ABSTRACT

Title of Dissertation: ESSAYS ON LAND USE POLICY, CONSERVATION INCENTIVES, AND AGRICULTURAL TECHNOLOGY ADOPTION

Samuel E Williamson, Doctor of Philosophy, 2026

Dissertation directed by: Professor David Newburn, Department of Agricultural and Resource Economics

This dissertation consists of three essays on agricultural and resource economics.

The first essay investigates the effectiveness of zoning with quantity restrictions as a tool for managing exurban development patterns beyond the urban fringe. Using a quasi-experimental research design focusing on a novel statewide growth management policy in Maryland, I estimate the effects of prohibiting major residential subdivisions in agricultural and conservation areas using a two-stage triple-difference econometric model. The results indicate that the policy significantly altered exurban development patterns, reducing the probability of development by 49 percent on parcels for which the policy binds, compared to the counterfactual rate of development.

The second essay evaluates the role of spatial coordination incentives in restoring contiguous wildlife habitat, specifically examining a threshold bonus scheme within the Oregon Conservation Reserve Enhancement Program (CREP). By linking parcel enrollment with high-resolution stream network data, the analysis investigates endogenous spatial interactions among enrollees. Contrary to the assumption of minimal impact, the findings reveal that existing streambank enrollment significantly raises the likelihood of neighboring enrollment, particularly after the bonus payment threshold is met. This indicates that the bonus scheme generates positive spatial externalities, accelerating enrollment among neighboring parcels and facilitating watershed-level restoration goals.

The third essay examines the evolution and economic impacts of western corn rootworm resistance to genetically engineered (Bt) corn in the United States. Using Agricultural Resource Management Survey (ARMS) data from 2005, 2010, and 2016, the study analyzes seed and insecticide choices to quantify the regional costs of resistance. The findings demonstrate that resistance is a distinct regional phenomenon that imposes significant economic burdens; specifically, rootworm resistance in the worst-affected regions cost farmers approximately \$100 million in 2010 and \$75 million in 2016 (inflation-adjusted). Additionally, total yield losses attributable to resistance rose over this period, highlighting the persistent challenge of managing pest resistance in major agricultural systems.

ESSAYS ON LAND USE POLICY, CONSERVATION INCENTIVES, AND AGRICULTURAL TECHNOLOGY ADOPTION

by

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Foreword

This dissertation includes jointly authored work in Chapters 2 and 3. As required by the University of Maryland Graduate School, I certify that I made substantial contributions to the jointly authored work included in this dissertation.

Chapter 2 is co-authored with Professor Lori Lynch. I was the lead author and made substantial contributions to all major aspects of the chapter, including the study design, empirical analysis, and manuscript writing and revision. Professor Lynch contributed guidance on the research design, interpretation of the results, and revision of the manuscript.

Chapter 3 is co-authored with Seth J. Wechsler and Jonathan R. McFadden of the U.S. Department of Agriculture Economic Research Service. This chapter was developed collaboratively among the three authors, and I made substantial contributions to the state-level resistance-identification design, empirical analysis, and preparation of the manuscript's tables and figures.

To Stephen and Diane,

who instilled in me the virtue of diligence and a love of learning.

For Molly,

whose support was my foundation.

And for Rowan,

who reminded me to put the work down and play.

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This work benefited immensely from collaboration. I specifically thank Professors Lori Lynch and Erik Lichtenberg for their instrumental support in the development of Chapters 2 and 3. My co-authors on these chapters—Professor Lynch, Seth J. Wechsler, and Jonathan R. McFadden—provided not only technical expertise but also a spirit of shared inquiry that made this research possible. I also thank Seth and Jonathan for their mentorship during and after my time at the U.S. Department of Agriculture.

I wish to thank the faculty and staff at the Department of Agricultural and Resource Economics for creating a stimulating and supportive academic environment. My fellow graduate students provided not only intellectual camaraderie but also cherished friendships that made this journey more enjoyable.

Finally, I owe my deepest gratitude to my family and friends for their endless love, patience, and encouragement. To my parents, Stephen and Diane, and my partner, Molly: thank you for believing in me, even when I doubted myself. To my son, Rowan: thank you for bringing so much light and laughter into my life throughout this process. This dissertation would not have been possible without you.

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Chapter 1

Zoning with quantity restrictions for managing exurban development patterns beyond the urban fringe

Large-lot rural residential development has been the dominant land use (in terms of acreage developed) in the United States since the 1950s, particularly in exurban areas beyond the urban fringe of exiting metropolitan areas (Heimlich & Anderson, 2001; Brown et al., 2005). Managing this highly fragmented form of low-density development has proven challenging using existing state and local growth management strategies to reduce sprawl, such as agricultural downzoning and urban growth boundaries, which have yielded mixed results in altering exurban development patterns (Newburn & Berck, 2006; Lewis et al., 2009b; Newburn et al., 2022). Although some studies have found certain approaches may reduce the density of exurban development, such as local agricultural zoning policies (McConnell et al., 2006; Newburn & Berck, 2006; Newburn & Ferris, 2016), none have been shown to reduce the rate of farmland conversion to residential land uses (Newburn & Berck, 2006, 2011; Lewis et al., 2009b; Butsic et al., 2011; Newburn & Ferris, 2016). These findings thus reveal a need for alternative growth management strategies to help policymakers preserve the integrity of agricultural and resource areas threatened by this unique form of rural land conversion.

In this study, I analyze the effects of a novel statewide zoning policy implemented in Maryland in 2012 that tackle exurban, large-lot development. In contrast to conventional agricultural zoning policies which restrict the maximum density of residential development (i.e., minimum lot sizes), the new statewide regulation instead takes a unique quantity-based approach that restricts the maximum number of residential lots that can be built in new residential subdivisions in agricultural and conservation areas beyond existing/planned public sewer infrastructure. The statewide policy, known as the septic law, is essentially a new regulatory approach that restricts landowners' development rights to a "minor" residential subdivisions – defined as seven lots or fewer by the new policy – in priority resource areas often already zoned for land, agricultural, or resource protection by local county governments. Hence, the statewide policy strengthens local jurisdictions' existing land use regulations to combat exurban sprawl by placing a strict cap on the total number of lots that can be built on new subdivisions in downzoned areas (designated by local governments). Meanwhile, "major" subdivisions (defined as eight or more lots) on septic systems are still permitted in other rural regions, which are typically already zoned for large-lot residential develompent (i.e., business as usual). By 2037, the septic law aims to prevent 50,000 new residential septic systems, reduce as much as 1.1 million pounds of nitrogen pollution from entering the Chesapeake Bay, and prevent the loss of at least 100,000 acres of forest and farmland (Maryland Department of Planning, 2013).

I exploit this plausibly exogenous change in state land use policy (from local land developers' perspectives) as a natural experiment to analyze the effectiveness of quantity-based zoning restrictions for managing exurban development patterns. To do so, I estimate the joint density-timing impacts of the statewide policy using a spatially explicit panel dataset of rural residential subdivision activity from six urbanizing counties in Maryland (mostly in the greater Baltimore and Washington, D.C. commuting zones). Specifically, I estimate a panel Heckman selection model of rural land conversion with two jointly estimated stages. The first stage is a discrete-time, single-event duration model to estimate the landowner's decision on whether to develop or remain undeveloped in each period of my study. The second stage then estimates the choice of residential density (i.e., the number of residential lots per acre), conditional on being chosen for development in the first stage. Notably, the Heckman selection model corrects for potential sample selection bias in the second model stage by accounting for correlation between both land use decisions (Heckman, 1979). Land use decisions in both stages are estimated using fine-scale parcel and neighborhood attributes, including tier designation, zoning designation, accessibility to employment centers and transportation infrastructure, land quality and septic suitability, and surrounding land uses.

Two main factors complicate the identification of program impact in both model stages. First, it is unlikely the locations where the state policy prohibits large subdivision development is an exogenous determinant of a parcel's value for development, given that downzoned areas tend to be rural regions previously zoned by local governments for agricultural and conservation purposes. A second concern are potential spatial spillovers *within* downzoned areas (where major subdivisions are prohibited under the septic law), specifically on parcels whose development rights are not explicitly affected by the new quantity-based restriction (that is, parcels whose existing development rights fall below the maximum quantity threshold under the state law). Such spillovers may lead to biased estimates of program effectiveness using quasi-experimental approaches employed in prior studies, such as spatial difference-in-differences (DID), that only control for baseline differences across downzoned and non-downzoned areas (e.g., Cunningham 2007; Dempsey & Plantinga 2013; Newburn & Ferris 2016; Towe et al. 2017) to identify the treatment effects of a new land use policy. Hence, I employ a triple-difference (DDD) modeling formulation to address both identification concerns simultaneously, described in detail below.

The first difference of my DDD estimator compares the differences in both land use outcomes between treated and control units in downzoned areas where major subdivision development is prohibited. I define treated and control units as those authorized for major and minor residential subdivision development, respectively, in the pre-regulatory period (i.e., in absence of the statewide zoning policy). Following a standard difference-in-difference design, my second difference then compares the relative changes in land use outcomes across treated and untreated units spanning serveral years before (2007-2011) and after (2013-2019) policy implementation in 2012. Lastly, my third difference compares the double-difference estimate for downzoned rural areas with other rural areas unaffected by the septic law (i.e., where major subdivisions on septic systems are still allowed, per usual). In other words, my DDD model formulation is equivalent to the difference between two difference-in-differences: one for downzoned areas and another for non-downzoned areas. Importantly, this DDD modeling approach controls for baseline differences both within and across downzoned and non-downzoned areas based on whether the new quantity-based development restrictions binds for each individual parcel (in accordance with existing zoning and other land use regulations). Hence, my DDD approach compares the relative differences between major- and minor-eligible parcels in downzoned areas before and after policy implementation, compared to similar groups of parcels in areas unaffected by the new regulation (i.e., where major subdivisions on septic systems are still allowed).

My results indicate that the state land use policy significantly altered exurban development patterns on parcels in downzoned areas that otherwise would have been developable into large-lot residential subdivisions. First, my DDD estimates reveal the state policy reduced both the probability and density of development for this group of downzoned parcels whose development rights were restricted to a minor residential subdivision, as compared to similar parcels in other rural regions where major subdivision development on septic systems is still allowed. Similarly, the average treatment effects indicate that the policy reduced the probability of development by 48.6 percent on downzoned parcels for which the policy binds, compared to the counterfactual rate of residential growth that would have been observed in the absence of the policy, although I do not find statistically significant average treatment effects along the intensive margin. These findings contrast those by Butsic et al. (2011) and Newburn & Ferris (2016) which find no effect on the probability of development of local downzoning policies in rural Wisconsin and Maryland, respectively, although the latter do find a statistically significant reduction on the density of residential development.

My analysis highlights several key findings and contributions to the literature. This is the first study, to my knowledge, which uses a triple-difference modeling approach to estimate the effects of a new land use policy instrument. This estimator is helpful in my study context in controlling for baseline unobservable differences in both land use outcomes analyzed in my model and mitigating potential spatial spillovers within downzoned areas where the new policy heterogeneously binds. Second, the triple-difference estimates suggest that the policy significantly altered exurban development patterns in downzoned areas, particularly along the extensive margin. These findings suggest the state land use policy generated synergistic effects for local land preservation and growth management efforts by effectively strengthening local jurisdictions' existing land use regulations intended to protect rural resource areas from urban sprawl. Third, this is one of the few studies explicitly analyzing the heterogeneous roles of major and minor subdivisions in rural land conversion process. Although minor subdivisions comprise most remaining development rights in my study region, major subdivisions often have larger development footprints and thus disproportionately affect rural resource areas compared to smaller minor subdivisions. Maryland is at the vanguard of land-use policy, as this is the only state to implement zoning restrictions based on lot quantity, rather than maximum density. Hence, this study provides the first analysis of the policy effectiveness, which may be relevant to other states who are aiming to bolster farmland and forest preservation efforts. My analysis suggests that the implementation of the lot quantity-based zoning restrictions reduces the rate of exurban sprawl and thus could provide guidance to other regions interested in implementing similar policies to preserve forests and farmland.

1.1 Background

1.1.1 Policy background and regulatory context

Protecting rural working land, rural resource areas, and other environmentally sensitive areas from urban sprawl has been a driving force behind growth management policy in the United States since the 1950s (Chapin, 2012). Although some anti-sprawl efforts have proven successful at managing some forms of sprawl, they have been far less effective at exurban, large-lot development that often occurs in rural areas far outside existing metropolitan areas (Newburn & Berck, 2006; Lewis et al., 2009b; Newburn et al., 2022). For instance, Newburn & Berck (2006) analyze the effects of urban growth boundaries (UGBs) that constrain the expansion of public services like water and sewer in eight cities located in Sonoma County, California. Their findings suggest that UGBs largely constrain suburban growth, while exurban development (less than one house per acre) that rely on individual septic systems is able to leapfrog into rural regions outside UGBs. Similarly, Newburn & Ferris (2016) find using a spatial difference-in-differences modeling approach that agricultural downzoning did not affect the probability of development in Baltimore County, Maryland (although the authors do find the policy reduced the density of development). Meanwhile, Lewis et al. (2009b) find that priority funding areas (PFAs) for targeted state spending on public infrastructure had little influence on exurban development patterns compared non-PFA areas. These findings reveal a need for new intervention strategies to tackle the unique challenges posed by exurban development that is able to leapfrog far beyond municipal sewer service areas needed for urban and suburban development.

Table 1.1 illustrates the degree to which the septic law's restriction on major residential subdivisions binds for different-sized parcels in Tier IV areas under various minimum lot size restrictions.

Table 1.1: Potential impacts of septic law on subdivision development capacity

Min. lot size (acres/lot)	Parcel area (acres)	Zoned development capacity (lots)[a]	Septic minor development capacity (lots)	Development capacity reduction[b]
2	50	25	7	-18
	100	50	7	-43
	200	100	7	-93
5	50	10	7	-3
	100	20	7	-13
	200	40	7	-33
10	50	5	7	0
	100	10	7	-3
	200	20	7	-13
20	50	2	7	0
	100	5	7	0
	200	10	7	-3

[a] Zoned development capacity = parcel area / minimum lot size
[b] Septic lot reduction = min(septic minor threshold – zoned development capacity, 0)

The policy never binds for parcels whose zoned development capacity in the third column (that is, what is allowed by existing minimum lot size restrictions) falls below the local threshold for minor subdivisions (fourth column). Typically, these are either smaller-sized rural parcels and/or parcels in very-low-density agricultural zoning districts (e.g., 10-25 acres/lot). Conversely, for parcels whose zoned development capacity is above the "septic minor" threshold – that is, parcels that otherwise would have been able to develop into a major residential subdivision – the degree to which the policy binds increases as the zoned development capacity increases. Typically, these are parcels in rural areas zoned for large-lot development (e.g., 2 acres/lot or less) or larger parcels (100-200 acres) in very-low-density agricultural and conservation zoning districts.

1.2 Econometric model

I draw on an extensive literature in economics using reduced-form models of parcel-level land use change in developing my econometric model. The conversion of rural land to residential development is typically modeled as an "optimal stopping problem" in the theoretical literature (e.g., Turnbull 1988; Capozza & Helsley 1989; Turnbull 1991), in which landowners choose the optimal development time t that maximizes the discounted profits per unit of land. In this framework, the original "parent" parcel is treated as the observation unit, for which the landowner decides whether to subdivide into multiple residential lots or to remain in productive, undeveloped uses (e.g., agricultural production). It is assumed that development decisions are irreversible, so once a parcel is developed, it is no longer considered developable in subsequent periods. Conditional on being undeveloped in the current period, parcel i is expected to convert to developed uses in time period t in which the expected net returns to development exceed those from productive, undeveloped uses. In practice, estimating landowners' returns from residential development is empirically challenging, given the lack of detailed revenue and cost data from real estate developers. As an alternative, many empirical studies specify a latent profit function of the net returns from residential development based on observable parcel-level attributes. Doing so, one can approximate the optimal timing of development as modeled in the theoretical literature.

Following Newburn & Ferris (2016), I analyze the effects of the state policy on the probability and density of development using a two-stage, reduced-form model of rural land conversion. The first stage of my econometric model analyzes the extensive margin impacts of the policy (i.e., the likelihood of development) by estimating a single-event duration model of residential subdivision development.[1] While helpful in approximating the timing of rural land conversion, duration models fail to capture other relevant land use outcomes, such as the type of residential development that occurs (Wrenn & Klaiber, 2019). Hence, the second stage of my model estimates the impacts of the policy on the density of development to analyze the intensive margin impacts.[2]

Accounting for potential correlation between both stages of my model is critical to avoiding potential sample selection bias, based on which parcels are ultimately chosen for residential development. Hence, I estimate a Heckman selection model with two stages that are jointly estimated Heckman (1979). In the first stage, a panel probit model estimates the landowner's discrete decision of whether to develop or remain undeveloped. In the second stage, I assess the impact of the policy on the density of development, conditional on being chosen for subdivision development in the first stage. Both stages of my model are described in detail below.

1.2.1 First stage: extensive margin impacts on exurban development

In the first stage, I assume a profit-maximizing landowner for parcel i decides in each time period t whether to develop his land or remain undeveloped based on the expected net returns from development. Lacking explicit revenue and cost data from rural residential developers, the single-event, latent profit equation from residential development of parcel i in time t is

[1]Single-event, reduced form duration models have been widely employed to analyze the timing of land use conversion in a variety of contexts (e.g., Irwin & Bockstael 2002; Newburn & Berck 2006; Cunningham 2007; Towe et al. 2008; Butsic et al. 2011). These models consider the fact that an action taken in period t was not taken in any prior periods ($T < t$), thus making them suitable for analyzing the timing decisions in land use change models.

[2]Several recent papers have adopted other econometric approaches to investigate the joint density-timing outcomes of rural land conversion, see, e.g., Hite et al. 2003; Towe 2011; Newburn & Ferris 2016; Wrenn & Klaiber 2019.

specified:

$$\begin{aligned} y_{it}^* =& M_i\beta_1 + Z_i\beta_2 + M_iZ_i\beta_3 + \tau\beta_4 + M_i\tau\beta_5 + Z_i\tau\beta_6 + M_iZ_i\tau\beta_7 \\ &+ X_{it}\beta_8 + \theta_{it}\beta_9 + \alpha_t + \alpha_j + \varepsilon_{it}, \end{aligned} \tag{1.1}$$

where y_{it}^* are the latent expected returns from development of parcel i in time period t, M_i is an indicator variable equal to 1 if parcel i is authorized for major subdivision development in accordance with *existing* minimum lot-size zoning regulations (i.e., in absence of the policy's restriction on major subdivision development in Tier IV areas) and 0 otherwise (i.e., only ever eligible for a minor subdivision), Z_i is an indicator equal to 1 if parcel i resides in Tier IV area (where no major subdivisions are allowed by the new policy) and 0 otherwise (i.e., Tier III areas), X_{it} is a vector of other observable parcel- and neighborhood-level attributes hypothesized to affect the expected returns from residential development (e.g. slope, elevation, distances to major employment centers, etc.) for parcel i, ϕ_{it} is a vector of exclusion restrictions omitted from the second stage of my model for identification purposes, α_t are annual time fixed effects, α_j are county-level fixed effects, and ε_{it} is an idiosyncratic error term clustered at the parcel level.

My DDD estimator is thus implemented in Eq. (1.1) via the Z_i, M_i, and τ indicator variables and their corresponding two- and three-fold interaction terms. The M_i variable controls for the baseline difference in the probability of development for parcels that, all else equal, are potentially developable into major residential subdivisions in the absence of the policy ($M_i = 1$), as compared to parcels that are only eligible for minor subdivision development ($M_i = 0$). Similarly, Z_i controls for the baseline difference in the probability of development for parcels in Tier IV-designated areas ($Z_i = 1$), which tend to be remote rural areas zoned for agricultural and resource protection, as compared to Tier III areas ($Z_i = 0$)

which tend to be rural residential areas zoned for large-lot development.[3] Hence, the two-fold interaction term $M_i Z_i$ captures the additional effect of being eligible for major subdivision development *and* residing in a Tier IV-designated area with respect to the baseline differences captured by M_i and Z_i. The τ variable captures the baseline difference in the probability of development in the post-regulatory period (2013-2019), all else equal, compared to the pre-policy baseline (2007-2011). The $M_i\tau$ and $Z_i\tau$) interaction terms capture the additional changes in the probability of development in the post-regulatory period relative to their respective pre-policy baselines. Lastly, the main policy parameter of interest – the triple interaction term $M_i Z_i \tau$ – thus identifies the effect of the development restrictions on major residential subdivisions in Tier IV areas imposed by the septic law. Assuming the restriction on major subdivision development reduces the expected returns to development, all else equal, I expect β_7 to be negative.

In practice, the latent variable y_{it}^* for net returns to development is unobserved by the econometrician. Development status, however, is observable. Hence, I create a binary dependent variable y_{it} that takes a value of one if parcel i is converted to residential use in time t and zero otherwise. The land use decision is thus interpreted as the probability that the discounted net present value of the expected returns to development is maximized, that is:

$$y_{it} = \begin{cases} 1 & \text{if } y_{it}^* \geq 0 \\ 0 & \text{if } y_{it}^* < 0 \end{cases} \tag{1.2}$$

[3]Another potential identification concern is unobserved heterogeneity in local land use regulations, which may vary within and across local jurisdictions over space and time. I conduct two sets of robustness checks to explore the sensitivity of my results to local unobserved heterogeneity, described at the end of my results section later in the article. Generally, my main findings do not qualitatively change or slightly improve when accounting for additional local unobserved heterogeneity.

In probabilistic terms, the corresponding land use decision is specified:

$$\Pr(y_{it} = 1) = \Pr(y^*_{it} \geq 0) = \Pr(\varepsilon_{it} \geq -\Omega_{it}) = \Phi(\Omega_{it}), \tag{1.3}$$

where $\Omega_{it} = M_i\beta_1 + Z_i\beta_2 + M_iZ_i\beta_3 + \tau\beta_4 + M_i\tau\beta_5 + Z_i\tau\beta_6 + M_iZ_i\tau\beta_7 + X_{it}\beta_8 + \theta_{it}\beta_9 + \alpha_t + \alpha_j$ in Eq. (1.1) and $\Phi(\cdot)$ represents the cumulative normal distribution function.

Eq. (1.3) is estimated using a panel probit model. Specifically, I stack parcel data over time to form an unbalanced panel data set. This framework is analogous to a discrete-time proportional hazard model, where the time fixed effects account for the baseline probability of development in time t (see, e.g., Beck et al. 1998).

1.2.2 Second stage: intensive margin impacts on exurban development

Conditional on being chosen for residential development, I analyze the impact of the septic law on the extensive margin in the second stage of my Heckman selection model. The dependent variable for the second stage is a latent variable for the natural logarithm of residential density (i.e., the number of residential lots per acre) if parcel i were developed in time t, denoted $\ln(D^*_{it})$. However, I only observe $\ln(D^*_{it})$ for the subset of parcels that are developed and do not consider this variable otherwise (that is, $\ln(D_{it}) = \ln(D^*_{it})$ if and only if $y_{it} = 1$). As in my first stage, I assume that $\ln(D^*_{it})$ is a linear function of observable attributes that affect the expected returns from residential development, specified as follows:

$$\begin{aligned}\ln(D^*_{it}) = \gamma_0 + &M_i\gamma_1 + Z_i\gamma_2 + M_iZ_i\gamma_3 + \tau\gamma_4 + M_i\tau\gamma_5 + Z_i\tau\gamma_6 + M_iZ_i\tau\gamma_7 \\ &+ X_{it}\gamma_8 + \lambda_t + \lambda_j + \eta_{it}.\end{aligned} \tag{1.4}$$

The right-hand side of (1.4) is equivalent to that of (1.1) except for θ_{it}, which I omit from my second stage for identification purposes. Like the first stage, I expect that the restrictions on major subdivision development will reduce the density of residential development on parcels for which the septic law potentially binds. Thus, I expect γ_7 to be negative.

Eq. (1.1) and Eq. (1.4) are estimated simultaneously via a full information maximum likelihood (FIML) Heckman selection model with the following error structure:

$$\begin{bmatrix} \varepsilon_{it} \\ \eta_{it} \end{bmatrix} = N\begin{pmatrix} 0 \\ , \\ 0 \end{pmatrix} \begin{bmatrix} 1 & \rho \\ & \sigma^2 \end{bmatrix} \Bigg) \tag{1.5}$$

Parameter ρ in (1.5) represents the coefficient of correlation between (1.1) and (1.4): a positive value would suggest that parcels selected for development develop at higher residential densities than would occur for undeveloped parcels, while a negative value would suggest the opposite. Regardless of sign, failure to account for correlation across both residential outcomes may result in inconsistent parameter estimates.

1.2.3 Marginal and average treatment effects

Model coefficients cannot be interpreted as marginal effects in non-linear models, such as the probit model in my first stage (Ai & Norton, 2003). Hence, I explicitly estimate the marginal effects for covariates included in the first-stage probability of development and second-stage residential density equations, as described below.

Let $K_{it} = \{Z_i, M_i, X_{it}, \theta_{it}, \tau, \alpha_j, \lambda_j, \alpha_t, \lambda_t\}$ be a vector of covariates included in equations (1.3) and (1.4). Beginning with the first stage, marginal effects on the annual probability of development are calculated for continuous variables as follows:

$$\frac{\partial \Pr[Y_{it} = 1 | K_{it}]}{\partial k_{it}^c} = \frac{\partial \Phi[K_{it}\beta]}{\partial k_{it}^c} = \beta^c \Phi'(K_{it}\beta), \tag{1.6}$$

where β_c is the regression coefficient for the continuous variable k_{it}^c.

First-stage marginal effects for discrete variables, in contrast, are more appropriately derived by partial differences rather than partial derivatives (Corneließen & Sonderhof, 2009), i.e.:

$$\begin{aligned}\frac{\partial \Pr[Y_{it}=1|K_{it}]}{\partial k_{it}^d} &= \Pr[Y_{it}=1|K_{it}, k_{it}^d=1] - \Pr[Y_{it}=1|K_{it}, k_{it}^d=0] \\ &= \Phi[K_{it}\beta|k_{it}^d=1] - \Phi[K_{it}\beta|k_{it}^d=0]\end{aligned} \tag{1.7}$$

for the discrete variable $k_{it}^d \in K_{it}$.

Analogously, first-stage marginal effects for two- and three-fold interaction terms, such as those used to construct my DDD estimator, are derived by taking the second and third differences of (1.7) with respect to each variable component, e.g.:

$$\begin{aligned}\frac{\partial^2 \Pr[Y_{it}=1|K_{it}]}{\partial k_{it}^d \partial k_{it}^{d\prime}} &= \frac{\partial}{\partial k_{it}^{d\prime}}\left[\frac{\partial \Pr[Y_{it}=1|K_{it}]}{\partial k_{it}^d}\right] \\ &= \left\{\Phi[K_{it}\beta|k_{it}^d=1, k_{it}^{d\prime}=1] - \Phi[K_{it}\beta|k_{it}^d=1, k_{it}^{d\prime}=0]\right\} \\ &\quad - \left\{\Phi[K_{it}\beta|k_{it}^d=0, k_{it}^{d\prime}=1] - \Phi[K_{it}\beta|k_{it}^d=0, k_{it}^{d\prime}=0]\right\}\end{aligned} \tag{1.8}$$

for discrete variables k_{it}^d and $k_{it}^{d\prime}$. Hence, marginal effects for the triple interaction term of my DDD model are estimated by taking the cross difference of (1.8) in both model stages.

In the second stage, the marginal effect for each covariate on the natural log of residential density is conditional upon a parcel being selected for development in the first stage. For continuous variables $k_{it}^c \in K_{it}$, marginal effects are calculated:

$$\frac{\partial \mathbb{E}[\ln D_{it}|Y_{it}=1, K_{it}]}{\partial k_{it}^c} = \gamma^c - \rho\left(\frac{\phi[K_{it}\beta]}{\Phi[K_{it}\beta]}\right)\left(K_{it}\beta + \frac{\phi[K_{it}\beta]}{\Phi[K_{it}\beta]}\right), \tag{1.9}$$

where γ^c is the model coefficient on covariate k_{it}^c and $\phi(\cdot)$ is the normal probability density function. Importantly, (1.9) accounts for the direct effect of covariate k_{it}^c (via γ^c) as well as the indirect effect on which parcels are selected for development.

As in the first stage, marginal effects for discrete variables on the density of residential development are obtained by partial differences rather than partial derivatives, e.g.:

$$\frac{\partial \mathbb{E}[\ln D_{it}|Y_{it}=1, K_{it}]}{\partial k_{it}^d} = \mathbb{E}[\ln D_{it}|Y_{it}=1, k_{it}^d=1, K_{it}] - \mathbb{E}[\ln D_{it}|Y_{it}=1, k_{it}^d=0, K_{it}], \tag{1.10}$$

for discrete variable $k_{it}^d \in K_{it}$. Marginal effects for double- and triple-difference interaction terms are analogously obtained by taking the second and third differences, respectively.

Lastly, I estimate the average treatment effects (ATE) on the first-stage annual probability of development and the second-stage density of development for the subset of Tier IV parcels whose development rights are explicitly restricted by the policy. The ATE is defined as the expected average difference between the *observed* outcome, Y_{it}, and the *counterfactual* outcome, Y_{it}^0, had the restrictions on major subdivision development in Tier IV areas not been imposed under the state policy. Following Puhani (2012), the ATE on the annual probability of development (denoted $\delta_{\Pr(dev)}$) is:

$$\begin{aligned} \delta_{\Pr(dev)} &= \mathbb{E}[y_{it}|M_{it}=1, Z_{it}=1, \tau=1, \Psi_{it}\beta] - \mathbb{E}[y_{it}^0|M_{it}=1, Z_{it}=1, \tau=1, \Psi_{it}\beta] \\ &= \Phi(\beta_1+\beta_2+\beta_3+\beta_4+\beta_5+\beta_6+\beta_7+\Psi_{it}\beta) \\ &\quad - \Phi(\beta_1+\beta_2+\beta_3+\beta_4+\beta_5+\beta_6+\Psi_{it}\beta) \end{aligned} \tag{1.11}$$

where $\tilde{\Psi}_{it} = X_{it}\beta_8 + \theta_{it}\beta_9 + \alpha_t + \alpha_j$.[4] Eq. (1.11) shows that the treatment effect is the incremental effect of the coefficient for the DDD parameter of interest ($M_i Z_i \tau$). Note that,

[4] Although Puhani (2012) specifically focuses on treatment effects in nonlinear DID models, the general findings also hold for nonlinear DDD models. Refer to Puhani (2012) for additional details on treatment effects in nonlinear DID models.

due to the nonlinearity of the DDD probit model, the treatment effect on the annual probability of development is not constant across the population, as in a linear model. Hence, the reported first-stage ATE is calculated by taking the average across all treated units for which the policy binds.

In the second stage, the ATE for the residential density (denoted δ_{Dens}), conditional on development, is:

$$\begin{aligned}
\delta_{Dens} &= \mathbb{E}[D_{it}|Z_{it}=1, M_{it}=1, \tau=1, \Psi_{it}\beta] - \mathbb{E}[D^0_{it}|Z_{it}=1, M_{it}=1, \tau=1, \Psi_{it}\beta] \\
&= \exp(\beta_1+\beta_2+\beta_3+\beta_4+\beta_5+\beta_6+\beta_7+\Psi_{it}\beta) \\
&\quad - \exp(\beta_1+\beta_2+\beta_3+\beta_4+\beta_5+\beta_6+\Psi_{it}\beta) \\
&= \exp(\beta_7),
\end{aligned} \tag{1.12}$$

Hence, unlike the first stage, the marginal effect on the density of development is constant across the population.

1.3 Data

To estimate my model, I construct a unique panel dataset of parcel subdivision activity for six urbanizing counties in Maryland: Anne Arundel, Baltimore, Cecil, Charles, Frederick, and Harford County. Specifically, I restrict my study area to rural regions outside planned/existing sewer service areas that are either designated as Tier III (typically areas zoned for large-lot rural residential development) or Tier IV (typically areas zoned for agricultural and resource conservation) by local jurisdictions (Figure 1.1). To keep the policy setting comparable across local jurisdictions, I focus on rural Tier III and Tier IV areas in urbanizing Maryland counties that sit along the broader Baltimore-Washington regional development frontier. This sample captures the part of the state where the septic law is most relevant to exurban subdivision decisions and where parcel records, plat histories, and tier maps can be linked consistently over time. Under current Census delineations, five of the six counties fall within the Washington-Baltimore-Arlington combined statistical area, while Cecil County is linked instead to the Philadelphia-Camden-Wilmington system. I therefore treat the sample as a policy-relevant set of urbanizing Maryland counties while recognizing that Cecil is the clearest regional outlier.[5]

Figure 1.1: Tier maps for study area: Anne Arundel, Baltimore, Cecil, Charles, Frederick, and Harford

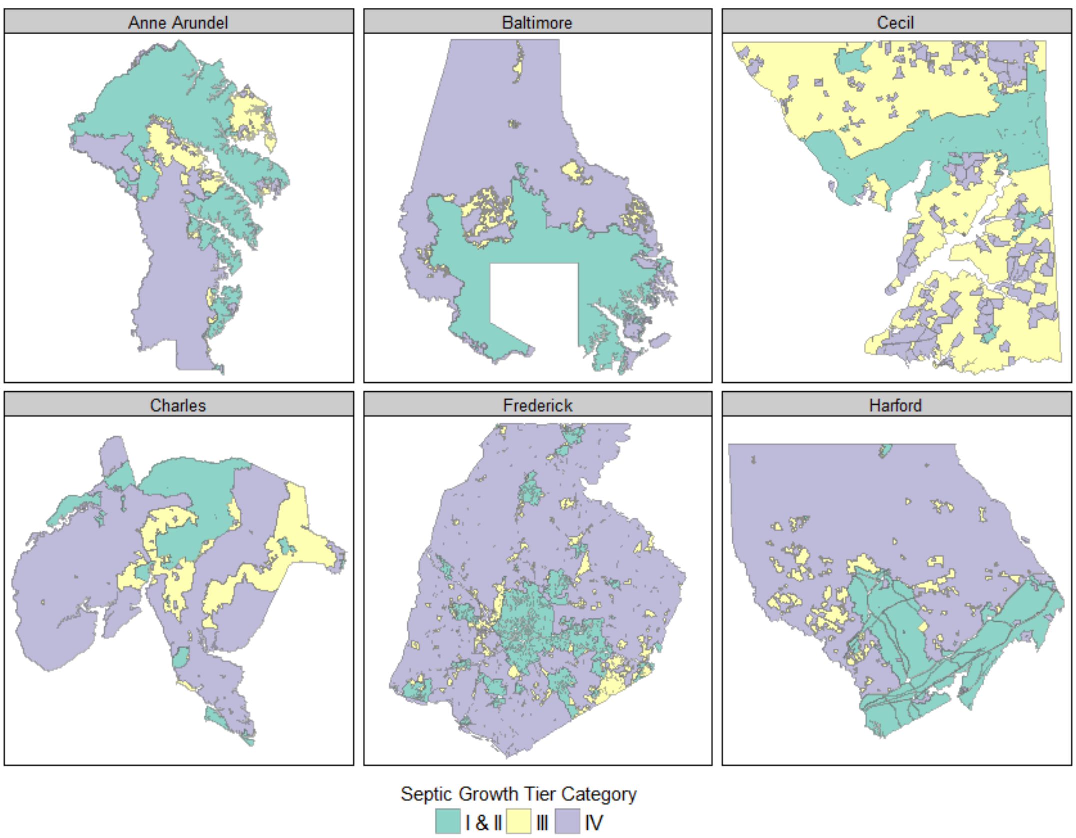


Figure source: created by author using GIS tier maps for each jurisdiction.

