Temporal Spillovers in Land Conservation

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February 2014

Abstract:

Temporal spillovers occur when a conservation program changes what happens to land outside the temporal window of the conservation contract. This may happen when conservation improves land so that returns to non-conservation uses are increased, or when landowners' preferences become more pro-conservation as they see land flourish under conservation, for example. These post-contract changes may occur on the extensive margin (acres of land conserved) or intensive margin (intensity of land in a given use). If temporal spillovers exist, benefits from conservation programs estimated by focusing solely on the effects that occur during the conservation contract will overstate or understate the true benefits of the program. I lay out a simple model of temporal spillovers. I test this model in the context of the United States Conservation Reserve Program (CRP). I use a pre-analysis sample specification step to choose counterfactual land most like the CRP land. On the extensive margin, I find that CRP causes some land to be 22-27% more likely to be farmed, potentially offsetting some environmental benefits. However, farmed ex-CRP land is slightly more likely to use a conservation practice. This is a mitigating factor on the intensive margin.

Keywords: temporal spillovers, slippage, land use, payments for environmental services, Conservation Reserve Program

JEL Classification: Q15, Q18, Q24, Q58, R14

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[*] I am very grateful to Matt Kotchen, Nick Parker, Spencer Banzhaf, Paul Ferraro, and Michael Roberts for extremely helpful input, and to Ruben Lubowski for sharing data. I also thank the following for their useful comments: Ragan Petrie, James Cox, Vjollca Sadiraj, Daniel Hellerstein, Doug Gollin, Jason Delaney, and participants at various seminars and workshops. I am particularly grateful to organizers of and participants in the University of Tennessee, Knoxville Baker Center Workshop on Identification of Causal Effects in Environmental and Energy Economics.

Payment for environmental services programs are popular conservation tools because these incentive-based methods have desirable properties. However, when researchers seek to understand the net benefits from these programs, they generally focus only on the effects that occur when the land is in a conservation contract. Many conservation contracts lock conservation in for a fixed term renewable upon reapplication, so land may go into and out of such contracts. If conservation changes properties of the land or landowner, then it may change the use the land is put into after the contract ends (as compared to what would have happened had the land not joined the program). We must ask, therefore: do the benefits estimated from acres in conservation overstate or understate benefits from the program because of effects that occur outside of the contract period? I call such temporally-shifted effects "temporal spillovers," and in this paper I demonstrate the general importance of these effects in a simple model, and I find evidence of their existence in the United States Conservation Reserve Program (CRP).

I use the word spillover not to refer to a simple externality, but rather to refer to effects that occur outside the window of treatment, either spatially or temporally. Such unintended consequences may arise in many ways, and have been referred to by different terms.[1] Spatial "slippage" may occur when conservation increases land scarcity, causing other acreage to be contemporaneously brought into production because of higher returns, as discussed in the context of the CRP in Wu (2000) and related articles and in the context of a Mexican program in Alix-Garcia et al. (2012). Localized crowd-in and crowd-out associated with CRP and other programs is explored in Parker and Thurman (2011). Unintended consequences have been noted outside the window of treatment in other environmental policy situations. For example,

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[1] Even policies with goals other than land conservation may have perverse effects on land use. For example, Lueck and Michael (2003) find that the Endangered Species Act causes preemptive early harvest of timber stands if endangered woodpeckers might be expected to move into these stands.

"leakage" (e.g. Fischer and Fox, 2012) in an emissions reduction policy occurs when parties that are not subject to the policy increase emissions and thus offset the policy's gains, and "rebound" (Borenstein, 2013) occurs when improvements on one dimension (e.g. fuel economy) are offset by losses on another dimension (miles driven).

Temporal spillovers can be caused by temporary contracts like those used in some land conservation programs, including REDD+ contracts. Kerr (2013) notes that non-permanence of REDD+-type conservation contracts can act like "leakage" across time. If land that enters such contracts simply shifts intensive use to a post-contract period, some environmental benefits may be reduced. The program's net benefits are further reduced if participation in the program causes an increase in later intensive use, as may be the case if conserved land becomes improved in quality. These temporal spillovers matter because if we study a program's impact by examining outcomes that occur only during a contract period, we may overstate the program's benefits.

