The Effects of Conservation Reserve Program Participation on Later Land Use

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Abstract

I use a treatment effect framework to investigate whether participation in the Conservation Reserve Program (CRP) changes a parcel's later land use. The CRP, which pays farmers to retire agricultural land, is the United States' largest conservation program. The program has several goals, including protecting the environment and improving the long-term productivity of farmland. I compare samples of CRP land to samples of non-CRP land to find the causal effect of CRP participation on land use outcome, using regression and matching techniques. By comparing land that exits the CRP to the best counterfactual land (land that faced similar transition costs and probably had similar unobservable qualities), I find that CRP participation makes land 21-28% more likely to be farmed after program exit than it would otherwise have been. This is unsurprising if the CRP improves low-quality land, because that makes the land more attractive to farm. This long-term effect comports with the agricultural goals of the program, but may counteract the environmental benefits of the program. I also find that farmed ex-CRP land is more likely to adopt conservation practices than the most similar land that had not been in the CRP. However, this may not be a result of CRP participation.

Keywords: Conservation Reserve Program, land use, environmental policy, farm policy

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Introduction

The United States Conservation Reserve Program (CRP) pays farmers to retire land from agriculture. The program has several goals, including the preservation of environmental assets and the long-term improvement of the country's agricultural productivity. Balancing multiple goals is always difficult, and may be especially challenging with regard to the long-term effects of such a program. In the case of the CRP, the disposition of land that leaves the program deserves study because the program is extremely large and market and political factors can trigger policies that suddenly release quantities of CRP land. For example, the 2008 Farm Bill dictated a drop in the CRP's enrollment cap from 39.2 million acres to 32 million acres. If land that leaves the CRP persists in conservation, this long-term effect boosts the program's environmental benefits. If ex-CRP land returns to farming at a high rate, however, this works against the program's environmental goals, but may indicate that the CRP made the land more productive.

We examine land use of parcels that have been in the CRP to determine whether the CRP has an effect on the land's later use. We ask whether CRP participation causes land to be more or less likely to be farmed (or to take up another land use) and whether past CRP participation is correlated with the use of conservation practices on farmed land. We focus on land that exited the CRP between 1992 and 1997, the first period in which land left the program in great quantity. We use a treatment effect framework, with regression and matching methods. This approach makes an explicit comparison between land that has been in the CRP ("treated" parcels) and land that has not been in the program ("control" parcels), a comparison we must make to attribute causality to CRP participation.

The CRP was established by the 1985 Food Security Act. A CRP contract binds a landowner to abstain from farming the land for a period (10-15 years) and to plant a conservation cover for that period. To join the program, farmers submit bids consisting of the rental payment they will accept, the land they would like to enroll, and the conservation cover they will plant. The “best” bids (according to criteria that evolved over time) are accepted. The USDA reimburses farmers for some of the cost of planting conservation cover. If a farmer exits before his contract expires, he pays a penalty.

The program has been very popular. Enrollment has usually been near its acreage cap (36.4 million to 39.2 million acres from 1992 to 2002), and competition has been keen for CRP contracts. The first contracts expired in 1996. Very few parcels left the program before that, and in 1996 (as in later years) most parcels with expiring contracts re-enrolled. There is some variability in a parcel’s ability to exit or re-enroll in the program. For example, around 1996, some holders of unexpired contracts were allowed to remove their land from the program without penalty, and some expiring contracts were automatically extended for one year if the contract-holder wanted to do so. The CRP’s eligibility criteria and bidding system also changed over the years to improve incentive-compatibility and to take into account an Environmental Benefits Index (EBI). As a result, some early enrollment waves accepted land for contracts that, when expired, were not renewable because the land did not meet the CRP’s new criteria.

Post-CRP land disposition has been of interest for some time, but since CRP exits did not occur in quantity until the first contracts expired in 1996, studies before 1996 were performed using surveys and simulations. Researchers (for example, Cooper and Osborn 1998; Johnson, Misra, and Ervin 1997) surveyed farmers to gauge their intentions to remain in the program and their plans for the land if the CRP were eliminated. These surveys give useful qualitative results;

for example, they indicate that not all CRP land would be farmed if it were not in CRP and that market prices drive land use decisions.

General equilibrium simulators estimate parameters or elasticities from observed land use and then use those estimates to predict the land use transitions that would occur under different market and policy conditions. These simulations account for the effects of CRP entry and exit on agricultural supply and therefore on price, which can feed back into other parcels' entry into and exit from agriculture.[2] Using the POLYSYS simulator, De La Torre Ugarte et al. (1995) estimate that 57% of CRP land would be farmed if the CRP was terminated, and De La Torre Ugarte and Helliwinckel (2006) estimate that 37% of CRP land would be farmed if the CRP were gradually eliminated. Secchi and Babcock (2007) use the EPIC simulator to show that crop prices have a very strong influence on the decision to un-retire CRP land. Lubowski, Plantinga, and Stavins (2008) find that the CRP accounted for a reduction in cropland of 29 million acres between 1982-1997 (given 32.8 million acres estimated enrolled in 1997, this implies that at least 88% of CRP land would have been farmed).

While general equilibrium simulation is essential for market-level analyses, it may not be well-suited to the analysis of parcel-level transitions, as discussed in Roberts and Lubowski (2007). General equilibrium simulators predict land use by assuming that land will enter its highest return use. These models' ability to predict in this context is limited by the fact that returns (and land characteristics determining those returns) are not fully observed. Therefore, micro-level analyses of land use changes are a useful complement to general equilibrium simulations.

[2] One such phenomenon is "slippage," as discussed in Wu (2000) and a series of related papers.

One such analysis is Roberts and Lubowski (2007), who also examine post-CRP land use decisions. They use post-CRP land use in 1997 to parameterize a model of land use for ex-CRP land, and perform a partial equilibrium simulation to predict that 58% of CRP land would enter farming if the program were eliminated. Roberts and Lubowski use a Heckman two-stage model to control for selective exit from the CRP. However, a remaining concern is selection in CRP enrollment: land that enters the program must be have low opportunity costs, i.e. low returns from farming. To assign causality to CRP participation, this selection must be considered so that CRP parcels are compared to parcels that are similar in quality—that is, to land that is also marginal and therefore likely to leave farming anyway.

