_{kt}, G_{kt}, X_{kt})$ for each county k at time t, and then use the estimated investment probability $\hat{g}(\Omega_{kt}; \hat{\theta})$ to simulate the investment decision I_{ikt} for each potential investor i in that county k at that time t. Once a potential investor i makes an investment ($I_{ikt} = 1$), that investor exits the sample. We then update N_{kt} for year $t + 1$ to account for any investments made in each county k in year t. We use the observed data for the exogenous variables G_{kt} and X_{kt}. We repeat for each year through 2008 (the 13th year), updating N_{kt} for each period.

After simulating investment (entry) for each year over the period 1996-2008, we record the total number of entrants E and the number of entrants E_t in each year t. We also calculate the welfare w_e of each entrant, which we define as the expected current-value investment payoff for the entrant, by evaluating equation (9) using the estimated parameters $\hat{\theta}$ and the state variables Ω_{kt} at the time t when the entrant enters. For each simulation, we also calculate the total welfare W summed over all entrants, the mean welfare per entrant $\bar{w}_e$ taken over all entrants in all years, and the standard deviation s_e of the welfare per entrant over all years. Since our dynamic discrete choice model only identifies relative welfare values, not absolute values, and since welfare is therefore unitless, we normalize welfare so that the mean welfare per entrant of the No Policy scenario is equal to 1.

We estimate the standard errors for the statistics (E, E_t, W, $\bar{w}_e$, s_e) using a nonparametric bootstrap. We randomly draw counties from the dataset with replacement to generate 250 independent panels of size equal to the actual sample size. These are the same datasets that we generated when bootstrapping the standard errors of the structural parameters. For each of the 250 bootstrap samples, we simulate 50 trajectories of play using the estimated parameters $\hat{\theta}$ and estimated probabilities of investment $\hat{g}(\cdot)$ associated with the particular bootstrap draw, and then take the average of the statistics (E, E_t, W, $\bar{w}_e$, s_e) across the 50 simulated trajectories. The standard error is then formed by taking the standard deviation of the estimated statistics from each of the random samples.

B.2 Counterfactual Policy Simulations

We also use our estimated structural parameters $\hat{\theta} = (\hat{\gamma}_N, \hat{\gamma}_G, \hat{\gamma}_X, \hat{\sigma})$ to run simulations to analyze counterfactual scenarios. Our simulations are summarized in Table A.1 in Appendix A. The counterfactual scenarios we run to disentangle the impacts of state and national policies on the timing and location of investment in the industry include the No RFS1, No RFS2, No Tax Credit, No MTBE Ban, and No Policy scenarios. The No RFS1, No RFS2, No Tax Credit, and No MTBE Ban counterfactual scenarios involve removing each respective policy individually. In the No Policy scenario, we remove all the policies (*MTBE Ban*, *RFS1*, *RFS2*, and *Tax Credit*) that might promote investment in ethanol plants.

The methodology for the counterfactual policy simulations is similar to the methodology described in Appendix B.1 for the Base scenario simulations we run to assess model fit, except that, for each counterfactual policy scenario in which we remove one or more of the government policies, we replace the indicators for the specified policy variables in G_{kt} with zero to form the respective counterfactual policy variables $\tilde{G}_{kt}$. For example, in the No RFS1 simulation, we set $RFS1 = 0$ for all observations.

Our counterfactual simulations capture several channels through which counterfactual government policies may affect the decision-making problem faced by a potential investor. First, since government policies affect the payoff from investing in an ethanol plant, the counterfactual removal of one or more government policies affects the payoff from investing and therefore the decision to invest in an ethanol plant. Second, since government policies affect the evolution of other government policies, the counterfactual removal of one government policy affects expectations about future values of other government policies, which in turn affect the expected payoffs from investing in the future and therefore the option value to waiting. Third, since government policies affect the evolution of economic factors, the counterfactual removal of one or more government policies affects expectations about future values of economic factors, including the ethanol price and the availability and cost of corn, and therefore the expected payoffs from investing in the future. Fourth, since government policies affect the decisions of other potential investors, the counterfactual removal of one or more government policies affects the decisions of other potential investors, which in turn affect the expected payoffs from investing in the future.