I seek evidence of temporal spillovers from the Conservation Reserve Program (CRP), the United States' largest conservation program, in a nationwide land use analysis. Paying careful attention to the specification of counterfactual land, I ask: how did CRP experience affect the land's later use? In particular, does CRP participation increase or decrease environmentally friendly land use in the long run, on both the extensive and intensive margins? A naïve analysis without careful specification of counterfactual land shows that ex-CRP land is farmed at a lower rate than other land. However, by comparing ex-CRP land to the most comparable non-CRP land, I find that the CRP causes at least some parcels to be 22-27% more likely to be farmed after exiting the contract, which is an environmentally negative effect on the extensive margin. This is evidence of a temporal spillover that reduces the program's environmental benefits. Selective exit from CRP could temper the implications of my results, but I provide suggestive evidence that the temporal spillover effect is robust to this. This result is novel in the literature but is not unexpected since the land should have improved while in the program. On the other hand, CRP land is slightly more likely to use conservation farming practices, and this is an environmentally positive correlation on the intensive margin. However, I cannot infer whether the increased conservation practice use is caused by CRP participation. Thus, I show that temporal spillovers of conservation programs may have deleterious effects on the extensive margin, but intensive margin outcomes may work in the opposite direction.

This paper proceeds as follows. In the next section, I give background on the CRP and how this paper fits into existing literature. Then I discuss a model of land use choice. Next, I describe the methods I will use to address the research questions. In the following section, I introduce the data. I give special attention to the sample specification step that will be used in the analysis. I present results in the following section. In the final section I conclude.

Background

The CRP was created with the 1985 Food Security Act. Since 1990, 30-35 million acres of US farmland have enrolled in the program. Farmers bid to enroll a parcel of agricultural land by proposing a desired payment amount and choosing a conservation practice to implement (usually, planting an approved cover crop). The government makes contracts with the best bids. Each contract lasts for 10-15 years, during which time the landowner receives annual payments as well as cost-shares covering up to 50% of the costs of conservation activities. When a contract ends, the landowner may try to re-enroll or may put the land to some other use. The CRP seeks to reduce erosion by giving a break in intensive cultivation and by using conservation cover crops to rebuild soil. The program also aims to improve agricultural productivity, and has broader environmental goals. There is evidence of success with regard to these goals: CRP reduces erodibility (e.g. Uri, 2001), with substantial benefits (e.g. Feather et al., 1999).

This land improvement could have unintended consequences, causing negative temporal spillovers, since the improved land is more agriculturally productive. By increasing returns to farming, CRP may make land more likely to be cultivated later than it would have been had it never entered the CRP. This effect is reinforced by an interaction between the CRP and other agricultural payments. Land removed from CRP can be immediately added back into a farm's crop acreage bases, which is often not true for land that was simply idled (Young et al., 2005). Base acres are used to calculate direct and counter-cyclical payments and other benefits. These rules regarding CRP and base acreage encourage farmers to convert CRP land (more than other idled marginal land) into cultivation because more government payments will be received on it.

An alternative hypothesis is that "CRP endures:" participation in the CRP makes land more likely to be conserved later. This could happen if time in conservation changes landowners' preferences, increasing quasi-rents to conservation for participating land and rendering it more likely to remain conserved. Some previous research (e.g. Chouinard et al., 2008; Sheeder and Lynne, 2011; Wallace and Clearfield, 1997) supports this hypothesis. The CRP could also cause more conservation later if crucial local input or output markets, particularly those in which there are returns to scale, are compromised when too much land in an area exits farming.[2]

The CRP may therefore cause an increased or decreased likelihood of farming on land that exits the program. This may reduce or increase the net environmental benefits produced by the program. In addition to the extensive margin effects (amount of land farmed), there could be

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[2] In addition, program enrollment can affect later land use decisions if program payments loosen credit constraints. Suggestive evidence for this channel's importance with poorer Mexican *ejidos* is shown in Alix-Garcia et al. (2012). This is less likely in the context of US farmers, for whom credit constraints are less binding.

intensive margin effects: land that has been in CRP may be richer and therefore need less chemical fertilizers, allowing more environmentally friendly farming.[3]

The earliest studies of post-CRP land use were surveys (e.g., Cooper and Osborn, 1998; Johnson et al., 1997). Since then, general equilibrium analyses have generated wide estimates of how much CRP land would return into farming if the program were eliminated: 57% in De La Torre Ugarte et al. (1995), 37% in De La Torre Ugarte and Hellwinckel (2006), and 88% in calculations from Lubowski, Plantinga, and Stavins (2008). Roberts and Lubowski (2007) account for selective CRP exit and estimate that 58% of CRP land would enter farming if the program were eliminated, and conclude that a large segment of ex-CRP land persists in conservation. However, these studies do not account for selective CRP entry, that is, for the difference between CRP and non-CRP land, and therefore cannot tell us whether the CRP *causes* land to be more or less likely to be farmed after exiting the program.