We compare ex-CRP parcels to parcels that have not been in the program to ask: did CRP participation cause changes in the later land use of enrolled parcels (compared to the use they would have entered had they not ever been in CRP)? In contrast, Roberts and Lubowski ask: given a known function that determines post-CRP land use, what will happen to currently-enrolled CRP land if the program disappears?

This paper contributes to the literature in a number of ways. We attribute causality to CRP experience by using plausible counterfactual (non-CRP) land parcels. Our methodology is innovative in the use of a sample specification step that trims the treatment (CRP) and control (non-CRP) groups to the most comparable units. We also perform a multinomial logit analysis, which more thoroughly explores the land use decision. Finally, we study adoption of conservation practices on land that exits CRP and is later farmed.

We find that a naïve analysis, without careful specification of counterfactual land, shows that ex-CRP land is farmed at a lower rate than other parcels. However, when compared with the best counterfactual group, ex-CRP land is 21-28% more likely to be farmed than non-CRP land. Because this counterfactual group is very much like the ex-CRP group, we can infer that the increase in cultivation is caused by CRP participation. This result is novel in the literature, but is not unexpected, since the land should have improved while in the program. Thus, the CRP's long-term effects comport with some of the program's goals (such as agricultural efficiency) while working against others (environmental protection). We also show that cultivated ex-CRP land is more likely than similar land to adopt a conservation practice such as contour farming. However, we cannot infer whether the conservation practice result is caused by CRP participation.

The remainder of this chapter proceeds as follows. In the next section, we discuss a model of land use choice. Next, we describe the methods we will use to address the research questions. In the following section, we introduce the data and provide summary information. We give special attention to the potential "treatment" (CRP) and "control" (non-CRP) groups that will be used in the analysis. We present results in the following section. In the final section we conclude.

Theory

The essential insight behind the analysis is that a land parcel's use is determined by the relative returns of all possible uses for that parcel. This model has its roots in the ideas of Ricardo and von Thünen: land enters the use that provides the highest quasi-rents, and these quasi-rents depend on land quality and land location. In this model, we may abstract away from landowner preferences, because idiosyncratic values are reflected in quasi-rents if markets are competitive and complete.

We denote land use as $a \in A$, where A is the set of all possible land uses. We define **p** as the vector of input and output prices in the economy, **x** as the vector of land characteristics, and **t** as the vector of technology and policy factors. Each parcel has a quasi-rent corresponding to each potential use in set *A*. The quasi-rents are functions of **p**, **x**, and **t**: that is, landowner rents from use *a* are $\pi(a \mid \mathbf{p}, \mathbf{x}, \mathbf{t})$. Observed land use is:

$$a^* = \arg\max_{a \in A} \pi\left(a \mid \mathbf{p}, \mathbf{x}, \mathbf{t}\right)$$

Some important points inform the analysis. First, we must be concerned with both observable and unobservable elements of the land characteristic vector **x**. Land characteristics have static elements and elements that vary over time in ways that depend on the land's history. For example, soil can become more erodible as land is farmed. Notably, soil erodibility can improve as land is removed from agriculture and conserved, such as when it is planted with conservation cover, and CRP participation has been shown to improve land quality (e.g., Uri 2001). Low soil quality (e.g., high erodibility) makes the land less productive to farm, so parcel-level quasi-rents also depend on interactions between land characteristics and market prices.

Land use transitions should occur when the quasi-rent from the current land use is less than the quasi-rent that could be earned from some alternative land use. However, some parcels face transition costs when changing land use. For example, parcels in CRP contracts face penalties if they exit the program before the contract has expired. Even on contract expiration, land seeking to leave CRP and enter agriculture faces irreversible transition costs and uncertain future returns; these characteristics can delay or forestall transitions. For land leaving CRP, transition costs back into agriculture are affected by the type of conservation cover planted for CRP. Grasses are the most common cover (the others are forest and wildlife habitat) and carry the lowest transition cost. Because of transition costs, changes observed over short periods (such as the transitions we investigate) should *understate* the long-term transitions that will occur.

Transitions into the CRP are costly, but are subsidized by the government: the USDA reimburses some of the costs of planting conservation cover. These transitions are not possible for all parcels because of CRP eligibility requirements. As the CRP's criteria changed to emphasize environmental benefits, some parcels in the program ceased to be eligible. In these cases, if the contract-holder re-applied upon expiration of the contract, the application was rejected. Thus, when a parcel leaves the CRP, it could be because the landowner prefers to put the land into another use (i.e. because the return to another use is now higher) or because it is no longer eligible to stay in the program.[3]

When a parcel leaves the CRP voluntarily or is forced out, it should transition into the most profitable non-CRP land use. CRP contracts are only granted to land that was cultivated cropland at the time of application, so farming may be the next most profitable activity for much of this land. However, as noted above, transition costs may delay or forestall desired transitions. Additionally, land characteristics and market conditions may have changed to make farming either more or less profitable for any given parcel.

Many factors described above would have influenced land use decisions even without the CRP. We seek to identify the change in land use decisions that occurs because of parcels' experience in the CRP. How can CRP tenure cause changes in post-program land use? The CRP experience could change landowner preferences for conservation to reduce the likelihood of farming. Recall, however, that the model assumes that preferences are reflected in quasi-rents, so the model does not allow identification of such an effect. Practically, however, these

[3] Parcels that are potentially eligible with respect to land characteristics may be rejected if the bid was too high. If the bidding mechanism is incentive-compatible, the bid reflects the parcel's opportunity cost. Such a rejected bid would mean that the return to another use (the opportunity cost) now exceeds the return to CRP participation.

preferences could be imperfectly capitalized or not perfectly reflected in quasi-rent proxies. On the other hand, land improvement while in the program increases the returns to farming, and this should increase the likelihood of farming. Land that is retired from farming but does not enter CRP may also improve, but CRP's subsidization of conservation practices and CRP's enforced 10-year retirement should cause greater improvement. Thus, theory is ambiguous as to what effect CRP participation should have on later land use.