Within my study area, I compile fine-scale data on parcel-level land use, location, and other geographical variables from multiple sources.

[5]The robustness checks reported later show that excluding Charles and Harford Counties, two outlying counties that also adjusted their local minor-subdivision thresholds after passage of the state law, does not qualitatively change the main DDD estimates.

The primary source of historical GIS land use data comes from digital parcel polygon tax records maintained by each local jurisdiction. Within these tax records, I only observe subdivided parcels and the subdivision plat maps to which they belong – not the original "parent" parcel as it existed before subdivision. To recover the original parcel boundaries, I aggregate all spatially contiguous parcels sharing the same subdivision plat map identifier and calculate the total number of subdivided lots and the average development density (lots/acre) for each parent parcel. Lastly, I identify the date of final planning approval for each subdivision event by matching the plat identifier with local historical plat records. I use final plat approval date to designate the year in which residential subdivision occurred in my dataset. Thus, my final parcel polygon dataset for my study region consists of all parent polygons as they existed in 2007 and the history of parcel conversion activity from 2007 through 2019.

Using historical zoning boundaries for each jurisdiction, I calculate the excess zoned capacity (i.e., the maximum number of buildable lots) for each parcel in my dataset. Specifically, I divide the total area of each parent polygon by the minimum lot size allowed per local zoning district regulations, rounded down to the nearest whole number. I then restrict my dataset by dropping all parcels whose zoned capacity is below two buildable lots, keeping only those that can potentially be subdivided into two or more residential lots. Based on each parcel's zoned development capacity, I determine whether a parcel is authorized for major or minor residential subdivision development, based on each jurisdiction's lot threshold for major/minor subdivisions. By doing so, I am able to identify the subset of parcels in treated regions whose development rights are restricted by the policy to a minor residential subdivision (i.e., those that were otherwise zoned for major subdivision development in Tier IV areas).

Table 1.2 summarizes the raw numbers of new residential subdivisions, lots, acreages developed, and average residential densities before (2007-2011) and after (2012-2019) the state passed the septic law, based on the subdivision potential (authorized minor or major) and zoning type (Tier III or IV).

Table 1.2: New subdivisions, residential lots, acreage developed, and average density by septic growth tier and subdivision development potential

	Tier III		Tier IV	
Authorized subdivision type	2007-2011	2012-2019	2007-2011	2012-2019
Subdivisions				
Authorized major	397	264	96	58
Authorized minor	196	111	115	131
Lots built				
Authorized major	1,251	865	492	235
Authorized minor	731	412	540	640
Acreage developed				
Authorized major	11,511	9,654	5,921	6,568
Authorized minor	2,190	1,645	4,150	6,191
Average density (lots/acre)				
Authorized major	0.224	0.225	0.192	0.116
Authorized minor	0.487	0.425	0.3	0.313

Across both regions, the total number of subdivisions fell in the post-regulatory period. In Tier III areas, the number of subdivision developments fell roughly 30 percent on parcels authorized for major subdivision development and 40 percent on parcels authorized for minor subdivision development. In Tier IV areas, the number of subdivision developments fell by roughly 40 percent on parcels authorized for major subdivisions, while the total number of subdivisions on parcels authorized for minor subdivisions increased approximately 10 percent. Meanwhile, acreages developed fell in Tier III areas and increased in Tier IV areas in the post-regulatory period. Furthermore, the average residential density fell nearly 50 percent on parcels authorized for major subdivision development in Tier IV areas after the policy intervention, while average densities were relatively unchanged for other parcel and zoning types.

The raw figures in Table 1.2 do not control for parcel characteristics or other market factors that may vary within and across local jurisdictions over time. Thus, I estimate my two-stage Heckman sample selection model outlined in Eqs. 1.1-1.4 to examine the effect of the statewide regulation on the probability and density of exurban development. Land use decisions in both model stages are estimated using geospatial covariates hypothesized to affect the returns to residential development, including zoning designation, accessibility to employment centers and transportation infrastructure, land quality, and surrounding land uses. The remainder of this section provides details about the outcome and control variables used in my econometric analysis. Summary statistics for control variables are presented in Table 1.3.

I create three binary indicator variables to implement my DDD modeling approach. The first, M_i, is set equal to 1 if parcel i is authorized for major residential subdivision development (per local zoned density requirements) and equal to 0 if the parcel is only authorized for minor residential subdivision. The second, Z_i, is set equal to 1 if parcel i resides in a Tier IV-designated area (where major subdivisions are no longer allowed) and 0 if parcel i resides in a Tier III area (where major subdivisions on septic systems are still allowed, per usual). The resulting two-fold interaction term M_iZ_i thus identifies the subset of treated parcels before the restrictions on major subdivision development took effect. Lastly, I create a post-regulatory interaction term, τ, which I set equal to 1 in all periods after the septic law was implemented (i.e., 2013-onwards) and equal to 0 otherwise (2011 and earlier).[6] I then interact τ with Z_i, M_i, and M_iZ_i to estimate the post-regulatory impacts of the septic law. The key variable of interest, $M_iZ_i\tau$, thus captures the impact of the restriction on major subdivision development on the subset of parcels in locally designated

[6] I drop 2012 from my estimation sample to avoid potential responses by developers while county governments were implementing the septic law.

Table 1.3: Summary statistics of covariates

	Mean	Std. Dev.	Min	Max
Policy variables				
Tier IV ($Z_i = 1$)	0.6477	0.4777	0.0000	1.0000
Authorized major subdiv. ($M_i = 1$)	0.1692	0.3749	0.0000	1.0000
Parcel characteristics				
log(Acres)	2.5765	1.2726	0.0000	8.2232
Elevation (feet)	131.1640	78.2177	0.0000	530.0000
Average slope (degrees)	0.8612	0.7619	0.0078	8.2540
Hydric soils (% of parcel area)	6.9103	11.2478	0.0000	98.0000
100-year floodplain (% parcel area)	4.2169	12.4793	0.0000	100.0000
Crop productivity index (NCCPI)	0.5423	0.1377	0.0820	0.8950
Distance (in miles) to nearest highway	6.3431	4.7929	0.0233	22.6824
... to Baltimore City	35.4895	14.6781	8.2671	71.6018
... to Washington, D.C.	51.3953	15.7647	14.1257	87.7343
... to local county seat	11.4876	4.7449	0.5241	24.1689
... to nearest rural village	1.5464	1.7178	0.0000	9.0000
Existing dwelling (=1)	0.7277	0.4452	0.0000	1.0000
Ag. UVA (=1)	0.4728	0.4993	0.0000	1.0000
Easement eligibility (=1)	0.5540	0.4971	0.0000	1.0000
Rural Legacy Area (=1)	0.2904	0.4539	0.0000	1.0000
Surrounding land use w/in one mile (% of buffer area around parcel polygon)				
Farmland	39.8348	21.2751	0.0000	91.0000
Forest	40.1960	17.7141	1.0000	88.0000
Water	1.9171	7.1342	0.0000	76.0000
Residential areas	22.6674	12.3049	0.0000	77.1179
Protected areas	11.7338	14.5029	0.0000	83.5745
Jurisdictions				
Anne Arundel	0.0342	0.1818	0.0000	1.0000
Baltimore	0.2800	0.4490	0.0000	1.0000
Cecil	0.1731	0.3784	0.0000	1.0000
Charles	0.0988	0.2984	0.0000	1.0000
Frederick	0.2649	0.4413	0.0000	1.0000
Harford	0.0775	0.2674	0.0000	1.0000
Observations		172,494		
Parent parcels		14,226		
Subdivision events		1,191		
Study period		2007-19		

priority preservation areas that otherwise would have been able to be converted into major residential subdivisions.

In addition, I construct several fine-scale parcel- and neighborhood-level attributes using multiple geospatial data sets to control for other observable features that affect the potential returns to development. Parcel area, represented in natural log form, is expected to increase the probability of development, all else equal. Using detailed soil survey maps from the United States Geological Survey (USGS), I calculate the share of parcel area classified as having hydric soils which inhibit septic system percolation. Higher levels of hydric soils are expected to constrain residential development and thus reduce the likelihood and density of development. Using floodplain maps maintained by the Federal Emergency Management Agency (FEMA), I calculate the share of each parcel that resides in a 100-year floodplain, which is expected to reduce the probability and density of development. I measure each parcel's average slope and elevation using digital elevation maps. Parcels with steeper slopes tend to be more costly to develop and thus are expected to have lower probabilities and densities of development. In contrast, scenic views of parcels at higher elevations may increase the likelihood and density of development.

Conservation easements are an alternative land use option for certain rural parcels, particularly those with high resource values. Thus, parcels eligible for enrollment in a conservation easement program may be less likely to develop or may develop at lower densities. To control for easement eligibility, I create a temporally lagged variable indicating whether a parcel is eligible for enrollment in Maryland's two major statewide conservation easement programs: the Maryland Agricultural Land Preservation Fund (MALPF; est. 1976) and the Maryland Environmental Trust (MET; est. 1967). Eligibility for both programs depends on soil quality and parcel size: MALPF requires at least 50 acres (or adjacency to equivalent-sized protected areas) and at least 50% of land area with soil capability classes I, II, or III. MET requires enrolled parcels to be at least 25 acres in size (or adjacency to equivalent-sized protected areas) or have high resource value for smaller parcels (specifically, at least 50% prime farmland soils). I also construct an indicator variable to identify whether a parcel resides in any locally designated Rural Legacy Areas (RLAs) used to provide funding for local conservation efforts. Parcels in designated RLAs may be less likely to be converted to residential uses or develop at lower densities, although empirical evidence suggests this may not be the case (Lynch & Liu, 2007). Lastly, I construct another indicator variable equal to one if a parcel is enrolled in the Maryland agricultural use-value assessment (UVA) program one year before the beginning of my study period (2006). All else equal, I expect to reduce the probability and density of development.

I construct several time-varying neighborhood variables using one-mile buffers generated around the border of each parcel polygon to control for the impacts of surrounding land uses on residential development. First, I measure the share of the surrounding area classified as residential development and conservation easement activity in each period. The effect of surrounding residential land use is ambiguous since neighboring development may generate positive (e.g., access to nearby infrastructure) or negative (e.g., traffic congestion) amenity values. Surrounding conservation easements have an ambiguous effect on the likelihood and density of development since they provide open space amenities and make enrollment easier for neighboring parcels. Finally, using land use/land cover data from the USGS National Land Cover Database (NLCD), I calculate the share of surrounding areas classified as farmland, forestland, water, and wetlands. Generally, I expect high levels of these surrounding characteristics, which tends to denote remote rural areas, to decrease the probability and density of development, all else equal.

Lastly, I create several exclusion restriction variables, which I only include in the first stage of my Heckman sample selection model for identification purposes. I do not expect these excluded instruments to influence the density of development, conditional on being chosen for development in the first stage. The first is a time-varying indicator variable to indicate the presence of an existing dwelling on the original parcel. An existing dwelling may indicate working farmland or the presence of a rural ranch-style house and, thus, may reduce the probability of development relative to undeveloped parcels of land. The second is a time-varying indicator variable for enrollment eligibility into the state's two largest conservation easement programs (MALPF and MET). Conservation easements represent an additional outside option for rural land conversion and thus are expected to lower the probability of development, all else equal. Similarly, I create a binary variable to indicate whether a parcel resides in a Rural Legacy Area (RLA), which provide state funding to state and local conservation easement programs to preserve large, contiguous tracts of agricultural and resource areas with historical value. Parcels in these areas are expected to be less likely to be converted to residential uses than similar non-RLA parcels. Lastly, I create a time-varying indicator variable for enrollment into the state agricultural use-value assessment (UVA) program in the previous year. The UVA is a voluntary program that lowers the tax liability for agricultural working lands.[7] I include these variables only in the first stage because they are intended to shift whether and when a parcel is converted, rather than the density chosen conditional on development. Existing dwellings, easement eligibility, and lagged UVA enrollment proxy fixed conversion costs or outside preservation options that bear most directly on the development decision, consistent with related land-use studies of development timing and preservation incentives (Newburn & Ferris, 2016; Towe et al., 2008; Anderson & England, 2015). Conditional on development, residential density is more directly constrained by zoning, parcel size, and other site characteristics already included in the second stage (Wrenn & Klaiber, 2019; Lewis et al., 2009a).[8]

[7] Landowners who decide to develop a parcel of land enrolled in the UVA must pay additional taxes based on the fair market value. Thus, conversion to residential development incurs an additional fixed cost on parcels actively enrolled in the UVA.

[8] Rural Legacy designation is less clean because it may also proxy targeted preservation geography or local amenity values (Lynch & Liu, 2007; Lewis & Knaap, 2012). I therefore interpret that variable more cautiously than the other three exclusions.

1.4 Estimation results

Table 1.4 presents the triple difference (DDD) estimates and resulting marginal effects of the septic law on the probability and density of residential development.

The estimated correlation parameter $\hat{\rho}$ is -1.057 and statistically significant at the 1% level, suggesting that similar results would not be obtained if both model stages were estimated independently. Standard errors are based on a nonparametric block bootstrap procedure (1,000 replications, with blocks drawn based on parcel ID numbers) and clustered at the parcel level. The delta method is used to compute the standard errors for the marginal effects. The estimated regression coefficients need not have the same sign or statistical significance as the marginal effects in nonlinear models Ai & Norton (2003). Hence, I emphasize the significance of the marginal effects in the subsequent discussion. The estimated model coefficients and marginal effects for covariates used as control variables and exclusion restrictions generally conform to expectations when significant. In the Supplemental section, I discuss my full set of econometric results in detail.

In the baseline period, the marginal effect of the M_i variable is positive in both stages but is only statistically significant in the latter, indicating that parcels authorized for major subdivision development develop at higher residential densities, all else equal, compared to similar parcels that are only eligible for minor subdivision development. In other words, the number of residential lots built per acre increases the total number of development rights (authorized by local land use regulations) increases. The baseline marginal effect of the Z_i variable is negative in both model stages but is only statistically significant in the former, suggesting that parcels in Tier IV-designated areas (which tend to be areas zoned for agricultural and conservation purposes) develop at lower rates compared to similar parcels in Tier III areas (which tend to be areas zoned for large-lot rural residential development). Lastly, the baseline marginal effect for the M_iZ_i interaction term is negative in both model

Table 1.4: Triple difference (DDD) model coefficients and marginal effects of development restrictions on major subdivision on the likelihood and density of development

	Model coefficients		Marginal effects	
Variable	Pr(dev)	ln(density)	Pr(dev)	ln(density)
M_i	-0.06433	0.20010**	0.00019	0.11667**
	(0.0597)	(0.0912)	(0.0006)	(0.0491)
Z_i	-0.05751	-0.06740	-0.00164**	-0.07597
	(0.0534)	(0.0692)	(0.0008)	(0.0600)
$M_i Z_i$	-0.01001	0.12321	-0.00367***	-0.12895
	(0.0783)	(0.1213)	(0.0011)	(0.0845)
τ	-0.53762***	-0.01368	-	-
	(0.0717)	(0.1016)	-	-
$M_i \tau$	0.36660***	-0.12217	0.00283**	-0.18522**
	(0.0778)	(0.1241)	(0.0014)	(0.0933)
$Z_i \tau$	0.03334	0.04995	0.00039	0.00124
	(0.0566)	(0.0853)	(0.0014)	(0.0694)
$\boldsymbol{M_i Z_i \tau}$	**-0.34482*** **	**-0.22828**	**-0.00516** **	**-0.41106*** **
	(0.1083)	**(0.1658)**	**(0.0026)**	**(0.1591)**
Constant	-2.13096***	3.12655***	-	-
	(0.1578)	(0.3104)	-	-
ρ	-1.05767***		-	
	(0.0874)		-	
Parcel controls	✓			
Exclusion restrictions	✓			
Time FE	Annual			
Local FE	County			
Observations	172,494			
Parent parcels	14,226			
Subdivision events	1,191			

Notes: First-stage dependent variable is a binary indicator equal to 1 in the time period a given parcel is approved for residential subdivision development and 0 otherwise. Second-stage dependent variable is the logged density of residential development (residential lots/acre) conditional on development in the first stage. Parcel controls in both model stages include parcel size (in acres); elevation (in feet); slope (in degrees); shares of parcel area containing hydric soils or residing in a 100-year floodplain; crop productivity index (NCCPI); distances (in miles) to nearest highway, Baltimore City, Washington, D.C., local county seats, and nearest rural village; and share of surrounding areas (within one-mile radius of parcel polygons) classified as farmland, forest, water, residential, or protected areas. First-stage exclusion restrictions include four binary indicator variables denoting an existing dwelling (=1), enrollment in the state agricultural use value assessment program (=1), easement eligibility (=1), and residing in a designated rural legacy area (=1). Additional controls include annual time fixed effects and county-level fixed effects. The full set of model results are presented in Supplemental Table A1. Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

stages but is only statistically significant in the former, indicating that parcels authorized for major subdivision development in Tier IV areas develop at lower rates, as compared to similar parcels in Tier III areas, all else equal.

In the post-regulatory period, the marginal effect of the $M_i\tau$ variable is positive in the first stage, negative in the second stage, and statistically significant in both stages. This suggests that the likelihood of development increased, but the development density decreased for parcels still eligible for major subdivision development in the post-regulatory period. This finding is particularly important as it provides the counterfactual time trend for treated units no longer eligible for major subdivision development in the post-regulatory period. The $Z_i\tau$ variable is positive in both model stages but not statistically significant in either stage, indicating no significant change in the likelihood or density of development for Tier IV parcels relative to the baseline period. Lastly, the marginal effect for the primary variable of interest - $M_iZ_i\tau$ - is negative and statistically significant in both model stages. The former suggests that the likelihood of residential development in the post-regulatory period is 0.516 percentage points lower on parcels in Tier IV areas whose development rights were restricted to a minor residential subdivision, as compared to similar parcels in Tier III areas still eligible for major subdivision development. Meanwhile, the latter indicates that there were 0.66 fewer lots per acre on treated Tier IV parcels chosen for residential development relative to the same Tier III comparison group. These findings suggest that the restrictions on major subdivision development in Tier IV areas significantly affected exurban development patterns relative to Tier III areas where there were no new restrictions on major subdivisions.

Table 1.5 reports the average treatment effects for the annual probability and residential density specifically for treated units (i.e., those that otherwise would be eligible for major subdivision development), which I calculate using equations (1.11) and (1.12), respectively. The annual probability of development is 0.626 percent, on average, when their development rights are restricted to a minor residential subdivision under the state policy, as compared

Table 1.5: Average treatment effects on the probability and density of residential development

Outcome variable	Observed	Counterfactual	Difference	Pct. Difference
Annual probability of development	0.00626***	0.01114**	-0.00488	-48.644%**
	(0.0009)	(0.0049)	(0.0046)	(0.2372)
Residential density (lots per acre)	0.07244***	0.09101***	-0.01857	-20.410%
	(0.0070)	(0.0141)	(0.0145)	(0.1320)

Notes: Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

to 1.11 percent for the counterfactual level in the absence of these additional development restrictions. The average treatment effect on the annual probability is -0.00488, which translates to a statistically significant 48.6 percent decrease, on average, in the annual likelihood of development. Meanwhile, the density of development is 0.072 lots per acre, on average, for treated units in the post-regulatory period, compared to the counterfactual 0.091 lots per acre had major subdivision development not been outlawed. The average treatment effect for the density of development is thus -0.019 lots/acre, which translates to a 20.4 percent reduction, although neither is statistically significant. Together, my findings suggest the state policy had the most significant impact on the extensive margin by reducing the average probability of development by nearly 50 percent, on average, on treated parcels no longer eligible for major subdivision development.

It is helpful to contextualize my results to those in prior studies analyzing the effect of other growth management policies for managing exurban sprawl, particularly those using quasi-experimental approaches to account for the non-random application of such policies across the spatial landscape. Butsic et al. (2011) analyze the impact of agricultural zoning on a landowner's decision to subdivide in a rural Wisconsin county. After using propensity score matching methods to account for selection bias of zoning policies, the authors find no effect of zoning on the rate at which agricultural land was converted to residential uses. In other words, the authors find that zoning policies more or less "follows the market" rather than influence residential development patterns. Meanwhile, Newburn & Ferris (2016) investigate the effect of agricultural downzoning on exurban development patterns outside Baltimore, Maryland. Using a two-stage difference-in-differences econometric model, the authors estimate the impacts on both the probability *and* density of rural residential development in agricultural and resource areas that the local county government downzoned in 1976. The authors similarly find no effect of downzoning on the likelihood of development, although they do find the policy significantly reduced the density of development by 54-60% in downzoned areas relative to their respective pre-policy baselines (i.e., pre-1976). In other words, the policy promoted lower-density development but did not stop the rate at which rural agricultural parcels were converted to residential uses in downzoned areas. The authors point to a "minor exemption rule" under the policy as a potential explanatory factor for the policy's shortcomings. Instituted initially as a political compromise, the rule permitted downzoned parcels to be subdivided into a minor residential subdivision, thus allowing some low-density minor subdivision development to occur in downzoned areas. In addition to large-lot zoning, other studies have analyzed the effects of different growth management strategies, such as urban growth boundaries (UGBs), and have found such policies may exacerbate exurban sprawl by restricting the expansion of municipal sewer service areas necessary to support higher-density urban and suburban development (see, e.g., Newburn & Berck 2006, 2011; Lewis & Parker 2021).

This paper's findings in contrast indicate that the statewide restriction on major subdivision development significantly decreased the rate at which rural farmland and forested parcels were converted to low-density, exurban development in agricultural and resource areas (Table 1.5). Further, the triple difference estimates in Table 1.4 suggest that, after controlling for time-invariant unobservables and common temporal effects among plots, the probability and density of development significantly decreased on treated parcels whose development rights were restricted to a minor subdivision, as compared to similar parcels eligible for major subdivision development whose development rights were unaffected by the state policy. Hence, my results suggest the restriction on major subdivision development in agricultural and preservation areas had the most significant impact on the extensive margin (i.e., the probability of development). In other words, the policy reduced the rate of sprawl in high-priority conservation and preservation areas by reducing the rate at which agricultural and forested land was permanently converted to residential uses.

1.4.1 Robustness checks

I conduct several robustness checks to provide empirical evidence to support my DDD identification strategy and to analyze the sensitivity of my estimation results. First, I provide empirical evidence to support the parallel trends assumption for my estimation results to have a causal interpretation. Even though the triple difference is the difference between two difference-in-differences, it does not require two parallel trend assumptions (Corneließen & Sonderhof, 2009). Instead, it requires the relative outcomes between group A (i.e., major-eligible parcels) and group B (i.e., minor-eligible parcels) in areas where the state law restricts major subdivision development to trend in the same way as the relative outcome of group B and group A in other regions unaffected by the new policy. To test this assumption, I conduct a temporal falsification test in the pre-regulatory period (2007-2011) and move the regulatory event to an arbitrary year within the restricted period. I then re-estimated my two-stage DDD model while considering the impact of a false regulatory event in 2009. The false regulatory event in 2009 thus splits my restricted pre-regulatory sample into a false "pre" period (2007-2008) and a false "post" period (2009-2011). A statistically significant point estimate on the DDD estimator $M_i Z_i \tau$ in either model stage would thus violate the null hypothesis of parallel trends by suggesting the presence of potential confounding influences from other time-varying, unobservable factors on rural land conversion decisions. I find no significant differences for either land use outcome using the pseudo-policy intervention, thus supporting the parallel trends identifying assumption for my DDD estimator (Table A3).

Second, I analyze the sensitivity of my econometric results to unobserved variation in local land use regulations, particularly minimum lot size zoning and subdivision development requirements, which may also affect rural landowners' development decisions. These local regulations vary considerably within and across local jurisdictions over space and time and thus may bias my estimates of program effectiveness if ignored. It is ultimately an empirical question, however, whether this additional unobserved heterogeneity qualitatively affects my main econometric results. Thus, I re-estimate my model using two alternate specifications. The first uses fine-scale zoning district fixed effects rather than county-level fixed effects. The second excludes jurisdictions that made substantive changes to their local land use ordinances from my estimation sample. There were only two jurisdictions – Charles and Harford County – that made (relatively minor) changes to their local land use policies, both of which increased their lot thresholds for minor residential subdivisions from 5 to 7 lots in response to the new state policy (no counties in my study made substantive changes to their existing zoning policies). The results of these two robustness checks are presented in Table A4 and Table A5, respectively. Table A4 shows that using zoning district fixed effects does not qualitatively affect my triple difference estimates of the program impacts compared to my estimates obtained when using county-level fixed effects. This suggests that my DDD modeling approach sufficiently captures the baseline, time-invariant differences between treated (Tier IV) areas and untreated (Tier III) areas in my study region. Meanwhile, Table A5 shows that dropping Charles and Harford from my estimation sample does not qualitatively affect my triple-difference estimates on the probability of development. They do, however, strengthen my triple-difference estimates on the density of development, although the main policy parameter of interest is only statistically significant at the 10%. This provides some initial reassurance that the baseline results are not being driven solely by the more outlying counties in my sample. Hence, I do not find that local unobserved heterogeneity does not qualitatively affect my main econometric results in general.

Third, I re-estimate both stages of my model independently to analyze the sensitivity of my model results to selection bias in my Heckman sample selection model. The significance of the estimated correlation parameter ρ suggests that selection in the first-stage development equation may bias the estimation for the second-stage density equation. As expected, the triple-difference estimate on the density of development with the two equations estimated separately differs considerably (roughly double in size) from the model with these equations estimated jointly (Table A6). These results suggest that accounting for potential sample bias selection is crucial to avoiding biased estimates on the density of development.

Lastly, I explore the sensitivity of my triple-difference estimates by excluding certain parcels from my estimation sample that may be less likely to be developed into large-lot rural residential subdivisions. Specifically, I re-estimate my model after: dropping parcels (*i*) 5 acres or smaller or (*ii*) 100 acres or larger; and dropping parcels whose zoned development capacity is only (*iii*) two or (*iv*) three lots. The results presented in Table A7 show that my triple-difference estimates are qualitatively similar to those from my primary model and, in some cases, slightly stronger.

1.5 Conclusion and next steps

In this paper, I analyze the effects of a novel, quantity-based statewide downzoning policy in Maryland to reduce exurban sprawl in agricultural and resource areas, specifically by restricting the development of large residential subdivisions on septic systems that are eight lots or greater in size. Using a two-stage, reduced-form model of rural land conversion, I estimate the effects of the statewide policy on both the probability and density of residential development using a triple-difference modeling approach. My econometric results indicate that the policy significantly altered exurban development patterns on parcels in downzoned areas that otherwise would have been eligible for major subdivision development, particularly on the extensive margin. Specifically, my estimated average treatment effects suggest that the probability of development decreased by 48% on parcels for which the policy explicitly binds, compared to their counterfactual rate of development that would have occurred in the absence of the policy. Meanwhile, the average treatment effects on the density of development are negative but not statistically significant.

Overall, my results suggest that the policy had a minimal impact on the density of development but decreased the rate at which large agricultural and resource parcels were permanently converted to low-density residential land uses. One potential explanation for the policy's low effectiveness in reducing the density of development is that developers may have selected smaller parcels in downzoned areas to develop in response to the ban on major subdivision development on septic systems. In such a scenario, the density of development would be relatively unchanged while the total number of lots would be lower on parcels chosen for subdivision development.

Similar to Newburn & Ferris (2016), my results reveal that the statewide downzoning policy had different effects on the probability and density of development and thus underscore the importance of assessing impacts of new land use policies on both the extensive and intensive margin. My findings also suggest that similar growth management strategies targeting large residential subdivisions on septic systems may have synergistic effects for local zoning regulations and other land use policies aimed at reducing sprawl by effectively strengthening existing local land use regulations to reduce exurban sprawl in areas zoned for agricultural and resource protection. This type of analysis is rare, as most land use policy in the United States is administered locally rather than at the state level. Understanding the design and effectiveness of various land management strategies - and how different strategies may interact with one another - is crucial to help policymakers effectively achieve their long-term land preservation and development goals.

Chapter 2

The role of spatial coordination incentives in restoring contiguous habitat: evidence from the Oregon Conservation Reserve Enhancement Program (CREP)[1]

Payments for ecosystem services (PES) schemes compensate landowners for voluntary conservation efforts that address environmental challenges. These schemes have been widely adopted both domestically and internationally. Within the agricultural sector, many PES programs primarily focus on enrolling individual farms, with compensation often tied to on-site, productivity-related criteria (Wu & Boggess, 1999). While these programs have successfully incentivized farmers to adopt environmentally beneficial practices, they have proven to be largely ineffective in achieving landscape-level outcomes (Bos et al., 2017).

The ineffectiveness at achieving landscape-level outcomes largely stems from the fundamental design of most agricultural PES schemes. These conventional programs focus on individual farm-level actions rather than integrated landscape-level units for conservation interventions (Nguyen et al., 2022). Because of this design, which largely incentivizes isolated efforts, these programs neither require nor actively promote coordination among landowners (Fooks et al., 2016). The resulting scattered and disconnected conservation efforts create fragmentation that often fails to meet the contiguous patterns or ecological thresholds needed for effective restoration of many ecosystem services, such as habitat restoration or water quality improvement (Leventon et al., 2017).

[1] This chapter is co-authored with Lori Lynch, Department of Agricultural and Resource Economics, University of Maryland, 2200 Symons Hall, College Park, MD 20742, United States.

Recognizing these limitations, Smith & Shogren (2001, 2002) proposed the agglomeration bonus that would be added to landowners' participation payments for contiguous parcels, specifically inventivizing adjacent landowners to enroll in conservation progams together. Since this seminal work, a growing body of theoretical and experimental research has examined the effectiveness of various spatial incentive structures, including agglomeration bonuses, threshold bonuses, and threshold payments (see Nguyen et al. 2022 for a review of this literature). These mechanisms differ in how they reward spatial coordination. The agglomeration bonus (AB) supplements standard payments when a landowner's parcel borders other enrolled lands, providing incremental benefits for adjacency. A threshold bonus (TB) instead conditions a bonus payment on reaching a predefined target for total contiguous enrollment, awarding participants only once cumulative conservation effort meets the threshold. Threshold payments (TP) go further, offering compensation only after the coordination threshold is met—without layering onto any baseline payment.

Similarly, two main types of programs utilizing spatial incentives have emerged. In a coordination program, landowners act individually, but the program incentivizes a specific spatial pattern, prompting landowners to consider the spatial aspect in their decisions. The program tracks enrollment to determine if the spatial goal is reached. Alternatively, collaboration programs encourage adjacent landowners to work together to achieve a landscape-level conservation target and jointly receive the bonus.

A growing body of literature analyzing spatial coordination incentives' ability to promote the spatial connectivity of conservation activities. Among these designs, the agglomeration bonus has been most widely studied. For instance, theoretical studies predict the AB can reduce monitoring costs (Bell et al., 2016) and mitigate leakage of environmentally damaging practices outside target areas (Delacote et al., 2016) through positive spatial interactions among neighboring landowners. At the same time, Smith & Shogren (2001, 2002) caution that the AB's performance could decline if landowners know the regulator's preferences, potentially leading to rent-seeking behavior that undermines cost-effectiveness. Experimental work largely confirms that spatial incentives boost participation and contiguity—though the magnitude and environmental payoff can vary by program design and context (Parkhurst & Shogren, 2007; Banerjee et al., 2014; Fooks et al., 2016; Banerjee, 2018; Kuhfuss et al., 2022).

Despite these theoretical and experimental insights, empirical evaluations of operational payments for ecosystem services (PES) with built-in spatial coordination remain scarce. To date, evidence on AB effectiveness comes primarily from two settings. In Malawi, studies find that an agglomeration bonus significantly raised enrollment rates in national conservation programs (Bell et al., 2018b,a). In Switzerland, researchers document that ABs increased both scheme uptake and the spatial clustering of enrolled parcels (Krämer & Wätzold, 2018; Huber et al., 2021). The limited geographic scope of these evaluations makes it difficult to generalize findings across diverse landscapes and mechanism designs (Nguyen et al., 2022).

We address the empirical gap in understanding how threshold-based payments shape the spatial connectivity of conservation actions by examining a novel TB scheme under the Oregon Conservation Reserve Enhancement Program. The program offers a one-time Cumulative Impact Incentive Payment (CIIP) in addition to base rental rates when landowners within any continuous five-mile stream segment enroll more than half of the total streambank length. This makes Oregon's TB design unique among U.S. CREP schemes and explicitly seeks to align landowner decisions with watershed-scale habitat objectives for salmon and trout.

Our primary focus is on the endogenous spatial interactions that emerge as neighbors respond to the CIIP incentive. We hypothesize that existing enrollments raise the expected return for adjacent parcels, thereby motivating previously unenrolled landowners to join the program. Coordination may occur directly—through peer-to-peer encouragement—or indirectly, as agency field staff target outreach toward parcels that push a segment past the

50 percent threshold. We pay special attention to how enrollment probabilities change as cumulative streambank protection nears or crosses this critical cutoff.

In this study, we assemble a parcel-level panel of CREP enrollments from 1998 to 2009 and geo-link each tract to a detailed stream-network map. This structure enables us to estimate the extent to which *hydrologically connected* enrollment influences subsequent conservation enrollment activity via positive or negative spillovers. By tracking cumulative streambank enrollment over time within every five-mile segment intersecting a parcel, we can distinguish between CIIP-relevant neighbor effects and broader spatial influences. Our recursive panel specification leverages variation in enrollment timing and categorical bins of neighboring enrollment shares to study the threshold dynamics at the heart of the incentive design.

As a preview of our findings, we find evidence that hydrologically connected enrollment influences subsequent conservation activity, and the strength of these spatial interactions grows as parcels near bonus eligibility. Our results suggest that, on average, the probability of enrollment rises by 12.4 percentage points when neighboring landowners have protected 10–20 percent of a five-mile stream segment, relative to parcels where adjacent enrollment is minimal (1–10 percent protection). This effect grows to a 22.4 percentage point increase when neighbors reach 41–50 percent protection. Once cumulative enrollment surpasses the 50 percent threshold and triggers the CIIP bonus, the likelihood of participation jumps to 50.5 percent.