In analyzing the short-run effects of each counterfactual policy scenario, we assume that the counterfactual policy change we simulate is one that potential entrants do not anticipate, and that the counterfactual scenario does not change which equilibrium is played. Adapting the policy invariance assumption and approach of Benkard, Bodoh-Creed and Lazarev (2019), we therefore assume that the policy functions (as functions of state variables), transition densities of unaffected state variables (as functions of lagged state and action variables), and structural parameters we estimate themselves do not change under the different counterfactual policy changes.

For each counterfactual policy scenario, we simulate the effects of the counterfactual policy change on the number of entrants and the welfare of entrants. We use two-sample t-tests to compare the results of each of the counterfactual scenarios to those of the Base scenario. We also use two-sample t-tests to compare the results of each counterfactual scenario that involves removing a policy individually to the those of the No Policy scenario.

For the No MTBE Ban scenario, we can only run the simulations for the pre-RFS period (1996-2004) because 2004 was the last year any state in our sample permitted the use of MTBE. All the Midwestern states in our sample implemented MTBE bans by 2005, when the Renewable Fuel Standard was first implemented. Thus, the MTBE ban was implemented in all states in our sample in each of the two years that the federal RFS1 was in place in all states (2005 and 2006); and similarly the MTBE ban was implemented in all states in our sample in each of the years in our data set that the federal RFS2 was in place in all states (2007 onwards). As a consequence, in our data whenever we observe one of the RFS standards in place in any county in any year, we also observe the MTBE Ban in place in that county and year. In contrast, we never have any county-year observations in which one of the RFS standards is in place, but the MTBE ban is not. This means that it would therefore be impossible to identify a counterfactual state of the world in which one of the RFS standards in place, but the MTBE ban is not, since we never observe this counterfactual state of the world in the data.

One challenge in simulating alternate policy scenarios is that, because entry is random in the counterfactual simulations, we sometimes simulate counterfactual states of the world that we do not observe in the data, and, as a consequence, are unable to evaluate the investment policy function $\hat{g}(N_{kt}, \tilde{G}_{kt}, X_{kt}; \hat{\theta})$ at the simulated counterfactual state $\Omega_{kt} = (N_{kt} = n, \tilde{G}_{kt} = g, X_{kt} = x)$. To address this issue, we use the following rules to replace the missing value of $\hat{g}(\cdot)$ for simulated counterfactual states of the world Ω_{kt} that we do not observe in the data.

A common reason why a simulated counterfactual state of the world $\Omega_{kt} = (N_{kt} = n, \tilde{G}_{kt} = g, X_{kt} = x)$ is missing in the data is that we simulate investment (entry) in a county k that did not have any ethanol plants in the data. As seen in Section 4, our structural parameter estimates show that the dummy for existing plants N_{kt} does not have a significant net effect on the payoff from investment. Consequently, our first replacement rule is replace the investment probability $\hat{g}(N_{kt} = 1, \tilde{G}_{kt} = g, X_{kt} = x; \hat{\theta})$ with $\hat{g}(N_{kt} = 0, \tilde{G}_{kt} = g, X_{kt} = x; \hat{\theta})$ when we do not observe $\Omega_{kt} = (N_{kt} = 1, \tilde{G}_{kt} = g, X_{kt} = x)$ in the data. In other words, if we do not observe the state tuple $\Omega_{kt} = (N_{kt} = 1, \tilde{G}_{kt} = g, X_{kt} = x)$ in the data, we evaluate the investment probability $\hat{g}(N_{kt}, \tilde{G}_{kt} = g, X_{kt} = x; \hat{\theta})$ at $N_{kt} = 0$ instead of $N_{kt} = 1$, holding all other state variables in that state tuple fixed. Since the dummy for existing plants N_{kt} does not have a significant net effect on the payoff from investment, it should not matter much whether the investment probability $\hat{g}(N_{kt}, \tilde{G}_{kt} = g, X_{kt} = x; \hat{\theta})$ at a given state tuple $\Omega_{kt} = (N_{kt} = n, \tilde{G}_{kt} = g, X_{kt} = x)$ is evaluated at $N_{kt} = 0$ instead of $N_{kt} = 1$, holding all other state variables in that state tuple fixed.