Methods

The fundamental problem of causal inference is that it is impossible to observe any unit as both a treated unit and a counterfactual (non-treated) unit. Here, end-of-period (1997) land use is the outcome and CRP participation is the treatment. To identify effects of CRP participation on land use, we must compare the land use outcomes of each CRP parcel to the outcomes that would have occurred had the parcel never entered CRP. The fundamental problem of causal inference appears here in that we cannot know what would have happened to these CRP parcels had they not entered the program. These parcels are different from non-CRP parcels because of selectivity in CRP entrance, and therefore a simple comparison to the land use outcome of non-CRP parcels could produce biased results. We must choose non-CRP parcels that can act as good counterfactuals.

Controlling for land characteristics, the returns to various land uses, and the interactions between these would yield unbiased results if the treatment and control groups are sufficiently similar. For our first step, we perform a variety of "naïve" analyses using a full data set. We regress end period (1997) farming outcome on past CRP status, controlling for land characteristics, estimated returns to land uses, interaction terms, and region dummies, with appropriate spatial error clustering.

If the parcels in the control (non-CRP) group are very unlike parcels in the treatment (CRP) group, regression results can be biased, as demonstrated in Rosenbaum (2002). This is because treatment units are compared to inappropriate control units, and given the inevitable mis-specification of a linear model, differences in sample characteristics may drive apparent differences in outcomes. To try to counteract this bias, we perform a matching analysis on the same data set. The matching algorithm chooses the control (non-CRP) units that are "most like" the treated (CRP) units based on observable characteristics. All matching results reported in this paper use nearest neighbor matching, without calipers, using the Stata package psmatch2 (Leuven and Sianesi 2003). A propensity score is estimated (as a function of land characteristics, rent proxies, and region dummies) to determine a given parcel's likelihood of being in CRP. A Mahalanobis metric (a way of calculating the difference between points in a multidimensional space) is used to do the matching, and this metric is calculated from the land characteristics, rent proxies, region dummies, and the propensity score. We calculate the average treatment effect on the treated (ATT) based on the matched sample.

Matching can only account for differences in observable characteristics. If treatment and control groups differ in unobservable characteristics that are correlated with the outcome of interest, matching results may still be biased. What unobservable characteristics are important in this analysis? Land characteristics are imperfectly observed, and transition costs are difficult to estimate, but both are crucially important in the determination of land use outcomes.

To address this, we perform an additional sample specification step. We trim the samples of CRP and non-CRP data to include only the parcels that are most appropriate to compare, based on theory and program characteristics. Conceptually, this is a population-level process much like the observation-level process of matching: the most comparable treatment and control group are selected based on observables to reduce bias. Comparability of the populations is difficult to ensure because of unobservable differences in the unobservable characteristics of land. However, inferences about these land characteristics can be drawn from the land use into which the parcel's owner has chosen to put it. This sample specification step, therefore, is a selection process targeting the CRP and non-CRP parcels that are most comparable based on the characteristics that can be inferred from their patterns of land use. The results comparing different subsamples used in the analysis in this paper can be thought of as ways of approaching the data with different research questions.

We specify two different treatment groups: CRP-Eligible and CRP-Exit. CRP-Eligible includes land that enrolled in the CRP in an early signup wave and was therefore probably eligible to exit by 1997. (Publicly-available data do not identify contract end dates, so exit eligibility is not known and must be guessed.) If the treatment group included land outside this group, many CRP parcels would be locked into contracts that are costly to exit, and the results would greatly understate the likelihood of transitions. However, much of the land that was eligible to exit actually stayed in the CRP, in part because of the automatic contract extension offered by the USDA in 1996. If a parcel stayed in the program, CRP participation may have changed the use this land would have adopted had it left the program, but it did not increase any of those returns enough to drive the land out of the program, or at least not by 1996. In this sense, it is not possible to fully observe the CRP's effect on land use for any parcels that stayed in the program.

We therefore create the group CRP-Exit, containing only parcels that actually exited. CRP-Exit contains some parcels that choose to leave (perhaps because they could earn a greater return outside the program), and some that are forced out (including parcels that do not meet new

CRP eligibility rules), but it is not possible to determine why any given parcel left the program. Given the automatic contract extensions offered in 1996, it is likely that most CRP exits at this time were voluntary.

Using CRP-Eligible as the treatment group asks the question, “Given that staying in CRP is allowed, what land use is observed on land that has been in CRP?” Using CRP-Exit asks, “Given land that has exited the CRP, what land use is observed?” This is a narrower question, because it is likely that the parcels that exit CRP are better-quality than the parcels that remain in the program. Some parcels may have exited specifically so that they can be farmed. The results from an analysis that uses CRP-Exit are informative, but caution should be exercising when extrapolating to other CRP parcels.

We also specify two different control (non-CRP) groups. Control-All includes all land that was farmed in 1982, the start of our analysis period. This restriction is important, because only farmed land can enter the CRP. Control-All includes productive farmland that is very unlike CRP land. We can control for observable characteristics, but these may understate the differences between CRP and Control-All parcels.

To address this difference in unobservables, we specify another non-CRP group called Control-Unfarmed. Control-Unfarmed parcels were farmed in 1982, so share a history of cultivation with the CRP parcels, but were not farmed in 1992. This ensures that non-CRP parcels face transition costs if they are to be cultivated, as CRP parcels do. It also restricts the sample to land of low enough quality that the landowner is willing to remove it from cultivation, which must also be true of CRP land. That is, by his land use choice, the landowner of a Control-Unfarmed parcel has revealed that his parcel’s unobservable attributes are of low quality, just as the landowner of a CRP parcel has.

There are a few sources of possible differences between Control-Unfarmed and CRP land. First, Control-Unfarmed land may be of lower quality than ex-CRP land because Control-Unfarmed was willing to retire from farming without a subsidy, while some CRP land probably would not have retired without being paid. This would make Control-Unfarmed less likely to be farmed later. Second, and conversely, some Control-Unfarmed land may be higher quality because it left farming but did not choose to enter CRP (we cannot identify which land applied to CRP and was rejected and which did not apply). These farmers may have wanted to keep the value of the option to farm during the coming ten years. This would make Control-Unfarmed more likely to be farmed.