To identify the temporal spillovers, one must find appropriate counterfactual land to which to compare land leaving conservation contracts. Ideal counterfactual land is otherwise identical to conserved land but did not enter the program. Observable land differences can be controlled for; however, unobservable differences in land quality may be large and may be correlated with both program enrollment and later intensity of use. Analyses ignoring this will be biased. In this paper, I compare ex-CRP land to land that has not been in the program to ask: did CRP participation cause changes in later land use of enrolled parcels (compared to the use they would have seen had they never been in CRP)? That is, does the CRP have temporal spillovers?

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[3] Doran et al. (1997) find that a parcel of land that was in CRP is well suited to conservation tillage; however, I am aware of no research that shows that farmed ex-CRP land actually uses less intensive methods or fewer inputs.

Theory

The model assumes that land use is driven by returns and that land characteristics determine those returns. Each land parcel has characteristics, some static (s_i) and some varying with time (x_{it}). Parcel *i*'s return (or quasi-rent) at time *t* to use $a \in A$, where *A* is the set of all possible land uses, is a function of that parcel's characteristics and local market parameters (p_{it}). I focus on a single one-time land use transition where likelihood of transition is determined by these factors, some of which are influenced by past land use (and thus past transitions).

Some land characteristics that affect quasi-rents are observable, including static features like location and dynamic features like land quality and local demographics. I assume market prices are observable, although quasi-rents for each use depend on interactions between exogenous prices and land characteristics. However, unobservable land characteristics will also inevitably affect land use. Let z_{it} be an unobservable element of land quality of parcel *i* in time *t* that is correlated with the returns to farming and that ranges from 0 to 1.

The parcel-specific return from each land use is:

$$\pi_{ait} = \pi\left(a \mid s_i, x_{it}, p_{it}, z_{it}\right) \tag{1}$$

Then observed land use is:

$$a^* = \arg\max_{a \in A} \pi\left(a \mid s_i, x_{it}, p_{it}, z_{it}\right) \tag{2}$$

It would be possible to include unobservable landowner preferences (as, say, σ_{it}) in the model. This could affect the quasi-rents to a conservation use. For simplicity I do not include this variable, but I will revisit this idea below.

There are several land uses of interest. Cultivated cropland ($a = a_f$) is environmentally intensive but potentially lucrative for high-quality land. Non-cultivated cropland ($a = a_n$), which is often used to produce hay, is less intensive and less lucrative. Conservation ($a = a_c$) provides fixed payments and improves the land (makes some elements of x_{it+1} and z_{it+1} increase).

Since land will enter the use with the highest profit, the likelihood that parcel *i* is in cultivation at time *t* is:

$$\Pr\left(\pi_{fit} \geq \pi_{nit} \cap \pi_{fit} \geq \pi_{cit}\right) \tag{3}$$

We can then say that some function ϕ determines the likelihood of farming, driven by the land and market characteristics.

$$\Pr\left(a_{it} = a_f\right) = \phi\left(s_i, x_{it}, p_{it}, z_{it}\right) \tag{4}$$

We assume in particular that $\frac{\partial \phi}{\partial z_{it}} > 0$.

Similar equations would predict the probability that a land parcel enters any other use. Of the three uses of interest, farming will tend to have the highest returns for land that is fairly fertile. Land that is of lower quality will be less profitable as farmland, so landowners would have to decide between non-cultivation and conservation. This outcome may not be entirely driven by the landowners' returns, however: conservation programs like CRP limit enrollment with acreage limits, bid caps, and eligibility criteria. A parcel of land that is observed in non-cultivation may therefore have higher expected profits from conservation but might have failed to join the program (e.g. may have been rejected). In practice, in the first decade of the CRP, bid rejections were often caused by requests for rental payments that were too high.[4]

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[4] Eligibility is now based on an Environmental Benefits Index (EBI), and the payment requested is part of that. However, correspondence with USDA staff reveals that the first waves during the program's ramp-up (when the EBI did not exist, and eligibility was determined by erodibility index) did not have high rejection rates, and that rejections in those years were largely caused by high bids. The bid cap for each county was kept private to reduce strategic behavior (Shoemaker, 1989). If the process were incentive compatible, bids would represent opportunity costs, and therefore conservation would not provide the highest return for parcels that are rejected. However, if farmers know bid caps, the process is not incentive compatible and thus bids will not represent opportunity costs.