Another reason why we do not observe some simulated counterfactual states of the world $\Omega_{kt} = (N_{kt} = n, \tilde{G}_{kt} = g, X_{kt} = x)$ is that for some values of the economic factors X_{kt}, we may not observe that value X_{kt} under counterfactual values of the policy variables $\tilde{G}_{kt}$. Consequently, for the second replacement rule, we find a state of the world $\Omega'_{kt} = (N_{kt} = n, \tilde{G}_{kt} = g, X_{kt} = x')$ that we do observe in the data for which the variables in $X_{kt} = x'$ that have a statistically significant effect on the payoff from investing in building an ethanol plant and the policy variables $\tilde{G}_{kt}$ match our simulated counterfactual data, and then replace the investment probability $\hat{g}(N_{kt} = n, \tilde{G}_{kt} = g, X_{kt} = x; \hat{\theta})$ with $\hat{g}(N_{kt} = n, \tilde{G}_{kt} = g, X_{kt} = x'; \hat{\theta})$.

Almost all replacements are made using either the first or second replacement rule above. The third and final replacement rule for the simulated counterfactual states of the world Ω_{kt} that we do not observe in the data (and that are not covered by either the first or second replacement rule above) is to use the annual mean $\bar{g}_t$ in place of the missing $\hat{g}(\cdot)$ for the simulated counterfactual states of the world Ω_{kt} that we do not observe in the data and that are not covered by either the first or second replacement rule above.

Table A.2 in Appendix A shows which replacement rule we use in each counterfactual scenario for the simulated states of the world Ω_{kt} that we do not observe in the data. Almost all replacements were made in Rule 1 or Rule 2. The No Policy simulation was the most challenging in this respect because there were relatively few years and counties among which to find replacements.

C Results of Counterfactual Policy Scenarios by Year

We disaggregate the results of our counterfactual policy scenarios by year in Table C.1 to further explore the interactions among the policy effects. Viewing the simulated entrants by year is useful to begin to disentangle the effects of the MTBE Ban and the RFS. Figure C.1 shows the cumulative number of entrants and the total cumulative welfare of entrants over time. Entry and total welfare of entrants increased faster in the later years of the analysis, particularly in the years during which the RFS2 was in effect (2007-2008). In the Base replication the number of entrants per year increased over time, with a maximum of 32.5 new plants in 2007 (the second to last year of the simulation).

As seen in Table C.1, the No Tax Credit simulation yielded on average 9% fewer entrants per given year compared to the Base simulation. The impact was smaller in the earlier years of the simulation, when fewer states had policies in place. The No RFS1 simulation had a slightly larger impact on the number of entrants than the No Tax Credit simulation for the years when RFS1 was in effect (2005-2006), though the cumulative number of entrants was still greater under the No RFS1 scenario because it was in effect for fewer years. The No RFS2 scenario led to a much more marked decrease in the number of entrants per year compared to the no RFS1 and No Tax Credit scenarios (Figure C.1), though the number of entrants per year during the RFS2 period (2007-2008) was still greater than the beginning of our analysis period due to other favorable economic conditions (Table C.1). Though we can only identify the No MTBE Ban scenario in the pre-RFS era (before 2005), we find similar magnitude of impact on the number of entrants as the No RFS2 scenario, particularly as we get closer to 2005, when all the states in our analysis had banned MTBE. In the No Policy scenario, entry was slow and relatively constant over time, ranging from 1.6 to 4.1 new plants each year.

Figure C.2 shows how the mean welfare per entrant by year changed over time under each scenario. The lines for the No RFS1 and the No Tax Credit scenarios closely track the Base scenario, indicating that these policies had relatively small impacts on the profitability for entrants. Nevertheless, both the No MTBE Ban and No RFS2 scenarios led to significantly lower welfare for entrants compared to the Base scenario in respective the years when the MTBE ban and the RFS2 were in effect.

Welfare per entrant was lower in the pre-RFS era, which is why there were fewer entrants. The first states in our sample banned MTBE as early as 2000, when we see the welfare per entrant under the No MTBE Ban scenario drop significantly below that of the Base replication. During the period 2000-2004, which represents the period during which there were some MTBE bans but no RFS1 or RFS2, the MTBE ban accounted for 54% of the entrants in the period. Without the ban, there would have been 16 new plants instead of the 35 that entered in the Base scenario. The RFS2 had a larger impact in percentage and real terms. Nevertheless, the level of entry in the Base scenario was higher in later years due to a combination of policy and market factors.