Because of these factors, Control-Unfarmed land may be slightly better or slightly worse than CRP land. We acknowledge this potential difference. However, we expect that the resulting bias should be small, and we feel that Control-Unfarmed is still the best possible counterfactual group for CRP land.

Additionally, factors related to the timing of transitions may bias the results of comparison between CRP land and Control-Unfarmed. First, some Control-Unfarmed land may be in fallow cycles. The data set we use tries to classify land in a fallow cycle as cultivated cropland. Land classified as non-cultivated cropland (which is mostly hay) or pastureland is verified to have not been cultivated cropland for the last three years. However, if the fallow cycle is five years or longer, this land may be misclassified so that it ends up in the Control-Unfarmed category. Parcels that exit CRP may begin farming immediately because they will have just emerged from a 10-year fallow period (their CRP tenure), but only some non-CRP parcels in long fallow cycles will be ready to farm. This may introduce a bias making Control-Unfarmed less likely to be farmed in 1997 as compared to ex-CRP land. Second, and conversely, if transitions into farming take time our results may be biased to show Control-Unfarmed land more likely to be farmed. This is because, whereas non-CRP land can transition into farming in any year, CRP land can only transition after contracts end in 1996. Thus, ex-CRP land transitions may be under-recorded relative to non-CRP land transitions, thus biasing our results toward zero.

Using the pre-processed treatment and control groups, we perform the same regression and matching analyses described for the naïve sample. Comparing transitions into cultivated cropland among CRP-Exit parcels to those observed among Control-Unfarmed parcels is akin to performing a difference-in-difference analysis. It is the difference between CRP and non-CRP land in the difference in land use between the earlier period (1982) and the end period (1997). Analyses on the broader data set, and on the other combinations of treatment and control groups, do not yield valid difference-in-difference results because in those cases, the samples are not comparable.

Data and Summary Statistics

CRP contract information is sensitive and therefore is not distributed at a level of detail that is useful for parcel-level analysis. We use data from a large nationwide land survey to collect parcel-level characteristics, and we match those data with county-level estimates of the returns to various land uses.

First, parcel-level data were obtained from the USDA’s National Resource Inventory (NRI; see US Department of Agriculture 2001). The NRI is a panel survey of over 800,000 land parcel samples throughout the country. It provides data for 1982, 1987, 1992, and 1997. The NRI is a stratified survey, and the analysis that follows takes into account the NRI’s sampling structure. Data are not available for Alaska or the US Virgin Islands, and we exclude parcels in

Hawai'i and Puerto Rico, since land use decisions in those areas are likely to be quite different from land use decisions in the forty-eight contiguous states. Variables reflecting essential land quality data are not recorded for land that is urban, transportation, federal, or water. As a result, we exclude parcels that entered one of these land uses in 1997. This exclusion is particularly acceptable because this land's use may be idiosyncratic.[4]

The NRI land quality data of interest for this analysis are slope, erodibility, land capability classification (defined in US Department of Agriculture 2009), and the prime farmland indicator (*ibid*.). The NRI records land use in broad categories (e.g., cultivated cropland, non-cultivated cropland (which is mostly hay), pasture, and CRP) and narrower sub-categories. Throughout this paper, cultivated cropland will be referred to as "farmed" land. This use includes all close and row crops, and will be an outcome of particular interest because of farming's environmental impact. Parcel location information is available but limited in precision because the NRI data are designed to render precise sample location identification impossible.

The second source of data is a set of county-level proxies for the returns to various land uses. These data consist of 1996 rent levels and 1986-1996 changes in rent levels for various land use categories. These rent data are described in the appendix of Roberts and Lubowski (2007). These county-level proxies are only available for 1600 counties, and the analysis considers only counties for which rent data are available.[5]

Table 1 shows the characteristics of land in subpopulations of interest. The first column shows all land that was farmed in 1982, and the second column shows all land that was in CRP in 1992. As expected, CRP land is worse than the broader sample of originally-farmed land by

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[4] Results hold for specifications excluding land classification (the characteristic available for the fewest uses) from the control variable set and including the land from these 1997 uses (available upon request).

[5] All NRI counties with CRP parcels have rent data; only non-CRP parcels are thus excluded. Summary statistics including parcels without rent data are similar to those presented here (available upon request).

all measures (less likely to be prime or have a good land classification, more erodible, and more sloped). CRP land also tends to stay in CRP at a very high rate, and farmed land tends to continue to be farmed at a high rate. Among the specified subsamples, Control-All (the general non-CRP land sample) is of better quality than all of the other groups are, including Control-Unfarmed (the land that left farming but did not enter CRP). Both Control groups are of better quality than both CRP groups. CRP-Exit is of somewhat better quality by most measures than the CRP-Eligible group, which supports the intuition that some land leaves CRP specifically to be farmed. Interestingly, Control-Unfarmed is very slightly better than CRP-Exit on observable characteristics.

