Voluntary Programs To Encourage Refuges for Pesticide Resistance Management: Lessons from a Quasi-Experiment

Zachary S. Brown

Economists often treat pesticide resistance as a common-pool resource problem. While pecuniary economic incentives are the standard prescription for open-access market failures arising from such resources, non-pecuniary behavioral approaches (e.g., "nudges") are also effective in some cases. Yet non-pecuniary instruments have not previously been evaluated for managing pesticide resistance. I empirically evaluate the performance of such an intervention to manage pest resistance to genetically engineered *Bacillus thuringiensis* (Bt) corn. The U.S. Environmental Protection Agency permits sale of Bt seed conditional on seed producers compelling customers to plant mandated levels of non-Bt refuge to delay the evolution of Bt resistance. Because of compliance challenges, the Bt seed producer Monsanto piloted a social marketing program to promote refuge in 17 North Carolina counties in 2013–2014. Using 2013–2016 sales data, I use difference-in-differences, fractional regression, discrete changes-in-changes, and matched differences econometric models to identify the average treatment effect of the program on refuge planting. Results suggest that if it had covered all corn growers in North Carolina, the intervention would have led the average grower to plant between 2.6% (preferred estimate) and 5.8% more refuge in 2014 compared to the counterfactual. The program increased by at least 12% the average probability of planting *any* refuge in 2014. I find little evidence that effects of the program persisted in subsequent years after cessation, nor that the program increased compliance with mandated refuge thresholds. Informed by behavioral economics research on other environmental and resource policies, I discuss the implications of these findings for pesticide resistance management.

Key words: Bt refuges, difference-in-differences, discrete changes-in-changes, fractional regression, matching, moral suasion, nudges, quasi-experiments, resistance management, social marketing.

JEL codes: Q12, Q13, Q15, Q16, Q18, Q28.

One of the most widely adopted biotechnologies in agriculture is the insertion into crops of genes expressing a pesticidal toxin naturally produced by the bacterium *Bacillus thuringiensis* (Bt). Some of these toxins are lethal to specific coleopteran and lepidopteran insect pests, which are among the most significant insect pests in staple crops like corn and cotton. So-called Bt varieties of these crops have been estimated to significantly increase crop yields in many areas at risk of pest damages (Cattaneo et al. 2006; Hutchison et al. 2010), and to reduce the need for other pest control inputs into production (Lu et al. 2012). However, there has been recognition—and increasing evidence—that exposed pest populations evolve resistance to Bt toxins, threatening the sustainability of the technology (Gassmann, Carrière, and Tabashnik 2008; Carrière, Crowder, and Tabashnik 2010; Huang et al. 2014; Reisig and Reay-Jones 2015).

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Entomological research has shown that an effective way to sustain Bt effectiveness is to create *refuges* of non-Bt varieties planted near Bt varieties to maintain the genetic viability of insect pests still susceptible to these toxins in order to extend the future benefits of the technology (Tabashnik 1994; Gould 1998; Gould 2000; Gahan, Gould, and Heckel 2001; Tabashnik et al. 2003). Economic research has subsequently investigated the intertemporal tradeoffs involved in determining the optimal refuge size, since planting more refuge involves sacrificing some production to more pest damage today in order to reduce pest damage and achieve more production in the future (Laxminarayan and Simpson 2002; Livingston, Carlson, and Fackler 2004; Grimsrud and Huffaker 2006; Qiao, Wilen, and Rozelle 2008; Mitchell and Onstad 2014).

These dynamics and individual growers' lack of direct control over area-wide Bt resistance create the potential for it to manifest as an open-access resource problem. Within a given region, pest susceptibility can be viewed as non-excludable but rival (Miranowski and Carlson 1986). Without regulation, any grower may plant Bt crops and use the 'pool' of pest susceptibility to Bt toxins (non-excludability), whereas greater use of Bt crops by one grower over time increases resistance and decreases the effectiveness of Bt crops for neighbors (rivalry).[1] These characteristics suggest that growers would overuse Bt varieties relative to the social optimum. Conventional economic prescriptions for insect resistance management (IRM) therefore focus on the standard pecuniary instruments associated with common-pool resources, that is, enforceable (and ideally transferable) quotas or user fees that internalize the user costs of resistance (Ambec and Desquilbet 2012).

A number of governments employ a nontransferable quota approach by mandating minimum refuge sizes. The United States, through the Environmental Protection Agency (EPA 2001), was the first country to implement these requirements in the late 1990s. The EPA's refuge mandate policy has been referred to as "the most impressive mandatory IRM system ever developed" (Bourguet, Desquilbet, and Lemarié 2005), and other countries including Australia and India have implemented similar policies (Downes et al. 2010; Singla, Johnson, and Misra 2012). Implementation of the refuge quota in the United States is complicated: the EPA does not directly enforce these mandates with growers. Instead, the EPA requires producers of Bt seeds to ensure compliance among growers purchasing their products in order for producers to receive environmental permits from the EPA to sell Bt seeds (Bourguet, Desquilbet, and Lemarié 2005). To meet Bt refuge requirements, a consortium of Bt seed producers—the Agricultural Biotechnology Stewardship Technical Committee (ABSTC)—monitors and enforces compliance among growers through periodic audits and by time-limited denial of access to Bt seed products for growers found in significant noncompliance (Smith and Smith 2013). This indirect regulatory structure, coupled with the open-access resource issues raised above, has predictably led to noncompliance problems in Bt resistance management, particularly in the southeastern United States (Smith and Smith 2013; Hurley and Mitchell 2014; Reisig 2017).

In this paper I evaluate a pilot program testing a nonpecuniary approach to Bt resistance management among growers using Bt and non-Bt seed sales data. I econometrically evaluate a social marketing program implemented between the 2013 and 2014 growing seasons to increase Bt refuges among corn farmers in eastern North Carolina (NC). As the first empirical evaluation of a nonpecuniary resistance management intervention, this paper contributes to research exploring the usefulness of alternative, nonpecuniary approaches to managing open-access resources (Ostrom 2009, 2010). In addition, by analyzing three years of data following the pilot (which ended in 2014), I am also able to evaluate the persistence of the program's effects, a topic of increasing scrutiny in the behavioral economics literature (Brandon et al. 2017).

Using a number of econometric methods to construct a counterfactual based on untreated corn-growing counties in NC, I find a small but significant treatment effect on growers purchasing relatively more refuge seed in the first growing season following the program (I later justify why seed sales data

[1] A complication in this context is that there are likely countervailing public goods aspects to Bt adoption, whereby one grower's deployment of the technology has spillover, pest-reduction benefits to the grower's neighbors. This biological effect has been documented in cases of European corn borer reductions resulting from areawide adoption of Bt corn (Hutchison et al. 2010).

can be used as a proxy for refuge planting). This effect appears to have dissipated in subsequent years. The analysis also reveals that the program increased the probability that a grower planted *any* refuge in 2014 by 12%, but finds little evidence that the program affected grower compliance with the exact size requirements of the EPA's refuge mandate.

This research is important to consider against the application of traditional common-pool resource economics to IRM. The transferable quotas and user fees traditionally advocated for managing commons aim to institute well-defined property rights to incentivize the economically efficient conservation of pesticide susceptibility (Vacher et al. 2006; Ambec and Desquilbet 2012). Yet current refuge mandates, as implemented in the United States and elsewhere, are deficient in two of the three requirements for well-defined property rights: enforceability and transferability. As discussed above, the EPA indirectly enforces refuge mandates with growers through Bt seed producers, whose profit incentives in selling their higher-priced Bt products (Shi et al. 2013) may conflict with regulatory incentives for refuge enforcement.[2] Meanwhile, making refuge quotas transferable between growers, aside from limited practicality within current regulatory structures, poses a number of bioeconomic questions about the effectiveness of heterogeneously distributing refuge. If a grower in NC increases her refuge size above required levels, and sells this "excess refuge" to a noncompliant grower, the bioeconomic effects of this transaction would differ depending on the purchaser's location relative to the seller, for example, whether the purchaser is in a nearby county or a different region of the country. A host of entomological questions related to pest mobility, habitat and spatial configuration of the crop area would likely be relevant to consider in efficiently implementing transferable refuge mandates (Storer et al. 2003; Storer 2003).

These impediments to institutionalizing well-defined property rights for refuges may reflect larger challenges of using traditional economic prescriptions for common-pool resources in resistance management. This further argues for applying behavioral "nudges" studied in other environmental policy contexts (Croson and Treich 2014) to resistance management. This could include efforts to cement and institutionalize social norms to promote pro-social behavior, that is, *moral suasion* (Romans 1966). The effectiveness of moral suasion in other contexts has shown mixed results (van Kooten and Schmitz 1992; Torgler 2004; Dal Bó and Dal Bó 2014). A related approach that has received much recent attention is the use of social comparisons, whereby individuals are given information about how their behavior with respect to a policy goal (e.g., energy or water use) compares with their peers. Social comparisons have been shown to have small but measureable effects on promoting target behaviors, often at low cost (Allcott 2011; Ferraro and Price 2011; Brent, Cook, and Olsen 2015). The mechanisms by which social comparisons operate remains underexplored in the economic literature (which is more concerned simply with cost-effectiveness of such approaches), though some have proposed that such comparisons communicate an implicit norm to individuals and thus amount to a form of moral suasion (Ferraro and Price 2011). The social marketing program analyzed here involved both moral suasion and social comparison elements. Cooperative approaches to common-pool resource management have also received attention as a potentially more effective means of management than traditional economic theory would predict (e.g. Rustagi, Engel, and Kosfeld 2010). Community-based social marketing may catalyze such cooperation in some contexts (Kennedy 2010). Cooperative approaches to resistance management could take the form of growers' associations generating their own conservation rules, implementing coordination, and enforcement mechanisms.