2.1 Background

In the Pacific Northwest, declines in native salmon and steelhead populations have spurred substantial conservation investments (Wu & Skelton-Groth, 2002). Since the 1980 enactment of the Pacific Northwest Electric Power Planning and Conservation Act, an estimated billion has allocated toward recovery efforts in the Columbia River Basin (Jaeger & Scheuerell, 2023). Despite these expenditures, water temperature and dissolved-oxygen standards – key determinants of salmonid productivity – are still not met in many streams (ODEQ, 2022). These impairments are linked to riparian vegetation removal, water diversions, and stream channel alterations associated with agriculture, logging, mining, and irrigation. Restoration efforts to date have typically been implemented on a parcel-by-year basis, guided by site-specific criteria such as soil erosion rates rather than coordinated watershed-scale plans (Barnas et al., 2015).

In 1998, Oregon became one of the first states to implement a Conservation Reserve Enhancement Program (CREP) under the USDA's Conservation Reserve Program (CRP), forging a state-federal partnership tailored to local environmental priorities.[2] The Oregon CREP specifically targets riparian restoration for salmon and steelhead by encouraging landowners to establish vegetative buffers along qualifying streambanks.[3] Participants enter 10- to 15-year contracts in exchange for annual rental rates calibrated to soil productivity and local conditions, cost-share funding for buffer installation, maintenance payments, Stewardship Incentive Payments (SIP), and Practice Implementation Payments (PIP).[4] Certain practices may also qualify for additional incentive payments equal to 25–50 percent of the base rental rate, providing further motivation to adopt priority conservation measures.

Oregon's CREP also includes a unique spatial coordination incentive – the only one of its kind among all state CREP agreements – known the Cumulative Impact Incentive Payment (CIIP). Under this threshold-based bonus, landowners within any continuous five-mile stream segment become eligible for a one-time payment equal to four times the base rental rate when

[2]CREP is a subsidiary of the USDA's Conservation Reserve Program (CRP) that is focused on high-priority conservation issues – such as water supply protection, threatened-species habitat loss, soil erosion, and fish population declines – and typically offers longer contracts with enhanced incentives (practice implementation payments, cost-sharing, etc.) compared to standard CRP agreements (Allen, 2005).

[3]Environmental goals of the Oregon CREP include reducing water temperature to natural levels; minimizing sediment and nutrient pollution from adjacent farmland; restoring streambank vegetation to properly functioning conditions; and stabilizing streambanks to prevent erosion during non-flood flows (USDA-FSA, 2009).

[4]Refer to Figure S1 in the Appendix for additional financial incentives offered through the Oregon CREP.

at least half of that segment's streambank is enrolled in the program. Importantly, this 50 percent threshold is calibrated to ensure restoration reaches critical ecological tipping points – nutrient and sediment filtration, bank stabilization, and sufficient riparian shading to lower water temperatures – key drivers of salmonid spawning success and juvenile survival (USDA-FSA, 2009). By explicitly linking bonus eligibility to these habitat thresholds, the CIIP internalizes watershed-scale benefits, aligns individual enrollment decisions with landscape-level conservation objectives, and fosters cross-property coordination for contiguous riparian habitat formation.

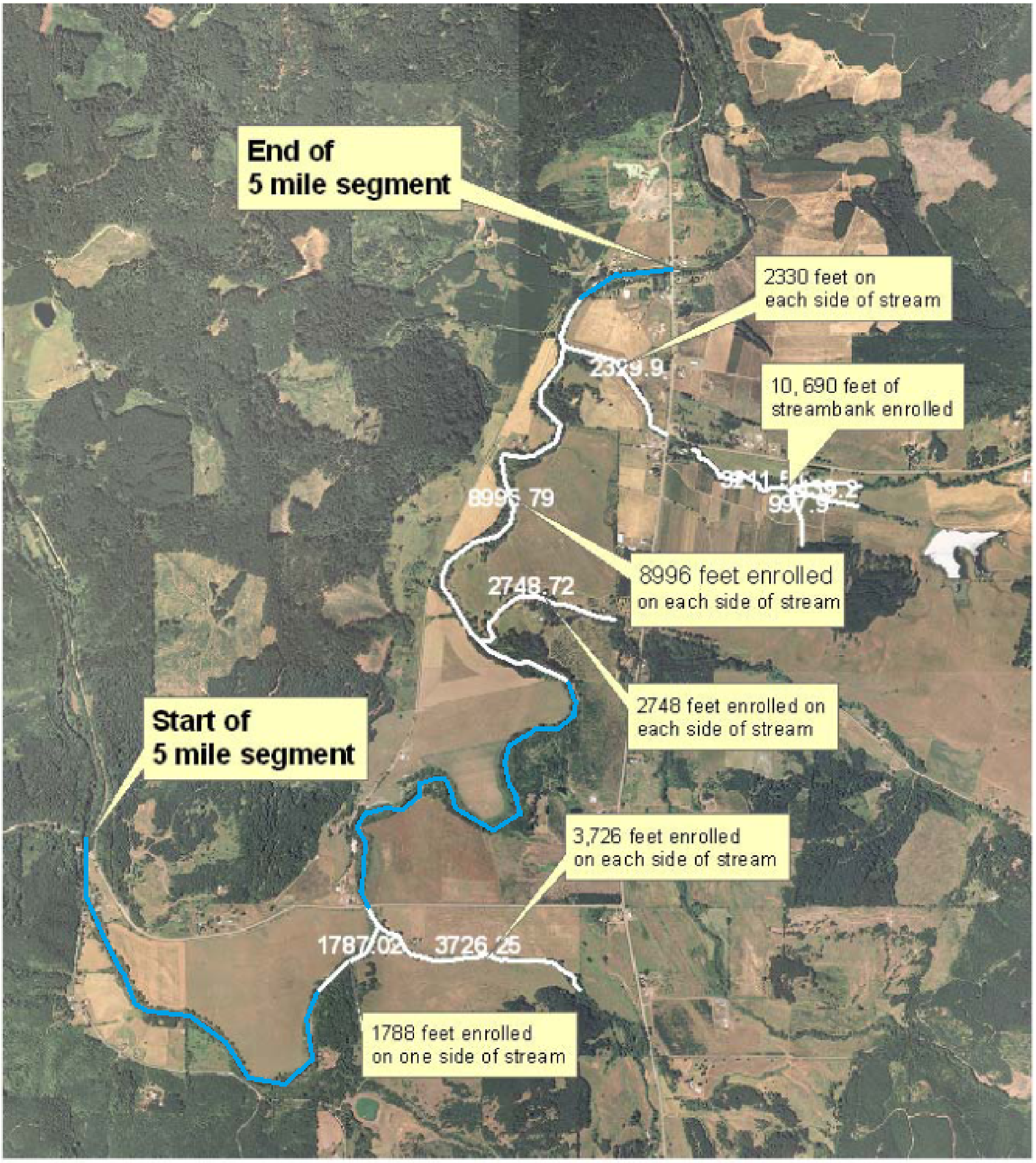
Figure 2.1 illustrates the CIIP in practice. In the highlighted five-mile segment, enrolled buffers total 19,780 feet – 1,788 feet on one bank and 8,996 feet on both banks of a downstream reach – representing 33.7 percent of the segment's 52,800-foot streambank.[5] To trigger the bonus, landowners must enroll the remaining 8,620 feet (1.63 miles) contiguously along that same channel before extending into an upstream or downstream segment.

2.2 Neighborhood interactions and the bonus effect

Although theoretical models and controlled experiments suggest that threshold bonuses should concentrate riparian buffers and knit together habitat corridors, real-world evidence on their operation is limited. The financial incentive targeted to achieving connectivity is hypothesized to encourage participation on adjacent (unenrolled) farmland, thereby accelerating the formation of contiguous riparian habitat at the landscape scale. While enrollment is decided independently by each landowner, participants may encourage their neighbors to join in hopes of receiving the bonus. In addition, agency field staff could target landowners whose neighbors have already enrolled, leveraging existing participation. We test whether

[5] A five-mile segment has five miles (26,400 feet) of streambank per side each side and 10 miles (52,800 feet) of total streambank.

Figure 2.1: The Conservation Initiative Interim Partnership (CIIP) in practice



this process unfolds dynamically with new enrollments influencing neighboring landowner decisions, and whether this reinforces habitat connectivity.

To investigate these dynamics, we use a field-level dataset documenting all riparian buffers established through the Oregon CREP during its first twelve years (1998–2009). This enrollment data is linked to fine-scale stream-network data to identify hydrologic connectivity among adjacent tracts. We merge our data with a large set of observable field characteristics (e.g., soil productivity, slope, cropping history), neighboring land uses, and county-year program payment rates. Exploiting the panel structure of our data, we specify a recursive model in which variation in enrollment timing helps identify endogenous spatial interactions (Brock & Durlauf, 2001). We include categorical indicators for the share of streambank already enrolled by hydrologically connected neighbors to capture potential non-linear "jump" effects as tracts approach or exceed the bonus threshold.

The primary goal of our empirical analysis is to quantify the extent to which Oregon's threshold-based bonus mechanism influences the spatial configuration of conservation effort at the landscape level. We hypothesize that each parcel's participation benefits from existing enrollment on hydrologically connected neighbors by increasing the expected return to riparian retirement relative to continued agricultural use. Because of temporal lags in the enrollment process, this interaction is viewed as a recursive process, so that a field's value in an enrolled use is a function of the prior period's land use states of fields located within a local neighborhood of each field. Moreover, we anticipate that the strength of these spillovers increases as the cumulative enrollment on segments passing through a tract nears or surpasses the threshold. If sufficiently strong, these dynamics may produce a 'crowding in' effect on conservation efforts, accelerating the formation of connected riparian corridors and enhancing the delivery of ecosystem services across the landscape.

Interactions among landowners can either manifest through active collaboration, with landowners jointly enrolling to secure the bonus, or through indirect coordination, whereby individuals act independently toward the same goal. In either scenario, local agency field staff play a central role in facilitating landowner interactions and promoting contiguous streambank restoration. Given the inherent challenges of tracking overall enrollment across an entire watershed, these staff members leverage their understanding of existing enrollment activity to guide landowners in designing and implementing stream restoration practices that increase their chances of receiving the bonus. Furthermore, their intimate knowledge of the local participation network enables them to conduct targeted outreach, particularly to landowners whose neighbors have already enrolled, using the bonus as a compelling incentive.

Two primary identification challenges complicate efforts to estimate the influence of spatial interactions and the threshold bonus on land use decisions. The first difficulty is separating the hypothesized interaction effect from other spatial effects that may generate spatial correlation among neighboring land use decisions (Irwin & Bockstael, 2002).[6] In the context of conservation programs, neighboring landowners may have similar enrollment preferences because they share similar landscape characteristics, which tend to be positively correlated due to spatial clustering. For example, fields with comparable land cover, soil productivity, or streambank access may have similar suitability for riparian buffer practices. Enrollment may also be influenced by exogenous characteristics of neighboring fields, for example, fields adjacent to existing habitat are more likely to be targeted by conservation agencies in an attempt to capture larger ecological benefits (Lawley & Yang, 2015). In the context of riparian buffers, the degree of hydrologic connectivity across stream segments can affect the ecological value of buffers, raising the expected returns to participation regardless of neighbors' enrollment choices. Such spatial effects, if unobserved, can lead to spatial autocorrelation in the error term, causing landowner decisions to appear interconnected even when they are not.

[6] Similar identification issues arise in other domains—for instance, in modeling social interactions Manski (1993, 2000); Brock & Durlauf (2001). For a more detailed treatment within the land use context, see Irwin & Bockstael (2002).

Because the unobserved spatial heterogeneity is contemporaneous with enrollment, using temporally lagged values of neighboring land uses provides a potential means for addressing this problem (Manski, 1993). However, to the extent that unobserved spatial heterogeneity is constant over time, the identification problem will persist (Irwin & Bockstael, 2002). Given the relatively limited duration of our study period (1998–2008), it is likely that at least a portion of the unobserved heterogeneity will be time-invariant, implying that coefficients associated with lagged neighboring variables will still be biased. Given the positive correlation of most spatial data, this unobserved heterogeneity is most likely to induce a positive bias in our estimates. Consequently, the reported effects of neighboring land use should be interpreted as representing an upper bound for the true estimates.

The second identification challenge lies in disentangling the direct influence of spatial coordination incentives from the indirect effects of other endogenous spatial interactions not directly tied to the scheme. Landowners may respond to endogenous spatial spillovers through observational learning, reputation effects, or changing social norms even in the absence of any spatial coordination incentives (Lawley & Yang, 2015).[7] In our context, this includes the influence of neighbors in adjacent watersheds, whose enrollment does not directly affect a landowner's bonus eligibility.

We use the institutional structure of the CIIP to help distinguish bonus-relevant enrollment from broader endogenous spillovers. Because bonus eligibility depends only on enrollment along hydrologically connected five-mile segments, we measure exposure to the incentive using enrolled streambank on those segments. We separately control for nearby CREP activity on non-hydrologically connected fields, which may capture observational learning, social interactions, or broader agency activity but does not directly affect bonus eligibility. This comparison does not fully separate the threshold mechanism from all other

[7] Lynch & Lovell (2003), for instance, present survey evidence that landowners who learn about agricultural land preservation programs from their neighbors and are subsequently more likely to participate.

endogenous spatial spillovers or targeting behavior. It does, however, help clarify whether the strongest responses are concentrated where the bonus is directly relevant rather than simply where enrollment is accumulating more broadly.[8]

Given that multiple five-mile stream segments can traverse a single field, accurately measuring bonus-relevant exposure requires careful measurement. To this end, we incorporate the total streambank distance enrolled within the *specific* five-mile segment exhibiting the highest level of participation in each period. This measure serves as our tract-level proxy for the strongest CIIP-relevant signal a landowner faces as a tract approaches or reaches the bonus threshold. Beyond the specific five-mile segment with the highest participation, we also incorporate the total amount of streambank enrolled across *all* hydrologically connected fields intersecting or adjacent to a given parcel. This aggregate measure accounts for diffuse bonus potential that may shape behavior independently of the most actively coordinated segment.[9]

A final empirical challenge involves defining the relevant spatial extent of the neighborhood. We once again leverage the unique design of the bonus scheme to address this, defining our neighborhoods to include all parcels within a five-mile buffer around the proximity of the focal parcel. While it is likely that the spatial spillovers of neighboring enrollment generally exhibit some form of distance decay, the unique nature of the bonus scheme mitigates this effect. Because bonus eligibility is determined by enrollment along a precisely defined five-mile stream segment, even distant parcels within the buffer may exert meaningful influence if they contribute to coordinated streambank enrollment within that threshold.

[8]Appendix Figure S2 and Table S1 provide additional context on this distinction. They show that individual enrollments often make material progress toward the CIIP threshold, but threshold attainment remains concentrated among routes already near the cutoff.

[9]For instance, the likelihood that a parcel qualifies for the bonus increases with the number of five-mile stream segments it intersects that are near the enrollment threshold. Conversely, parcels with fewer such segments face a reduced chance of qualifying, as they have fewer distinct opportunities to meet the coordination requirement.

2.3 Econometric model

We model a landowner's decision to enroll an unenrolled agricultural tract i in time t in the Oregon CREP. This decision is governed by an unobserved latent variable e^*_{it}, which represents the net utility derived from enrollment over the fixed contract period, relative to the status quo of continued agricultural use. The landowner is assumed to enroll in the time period t' when $e^*_{it'} > 0$. While the landowner's precise utility e^*_{it} is unobservable, we do observe the discrete time period of enrollment. We therefore define a binary indicator, e_{it}, where $e_{it} = 1$ if $e^*_{it'} > 0$ and $e_{it} = 0$ otherwise. Consequently, the probability of tract i enrolling in CREP at time t, conditional on its prior non-enrollment, is expressed as:

$$\Pr(e_{it} = 1 | e_{it-1} = 0) = \Pr(e^*_{it} > 0 | e^*_{it-1}, \leq 0). \tag{2.1}$$

The latent variable e^*_{it} is specified as:

$$e^*_{it} = \alpha' x_{it} + \beta' m_{n(i)t} + \gamma' b_{it} + \lambda_t + \varepsilon_{it}, \tag{2.2}$$

where $\alpha, \beta, \gamma,$ and λ_t are vectors of unknown parameters to be estimated and ε_{it} is a normally distributed error term that is independently and identically distributed but clustered at the parcel level; x_{it} is a vector of observed tract characteristics and neighboring landscape characteristics thought to influence the likelihood of enrollment, alongside spatial fixed effects; $m_{n(i)t}$ is a vector representing cumulative acreages of CREP enrollment neighboring agricultural fields at the beginning of time t within the neighborhood of tract i, denoted $n(i)$; b_{it} is a vector of categorical variables representing the cumulative length of existing streambank enrollment on the most highly enrolled five-mile stream segment intersecting tract i at the beginning of time t; and λ_t is a vector of annual time dummy variables used to capture used to capture broader temporal trends common across all tracts, such as changes in commodity prices or interest rates.

Our key parameters, collected in the vector γ, quantify how existing streambank enrollment on the most heavily enrolled five-mile segment influences a tract's probability of joining CREP. Since neighboring enrollment increases the likelihood a tract may qualify for the bonus, we generally expect these coefficients to be positive, indicating a positive spillover effect of existing conservation efforts on nearby fields. Furthermore, as each additional mile of nearby enrollment boosts the likelihood of receiving the bonus payout—and thus the net benefit of participation—we anticipate that the magnitude of γ will increase as neighboring enrollment rises, with the strongest effects appearing as local participation nears or exceeds the bonus threshold.

To capture these heterogeneous effects, we define the corresponding vector b_{it} as a set of categorical indicators to measure the proximity to the bonus threshold. Specifically, corresponding to whether neighboring enrollment is none, low, moderate, near the CIIP threshold, or above it, with low as the omitted baseline. This framework allows us to trace how incremental steps toward bonus eligibility—from minimal activity to guaranteed payout—influences the likelihood of enrollment.

Given the dynamic nature of the enrollment decision, we estimate the parameters of Eq. (2.2) using a duration model (Irwin & Bockstael, 2002; Cunningham, 2007; Towe et al., 2008; Wrenn & Irwin, 2015; Newburn & Ferris, 2016). Duration models take account of the fact that an action taken in period t implies the action was not taken in any previous period $T < t$, which is the essence of an optimal stopping problem. Given that our data are only available at yearly time steps, we estimate the parameters of Eq. (2.2) using a discrete-time duration method developed by Beck et al. (1998). The authors show that, when data are observed in discrete time intervals, a binary discrete choice model with time fixed effects provides the same fit as a continuous-time model.

We estimate the duration model as a panel probit model with a binary indicator for enrollment status that takes a value of one in the year of enrollment and zero otherwise. Annual time fixed effects (λ_t) are used to control for the baseline (hazard) rate of enrollment, representing the underlying probability of enrollment in each year. To construct our dataset, we stack tract-level observations over time, resulting in an unbalanced panel due to the nature of CREP enrollment. Once parcel is enrolled, it is no longer considered enrollable and, thus, is dropped from the set of enrollable parcels in subsequent years for the model.

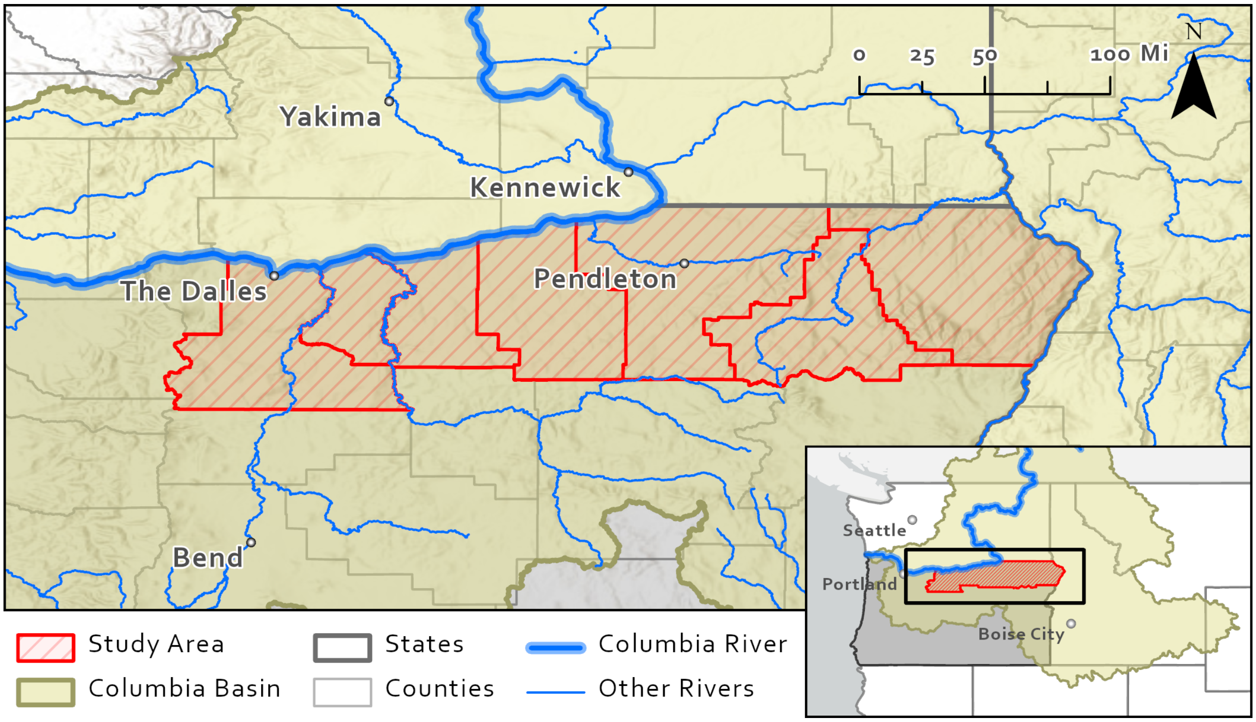
2.4 Data

We construct a spatially explicit, parcel-level dataset to examine riparian streambank enrollment under the Oregon Conservation Reserve Enhancement Program (CREP) during its first 12 years of implementation (1998–2009). Our analysis focuses on six counties within the Columbia River Basin (Figure 2.2), which account for the majority of CREP activity statewide during this period.

Our baseline unit of analysis is the agricultural tract, defined by the USDA's Common Land Unit (CLU) database as a contiguous parcel of farmland under common ownership, segmented by fixed boundaries such as fences, streams, or woodlands.[10] Field-level boundaries are merged with historical CREP contract records using linked identifiers to reconstruct a panel of enrolled fields. For tracts with multiple fields, enrollment is timed to the initial contract start date, reflecting the landowner's earliest participation decision. Given our focus on spatial interactions among neighboring landowners, we aggregate fields to the tract level to track the temporal enrollment status of each agricultural tract throughout the study period.

[10] A tract may contain multiple fields, each delineated separately within the CLU dataset.

Figure 2.2: Study area, major cities, and Columbia River Basin



Our analysis is limited to agricultural parcels that met core eligibility requirements for the Oregon CREP during the study period. Specifically, our sample comprises tracts of farmland or marginal pastureland located alongside agricultural streams that satisfy at least one of three criteria: (i) location within an approved Oregon Agricultural Water Quality Management Plan area, (ii) current or historical presence of threatened or endangered fish species, or (iii) designation as reservation or tribal trust lands. To ensure meaningful conservation potential, we also exclude parcels with less than 0.1 miles of eligible streambank. The final sample includes 1,943 tracts, of which 276 enrolled in CREP during the study period, yielding 16,488 tract-by-year observations. Table 2.1 provides the summary statistics for all model covariates discussed below.

Table 2.1: Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Program variables (all lagged by one year)				
max(CREP stream miles on any 5-mi. stream segment)	0.05	0.46	0.00	9.94
CREP acres on hydrologically connected (HC) farms	45.26	93.63	0.00	1006.32
CREP acres on non-hyd. con. (NC) farms	86.62	145.30	0.00	1200.73
County CRP enrollment (acres)	55,767	41,150	3,507	155,942
County avg. soil rental rate	49.31	6.47	31.62	61.16
Tract variables				
Farm size (acres)	1,233	1,487	9	6,906
Tract streambank (miles)	0.38	0.39	0.100	4.92
Tract number of streams	8.52	9.94	1	108
Tract soil productivity (NCCPI)	0.20	0.09	0.01	0.50
Tract cropland acreage (%)	49.57	39.15	0	100
Tract forest acreage (%)	6.00	17.77	0	100
Suitable salmon/trout habitat (=1)	0.33	0.47	0	1
Neighborhood variables (5-mi tract buffer)				
HC farms	27.49	26.31	1	226
log(Stream miles on HC farms)	3.04	0.99	0.63	5.76
Soil productivity on HC farms (NCCPI)	0.19	0.08	0.04	0.44
Cropland acreage on HC farms (%)	43.41	31.25	0.00	98.70
Forest acreage on HC farms (%)	8.34	15.14	0.00	83.80
NC farms	125.24	84.26	2	528
log(Stream miles on NC farms)	5.19	0.57	2.32	6.52
Soil productivity on NC farms (NCCPI)	0.19	0.07	0.06	0.37
Cropland acreage on NC farms (%)	42.44	27.03	0.00	96.90
Forest acreage on NC farms (%)	8.63	14.11	0.00	81.00
Roads (miles)	10.12	6.13	0.00	41.92
Number of observations		16,451		
Number of eligible tracts		1,940		
Number of tracts enrolled		274		
Study period		1998-2009		

Notes: Summary statistics for all model covariates. Sample includes 1,943 agricultural tracts and 16,488 tract-by-year observations.

2.4.1 Measuring endogenous spatial interactions from surrounding enrollment

Our empirical strategy aims to isolate tract-level spatial interactions that arise specifically from Oregon CREP's bonus incentive mechanism—the Cumulative Impact Incentive Payment (CIIP). Under this scheme, a tract becomes eligible for a bonus when cumulative streambank enrollment along any connected five-mile segment exceeds 50 percent. Accurately attributing enrollment behavior to the bonus scheme therefore requires precisely mapping each tract's bonus-relevant exposure while controlling for other confounding spatial effects that may also influence enrollment decision making.

To accomplish this, we link tract-level CREP participation data with detailed stream network data from the USGS National Hydrology Dataset (NHD). The NHD's streams are subdivided into "reaches," serving as building blocks that allow tracing of water movement for defined distances from any starting point. We utilize this functionality to first identify all five-mile downstream stream segments that intersect each tract. For each tract, we then measure the cumulative amount of streambank enrolled (in miles) in each year on neighboring fields across every linked five-mile segment.[11] Thus, we are able to observe the total enrolled streambank length within every qualifying segment, allowing us to observe how bonus-relevant enrollment accumulates and shifts over time.

We use this segment-level enrollment data to derive two complementary tract-level measures of bonus exposure. First, we calculate the maximum enrolled streambank length, in miles, across every five-mile segment linked to the focal tract at the beginning of each period. This measure reflects the most salient bonus signal a landowner may perceive—the segment

[11] To calculate the total enrolled streambank for a specific stream segment, we start by measuring the amount of streambank enrolled by each individual field. For every field, we pinpoint where it begins and ends along the stream network by creating evenly spaced points (100 meters) around the field's perimeter and then aligning those points with the stream. The distance along the stream between these two aligned points gives us the total enrolled streambank for that particular field within the segment. Finally, we sum up these individual field measurements to get the cumulative enrolled streambank for the entire segment.

offering the highest likelihood of CIIP qualification based on total enrolled length. We treat this maximum value as our primary measure of bonus exposure, under the assumption that landowners are most responsive to the strongest available incentive signal in their stream network. To measure the potential heterogeneous impacts of cumulative enrollment, we categorize this maximum existing streambank enrollment into seven distinct bins: 0 miles, 0.1 to 1.0 miles, 1.1 to 2.0 miles, 2.1 to 3.0 miles, 3.1 to 4.0 miles, 4.1 to 5.0 miles, above 5 miles. We do so by employing a vector of binary variables with the 0.1-1.0 mile category as the omitted baseline. These categories correspond to 0%, 1-10%, 11-20%, 21-30%, 31-40%, 41-50%, and over 50% of the five-mile segment length, respectively. Second, we compute the cumulative enrolled riparian acreage across *all* five-mile segments linked to the tract at the beginning of each period, reflecting the overall density of bonus-eligible enrollment activity in its hydrologic vicinity. This measure accounts for broader exposure to multiple segments that may be partially enrolled, representing a more diffuse but potentially reinforcing incentive environment. Including this term allows the specification to distinguish the strongest threshold-relevant signal on the focal segment from broader hydrologically connected CREP activity that may instead capture local program saturation, targeting intensity, or diffuse bonus potential across nearby segments.

To distinguish CIIP-specific effects from broader, indirect endogenous spillovers, we quantify CREP enrollment on non-hydrologically connected parcels within a five-mile buffer around the parcel boundary. These nearby fields—lacking direct stream connectivity—may nonetheless influence participation through visibility, social norms, or regional outreach but do not factor into bonus eligibility. Finally, we include county-level acreage enrolled in the standard Conservation Reserve Program (CRP) as a proxy for broader conservation efforts by non-CREP agencies. While these landscape-scale activities are not directly tied to the CIIP, they can shape local norms, reputational dynamics, and agency targeting, potentially crowding in or crowding out CREP participation.

2.4.2 Tract-level characteristics

To accurately model CREP enrollment likelihood, we incorporate a comprehensive set of observable, tract-level and neighborhood characteristics hypothesized to influence landowner enrollment decisions. First, we control for farm size using log-transformed tract acreages from the Common Land Unit (CLU) data, as larger tracts may possess more eligible land or their owners might have greater program awareness. We also include the share of cropland/pastureland and forested areas for each tract, derived from USDA National Land Cover Dataset (NLCD) 2001 data. The share in other non-eligible land uses (developed, herbaceous, barren, water) is the omitted category. Tracts with more cropland/pastureland may be more likely to enroll due to a larger eligible area, whereas those with more forested land may be more likely given conservation agency targeting. To account for local infrastructure and urban development pressure, we include the total miles of primary/secondary roads within a five-mile radius.

Tracts with more productive soils are expected to be less likely to enroll due to higher opportunity costs from foregone agricultural uses. Therefore, we calculate the average soil productivity at the tract level using USGS Soil Survey Geographic Database (SSURGO), specifically the National Commodity Crop Productivity Index (NCCPI). The NCCPI, ranging from 0 (lowest) to 1 (highest), measures inherent soil capability for crop production without irrigation, considering soil quality, climate, and landscape features. We specifically use the NCCPI submodel for small grains, common in our study region.

We construct two variables to capture the influence of riparian resources and hydrologic connectivity on landowner enrollment decisions. First, we include the log-transformed total miles of riparian streambank on each tract, derived from our stream network data. We expect tracts with more streambank to be more appealing for enrollment, as they provide greater area for riparian buffers. Second, to account for a tract's hydrologic position, we incorporate the log-transformed total number of stream segments flowing into or out of it. While a higher number of stream segments might indicate greater watershed-level environmental benefits and thus boost enrollment, it could also introduce strategic uncertainty in optimizing buffer placement for bonus maximization, potentially leading to a decrease in enrollment.

We construct a tract-level indicator for suitable habitat for endangered or threatened salmonid species to account for the influence of environmental sensitivity and conservation priorities. This indicator uses spatial data from the Oregon Department of Fish and Wildlife (ODFW) Fish Habitat Distribution (FHD) Database, which identifies areas currently or historically used by species such as bull trout, Chinook salmon, chum salmon, coho salmon, cutthroat trout, sockeye salmon, and steelhead. We anticipate that higher habitat suitability will increase enrollment likelihood, potentially stemming from heightened landowner awareness or increased outreach efforts in these ecologically significant areas.

2.4.3 Exogenous neighborhood characteristics

Beyond a tract's intrinsic characteristics, exogenous characteristics of neighboring land can also influence participation independently of neighbors' enrollment decisions. For instance, landowners might be more inclined to install riparian buffers if adjacent properties feature extensive forested riparian habitat, as they may perceive a greater effectiveness for ecosystem service delivery in an already buffered landscape. Similarly, agency and conservation partner outreach might be more concentrated in areas with abundant riparian habitat, irrespective of formal enrollment.

We include key land-cover, soil-quality, and hydrologic characteristics among neighboring tracts within a five-stream-mile buffer to mitigate the potential influence of exogenous spillovers in our analysis. We do so separately for hydrologically connected parcels from those without direct stream linkage to account for heterogeneous influences based on riparian connectivity. For instance, agency field staff may be more likely to target parcels on five-mile stream segments with relatively little existing forest cover, where there is greater scope for new riparian buffer establishment and a higher likelihood that additional enrollment could contribute to bonus qualification.[12] For each group, we compute the proportion of land in cropland/pasture and forested uses to capture differences in buffer eligibility and habitat context; the average NCCPI soil productivity index as a proxy for local opportunity costs; the total length of eligible streambank to reflect restoration potential on adjacent fields; and the count of neighboring farms to proxy the density of local landowner networks and peer influence.

Lastly, we include local watershed fixed effects using the NHD Watershed Boundary Dataset (WBD) to control for other spatially correlated unobservables that influence enrollment decisions in a given watershed, such as broader regional habitat suitability or prevailing conservation attitudes. We use the finest hydrologic unit codes (HUC) in the WBD, HUC12s, which typically range from 10,000 to 40,000 acres in total area. As previously mentioned, it is possible that time-varying unobservables are spatially correlated with our time-varying measure of neighboring protected status. Given the inherent positive autocorrelation observed in spatial data, our estimates of spatial interactions would most likely be biased upward. Therefore, our estimates should be interpreted as providing the upper bound of any "true" interaction.

[12]We examine this issue descriptively in Appendix Figure S3. Five-mile stream segments with 0 percent forest cover account for about 30 percent of the full tract-linked segment universe but 47 percent of segments that ever exhibit positive CREP-enrolled stream miles, while segments with 0 to 20 percent forest cover make up roughly one-half of all tract-linked five-mile stream segments but more than four-fifths of the segments that ever exhibit positive CREP enrollment. This pattern is consistent with agency targeting of lower-forest areas with greater scope for riparian habitat restoration, although it could also reflect broader agency targeting or landowner selection.

2.5 Results

Full estimation results for the panel probit model of CREP enrollment in Oregon are shown in Table 2.2, with robust standard errors clustered at the tract level. Table 2.3 reports the marginal effects of each covariate on the annual probability of CREP enrollment, with standard errors computed via the delta method.

Unlike linear models, where the marginal effect of an explanatory variable is constant and equal to the estimated regression coefficient, nonlinear models like the probit used in our econometric analysis exhibit varying marginal effects that are not directly represented by the regression coefficients. Hence, we focus on interpreting the marginal effects in the proceeding discussion.

Our primary focus is on the marginal effects demonstrating how neighboring participation, particularly within the bonus-eligible five-mile stream segments, influences the likelihood of subsequent enrollment.

The marginal effects for maximum existing streambank enrollment are positive and statistically significant across all categories, relative to the baseline category for maximum streambank enrollment at 0.1 to 1.0 miles.

In contrast, the estimate for the 0-mile category is negative and statistically significant, indicating that tracts surrounded by entirely unenrolled neighbors are even less likely to participate than those with minimal neighboring enrollment, even after accounting for other tract and neighborhood characteristics.