Table 1. Characteristics of Land in Subsets of NRI Sample

	Farmed in 1982	CRP in 1992	CRP-Exit	CRP-Eligible	Control-All	Control-Unfarmed
Prime farmland? [a, d]	0.547	0.286	0.345	0.262	0.576	0.438
	(0.002)	(0.002)	(0.009)	(0.004)	(0.002)	(0.006)
Good land class? [a, c, d]	0.952	0.875	0.888	0.873	0.960	0.895
	(0.001)	(0.002)	(0.005)	(0.002)	(0.001)	(0.004)
Erodibility index [a, d]	7.330	12.990	12.209	13.759	7.176	10.579
	(0.026)	(0.971)	(0.221)	(0.108)	(0.030)	(0.221)
Slope [a, d]	2.759	4.260	4.841	4.270	2.783	4.166
	(0.009)	(0.018)	(0.010)	(0.025)	(0.011)	(0.046)
Return: crops [b]	93.365	80.739	96.169	81.074	94.863	98.626
	(0.150)	(0.097)	(0.754)	(0.275)	(0.168)	(0.820)
Return: government	10.967	8.856	9.364	8.870	11.192	9.023
payments [b]	(0.014)	(0.010)	(0.063)	(0.275)	(0.016)	(0.061)
Return: pasture [b]	29.050	27.052	30.967	28.109	29.267	31.005
	(0.064)	(0.024)	(0.181)	(0.104)	(0.072)	(0.308)
Return: range [b]	10.912	11.228	8.937	11.179	10.872	10.553
	(0.032)	(0.027)	(0.201)	(0.064)	(0.036)	(0.145)
Return: forest [b]	11.725	12.189	21.662	12.807	11.659	14.453
	(0.044)	(0.011)	(0.140)	(0.065)	(0.049)	(0.181)
Return: urban [b]	2331.954	2426.994	2450.342	2566.866	2318.095	2260.462
	(4.968)	(3.711)	(31.431)	(8.409)	(5.517)	(21.918)
Early CRP signup	-	0.617	0.821	1	-	-
wave		(0.002)	(0.005)	(0)		
CRP cover of grass in	-	0.906	0.920	0.927	-	-
1992		(0.001)	(0.004)	(0.001)		
CRP in 1992	0.082	1	1	1	-	-
	(0.000)	(0)	(0)	(0)		
CRP in 1997	0.078	0.895	-	0.863	0.007	0.005
	(0.000)	(0.001)		(0.002)	(0.000)	(0.000)
Farmed in 1992	0.821	-	-	-	0.921	-
	(0.001)				(0.001)	
Farmed in 1997	0.790	0.053	0.528	0.068	0.891	0.271
	(0.001)	(0.001)	(0.007)	(0.002)	(0.001)	(0.005)
Conservation practice	0.233	0.251	0.258	0.271	0.232	0.181
if farmed in 1997	(0.001)	(0.013)	(0.014)	(0.016)	(0.001)	(0.008)
Hundreds of acres	3,755,742	340,400	31,417	188,208	2,699,633	211,329

Standard errors in parentheses

[a] 1997 land characteristic data

[b] 1996 land use returns

[c] "Good land class" is an indicator for land classification of 1, 2, 3, or 4, indicating few restrictions on use

[d] Land characteristic data only available for certain land uses, so these are means over available data: erodibility index not available for pasture; erodibility and slope not available for pasture, range, forest, other rural; erodibility, slope, prime, and land class not available for urban, water, and federal land.

Table 1 also shows that 61.7% of all land that was in the CRP in 1992 was part of an early signup wave, but only 82.1% of all land that exited CRP between 1992-1997 was in an early signup wave (probably because of contract releases that were granted at that time). Grass is the most common conservation cover planted on all land in CRP, and is not planted on CRP-Exit land at a higher rate (92%) than on CRP-Eligible land (92.7%). Consistent with intuition, most (86.3%) CRP-Eligible land stays in CRP in 1997, and most Control-All land was farmed in 1992 (92.1%) and 1997 (89.1%). Finally, CRP-Exit land is unconditionally more likely to be farmed than the Control-Unfarmed sample. This result will continue to hold throughout the analyses.

Results

The data and methods employed allow an examination of land use outcome and the adoption of conservation practices on farmed land. We will study land use outcome both as a binary choice (cultivated or not) and as a multinomial choice. Here we can infer causality on the part of the CRP because we have strong reason to believe that we have matched parcels with similar characteristics. In the conservation practice results, the sample is further restricted to land that is farmed in 1997. There are competing reasons why ex-CRP land may adopt conservation practices at a higher rate, so we cannot infer causality for this result.

Binary Land Use Outcome Results

To study land use, we begin with a simple binary choice model. The landowner chooses whether to put the land into “farming” (cultivated cropland) or into some other use. Other uses include non-cultivated cropland and pasture; these uses are obviously important to agriculture, but cultivated cropland has a larger environmental impact so is of particular interest. Cultivated cropland includes row and close crops. The vast majority (95.58%) of 1997 non-cultivated cropland is used to grow hay.

We form a naïve sample of all NRI observations for which county-level rent data are available. Analysis of this broad sample provides interesting insights, as shown in Table 2. In three different OLS specifications,[6] we use mutually exclusive dummies to compare 1997 use of CRP land to 1997 use of other land. Specification I uses dummies to indicate 1992 CRP participation and 1992 farming. This specification shows that land that was in the CRP in 1992 is less likely to be farmed in 1997 than land that was farmed in 1992 and even than land that was neither farmed nor CRP in 1992. Using the same data set, we perform a matching analysis using 1992 CRP participation as the treatment variable and the same explanatory variables as elements of the Mahalanobis metric and propensity score. Using the matched sample, the estimate of average treatment effect on the treated indicates that CRP land is 73.8% (standard error 0.014) less likely to be farmed than all other land. This result was driven by land that was farmed in 1992 and 1997 and land that was in CRP in 1992 and 1997.

[6] Results are very similar for logit and probit specifications as well; results available upon request.

Table 2. Land Use Results with Naïve (Un-Pre-Processed) Sample

	Specification I	Specification II	Specification III
CRP dummy	-0.233***		
	(0.005)		
Early CRP dummy		-0.216***	
		(0.006)	
Late CRP dummy		-0.260***	
		(0.005)	
CRPEXIT dummy			0.401***
			(0.007)
FARM92 dummy	0.629***	0.629***	0.791***
	(0.005)	(0.005)	(0.003)
N (hundreds of acres)	3,006,841	3,006,841	3,006,841
F	15,619.34	19,859.11	3,311.24
R^2	0.616	0.616	0.612

* significant at 10%, ** significant at 5%; *** significant at 1%

Standard errors in parentheses. Regressions are OLS. All included land was cultivated in 1982, is in a county for which rent data is available, and does not enter 1997 land uses: urban/built-up, water, or federal. Specifications control for 1997 land characteristics (prime farmland indicator, good land class indicator), 1996 land use rents and changes, region code dummies, and land characteristic x rent interactions. Survey regressions performed with data points appropriately weighted and clustered errors.