Recall that z_{it} is unobservable and is correlated with agricultural productivity.[5] Two parcels with similar observable features and different land uses should have different levels of z_{it}. In this way, we can draw inferences about unobservable land quality from chosen land use. Suppose z_{it} varies as demonstrated in Figure 1. Very high quality land $\left(\overline{z} < z \le 1\right)$ is very agriculturally productive and will always be farmed. Very low quality land $\left(0 \le z < \underline{z}\right)$ is never productive enough to be farmed. Marginal farmland $\left(\underline{z} \le z \le \overline{z}\right)$ may fall in and out of farming as conditions vary. Some marginal land that leaves farming may enter conservation programs.[6] Other marginal land may simply remain uncultivated. Regardless, land that is marginal enough to enter conservation has a z_{it} value most like land that occasionally exits farming.

FIGURE 1 ABOUT HERE

High quality land has a higher average value of z than does marginal land, including land that is conserved. Since this difference is unobservable, land use regressions that ignore this selective conservation will be biased toward finding that conserved (e.g., CRP-enrolled) land is later farmed at a lower rate.

To eliminate this bias, conserved land must be compared to land with z values in the same range. If conserved land is compared to marginal-quality land that enters temporary non-cultivation, the average value and distribution of z should be similar across the two groups of land and therefore differences in later land use should be attributable to program participation.

How may conservation program participation cause changes in later land use? Land use changes will be caused by changes in the factors that determine returns.[7] Conservation can

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[5] The logic regarding an unobservable quality variable that determines land disposition is very similar to that in Stavins and Jaffe (1990).

[6] CRP and similar programs only enroll land that is currently cultivated.

[7] Conservation program enrollment can also affect outcomes through local markets: if enough farmland in an area enrolls, parts of the local supply chain that depend on economies of scale (like a grain elevator) may cease to be

change unobservable land quality z_{it}. Specifically, conservation improves soil quality: CRP participation reduces erodibility (e.g., Uri, 2001), and this should increase the return to cultivation. Thus marginal land that enters a program like CRP with a low z_{it} may emerge from the program with a higher z_{it+1} and thus be more likely to be farmed. I model this by assuming that z_{it} has a persistent component (i.e., depends in part on z_{it-1}) but also depends on participation in the conservation program in the last periods, C_{it-1}.

$$\Pr\left(a_{it} = a_f\right) = \phi\left(s_i, x_{it}, p_{it}, z_{it}\left(C_{it-1}, z_{it-1}\right)\right) \tag{5}$$

Here again, $\frac{\partial \phi}{\partial z_{it}} > 0$. If $\frac{\partial z_{it}}{\partial C_{it-1}} > 0$, past conservation program participation increases unobservable quality. It must therefore be true that $\frac{\partial \phi}{\partial C_{it-1}} > 0$. This is a temporal spillover: past conservation causes increased probability of farming later.

In this way, a conservation program may work like a subsidized fallow program. Land also improves while it is simply non-cultivated, but more improvements occur under programs like CRP because the government shares the costs of improving the land. In the case of CRP, even greater improvement occur because contracts are 10-15 years long, which is longer and therefore more enriching than most fallow cycles.

A transition into farming may also yield greater profits for ex-CRP land as compared to other idle land of identical quality because it gives higher commodity program payments, as discussed above, through an increase in base acreage. Thus even if CRP and non-CRP land have identical characteristics and operate in the same markets, CRP land may have larger cultivation-related elements of p_{it} and as a result may have a greater likelihood of converting to farming.

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profitable and therefore close, making it harder to start farming on land in this area in the future. This outcome is an effect of program enrollment in one sense, although it occurs at an aggregate level rather than at parcel-level.

The argument I have made for differences and changes in unobservable land quality z_{it} could also be made for σ_{it}, an unobservable taste for stewardship. Landowners with little taste for stewardship may continuously cultivate farmable land, but those that like stewardship may take breaks from cultivating sensitive land or may enroll sensitive land in a conservation program. If experiencing conservation in period t increases a landowner's taste for stewardship then conservation could decrease later cultivation by causing σ_{it+1} and therefore π_{cit+1} to increase.