Table C.1: Number of entrants and mean welfare per entrant by year

Number of Entrants	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
Base	1.7	3.4	3.6	4.4	4.2	6.8	7.2	6.7	9.8	13.7	11.2	32.5	30.9
	(0.7)	(1.4)	(3.0)	(6.8)	(1.8)	(1.8)	(2.2)	(1.8)	(2.8)	(3.7)	(3.2)	(5.4)	(4.9)
No RFS1	1.7	3.4	3.6	4.4	4.2	6.8	7.3	6.7	9.8	10.9	9.4	32.6	31.0
	(0.7)	(1.4)	(3.1)	(6.8)	(1.8)	(1.8)	(2.1)	(1.7)	(2.8)	(3.1)	(3.9)	(5.4)	(5.0)
No RFS2	1.7	3.4	3.6	4.4	4.2	6.8	7.3	6.7	9.8	13.8	11.2	9.7	9.4
	(0.7)	(1.4)	(3.1)	(6.8)	(1.8)	(1.8)	(2.1)	(1.7)	(2.8)	(3.7)	(3.2)	(4.2)	(4.0)
No Tax Credit	1.6	3.1	3.4	4.1	3.8	5.8	6.7	5.8	8.7	12.6	10.0	29.5	28.1
	(0.7)	(1.4)	(2.9)	(6.6)	(1.6)	(1.5)	(2.2)	(1.6)	(2.6)	(3.4)	(2.9)	(5.2)	(4.9)
No MTBE	1.7	3.3	3.6	4.4	2.3	3.1	3.5	3.5	3.5				
	(0.7)	(1.4)	(3.1)	(6.8)	(1.9)	(1.3)	(1.3)	(1.1)	(1.5)				
No Policy	1.6	3.1	3.4	4.1	2.1	2.7	3.2	3.0	3.0	3.2	2.4	2.5	2.4
	(0.7)	(1.4)	(2.9)	(6.6)	(1.8)	(1.1)	(1.2)	(0.9)	(1.3)	(1.4)	(0.9)	(1.0)	(1.0)
Mean Welfare per Entrant	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
Base	-0.29	0.37	0.57	0.77	1.10	1.52	1.59	1.66	1.86	2.04	2.04	2.51	2.56
	(0.33)	(0.24)	(0.26)	(0.30)	(0.22)	(0.19)	(0.22)	(0.17)	(0.16)	(0.19)	(0.23)	(0.21)	(0.20)
No RFS1	-0.28	0.36	0.56	0.78	1.11	1.52	1.60	1.66	1.86	1.95	1.96	2.51	2.56
	(0.33)	(0.24)	(0.27)	(0.30)	(0.22)	(0.19)	(0.21)	(0.17)	(0.16)	(0.16)	(0.24)	(0.21)	(0.21)
No RFS2	-0.28	0.36	0.56	0.78	1.11	1.52	1.60	1.66	1.86	2.04	2.04	2.05	2.12
	(0.33)	(0.24)	(0.27)	(0.30)	(0.22)	(0.19)	(0.21)	(0.17)	(0.16)	(0.19)	(0.23)	(0.23)	(0.22)
No Tax Credit	-0.35	0.30	0.50	0.72	1.02	1.44	1.55	1.60	1.81	2.01	2.00	2.48	2.53
	(0.31)	(0.23)	(0.25)	(0.29)	(0.19)	(0.19)	(0.22)	(0.18)	(0.16)	(0.19)	(0.22)	(0.21)	(0.21)
No MTBE Ban	-0.28	0.36	0.57	0.78	0.60	0.94	1.12	1.27	1.35				
	(0.32)	(0.25)	(0.26)	(0.30)	(0.26)	(0.25)	(0.19)	(0.18)	(0.21)				
No Policy	-0.35	0.30	0.50	0.72	0.55	0.87	1.08	1.22	1.29	1.43	1.44	1.56	1.66
	(0.31)	(0.23)	(0.25)	(0.29)	(0.24)	(0.23)	(0.18)	(0.18)	(0.19)	(0.19)	(0.23)	(0.22)	(0.21)

Notes: For each scenario, the reported statistics are averages over 50 simulations. We normalize welfare so that the mean welfare per entrant of the No Policy scenario is equal to 1. Standard errors are in parentheses, and are calculated from using the parameter estimates from each of the 250 bootstrap samples. For each of the 250 bootstrap samples, 50 simulations are run using the parameter estimates from that bootstrap sample. Standard errors for a statistic is the standard deviation of the respective statistics over all 250 bootstrap samples.