Specification II in Table 2 replaces the CRP dummy with dummies to indicate early CRP signup and late CRP signup, with results similar to Specification I. Specification III in Table 2 hints at the need for pre-processing. A dummy is included to indicate CRP exit, and this dummy has a positive coefficient. This land is more likely to enter farming than the baseline group. The baseline group, however, is unintuitive: it includes land that was not farmed in 1992 and land that stayed in (did not exit) CRP.

For a more interpretable result, we must look to the re-specified subsamples. Results are shown in Table 3. The table shows the coefficient on (or the marginal effect of) the CRP dummy. Results are shown for OLS, logit, and probit regressions, and matching. Covariate balancing tables for matching analyses are available upon request.

Table 3. Land Use Results with Pre-Processed Samples

Specification:	I	II	III	IV
What CRP land included?	Early signup wave (CRP-Eligible)	Early signup wave (CRP-Eligible)	Exit 1992-7 (CRP-Exit)	Exit 1992-7 (CRP-Exit)
What non-CRP (previously farmed) land included?	All land (Control-All)	Not farmed 1992 (Control-Unfarmed)	All land (Control-All)	Not farmed 1992 (Control-Unfarmed)
OLS	-0.775***	-0.185***	-0.316***	0.231***
Logit (marginal effect)	-0.835***	-0.219***	-0.285***	0.275***
Probit (marginal effect)	-0.823***	-0.224***	-0.301***	0.266***
Matching (ATT)	-0.712***	-0.159***	-0.305***	0.207***
N (hundreds of acres)	2,887,997	399,719	2,731,206	242,928

* significant at 10%, ** significant at 5%; *** significant at 1%

All included land was cultivated in 1982, is in a county for which rent data is available, and does not enter 1997 land uses: urban/built-up, water, or federal. Specifications control for 1997 land characteristics (prime farmland indicator, good land class indicator), 1996 land use rents and changes, region code dummies, and land characteristic x rent interactions. Survey regressions performed with data points appropriately weighted and clustered errors. Matching uses Mahalanobis metric plus propensity score.

Each specification (column) in Table 3 represents a combination of a treated (CRP) sample and a control (non-CRP) sample. The results generate consistent estimates within each column, so the results are not driven by functional form. However, the results change greatly between columns. This is expected, since each treatment-control group pair represents a different question addressed to the data.

Specifications I and II in Table 3 show results using the CRP-Eligible sample (CRP parcels that were part of early CRP signup waves) as the treatment group. Regardless of the control (non-CRP) group chosen, the CRP-Eligible parcels are less likely to be farmed than the counterfactuals. Recall from Table 1 that 86.3% of CRP-Eligible stayed in CRP. As argued in the Methods section, the comparison of the CRP-Eligible sample with non-CRP parcels does not address the question of *post*-CRP land use since much of the eligible land stayed in the program.

Specifications I and III in Table 3 show results using Control-All (non-CRP parcels that were farmed in 1982) as the control group. In both cases, again, CRP parcels are less likely to be farmed. This result is again informed by Table 1. Most of the Control-All parcels are continuously farmed. When we compare CRP land to this land, we are asking whether CRP makes a parcel more likely to be farmed as compared to most of the nation's farmland, including the best land. This may not be a causal relationship, since it may be driven by unobservable differences in land quality and transition costs.

Specification IV in Table 3 is the most revealing. Here, CRP-Exit (land that exited the CRP program between 1992 and 1997) is the treatment group. It is compared to Control-Unfarmed—land that, though farmed in 1982, was in some other land use in the intervening years. Control-Unfarmed land has been revealed, by the land use chosen by its landowner, to be similarly low-quality as compared to CRP land and to face similar transition costs to enter farming. Therefore, the Control-Unfarmed land is the best counterfactual for CRP-Exit land for this research question. In these analyses, the CRP-Exit land is *more likely* to be farmed than the non-CRP land. This treatment effect is not just statistically significant and consistent across specifications, but it is also economically significant, at 21-28%.

To restate this result, the CRP seems to make this land 21-28% more likely to be farmed than it would have been had it never been in the CRP. This result is expected if the returns to agriculture have increased on this land, which in turn is expected because CRP improves land quality. (Control-Unfarmed land should have also improved while it was not farmed, but CRP's period of non-farming is probably longer, and conservation practices that cause land improvement are subsidized in CRP.)

Is this result driven by the selective exit of only the best CRP land? If CRP-Exit contained the upper envelope of CRP land, but Control-Unfarmed contained only very bad land, this difference in land quality would reduce the implications of the results. To check, we reduce the CRP and non-CRP land samples to only the best land using a specification with only land indicated as prime farmland, and a specification with only land that has a relatively good (unrestricted) land classification. Results are robust to these checks, with coefficients still significant and slightly increased in magnitude (available upon request). Of course, this check has limited power if observable characteristics understate the differences in land characteristics. It is still likely that the "best" CRP land (by unobservable characteristics) had a higher tendency to leave the program. If this is the case, the magnitude of the effect observed is only accurate for the parcels that actually exited; the effect could be different (most likely smaller) if parcels were ejected from the program randomly or if the program were ended. Even so, the tendency for CRP to cause at least some land to be more likely to be farmed is still compelling and interesting.

As discussed in the Methods section, these results may be biased toward zero if Control-Unfarmed land is unobservably better than CRP-Exit land or if there is a delay in transitioning into farming, and the results may be biased upward if Control-Unfarmed land is unobservably worse than CRP-Exit land or is in a long fallow cycle. This upward bias is less likely because Control-Unfarmed was shown to be (on average) better in observable qualities than CRP-Exit, and because the NRI tries to accurately record fallow land, but it is still possible.

We check the robustness of the matching result using Rosenbaum's recommended sensitivity test (Rosenbaum 2002). This test simulates conditions in which the treatment variable (1992 CRP participation) and the outcome (1997 farming of the land) are both driven by an unobservable factor. This test assumes that the unobservable factor determines with certainty whether land will be farmed in 1997. The correlation between this unobservable factor and the propensity to receive treatment (here, likelihood of being in CRP) is varied, and the test reports the range of unobservable values for which the matching result is still valid. When CRP-Exit land is compared to Control-All and Control-Unfarmed land, the result is robust up to an unobservable factor of 2.7 and 2.6 respectively. That is, even if some parcels are 2.7 (or 2.6) times as likely to enter CRP due to an unobservable factor perfectly correlated with later farming, the matching result showing that CRP-Exit land is more likely to be farmed than Control-Unfarmed (and less likely to be farmed than Control-All) would still indicate a positive causal effect.[7] These results do not tell us that there is such a factor; it simply indicates how strong a factor would have to be to create these results. Since this test assumes that this unobservable factor wholly determines farming outcome, we feel that a robustness of 2.6 is sufficiently strong for these results to be convincing.