We also find that the marginal effects for maximum streambank enrollment increase with the cumulative amount enrolled on that segment. Relative to parcels where adjacent enrollment is minimal (0.1–1.0 miles), the probability of enrollment rises by 12.4 percentage points when neighboring landowners have protected 10–20 percent of a five-mile segment and by 22.4 percentage points when neighboring enrollment reaches 41–50 percent. The largest

Table 2.2: Regression coefficients

Variable	Coefficient		Std. Err.
Program variables (all lagged by one year)			
max(CREP stream miles on any 5-mi. stream segment)†			
Streambank enrolled, 0 miles	-1.5872	***	0.1489
Streambank enrolled, 1.0-2.0 miles	0.4734	**	0.2231
Streambank enrolled, 2.0-3.0 miles	0.6380	**	0.3167
Streambank enrolled, 3.0-4.0 miles	0.5704	**	0.2504
Streambank enrolled, 4.0-5.0 miles	0.7941	***	0.2855
Streambank enrolled, 5+ miles	1.6078	***	0.2867
CREP acres on hydrologically connected (HC) farms	-0.0031	***	0.0005
CREP acres on non-hyd. con. (NC) farms	0.0003		0.0003
log(County CRP acres)	-0.3243		0.2666
County avg. soil rental rate	-0.0958	**	0.0431
Tract variables			
log(Farm acreage)	0.0817	**	0.0324
log(Tract stream miles)	0.0013		0.0628
log(Tract number of streams)	-0.0078		0.0675
Tract soil productivity (NCCPI)	-1.2754		0.8827
Tract cropland acreage (%)	0.0426		0.1332
Tract forest acreage (%)	-0.2552		0.2626
Suitable salmon/trout habitat (=1)	0.1628	*	0.0945
Neighborhood variables (5-mi tract buffer)			
HC farms	-0.0019		0.0022
log(Stream miles on HC farms)	0.2499	***	0.0680
Soil productivity on HC farms (NCCPI)	3.5135	*	1.9594
Cropland acreage on HC farms (%)	-0.1119		0.3660
Forest acreage on HC farms (%)	0.4387		0.4746
NC farms	-0.0014		0.0012
log(Stream miles on NC farms)	-0.1784		0.1153
Soil productivity on NC farms (NCCPI)	-3.7972		2.3108
Cropland acreage on NC farms (%)	0.1290		0.4061
Forest acreage on NC farms (%)	0.2145		0.4454
log(Road miles)	-0.0556		0.0662
Constant	7.1423	***	2.2034
R-squared		0.3255	
N(obs)		16,451	
N(tracts)		1,940	
N(enroll)		274	
Period FE		✓	
Watershed FE		✓	

Notes: Standard errors are robust and clustered at tract level. Superscript $\dagger$ indicates a discrete change from the baseline category for streambank enrollment (0.1-1.0 miles of neighboring streambank enrolled). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2.3: Marginal effects

Variable	Coefficient		Std. Err.
Program variables (all lagged by one year)			
max(CREP stream miles on any 5-mi. stream segment)†			
Streambank enrolled, 0 miles	-0.16080	***	0.03180
Streambank enrolled, 1.0-2.0 miles	0.12472	**	0.06061
Streambank enrolled, 2.0-3.0 miles	0.17626	*	0.09530
Streambank enrolled, 3.0-4.0 miles	0.15468	**	0.07170
Streambank enrolled, 4.0-5.0 miles	0.22791	**	0.08989
Streambank enrolled, 5+ miles	0.50517	***	0.08391
CREP acres on hydrologically connected (HC) farms	-0.00009	***	0.00002
CREP acres on non-hyd. con. (NC) farms	0.00001		0.00001
Tract variables			
log(Farm acreage)	0.00235	**	0.00093
log(Tract stream miles)	0.00004		0.00181
log(Tract number of streams)	-0.00023		0.00194
Tract soil productivity (NCCPI)	-0.03672		0.02542
Tract cropland acreage (%)	0.00123		0.00384
Tract forest acreage (%)	-0.00735		0.00756
Suitable salmon/trout habitat (=1)	0.00481	*	0.00289
Neighborhood variables (5-mi tract buffer)			
HC farms	-0.00005		0.00006
log(Stream miles on HC farms)	0.00720	***	0.00197
Soil productivity on HC farms (NCCPI)	0.10116	*	0.05685
Cropland acreage on HC farms (%)	-0.00322		0.01054
Forest acreage on HC farms (%)	0.01263		0.01366
NC farms	-0.00004		0.00004
log(Stream miles on NC farms)	-0.00514		0.00332
Soil productivity on NC farms (NCCPI)	-0.10933		0.06694
Cropland acreage on NC farms (%)	0.00372		0.01170
Forest acreage on NC farms (%)	0.00617		0.01282
log(Road miles)	-0.00160		0.00191
Period FE		✓	
Watershed FE		✓	

Notes: Average marginal effects (AME) computed based on parameter estimates in Table 2.2. Standard errors are computed using the delta method. Superscript † indicates a discrete change from the baseline category for streambank enrollment (0.1-1.0 miles of neighboring streambank enrolled). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 2.3: Connectivity within 5-mile stream segments

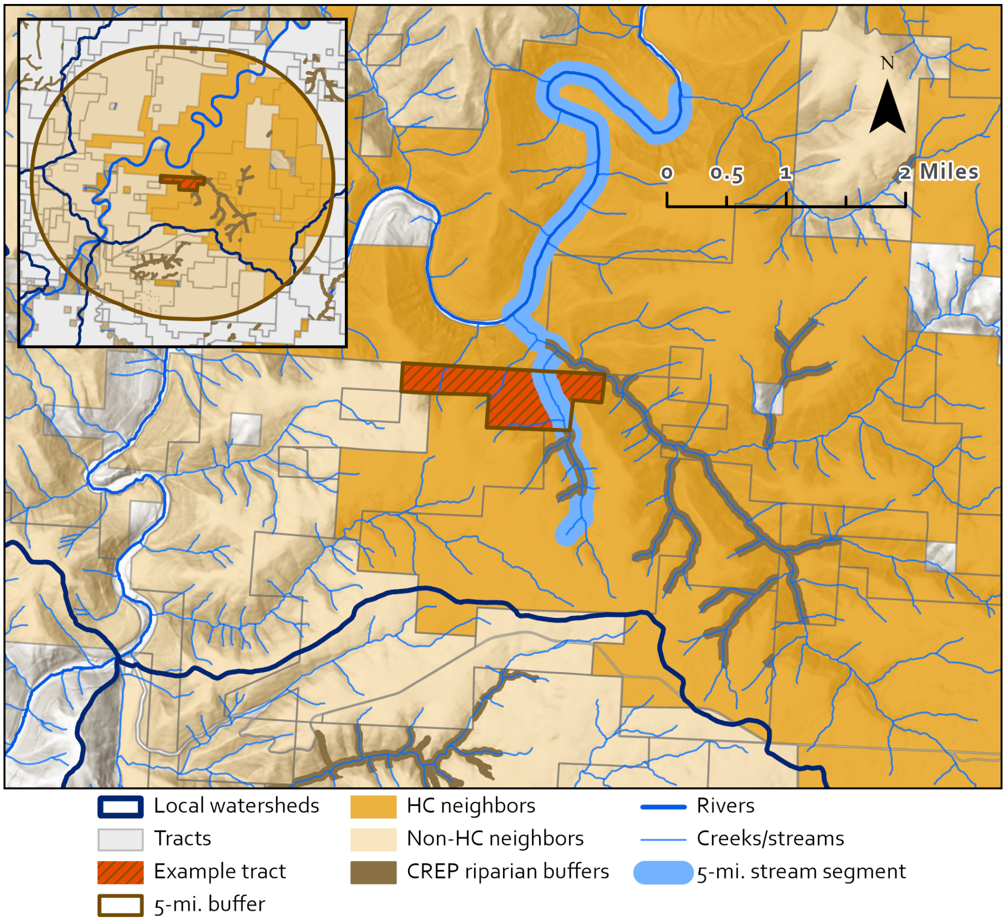
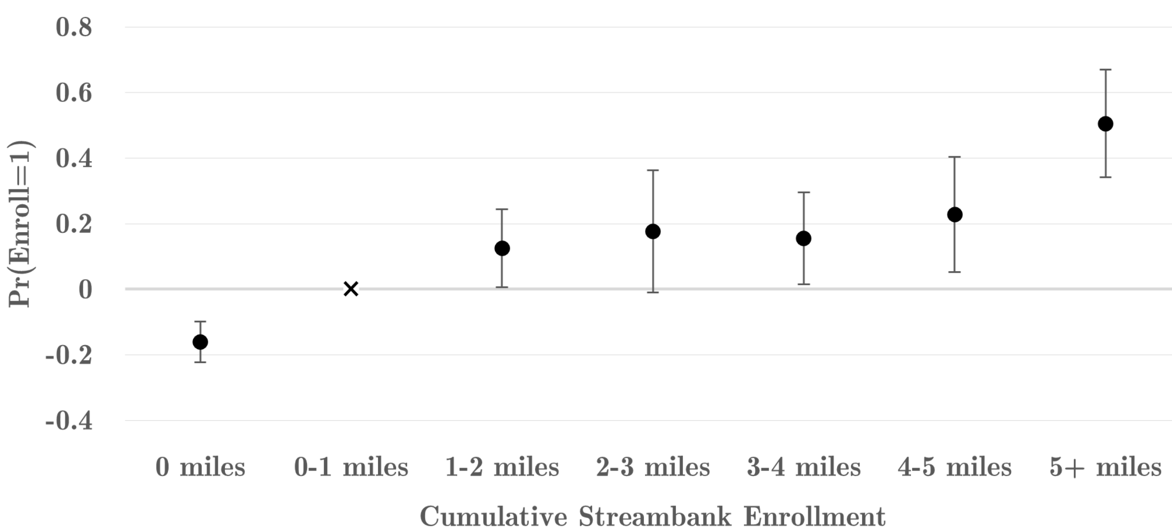


Figure 2.4: Marginal effects of neighboring streambank enrollment



effect appears once cumulative enrollment exceeds the 50 percent CIIP threshold, where the probability of participation increases by 50.5 percentage points. This pattern indicates that enrollment responses strengthen markedly as connected routes approach and exceed the bonus-relevant cutoff.

We also find evidence of a crowding out effect on CREP enrollment at the broader landscape scale. We specifically find the marginal effect on the cumulative amount of CREP acreage across all hydrologically connected farms within a five-mile buffer is negative and statistically significant. However, the estimated coefficient is relatively small compared to our primary variables of interest discussed above. For example, at the sample mean of 45.26 acres of CREP acres on hydrologically connected farms, the likelihood of a tract enrolling decreases by 0.4 percentage points in a given period. A potential explanation is decreased targeting by agency field staff and conservation partners in areas with sufficiently high levels of existing participation, where the marginal environmental benefits are minimal.

Marginal effects for other covariates on the probability of enrollment generally conform to expectations, particularly those that are statistically significant. The marginal effect for the county average dryland rental rates (lagged one year) is negative and statistically significant. This conforms with expectations that landowners in areas with higher agricultural rents face higher opportunity costs and thus are less likely to retire their farmland, on average. Meanwhile, the marginal effect on farm acreage is positive and significant, indicating that larger farms are more likely to enroll in CREP. This may be due to the ability of larger operations to spread fixed costs across more acres, lowering per-acre expenses, or greater familiarity with conservation programs, making participation more likely. The marginal effect on the salmonid habitat suitability indicator is positive and weakly significant, suggesting that tracts more suitable for these species are more likely to be enrolled. This outcome could reflect greater awareness among landowners of the potential benefits, or it could be a result of more focused targeting efforts by agency field staff.

Regarding other neighborhood covariates, the marginal effect of stream miles on hydrologically connected farms (within a five-mile buffer) is positive and statistically significant, suggesting that landowners are more likely to enroll in areas where there is greater stream frontage in the local watershed. This may reflect targeted outreach to tracts with higher riparian connectivity, which offer greater environmental value given their position within the watershed. Similarly, the marginal effect of soil productivity on hydrologically connected farms is positive and statistically significant, potentially indicating a strategic emphasis on establishing riparian habitat adjacent to high-quality soils, where long-term restoration outcomes may be more viable. By contrast, none of the marginal effects for covariates on non-hydrologically connected farms are statistically significant. Taken together, these results provide evidence of exogenous spatial spillovers from hydrologically connected neighbors, but not from those lacking such connectivity. This asymmetry is consistent with the ecological significance of hydrologic linkages in salmonid restoration and suggests that agency outreach and program engagement may be strategically concentrated in areas where upstream and downstream connections enhance the environmental value of restoration efforts.

We perform several robustness checks to assess the stability of our econometric results, summarized here and detailed in the Supplemental Appendix. First, we re-estimate our model using two parsimonious sets of covariates–one including only endogenous variables and another incorporating tract-level characteristics. In both cases, the estimated effect of neighboring streambank enrollment remains positive and statistically significant, indicating a stable relationship between local participation and enrollment likelihood. This consistent finding reinforces our core argument that the bonus payment is associated with positive endogenous spatial spillovers and more contiguous conservation efforts.

Second, we refine our categorical measure of maximum streambank enrollment above the five-mile bonus threshold by applying 1-mile bins consistent with those used for lower enrollment levels (0.1–1.0 miles, 1.1-2.0 miles, etc.). While limited data in the upper categories prevents estimation of some coefficients, the available results closely mirror our core findings. Third, we implement an alternative specification treating maximum streambank enrollment as a continuous variable rather than discretized categories. This allows for greater flexibility in capturing nonlinear effects and avoids the limitations inherent to categorical indicators. The results from this specification are qualitatively consistent, further corroborating the strong, positive influence of neighboring streambank enrollment on tract-level participation and the effectiveness of this spatial coordination incentive in achieving landscape-scale goals.

2.6 Conclusion

Our study provides empirical evidence that helps fill the current gap in the literature on real-world spatial coordination incentives, particularly threshold bonus schemes. We find that the Oregon Conservation Reserve Enhancement Program's (CREP) threshold-based bonus is associated with stronger enrollment responses as connected routes approach and exceed the eligibility cutoff, contributing to the spatial connectivity of conservation activity at the landscape level. This stands in contrast to conventional agricultural PES schemes, which often fail to achieve landscape-scale outcomes because they focus on isolated, farm-level actions rather than desirable spatial patterns.

Our findings show that existing streambank enrollment significantly increases the likelihood of subsequent enrollment on neighboring stream-adjacent land, with the largest responses arising as cumulative enrollment approaches and surpasses the 50 percent threshold for the bonus payment. We therefore interpret the CIIP less as the sole driver of contiguous buildout from the outset and more as a late-stage coordination device that helps convert ongoing accumulation into connected riparian habitat.

These results carry important implications for the design of future conservation programs. They suggest that well-designed spatial coordination incentives, such as the threshold bonus, can be an effective tool to overcome the challenge of fragmented conservation efforts. By explicitly rewarding contiguous conservation, such schemes may successfully guide landowner behavior towards achieving the ecological thresholds necessary for robust landscape-level outcomes. This evidence highlights the value of exploring strategies beyond individual parcel-based incentives that proactively promote coordination and spatial connectivity for enhanced environmental benefits. Further research is warranted to explore the long-term sustainability of these coordinated efforts and their transferability to diverse socio-ecological contexts.

Beyond the specific context of riparian restoration, these findings contribute to a broader understanding of how to operationalize spatial incentives for collective environmental action. Policymakers designing conservation initiatives may consider incorporating similar threshold or agglomeration bonus structures to encourage the formation of ecologically significant networks, particularly in landscapes where contiguous habitat is paramount. However, the nuanced 'crowding out' effect observed at higher enrollment levels suggests the importance of adaptive management and careful monitoring to optimize program efficiency and prevent unintended disincentives as programs mature.

Building on our findings, future research should explore the mechanisms of peer-to-peer influence and agency-landowner interactions while linking spatial enrollment clusters to ecological and economic performance. Mapping enrollment trajectories against riparian health metrics–as vegetation cover, thermal regulation, and salmonid habitat suitability–will quantify the environmental returns of contiguous conservation. Integrating remote sensing, targeted field monitoring, and detailed cost records can then evaluate the marginal cost-effectiveness of threshold bonuses relative to alternative incentive designs. Finally, longitudinal comparisons across diverse watershed contexts will test the durability and transferability of spatial coordination schemes, informing adaptive program designs that maximize both ecological integrity and fiscal efficiency.

Chapter 3

Insecticide use, resistance, and the billion-dollar bug: evidence from farmers' management of US corn fields[1]

3.1 Introduction

Rootworms (*Diabrotica spp.*) are one of the most destructive corn pests in the United States, costing U.S. growers over 1 billion dollars per year (Metcalf, 1983; Olmstead & Rhode, 2008). The persistently large costs associated with rootworm infestations stem from the adaptability and resilience of the pest, which has demonstrated the ability to evolve resistance to synthetic insecticides and acclimate to crop rotations (Ball & Weekman 1962; Gray et al. 2009). Over a decade of evidence suggests that rootworms have also evolved resistance to the organic, insecticidal, crystalline (Cry) proteins produced by genetically engineered rootworm resistant (Bt-CRW) corn. Resistance to Bt increases insecticide use (Wechsler & Smith, 2018), while reducing corn growers' yields and profits (Andow et al., 2016; Hurley & Mitchell, 2020).

Rootworms are not the first pest to evolve resistance to the Cry proteins produced by Bt crops. Bollworms in North Carolina were found to be resistant to the Cry proteins Cry1Ac and Cry2Aa as early as 2000 (Burd et al., 2003), while armyworm populations in Puerto Rico evolved resistance to Cry1F in 2006 (Storer et al., 2010). The first unexpectedly severe damages from rootworms in Bt-CRW corn fields occurred in 2009, six years after Bt-CRW seeds were commercialized. Scientists later determined that rootworms on problem fields

[1] This chapter is co-authored with Seth J. Wechsler and Jonathan R. McFadden, Economic Research Service, U.S. Department of Agriculture, 1400 Independence Ave., SW Mail Stop 1800, Washington, DC 20250-1800, United States.

in Iowa had evolved resistance to Cry3Bb1 and mCry3A (Gassmann et al., 2011, 2014). Localized resistance to Cry3Bb1 was also found in Illinois in 2012 (Schrader et al., 2017) and Minnesota in 2013 (Ludwick et al., 2017).

In 2012, an independent scientific advisory panel for the US Environmental Protection Agency (EPA) did not find enough evidence to conclude that the regulatory definition of confirmed resistance had been met (EPA 2013b). By 2016, however, EPA had developed a new framework for rootworm resistance management and concluded that rootworms had evolved resistance to some single trait Bt products (EPA 2016). A decade later, EPA continues to work to delay the evolution of resistance by requiring that 1) companies supplying Bt corn work with growers to implement Integrated Pest Management programs, 2) companies supplying Bt corn investigate reports of unexpected rootworm damage, and 3) mitigation actions be conducted in a half mile area around sites of confirmed resistance (EPA 2025).

Growers depend on estimates of the severity of infestations and the extent of rootworm resistance to make informed pest management decisions (Mitchell & Onstad, 2014). However, there is a dearth of public data on these topics because it is difficult to collect information about rootworm infestations on an area-wide scale (EPA 2013a).

Our study seeks to answer the following three questions: (1) What does survey data suggest about how Bt-CRW seed use affects corn yields and insecticide use? (2) Has the marginal productivity of Bt-CRW seeds, or the marginal rate of technical substitution between Bt-CRW seeds and soil insecticides, changed over time due to the evolution of rootworm resistance? (3) Is it profitable for farmers to continue using single-trait or pyramided Bt-CRW corn (i.e., corn that produces multiple Cry proteins targeting rootworms) in areas where resistance is severe?

We contribute to the field of empirical work that estimates economic damages from pests and analyzes the effectiveness of pest management strategies. This body of literature includes studies that analyze data from field trials (Dun et al., 2010; Tinsley et al., 2016;

Nolan & Santos, 2012), studies that analyze repeated cross-sections of survey data (Wechsler & Smith, 2018), and studies that use highly aggregated, scenario-based methods to generate country-level damage costs (Wesseler & Fall, 2010).

We build on Wechsler & Smith (2018), who tested whether soil insecticide use was higher on fields where Bt-CRW corn was planted in three or more consecutive years by analyzing field-level data from commercial farms. Here, we explore whether survey based methods can corroborate the presence of resistance at the regional level, particularly in parts of the Corn Belt where field-evolved resistance to Bt corn has been documented extensively (e.g., Gassmann et al. 2011, 2012, 2014; Ludwick et al. 2017; Zukoff et al. 2016; Schrader et al. 2017; Shrestha et al. 2018). Our unique contributions include: 1) demonstrating that changes in farmer behavior can be used to identify pest resistance and quantify its impacts, 2) testing whether single-trait or pyramided seeds are profit-maximizing when resistance is and is not present (at optimal soil insecticide use levels), and 3) providing area-wide estimates of the impacts of rootworm resistance in 2010 and 2016.

While a reduced form approach could be used to analyze growers' insecticide use, the structural approach we employ allows us to recover estimates of control provided by insecticides and Bt-CRW seeds, to quantify the economic damages caused by rootworms, and to estimate the marginal products of Bt-CRW seeds and soil insecticides. Unlike field trials, our structural model allows us to explore how changes in model parameters (related to pest pressure, the evolution of resistance, and prices) affect farmer behavior. We provide regionally representative estimates of damages from rootworms on single trait and pyramided Bt-CRW corn fields. To our knowledge, no prior studies take into consideration the differential efficacies of single and pyramided Bt-CRW seeds within areas of localized resistance while accounting for endogenous farmer behavior in a unified, structural framework.

Our results suggest 1) that rootworm resistance was present in southern Minnesota, northern Iowa, and northwest Illinois in 2010 and 2016, 2) that the marginal product and marginal rate of technical substitution of Bt-CRW seeds have decreased over time in areas where entomologists have identified resistance, and 3) that planting pyramided Bt-CRW corn is profit maximizing in areas with resistance, while planting single trait corn is profit maximizing in areas where resistance is not present. Had resistance not developed, corn rootworm control in southern Minnesota, northern Iowa, and northwest Illinois would have been approximately 18 to 20 percent higher in 2010 and 2016, yields would have been approximately 0.5 to 0.7 bushels per acre higher, insecticide use would have been 81 to 97 percent lower, and variable profits would have been 2.8 to 3.3 dollars per acre higher.

3.2 Structural model

As in Lichtenberg & Zilberman (1986), we specify the production function such that $Y = \Upsilon G(Z, v)$ where Y are observed yields, Υ are pest-free yields (i.e., yields in the absence of pest pressure), $G \in (0, 1)$ is an abatement function (i.e., the percentage of Υ not damaged by pests), Z reflects the intensity of pest pressure, and v is a vector of damage abating inputs.

We assume that there are two corn pests: rootworms (R) and weeds (W). Abatement is assumed to be multiplicatively separable such that $G = G_W G_R$, where G_W is the abatement function for weeds and G_R is the abatement function for rootworms. It is assumed that $G_j = 1 - D_j(Z_j, C_j(v_j))$ where $D_j \in (0, 1)$ is the percentage of corn yields damaged by pest j, Z_j is the size of the pest population j prior to treatment, C_j is the percentage of the pest population j controlled, and $j \in \{W, R\}$. Generally, damages are assumed to be increasing in the size of the pest population ($\frac{\partial D_j}{\partial Z_j} > 0$) and decreasing in the percent of the pest population controlled ($\frac{\partial D_j}{\partial C_j} < 0$). Control is assumed to be increasing in abating input use ($\frac{\partial C_j}{\partial v_j} > 0$).

We specify the functional form for damages and control for rootworms using exponential cumulative distribution functions. After simplifying notation by dropping subscripts and setting $Z_R = R$, the damage function for rootworms is:

$$D = 1 - \exp(-dR(1 - C)), \tag{3.1}$$

where d is a damage parameter and R is the size of the rootworm population prior to treatment. Conceptually, d reflects the amount of lodging or stunted plant growth that a rootworm population can cause ($\frac{\partial D}{\partial d} > 0$). The product of d and R reflects the intensity of rootworm pressure.

The pest control function for rootworms is:

$$C = 1 - \exp(-aI - bBt), \tag{3.2}$$

where a and b are "pest control" parameters, I is soil insecticide use, and Bt is an indicator for Bt-CRW seed use. The parameters a and b reflect the efficacy of the rootworm controlling inputs ($\frac{\partial C}{\partial a} > 0$ and $\frac{\partial C}{\partial b} > 0$).

Given equations 3.1 and 3.2, the abatement function for corn rootworms is:

$$G_R = \exp(-\exp(\ln(dR) - aI - bBt)), \tag{3.3}$$

which is a type one extreme value cumulative distribution function:

$$G_R = \exp\left(-\exp\left(-\left(\frac{x - \mu}{s}\right)\right)\right), \tag{3.4}$$

where $x = aI$, the location parameter is $\mu = \ln(dR) - bBt$, and the scale parameter is $s = 1$.[2]

[2]This specification resembles the Gompertz model, which has been used by plant scientists to model the relationship between crop growth and pests (Evans et al., 2003; Hall et al., 1992).

3.2.1 Profit maximization and seed trait bundling

The farmer's objective is to maximize expected profits per thousand lineal feet of planted corn:

$$\begin{aligned} \max_{I,H\geq 0, Bt, HT\in\{0,1\}} & \mathrm{E}[PY - p_I I - p_H H - p_{Bt}(HT)Bt - p_{HT}(Bt)HT] \\ \text{s.t. } & Y = \Upsilon G_W G_R \exp(\varepsilon) \text{ and} \\ & G_R = \exp(-\exp(\ln(dR) - aI - bBt)) \end{aligned} \tag{3.5}$$

where P is the price of corn, p_I is the price of soil insecticides, p_H is the price of herbicides, $p_{Bt}(HT)$ is the conditional premium paid for seeds with Bt-CRW traits, $p_{HT}(Bt)$ is the conditional premium paid for seeds with HT traits, Bt and HT are indicators of genetically engineered (GE) seed use, and ε reflects uncertainty.[3] We analyze expected profits per 1,000 lineal feet instead of expected profits per acre because 1) manufacturers' recommendations about insecticide application rates are often made in these terms, and 2) row spacing decisions vary among the growers in our sample and affect the number of lineal feet per acre.

3.2.2 Derived demand for soil insecticides

We impose a non-negativity constraint on the pest pressure vector by setting:

$$dR = \exp(\alpha + \boldsymbol{X}^{R\prime}\boldsymbol{\beta} + m), \tag{3.6}$$

where α is a constant, $\boldsymbol{X^R}$ is a vector of variables reflecting farm and field-level conditions, and m is an error term which is observed by farmers but not observed by the econometrician.

[3] The premia paid for genetically engineered seed traits are referred to as conditional because it is assumed that the average price per trait decreases as the number of traits rises. This implies that the price premium paid for Bt-CRW seeds is lower when rootworm and herbicide tolerance traits are bundled.

Given similar assumptions, Wechsler & Smith (2018) demonstrate that the derived demand function for soil insecticides is:

$$I = \max[0, I^*] \text{ s.t. } I^* = \frac{1}{a}\left[\alpha + \boldsymbol{X}^{R\prime}\boldsymbol{\beta} + \ln\left(\frac{aP\,\mathrm{E}[Y]}{p_I}\right) - bBt\right]. \tag{3.7}$$

To test whether the efficacy of Bt-CRW traits change over time or by location, we specify the b parameter in equation 3.7 such that it varies by year, region, and seed type. Specifically, we let:

$$\begin{aligned} b \equiv \boldsymbol{X}^{Bt\prime}\boldsymbol{b} = (b_1 + b_2T10 + b_3T16) + (b_4 + b_5T10 + b_6T16) \times ResRgn \\ +(b_7 + b_8ResRgn) \times Pyr, \end{aligned} \tag{3.8}$$

where b_1 through b_8 are parameters to be estimated; $T10$ and $T16$ are indicator variables equal to 1 if the year is 2010 or 2016 respectively (0 otherwise); $ResRgn$ is an indicator variable equal to 1 in regions (counties) where resistance or unexpected rootworm damages to Bt corn has been documented; and Pyr is an indicator variable equal to 1 if GE corn seeds are "pyramided" to contain multiple Bt-CRW traits (e.g., mCry3A + Cry34/35Ab).[4]

The parameters in the first set of parentheses ($b_1, b_2,$ and b_3) allow the effectiveness of Bt-CRW seeds to vary by year. The average effectiveness of Bt-CRW corn can be influenced by latent factors such as improvements in corn seed genetics, shifts in the use of Bt-CRW products which express different Cry proteins, and changes in environmental conditions.

The parameters in the second set of parentheses ($b_4, b_5,$ and b_6) allow the effectiveness of Bt-CRW seeds to vary by location. These parameters allow us to test whether there is evidence of widespread resistance in 2010 and 2016 in regions where resistance has been documented. Because the first documented case of field-evolved resistance was recorded in

[4]The ARMS Phase II data (Production Practices and Cost Report) do not have sufficiently detailed trait level information to allow us to differentiate between Bt-CRW corn with different Cry proteins. However, we can differentiate between single trait Bt-CRW corn and corn with pyramided traits.

2009, we assume that resistance was not widespread in 2005. If resistance was regionally present but not nationally widespread in 2010, then we would expect b_5 to be negative. Similarly, if resistance was regionally present in 2016, then we would expect b_6 to be negative.

The parameters in the third set of parentheses (b_7 and b_8) allow the effectiveness of Bt-CRW seeds to vary by location and seed type. These parameters allow us to test whether resistance evolved at a different rate on single trait than on pyramided Bt-CRW corn. Pyramided Bt-CRW seeds, which were first registered in 2012 (Gassmann et al., 2012), can 1) help control rootworms that have developed resistance to a single Cry protein, and 2) delay the evolution of resistance.

3.3 Model estimation

We estimate the parameters of our structural model using a two-stage method. In the first stage, we use a reduced form tobit to estimate the parameters of the soil insecticide derived demand function (equation 3.7). In the second stage, we use the recovered parameters to identify the efficacy of damage abating inputs.

3.3.1 First stage: Soil insecticide use

Substituting the non-negativity constraint into the derived demand function for soil insecticides (equation 3.7), we get:

$$I = \max[0, I^*] \text{ s.t. } I^* = \frac{1}{a}\left[\alpha + \boldsymbol{X}^{R\prime}\boldsymbol{\beta} + \ln\left(\frac{aP\,\mathrm{E}[Y]}{p_I}\right) - \boldsymbol{X}^{Bt\prime}\boldsymbol{b}Bt + m\right]. \tag{3.9}$$

We assume that the error term m is normally distributed with mean zero and variance σ^2. The parameter σ reflects the intensity of rootworm pressure that is observed by farmers but not by the econometrician.

We estimate the parameters of equation 3.9 using a tobit model. The tobit model accounts for the fact that soil insecticide use is censored at zero. We include a variety of variables in the pest pressure vector ($\boldsymbol{X}^R$) to control for factors that influence rootworm pressure, such as soil characteristics, climate, and crop rotation.

3.3.2 Identification and exclusion restrictions

To identify the parameters of the structural model, we require exclusion restrictions. Specifically, we need variables that influence the decision to use Bt-CRW seeds or expected yields but do not directly influence the decision to use soil insecticides.

We use the presence of highly erodible soils and highly productive soils as exclusion restrictions. These variables are hypothesized to influence Bt-CRW seed use and expected yields, respectively, but are not expected to directly influence rootworm pressure or the efficacy of soil insecticides.

Soil productivity is intended to capture variation in expected yield potential, while highly erodible land is intended to capture differences in management and seed-choice incentives that can shift adoption decisions without directly changing latent rootworm pressure. Conditional on the included pest-pressure controls, climate measures, and rotation variables, these exclusions are therefore intended to operate through expected revenues and technology choice rather than through the underlying effectiveness of soil insecticide use.

3.3.3 Second stage: Identification of efficacy parameters

In the second stage, we use the parameter estimates from the tobit model to identify the efficacy parameters (a and b). The coefficient on the corn-to-insecticide price ratio in the tobit model identifies the $1/a$ parameter. Specifically, the parameter a is the inverse of the coefficient on the price ratio.

Once a is identified, the efficacy of Bt-CRW seeds (b) can be identified from the coefficient on the Bt-CRW seed use indicator in the tobit model. Specifically, b is the product of a and the coefficient on the Bt-CRW indicator.

3.3.4 Generalized residuals and endogeneity

A potential concern in our empirical model is the endogeneity of Bt-CRW seed use and expected yields. To address this, we include generalized residuals from the first-stage models for Bt-CRW seed use and expected yields in our tobit model. The significance of these generalized residuals provides a test for endogeneity. If the generalized residuals are significant, it suggests that latent variables are correlated with both the endogenous variables and the soil insecticide use decision, and failing to control for them would lead to biased parameter estimates.

3.4 Data

Responses to the 2005, 2010, and 2016 ARMS Corn Production Practice and Cost Report (PPCR) surveys are the primary sources of data used in this analysis. Although the Corn PPCR was administered in 2021, the COVID-19 pandemic affected both the administration of the survey and growers' access to pesticides, the supply of which was constrained by supply chain disruptions.

The sample is restricted to fields in the US Corn Belt (Illinois, Indiana, Iowa, Kansas, Kentucky, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, and Wisconsin). These fields represented approximately 62 million acres of corn in 2005, 64 million in 2010, and 55 million in 2016, which accounted for roughly 76 percent, 73 percent, and 59 percent of U.S. planted corn acres in those years, respectively.

Following the approach taken by St. Clair et al. (2020), we identify regions where farmers are likely to report greater than expected damage by rootworms (Appendix C.0.7). Figure D.1 depicts: 1) problem counties identified by the EPA's Biopesticides and Pollution Prevention Division from 2009 to 2012 (EPA 2013a), and 2) problem counties within 25 crop reporting districts (CRDs) identified by Carrière et al. (2020) using data from 2011 to 2016. We exploit the geographic variation across these regions to define two study areas for our analysis: a "total" resistance region (which includes counties in Colorado, Illinois, Iowa, Kansas, Minnesota, and Nebraska) and a "core" resistance region (which includes counties in northwest Illinois, northern Iowa, and southern Minnesota). The "total" region encompasses all problem counties identified by either Biopesticides and Pollution Prevention Division (2013) or Carrière et al. (2020). The "core" resistance region, which only includes problem counties identified in both datasets, is generally where entomologists believe rootworm resistance is most severe (St. Clair et al., 2020; Sappington & Spencer, 2023).

The ARMS Phase II data contains detailed information about every pesticide product applied. This makes it possible to differentiate the insecticides used to kill rootworms from the insecticides used to kill other insects. Because the effectiveness of a pound (or fluid ounce) varies from product to product, insecticide applications were "quality-adjusted" using the manufacturers' recommended rate, which can be found on insecticide product labels.[5]

A proprietary data source was used to estimate state-level prices for Force 3G (a formulation of the active ingredient tefluthrin) and Aztec 2.1 (which contains the active ingredients tebupirimphos and cyfluthrin). Averages of the state level median prices of these products were used as a proxy for insecticide prices in 2005, 2010, and 2016. A hedonic approach was used to estimate premia for single trait and pyramided Bt-CRW seeds (Appendix C.0.7).