It is important to note that the types of nonpecuniary, behavioral interventions discussed above differ in important ways from conventional university extension programs aimed at increasing the adoption of agricultural technologies and practices (e.g., Baumgart-Getz, Prokopy, and Floress 2012; Rogers 2003). Much traditional agricultural extension disseminates information about the impacts of new technologies or practices. We would not expect such efforts to solve common-pool resource dilemmas, since even (and perhaps especially) fully informed

[2] Whether Bt seed sellers have a profit incentive to preserve Bt susceptibility, and hence enforce refuge mandates independent of EPA regulatory pressure is an interesting question that likely relates to the nature of the intellectual property and patents associated with Bt seed products. There appears to be no empirical economic research on this question, presenting an opportunity for future work.

resource users often find it in their self-interest to ignore scarcity rents associated with such resources. In line with this prediction, Reisig (2017) argues that such extension-based information dissemination has been ineffective at increasing farmers' Bt refuge planting intentions. In contrast, the behavioral intervention analyzed in this paper focused instead on enhancing the salience of community values, rather than disseminating technical information on refuge mandates. While such interventions have previously been proposed for improving IRM (EPA 2001; Hurley and Mitchell 2014; NCCA 2015), this paper provides a first empirical evaluation.

The following sections of this paper proceed as follows. The next section discusses the EPA's refuge mandates and publically available compliance information in more detail in order to better frame the context of the analysis. This section then goes on to describe the implementation of the social marketing program under evaluation. The remaining sections describe the data used in the analysis, as well as the econometric methods and results, before concluding with a discussion about the study's broader implications, open questions, and limitations.

Background on U.S. Refuge Mandates and the Evaluated Social Marketing Program

The size of mandated refuge in the United States depends on the location of the grower and the type of Bt corn grown. Currently (and over the time period analyzed), a grower in states belonging to the Corn Belt (effectively, the midwestern United States) using a Bt corn variety with a single type of Bt gene is required to plant at least 20% of any corn field as a non-Bt refuge. If a Corn Belt grower uses a plant variety containing multiple types of Bt genes (so-called "stacked" trait varieties), the refuge mandate falls to 5% of any corn field. For corn growers in Cotton Belt states (effectively, the southeastern United States, including NC), the mandated refuge size increases to 50% when planting single-trait and 20% for stacked-trait Bt corn (EPA 2017). The scientific rationale for the smaller refuge requirement for stacked-trait Bt corn is that the pest evolution of cross-resistance to multiple types of Bt genes, each producing slightly different versions of the Bt pesticides, will arise less frequently than resistance to a single Bt gene (EPA 2017). The rationale for differentiating the mandate between the Corn and Cotton Belt is that key pests like the corn earworm infest both corn and cotton fields, and therefore the mandate should account for greater selection pressure on these pests through the use of Bt corn and cotton in this region (EPA 2001; Singla, Johnson, and Misra 2012). While some level of refuge is almost certainly justified based on the common-pool resource logic discussed in the introduction, economic efficiency was not used as an explicit criterion in formulating refuge policy. Further, there is still little known as to whether these refuge thresholds are economically efficient. Livingston, Carlson, and Fackler (2004) find in simulation modeling that existing cotton refuge requirements are higher than is economically efficient, though Qiao, Wilen, and Rozelle (2008) find that such a conclusion may be highly sensitive to biological parameters determining whether Bt susceptibility is renewable in the pest population.

To enforce refuge requirements, the ABSTC develops a Compliance Assurance Program (CAP) implemented by each industry member. CAP activities consist of grower education of refuge requirements, and growers found in non-compliance through on-farm assessments are subject to a "phased compliance program," which can ultimately result in the grower being prevented from purchasing Bt products for a period of two years. In addition, the EPA requires yearly CAP reports from the ABSTC, using data collected using anonymous web-based grower surveys and on-farm assessments of limited numbers of growers.

Despite these efforts, compliance with the EPA's refuge requirements has been an ongoing challenge. CAP reports from the 2013 web surveys indicated a 73% nationwide rate of compliance with refuge size requirements based on a sample of 1,001 growers (Smith and Smith 2013). The compliance rate in the Cotton Belt was 51%, based on a sample of 95 growers. One in five Cotton Belt growers reported that in 2013 they did not plant any refuge. Because they are based on (albeit anonymous) self-reports, these surveys are likely to be overestimates of actual compliance rates, for example, due to growers misunderstanding refuge requirements (Martinez 2014). On-farm assessments, in contrast, are performed by auditors who directly verify compliance. However, these assessments are targeted in regions with high pest resistance risk, and are not statistically random samples. Nevertheless, the 2013 on-farm assessment of 1,751 growers nationwide found 423 (24%) in non-compliance (Martinez 2014). While state-level data is not publically available for either the web surveys or on-farm assessments, research by Reisig (2017) suggests that growers in North Carolina—the focus of this paper—appear particularly non-compliant. In recent years ABSTC has also explored the use of seed sales data (like those analyzed here) to target assessments at growers who are likely out of compliance due to the underpurchase of refuge seed (Martinez 2014).

In addition to the above CAP activities, which aim at changing grower behaviors to comply with refuge requirements, "refuge in a bag" (RIB) products have been deployed in the Corn Belt to ensure compliance. RIB seed mixes Bt and non-Bt seed at predetermined proportions prior to selling the product in order to meet refuge requirements. This can be used in principle to fully control compliance by making RIB seed the only Bt product available on the market. However, these products—which lead to planted refuge being uniformly mixed with Bt varieties throughout the field—are less effective at delaying resistance than structured refuge due to RIB's more diffuse selection pressure on pest populations (Mallet and Porter 1992; Onstad et al. 2011) and to its greater potential for cross-pollination between Bt and non-Bt varieties (Yang et al. 2014). Because of this greater resistance risk, RIB seed is not currently approved for meeting refuge requirements in the Cotton Belt. Thus, RIB reflects the reality of a tradeoff between biological and human behavioral constraints (Onstad et al. 2011), and the limitations on its use necessitate alternative behavioral instruments.

Social Marketing for Refuge

Since the EPA's introduction of refuge requirements, researchers have recognized the need for focused efforts on changing grower behavior to comply with these requirements (EPA 2001; Hurley and Mitchell 2014; NCCA 2015). Most documented efforts in this domain are directed at "awareness-building" activities (Martinez 2014). Assessments of the limited effectiveness of these efforts (Reisig 2017) motivate alternative behavioral interventions of the type found effective at managing other common-pool resources.

This paper focuses on a social marketing program piloted by Monsanto. Between the 2013 and 2014 planting seasons (which run from March through May), the company's Southern Land Legacy (SLL) program was launched in 17 contiguous counties in the coastal plain of North Carolina, the main corn-growing region of the state (see figure 1). The program combined a philanthropic effort guided by grower input with an advertising campaign to promote compliance with refuge regulations. The philanthropic effort was conducted as follows: Monsanto pre-selected three local charities that would be considered for a single grant of $2,000. To select which of the three charities would receive the award, Monsanto held a vote among corn growers who (*a*) farmed in one of the SLL pilot counties in 2013 and (*b*) planted the "required amount of Monsanto refuge seed." The required amount of refuge seed is clearly defined on the product tag of Monsanto's Bt products, and reflects the EPA's Bt refuge requirements.

The advertising campaign consisted of billboards and print ads, along with a website (Monsanto 2015). These materials promoted farmers who were deemed (by Monsanto) as exceptional in their refuge planting; agreeing to such promotion was voluntary and did not bring any additional compensation to farmers. In the parlance of behavioral economics and psychology, these promotional materials were used as a form of moral suasion with a social comparison element. The ads appealed to a sense of community and preserving the effectiveness of Bt seed for future generations as a reason to plant refuge, using role model growers in the community to establish a social norm of refuge compliance. Some quoted text from three such advertisements (found in full at the website) are shown in figure 2.

The key message across all three quotations articulates the strong externalities involved in refuge planting, as well as the proposed means of addressing them: moral suasion to induce voluntary compliance. The externality arises through the asymmetric individual costs of planting refuge instead of Bt varieties and the diffuse community-wide benefits of preserving effectiveness of the technology for the future. Painted in this light, the rational, self-interested grower would not plant refuge (no mention is made of the EPA regulations in these quotations, or in any of the reviewed marketing materials). Yet the advertising campaign appealed to growers' concerns for their

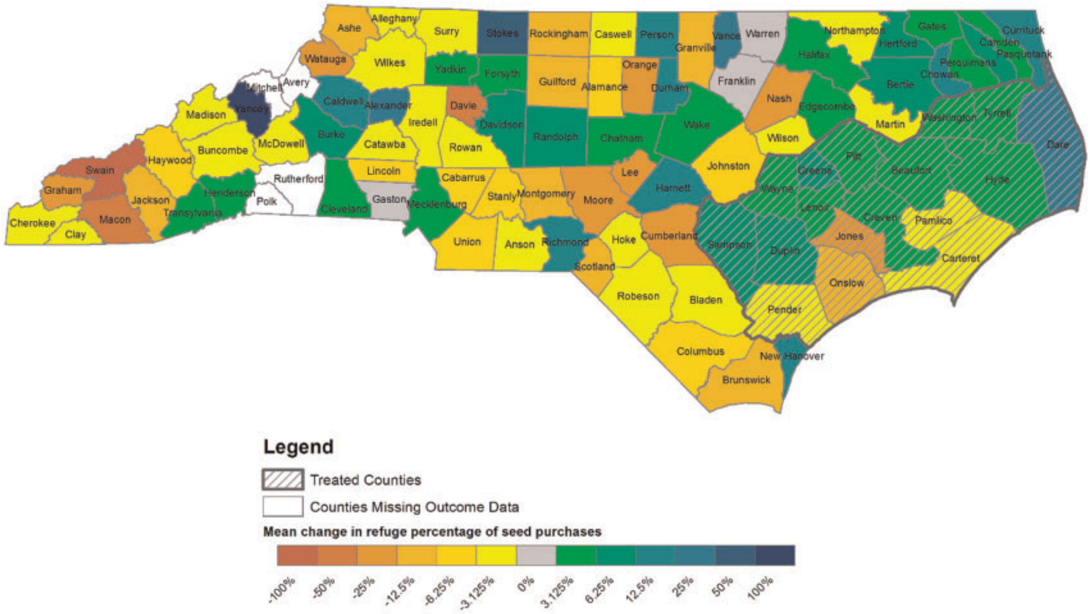

Figure 1. North Carolina counties eligible for the Southern Land Legacy program in 2014 pilot, by percent change in 2013–2014 refuge seed purchases

<u>*Advertisement 1*</u>
"I've always been told that the right thing and the hard thing are the same thing. And when times are tough, those decisions get tougher to make. But with refuge planting, we can't afford to take chances. As farmers, we have a duty to protect the land and the technology, not just for ourselves, but for our community."