Multinomial Land Use Outcome Results

Cultivated cropping is the land use of greatest interest. However, it is also interesting to learn how CRP participation affects the entire distribution of final land uses. Table 4 presents multinomial logit land use outcome results. The specifications (columns) again correspond to pairs of treatment and control groups. Land use categories were: cultivated cropland, non-cultivated cropland, pasture, CRP, and other.[8] When CRP-Eligible is compared to either control group (Specifications I and II in Table 4), the results are mainly driven by the tendency of this CRP land, while probably eligible to exit, to stay in the program. CRP-Eligible land is much more likely to be in CRP in 1997, and less likely to be cultivated, than the control group. Also, when compared to the Control-Unfarmed group, CRP-Eligible land is less likely to become non-cultivated cropland or pasture, which again is because most of CRP-Eligible stayed in the CRP (and much of Control-Unfarmed became or remained non-cultivated cropland or pasture).

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[7] When CRP-Eligible parcels are compared to either control group, the result is robust up to an unobservable factor of at least 5.

[8] "Other" includes rangeland, forest, and "other rural"; these uses make up small but significant elements of CRP-Eligible (2.19%), CRP-Exit (14.63%), Control-All (2.22%), and Control-Unfarmed (17.55%).

Table 4. Land Use Multinomial Logit Results

Specification:	I	II	III	IV
What CRP land included?	Early signup wave (CRP-Eligible)	Early signup wave (CRP-Eligible)	Exit 1992-7 (CRP-Exit)	Exit 1992-7 (CRP-Exit)
What non-CRP (previously farmed) land included?	All land (Control-All)	Not farmed 1992 (Control-Unfarmed)	All land (Control-All)[a]	Not farmed 1992 (Control-Unfarmed)[a]
Cultivated cropland	-0.832***	-0.252***	-0.274***	0.270***
Non-cultivated cropland	-0.020***	-0.251***	0.059***	-0.155***
Pasture	0.001	-0.245***	0.126***	-0.095***
CRP	0.848***	0.851***	N/A	N/A
Other	0.004***	-0.104***	0.090***	-0.019***
N (hundreds of acres)	2,887,997	399,719	2,712,792	241,650
F	3,169.99	722.44	213.20	51.66

* significant at 10%, ** significant at 5%; *** significant at 1%

Cells show marginal effects of 1992 CRP status dummy on each land use. All included land was cultivated in 1982, is in a county for which rent data is available, and does not enter 1997 land uses: urban/built-up, water, or federal. Specifications control for 1997 land characteristics (prime farmland indicator, good land class indicator), 1996 land use rents and changes, and region code dummies. Survey regressions performed with data points appropriately weighted and clustered errors.

[a] For these regressions only, both Control groups exclude land that becomes CRP in 1997 (0.67% of Control-All and 0.53% of Control-Unfarmed).

In Specifications III and IV of Table 4, in which land that exits CRP is compared to non-CRP land, the results again depend on the set of control parcels used. When CRP-Exit is compared to Control-All (Specification III), we see again that the ex-CRP land is less likely to be farmed, and that the ex-CRP land has a greater tendency to go into pasture, non-cultivated cropland (hay), and other uses. This is unsurprising: as discussed above, most land that exits CRP is planted with grasses as a conservation cover, so the conversion to pasture is trivial, and the conversion to hay or other non-cultivated uses may also be inexpensive. When CRP-Exit is compared to Control-Unfarmed in Specification IV, the ex-CRP land is 27% more likely to become cultivated cropland. This result is similar to the binary model estimates. The CRP-Exit parcels are less likely than Control-Unfarmed parcels to be non-cultivated cropland or pasture in 1997. This is probably because over 80% of Control-Unfarmed land had already been in non-cultivated cropland (47.54%) or pasture (36.25%) in 1992.

Both pasture and non-cultivated cropland convert to cultivated cropland at relatively high rates: 44.8% of 1992 non-cultivated cropland, and 28.2% of 1992 pasture, was farmed in 1997. The high rates of conversion to cropping over a five-year period demonstrated in these data may indicate some error in the NRI's verification of fallow cycles. We previously noted that (the issue of long fallows aside) CRP parcels have less opportunity to transition than do Control-Unfarmed parcels because CRP contracts end in 1996. This under-measurement of transition may be particularly large because CRP-Exit parcels that do not enter farming tend to enter non-cultivated cropland and pasture at a relatively high rate, and may therefore transition into farming in the future. However, again, this would bias CRP's tendency to enter cultivated cropland toward zero.

However, since the control (non-CRP) parcels of greatest interest were uncultivated in 1992 just as the CRP parcels were, transition costs are held constant between the treatment and control groups. Further, while the ex-CRP parcels exited the program in or around 1996, the control parcels were bound by no contract and could make any transition that was profitable at any time after the 1992 observation. This would bias the results to make the ex-CRP parcels look less likely to be farmed, and our result is robust to this direction of bias.

Conservation Practice Results

The 1997 NRI sample contains data on whether any of 22 different conservation practices was adopted on cultivated cropland, summarized in Table 1 for the populations of interest. Of all acreage cultivated in 1997, 22.77% was engaged in some conservation practice. The most popular practices were terraces (6.2% of land), contour farming (5.76%), grassed waterways (4.28%), and surface drainage (4.02%). Most of the conservation practices help to conserve water and/or reduce erosion. In some cases, these practices are strongly recommended or required for land that is very sensitive (e.g., highly erodible). There are government programs at various levels to promote and subsidize practices of this type.