[5]The manufacturer's recommend rate is often listed as a weight (e.g. ounces) or a volume (e.g. fluid ounces) per 1,000 lineal feet. The number of lineal feet per acre were calculated by dividing the number of square feet per acre (43,560 ft^2) by the feet between rows (Phillips & Goldy, 2016). All product labels used in this analysis are available upon request.

The 2016 ARMS Phase II corn survey includes questions about the field-level severity of rootworm infestations. However, response rates for these questions were low, and these questions were not included in the 2005 and 2010 ARMS Phase II corn surveys. Instead, state level averages of responses to the question, "If untreated (either with insecticides or Bt seed), how much yield loss do you think corn rootworms would most likely cause on this field?" were used as a proxy for the expected severity of rootworm infestations.

Expected state-level corn prices were estimated using the methodology discussed in Barr et al. (2011). First, the basis was calculated by subtracting the February cash price from the March futures price contracted for December delivery (CME Group 2024). Next, the basis was subtracted from the February futures price contracted for December delivery. All prices were adjusted to 2016 levels using the Bureau of Labor Statistic's Consumer Price Index.

NCCPI values from the Corn and Soybeans submodel aggregate a variety of climate attributes and soil properties (Albers et al., 2022). County-level estimates of soil pH, which tends to be lower in wetter climates (Jenny, 1994), and soil textures (percentages sand and silt, using clay as the implicit baseline category) were calculated using data from the NRCS Soil Geographic SSURGO database (USDA-NRCS 2015). County-level accumulated growing degree days and the total amount of precipitation (in inches) were calculated up to the time of planting using high-resolution spatial climate data collected by Oregon State University's PRISM Climate Group (PRISM Climate Group, 2019).[6]

Table 3.1 presents select summary statistics.[7]

[6] Previous studies have found that environmental factors such as temperature and precipitation may affect WCR pressure. For instance, Jaksons et al. (2022) find that warmer spring temperatures are associated with earlier peak emergence of WCR beetles in the growing season, when corn is most susceptible to damages from larval root feeding. Their findings also suggest a possible association between winter precipitation and WCR beetle emergence.

[7] Summary statistics for insecticide prices are not reported because these data are proprietary.

Table 3.1: Summary statistics

Description	Units	2005 Mean	2005 SD	2010 Mean	2010 SD	2016 Mean	2016 SD
Soil insecticide use	Percent of label rate	12.7	1.6	6.0	0.7	4.0	0.6
–	Percent of corn fields	14.5	1.6	7.3	0.8	5.0	0.7
Bt-CRW seed use	Percent of corn fields	10.2	1.7	56.2	1.4	59.3	1.2
Pyramided Bt-CRW seed use†	Percent of corn fields	-	-	-	-	31.9	1.1
Lagged Bt-CRW seed use	Percent of corn fields	2.8	1.2	12.5	0.6	17.4	1.0
HT seed use	Percent of corn fields	25.0	2.8	75.4	1.3	84.9	1.0
Stacked seed use‡	Percent of corn fields	3.0	0.9	48.7	1.2	54.1	1.3
Yield goal	Bushels/acre	158	1.6	170	0.7	182	1.1
Farm size	Acres	263	12.6	649	22.8	626	22.3
Expected yield loss if RW are not treated	Bushels/acre	16	0.2	17	0.1	22	0.1
Soil pH	Index [0,14]	6.23	0.02	6.24	0.01	6.22	0.01
Accumulated growing degree days at planting	Growing degree days	118.91	4.00	147.63	4.00	136.98	3.07
Total precipitation prior to planting	Inches	7.75	0.16	6.17	0.09	7.88	0.11
Corn lag	Percent of corn fields	24.2	2.3	25.6	1.2	25.9	1.1
Highly erodible soil	Percent of corn fields	21.1	2.4	12.3	0.6	19.7	1.2
Soil productivity	Index [0,1]	0.51	0.01	0.49	0.00	0.48	0.01
Corn price	Dollars/bushel (2016 dollars)	2.41	0.002	5.76	0.005	3.36	0.003
Expected corn price	Dollars/bushel (2016 dollars)	2.77	0.003	4.28	0.004	3.80	0.004
Observations		1,226		1,381		1,114	

† Pyramided Bt-CRW corn seeds express two or more Cry proteins toxic to corn rootworms.
‡ Stacked Bt-CRW corn seeds have both herbicide tolerance and rootworm tolerance traits.

Over the course of the study period, average yield goals increased by approximately 24 bushels per acre and average farm size more than doubled. Expected corn prices increased by 54 percent from 2005 to 2010 (from 2.77 to 4.28 inflation adjusted dollars per acre), before decreasing 11 percent from 2010 to 2016 (to 3.80 dollars per acre). Realized corn prices were lower than farmers expected in 2005 and 2016, but higher than expected in 2010. The premia paid for Bt-CRW seeds increased gradually between 2005 and 2016 (Table E.1).

Soil insecticides were routinely applied to over 50 percent of corn in the early 1980s (Eichers et al., 1978). By contrast, the percent of corn fields treated with soil insecticides was 14.5 percent in 2005 and 5 percent in 2016. The average application rate (i.e., average insecticide use for growers that applied soil insecticides) was close to the label rate in all years of the study period, approximately 88 percent in 2005, 82 percent in 2010, and 81 percent in 2016. Average soil insecticide use (across all fields in the sample, i.e., including growers that did not use soil insecticides) fell by approximately 69 percent over our study period, from 12.7 percent of the label rate in 2005 to 4 percent in 2016.

The percent of fields planted with Bt-CRW seeds increased from approximately 10 percent in 2005 to almost 60 percent in 2016. The percent of fields planted with HT seeds increased from approximately 25 percent in 2005 to 85 percent in 2016. While only 3 percent of farmers in the sample used GE seeds stacked with both HT and Bt-CRW traits in 2005, over half (54 percent) used stacked seeds in 2016. By 2016, just three years after pyramided Bt-CRW seeds were registered (Carrière et al., 2015), over half of the Bt-CRW corn in our study region expressed multiple Cry proteins targeting rootworms.

3.5 Results

The results from the first stage of our empirical model (Table 3.2) suggest that we have met the rank and order conditions. Specifically, the two variables we excluded from the second stage of the empirical model are significant in the first stage. The presence of highly erodible soils decreases expected yields and Bt-CRW seed use, while the presence of highly productive soils increases expected yields. Additionally, the non-zero linear restriction that we imposed on the corn to insecticide price ratio binds; the parameter estimate for the price ratio is different in the first stage of the model than in the second.[8]

Table 3.2: Stage 1: Reduced-form Tobit equations

	OLS		Bivariate probit			
	Expected yields		Bt-CRW		HT	
Instruments						
Highly-erodible soil	-0.032	**	-0.180	**	0.088	
Soil productivity	0.276	***	-0.008		-0.853	***
ln(Corn-to-insecticide price ratio)†	0.349	***	-0.125		0.269	
Other variables						
2010	-0.103	***	1.444	***	1.147	***
2016	-0.001		1.533	***	1.618	***
ln(Farm size)	0.010	*	0.087	***	0.048	
Expected yield loss	0.003	***	0.010	**	0.002	
Soil pH	0.044	***	0.213	***	0.180	**
Corn lag	0.021	*	0.517	***	0.138	*
Resistance region‡	0.075	***	0.322	***	0.342	***
Growing degree days	0.000	***	0.000		0.001	***
Precipitation	0.006	***	-0.027	**	-0.045	***
Constant	1.402	***	-3.114	***	-1.847	***
Rho			0.332	***		
Observations	3,721		3,721			
Pseudo R2	0.213		0.207			

Note: Standard errors were jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
† The corn-to-insecticide price ratio is a linear, but non-exclusion, restriction implied by the structural model.
‡ The results presented in Table 2 are based on the "core" resistance region defined in the text.

[8] As discussed in Section 3.3, non-zero linear restrictions (such as $B_1 = B_2$ in equation 3.9) contribute to the order condition in the same way that exclusion restrictions do (Fisher, 1976).

Table 3.3 presents parameter estimates for the reduced form Tobit model of soil insecticide use. Models (1) and (2) analyze the "total" resistance region, while Models (3) and (4) analyze the "core" resistance region. Models (1) and (3) include generalized residuals for both Bt-CRW seed use and expected yields. Because the generalized residual for expected yields is not statistically significant in Models (1) or (3), we cannot reject the null hypothesis that expected yields are exogenous to soil insecticide use decisions.[9] Consequently, we do not include the generalized residual for expected yields in Model (2) or Model (4). The generalized residual for Bt-CRW seed use is significant across all specifications tested. This suggests that latent variables (likely related to rootworm pressure) are correlated with both soil insecticide and Bt-CRW seed use decisions. Failing to control for these latent variables would bias the results of the analysis.

The vector of reduced form parameter estimates for Bt-CRW seed use have larger standard errors in Models (1) and (2) than in Models (3) and (4). This indicates that the performance of Bt-CRW seeds varies more substantively throughout counties in the total resistance region than it does in the core resistance region (i.e., southern Minnesota, northern Iowa, and northwestern Illinois). Though the magnitudes of the parameter estimates are similar across specifications, we focus on the results implied by our preferred specification, Model (4), in the exposition that follows.

The reduced form results suggest that Bt-CRW seeds reduce soil insecticide use, but that the magnitude of this effect decreased from 2005 to 2016 in regions where resistance has been documented. This finding is consistent with the hypothesis that rootworm resistance developed in the Midwest during our study period. Generally, we find that there tends to be more soil insecticide use on larger farms, on fields where expected yield losses from rootworms

[9]Robust standard errors are not needed when calculating variable addition tests, i.e., $t-$tests of the significance of the generalized residuals (Wooldridge, 2014).

Table 3.3: Stage 2: Soil insecticide use (Reduced-form Tobit results)

	(1) Total Res. Rgn., All Ctrl. Functions		(2) Total Res. Rgn., Sig. Ctrl. Functions		(3) Core Res. Rgn., All Ctrl. Functions		(4) Core Res. Rgn., Sig. Ctrl. Functions	
Variables that affect the efficacy of Bt-CRW								
Bt-CRW	-1.949		-1.965		-1.825	*	-1.815	*
× 2010	0.500		0.488		0.522		0.517	
× 2016	-0.321		-0.350		-0.470		-0.481	
× Resistance region	-0.393		-0.407		-1.444	**	-1.452	**
× Resistance region × 2010	0.669		0.693		1.393	**	1.397	**
× Resistance region × 2016	0.916		0.970		2.200	***	2.221	***
× Pyramided traits	-0.023		-0.005		0.078		0.083	
× Pyr. traits × Resistance region	-0.230		-0.255		-0.704		-0.711	
Variables that affect pest pressure								
Constant	-8.315	***	-9.274	***	-9.497	***	-9.953	***
2010	-0.588		-0.760	**	-0.714		-0.806	**
2016	-0.734		-0.846	**	-0.842	*	-0.903	**
ln(Farm size)	0.324	***	0.320	***	0.328	***	0.325	***
Expected yield loss	0.071	***	0.069	***	0.079	***	0.078	***
Soil pH	0.160		0.161		0.244		0.240	
Resistance region	0.604	***	0.544	***	0.606	***	0.574	***
Growing degree days	-0.001		-0.001		-0.001		-0.001	
Precipitation	0.066	*	0.059	*	0.054	*	0.051	*
Corn lag	0.615	***	0.616	***	0.642	***	0.640	***
Corn lag × Bt-CRW lag	-0.266		-0.263		-0.306	*	-0.303	*
ln(Expected revenue)	0.795		1.142	***	1.029	*	1.204	***
Generalized residual, Bt-CRW†	0.793	**	0.809	**	0.845	**	0.843	**
Generalized residual, expected yields/lft†	0.432		-		0.216		-	
Sigma	1.581	***	1.583	***	1.594	***	1.594	***
Observations	3,721		3,721		3,721		3,721	
Pseudo R^2	0.152		0.151		0.150		0.150	

Note: Standard errors, except those for the generalized residuals for Bt-CRW, were jackknifed to account for the complexity of the survey design (Dubman, 2000). Standard errors for the generalized residuals are linearized. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

† Standard errors are not robust, because robust standard errors are not necessary for variable addition tests (Wooldridge, 2014).

is higher, and in continuous corn rotations. Soil insecticide use increases as expected revenues rise, because the opportunity cost associated with damages is higher on these fields.

In addition to drawing inference directly from the reduced form results of Table 3.3, we recover parameters of the structural model (Table 3.4) and use these parameters to generate structural predictions (Table 3.5) and marginal effects (Table 3.6 and Table 3.7). We report estimates of the impacts of resistance in the core resistance region in Table 3.8 and explore how further development of resistance would affect growers using the elasticities reported in Table 3.9.

Table 3.4: Structural parameters

Parameter	Expression	Estimate	
Variables that affect the efficacy of damage-abating inputs			
Efficacy of soil insecticides	a	0.830	***
Efficacy of Bt-CRW seeds	b_1	1.507	
× 2010	b_2	-0.429	
× 2016	b_3	0.399	
× Resistance region	b_4	1.206	**
× Resistance region × 2010	b_5	-1.160	**
× Resistance region × 2016	b_6	-1.844	***
× Pyramided traits	b_7	-0.069	
× Pyr. traits × Resistance region	b_8	0.591	
Variables that affect pest pressure			
Constant	β_1	-8.080	***
2010	β_2	-0.669	**
2016	β_3	-0.750	***
ln(farm size)	β_4	0.270	***
Expected yield loss	β_5	0.065	***
Soil pH	β_6	0.200	
Resistance region	β_7	0.476	**
Growing degree days	β_8	< 0.001	
Precipitation	β_9	0.042	*
Corn lag	β_{10}	0.532	***
Corn lag × Bt-CRW lag	β_{11}	-0.252	*
Generalized residual, Bt-CRW	β_{12}	0.700	*

Note: Reported structural parameters are derived from the parameters estimated in model (2). Standard errors are jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.5: Structural predictions

	Resistance Region						Non-resistance Region					
Control Type†	2005		2010		2016		2005		2010		2016	
	Effectiveness of Damage Abating Inputs (Aggregated Structural Parameters)											
Soil Insecticides	0.83	***	0.83	***	0.83	***	0.83	***	0.83	***	0.83	***
Bt-CRW Seeds, Sin.	2.71	***	1.12	*	1.27	**	1.51		1.08	*	1.91	**
Bt-CRW Seeds, Pyr.	-		-		1.79	**	-		-		1.84	**
	Control (% of pest pressure)											
Soil Insecticides	56.4%	***	56.4%	***	56.4%	***	56.4%	***	56.4%	***	56.4%	***
Bt-CRW Seeds, Sin.	93.4%	***	67.5%	***	71.9%	***	77.8%	***	66.0%	***	85.1%	***
Bt-CRW Seeds, Pyr.	-		-		83.3%	***	-		-		84.1%	***
	Damages (% of pest-free yields)											
No Control	3.9%	**	2.2%	*	2.4%	**	2.1%	**	1.4%	*	1.9%	**
Soil Insecticides	1.8%	*	1.0%		1.1%	*	0.9%	*	0.6%		0.8%	*
Bt-CRW Seeds, Sin.	0.3%		0.7%	*	0.7%	*	0.5%		0.5%		0.3%	
Bt-CRW Seeds, Pyr.	-		-		0.4%		-		-		0.3%	
	Damages (bushels/acre)											
No Control	6.87	**	4.20	*	5.06	**	3.30	**	2.40	*	3.47	**
Soil Insecticides	3.05	*	1.85		2.23	*	1.45	*	1.05		1.53	*
Bt-CRW Seeds, Sin.	0.47		1.38	*	1.44	*	0.74		0.82		0.52	
Bt-CRW Seeds, Pyr.	-		-		0.86		-		-		0.56	
	Damages (dollars/acre)											
No Control	16.58	**	24.02	*	16.78	**	8.01	**	13.87	*	11.80	**
Soil Insecticides	7.37	*	10.56		7.38	*	3.53	*	6.08		5.19	*
Bt-CRW Seeds, Sin.	1.14		7.89	*	4.78	*	1.80		4.75		1.78	
Bt-CRW Seeds, Pyr.	-		-		2.84		-		-		1.90	
	Yield Losses Abated (bushels/acre)											
Soil Insecticides	3.82	***	2.35	**	2.84	**	1.84	***	1.35	**	1.95	***
Bt-CRW Seeds, Sin.	6.40	**	2.82		3.62	*	2.56	**	1.58		2.95	**
Bt-CRW Seeds, Pyr.	-		-		4.21	*	-		-		2.91	**
	Yield Losses Abated (dollars/acre)											
Soil Insecticides	9.21	***	13.46	**	9.40	**	4.48	***	7.79	**	6.61	***
Bt-CRW Seeds, Sin.	15.45	**	16.13		12.01	*	6.21	**	9.11		10.02	**
Bt-CRW Seeds, Pyr.	-		-		13.94	*	-		-		9.90	**

Note: Sin. = single-trait Bt-CRW corn, Pyr. = pyramided Bt-CRW corn. Reported structural predictions are derived from the parameters estimated in Model (4). Standard errors are jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.6: Average marginal effects for single-trait Bt-CRW corn

	Resistance Region						Non-resistance Region					
Description	No Bt		With Bt		M.E.		No Bt		With Bt		M.E.	
2005	(206 observations)						(1,020 observations)					
Yield (bushels/acre)	168.65	***	172.52	***	3.87	**	153.68	***	155.62	***	1.94	**
Control (%)	23.14%	***	93.38%	***	70.24%	***	9.03%	***	78.01%	***	68.98%	***
Damage (%)	2.50%	**	0.27%		-2.23%	**	1.68%	**	0.45%		-1.23%	**
Probability of using soil insecticides (%)	34.52%	***	0.67%		-33.85%	***	15.35%	***	1.56%		-13.79%	***
Application rate (% of label rate)	87.32%	***	36.27%	***	-51.05%	***	64.45%	***	40.37%	***	-24.08%	**
Soil insecticide use (% of label rate)	36.24%	***	0.34%		-35.90%	***	12.70%	***	0.93%		-11.77%	**
Revenue (dollars/acre)	402.83	***	412.13	***	9.30	**	372.62	***	377.31	***	4.70	**
Seed cost (dollars/acre)	49.46	***	56.39	***	6.93	***	41.25	***	47.50	***	6.26	***
Soil insecticide cost (dollars/acre)	7.17	***	0.07		-7.11	***	2.65	***	0.19		-2.46	**
Variable profit (dollars/acre)	346.20	***	355.67	***	9.48	*	328.72	***	329.61	***	0.90	
2010	(266 observations)						(1,115 observations)					
Yield (bushels/acre)	187.10	***	188.67	***	1.57	*	163.99	***	165.10	***	1.12	
Control (%)	23.35%	*	69.16%	***	45.81%	***	11.19%		66.68%	***	55.49%	***
Damage (%)	1.48%	**	0.66%	*	-0.83%	*	1.12%	**	0.46%		-0.66%	
Probability of using soil insecticides (%)	35.06%	**	9.74%	***	-25.32%	*	18.25%	**	4.20%	***	-14.04%	
Application rate (% of label rate)	86.14%	***	56.66%	***	-29.48%		66.28%	***	46.41%	***	-19.88%	*
Soil insecticide use (% of label rate)	34.90%	*	6.60%	***	-28.30%		15.67%		2.60%	***	-13.06%	
Revenue (dollars/acre)	1067.89	***	1076.85	***	8.96	*	947.53	***	953.97	***	6.44	
Seed cost (dollars/acre)	77.63	***	87.67	***	10.04	***	66.77	***	75.91	***	9.14	***
Soil insecticide cost (dollars/acre)	7.02	*	1.32	***	-5.70		3.28		0.54	***	-2.74	
Variable profit (dollars/acre)	983.24	***	987.87	***	4.62		877.48	***	877.52	***	0.04	
2016	(211 observations)						(903 observations)					
Yield (bushels/acre)	202.32	***	204.53	***	2.21	**	175.52	***	177.58	***	2.06	***
Control (%)	22.20%	*	72.82%	***	50.62%	***	14.54%	*	85.23%	***	70.69%	***
Damage (%)	1.74%	***	0.66%	*	-1.08%	**	1.42%	**	0.28%		-1.14%	***
Probability of using soil insecticides (%)	33.86%	**	6.92%	***	-26.94%	**	22.88%	**	1.36%	***	-21.52%	**
Application rate (% of label rate)	84.30%	***	53.13%	***	-31.16%	*	72.06%	***	39.23%	***	-32.83%	**
Soil insecticide use (% of label rate)	32.25%	*	4.25%	***	-28.00%		21.05%	*	0.72%	***	-20.33%	*
Revenue (dollars/acre)	668.76	***	676.08	***	7.32	**	592.03	***	598.98	***	6.95	***
Seed cost (dollars/acre)	93.64	***	100.88	***	7.24	***	81.97	***	88.62	***	6.65	***
Soil insecticide cost (dollars/acre)	7.23	*	0.95	***	-6.28		4.64	*	0.16	***	-4.48	*
Variable profit (dollars/acre)	567.88	***	574.25	***	6.37		505.42	***	510.20	***	4.78	

Note: Average marginal effects (M.E.) are derived from the parameters estimated in model (2). Standard errors are jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.7: Average marginal effects for pyramided Bt-CRW corn

	Resistance Region			Non-resistance Region		
Description	No Bt	With Bt	M.E.	No Bt	With Bt	M.E.
<u>2016</u>	(211 observations)			(903 observations)		
Yield (bushels/acre)	202.32 ***	205.06 ***	2.74 **	175.52 ***	177.54 ***	2.02 **
Control (%)	22.20% *	83.49% ***	61.29% ***	14.54% *	84.19% ***	69.65% ***
Damage (%)	1.74% ***	0.40%	-1.33% **	1.42% **	0.30%	-1.12% **
Probability of using soil insecticides (%)	33.86% **	2.67% **	-31.19% **	22.88% **	1.56% **	-21.31% **
Application rate (% of label rate)	84.30% ***	45.35% ***	-38.95% **	72.06% ***	39.95% ***	-32.11% **
Soil insecticide use (% of label rate)	32.25% *	1.40% **	-30.85%	21.05% *	0.84% **	-20.21% *
Revenue (dollars/acre)	668.76 ***	677.82 ***	9.07 **	592.03 ***	598.86 ***	6.83 **
Seed cost (dollars/acre)	93.64 ***	101.71 ***	8.07 ***	81.97 ***	89.38 ***	7.42 ***
Soil insecticide cost (dollars/acre)	7.23 *	0.32 **	-6.92	4.64 *	0.18 **	-4.46 *
Variable profit (dollars/acre)	567.88 ***	575.80 ***	7.91	505.42 ***	509.29 ***	3.87

Note: Average marginal effects (M.E.) are derived from the parameters estimated in model (2). Standard errors are jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.8: Impacts of rootworm resistance in the core region

Variable	Units	Resistance		No Resistance (Counterfactual)		Effect of Resistance	
2010							
Probability of insecticide use	%	6.13	***	1.45	*	4.68	***
Application rate	% of label rate	50.30	***	40.29	***	10.01	**
Average insecticide use	% of label rate	3.93	***	0.76	*	3.16	***
Control	% of pest pressure	67.96	***	81.36	***	-13.41	
Damages	% of pest-free yields	0.50	*	0.25		0.25	
Yield losses abated	bushels/acre	2.05		2.52	*	-0.47	
Variable profits	dollars/acre	937.64	***	940.96	***	-3.31	*
2016							
Probability of insecticide use	%	4.12	***	0.16		3.96	***
Application rate	% of label rate	45.38	***	32.84	***	12.54	***
Average insecticide use	% of label rate	2.50	***	0.06		2.44	***
Control	% of pest pressure	77.49	***	91.58	***	-14.09	
Damages	% of pest-free yields	0.47	*	0.13		0.34	*
Yield losses abated	bushels/acre	2.91	*	3.60	**	-0.69	*
Variable profits	dollars/acre	531.05	***	533.88	***	-2.83	**

Note: Average marginal effects are derived from the parameters estimated in model (2). Standard errors are jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.9: Elasticities

Variable	**Expression**	**Estimate**	
Soil insecticide use	$\frac{\partial E[I]}{E[I]} \Big/ \frac{\partial b}{b}$	-3.35	**
Probability of soil insecticide use	$\frac{\partial Pr[I>0]}{Pr[I>0]} \Big/ \frac{\partial b}{b}$	-2.87	**
Application rate	$\frac{\partial E[I\|I>0]}{E[I\|I>0]} \Big/ \frac{\partial b}{b}$	-0.48	**
Expected yield	$\frac{\partial E[Y]}{E[Y]} \Big/ \frac{\partial b}{b}$	0.01	*
Rootworm control	$\frac{\partial E[C]}{E[C]} \Big/ \frac{\partial b}{b}$	0.01	**

Note: Reported elasticities are derived from the parameters estimated in model (2). Standard errors were jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

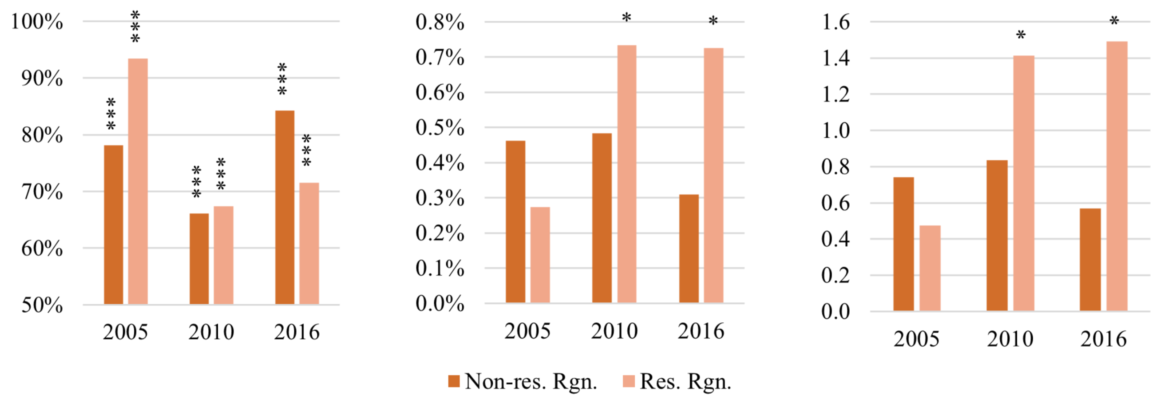
Table 3.4 reports key parameters of the structural model. Nonlinear combinations of these parameters can be used to generate predictions using the structural equations in Section 3.2. For instance, the percent of rootworms killed using one application of soil insecticides (at the recommended label rate) is $C = 1 - \exp(-(.83 \times 1)) \times 100 = 56.4$ percent. Though not all of the structural parameter estimates governing the effectiveness of Bt-CRW seeds (b_1 to b_8) are statistically significant, most linear combinations of them are (Table 3.5).

Table 3.5 reports select predictions of the structural equations (Figure 3.1). These estimates are not marginal effects and do not account for changes in grower behavior. They are similar to the results of field trials in that they directly compare different pest management strategies while holding other variables constant. The first set of rows in Table 3.5 (found in the *Effectiveness of Damage Abating Inputs* section) aggregates interaction terms (b_1 through b_8) across time and region to characterize the effectiveness of rootworm controlling inputs. We find that Bt-CRW corn was more effective than soil insecticides in every year and region analyzed. Growers' choices imply that pyramided corn was more effective than single-trait corn inside the core resistance region in 2016, but slightly less effective than single-trait corn outside it.

The rows in the the *Control* section of Table 3.5 report estimates of the percent of rootworms killed using soil insecticides and Bt-CRW seeds. Inside the resistance region in 2005 (four years before the first field-evolved case of rootworm resistance was reported), we find that planting single-trait Bt-CRW seeds controlled approximately 93.4 percent of the rootworms present. By 2016, the control provided by single trait Bt-CRW corn in the resistance region had fallen to 71.9 percent.

Our results suggest that single trait Bt-CRW corn provided over 13 percent less control than pyramided seeds inside the core resistance region in 2016. The fact that single-trait seeds substantively underperform pyramided seeds inside the resistance region is consistent

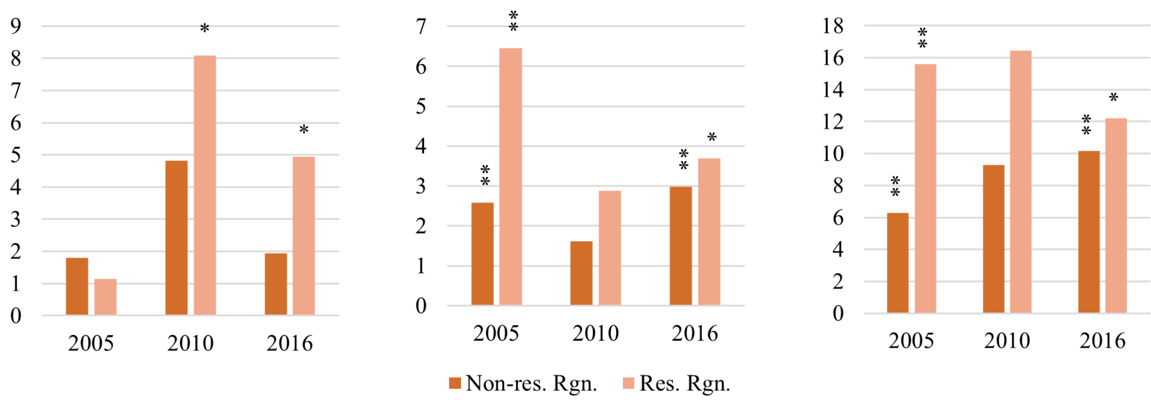
Figure 3.1: Structural predictions for control, damages, and yield-losses abated of single-trait, Bt-CRW corn inside and outside resistance regions in 2005, 2010, and 2016



(a) Control (% of latent CRW pressure)

(b) Damages (% of CRW-free yields)

(c) Damages (bushels per acre)



(d) Damages (dollars/acre)

(e) Yield losses abated (bushels/acre)

(f) Yield losses abated (dollars/acre)

Notes: Average predictions calculated on full sample, using model parameters derived from Model 2 (holding pest pressure constant). Standard errors were jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

with the hypothesis that resistance to Cry proteins evolved during our study period, but that it was localized.

Our estimates suggest that single-trait Bt-CRW seeds provided approximately 1.7 times as much control as soil insecticides inside the resistance region in 2005, and 1.3 times as much control in 2016. Outside the resistance region, Bt-CRW seeds provided 1.4 times as much control as soil insecticides in 2005 and 1.5 times as much control in 2016. These results are largely consistent with field trials, which suggest that Bt-CRW seeds are 1.5 to 3 times more effective than many commonly applied soil insecticides (Eisley & Hammond, 2006; Oleson et al., 2006; Tinsley et al., 2016).

The rows in the *Damages* sections of Table 3.5 quantify losses from rootworms across pest management strategies in terms of yield, value, and percentage of potential yield. Our results suggest that an untreated rootworm infestation inside the resistance region in 2005 reduced potential yields by approximately 4 percent (approximately 6.9 bushels, worth 16.58 dollars per acre). By 2016, these damages had dropped to 2.4 percent (approximately 5.1 bushels per acre, worth 16.78 dollars per acre). Our estimates suggest that total yield losses from rootworms fell throughout our study period—from approximately 189 million bushels in 2005, to 92 million bushels in 2010, to 72 million bushels in 2016. This finding is consistent with the hypothesis that Bt-CRW seed use provided area-wide suppression of pest pressure from 2005 to 2016.

The rows in the *Yield Losses Abated* sections of Table 3.5 indicate that single-trait Bt-CRW corn abated 6.40 bushels per acre (worth $15.45 per acre) in the core region in 2005 and 3.6 bushels per acre (worth $12.01 per acre) in 2016. Inside the core region, pyramided seeds out-yielded single traited Bt-CRW corn by 0.6 bushels per acre in 2016. Outside the core region, however, the yield losses abated by pyramided and single trait seeds were similar.

Notably, our estimates of yield losses and economic damages are lower than some estimates reported in recent studies. For instance, Carrière et al. (2020) found that rootworms caused over 200 dollars per acre on Cry3Bb corn from 2011 to 2013 on "problem" fields. Ye et al. (2025) found that rootworms caused 47.5 bushels per acre of yield losses on trial plots designed to act as trap crops from 2014 to 2016. Our model predicts that an average of 7.37 dollars per acre was lost on single trait corn in the core resistance region in 2010 and that rootworms would have damaged an average of 5.2 bushels per acre in the core region in 2016 if fields were untreated. The relatively low magnitude of our estimates may stem from the fact that they are area-wide averages, not "worst-case scenarios" on "problem" fields. Our averaged effects reflect damages from rootworms on all fields—not just those with the highest rootworm pressure or the most field-evolved resistance.[10]

Table 3.6 and Table 3.7 report average marginal treatment effects for single-trait and pyramided Bt-CRW seeds. We find that planting single-trait Bt-CRW corn decreased insecticide use (as a percent of the label rate) in the resistance region by 35.9 percent in 2005, 28.3 percent in 2010, and 28.0 percent in 2016. This effect was driven by large reductions in the probability of soil insecticide use, though Bt-CRW seed use also decreased application rates.

We find that the marginal product of single-trait Bt-CRW corn inside the core region was 3.9 bushels per acre in 2005, 1.6 per acre in 2010, and 2.2 bushels per acre in 2016. These results are similar to those reported in Nolan & Santos (2012), who found that genetically engineered rootworm resistant corn outperformed conventional varieties by approximately three bushels per acre. The marginal products reported in Table 3.6 are lower than the yield losses abated reported in Table 3.5 because our structural model predicts that adopting Bt-CRW seeds does not always eliminate soil insecticide use.

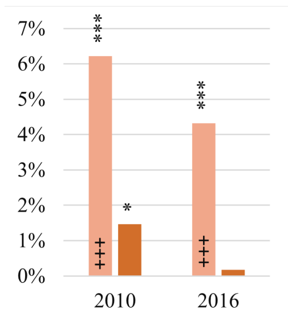
[10] In their supplementary material, Ye et al. (2025) state that trial plots were "designed to act as trap crops, effectively maximizing female rootworm oviposition by planting maize following maize and planting late in the season compared with background maize planting." They characterize the egg deposition and rootworm pressure they analyze as "worst-case" scenarios. The estimates of economic damages reported by Carrière et al. (2020) are high for the problem fields in their study, but they characterize the number of problem fields as "low" (less than 1 percent of Cry3Bb acres) from 2012 to 2016.

The marginal value products of Bt-CRW corn reported in Table 3.6 and Table 3.7 are not strongly statistically significant. However, the point estimates suggest that planting pyramided seeds was profit maximizing in the core resistance region, while planting single-trait Bt-CRW corn was profit maximizing outside it. In the core resistance region, planting pyramided Bt-CRW corn would have increased profits by approximately 1.54 dollars more per acre than planting single trait seeds in 2016. Outside the core region, planting single trait Bt-CRW corn would have out-earned planting pyramided Bt-CRW corn by approximately 0.91 dollars per acre in 2016.