<u>*Advertisement 2*</u>
"As a second-generation farmer, most of what I know I learned from my father. He taught me the basics like seed planting and soil health, but he also taught me that our farm is an important resource to the community. Our neighbors are counting on us for food and jobs, so to ensure my farm will always be there, I can't just focus on the here and now. I have to be thinking ahead, I have to plant a refuge."

<u>*Advertisement 3*</u>
"It's easy to think that buying refuge seed is just another of the many choices we make each fall as farmers. But it's a decision that's bigger than farming. When I buy seed, I have to think about the wellbeing of my community, the people counting on me every day for jobs, food, and support. If I base seed decisions on my priorities alone, what does that say about my commitment to those who matter most?"

Figure 2. Social marketing examples from Southern Land Legacy advertising campaign

Source: Monsanto, http://southernlandlegacy.com/.

community. One could argue that such concerns could arise either for purely altruistic reasons or perhaps because of "enlightened self-interest" (Besser 2004).

Data and Summary Statistics

To evaluate the effect of the SLL pilot, I use grower point of sale (GPOS) data from Monsanto for corn seed sales by Monsanto in North Carolina for 2013–2016. Because the objective of the SLL program was to increase refuge compliance, any target impacts of the program should show up in the GPOS data through increases in the proportion of seed sales associated with non-Bt varieties. The original data are disaggregated by 13 different seed varieties, 10 of which contain Bt genes. Of the 10 Bt varieties, four include both a standard and a RIB version. I only

Table 1. Summary Statistics for Bt and Non-Bt Corn Seed Sales in SLL and Non-SLL Counties

	Full sample	SLL counties	Non-SLL counties
Total growers	493	96	397
Number of counties	96	17	79
Relative Bt share of corn seed sales[a]			
2013	[redacted]	+3.88%***	−1.05%
	(0.786%)	(0.838%)	(0.928%)
2014	+1.45%*	+3.06%**	+1.01%
	(0.827%)	(1.20%)	(1%)
2015	+2.27%**	+4.56%***	+1.67%
	(0.885%)	(1.21%)	(1.06%)
2016	+3.49%***	+5.43%***	+2.98%***
	(0.603%)	(0.746%)	(0.733%)
Relative 2013 seed sales[b]	1	3.27***	0.727***
	–	(0.648)	(0.989)
Mean yield, 2000-2013[c]	102	117***	98
(bushels / acre)	(26)	(12)	(28)

Note: Standard errors of mean estimates appear in parentheses, clustered by grower county. Superscript [a] indicates mean percentage of seed sales for Bt products. Mean statistics calculated relative to 2013 full sample (by subtracting 2013 full sample mean, not reported); [b] = geometric mean of seed sales, divided by geometric mean for full sample; [c] = county-level mean yields averaged over 2000-2013. Asterisks *, **, and *** indicate statistical difference of 10%, 5%, and 1% levels relative to the full sample mean (in 2013, for time-varying variables). Redacted cells pertain to proprietary information that cannot be disclosed.

have access to the disaggregated product data for 2013 and 2014, and only the volumes of refuge and Bt seed in 2015 and 2016. While RIB may be planted in North Carolina, these products may not be used to satisfy refuge requirements due to the state's being within the Cotton Belt. Within this region, RIB is thus considered simply as Bt seed. I categorize "Bt varieties" as consisting of standard Bt and RIB products. Due to the proprietary nature of these data, table 1 reports statistics as either the in-year subsample differences from the full sample mean, or the change between 2013 and each subsequent year. As a control and matching variable in the econometric analysis, I also use data on pre-SLL mean corn yields from the National Agricultural Statistics Service (NASS 2017) County Agricultural Production Survey for North Carolina from 2000–2013 (see table 1).

I analyze three outcomes, all derived from the raw GPOS data: the percentage of corn seed sales corresponding to Bt versus non-Bt varieties, the probability that a grower plants *any* refuge, and imputed grower compliance with EPA refuge mandates. Fraction of refuge planted is most directly relevant for the evolution of Bt resistance. The fraction of growers planting any refuge is a key metric frequently reported in regulatory documents (Smith and Smith 2013; Martinez 2014). Compliance with the mandate is the object of regulation and most frequently discussed in literature on grower IRM behavior (e.g., Hurley and Mitchell 2014).

Table 2 reports the percentage of Bt seed sales that is directly computed for each grower-year in the data. An indicator for whether a grower plants any in a given year is imputed from the seed sales data as a binary variable taking a value of one when Bt seed sales are less than 100% in that grower-year, and zero otherwise. Grower compliance with the refuge requirements in a given year is imputed from the seed sales data as a binary variable taking a value of one when Bt seed sales are less than 80% in that grower-year, and zero otherwise. This reflects the EPA refuge mandate in NC that 20% of corn grown on farms must be non-Bt when Bt corn expressing two or more Bt proteins is grown (I refer to this as "multi-trait" Bt corn). The refuge mandate rises to 50% when single-trait Bt corn is grown (EPA 2014). Most corn grown in NC is now multi-trait: the 2013 GPOS data indicate that over 99.1% of Bt seed sales were multi-trait, and so we use the 20% refuge target to impute compliance. Nevertheless, it is important to emphasize there is likely measurement error in this variable, some reasons for which are taken up later.

In the full sample, the share of Bt products in total sales steadily increased between 2013 and 2016. The Bt share of seed sales was higher in SLL counties throughout this time period. Comparing time trends, SLL counties saw a decrease of 0.82% in the Bt share of

Table 2. Percentages of Growers Planting Any Refuge and Imputed Mandate Compliance

	Full sample	SLL counties	Non-SLL counties
Grower planted any refuge [a]			
2013	63.4%	67.0%	62.4%
	(2.74%)	(7.20%)	(0.0289)
2014	54.8%***	68.1%	51.2%***
	(3.11%)	(0.0744)	(3.24%)
2015	46.0%***	55.3%	43.5%***
	(0.0306)	(7.28%)	(3.29%)
2016	45.7%***	54.3%	43.5%***
	(2.83%)	(6.65%)	(2.99%)
Imputed refuge compliance [b]			
2013	12.8%	3.3%***	15.4%
	(1.69%)	(1.94%)	(1.88%)
2014	11.2%	4.4%***	13%
	(1.86%)	(2.24%)	(2.25%)
2015	7.19%***	4.21%***	7.94%***
	(1.22%)	(2.18%)	(1.42%)
2016	6.49%***	1.04%***	7.85%***
	(1.14%)	(0.992%)	(1.37%)

Note: Standard errors of mean estimates appear in parentheses, clustered by grower county. Superscript [a] = calculated as a percentage of growers purchasing any refuge (non-Bt) seed; [b] = percentage of growers with refuge seed > 20% of total purchase volume. Asterisks *, **, and *** indicate statistical difference of 10%, 5%, and 1% levels relative to the full sample mean.

seed sales between 2013 and 2014 (the period in which the SLL program was piloted) before rising by 1.5% in 2015 and 0.87% in 2016. Non-SLL counties saw an increase of 2.06% in the share of Bt seed sold in 2014, another 0.67% increase in 2015, and a 1.31% increase in 2016. To foreshadow the econometric analysis that follows, if we view the non-SLL counties as a control group, we can compare the 2013–2014 change in the average grower's Bt percentage of seed sales in SLL counties (the treatment group) versus non-SLL counties (the control group). This exercise suggests that the SLL program appears to have yielded a 2.88% (= 0.82% - (-2.06%)) decrease in the Bt percentage of seed sales for the average grower in the SLL-eligible counties, compared to the expected 2.06% increase that would have occurred in the absence of the SLL program—which we observe in the non-SLL counties. Similar logic suggests the program produced a 2.05% decrease in 2015 and a 2.48% decrease in 2016.

Similar differences-in-differences (DID) logic suggests that the SLL program generated a 12.3% increase in growers planting refuge in 2014 (summarized in table 2), compared to what would have happened without the program. As with the fraction of Bt planted, this program effect appears to attenuate in subsequent years. The program also appears to have increased compliance in treated counties by 3.5% in 2014 for the average grower, and by even higher percentages in later years: 8.37% in 2015 and 5.29% in 2016. However, the limitations of a DID exercise on the data are particularly apparent with refuge compliance, particularly because of the zero lower bound on this outcome variable (zero/one bounds also apply with the Bt share of seed sales, but pose less of a problem in econometric analysis, as we will see later). With a 2013 imputed refuge compliance level of 3.3% in the SLL counties, the DID counterfactual for SLL counties in 2015, for example, would imply the mathematically nonsensical result that without the program refuge compliance would have been negative (3.3% −7.46% = −4.16%). One way to address this issue is to consider differences in proportional changes in outcomes: In non-SLL counties, 2015 refuge compliance is 0.85 of 2013 compliance. Applying this proportional decline in compliance to SLL counties implies a counterfactual of 2.79% compliance in the SLL counties in 2015 in the absence of the SLL program, suggesting the SLL program improved compliance by 1.4%, which is much more modest than the 8.37% increase implied by a naïve DID estimate. We also conduct inference on these proportional changes in the econometric analysis.