These changes are made harder to detect by transition costs, particularly the costs of entering cultivation. To cause a land use change, the difference in profits between the current use and the preferred use must be large enough to overcome transition costs. Observed land use transitions will understate changes in quasi-rents caused by program participation.

Methods

To identify conservation's effect on later land use, one must compare the land use on a parcel that has been in the program to the use it would have been in had that parcel not been in the program. To identify this effect, I compare end-of-period (1997) use on land that was in the CRP in an earlier period to land that has not been in the CRP and see which is more likely to enter cultivation. However, any effects of the CRP may be masked if unobservable land quality z_{it} drives land use. I perform a sample selection step to choose the best counterfactual land so that causality can be attributed to CRP participation. This is a population-level process like the observation-level process of matching: comparable treatment and control units are selected based on observables.[8] This sample specification step targets the CRP and non-CRP parcels that are

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[8] Indeed, Ho et al. (2007) refer to matching as a way of "preprocessing" a data set.

most comparable based on characteristics inferred from their patterns of land use. Specifically, I infer a parcel's z_{it} from the parcel's land use and use that to identify comparable land.

The first issue in sample specification is that most CRP land is locked into a conservation contract at any time and faces a penalty for early exit, creating a large barrier to transition. It is sensible to only examine CRP parcels that have a potential land use decision to make. However, CRP contract information is considered sensitive and is therefore not publicly released, so it is not possible to identify which CRP land parcels have expiring contracts. Contract start dates are publicly known, but durations vary (each contract lasts 10-15 years). Also, original contract terms do not always determine true contract end dates: land is sometimes allowed to leave the CRP before contract expiration, and in the time I study the USDA automatically granted one-year extensions to many expiring contracts to ease the administrative burden of the first major round of expirations. Therefore, my analysis uses only land that exits the program (CRP-Exit). This land verifiably had a post-CRP land use decision to make. Inferences made about this land must be tempered by concerns about selective exit: landowners take land out of the program for economic reasons, as shown in Roberts and Lubowski (2007). For this reason, my results only strictly apply to land that exited CRP; that is, estimates are local. However, I perform robustness checks that provide suggestive evidence that my results are not driven solely by selective exit.

The more nuanced issue is unobservable quality of counterfactual (non-CRP) land. As discussed in the Theory section, an unrestricted sample of non-CRP agricultural land will include a range of land qualities $\left(\underline{z} \le z < 1\right)$. Much of this land (with $\bar{z} < z \le 1$) is likely to stay in cultivation. This land is a poor counterfactual for CRP land, which is likely marginal. Analysis that ignores this will be biased toward finding CRP land less likely to be cultivated. For this reason, my analysis uses a subsample of non-CRP land containing counterfactuals for CRP land because, like CRP land, it should be marginal in the sense of having a moderate z $\left(\underline{z} \leq z \leq \overline{z}\right)$. I infer this moderate level of z from the uses the land has been put to. Marginal farmland may be taken in and out of farming over time as conditions vary. My non-CRP sample (Control-Unfarmed) includes land that, like the CRP land, started out being cultivated, but is restricted to land that was not cultivated for some intervening period. Control-Unfarmed should have a z similar to that of CRP land. This land also faces transition costs to re-enter cultivation just as CRP land would. These transition costs would not exist for land the continued to be cultivated.[9]

In my analysis, I use OLS and probit regressions to compare the tendency to enter into cultivated cropping and (for farmed land) to use conservation practices. I make these comparisons between CRP land that exited the program (CRP-Exit) and non-CRP land of similarly marginal quality (Control-Unfarmed). The sample specification step reduces the potential bias caused by observable and unobservable differences between CRP and non-CRP land. However, some sources of bias may persist. I discuss these when I present my results.

Data and Summary Statistics

As I will describe in detail shortly, I use data from a large nationwide land survey to collect land characteristics at the parcel level. I supplement those data with county-level demographics and estimates of returns to land uses. CRP contract information is sensitive and therefore is not distributed at a level of detail that is useful for parcel-level analysis, so I do not use it.