Is ex-CRP land that becomes cultivated cropland more likely than other land to adopt these conservation practices? Table 1 shows that unconditionally, cultivated ex-CRP land appears more likely to adopt conservation practices than either cultivated Control-All or cultivated Control-Unfarmed land. However, this unconditional result is confounded by differences in land quality and other factors.

The conservation practice analysis can be performed in much the same way that the land use outcome analysis was performed. However, since some low-quality land is required to adopt conservation practices, the sample must first be reduced to include only land that is not highly erodible, with an erodibility index of 8 or less (results without the erodibility index restriction are similar, and available on request).

Even after this extra restriction on the samples, causality cannot be attributed to the CRP. While significant results could point to a causal role for the CRP, they could also indicate that the kinds of landowners who sign their land up for CRP are also the kinds of people who will adopt conservation practices (because of personal proclivity, better knowledge of government programs, or variation in local programs that promote conservation). The land use model abstracts from landowner characteristics in a way that is fairly plausible for land use outcomes, since market returns should be strong drivers of land use. However, in the decision to adopt conservation practices that assumption seems dubious for a number of reasons. For example, government programs are not always easy for potential participants to understand, and idiosyncratic landowner information may not make its way into market prices. Even without clear causality, however, the results are still interesting.

Table 5 shows the coefficient of (or marginal effect for) CRP participation on the adoption of some conservation practice in 1997. The columns again represent different combinations of ex-CRP and control (non-CRP) land, with data restricted to non-highly-erodible land that was cultivated in 1997. In Specification I, the control group includes the portion of Control-All (all land farmed in 1982) that was farmed in 1997. For this comparison, the CRP land is not significantly more nor less likely to adopt a conservation practice. (Most of Control-All was farmed in 1992, but we cannot know which parcels adopted a conservation practice in 1992 because data on conservation practices for 1992 are not available.)

Table 5. Conservation Practice Results

	Specification I	Specification II
What CRP land included?	Exit 1992-7 (CRP-Exit)	Exit 1992-7 (CRP-Exit)
What non-CRP (previously farmed) land included?	All land (Control-All)	Not farmed 1992 (Control-Unfarmed)
OLS	0.018	0.045**
Logit	0.026	0.043**
Probit	0.026	0.043*
Matching	0.003	0.076**

* significant at 10%, ** significant at 5%; *** significant at 1%

Only non-highly-erodible land included

Land was in the categories described in the text but only non-highly erodible (erodibility index of 8 or lower). All included land was cultivated in 1982, is in a county for which rent data is available, and does not enter 1997 land uses: urban/built-up, water, or federal. Specifications control for 1997 land characteristics (prime farmland indicator, good land class indicator, slope, and erodibility index), 1996 land use rents and changes, region code dummies, and land characteristic x rent interactions. Survey regressions performed with data points appropriately weighted and clustered errors. Matching uses Mahalanobis metric plus propensity score.

For Specification II of Table 5, the control group includes the portion of Control-Unfarmed (land farmed in 1982 but not 1992) that was farmed in 1997. In this specification, both CRP and non-CRP parcels faced a transition back into farming. Adopting a new conservation practice for the first time requires some investment of time and money, particularly in planning and design, so transition costs into farming with conservation practices are greater than transition costs into farming without conservation practices. CRP parcels are 4-8% more likely to adopt a conservation practice than this very similar land. This is large relative to the baseline adoption rate of conservation practices of 18.1% for this land.

We perform another Rosenbaum test for sensitivity to unobservable factors, and the conservation practice result is very sensitive to unobservable factors that increase the CRP signup rate by as little as 40%. This leaves open the possibility that these results are driven by unobservable selection.

Conclusion

The Conservation Reserve Program uses selective retirement of agricultural land to protect the environment and to increase long-term agricultural productivity. There is a natural tension between this program's goals. The conflict between the agricultural and environmental goals of the CRP is particularly notable in land's disposition after it leaves the CRP. We seek causal effects of the CRP on later land use, and we find that CRP participation seems to cause land to be 21-28% more likely to be farmed than it would have been had it never been in the program. This result is congruent with findings that the CRP improves land quality, because that improvement would increase the returns to cropping. The CRP may act as a long, subsidized fallow period for some landowners.

The innovation of our analysis is the use of a sample re-specification step. This reduces bias by using the analyst's knowledge to restrict the treatment and control groups to the most appropriate parcels. Unobservable factors determine the potential returns to cropping and other land uses, so estimates of land use model parameters and elasticities may be biased if calculated using incomparable parcels. Comparable parcels can be identified by their use: land that is similarly low-quality and faces similar transition costs (i.e. land with similar unobservable characteristics) may drop out of regular cropping.

We find that treatment effect estimation is very sensitive to the use of inappropriate data samples. Once an appropriate sample is specified, however, results are robust to different econometric specifications. This sensitivity to data set specification may also be applicable to the calibrations used, for example, in general equilibrium simulations. On a related note, we should be aware that the 21-28% increase in likelihood of farming is an average treatment effect on the treated (ATT) and that it is only applicable to parcels that exited CRP between 1992 and 1997. It would be incorrect to infer that the same rate of increase in cultivation would occur on CRP parcels that did not exit by 1997, or on parcels that may enter CRP in the future. Notably, it would be inappropriate to infer that if the CRP were dissolved, the end use of all ejected parcels would reflect a 21-28% increase in the likelihood of farming. However, the results are still interesting in indicating that *at least some* CRP participants take advantage of CRP-induced land improvements by farming their land more intensively.

Is the increase in cultivation on these ex-CRP parcels socially desirable? The welfare effects of this increased tendency to farm are ambiguous, so this question cannot be answered without use of a social welfare function that places explicit weight on the opposing environmental and agricultural benefits of the CRP. However, some of the environmental damage that could result from this increased tendency to farm is mitigated by farmed ex-CRP land's increased tendency to adopt conservation practices.

Finally, we might infer from these results that farmers are being paid too much to conserve their land since they are getting productivity gains from the improved land, and that government payments are therefore inefficiently high. On the other hand, if the bidding process were incentive-compatible, farmers' CRP bids would be reduced by their expectation of productivity benefits. If this were the case, inefficiency would be reduced.

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