A number of other confounding factors are important to consider in econometric analysis. In particular, one may question the validity of

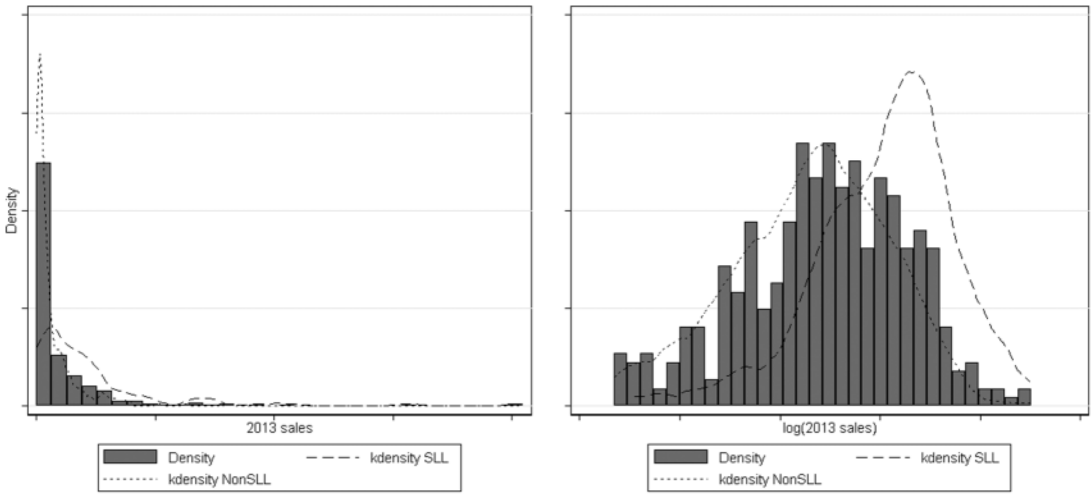

Figure 3. Histograms and kernel densities of raw and log-transformed 2013 seed sales volume, by SLL and non-SLL counties

Note: Data shifted by a randomly drawn constant, and axes values removed to protect the proprietary dataset. Kernel densities calculated using the Epanechnikov kernel with Stata's default bandwidths.

using the non-SLL counties as a control group to proxy for the counterfactual if the SLL counties had preexisting characteristics that were significantly different from the non-SLL counties. The most notable difference between the SLL and non-SLL counties is that counties targeted by the SLL program are concentrated in the major corn-growing area of North Carolina (see figure 1). Indeed, it is highly likely that Monsanto purposively selected the eligible counties to focus impact on those areas accounting for a high volume of their Bt sales. We see that the SLL-eligible counties appear to have much larger seed purchases in SLL-eligible counties reported in table 1. On average, growers in SLL-eligible counties purchased over four times the volume of seed relative to the non-SLL counties (3.27/ 0.73=4.5) in 2013 before implementation of the SLL program, a difference that is statistically significant. Examining these data graphically in figure 3, we first note that seed sales volumes are highly right-skewed. Furthermore, examining the NASS data, SLL counties' higher corn yields are statistically significantly different from state averages (table 1). However, year-to-year changes in yields appear similar between SLL and non-SLL counties (figure 4); further analysis discussed below suggests no statistically significant difference in annual yield changes between these two groups. One also might be concerned that the SLL program increased sales in the eligible counties, but I find no evidence for this. Mean growth in sales was of a similar magnitude in SLL and non-SLL counties, and not statistically significant.

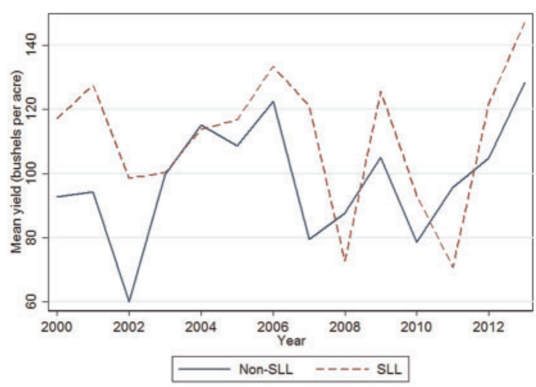

Figure 4. North Carolina corn grain yields, by SLL and non-SLL counties (2000-2013)

Source: National Agricultural Statistics Service, County Agricultural Production Survey data, downloaded from https://quickstats.nass.usda.gov.

Econometric Methods

To assess the impact of the SLL program on the target outcomes, I focus on estimating the average treatment effect (ATE) of the program in terms of changes in refuge adoption (both in fraction of seed purchased and whether a grower purchased any refuge seed) and imputed compliance between 2013 and 2016 in SLL-eligible and ineligible counties. For refuge adoption levels, I examine the ATE with respect to both the average grower and average hectare. The former focuses on measuring the effect on grower behavior. The latter focuses on measuring the effect on overall refuge size. Both outcomes are important for assessing impact. Overall refuge size is the key indicator considered in the biological modeling used to justify refuge mandates (EPA 2001), but changing grower behavior is a necessary precursor for achieving target refuge sizes. Additionally, I investigate whether the program caused any change in growers adopting *any* refuge, an intermediate outcome of interest to the EPA (Smith and Smith 2013; Martinez 2014). Compliance is the key regulatory outcome determining penalties imposed on growers by Bt seed producers (Hurley and Mitchell 2014; Bourguet, Desquilbet, and Lemarié 2005). Refuge portions of seed purchases are a fractional outcome, while the adoption of any refuge and compliance are both binary.

Denote $y^{\tau}_{i,c,t}$ as any one of these outcome variables for grower i, county c, year t, and treatment status τ (with $\tau = 1$ for SLL and $\tau = 0$ for non-SLL). For exposition, I refer in this section to the year index as either pre-treatment ($t = 0$) or post-treatment ($t = 1$). The data contain only one year of pre-treatment outcomes, but three years of post-treatment outcomes. The following discussion generalizes to ATEs for each of the post-treatment years, which I report in the results. To evaluate the impact of SLL on grower behavior, the ATE for outcome y is:

$$\begin{aligned}\alpha_y &:= \mathbb{E}\left(y^1_{i,c,1}\right) - \mathbb{E}\left(y^0_{i,c,1}\right)\\ &= \left[\mathbb{E}\left(y^1_{i,c,1}|c\in\mathcal{T}\right) - \mathbb{E}\left(y^0_{i,c,1}|c\in\mathcal{T}\right)\right]Pr(c\in\mathcal{T})\\ &\quad+\left[\mathbb{E}\left(y^1_{i,c,1}|c\notin\mathcal{T}\right) - \mathbb{E}\left(y^0_{i,c,1}|c\notin\mathcal{T}\right)\right]Pr(c\notin\mathcal{T})\end{aligned} \tag{1}$$

where $\mathcal{T}$ is the set of counties in the SLL pilot (making clear here that treatment is at the county level). The second line of this expression highlights the role of the counterfactual: while it is easy to compute sample analogs of $\mathbb{E}\left(y^1_{i,c,1}|c\in\mathcal{T}\right)$ and $\mathbb{E}\left(y^0_{i,c,1}|c\notin\mathcal{T}\right)$, the counterfactual conditional expectations $\mathbb{E}\left(y^0_{i,c,1}|c\in\mathcal{T}\right)$ (expected outcome of the treated in the absence of treatment) or $\mathbb{E}\left(y^1_{i,c,1}|c\notin\mathcal{T}\right)$ (expected outcome of the untreated with treatment) must be inferred.

I employ a variety of methods to estimate the counterfactual means, including DID, discrete outcome "changes in changes" (DCIC, Athey, and Imbens 2006), logit-transformed DID, and nearest neighbor (NN) matched differences (e.g., Girma and Gorg 2007; Gebel and Voßemer 2014). These methods involve different assumptions about treatment selection on observables and unobservables, and about the functional form of the econometric error distribution. Denote $\check{y}^0_1$ and $\check{y}^1_0$ as estimates of the counterfactuals $\mathbb{E}\left(y^0_{i,c,1}|c \in \mathcal{T}\right)$ and $\mathbb{E}\left(y^1_{i,c,1}|c \notin \mathcal{T}\right)$, respectively. I analyze the following estimators for $\check{y}^0_1$ (with symmetric definitions for $\check{y}^1_0$):

$$\text{DID:}\quad \check{y}^0_1 := \bar{y}_{10} + (\bar{y}_{01} - \bar{y}_{00})$$

$$\text{DCIC:}\quad \check{y}^0_1 := \begin{cases} \left(\dfrac{\bar{y}_{01}}{\bar{y}_{00}}\right)\bar{y}_{10}, & if\ \bar{y}_{01} < \bar{y}_{00} \\ 1 - \left(\dfrac{1-\bar{y}_{01}}{1-\bar{y}_{00}}\right)(1-\bar{y}_{10}), & if\ \bar{y}_{01} \geq \bar{y}_{00}\end{cases} \tag{2}$$

$$\text{Logit-DID:}\quad \check{y}^0_1 := \left[1 + \left(\frac{\bar{y}^{-1}_{01} - 1}{\bar{y}^{-1}_{00} - 1}\right)\left(\bar{y}^{-1}_{10} - 1\right)\right]^{-1}$$

$$\text{Matching-DID:}\quad \check{y}^0_1 := \bar{y}_{10} + \textstyle\sum_{c\in C_0} W_c \Delta\tilde{y}_c \quad \text{with} \quad \textstyle\sum_{c\in C_0} W_c = 1$$

where $\bar{y}_{\tau,t}$ are the subsample mean outcomes for treatment group τ in time period t, and where $\Delta\tilde{y}_c$ (in the Matching-DID estimator) is the mean change in outcome for county c between the pre- and post-treatment periods. Because all of the outcomes I analyze are binary or fractional, and because of the clear potential for selection of treatment on both observables and unobservables, none of the above estimators are ideal, but each addresses different econometric concerns. I thus describe and report estimates for each of these estimators, and seek patterns across them.