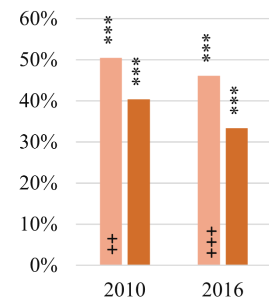
Table 3.8 presents estimates for control, damage, insecticide use, yield, and profit in the core resistance region for scenarios in which we assume that resistance is and is not present (Figure 3.2). We calculate the estimates for the counterfactual scenario (i.e., resistance is not present) by setting the interaction terms between single-trait Bt seed use and being in the resistance region in 2010 (b_5) and 2016 (b_6) equal to zero. Doing so allows us to explore what would have happened if Bt-CRW seeds remained fully effective in the resistance region in 2010 and 2016.

Table 3.9 suggest that the development of resistance would trigger large increases in soil insecticide use but that yields would be relatively unaffected. A 10 percent decrease in the efficacy of Bt-CRW seeds (b) would have increased the average soil insecticide use of Bt-CRW adopters in the resistance region by approximately 34 percent over the course of the study period. Likewise, a 10 percent decrease in efficacy would have increased the probability of soil insecticide use by roughly 29 percent, while increasing application rates by almost 5 percent. By contrast, a 10 percent decrease in the efficacy of Bt-CRW seeds would have decreased the expected yields of Bt-CRW adopters by only 0.1 percent (less than 1 bushel per acre). Yields are inelastic to changes in the efficacy of Bt-CRW seeds because farmers are able to compensate for decreases in the effectiveness of Cry proteins by increasing soil insecticide usage, both inside and outside the resistance region.

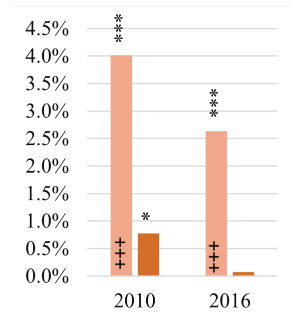
Figure 3.2: Impact of resistance on control, abatement, and yields in states where resistance has been identified



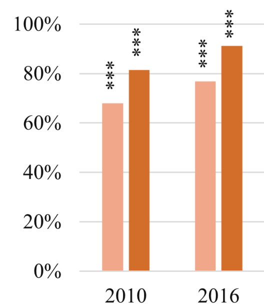
(a) Probability of insecticide use (%)



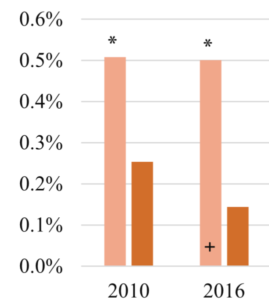
(b) Application rate (%)



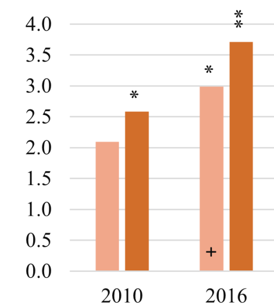
(c) Average insecticide use (%)



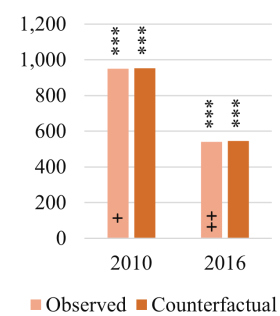
(d) Control (%)



(e) Damages (%)



(f) Yield losses abated (bu/ac)



(g) Variable profits ($/ac)

Notes: Average predictions calculated on the subsample of fields located inside our resistance region using model parameters derived from Model 2. Counterfactual outcomes were generated by setting the estimated interaction terms for single-trait Bt-CRW seed usage inside resistance regions in 2010 ($\hat{b}_{10res}$) and 2016 ($\hat{b}_{16res}$) equal to zero in Model 2. Asterisks ***, **, and * above bars indicate statistical significance at the 1%, 5%, and 10% level, respectively; +++, ++ and + indicate statistically significant within-year differences at the 1%, 5% and 10% level, respectively. Standard errors were jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.6 Conclusion

US farmers first reported unexpectedly severe damage on fields planted with Bt-CRW corn in 2009. By 2011, problem areas had been reported throughout the Midwest. Entomologists subsequently confirmed that resistance had developed in localized rootworm populations. As of 2012, the EPA had not find evidence that resistance was widespread. However, the EPA strengthened its rootworm resistance management requirements in 2016, and there is ongoing concern about the evolution of resistance to Cry proteins.

This article analyzes ARMS Phase II corn data collected in 2005, 2010, and 2016 to corroborate the evolution of rootworm resistance at the regional level and to quantify its area-level effects. We explicitly differentiate between fields where single-trait and pyramided Bt-CRW seeds are planted. This distinction is important because resistance had not been observed on fields where seeds with pyramided traits were planted during our study period.

In inflation adjusted terms (i.e., 2024 dollars), we find that rootworms cost farmers in our study region approximately 973 million dollars in 2005 (in combined yield losses and treatment costs), 2.0 billion dollars in 2010, and 1.2 billion dollars in 2016; 3.3 percent, 2.65 percent, and 2.62 percent of total revenue in those years (respectively). We estimate that rootworm resistance cost U.S. farmers 100 million dollars in 2010 and 75 million dollars in 2016. Rootworm resistance to Cry proteins does not appear to be nationally widespread. However, our findings suggest it was present in Minnesota, Iowa, and Illinois in 2010 and 2016. By 2016, resistance in these states had reduced the control provided by single-trait Bt-CRW seeds by 14 percent, increased average insecticide use by 2.4 percentage points, and reduced inflation adjusted profits by 2.83 dollars per acre. Reductions in the costs associated with resistance from 2010 to 2016 stem largely from decreases in corn prices, not a reduction in the severity of the rootworm resistance problem. Total yield losses from resistance rose from 11.2 million bushels in 2010 to 15.4 million bushels in 2016. Our findings suggest that demand for soil insecticides is elastic to changes in the efficacy of Bt-CRW seeds. This indicates that worsening resistance would induce large increases in soil insecticide use.

Future research will attempt to account for the possibility that latent rootworm pressure is systematically higher on pyramided than on single trait Bt-CRW corn fields. Despite the possibility of a structural break (attributable to the COVID-19 pandemic), it will analyze Corn PPCR data collected in 2021 or Corn PPCR data scheduled to be collected in 2026.

Appendix A: Chapter 1 Supplemental Results

A1 Full estimation results and discussion

In this section, I describe the complete set of econometric results in detail, shown in Table A2 below. The estimated regression coefficients need not have the same sign or statistical significance as the marginal effects in nonlinear models Ai & Norton (2003). Hence, I emphasize the significance of the marginal effects in the subsequent discussion. The estimated marginal effects for covariates used as control variables generally conform to expectations when significant. Likewise, the estimated marginal effects for the first-stage exclusion restrictions are all negative and thus conform to expectations.

The natural log of parcel size is positive and significant on the probability of development and negative and statistically significant on the density of development, suggesting that, all else equal, larger parcels are more likely to be developed, albeit at lower residential densities. Parcel elevation increases the likelihood of development but has no statistically significant effect on development density, while slope has no statistically significant effect on either outcome. Hydric soils reduce both the probability and density of development by limiting the use of on-site septic systems and/or increasing site development costs (e.g., requiring more advanced septic systems). Share of parcel area that is water reduces the density of development but has no statistically significant effect on the likelihood of development. Residing in a 100-year floodplain has a negative and statistically significant impact on the probability of development, suggesting that flood-prone parcels are less likely to be developed. However, residing in a 100-year floodplain has a negative but no statistically significant effect on residential density. High-quality soils increase the likelihood of development, all else equal, presumably due to lower site development costs such soils provide.

Distances to Baltimore, Washington D.C., county seats, and Tier III areas generally decrease the probability and density of development, although most estimates are not statistically significant. This may be partly driven by collinearity between certain distance variables, particularly distances to Baltimore and Washington, D.C. Conversely, distances to highways increase the probability of development, possibly due to traffic and congestion externalities associated with living near highway infrastructure. Distances to Washington, D.C. and the nearest rural village are negative and statistically significant on the density of development, indicating that parcels further away from the U.S. capital and the nearest rural residential area are developed at lower residential densities, all else equal.

Surrounding farmland, forest, and water within one mile generally conform to expectations but, in most cases, are not statistically significant. Meanwhile, the marginal effect on surrounding residential areas is positive and statistically significant in the first stage, indicating that parcels in residential areas are more likely to be converted to residential uses than parcels in less residential areas.

These first-stage exclusions are best interpreted as shifters of the conversion margin rather than as direct determinants of density once development occurs. Consistent with that logic, the existing dwelling, UVA, and easement-eligibility variables enter with the expected negative sign, while the Rural Legacy estimate is more difficult to interpret cleanly.

For my first-stage exclusion restriction variables, the presence of an existing dwelling is negative and statistically significant, suggesting that parcels with existing residential structures are less likely to be subdivided. This result conforms to expectations and prior studies (e.g., Newburn & Ferris 2016) that find similar effects of existing dwellings on the likelihood of development. Similarly, prior enrollment in the state agricultural use value assessment (UVA) program is negative and statistically significant, suggesting that actively farmed parcels are less likely to be converted to residential uses compared to similar parcels not enrolled in the state program. Easement eligibility is also negative and statistically significant, indicating that parcels eligible for enrollment in the state's two largest conservation easement programs are less likely to be developed, all else equal, compared to similar parcels ineligible for enrollment. Lastly, residing in a Rural Legacy Area (RLA) is positive and statistically significant, indicating that parcels in RLAs (designated by local governments) are more likely to be developed than similar parcels in non-RLA areas. This counter-intuitive result may either be the result of positive spatial externalities RLAs provide residential development or due to collinearity with my surrounding land use variables. Additional work is needed to explore these potential explanations.

Table A1: FIML estimation of panel Heckman selection model on the annual probability and density of development

Variable	Pr(dev) Coeff	Pr(dev) Std. Err.	log(density) Coeff	log(density) Std. Err.
DDD policy variables				
M_i	-0.06433	(0.05974)	0.20010**	(0.09118)
Z_i	-0.05751	(0.05338)	-0.06740	(0.06917)
$Z_i M_i$	-0.01001	(0.07832)	0.12321	(0.12129)
τ	-0.53762***	(0.07165)	-0.01368	(0.10159)
$M_i \tau$	0.36660***	(0.07775)	-0.12217	(0.12415)
$Z_i \tau$	0.03334	(0.05663)	0.04995	(0.08527)
$M_i Z_i \tau$	-0.34482***	(0.10830)	-0.22828	(0.16584)
Parcel characteristics and fisrt-stage exclusion restrictions				
log(Acres)	0.16397***	(0.01433)	-0.94157***	(0.01860)
Elevation (feet)	0.00070***	(0.00025)	0.00006	(0.00032)
Average slope (degrees)	0.01479	(0.01717)	0.00239	(0.02242)
Hydric soils (% of parcel area)	-0.00909***	(0.00134)	-0.00463**	(0.00187)
100-year floodplain (% parcel area)	-0.26380**	(0.11156)	-0.00279	(0.15790)
Crop productivity index (NCCPI)	0.16934*	(0.10238)	0.01910	(0.14675)
Distance (in miles) to nearest highway	0.00845**	(0.00361)	0.00248	(0.00470)
... to Baltimore City	-0.00251	(0.00283)	0.00148	(0.00391)
... to Washington, D.C.	-0.00399	(0.00286)	-0.00982**	(0.00441)
... to local county seat	-0.00172	(0.00339)	-0.00366	(0.00541)
... to nearest rural village	-0.02847**	(0.01215)	-0.02492	(0.02028)
Existing dwelling (=1)	-0.25619***	(0.02149)	-	-
Ag. UVA (=1)	-0.28411***	(0.02920)	-	-
Easement eligibility (=1)	-0.17015***	(0.03598)	-	-
Rural Legacy Area (=1)	0.13336***	(0.03483)	-	-
Surrounding land use w/in one mile (% of buffer area around parcel polygon)				
Farmland	0.00317**	(0.00162)	-0.00261	(0.00236)
Forest	-0.00092	(0.00151)	-0.00142	(0.00225)
Water	-0.00087	(0.00339)	-0.00556	(0.00434)
Residential areas	0.00691***	(0.00140)	-0.00424*	(0.00217)
Protected areas	0.00092	(0.00137)	-0.00126	(0.00185)
Constant	-2.13096***	(0.15776)	3.12655***	(0.31043)
ρ		-1.05767***	(0.0874)	
Time FE			Annual	
Local FE			County	
Observations			172,494	
Parent parcels			14,226	
Subdivision events			1,191	

Notes: Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A2: Marginal effects of covariates on the annual probability and density of development

Variable	Pr(dev)		log(density)	
	Coeff	Std. Err.	Coeff	Std. Err.
DDD policy variables				
M_i	0.00019	(0.00063)	0.11667**	(0.04913)
Z_i	-0.00164**	(0.00078)	-0.07597	(0.06000)
$Z_i M_i$	-0.00367***	(0.00109)	-0.12895	(0.08446)
$M_i \tau$	0.00283**	(0.00140)	-0.18522**	(0.09327)
$Z_i \tau$	0.00039	(0.00142)	0.00124	(0.06937)
$M_i Z_i \tau$	-0.00516**	(0.00262)	-0.41106***	(0.15909)
Parcel characteristics and fisrt-stage exclusion restrictions				
log(Acres)	0.00290***	(0.00026)	-0.85492***	(0.02016)
Elevation (feet)	0.00001***	(0.00000)	0.00043	(0.00030)
Average slope (degrees)	0.00026	(0.00030)	0.01020	(0.01931)
Hydric soils (% of parcel area)	-0.00016***	(0.00002)	-0.00943***	(0.00173)
100-year floodplain (% parcel area)	-0.00466**	(0.00201)	-0.14221	(0.13807)
Crop productivity index (NCCPI)	0.00299*	(0.00175)	0.10859	(0.13448)
Distance (in miles) to nearest highway	0.00015**	(0.00006)	0.00694	(0.00435)
... to Baltimore City	-0.00004	(0.00005)	0.00015	(0.00334)
... to Washington, D.C.	-0.00007	(0.00005)	-0.01193***	(0.00388)
... to local county seat	-0.00003	(0.00006)	-0.00456	(0.00481)
... to nearest rural village	-0.00050**	(0.00022)	-0.03996**	(0.01773)
Existing dwelling (=1)	-0.00453***	(0.00040)	-	-
Ag. UVA (=1)	-0.00502***	(0.00054)	-	-
Easement eligibility (=1)	-0.00301***	(0.00062)	-	-
Rural Legacy Area (=1)	0.00236***	(0.00062)	-	-
Surrounding land use w/in one mile (% of buffer area around parcel polygon)				
Farmland	0.00006**	(0.00003)	-0.00094	(0.00218)
Forest	-0.00002	(0.00003)	-0.00190	(0.00205)
Water	-0.00002	(0.00006)	-0.00602	(0.00386)
Residential areas	0.00012***	(0.00003)	-0.00059	(0.00191)
Protected areas	0.00002	(0.00002)	-0.00077	(0.00172)
Time FE			Annual	
Local FE			County	
Observations			172,494	
Parent parcels			14,226	
Subdivision events			1,191	

Notes: Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

A2 Robustness check estimation results

Table A3: Testing for parallel trends in the pre-regulatory period

	Model coefficients		Model marginal effects	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.
First stage dependent variable: probability of development				
M_i	0.0269	0.0906	-0.0017	0.0011
Z_i	0.0215	0.0917	-0.0007	0.0016
$M_i Z_i$	-0.1373	0.1148	-0.0036*	0.0018
τ	-0.3635*	0.0803	-	-
$M_i \tau$	0.0076	0.1186	0.0000	0.002
$Z_i \tau$	-0.0163	0.0806	-0.0004	0.0021
$M_i Z_i \tau$	-0.1039	0.1621	0.0010	0.0039
Second stage dependent variable: ln(density of development)				
M_i	0.3858*	0.1175	-0.0017*	0.0664
Z_i	-0.0985	0.0904	-0.0007	0.0793
$M_i Z_i$	0.0009	0.1514	-0.0036	1.1128
τ	-0.2319*	0.0885	-	-
$M_i \tau$	-0.2236	0.1553	0.0000	0.109
$Z_i \tau$	0.0320	0.0879	-0.0004	0.0766
$M_i Z_i \tau$	-0.0438	0.2045	0.0010	0.2013
ρ			0.549***	
Observations			68,779	
Parent parcels			14,226	
Subdivision events			677	

Notes: First-stage dependent variable is a binary indicator equal to 1 in the time period a given parcel is approved for residential subdivision development and 0 otherwise. Second-stage dependent variable is the logged density of residential development (residential lots/acre) conditional on development in the first stage. Parcel controls in both model stages include parcel size (in acres); elevation (in feet); slope (in degrees); shares of parcel area containing hydric soils or residing in a 100-year floodplain; crop productivity index (NCCPI); distances (in miles) to nearest highway, Baltimore City, Washington, D.C., local county seats, and nearest rural village; and share of surrounding areas (within one-mile radius of parcel polygons) classified as farmland, forest, water, residential, or protected areas. First-stage exclusion restrictions include four binary indicator variables denoting an existing dwelling (=1), enrollment in the state agricultural use value assessment program (=1), easement eligibility (=1), and residing in a designated rural legacy area (=1). Additional controls include annual time fixed effects and county-level fixed effects. Standard errors are robust and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A4: FIML estimation of panel Heckman selection model on the annual probability and density of development using alternate local fixed effects

	Base model (incl. county FE)		Alternate specification (incl. zoning district FE)	
Variable	Pr(dev)	ln(density)	Pr(dev)	ln(density)
M_i	-0.06433	0.2001**	-0.04852	0.21873**
	(0.05999)	(0.08701)	(0.06067)	(0.09050)
Z_i	-0.05751	-0.0674	0.01445	-0.11272
	(0.05210)	(0.06852)	(0.06387)	(0.07840)
$M_i Z_i$	-0.01001	0.12321	-0.09154	0.16738
	(0.07738)	(0.11493)	(0.07740)	(0.11765)
τ	-0.53762***	-0.01368	-0.52715***	0.01195
	(0.07011)	(0.09972)	(0.07044)	(0.09971)
$M_i \tau$	0.3666***	-0.12217	0.35725***	-0.14033
	(0.07584)	(0.12092)	(0.07576)	(0.12185)
$Z_i \tau$	0.03334	0.04995	0.02399	0.03594
	(0.05591)	(0.08248)	(0.05617)	(0.08166)
$\boldsymbol{M_i Z_i \tau}$	**-0.34482*** **	**-0.22828**	**-0.33133*** **	**-0.22504**
	(0.10770)	**(0.16373)**	**(0.10717)**	**(0.16351)**
Constant	-2.13096***	3.12655***	-2.14791***	2.85727***
	(0.15654)	(0.30122)	(0.17674)	(0.31292)
ρ	-1.05767***		-1.06845***	
	(0.07962)		(0.08237)	
Parcel controls	✓		✓	
Exclusion restrictions	✓		✓	
Time FE	Annual		Annual	
Local FE	County		Zoning district	
Observations	172,494			
Parent parcels	14,226			
Subdivision events	1,191			

Notes: First-stage dependent variable is a binary indicator equal to 1 in the time period a given parcel is approved for residential subdivision development and 0 otherwise. Second-stage dependent variable is the logged density of residential development (residential lots/acre) conditional on development in the first stage. Parcel controls in both model stages include parcel size (in acres); elevation (in feet); slope (in degrees); shares of parcel area containing hydric soils or residing in a 100-year floodplain; crop productivity index (NCCPI); distances (in miles) to nearest highway, Baltimore City, Washington, D.C., local county seats, and nearest rural village; and share of surrounding areas (within one-mile radius of parcel polygons) classified as farmland, forest, water, residential, or protected areas. First-stage exclusion restrictions include four binary indicator variables denoting an existing dwelling (=1), enrollment in the state agricultural use value assessment program (=1), easement eligibility (=1), and residing in a designated rural legacy area (=1). Additional controls include annual time fixed effects and county-level fixed effects (base model) or zoning district-level fixed effects (alternate specification). The full set of model results are presented in Supplemental Table A1. Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A5: Marginal effects for DDD estimators of the impact on the likelihood and density of residential development, excluding Charles & Harford counties from my estimation sample

	Base model (all jurisdictions)		Alternate specification (dropping Charles & Harford)	
Variable	Pr(dev)	ln(density)	Pr(dev)	ln(density)
M_i	-0.06433	0.2001**	-0.04693	0.05385
	(0.05999)	(0.08701)	(0.06959)	(0.10377)
Z_i	-0.05751	-0.0674	-0.11734*	-0.06454
	(0.05210)	(0.06852)	(0.06229)	(0.09057)
$M_i Z_i$	-0.01001	0.12321	0.106	0.45955**
	(0.07738)	(0.11493)	(0.11449)	(0.18643)
τ	-0.53762***	-0.01368	-0.56187***	-0.12654
	(0.07011)	(0.09972)	(0.08108)	(0.11270)
$M_i \tau$	0.3666***	-0.12217	0.45098***	-0.0133
	(0.07584)	(0.12092)	(0.08756)	(0.14058)
$Z_i \tau$	0.03334	0.04995	0.06669	0.18045*
	(0.05591)	(0.08248)	(0.06575)	(0.09797)
$\boldsymbol{M_i Z_i \tau}$	**-0.34482*** **	**-0.22828**	**-0.35786** **	**-0.46805* **
	(0.10770)	**(0.16373)**	**(0.15362)**	**(0.25671)**
Constant	-2.13096***	3.12655***	-2.15951***	3.19172***
	(0.15654)	(0.30122)	(0.19558)	(0.40722)
ρ	-1.05767***		-1.12372***	
	(0.07962)		(0.10047)	
Parcel controls	✓		✓	
Exclusion restrictions	✓		✓	
Time FE	Annual		Annual	
Local FE	County		Zoning district	
Observations	172,494		143,490	
Parent parcels	14,226		11,602	
Subdivision events	1,191		756	

Notes: First-stage dependent variable is a binary indicator equal to 1 in the time period a given parcel is approved for residential subdivision development and 0 otherwise. Second-stage dependent variable is the logged density of residential development (residential lots/acre) conditional on development in the first stage. Parcel controls in both model stages include parcel size (in acres); elevation (in feet); slope (in degrees); shares of parcel area containing hydric soils or residing in a 100-year floodplain; crop productivity index (NCCPI); distances (in miles) to nearest highway, Baltimore City, Washington, D.C., local county seats, and nearest rural village; and share of surrounding areas (within one-mile radius of parcel polygons) classified as farmland, forest, water, residential, or protected areas. First-stage exclusion restrictions include four binary indicator variables denoting an existing dwelling (=1), enrollment in the state agricultural use value assessment program (=1), easement eligibility (=1), and residing in a designated rural legacy area (=1). Additional controls include annual time fixed effects and county-level fixed effects (base model) or zoning district-level fixed effects (alternate specification). The full set of model results are presented in Supplemental Table A1. Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A6: Triple-difference (DDD) estimation results using independent model with separately estimated equations for development and residential density

	Base model (joint estimation)		Alternate specification (separate estimation)	
Variable	Pr(dev)	ln(density)	Pr(dev)	ln(density)
M_i	-0.06433	0.2001**	-0.05944	0.18297**
	(0.05999)	(0.08701)	(0.05978)	(0.08373)
Z_i	-0.05751	-0.0674	-0.05277	-0.0807
	(0.05210)	(0.06852)	(0.05207)	(0.06281)
M_iZ_i	-0.01001	0.12321	-0.0177	0.0968
	(0.07738)	(0.11493)	(0.07699)	(0.11302)
τ	-0.53762***	-0.01368	-0.53961***	-0.31503***
	(0.07011)	(0.09972)	(0.07004)	(0.09688)
$M_i\tau$	0.3666***	-0.12217	0.36563***	0.08086
	(0.07584)	(0.12092)	(0.07564)	(0.11588)
$Z_i\tau$	0.03334	0.04995	0.03154	0.06419
	(0.05591)	(0.08248)	(0.05587)	(0.08017)
$\boldsymbol{M_iZ_i\tau}$	**-0.34482*** **	**-0.22828**	**-0.3403*** **	**-0.38434** **
	(0.10770)	**(0.16373)**	**(0.10736)**	**(0.15786)**
Constant	-2.13096***	3.12655***	-2.15158***	1.81927***
	(0.15654)	(0.30122)	(0.15676)	(0.25431)
ρ	-1.05767***		-	
	(0.07962)		-	
Parcel controls	✓		✓	
Exclusion restrictions	✓		✓	
Time FE	Annual		Annual	
Local FE	County		Zoning district	
Observations	172,494		172,494	
Parent parcels	14,226		14,226	
Subdivision events	1,191		1,191	

Notes: First-stage dependent variable is a binary indicator equal to 1 in the time period a given parcel is approved for residential subdivision development and 0 otherwise. Second-stage dependent variable is the logged density of residential development (residential lots/acre) conditional on development in the first stage. Parcel controls in both model stages include parcel size (in acres); elevation (in feet); slope (in degrees); shares of parcel area containing hydric soils or residing in a 100-year floodplain; crop productivity index (NCCPI); distances (in miles) to nearest highway, Baltimore City, Washington, D.C., local county seats, and nearest rural village; and share of surrounding areas (within one-mile radius of parcel polygons) classified as farmland, forest, water, residential, or protected areas. First-stage exclusion restrictions include four binary indicator variables denoting an existing dwelling (=1), enrollment in the state agricultural use value assessment program (=1), easement eligibility (=1), and residing in a designated rural legacy area (=1). Additional controls include annual time fixed effects and county-level fixed effects (base model) or zoning district-level fixed effects (alternate specification). The full set of model results are presented in Supplemental Table A1. Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A7: Triple-difference estimates of the impact on the likelihood and density of residential development, after imposing additional sample restrictions

Variable	Base model	Drop small parcels $\geq$ 5 acres	Drop large parcels $\leq$ 100 acres	Drop small authminors < 3 lots	Drop small authminors < 4 lots
First stage: probability of development					
M_i	-0.06433	-0.12183*	-0.07129	-0.06596	-0.00493
Z_i	-0.05751	-0.12175*	-0.09343*	-0.10807*	-0.06759
Z_iM_i	-0.01001	0.15176*	0.11722	0.02857	0.06447
τ	-0.53762***	-0.52428***	-0.61534***	-0.53674***	-0.52693***
$M_i\tau$	0.36660***	0.37129***	0.35886***	0.38041***	0.35341***
$Z_i\tau$	0.03334	0.03628	0.01817	0.05538	0.10595
$\boldsymbol{M_iZ_i\tau}$	**-0.34482*** **	**-0.32971*** **	**-0.44585*** **	**-0.36782*** **	**-0.40858*** **
Second stage: ln(density of development)					
M_i	0.20010**	0.17772*	0.20838**	0.13418	-0.01202
Z_i	-0.06740	-0.11161	-0.08611	-0.09561	-0.15570
Z_iM_i	0.12321	0.12655	0.11698	0.09644	0.11587
τ	-0.01368	-0.00120	0.09387	-0.05467	-0.08063
$M_i\tau$	-0.12217	-0.15441	-0.09433	-0.09746	-0.07342
$Z_i\tau$	0.04995	0.02387	0.08375	0.08910	0.03615
$\boldsymbol{M_iZ_i\tau}$	**-0.22828**	**-0.18795**	**-0.16049**	**-0.22494**	**-0.11798**
ρ	-1.05767***	-1.06080***	-1.12590***	-1.07155***	-1.19777***
Sample restrictions					
Parcel size (acres)	-	$\geq$ 5	$\leq$ 100	-	-
Min. lot capacity	-	-	-	$\geq$ 3	$\geq$ 4
Observations	172,494	122,351	158,468	96,717	55,908
Parent parcels	14,226	10,257	13,075	8,166	4,739
Subdivision events	1,191	1,077	1,094	930	582

Notes: First-stage dependent variable is a binary indicator equal to 1 in the period a given parcel is approved for residential subdivision development and 0 otherwise. The second-stage dependent variable is the logged density of residential development (residential lots/acre) conditional on development in the first stage. Parcel controls in both model stages include parcel size (in acres); elevation (in feet); slope (in degrees); shares of parcel area containing hydric soils or residing in a 100-year floodplain; crop productivity index (NCCPI); distances (in miles) to the nearest highway, Baltimore City, Washington, D.C., local county seats, and nearest rural village; and share of surrounding areas (within one-mile radius of parcel polygons) classified as farmland, forest, water, residential, or protected areas. First-stage exclusion restrictions include four binary indicator variables denoting an existing dwelling (=1), enrollment in the state agricultural use value assessment program (=1), easement eligibility (=1), and residing in a designated rural legacy area (=1). Additional controls include annual time fixed effects and county-level fixed effects (base model) or zoning district-level fixed effects (alternate specification). The full model results are presented in Supplemental Table A1. Standard errors are bootstrapped (1,000 replications), robust, and clustered at parcel level. *** p< 0.01, ** p< 0.05, * p< 0.1.

Appendix B: Chapter 2 Full Regression Results

B.0.1 CREP Program Details

Figure S1: Summary of incentive payments available through the Oregon CREP

Payments to landowners	**Annual Payments** • Landowners receive an annual per acre payment that includes a base rental rate, a bonus incentive rate, and maintenance rate. • For cropland, the base rental rate is calculated by the cash agricultural value of the three predominant soil types at the site. • For marginal pasture, a seasonal and perennial stream base rental rate has been established for each county. **Cost-Share Payments** • Landowners receive cost-share payments for restoration practices, such as riparian forest buffers and fencing, once the practices are completed. Landowners are reimbursed for 75% of the practice costs (50% provided by USDA, 25% provided by the State) provided they do not exceed average costs for that activity. [1] **One-Time Payments** • Landowners receive a Signing Incentive Payment (SIP) for contracts that include a filter strip or riparian forest buffer. SIP = years in contract x $10 x number of acres. • Landowners receive a Practice Incentive Payment (PIP) once all the practices in the contract are completed, if the contract includes a filter strip or riparian forest buffer. PIP = total eligible practices cost x .40. • If a landowner or multiple landowners enroll over 50% of the streambank in a 5-mile segment of stream into CREP, they receive a Cumulative Impact Payment (CIP). CIP = base rental rate x 4 x number of acres.
Landowner responsibilities	• Landowner pays 25% of the cost to establish the riparian buffer, fencing, etc. • Landowners are responsible to complete and maintain practices.

Source: U.S. Department of Agriculture - Farm Service Agency (USDA-FSA) (2009)

B.0.2 Determining CREP eligibility for individual fields

We determine the eligibility for individual fields based on the CREP agreement signed by the U.S. Department of Agriculture and the state of Oregon on October 17, 1998 (U.S. Department of Agriculture, 1998). Land is considered eligible for enrollment if it is adjacent to a stream segment and is within an area covered by an approved Agricultural Water Quality Management Plan, along a stream where threatened or endangered fish are or were historically present, or on reservation or tribal trust lands (U.S. Department of Agriculture, 1998). The original agreement signed in 1998 only considered endangered salmon (trout and salmon) for eligibility based on fish species. However, an amendment made on June 21, 2000, expanded CREP eligibility to encompass all listed fish species.

We collected geospatial data on fish habitat distributions from the Oregon Department of Fish and Wildlife (ODFW), which we use to identify agricultural lands where endangered fish habitats have been previously documented. We consider tracts satisfying this criterion to be eligible at the beginning of our study period (1998). We also obtain geospatial maps of all agricultural water quality management areas located in our study region from the Oregon Department of Agriculture (ODA) and document the initial year of approval. We determine tract eligibility over time based on the initial year of approval for the management area in which they reside. We do not consider the third criterion, as there are no tribal lands in our study area.

B.0.3 Documenting cumulative streambank enrollment

Documenting cumulative streambank enrollment is empirically challenging for two main reasons. First, program administrators do not define stream segments explicitly, so *any* five-mile stream segment can trigger the bonus. In other words, measurement of a five-mile segment can begin at any reach (the smallest stream section building block) and follow any connected stream path downstream for five miles. Second, our contract data, which includes information on individual fields enrolled in CREP, does not indicate not which streams are enrolled or the total amount of streambank enrolled on each field.

We address these empirical challenges using our high-resolution stream network data and various geospatial techniques. First, we identify all downstream five-mile segments passing through each individual tract. To do this, we use our stream network data to generate five-mile downstream segments from each individual reach, then identify all five-mile segments that intersect each tract. Second, we identify all CREP-enrolled fields intersecting with or adjacent to each five-mile segment in each time period and measure the total stream distance traversed by each enrolled field.[11] Finally, we calculate the cumulative streambank enrolled over time along each five-mile segment by summing the field-level streambank distance enrolled in each time period. Thus, we observe cumulative enrollment levels on all five-mile stream segment passing through each field in each time period.

B.0.4 Parcel contributions and route-level accumulation near the threshold

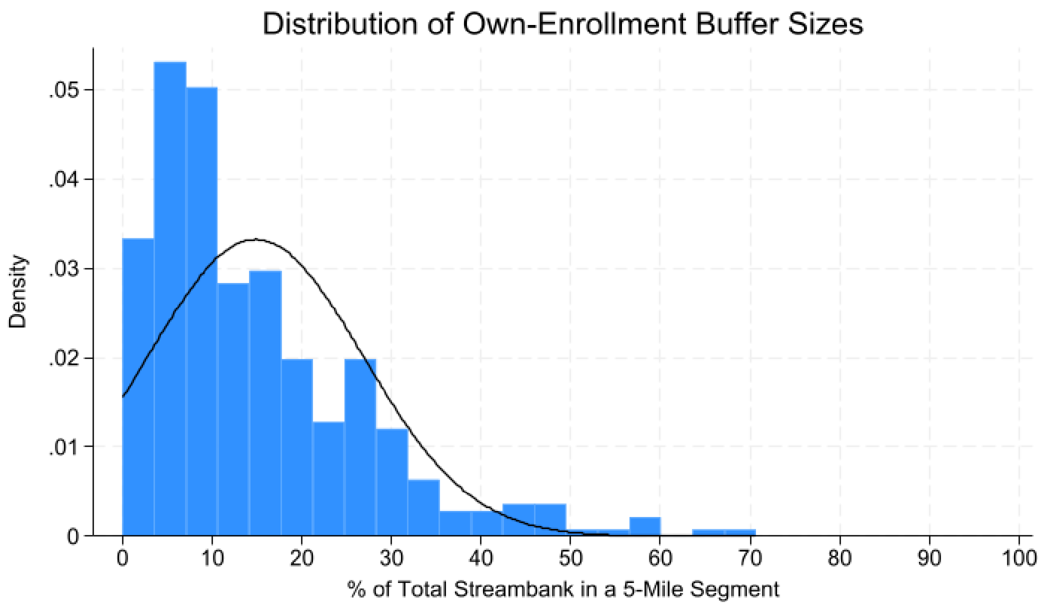
A central interpretive question in the main text is whether stronger responses near the cutoff reflect the CIIP threshold itself or a smoother accumulation of enrollment on routes where participation is already underway. Figure S2 and Table S1 provide additional context on that issue by summarizing the size of parcel-level contributions and the evolution of routes at different pre-existing enrollment levels.