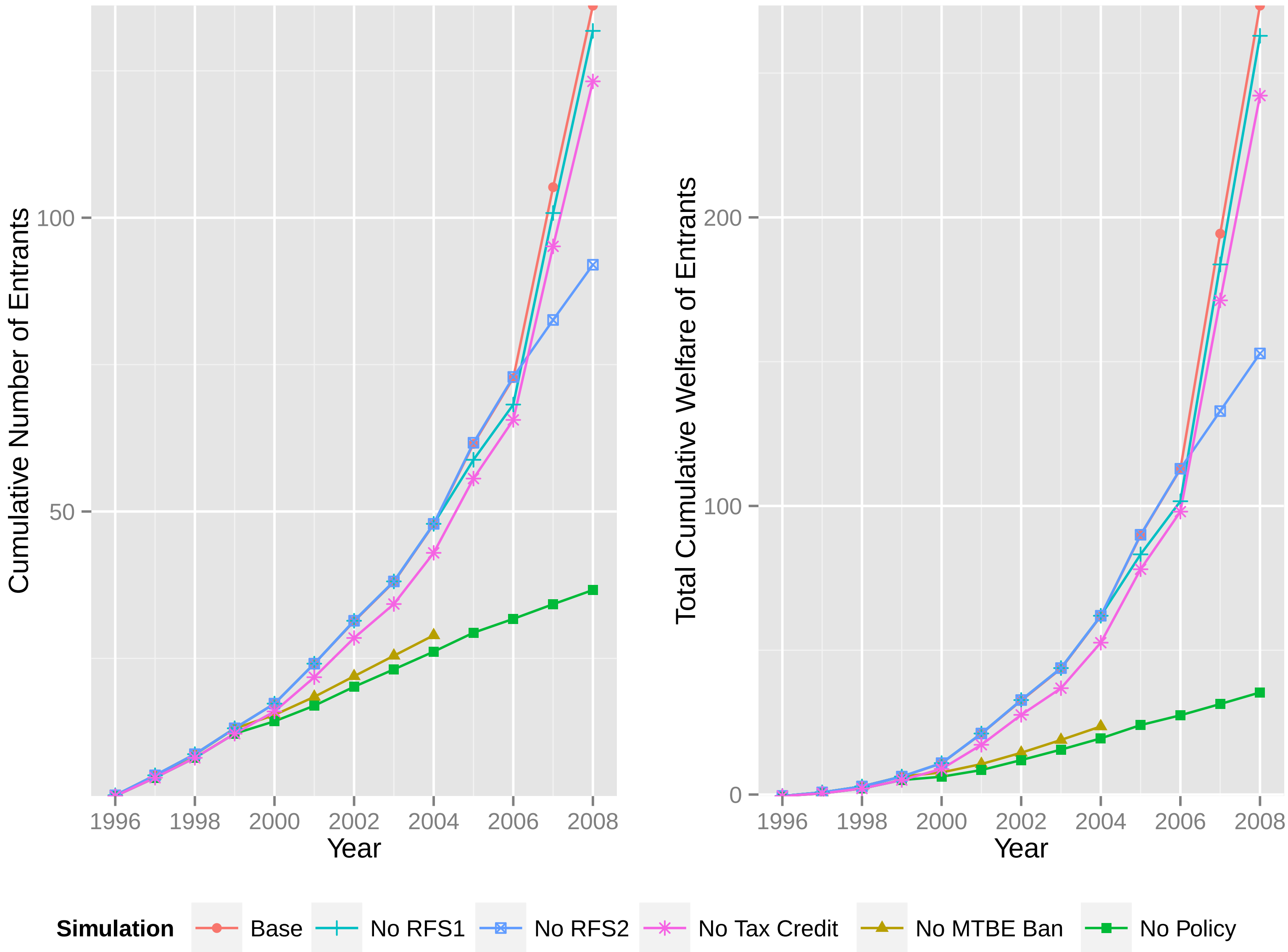

Figure C.1: Cumulative number of entrants and total cumulative welfare of entrants under different policy scenarios over time

Figure C.2: Mean welfare per entrant by year under different policy scenarios