As (2) shows, the basic DID, DCIC, and Logit-DID estimators can be computed purely from subsample mean outcomes. Identification is achieved in the DID case by assuming parallel changes over time in the pre-treatment outcomes in the treatment and control groups. With binary or fractional outcomes as used here, DID has the additional disadvantage of permitting counterfactuals below zero and greater than one. The DCIC model, in the case of binary outcomes, solves this problem by instead using the proportional change in the control group to impute what would have happened over time to the treatment group in the absence of treatment (and vice versa for the control group). Both methods address possible treatment selection on unobserved, time-invariant heterogeneity, but rather than a parallel time trend assumption of DID, DCIC assumes a proportional change over time in expectation between the two groups (Athey and Imbens 2006).[3] Similarly, the Logit-DID model restricts the range of the counterfactual outcome by assuming a parallel time trend in the logit transformation of the subsample outcome means.

While I only have one period of pre-treatment outcome data, this general common change assumption appears consistent with data from NASS showing parallel trends in corn yields between treatment and control counties in pre-treatment years (figure 4). Statistically, I find no significant difference between SLL and non-SLL counties in year-to-year yield changes, with an OLS regression of $(Yield_{ct} - Yield_{ct-1})$ on a SLL county dummy variable yielding a p-value of 68% (in contrast to a regression of yield levels on the SLL county dummy, which produces a p-value of less than 0.1%). Unfortunately, NASS does not report other relevant factors (e.g., transgenic corn adoption) at the county-level with enough temporal frequency to further investigate this common change assumption.

For estimating standard errors and robustness checks, it is helpful to write these estimators relative to the canonical linear DID regression:

$$(3) \quad y_{i,c,t} = \hat{\beta}_0 + \hat{\beta}_{treat}\mathbb{I}[c \in \mathcal{T}] + \hat{\beta}_{post}\mathbb{I}[t=1] + \hat{\alpha} \cdot \mathbb{I}[c \in \mathcal{T}] \cdot \mathbb{I}[t=1] + \hat{\epsilon}_{i,c,t}.$$

Here, the $\hat{\beta}$'s and $\hat{\alpha}$ are OLS coefficient estimates and $\hat{\epsilon}_{i,c,t}$ are the estimated residuals. As is well-known, this saturated regression simply estimates the pre- and post- treatment and control subsample means: $\bar{y}_{00} = \hat{\beta}_0$, $\bar{y}_{01} = \hat{\beta}_0 + \hat{\beta}_{post}$, $\bar{y}_{10} = \hat{\beta}_0 + \hat{\beta}_{treat}$, and $\bar{y}_{11} = \hat{\beta}_0 + \hat{\beta}_{treat} + \hat{\beta}_{post} + \hat{\alpha}$. Thus, from equation (2), the DID estimate of the ATE is simply $\hat{\alpha}$. The DCIC mean estimate of the ATE can also be derived directly from this regression as a nonlinear combination of the estimated coefficients, along with its standard error computed via the Delta method.[4] I estimate equation (3) to obtain the DID and DCIC estimators, and test robustness by estimating specifications disaggregating $\hat{\beta}_0$ and $\hat{\beta}_{treat}$ into grower fixed effects $\hat{\beta}_i$ and by including covariates in the regression: logs of historical mean county-level yield (from NASS) and 2013 sales volume (from Monsanto). Both covariates are time-invariant and collinear with individual fixed effects and so are not included in any fixed effects regressions. To address potential violations of the "stable unit treatment value assumption" (SUTVA) in causal inference (Rubin 1986), I also examine whether spillovers between neighboring treated and untreated counties may have occurred by estimating a linear DID regression dropping any observations in untreated counties that neighbor treated counties.

The Logit-DID estimator models the predicted outcome as $\hat{y} = (1 + \exp - \hat{\beta}x)^{-1}$, where $\hat{\beta}x$ denotes the same saturated linear form on the left-hand-side of equation (3). As with the linear DID, logit predictions for observed subgroups in this saturated regression equal the subgroup means. While the Logit-DID estimate of the counterfactual is based on differences in the logit-transformed mean outcomes, $-log(\bar{y}^{-1} - 1)$, the regression coefficient $\hat{\alpha}$ on the interaction term is no longer a direct estimate of the ATE in equation (1). Rather, a nonlinear combination of the logit regression coefficients generates the counterfactual in equation (2) in units of y, which keeps this ATE estimate comparable to the other methods employed.[5] As in the linear

[3] See the supplementary material online for a more in-depth discussion of DCIC validity and identifying assumptions when analyzing a fractional outcome.

[4] For the assumptions of the delta method to hold (specifically, a defined and nonzero gradient at the transformed point estimate), it is sufficient that all susbsample means appearing in the nonlinear expressions in equation (2) are nonzero, which is the case in the data. The *nlcom* command in the Stata software package was used to compute standard errors for nonlinear treatment effects.

[5] The interaction coefficient $\hat{\alpha}$ is an estimate of the ATE in the space of logit-transformed mean outcomes. In principle, the Logit-DID counterfactual can also be computed purely from a nonlinear combination subsample means as shown in equation (2), like the DCIC model, and hence from the linear DID regression in equation (3). But executing a separate maximum likelihood logit regression is more flexible (e.g. allowing the inclusion of covariates).

DID regressions, I test robustness in the Logit-DID model through inclusion of the same covariates as in the linear DID regression.

Lastly, the Matching-DID estimator acknowledges that the "average change" (however defined) in the control group between pre- and post-treatment periods may not accurately reflect the change that would have occurred to the treatment group in the absence of treatment. In our application, the most obvious reason for such an inaccuracy would be that Monsanto clearly targeted the SLL program at the more intensive, higher-yielding corn growing regions of NC, which may have fundamentally different dynamics than lower-yielding areas of the state (though the similar pre-treatment trends in corn yields shown in figure 4 mitigates this concern somewhat; we return to this point below). To address possible selection effects, matching methods use observable variables to construct a counterfactual. These methods' well-known identifying assumption is that selection into treatment is random once conditioned on these observables (Abadie et al. 2004; Abadie and Imbens 2006; Abadie and Imbens 2009). However, basic matching methods of the outcome do not directly address selection on unobservables, as do the DID (linear and logit) and DCIC estimators (assuming time-invariance of the unobservable factors).

Following other studies (Girma and Gorg 2007; Gebel and Voßemer 2014), I therefore use matching methods to predict the counterfactual difference in outcomes between pre- and post-treatment periods. Because Monsanto determined SLL program eligibility at the county-level, I match county-level observables using the same two covariates in the regression analysis. For a grower i in county c in the set of treatment counties, with observed change in outcome Δy_{ic}, the matched estimate of the counterfactual difference is

$$\Delta \check{y}_{i,c} := \sum_{d \in C_0} w(\bar{x}_c, \bar{x}_d) \Delta \widetilde{y}_d \text{ such that } c \in C_1 \text{ and } \sum_{d \in C_0} w(\bar{x}_c, \bar{x}_d) = 1 \quad (4)$$

where $\bar{x}_c$ is the vector of county-level matching variables for county c, C_1 is the set of SLL-eligible (treatment) counties, C_0 is the set of ineligible control counties, and $\Delta \widetilde{y}_c$ is the observed average change in outcome for county c. The weighting function $w(\bar{x}_c, \bar{x}_d)$ dictates the similarity between county c and d based on observables $\bar{x}_c$ and $\bar{x}_d$. The aggregate weights W_c in equation (2) used to estimate the ATE are therefore $W_c = \frac{1}{|C_1|} \sum_{d \in C_1} w(\bar{x}_d, \bar{x}_c)$ for $c \in C_0$. Because I have multiple continuous matching variables, I specify $w(\cdot)$ alternately using NN matching with Mahalanobolis and propensity-score matching (PSM) distance metrics (Zhao 2004; Abadie and Imbens 2011). I only report the NN estimates in the paper for concision; table S2 in the online supplement reports results from all matching methods. To ensure validity of the overlap assumption for matching estimators, I employed a data trimming procedure to ensure overlap in the logit-estimated propensity score between treated and control groups (Smith and Todd 2005). This results in an estimation sample of 383 growers across 79 counties (see supplement).[6] A remaining disadvantage of this Matching-DID estimator, and a reason I do not focus on it exclusively, is that like the linear DID estimator, the modeled counterfactual may fall below zero or exceed one (as can be seen in equation 2).[7]

Results

Table 3 reports ATE estimates for 2014–2016 for each of the methods listed in equation (2). Tables 4–6 report the regression estimates used to derive these ATEs. Table 7 shows estimates from a logit treatment selection regression establishing that the included explanatory variables are good candidates for use in matching.[8] Figure 5 shows a map of treatment propensity scores, as well as those counties that were trimmed in matching

[6] Calipers were also used in robustness testing, which resulted in excluding 33 observations from the matching estimators. Results did not change appreciably, mainly producing wider confidence intervals (though still statistically significant).

[7] Matching cannot be combined here with the DCIC and Logit-DID models because the county-level mean outcomes often take values of zero or one, leading to infinite or undefined proportional or logit-transformed changes. While my analysis suggests value from future research on approaches to combining matching with nonlinear changes for discrete or fractional outcomes, that effort lies beyond the scope of the present analysis.

[8] This logit regression is only for checking the power of the matching variables at predicting treatment. The matching results presented in table 2 are based on NN, not PSM. As noted above, a PSM model was also estimated using the logit regression in table 2 to compute propensity scores; results are in the supplemental material online and are qualitatively the same.