Figure S2 shows that individual parcel contributions are often meaningful relative to the full five-mile segment. The median enrolled parcel contributes about 1.1 stream miles and the 75th percentile about 2.1 stream miles. These magnitudes imply that individual parcels often make material progress toward the 50 percent CIIP criterion, although most are not large enough to move a route across the cutoff on their own. The histogram therefore points to cumulative buildout toward threshold attainment rather than a process driven primarily by large one-parcel enrollments.

Table S1 addresses the next question: whether contributions of that size are sufficient to move routes across the cutoff. The table traces the evolution of the adjacent five-mile route with the highest pre-existing enrolled stream miles at $t-1$ for each enrollment event. Threshold attainment is concentrated among routes already near the cutoff. For routes in

[11] We measure streambank enrollment of each CREP field using a series of geospatial techniques. First, we generate a series of equally spaced points (100 meters) around the perimeter of each CREP field. We then "snap" these points to the five-mile stream segment and identify the starting and ending point. Lastly, we calculate the total distance traversed along the five-mile stream segment by taking the difference of these starting and ending points.

Figure S2: Distribution of enrolled parcel contributions relative to total streambank on a five-mile segment



Notes: Each observation is an enrolled tract-year. The horizontal axis reports the focal parcel's enrolled stream miles as a percentage of the 10 total streambank miles on a five-mile segment.

Table S1: Enrollment-year and short-run route accumulation by pre-existing enrolled stream miles

Pre-enrollment route miles at $t-1$	N	Mean route miles at t	Crosses 5 mi. at t	At/above 5 mi. by $t+1$
0 miles	219	2.17	2.3%	3.7%
0.1–1.0 miles	34	1.47	2.9%	3.0%
1.1–2.0 miles	30	2.49	0.0%	0.0%
2.1–3.0 miles	42	3.65	14.3%	19.0%
3.1–4.0 miles	30	4.01	13.3%	25.0%
4.1–5.0 miles	29	5.65	65.5%	75.9%
5+ miles	18	6.48	0.0%	100.0%

Notes: Each observation is an enrolled tract-year. For each enrollment event, the focal route is defined as the adjacent five-mile route with the highest pre-existing enrolled stream miles in $t-1$. The table reports same-route cumulative enrolled stream miles at $t-1$ and t, along with the share of focal routes that cross the 5-mile threshold in the enrollment year and the share at or above 5 miles by $t+1$.

the 4.1–5.0 mile bin at $t-1$, mean same-route enrollment rises to 5.65 miles in the enrollment year, 65.5 percent cross five miles in t, and 75.9 percent are at or above five miles by $t+1$. The corresponding by-$t+1$ shares are 19.0 percent for routes in the 2.1–3.0 mile bin and 25.0 percent for routes in the 3.1–4.0 mile bin.

Taken together, these patterns indicate that individual enrollments often make meaningful progress toward the threshold, but threshold attainment remains concentrated among routes already near the cutoff. This supports the interpretation that the CIIP operates as a late-stage coordination incentive within a broader cumulative enrollment process.

B.0.5 Existing forest cover on tract-linked five-mile stream segments

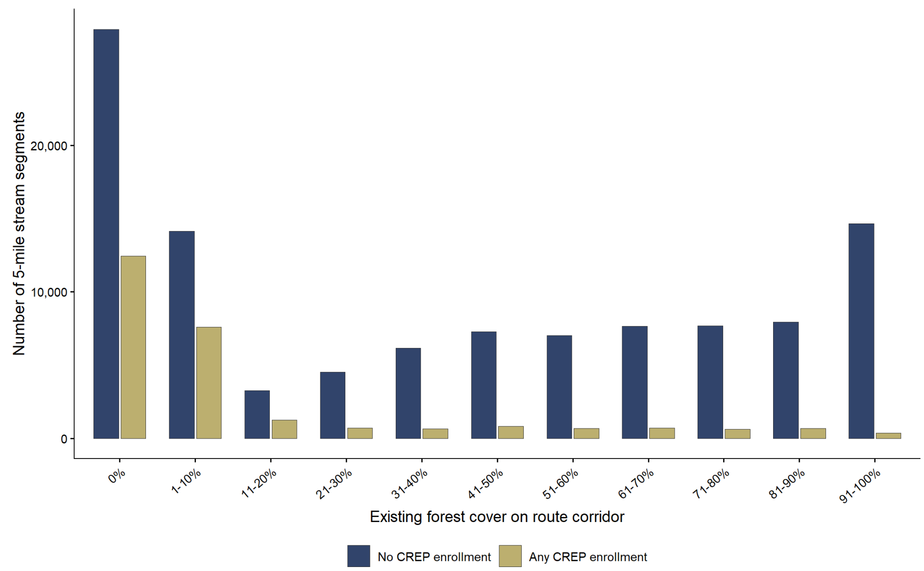
A remaining concern is that pre-existing forest cover may reduce the amount of streambank on a five-mile stream segment that could plausibly be enrolled, thereby changing the feasible denominator relevant for the CIIP threshold.

Table S2: Existing forest cover on tract-linked five-mile stream segments by observed CREP activity

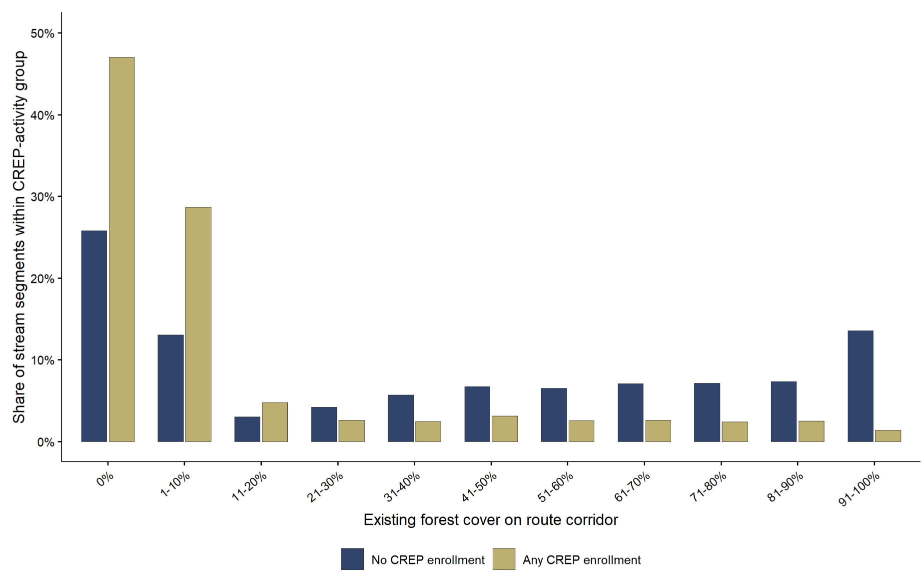
Existing forest cover	No CREP N	No CREP share	CREP activity N	CREP activity share
0%	27,902	25.8%	12,442	47.0%
1-10%	14,133	13.1%	7,583	28.7%
11-20%	3,267	3.0%	1,256	4.7%
21-30%	4,529	4.2%	694	2.6%
31-40%	6,151	5.7%	648	2.4%
41-50%	7,274	6.7%	827	3.1%
51-60%	7,031	6.5%	671	2.5%
61-70%	7,646	7.1%	691	2.6%
71-80%	7,691	7.1%	630	2.4%
81-90%	7,947	7.3%	667	2.5%
91-100%	14,652	13.5%	357	1.3%

Notes: Each observation is a distinct five-mile downstream stream segment linked to at least one sample tract. Existing forest cover is measured within a 25-meter corridor around each reconstructed stream segment using NLCD 2001. The table reports both counts and within-group shares for segments with and without positive observed CREP-enrolled stream miles in any period.

Figure S3: Existing forest cover across tract-linked five-mile stream segments by observed CREP activity



(a) Counts by forest-cover bin



(b) Within-group shares by forest-cover bin

Notes: The figure plots the unconditional distribution of existing forest cover across distinct five-mile downstream stream segments linked to sample tracts, separately for segments with and without positive observed CREP-enrolled stream miles in any period. Existing forest cover is measured on a 25-meter corridor around each reconstructed stream segment using NLCD 2001. Panel (a) reports the number of five-mile stream segments in each forest-cover bin. Panel (b) reports within-group shares, so bars sum to 100 percent within each CREP-activity group.

Figure S3 and Table S2 provide a descriptive check on how existing forest cover varies across tract-linked five-mile stream segments and how that distribution differs between segments with and without observed CREP activity. To construct the figure, we begin with the universe of distinct five-mile downstream stream segments linked to sample tracts through our GIS workflow, reconstruct each segment geometry from the NHD flowline network, buffer each segment using a 25-meter corridor, and then overlay the corridor on NLCD 2001 land cover to measure existing forest cover. We then classify segments according to whether they ever exhibit positive CREP-enrolled stream miles in the panel. Panel (a) reports the number of segments in each forest-cover bin, while panel (b) rescales those counts to within-group shares so that each CREP-activity category sums to 100 percent.

Figure S3 and Table S2 indicate that observed CREP activity is concentrated on five-mile stream segments with relatively little existing forest cover. The table shows that segments with zero forest cover account for about 30 percent of the full tract-linked segment universe, but 47 percent of segments that ever exhibit positive CREP enrollment. Segments with 1 to 10 percent forest cover account for another 16 percent of the overall segment universe, but 29 percent of CREP-active segments. Taken together, segments with 0 to 20 percent forest cover make up roughly one-half of all tract-linked five-mile stream segments, but more than four-fifths of the segments that ever experience positive CREP enrollment. By contrast, only about 11 percent of CREP-active segments are at least 50 percent forested, compared with 36 percent in the full segment universe and 42 percent among segments with no observed CREP activity.

We interpret this pattern as consistent with CREP enrollment being concentrated on five-mile stream segments where additional riparian buffer establishment was more feasible and more closely aligned with the program's restoration objectives. In that sense, the figure helps address the concern that a large share of observed CREP activity may have occurred on stream segments that were already too forested to plausibly reach the bonus threshold through new enrollment. At the same time, the figure is descriptive rather than causal. The contrast could reflect agency targeting, landowner selection, or simply the fact that less-forested stream segments offered more room for new buffer establishment.

B.0.6 Robustness checks

Using alternate sets of covariates

Our first set of checks involves re-estimating our discrete model using alternative sets of covariates. We begin with a parsimonious model including only our main variables of interest–the categorical measures of neighboring streambank enrollment–along with year fixed effects to account for the baseline hazard of enrollment in each period and watershed-level fixed effects to account for potential unobservables at the landscape level. We then progressively introduce additional covariates capturing various tract-level characteristics and neighborhood attributes.

The corresponding regression results and corresponding marginal effects for these alternative specifications are presented in Table S3 and Table S4. Notably, while the magnitude of some effects was somewhat reduced, the results using these alternate specifications remained qualitatively consistent with those from our main model, particularly regarding the positive and significant effects of our categorical variables measuring neighboring streambank enrollment. For instance, the point estimate on the categorical variable for neighboring streambank enrollment exceeding 5.0 miles indicated a slightly lower impact on the likelihood of enrollment, dropping from 50.5 percent in our main model to approximately 33 to 34 percent in these alternate specifications. This still represents a substantial increase in the likelihood of enrollment above the bonus threshold, further supporting the influence of neighboring participation on the adoption of riparian buffers.

Using alternate categorical bins for cumulative streambank enrollment

In our next robustness check, we examine the sensitivity of our econometric results to our chosen categorization of cumulative streambank enrollment. Recall that in our main specification, enrollment above 5.0 miles (the threshold for the network bonus) was grouped into a single category. To ensure that this aggregation did not mask important variations in the effect at higher levels of neighboring enrollment, we re-estimated our model using an alternative set of categorical variables. In this specification, we disaggregated enrollment above 5.0 miles into finer 1-mile increments (e.g., 5.1-6.0 miles, 6.1-7.0 miles, and so on), while retaining the 0.1-1.0 mile category as the omitted baseline.It's important to note that the frequency of observed enrollment decreases at these higher levels, which unfortunately prevented the derivation of precise point estimates for certain categories, specifically the 7.0-8.0 mile and 9.0-10.0 mile ranges. Nevertheless, this alternative categorization provides valuable additional detail regarding the impact of very high levels of neighboring CREP enrollment on a landowner's decision to enroll their land for riparian buffer establishment.

The detailed regression results and corresponding marginal effects for this alternate alternate specification are presented in Table S5 and Table S6. Consistent with our primary findings, these results reveal a discrete jump in enrollment likelihood above the 50% threshold. Specifically, the likelihood of enrollment increases from 14.4 percent when neighboring enrollment is between 4.0 and 5.0 miles to 33.7 percent when enrollment is between 5.0 and 6.0 miles. The strength of this effect further increases to 36.8 percent between 6.0 and 7.0 miles of enrollment and reaches 73.5 percent between 8.0 and 9.0 miles of enrollment. These results from our alternative categorization reinforce the central finding that neighboring CREP enrollment, particularly around and above the network bonus threshold, significantly and positively influences a landowner's decision to enroll their land for riparian buffer establishment.

Treating cumulative streambank enrollment as a continuous variable

Building off the robustness check above using additional 10 categorical bins (and one omitted category), we further re-estimated our model with existing streambank enrollment treated as a continuous variable, rather than a set of discrete categorical variables. This alternative specification also avoids potential information loss and biases associated with the arbitrary selection of category boundaries inherent in the discrete approach. Furthermore, this more flexible specification permits the estimation of marginal effects for higher levels of neighboring enrollment, where observed enrollment data becomes increasingly sparse. We present results for two continuous specifications where neighboring CREP enrollment variables are entered (1) linearly and (2) quadratically. We include all control variables found our the main model in both continuous specifications.

The regression results and corresponding marginal effects for this alternate specification are presented in Table S7 and Table S8 in the Supplemental Appendix. Reported standard errors for our regression estimates are robust and clustered at the tract level. Standard errors for the marginal effects are calculated using the delta method. As with our main model, we emphasize the marginal effects derived from this alternate specification.

The marginal effect for the maximum level of CREP stream miles (across all five-mile segments) in Table S8 is positive and significant. This suggests that an additional mile of CREP streambank enrollment increases the likelihood a tract enrolls in the program by 1.7 percent, on average, when the number of existing enrolled miles is entered linearly and 2.8 percent when entered as a quadratic. As in our main (discrete) model, the marginal effect of CREP acreage on hydrologically connected farms is negative and statistically significant. The point estimate on this variable implies that each additional acre of CREP enrollment on any hydrologically connected farms decreases the likelihood of enrollment by 0.01 percent when entered linearly and 0.02 percent when entered as a quadratic.

We use these results to derive to additional sets of average marginal effects, presented in Table S9, to further explore how the likelihood of CREP enrollment changes based on varying levels of neighboring streambank enrollment relative to the 50% bonus threshold. The first set, termed 'partial effects', measures the immediate change in the likelihood of joining the program at a specific level of existing streambank enrollment by upstream/downstream neighbors. The second set, termed 'cumulative effects', quantifies the total change in the likelihood of enrollment, relative to a baseline of zero neighboring enrollment.[12] These partial and cumulative effects are also visualized in Figure S5 for both the linear and quadratic forms of our alternate continuous specification.

The point estimates for the average marginal effects are statistically significant at the 1 percent level, with a few exceptions. Consistent with our main findings, the average marginal effects from our continuous specification indicate a substantial increase in the likelihood of enrollment once neighboring streambank enrollment surpasses the 50% threshold (5 stream miles). For instance, the cumulative effects derived from the linear specification reveal the likelihood of enrollment rises from 34.9 percent to 71.3 percent – a 36.4 percentage point increase – relative to the baseline category of zero neighboring enrollment. This sharp rise in the overall likelihood of enrollment coincides with an increase in the partial effects, with the greatest marginal increase in the likelihood of enrolling in CREP for riparian buffers occurring between 4 and 6 miles of existing streambank enrollment, peaking around the 50% bonus threshold. While the likelihood of enrollment continues to rise above this threshold, the rate of increase diminishes, indicating that landowners become less responsive to each additional neighbor enrolling once the critical mass for the bonus has likely been achieved, as shown by the trend in the cumulative effect (Figure S5a). These findings are consistent with expectations of landowner behavior if landowners are assumed to respond strongly to the incentive scheme.

In contrast, the results from our quadratic specification suggest a slightly different pattern, with landowners exhibiting the greatest responsiveness to neighboring participation at a lower level of existing streambank enrollment, between 2 and 3 miles, although the magnitude of this effect is smaller than that indicated by the linear specification around the 5-mile

[12]The partial effect can be interpreted as the change in the likelihood between two infinitesimally close points on the 'continuous scale of streambank enrollment, while the cumulative effect represents the overall change from a starting point of no neighboring enrollment.

threshold. The point estimates for this specification indicate an approximate 17 percentage point increase in the likelihood of enrollment at both 2 and 3 miles of existing streambank enrollment, after which the effect begins to decrease, suggesting a weakening of the overall (cumulative) effect at higher levels. Furthermore, the quadratic specification reveals that beyond 6 miles of neighboring streambank enrollment, where the overall likelihood of enrollment is highest, the rate of enrollment increase begins to decline, suggesting a potential weakening of the positive spillover effect at very high levels of neighboring participation, as illustrated in Figure S5b.

While these results from our continuous specifications similarly indicate that landowners' enrollment decisions for riparian buffers are indeed influenced by neighboring enrollment, particularly around the bonus threshold, they generally depict a more gradual, smoother increase in the likelihood of enrollment, as illustrated in Figure S5, compared to the discrete jump at the 50% threshold suggested by our main discrete specification. This divergence underscores the strength of using discrete bins in our primary model. By using categories, we directly capture the potentially abrupt change in enrollment behavior at the bonus threshold, providing a more targeted analysis of this critical point than a continuous specification.

Supplemental tables and figures

Table S3: Regression coefficients when using alternate sets of covariates

	(1)			(2)			(3)		
Variable	Coefficient		Std. Err.	Coefficient		Std. Err.	Coefficient		Std. Err.
Program variables									
max(CREP stream miles on any 5-mi. stream segment)†									
Streambank enrolled, 0 miles	-1.65511	***	0.12298	-1.59419	***	0.13664	-1.58725	***	0.14893
Streambank enrolled, 1.0-2.0 miles	0.33992	*	0.18750	0.28889		0.21489	0.47337	**	0.22310
Streambank enrolled, 2.0-3.0 miles	0.36046		0.24243	0.31974		0.30160	0.63804	**	0.31675
Streambank enrolled, 3.0-4.0 miles	0.40463	*	0.21845	0.30061		0.22830	0.57042	**	0.25038
Streambank enrolled, 4.0-5.0 miles	0.60105	**	0.23990	0.50493	*	0.26478	0.79408	***	0.28554
Streambank enrolled, 5+ miles	1.02119	***	0.26679	1.03741	***	0.26654	1.60784	***	0.28667
log(County CRP acres)	-		-	-0.16371		0.23558	-0.32425		0.26660
County avg. soil rental rate	-		-	-0.10723	***	0.04082	-0.09576	**	0.04310
CREP acres on hydrologically connected (HC) farms	-		-	-		-	-0.00305	***	0.00051
CREP acres on non-hyd. con. (NC) farms	-		-	-		-	0.00026		0.00028
Tract variables									
log(Farm acreage)	-		-	0.07597	**	0.03082	0.08171	**	0.03237
log(Tract stream miles)	-		-	0.03431		0.05844	0.00135		0.06282
log(Tract number of streams)	-		-	0.03001		0.06088	-0.00785		0.06750
Tract soil productivity (NCCPI)	-		-	-1.61478	**	0.77156	-1.27539		0.88266
Tract cropland acreage (%)	-		-	-0.03018		0.12364	0.04258		0.13320
Tract forest acreage (%)	-		-	-0.30333		0.24920	-0.25518		0.26255
Suitable salmon/trout habitat (=1)	-		-	0.33214	***	0.08042	0.16279	*	0.09448
Neighborhood variables (5-mi tract buffer)									
HC farms	-		-	-		-	-0.00186		0.00219
log(Stream miles on HC farms)	-		-	-		-	0.24994	***	0.06799
Soil productivity on HC farms (NCCPI)	-		-	-		-	3.51346	*	1.95943
Cropland acreage on HC farms (%)	-		-	-		-	-0.11187		0.36600
Forest acreage on HC farms (%)	-		-	-		-	0.43870		0.47458
NC farms	-		-	-		-	-0.00144		0.00124
log(Stream miles on NC farms)	-		-	-		-	-0.17840		0.11528
Soil productivity on NC farms (NCCPI)	-		-	-		-	-3.79718		2.31076
Cropland acreage on NC farms (%)	-		-	-		-	0.12905		0.40611
Forest acreage on NC farms (%)	-		-	-		-	0.21446		0.44539
log(Road miles)	-		-	-0.06109		0.06035	-0.05565		0.06616
Constant	-0.55319		0.45391	6.37239	***	1.91628	7.14234	***	2.20340
R-squared	0.27099			0.29959			0.32550		
N(obs)	20,372			16,451			16,451		
N(tracts)	2,399			1,940			1,940		
N(enroll)	304			274			274		

Notes: All models include period (year) and watershed fixed effects. Standard errors are robust and clustered at tract level. Superscript † indicates a discrete change from the baseline category for streambank enrollment (0.1-1.0 miles of neighboring streambank enrolled). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S4: Marginal effects when using alternate sets of covariates

	(1)		(2)		(3)	
Variable	Coefficient	Std. Err.	Coefficient	Std. Err.	Coefficient	Std. Err.
Program variables (all lagged by one year)						
max(CREP stream miles on any 5-mi. stream segment)†						
Streambank enrolled, 0 miles	-0.19596***	0.03154	-0.17522***	0.03157	-0.16080***	0.03180
Streambank enrolled, 1.0-2.0 miles	0.10110*	0.05727	0.07726	0.05935	0.12472**	0.06061
Streambank enrolled, 2.0-3.0 miles	0.10785	0.07706	0.08639	0.08666	0.17626*	0.09530
Streambank enrolled, 3.0-4.0 miles	0.12258*	0.06883	0.08071	0.06325	0.15468**	0.07170
Streambank enrolled, 4.0-5.0 miles	0.19111**	0.08150	0.14429*	0.08183	0.22791**	0.08989
Streambank enrolled, 5+ miles	0.34607***	0.09335	0.32999***	0.08905	0.50517***	0.08391
CREP acres on hydrologically connected (HC) farms	-	-	-	-	-0.00009***	0.00002
CREP acres on non-hyd. con. (NC) farms	-	-	-	-	0.00001	0.00001
Tract variables						
log(Farm acreage)	-	-	0.00226**	0.00092	0.00235**	0.00093
log(Tract stream miles)	-	-	0.00102	0.00174	0.00004	0.00181
log(Tract number of streams)	-	-	0.00089	0.00181	-0.00023	0.00194
Tract soil productivity (NCCPI)	-	-	-0.04805**	0.02298	-0.03672	0.02542
Tract cropland acreage (%)	-	-	-0.00090	0.00368	0.00123	0.00384
Tract forest acreage (%)	-	-	-0.00903	0.00743	-0.00735	0.00756
Suitable salmon/trout habitat (=1)	-	-	0.01074***	0.00291	0.00481*	0.00289
Neighborhood variables (5-mi tract buffer)						
HC farms	-	-	-	-	-0.00005	0.00006
log(Stream miles on HC farms)	-	-	-	-	0.00720***	0.00197
Soil productivity on HC farms (NCCPI)	-	-	-	-	0.10116*	0.05685
Cropland acreage on HC farms (%)	-	-	-	-	-0.00322	0.01054
Forest acreage on HC farms (%)	-	-	-	-	0.01263	0.01366
NC farms	-	-	-	-	-0.00004	0.00004
log(Stream miles on NC farms)	-	-	-	-	-0.00514	0.00332
Soil productivity on NC farms (NCCPI)	-	-	-	-	-0.10933	0.06694
Cropland acreage on NC farms (%)	-	-	-	-	0.00372	0.01170
Forest acreage on NC farms (%)	-	-	-	-	0.00617	0.01282
log(Road miles)	-	-	-0.00182	0.00180	-0.00160	0.00191
Period FE	✓		✓		✓	
Watershed FE	✓		✓		✓	
Tract & program variables			✓		✓	
Neighborhood attributes (5 miles)					✓	

Notes: Standard errors are computed using the delta method based on parameter estimates in Table S3. Superscript † indicates a discrete change from the baseline category for streambank enrollment (0.1-1.0 miles of neighboring streambank enrolled). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S5: Regression coefficients using 10 discrete bins for neighboring streambank enrollment

Variable	Coefficient		Std. Err.
Program variables (all lagged by one year)			
max(CREP stream miles on any 5-mi. stream segment)†			
Streambank enrolled, 0 miles	-3.3828	***	0.3447
Streambank enrolled, 1.0-2.0 miles	0.7983	*	0.4581
Streambank enrolled, 2.0-3.0 miles	1.1249	*	0.6493
Streambank enrolled, 3.0-4.0 miles	1.0626	**	0.4935
Streambank enrolled, 4.0-5.0 miles	1.0420		0.7432
Streambank enrolled, 5.0-6.0 miles	2.1288	***	0.8247
Streambank enrolled, 6.0-7.0 miles	2.3021	***	0.7205
Streambank enrolled, 7.0-8.0 miles	-		-
Streambank enrolled, 8.0-9.0 miles	5.0123	***	1.3200
Streambank enrolled, 9.0-10.0 miles	-		-
log(County CRP acres)	-0.4060		0.5694
County avg. soil rental rate	-0.2714	**	0.1234
CREP acres on hydrologically connected (HC) farms	-0.0066	***	0.0012
CREP acres on non-hyd. con. (NC) farms	0.0007		0.0007
Tract variables			
log(Farm acreage)	0.2277	***	0.0869
log(Tract stream miles)	-0.0372		0.1617
log(Tract number of streams)	-0.0528		0.1680
Tract soil productivity (NCCPI)	-3.4665		2.3037
Tract cropland acreage (%)	0.0632		0.3420
Tract forest acreage (%)	-0.7417		0.7004
Suitable salmon/trout habitat (=1)	0.3663		0.2304
Neighborhood variables (5-mi tract buffer)			
HC farms	-0.0061		0.0062
log(Stream miles on HC farms)	0.6591	***	0.1818
Soil productivity on HC farms (NCCPI)	7.5801		5.4349
Cropland acreage on HC farms (%)	-0.4100		1.0373
Forest acreage on HC farms (%)	1.1677		1.3138
NC farms	-0.0044		0.0035
log(Stream miles on NC farms)	-0.4420		0.2757
Soil productivity on NC farms (NCCPI)	-8.2388		5.9186
Cropland acreage on NC farms (%)	0.3909		1.0587
Forest acreage on NC farms (%)	0.8406		1.1918
log(Road miles)	-0.0965		0.1954
Constant	16.3076	***	5.4042
R-squared		0.3255	
N(obs)		16,451	
N(tracts)		1,940	
N(enroll)		274	
Period FE		✓	
Watershed FE		✓	

Notes: Standard errors are robust and clustered at tract level. Superscript † indicates a discrete change from the baseline category for streambank enrollment (0.1-1.0 miles of neighboring streambank enrolled). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S6: Marginal effects using 10 discrete bins for neighboring streambank enrollment

Variable	Coefficient		Std. Err.
Program variables (all lagged by one year)			
max(CREP stream miles on any 5-mi. stream segment)†			
Streambank enrolled, 0 miles	-0.1584	***	0.0367
Streambank enrolled, 1.0-2.0 miles	0.1056	*	0.0613
Streambank enrolled, 2.0-3.0 miles	0.1576		0.0983
Streambank enrolled, 3.0-4.0 miles	0.1473	**	0.0709
Streambank enrolled, 4.0-5.0 miles	0.1440		0.1147
Streambank enrolled, 5.0-6.0 miles	0.3365	**	0.1467
Streambank enrolled, 6.0-7.0 miles	0.3684	***	0.1231
Streambank enrolled, 7.0-8.0 miles		-	
Streambank enrolled, 8.0-9.0 miles	0.7354	***	0.0975
Streambank enrolled, 9.0-10.0 miles		-	
log(County CRP acres)	-0.0053		0.0074
County avg. soil rental rate	-0.0035	**	0.0016
CREP acres on hydrologically connected (HC) farms	-0.0001	***	0.0000
CREP acres on non-hyd. con. (NC) farms	0.0000		0.0000
Tract variables			
log(Farm acreage)	0.0030	***	0.0011
log(Tract stream miles)	-0.0005		0.0021
log(Tract number of streams)	-0.0007		0.0022
Tract soil productivity (NCCPI)	-0.0451		0.0300
Tract cropland acreage (%)	0.0008		0.0044
Tract forest acreage (%)	-0.0096		0.0091
Suitable salmon/trout habitat (=1)	0.0049		0.0031
Neighborhood variables (5-mi tract buffer)			
HC farms	-0.0001		0.0001
log(Stream miles on HC farms)	0.0086	***	0.0024
Soil productivity on HC farms (NCCPI)	0.0985		0.0709
Cropland acreage on HC farms (%)	-0.0053		0.0135
Forest acreage on HC farms (%)	0.0152		0.0171
NC farms	-0.0001		0.0000
log(Stream miles on NC farms)	-0.0057		0.0036
Soil productivity on NC farms (NCCPI)	-0.1071		0.0772
Cropland acreage on NC farms (%)	0.0051		0.0138
Forest acreage on NC farms (%)	0.0109		0.0155
log(Road miles)	-0.0013		0.0025
Period FE		✓	
Watershed FE		✓	

Notes: Standard errors are computed using the delta method based on parameter estimates in Table S5. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S7: Regression coefficients, continuous specifications

Variable	(1) Linear Coefficient		(1) Linear Std. Err.	(2) Quadratic Coefficient		(2) Quadratic Std. Err.
Program variables (all lagged by one year)						
max(CREP stream miles on any 5-mi. stream segment)†	0.54877	***	0.04311	1.04677	***	0.09891
max(CREP stream miles on any 5-mi. stream segment)^2	-		-	-0.08887	***	0.01558
CREP acres on hydrologically connected (HC) farms	-0.00286	***	0.00054	-0.00620	***	0.00095
(CREP acres on HC farms)^2	-		-	0.00001	***	0.00000
CREP acres on non-hyd. con. (NC) farms	0.00012		0.00029	-0.00044		0.00062
(CREP acres on NC farms)^2	-		-	0.00000	*	0.00000
log(County CRP acres)	-0.46158		0.28164	-0.41030		0.27402
County avg. soil rental rate	-0.08617	**	0.04262	-0.08116	*	0.04219
Tract variables						
log(Farm acreage)	0.09056	***	0.03136	0.08540	***	0.03239
log(Tract stream miles)	0.03446		0.06265	0.00895		0.06256
log(Tract number of streams)	-0.00090		0.06578	-0.00199		0.06802
Tract soil productivity (NCCPI)	-1.13268		0.86530	-1.27926		0.89286
Tract cropland acreage (%)	0.06945		0.13105	0.06915		0.13336
Tract forest acreage (%)	-0.22498		0.25668	-0.33667		0.26947
Suitable salmon/trout habitat (=1)	0.15064		0.09592	0.17397	*	0.09687
Neighborhood variables (5-mi tract buffer)						
HC farms	-0.00165		0.00211	-0.00187		0.00225
log(Stream miles on HC farms)	0.25691	***	0.06663	0.27506	***	0.06881
Soil productivity on HC farms (NCCPI)	2.62114		1.93037	2.72492		2.04302
Cropland acreage on HC farms (%)	-0.02852		0.36264	-0.00281		0.37718
Forest acreage on HC farms (%)	0.58046		0.44817	0.46892		0.45278
NC farms	-0.00083		0.00121	-0.00105		0.00124
log(Stream miles on NC farms)	-0.15198		0.11411	-0.13245		0.11706
Soil productivity on NC farms (NCCPI)	-3.42927		2.23629	-3.11598		2.30801
Cropland acreage on NC farms (%)	0.18218		0.39652	0.17694		0.40472
Forest acreage on NC farms (%)	0.36755		0.44632	0.30564		0.44952
Constant	5.90403	***	2.28638	5.07472	**	2.25853
R-squared	0.28376			0.30858		
N(obs)	16,488			16,488		
N(tracts)	1,943			1,943		
N(enroll)	276			276		
Period FE	✓			✓		
Wastershed FE	✓			✓		

Notes: Standard errors are robust and clustered at tract level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S8: Marginal effects, continuous specifications

Variable	(1) Linear Coefficient		(1) Linear Std. Err.	(2) Quadratic Coefficient		(2) Quadratic Std. Err.	(3) Coefficient		(3) Std. Err.
Program variables (all lagged by one year)									
max(CREP stream miles on any 5-mi. stream segment)[†]	0.01663	***	0.00142	0.02784	***	0.00262	0.02651	***	0.00258
CREP acres on hydrologically connected (HC) farms	-0.00009	***	0.00002	-0.00016	***	0.00003	-0.00009	***	0.00002
CREP acres on non-hyd. con. (NC) farms	0.00000		0.00001	-0.00001		0.00002	0.00001		0.00001
log(County CRP acres)	-0.01398		0.00861	-0.01210		0.00814	-0.01151		0.00790
County avg. soil rental rate	-0.00261	**	0.00129	-0.00239	*	0.00125	-0.00259	**	0.00123
Tract variables									
log(Farm acreage)	0.00274	***	0.00095	0.00252	***	0.00096	0.00246	***	0.00095
log(Tract stream miles)	0.00104		0.00190	0.00026		0.00185	0.00038		0.00185
log(Tract number of streams)	-0.00003		0.00199	-0.00006		0.00201	0.00007		0.00197
Tract soil productivity (NCCPI)	-0.03432		0.02624	-0.03774		0.02636	-0.03573		0.02604
Tract cropland acreage (%)	0.00210		0.00397	0.00204		0.00394	0.00176		0.00392
Tract forest acreage (%)	-0.00682		0.00778	-0.00993		0.00795	-0.00973		0.00786
Suitable salmon/trout habitat (=1)	0.00467		0.00306	0.00527	*	0.00304	0.00498	*	0.00299
Neighborhood variables (5-mi tract buffer)									
HC farms	-0.00005		0.00006	-0.00006		0.00007	-0.00006		0.00006
log(Stream miles on HC farms)	0.00778	***	0.00204	0.00811	***	0.00205	0.00775	***	0.00202
Soil productivity on HC farms (NCCPI)	0.07941		0.05880	0.08039		0.06054	0.08018		0.06003
Cropland acreage on HC farms (%)	-0.00086		0.01099	-0.00008		0.01113	-0.00005		0.01107
Forest acreage on HC farms (%)	0.01759		0.01358	0.01383		0.01336	0.01573		0.01339
NC farms	-0.00003		0.00004	-0.00003		0.00004	-0.00003		0.00004
log(Stream miles on NC farms)	-0.00460		0.00346	-0.00391		0.00345	-0.00479		0.00343
Soil productivity on NC farms (NCCPI)	-0.10389		0.06814	-0.09193		0.06844	-0.10711		0.06757
Cropland acreage on NC farms (%)	0.00552		0.01202	0.00522		0.01195	0.00561		0.01202
Forest acreage on NC farms (%)	0.01114		0.01353	0.00902		0.01326	0.01107		0.01329
log(Road miles)	-0.00134		0.00192	-0.00150		0.00191	-0.00149		0.00191
Period FE		✓			✓			✓	
Wastershed FE		✓			✓			✓	