Table 3. Estimated Average Treatment Effects

Year	DID	DCIC	Logit-DID	Matching-DID
Fraction planted to Bt: see regression table 4				
2014	−0.0288**	−0.0264**	−0.0323*	−0.0577***
	(0.0138)	(0.0126)	(0.0168)	(0.0201)
2015	−0.0204	−0.0124	−0.0130	−0.00794
	(0.0178)	(0.0229)	(0.0224)	(0.0143)
2016	−0.0247	−0.00890	−0.00993	−0.00870
	(0.0156)	(0.0173)	(0.0172)	(0.0139)
Fraction planted to Bt (volume weighted)				
2014	−0.0103	−0.00999	−0.0102	
	(0.00828)	(0.00794)	(0.00839)	
2015	−0.0126	−0.0116	−0.0117	
	(0.0109)	(0.0110)	(0.0110)	
2016	−0.00579	−0.00466	−0.00472	
	(0.0121)	(0.0113)	(0.0113)	
Probability of planting any refuge: see regression table 5				
2014	0.123**	0.126**	0.123**	0.404***
	(0.0579)	(0.0613)	(0.0601)	(0.121)
2015	0.0717	0.0810	0.0675	−0.0624
	(0.0841)	(0.0739)	(0.0894)	(0.105)
2016	0.0619	0.0718	0.0577	0.0659
	(0.0656)	(0.0569)	(0.0705)	(0.0734)
Imputed refuge compliance: see regression table 6				
2014	0.0347	0.0297	0.0564	−0.00681
	(0.0362)	(0.0313)	(0.104)	(0.0395)
2015	0.0792**	0.0669**	0.0905	0.0324
	(0.0369)	(0.0311)	(0.102)	(0.0389)
2016	0.0480	−0.0284	−0.0246	0.0218
	(0.0306)	(0.0501)	(0.0523)	(0.0365)

Note: Standard errors appear in parentheses. Asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels. DID, DCIC and Logit-DID estimates derived from regression models in tables 3 and 4 (baseline DID specifications). ATE standard errors in DCIC and Logit-DID estimated using the Delta method. Matching estimator uses nearest neighbor matching with at least one requested neighbor, Mahalanobis distance metric, bias adjustment method of Abadie and Imbens (2011), robust standard errors (Abadie and Imbens 2006), and same matching covariates as used in tables 4 to 6 regressions; see treatment selection regression in table 7. See figures S1 to S3 and tables S2 and S3 in the supplemental material for additional matching estimator details.

estimation due to nonoverlap. In the supplement, figures S1 and S2 contain quantile-quantile (qq) plots of the treatment versus control raw and matched samples. Figure S3 plots a qq plot of one of the key outcomes: the 2013–2014 difference in Bt seed shares. Tables S2–S3 provide additional diagnostics on the matching procedures.

I describe results for each outcome variable analyzed. In general, I adopt a conservative approach of claiming a finding only when an ATE estimate is significant and qualitatively concordant across each of the estimators presented in equation (2) and table 3, and when the regression results underlying these ATE estimates (tables 4–6) are robust to changes in specification. When I do find a significant and robust ATE estimate, I generally prefer the DCIC model because it can handle the binary or fractional outcomes and because in the binary case it is nonparametric, unlike the Logit-DID. As table 3 shows, the SLL program appears to have reduced the share of Bt corn planting for the average grower by between 2.6% (from DCIC) and 5.8% (from Matching-DID) in 2014. The estimated 2014 ATE is statistically significant across all models (though only at the 10% level in the case of the Logit-DID estimator). As table 4 shows, the DID estimate is robust to inclusion of covariates and grower-fixed effects. This table also shows that violations of SUTVA do not seem to be a concern. Removing observations in untreated counties neighboring treated counties does not appreciably change the ATE estimate (OLS column 4 of table 4). The fractional logit regression in table 4 also shows that, while the Logit-DID estimate in table 3 is only statistically significant at the 10% level, the interaction term between the SLL county and 2014 year

Table 4. Regression Estimates, Fraction of Bt Planted

Regression Model:	OLS				Fractional logit	
	(1)	(2)	(3)	(4)	(1)	(2)
SLL county	0.0493***	0.0316**			0.0403***	0.0278***
	(0.0124)	(0.0127)			(0.00950)	(0.0105)
Year						
2014	0.0206*	0.0179	0.0206*	0.0172	0.0190*	0.0165
	(0.0114)	(0.0111)	(0.0114)	(0.0130)	(0.0106)	(0.0102)
2015	0.0271**	0.0245*	0.0278**	0.0264*	0.0251**	0.0224*
	(0.0134)	(0.0124)	(0.0130)	(0.0148)	(0.0124)	(0.0115)
2016	0.0402***	0.0417***	0.0337***	0.0321**	0.0371***	0.0385***
	(0.0119)	(0.0116)	(0.0113)	(0.0133)	(0.0110)	(0.0106)
SLL county x Year						
x 2014	−0.0288**	−0.0262*	−0.0288**	−0.0254*	−0.0315*	−0.0282*
	(0.0138)	(0.0136)	(0.0138)	(0.0152)	(0.0175)	(0.0166)
x 2015	−0.0204	−0.0198	−0.0214	−0.0200	−0.0138	−0.0147
	(0.0178)	(0.0172)	(0.0175)	(0.0188)	(0.0241)	(0.0233)
x 2016	−0.0247	−0.0268*	−0.0207	−0.0191	−0.0125	−0.0171
	(0.0156)	(0.0156)	(0.0152)	(0.0167)	(0.0223)	(0.0238)
Log(1+Mean yield)		0.0152				0.00959*
		(0.0109)				(0.00571)
Log(2013 seed sales)		0.00638*				0.00628*
		(0.00372)				(0.00344)
Observations	1,761	1,645	1,761	1,523	1,761	1,645
Growers	493	423	493	428	493	423
Grower fixed effects	No	No	Yes	Yes	No	No
SLL buffer	No	No	No	Yes	No	No
(Pseudo-)R^2	0.014	0.0310	0.010	0.009	0.01	0.0184

Note: Standard errors appear in parentheses. Asterisks *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. OLS standard errors are clustered at the county level. R^2 in fixed effect regressions is calculated relative to a null model including only fixed effects. Marginal effects and pseudo-R^2 reported for fractional logit models. Redacted cells pertain to proprietary information.

dummies—interpretable as a logit-transformed DID estimate of the ATE—is more precisely estimated, and is also robust to the inclusion of covariates.

By 2015 and 2016, however, the effect of the SLL program on Bt planting appears to dissipate. Across all models, the ATE estimates for these years shrink relative to the 2014 ATE; they are all also statistically insignificant. In the two models accounting for the fractional nature of the outcome variable, the effect on Bt planting is halved in 2015 relative to 2014 and declines to effectively zero in 2016 (though it is still negative).

The effect of the SLL program on imputed refuge area (rather than average grower behavior) in table 3 appears negligible. These models weight each grower-level observation by that grower's total 2013 sales volume as a proxy for planted area. While all of the weighted ATE estimates are negative, they are generally smaller in magnitude than their unweighted analogues, and none are statistically significant. This lack of precision could simply be induced by the highly skewed weights (see figure 3, e.g., 10% of growers account for over half of the 2013 sales volume in the sample).

However, the smaller magnitude ATEs in the weighted estimates, across all models, may suggest that larger growers responded relatively less to the SLL program. To investigate this possibility, I estimate a linear DID regression with full, three-way interactions between Year × Treatment county × log(2013 sales). The regression results are presented in table S1 of the online supplement; general results are similar to the base DID specification (with an obvious loss of statistical precision due to the additional degrees of freedom). Figure 5 plots the key results from this regression, that is, estimated treatment effects across different grower sizes (only 2014 treatment effects on the fraction of Bt seed purchased are plotted, as this is the only year and outcome for which there appeared to be a significant ATE). Indeed, these results suggest that larger growers

Table 5. Regression Estimates, Probability of Planting Any Refuge

Regression model:	OLS (linear probability model)			Logit	
	(1)	(2)	(3)	(1)	(2)
SLL county	0.0461	−0.124*		0.0489	−0.123*
	(0.0758)	(0.0668)		(0.0818)	(0.0675)
Year					
2014	−0.112***	−0.105***	−0.112***	−0.110***	−0.103***
	(0.0312)	(0.0313)	(0.0312)	(0.0304)	(0.0302)
2015	−0.189***	−0.170***	−0.182***	−0.186***	−0.167***
	(0.0362)	(0.0375)	(0.0374)	(0.0356)	(0.0357)
2016	−0.190***	−0.172***	−0.165***	−0.187***	−0.170***
	(0.0314)	(0.0325)	(0.0330)	(0.0309)	(0.0316)
SLL county x Year					
x 2014	0.123**	0.116**	0.123**	0.121**	0.115**
	(0.0579)	(0.0580)	(0.0579)	(0.0589)	(0.0581)
x 2015	0.0717	0.0755	0.0768	0.0656	0.0709
	(0.0841)	(0.0837)	(0.0812)	(0.0857)	(0.0829)
x 2016	0.0619	0.0539	0.0558	0.0562	0.0524
	(0.0656)	(0.0632)	(0.0659)	(0.0679)	(0.0644)
Log(1+Mean yield)		−0.0457			−0.0459*
		(0.0283)			(0.0278)
Log(2013 seed sales)		0.128***			0.127***
		(0.00994)			(0.0100)
Observations	1,761	1,645	1,761	1,761	1,645
Growers	493	423	493	493	423
Grower fixed effects	No	No	Yes	No	No
(Pseudo)-R^2	0.0310	0.163	0.0401	0.0678	0.128

Note: Standard errors appear in parentheses. Asterisks *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. OLS standard errors are clustered at the county level. R^2 in fixed effect regressions is calculated relative to a null model including only fixed effects. Marginal effects and pseudo-R^2 are reported for logit models.

appeared less responsive to the program, with growers in the 10th percentile of 2013 sales volume effectively responding with an estimated 7% increase in refuge planting and growers in the 90th percentile not responding to the program at all.