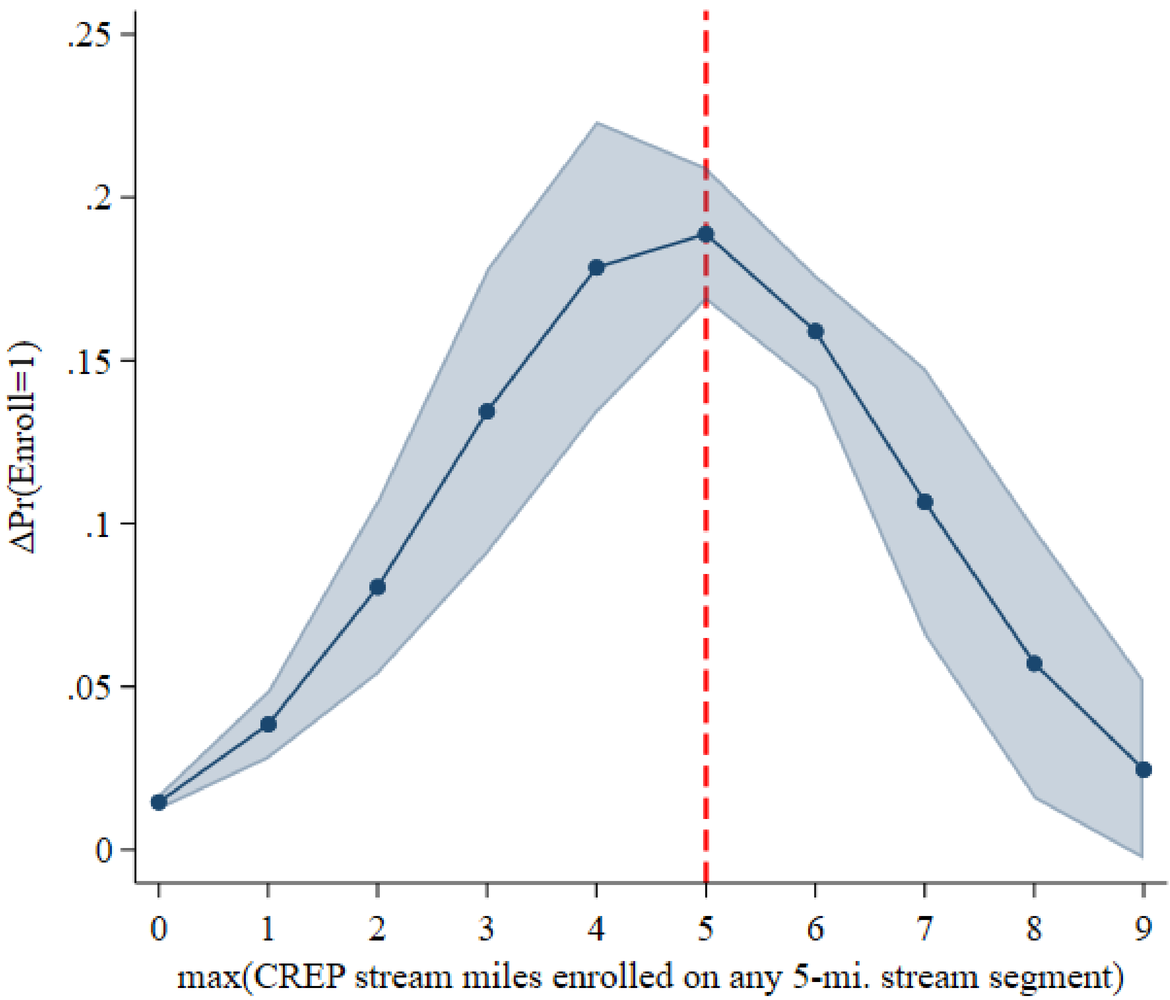
Notes: Standard errors are computed using the delta method based on parameter estimates in Table S7. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table S9: Average marginal effects, continuous specifications

Max streambank enrollment	(1) Linear			(2) Quadratic		
(5-mile segment)	AME		Std. Err	AME		Std. Err
Partial effect						
0 miles	0.0146	***	0.0013	0.0257	***	0.0025
1 miles	0.0384	***	0.0054	0.1015	***	0.0180
2 miles	0.0805	***	0.0137	0.1737	***	0.0265
3 miles	0.1344	***	0.0224	0.1709	***	0.0161
4 miles	0.1785	***	0.0229	0.1157	***	0.0167
5 miles	0.1888	***	0.0105	0.0522	**	0.0241
6 miles	0.1589	***	0.0089	-0.0064		0.0333
7 miles	0.1066	***	0.0210	-0.0658		0.0472
8 miles	0.0571	***	0.0212	-0.1298	**	0.0569
9 miles	0.0246	*	0.0141	-0.1779	***	0.0351
Cumulative effect						
1 mile	0.0251	***	0.0030	0.0599	***	0.0093
2 mile	0.0830	***	0.0122	0.2020	***	0.0333
3 mile	0.1902	***	0.0305	0.3809	***	0.0538
4 mile	0.3486	***	0.0540	0.5263	***	0.0594
5 mile	0.5358	***	0.0715	0.6100	***	0.0591
6 mile	0.7125	***	0.0723	0.6326	***	0.0665
7 mile	0.8460	***	0.0565	0.5969	***	0.0917
8 mile	0.9268	***	0.0345	0.4992	***	0.1352
9 mile	0.9661	***	0.0167	0.3423	*	0.1764

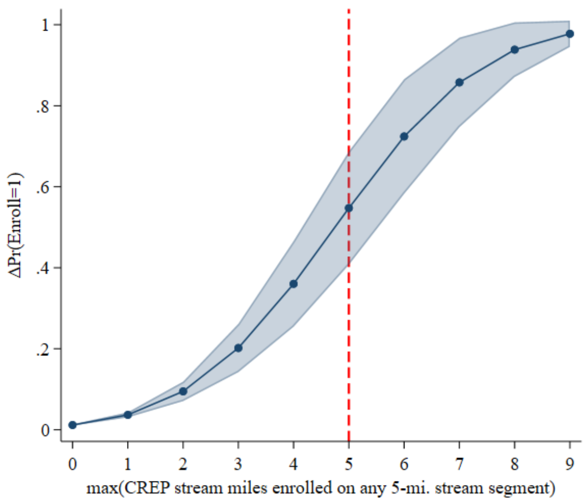
Notes: Standard errors are computed using the delta method based on parameter estimates in Table S7. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure S4: Marginal Effects with 95% Confidence Intervals for Discrete Specifications

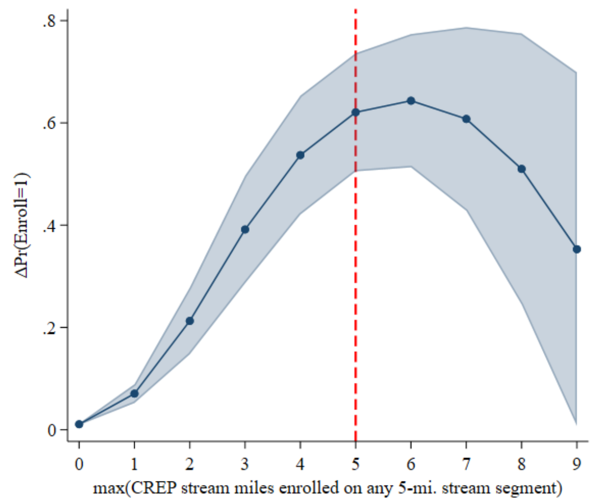


Notes: Average marginal effects are derived from parameter estimates in Table 2.2, relative to the baseline category (0-1 miles cumulative streambank enrollment). Standard errors computed via the delta method.

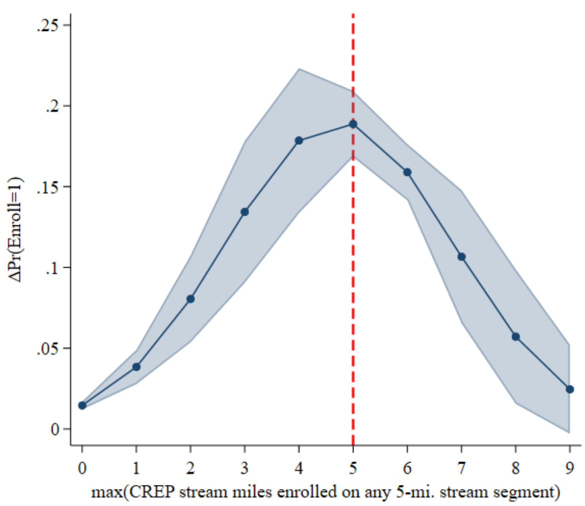
Figure S5: Partial and Cumulative Average Marginal Effects (AME) with 95% Confidence Intervals for Linear and Quadratic Specifications



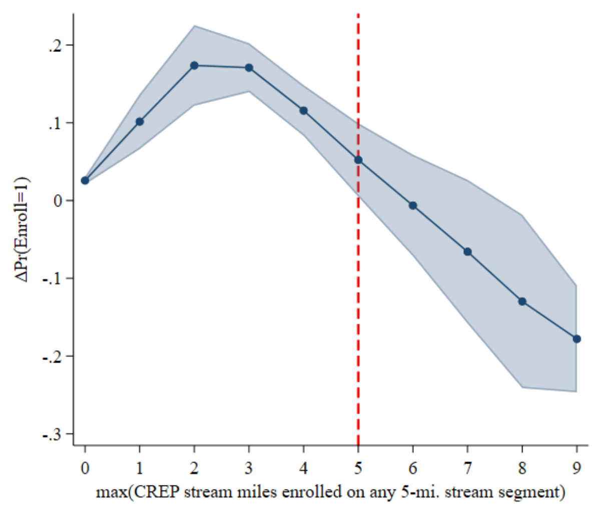
(a) Cumulative effects (linear)



(b) Cumulative effects (quadratic)



(c) Partial effects (linear)



(d) Partial effects (quadratic)

Notes: Average marginal effects are derived from parameter estimates in Table S7. Standard errors are computed using the delta method. Partial effects measures the instantaneous change in the response variable (the annual likelihood of enrollment) at a specified level of neighboring enrollment; cumulative effects measures the total change in the response variable relative to the baseline category (0 miles of neighboring streambank enrolled). Dashed, vertical line (in red) indicates the cumulative of streambank enrollment required to trigger the bonus payment on a five-mile stream segment.

Appendix C: Chapter 2 Conceptual Model

We examine an individual landowner's optimal enrollment timing decisions within voluntary conservation programs like the Oregon CREP, in which landowners agree to retire their land from agricultural uses in exchange for annual conservation payments and other financial incentives for a fixed contract period. We specifically examine how spatial coordination incentive schemes, such as the threshold-based Cumulative Impact Incentive Payment (CIIP) in our context, may generate spatial land use externalities that influence the concentration of conservation efforts at the landscape level.

We begin by formulating a landowner's decision concerning the conversion of an unenrolled portion of eligible farmland to conservation practices (e.g. riparian buffers) under a fixed-period contract. To simplify our analysis, we treat the land conversion to enrollment as effectively irreversible throughout the contract period.[13] Given our focus on the dynamic land conversion process, the critical decision for a landowner with eligible farmland is not simply *whether* to enroll, but precisely *when* to do so. For this reason, we treat the relevant decision from the landowner's perspective as one of optimal enrollment timing.

To represent the choice problem, first suppose that landowners are utility maximizing and derive utility from monetary income and (potential) conservation program participation. Let $e \in \{0, 1\}$ represent the landowner's enrollment decision for an eligible field (denoted i), with $e = 1$ denoting enrollment and $e = 0$ denoting non-enrollment (the status quo). Let y_e denote the monetary returns under enrollment choice e. In the unenrolled state ($e = 0$), returns are derived from agricultural land uses in time t, denoted $A(t)$, representing the opportunity cost of enrolling, such that $y_0 = y_0(t) = A(t)$. Conversely, in the enrolled state ($e = 1$), agricultural production ceases on the enrolled land, and returns become

[13] This is justified by the fact that reversing enrollment would necessitate incurring prohibitively high costs, including both the financial burden of land reconversion and substantial penalties for breaching the long-term CREP agreement.

annual fixed rental payments offered by the conservation program, denoted $R(x)$, during the contract period T, such that $y_1 = R(x)$. Here, $R(x)$ represents the payment level, a function of land characteristics like soil quality and program incentives like cost-sharing and practice incentive payments, captured in x. Thus, utility in time t is $u_0 \equiv u(0, A(t); x)$ in the unenrolled state, and $u_1 \equiv u(1, R(x); x)$ in the enrolled state.

To evaluate these choices dynamically, the landowner chooses the enrollment timing that maximizes his present discounted value of utility. Let denote δ as the discount factor, defined as $1/(1 + r)$ where r is the interest rate. Then the net present discounted value of utility from enrolling in period D represents the total discounted utility stream from that point forward and is given by:

$$\sum_{t=0}^{T} \delta^t \Big[u(1, R(x); x) - u(0, A(D + t); x)\Big] + \sum_{t=T+1}^{\infty} \delta^t u(0, A(D + t); x), \tag{3.10}$$

where the first summation represents the net present value of utility during the T-year enrollment contract, and the second summation represents the present value of utility after the contract expires (from period $D+T+1$ onwards), assuming the land reverts to agricultural use with utility $u(0, A(D + t); x)$ in each subsequent period.

Alternatively, the landowner might consider delaying enrollment by one period. In this case, the net present discounted value of utility from delaying enrollment – keeping the field in agricultural use in period D and enrolling in period $D + 1$ – discounted back to time D, is:

$$u(0, A(D); x) + \sum_{t=0}^{T} \delta^{t+1} \Big[u(1, R(x); x) - u(0, A(D + t + 1); x)\Big] \tag{3.11}$$

$$+ \sum_{t=T+1}^{\infty} \delta^{t+1} u(0, A(D + t + 1); x), \tag{3.12}$$

representing the utility from agricultural use in period D plus the discounted net present value of utility of enrolling in period $D+1$.

The optimal enrollment time will occur in period D only if the net present discounted value of utility from enrollment – the first summation in (3.10) – is positive *and* the total discounted value of utility from D-onwards is greater than or equal to (3.12). Thus, enrollment will occur in D if:

$$\sum_{t=0}^{T} \delta^t \Big[u(1, R(x); x) - u(0, A(D+t); x)\Big] > 0, \tag{3.13}$$

and

$$u(1, R(x); x) - u(0, A(D); x) > \delta^{T+1} \Big[u(1, R(x); x) - u(0, A(D+T+1); x)\Big]. \tag{3.14}$$

The landowner enrolls in period D only if (a) the net value of enrollment is positive and (b)

$$\frac{u(0; A(D); x) - u(0; A(D+T+1); x)}{u(1, R(x); x) - u(0, A(D), x)} < r, \tag{3.15}$$

where r is the interest rate.[14]

Equation 3.15 reveals several implications regarding optimal enrollment timing. First, a higher interest rate r implies that future utility is discounted more heavily, increasing the likelihood that 3.15 is satisfied in time period D. Second, increasing agricultural rents ($\partial A/\partial t > 0$) and positive marginal utility from rents ($\partial U/\partial A > 0$) imply higher opportunity costs from postponement, increasing the likelihood of enrollment in time period D. Third, higher immediate net utility gained from enrollment in period D (reflected in a larger denominator) increases the likelihood of enrollment in time period D.

[14] To see this, note 3.12 can be rewritten with $1/(1+r)$ replacing δ.

C.0.7 Incorporating spatial interactions and spatial coordination incentives

Interactions can be incorporated into (3.14) in several ways, depending on the assumed spatial structure of agent interdependence (Irwin & Bockstael, 2002). Drawing on the extensive literature of spatial interaction models in economic decision-making (e.g., Manski 1993; Brock & Durlauf 2001), which model an individual's choice as a function of either current or past choices of neighboring individuals, we assume that land use externalities create interaction effects among tracts of farmland that influence the valuation of enrollment. Our primary interest lies in interactions associated with the spatial coordination incentive offered through the Oregon CREP, the Cumulative Impact Incentive Payment. We hypothesize that enrollment generates positive land use externalities on neighboring fields located upstream or downstream, enhancing the value of enrollment in subsequent periods due to the increased probability of reaching the bonus threshold from neighboring enrollment. Given temporal lags inherent in the enrollment process, this interaction is viewed as a recursive process, whereby a field's enrollment value is a function of the past period's enrollment states of neighboring fields, specifically on *hydrologically-connected* fields within a five-mile reach of the stream network, consistent with the CIIP's spatial scope.

We further hypothesize that the CIIP's distinctive, threshold-based design generates nonlinearities in these enrollment interactions, contingent on the cumulative level of existing streambank enrollment relative to the 50% threshold required to trigger the bonus payout on a given five-mile stream segment. We assume the magnitude of interaction effects is assumed to be minimal far when enrollment is below the threshold, moderately positive when approaching the threshold, and strongly positive when nears or exceeds the threshold. In essence, we expect the CIIP to foster strong positive interaction effects, leading to a 'crowding in' of stream restoration practices, particularly as the level of enrollment approaches or meets the requirement for the additional bonus payout, thereby enhancing riparian buffer establishment.

Let $\lambda(N(t), \gamma)N(t)$ represent the interaction effect, where $N(t)$ is the proportion of riparian streambank that is enrolled in the focal field's local neighborhood at the beginning of time t, and γ denotes the threshold bonus trigger. We assume the interaction effect enters the landowner's utility function through income, given the potential payout from the threshold bonus. Consequently, the net returns from enrollment in period t are now given by $R(x) + \lambda(N(t), \gamma)N(t); x)$. Rewriting (3.10) to incorporate the effect of these interactions, the total discounted utility stream from enrolling the focal field in time period D becomes:

$$\sum_{t=0}^{T} \delta^t \Big[u(1, R(x) + \lambda(N(D), \gamma)N(D); x) - u(0, A(D+t); x)\Big] + \sum_{t=T+1}^{\infty} \delta^t u(0, A(D+t); x). \tag{3.16}$$

Specifically, if neighboring enrollment at the time of decision D already meets or exceeds the threshold (i.e., $N(D) \geq \gamma$), then the interaction effect simplifies to $\lambda(N(D), \gamma)N(D) = B$, where B represents the monetary value of the CIIP bonus.

Rewriting the conversion rule in 3.14, enrollment now occurs in the first period in which:

$$\begin{aligned} &u(1, R(x) + \lambda(N(D), \gamma)N(D); x) - u(0, A(D); x) > \\ &\qquad \delta^{T+1}\Big[u(1, R(x) + \lambda(N(D+T+1), \gamma)N(D); x) - u(0, A(D+T+1); x)\Big], \end{aligned} \tag{3.17}$$

which, for notational simplicity, we re-express (3.17) as:

$$\Delta u(D) > \delta^{T+1}\Delta u(D+T+1), \tag{3.18}$$

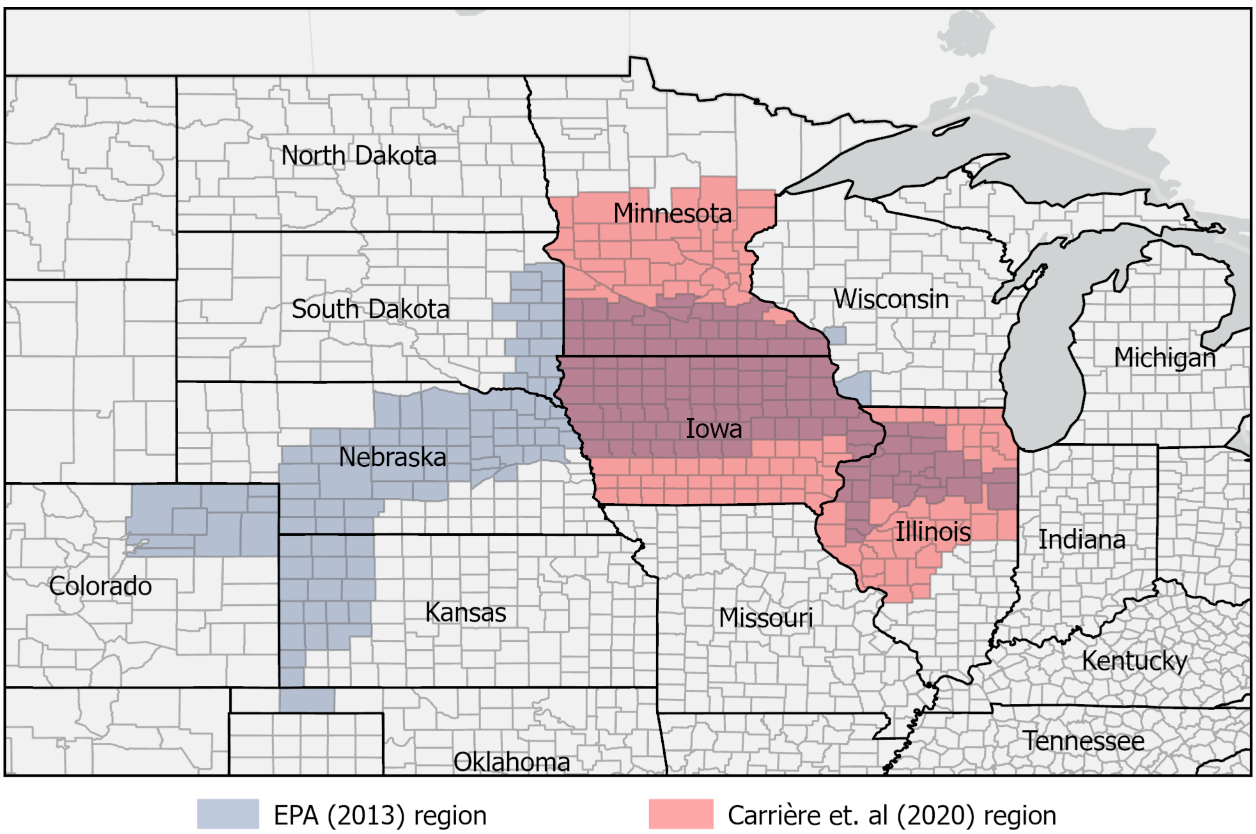
where $\Delta u(t; \gamma, x)$ denotes the net change in utility form enrollment in time period t with respsect to the status quo.

Because we are interested in testing whether the concentrated pattern of CREP enrollment we observe could be the result of net positive enrollment externalities, the hypotheses to be tested are: $\lambda(N(D)) > 0$, and $\frac{\partial \lambda}{\partial N} > 0$. In addition, we are interested in how the strength of these interactions varies at different levels of $N(t)$ with respect to the threshold γ. For instance, a strong net positive effect when $N(t)$ is just *below* the threshold would suggest a preemptive 'crowding in' effect of conservation effort, presumably as landowners feel more certain they will receive a payout. Conversely, a strong net positive effect *after* the the threshold would suggest that landowners tend to adopt a 'wait and see' approach, delaying their enrollment decision until receipt of the bonus is certain.

Appendix D: Chapter 3 Construction of Resistance Regions

Construction of Resistance Regions

Figure D.1: Regions where corn rootworm resistance or unexpected damages to Bt cornfields has been documented for years 2009 through 2016



Lacking explicit data on the spread and magnitude of resistance over the sample period, our empirical analysis proxies for the dynamic nature of resistance evolution by using a series of {0,1} indicators at the county level. For example, low resistance on a continuous scale might be characterized as 0.4, for example, while very low pest pressure could be characterized as 0.1. In either case, our model is consistent with mapping these instances of low pest pressure towards zeros (i.e., rounding down). Conversely, high pest pressure or very high pest pressure could be represented as a number greater than 0.5, in which case our model is consistent with rounding these up to 1. This method allows us to capture the general trend of resistance variation across regions, even with some uncertainty surrounding the exact boundaries of resistance regions.

Because of this uncertainty, which may also reflect heterogeneity in resistance patterns, we consider two resistance regions in our study. The first is a "core" resistance region encompassing counties in the Inner Western and Central Corn Belt where resistance has been documented or farmers have reported unexpected Bt corn damage. The second is a "total" resistance region encompassing a larger area that includes additional parts of the Greater Western and Central Corn Belt, as well as portions of the Great Plains.

We rely on two primary sources of information to construct our county-level dataset. The first is a map published by the Biopesticides and Pollution Prevention Division (BPPD) of the Environmental Protection Agency in 2013 (Figure D.2). The map depicts counties in the U.S. Corn Belt and Great Plains that had Cry3Bb1 corn fields with heavy damage and/or resistant western corn rootworm populations from 2009 to 2012. The map is based on BPPD's routine analysis of annual corn rootworm (CRW) resistance monitoring data submitted by the Monsanto Company (now owned by Bayer AG) for the Cry3Bb1 toxin. These data include rootworm populations randomly sampled within the Corn Belt, as well as follow-up investigations into problem fields with unexpected Cry3Bb1 damages in areas of the Corn Belt where elevated levels of corn root feeding had occurred.

The second source is a map published by Carrière et al. (2020) shown in Figure D.3. Their study examines data from 979 problem fields of Cry3Bb corn ($n = 927$) and Cry3Bb + Cry34/35Ab corn ($n = 52$) in northern Illinois, northern and central Iowa, and southern Minnesota. These fields were designated as problem fields due to 1) farmers reporting greater-than-expected rootworm damage to the Monsanto Company, 2) subsequent verification by

Monsanto personnel of appropriate Bt toxin production through ELISA testing, and 3) root damage exceeding established thresholds indicative of rootworm resistance to Bt corn. For our analysis, we identify the specific counties associated with each of the points in this dataset.

Figure D.2: 2013 Updated map showing the general areas in the US Corn Belt that have had Cry3Bb1 fields with heavy damage and/or resistant western corn rootworm populations (2009–2012)

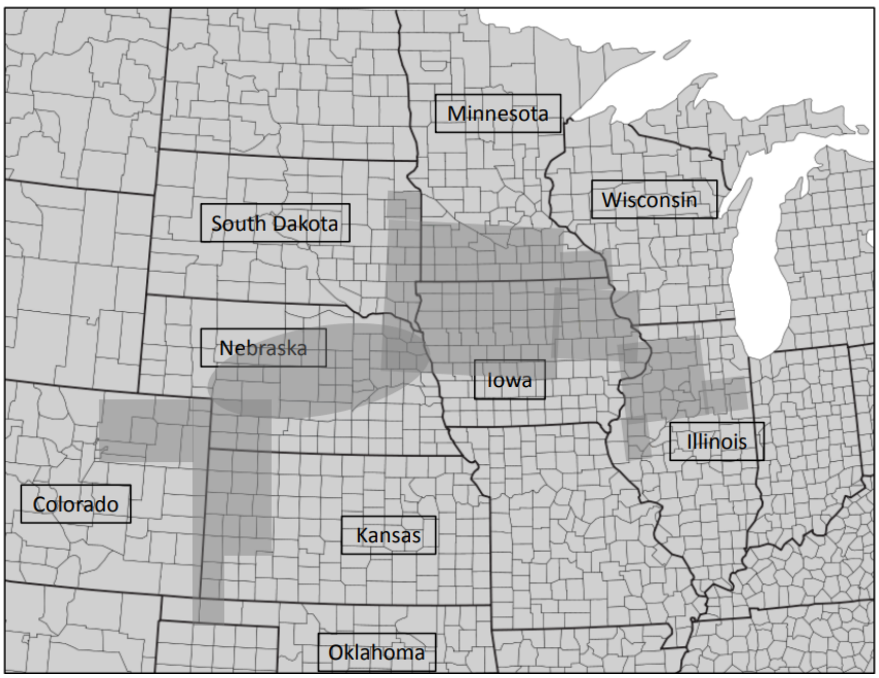


Figure source: Environmental Protection Agency (2013).

Figure D.3: Problem fields of Cry3Bb corn (blue) and Cry3Bb + Cry34/35Ab corn (yellow) from 2011 to 2016 in 25 crop-reporting districts (CRDs) in Illinois, Iowa, and Minnesota

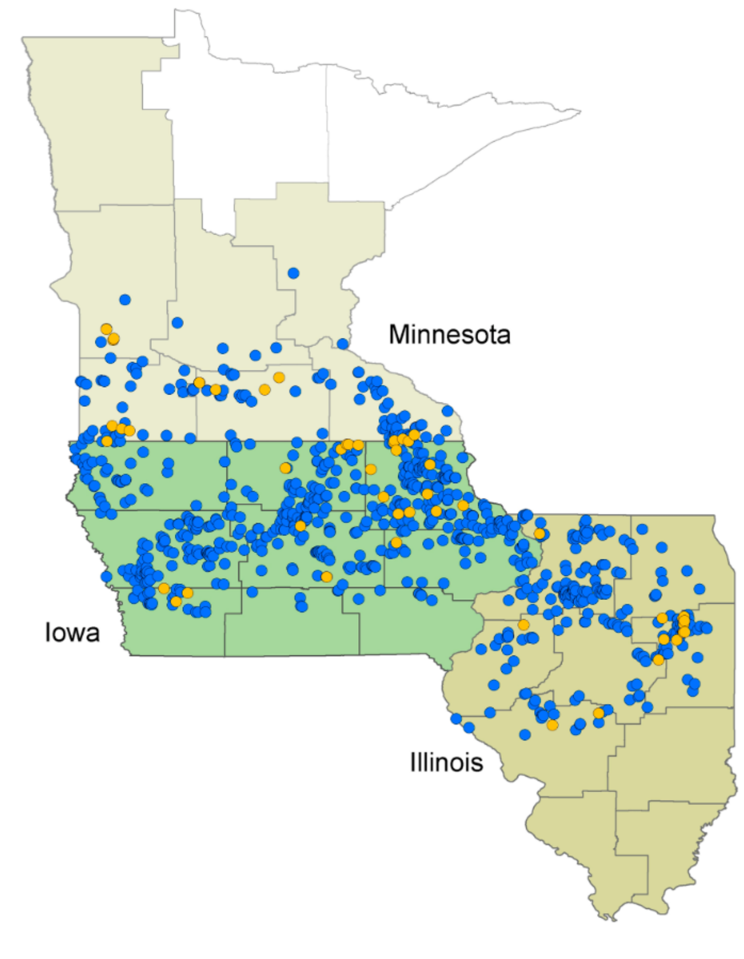


Figure source: Carriére et al., 2020.

Appendix E: Chapter 3 Hedonic Regression Approach

Hedonic Regression Approach to Constructing Seed Premia

Table E.1: Hedonic regression of seed prices

Seed Traits	Parameter Estimate	
Constant	127.93	***
2010	59.75	***
2016	94.01	***
Herbicide tolerance	18.68	***
× 2010	2.65	
× 2016	-5.79	
Corn borer resistance	13.15	***
× 2010	8.88	*
× 2016	1.90	
Rootworm resistance	18.06	***
× 2010	6.33	
× 2016	-0.83	
Pyramided traits	1.98	
Drought tolerance	1.94	
State indicator variables	Yes	
Observations	2,637	
Pseudo R^2	0.548	

Note: Standard errors were jackknifed to account for the complexity of the survey design (Dubman, 2000). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Hedonic approaches to analyzing agricultural goods are well established in the econometrics literature. In one of the earliest applications, Waugh (1928) used hedonic methods to explore how changes in vegetable quality affected vegetable prices. Subsequent studies extended hedonic methods to other agricultural products and inputs. For example, Griliches (1958) and Rayner & Lingard (1971) applied hedonics to fertilizer markets to estimate the implicit value of improved fertilizer qualities. In land markets, Palmquist (1989) demonstrated that farmland values could be explained by a hedonic function of land characteristics (e.g., soil quality), which allows for isolating the marginal contribution of each attribute to land prices.

The theoretical foundation for hedonic pricing was formalized by Rosen (1974), who modeled product differentiation under competitive equilibrium. Rosen showed that in a market of heterogeneous goods, an equilibrium "hedonic price function" emerges relating a product's price to its characteristics. The slope of this function with respect to any attribute gives the implicit marginal price of that attribute, reflecting both consumers' marginal willingness-to-pay and producers' marginal cost for the attribute in equilibrium. Assuming markets are competitive, a regression of observed prices on product traits provides unbiased estimates of the traits' implicit values.

In practice, hedonic regressions have been widely used to construct quality-adjusted price indices and to value new product innovations. For instance, Fernandez-Cornejo & Jans (1995) used a hedonic framework to construct quality-adjusted price indices for agricultural inputs (pesticides). In the context of genetically engineered (G.E.) crops, numerous studies have employed hedonic models to quantify the price premium of biotech traits. Shi et al. (2010) estimated the implicit prices of corn seed traits during the 2000s, finding significant positive premia for major biotech traits. More recently, Ciliberto et al. (2019) combined a discrete-choice demand model with a hedonic pricing approach and reported, among other findings, that U.S. farmers' willingness to pay for stacked corn traits grew over time.

In our analysis, we regress per-bag, state-level corn prices (expressed in 2016 dollars) on a variety of G.E. traits, trait-by-year interactions, an indicator of pyramided trait status, and an array of state fixed effects. By controlling for state, the trait coefficients are identified from within-state price variation, ensuring that, for example, the measured Bt trait premium is not confounded by regional price differences correlated with where Bt hybrids are grown. Evaluating the model for a given state, the predicted price of a rootworm-resistant hybrid minus the predicted price of an equivalent conventional hybrid gives the implicit Bt-CRW price in that year. Similarly, the difference between a pyramided Bt-CRW hybrid and a single-trait Bt-CRW hybrid gives the added value of the pyramided technology.

Table E.1 shows that the three major G.E. traits–herbicide tolerance (HT), corn borer resistance, and corn rootworm resistance–have economically large and statistically significant positive effects on seed price in the base year (2005). Having an HT trait adds about \$19 to the per-bag price in 2005. The corn borer resistance trait is associated with a roughly \$13 higher price, and the per-bag value of rootworm resistance in the base year is \$18. Based on these estimates, a triple-stack hybrid would have been about \$50 per bag higher than a conventional hybrid.

The trait-by-year indicators show an upward trend in overall seed price levels: even conventional seed became more expensive in later years (prices in 2010 are about \$60 higher than in 2005, and \$95 higher in 2016, all else equal. The corn borer trait shows a significant temporal shift: the premium for corn borer protection was higher in 2010 by about \$9, implying a total corn borer trait value of roughly \$22 in 2010. The rootworm resistance trait premium appears more stable over time. The point estimates suggest a moderate increase in the Bt-CRW premium in 2010 (\$6) and a slight decrease in 2016 (–\$1), but neither change is statistically significant.

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