Table 3 also presents estimated ATEs with respect to the probability of planting any refuge and grower compliance with refuge mandates. The effect of the SLL program on growers planting any refuge qualitatively parallels the ATE estimate for refuge fraction, but appears more pronounced. The 2014 ATE for this outcome is around 12% for the regression-based estimates, and climbs to a striking 40% in the matching model. Regarding compliance, the SLL program appears to have had ambiguous effects. While nearly all of the econometric models in table 3 (save the Matching-DID estimator) produce positive ATE estimates in 2014 and 2015, only the DID and DCIC estimates in 2015 are statistically significant (and are also robust to the inclusion of covariates; see table 4).[9] Nevertheless, the lack of consistency between these results and the Logit-DID and matching models argues against claiming any estimated effect on compliance as a key finding.

By 2016, measureable effects of the SLL program dissipate for all of the outcomes analyzed. Yet it is econometrically instructive to study the differences in ATE estimates in table 3 across models for this year. In general, we would expect the linear DID estimates to diverge from the DCIC and Logit-DID models when either the zero lower or unit upper bound becomes a relevant constraint on the counterfactual outcome. While I cannot precisely speak to this constraint with respect to the Bt fraction outcome (due to the confidential nature of the raw sales data), the constraint poses an issue in 2016, to some degree

[9] As tables 4 to 6 indicate, the regressions with covariates are estimated on a restricted sample (86% of growers) in the full data due to missing data in the controls. At the request of a reviewer, we also estimated OLS specifications (1) and (3) on this restricted sample to test robustness; results were qualitatively and quantitatively very similar.

Table 6. Regression Estimates, Imputed Refuge Compliance

Regression model:	OLS (linear probability model)			Logit	
	(1)	(2)	(3)	(1)	(2)
SLL county	−0.121***	−0.0796***		−0.0946***	−0.0720**
	(0.0266)	(0.0273)		(0.0266)	(0.0284)
Year					
2014	−0.0237	−0.0211	−0.0237	−0.0204	−0.0179
	(0.0227)	(0.0230)	(0.0227)	(0.0195)	(0.0194)
2015	−0.0696***	−0.0691***	−0.0752***	−0.0597***	−0.0585***
	(0.0240)	(0.0236)	(0.0237)	(0.0207)	(0.0202)
2016	−0.0703***	−0.0792***	−0.0644***	−0.0603***	−0.0674***
	(0.0207)	(0.0204)	(0.0205)	(0.0178)	(0.0174)
SLL county x Year					
x 2014	0.0347	0.0321	0.0347	0.0493	0.0443
	(0.0362)	(0.0364)	(0.0362)	(0.0920)	(0.0848)
x 2015	0.0792**	0.0809**	0.0858**	0.111	0.114
	(0.0369)	(0.0369)	(0.0368)	(0.123)	(0.117)
x 2016	0.0480	0.0590*	0.0499*	−0.0321	−0.0151
	(0.0306)	(0.0303)	(0.0288)	(0.0684)	(0.0791)
Log(1+Mean yield)		−0.0181			−0.00920
		(0.0153)			(0.00732)
Log(2013 seed sales)		−0.0211***			−0.0196***
		(0.00642)			(0.00568)
Observations	1,761	1,645	1,761	1,761	1,645
Growers	493	423	493	493	423
Grower fixed effects	No	No	Yes	No	No
(Pseudo)-R^2	0.0210	0.0410	0.0140	0.0678	0.137

Note: Standard errors appear in parentheses. Asterisks *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. OLS standard errors are clustered at the county level. R^2 in fixed effect regressions is calculated relative to a null model including only fixed effects. Marginal effects and pseudo-R^2 are reported for logit models.

Table 7. Marginal Effects of Observables on Treatment Selection

Model: logit	SLL-eligibility
Log(Mean yield)	0.0904*
	(0.0519)
County-level mean log(2013 sales)	0.178***
	(0.0483)
Observations	463
Counties	86
Degrees of freedom	2
Wald χ^2-test statistic	16.8***
Pseudo-R2	0.347

Note: Robust standard errors appear in parentheses, clustered at the county level. Asterisks ***,**, and * denote statistical significance at the 10%, 5%, and 1% levels, respectively.

in 2015, but not in 2014. Table 2 shows precisely how these constraints bind for refuge compliance. As noted in the previous section, very low 2013 imputed refuge compliance in the SLL counties confounds the linear DID estimator and motivates use of the DCIC and Logit-DID estimators. When the fractional/discrete constraint binds, these models tend to produce more conservative ATE estimates, smaller in magnitude and less statistically precise than the linear DID model. Yet when the constraints do not bind, the DCIC and Logit-DID models agree relatively well with the linear DID. The DCIC estimator in particular appears to retain more statistical precision from the linear DID model than the Logit-DID estimator. This may arise from the fact that, while both the DCIC and Logit-DID estimators can be viewed as nonlinear transformations of the subgroup means, as shown in equation (2), the DCIC transformation involves fewer nonlinear operations on subsample means than logit (and in this case is computed directly from the same OLS regression as the linear DID estimator). DCIC is also technically a nonparametric estimator in the case of discrete outcomes (Athey and Imbens 2006), whereas logit makes a (possibly mis-specified) functional form assumption on the distribution of the outcome variable.

The matching estimator, while it still relies on linear rather than proportional or

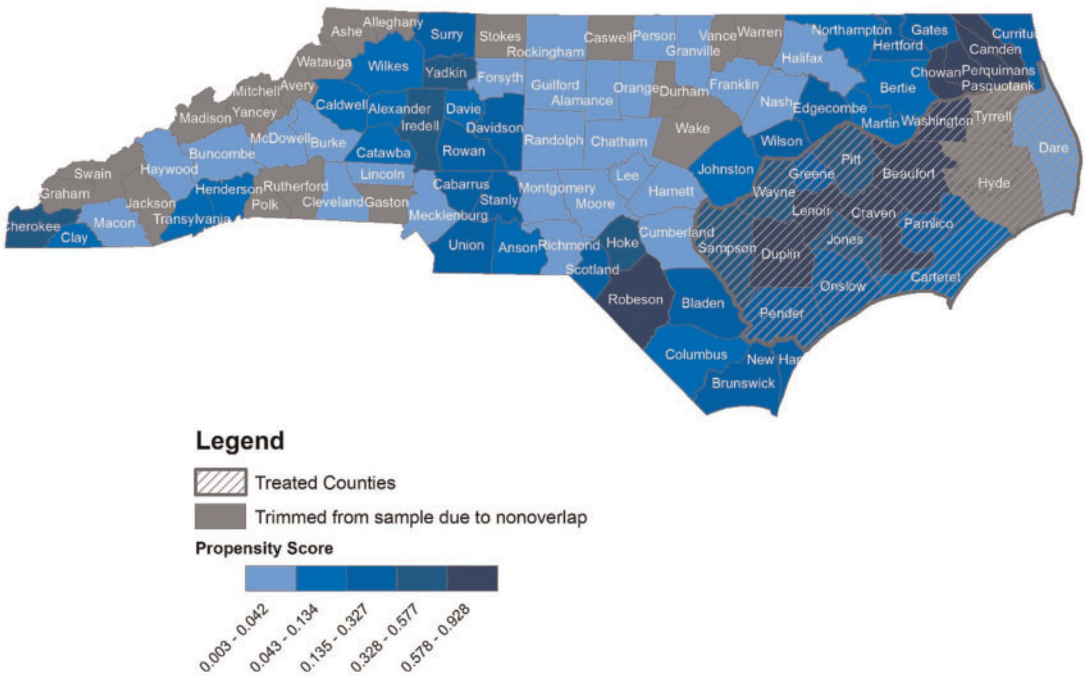

Figure 5. Map of county-level propensity scores for inclusion in SLL program

Note: Propensity scores are predicted values from logit regression in table 7.

logit-transformed differences, also appears to provide more conservative ATE estimates in cases where the counterfactual outcome is constrained. When there is a statistically significant effect in both the regression and matching models, the matching ATE estimates are always larger in magnitude than the regression-based estimates (bearing in mind the differences in estimation samples due to propensity score trimming procedure used in the matching estimators). Both matching and regression-based inclusion of covariates provide consistent ways of addressing potential treatment selection on observables. The logit regression in table 7 confirms that the OLS controls are also effective as matching variables predicting treatment selection. And figure 5 confirms visually that there is sufficient balance in treatment propensity between treated and untreated counties based on the observed treatment predictors.

However, OLS with covariates and matching each may be biased in different directions under certain conditions (Zhao 2004). Angrist and Pischke (2009) argue that differences between regression and matching can reflect underlying heterogeneity in treatment effects, corroborating the findings discussed above. The binary outcomes produce the largest discrepancies between the regression and matching estimates. In the case of probability of planting any refuge, the Matching-DID estimate for 2014 is over 230% that of the other models, and for compliance the matching estimates are quite different from other estimates for this outcome in all years. These large differences are somewhat perplexing, but possibly related to using a linear difference of strictly binary variables (refuge planting probability and compliance) as outcomes in the matching procedure, since the only possible values for such outcomes are -1, 0, or +1. For this reason, my preferred models for the binary outcomes are those based on regressions including covariates, and those which account for the discrete/fractional nature of outcomes (OLS columns 2 and 3 and Logit column 2 of tables 5 and 6). However, as with the base DCIC and Logit-DID models in table 6, the results for compliance are not even qualitatively robust: while the *SLLx2015* interaction term in OLS with covariates or fixed effects are significant and similar in magnitude to the base OLS regression, none of the logit regressions show measurable evidence that the SLL program impacted refuge compliance. These inconsistent effects on compliance may arise from issues with the proxy. For example, the 20% of non-Bt seed sales determining compliance does not allow for possibly different sowing densities between refuge and non-refuge corn (discussed below), and there may be measurement error induced by the cutoff. However, it deserves mentioning that when I modify the compliance cutoff by ±5% in econometric analysis as a robustness check, regression results still agree with table 6.

Discussion

Previous economic analysis of instruments for improving IRM have mostly been theoretical or based on simulation models. Moreover, most of this work has focused on the sort of standard, pecuniary instruments typically prescribed for common-pool resource management. This paper is the first to evaluate the effect of a behavioral nudge on farmers' IRM practices. In this case, the nudge aimed to motivate farmers focused on appeals to local sustainability and protecting future generations by using community-based advertising, social comparisons, and by offering indirect monetary rewards to local charities. In contrast to standard pecuniary instruments, this type of approach is more in keeping with the literature on alternative governance of commons using social norms and cooperation (Ostrom 1990).

My analysis provides robust evidence that the intervention had a significantly positive initial effect, increasing refuge planting (as proxied through seed purchases) for the average grower by 2.6% for the preferred DCIC model in the first year following the program. To put the magnitude of this effect into economic terms, consider the following back-of-the-envelope calculation: previous research suggests (in the absence of widespread resistance) the effect of planting Bt corn on a given hectare in the United States could be expected to increase yield by potentially 20% relative to planting currently available non-Bt corn varieties (Hutchison et al. 2010; Fernandez-Cornejo and Wechsler 2012). NASEM (2016) discusses a host of provisos for such an assumption, for example, that Bt fields may yield spillover pest suppression benefits on nearby fields, or as Shi et al. (2013) find, that Bt seeds bundle the pesticidal trait with inherently higher quality germplasm. Further, previous research has also shown that (e.g., Fernandez-Cornejo and Wechsler 2012), Bt adoption significantly increases variable profits, and that the majority of these profit increases are attributable—and approximately proportional—to yield increases. Based on these assumptions, my preferred ATE estimate suggests that the SLL program would cause the average grower to forego 0.52% of their current profits due to increased refuge planting. If the assumed yield improvement is halved (to 10%) then foregone contemporaneous profit from the SLL program would also be halved (to 0.26%).

The program's impact on refuge planting roughly halves and becomes statistically insignificant in 2015, and almost completely fades away by 2016. This significant initial effect of the social marketing program followed by fadeout after cessation agrees with the vast majority of econometric studies on social nudges, as observed by Brandon et al. (2017). In the few cases where the effects of nudges persist, such as the well-known social comparison and energy conservation experiments with the company Opower (Allcott and Rogers 2014), complementary analysis by Brandon et al. (2017) suggests that any persistent impact derives from changes in agents' investment in physical capital induced by the nudge (energy-efficient appliances, in the Opower case). Applying this logic to the case of Bt refuges and lessons for future social marketing programs, persistent effects may be achieved by inducing growers to adopt technology that reduces variable costs of planting refuge. This could include adoption of multi-hybrid corn planters, which would reduce growers' variable costs of planting multiple corn varieties with different soil and sowing density optima (Hest 2015), as well as greater use of "precision ag" services, for example in calculating efficient refuge configurations (Hopkins 2011).

While my analysis identifies an effect of the program on the average grower, I find much smaller and statistically insignificant effects of the intervention on overall planted refuge area, due evidently to larger growers not measurably responding to the program. Follow-up analysis (figure 6 and table S1) suggests that larger growers indeed responded more weakly to the SLL program. This finding is consistent with a dynamic story in which such growers have become large

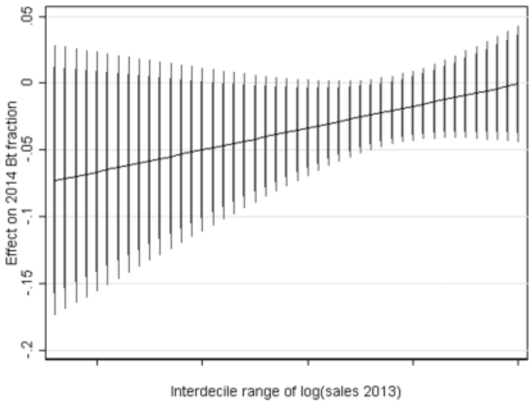

Figure 6. Heterogeneous treatment effect estimates for 2014 Bt fraction

Note: Corresponds to regression estimates from table S1: $\hat{\beta}_{\mathrm{SLL}\times 2014} + \hat{\beta}_{\mathrm{SLL}\times 2014\times \log(\mathrm{sales})} \times \log(2013\ \mathrm{sales})$, plotted over $\log(2013\ \mathrm{sales})$. Thick vertical lines are 90% confidence intervals; thin vertical lines are 95% confidence intervals.

because they are more economically competitive and may have higher managerial ability (Foltz 2004; Chavas, Chambers, and Pope 2010)—and possibly more sensitive to potential current period profit losses from planting refuge. On the other hand, large growers may derive more direct, private benefits from effective IRM than smaller growers, and hence one could also expect them to be *more* responsive to refuge promotion (Reisig 2017). In addition, this finding is at odds with research by Ferraro, Miranda, and Price (2011) on behavioral nudges for water conservation. These authors find high-water-use households responded relatively more to nudges than lower-use households. An additional possibility, noted by a reviewer, is that larger farms likely have more decision makers involved in their business operations than small farms. Exposure to a moral suasion intervention may therefore be more likely to sway the behavior of an individual (managing a small farm) than an entire group of managers on a larger farm. These hypotheses regarding the theoretical and empirical role of heterogeneity in the effects of nudges on IRM requires further research.

Whereas I find no consistent effect of the SLL program on my proxy for refuge compliance, I find a strong effect of the program on the probability of growers planting *any* refuge in the first growing season following the program. Taken together, these results show that the program's effects were concentrated among those growers who had not been planting any refuge, rather than prompting *almost* compliant growers to cross the threshold into compliance. Such a finding is consistent with the fact that none of Monsanto's SLL advertisements referred to the actual EPA regulations; the program's advertisements emphasized "carrots over sticks", seeking to promote more refuge planting and not invoking any threat of EPA enforcement actions with noncompliant growers. Even though extension agents and seed sellers regularly educate farmers about the details of refuge requirements (Reisig 2017), the exact requirements (both in terms of refuge size and structure) are rather complex, and growers face little financial incentive to comply exactly with refuge regulations given the low likelihood of an enforcement audit. Moreover, while the SLL program possibly led growers to plant more refuge out of an altruistic, prosocial, or enlightened self-interest motive, they may not have understood or believed there to be scientific basis for the exact refuge cutoffs used in regulation. This could explain why the program appeared to have no measurable effect on refuge compliance.

A number of limitations in the data for this study constrains inference about the effects of this voluntary instrument for IRM promotion. Given the primary objective of IRM to sustainably abate pest damage, biological outcomes like pest density and Bt susceptibility are of obvious interest. However, given the relatively small, short-lived observed change in refuge planting, combined with the relatively longer timescales in resistance evolution (Carrière, Crowder, and Tabashnik 2010), it is unlikely that I would have detected any effect of this program on contemporaneous Bt resistance in local pests with only the four years of data analyzed (the length of the panel, both pre- and post-intervention, being another limitation of this analysis). From a bioeconomic perspective, the findings of this study are more important in their suggestion that such nonpecuniary, moral suasion interventions may have a role to play, when scaled up and combined with other policies, in slowing the spread of resistance.

Even with respect to grower responses, a lack of data constrains inference. Even though seed sales has been increasingly acknowledged as an important indicator for informing refuge regulations (Smith and Smith 2013; Martinez 2014), the use of such data as a proxy for refuge planting raises questions about whether growers actually planted refuge (or Bt seed) they purchased. For example, growers may have purchased refuge seed merely to “buy” a vote in Monsanto’s charity donation. However, given the major contribution of seed costs to U.S. corn farmers’ expenditures (NASS 2016), coupled with the relatively small magnitude and indirect benefits of Monsanto’s donation, it seems unlikely that growers did not plant what they purchased (at least not in a systematic way). A more salient concern is that Bt seed is often sown in higher density than non-Bt hybrids (Lobell et al. 2014). However, it is easy to show mathematically that ATE estimates for the proportion of refuge seed purchased (assuming all seed is planted) is a lower bound for the ATE on the proportion of corn growing area planted to refuge, assuming Bt seed is sown at higher density.[10]

Another confounder relates to the SLL program itself, which as noted above involved social marketing efforts using moral suasion, social comparison, as well as an indirect reward in the form of the charity donation. Because the pilot of this program did not experimentally control these components, it is impossible to separately identify their effects—which is often the expressed aim of much behavioral economics research (e.g., Ferraro, Miranda, and Price 2011; Allcott 2011). However, as my analysis is the first to econometrically identify effects of such non-pecuniary interventions in IRM, this paper points to future research on which aspects of such interventions are most (cost-)effective.

Non-pecuniary behavioral interventions may offer additional, useful options for promoting both weed and insect resistance management, especially in situations where enforcement of well-defined property rights for pesticide susceptibility presents significant challenges (Barrett, Soteres, and Shaw 2016). Bt resistance and refuge policy comprise one such situation where enforcement is indirect and technical solutions such as RIB remain imperfect and unsuitable for some areas (like NC). Further investigation into behavioral tools should examine how the effectiveness of such interventions can be sustained, how they can be tailored to complement other approaches to resistance management, and how they can be used to achieve economically optimal outcomes for resistance management.

[10] Let α be the proportion of seed planted with Bt, $\tilde{\alpha}$ be the proportion of corn growing area grown with Bt, and $s > 1$ be the ratio of Bt to refuge seeding densities. Then $\tilde{\alpha} = \left[1 + \left(\alpha^{-1} - 1\right)s\right]^{-1} < \alpha$, from which it is easy to show that $\partial\tilde{\alpha}/\partial\alpha > 1$, meaning that a change in α produces an even larger change in $\tilde{\alpha}$.

Supplementary Material

Supplementary material are available at *American Journal of Agricultural Economics* online.

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