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

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[9] Land in the program could be different from unenrolled land on another unobservable dimension: tendency of the program agency to target land. Imagine that the agency uses data that are locally observable (but unobservable to the analyst) to target marginal land that is more likely to be farmed. If the agency is successful at enrolling this land, then land that has participated will later be more likely to be farmed—but this will be a cause, not a result, of program participation. This is less likely in the early CRP because targeting in general was poor, and because local USDA officials should prefer to cultivate cooperative (rather than adversarial) relationships with landowners.

parcel samples throughout the country. I include only data from the continental US. It provides data for 1982, 1987, 1992, and 1997.[10] The NRI is a stratified survey, and the analysis that follows takes into account the NRI's sampling structure. The unit of observation is essentially a sampled parcel, but each sampled parcel has an expansion factor defining the land area this observation represents (averaging 15.730 and ranging from 1 to 495 in the NRI overall).[11] The NRI is designed to render precise sample location identification impossible, but I use controls down to the county level. It is also not possible to obtain information about ownership of the land, e.g., which land parcels are owned by the same person. Data availability is further limited because some land quality features are not recorded for land that is urban, transportation, federal, or water, so I exclude parcels that entered one of these land uses in 1997. This exclusion is particularly acceptable because this land's use may be idiosyncratic.[12] Variables reflecting other land quality data (erodibility and slope) are not available for these land uses or for pasture, range, forest, and other rural; since pasture is an important alternate use, my main analysis will control for erodibility and slope from 1982, when all land in the selected samples was farmed.

The timeline of NRI samples as they relate to the start and end of the first CRP contracts is shown in Figure 2. The focus of this analysis is on land that exited CRP by the 1997 NRI sample, and that includes the first wave of CRP contract expirations. Figure 2 also shows the land use patterns that defines the subsamples specified for the analysis.

FIGURE 2 ABOUT HERE

The NRI land quality data of interest for this analysis are slope (Universal Soil Loss Equation (USLE) slope percent), erodibility index (defined as the maximum of wind and water

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[10] The NRI has been conducted since 1997, but the USDA does not make these later years' data available.

[11] Only 388 of the 266,992 NRI points represent more than 100 acres and only 83 more than 200 acres.

[12] 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).

erodibility values), land capability classification (US Department of Agriculture, 2009), and the prime farmland indicator (*ibid*.). Steeper-sloped and more erodible land is less agriculturally productive. Land capability classification is a categorical indicator of a soil's ability to produce crops; I use an indicator ("good land classification") for land with land capability classification of four or less. Land is identified by USDA as prime if it meets a set of criteria conducive to agricultural productivity, including frequency of flooding, irrigation, water table, and wind erodibility. I focus on several of the NRI's broad land use categories: cultivated cropland (all close and row crops), non-cultivated cropland (about 95% of which is hay), pasture, and CRP.

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, and are described in the appendix of Roberts and Lubowski (2007). These are only available for 1,600 counties, and the analysis considers only counties for which rent data are available.[13] Demographic data (population density, decadal change in population density, median household income, poverty rate, unemployment rate, percent black, percent white, median age, percent of population under 18, and percent of population over 65) from 1996 were collected at the county level from the Census and the Bureau of Labor Statistics.

The variables that will be used in this analysis are shown in Table 1 for the populations of interest: Table 1a shows parcel-level data, and Table 1b shows county level data.[14]

TABLE 1 ABOUT HERE

The first column includes all land that was cultivated in 1982, and the second column includes all land that was in CRP in 1992. As expected, CRP land is worse than the broader

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[13] All 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).

[14] Table A-1 in the Appendix shows additional characteristics of the same subsamples of land: averages of county-level rents and decadal changes in rents to various land uses, and parcel-level data on what crops are grown.

sample of originally-farmed land by all measures (less likely to be prime or have a good land classification, more erodible, and more sloped) and appears generally poorer and more rural. CRP land tends to stay in CRP at a very high rate, and farmed land tends to continue to be farmed at a high rate. CRP-Exit is slightly better by most measures as compared to the rest of CRP land, which supports the intuition that some land leaves CRP specifically to be farmed,[15] although it is still much worse than the general body of farmland. Control-Unfarmed is very slightly better than CRP-Exit on observable characteristics, but is much more like CRP-Exit than the rest of farmland. Overall, the data support the notion that CRP and Control-Unfarmed are roughly similar marginal land while Control-All is of much higher quality.[16]

Table 1 also shows that 61.7% of all land 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 granted at that time). Fully 89.5% of CRP land stays in the program. CRP-Exit land is unconditionally more likely to be farmed than the Control-Unfarmed sample. This result will continue to hold throughout the analyses.

The spatial distribution of the land of interest reinforces the story told by Table 1. Figure 3 shows all land farmed in 1982, and Figure 4 shows all land that was in CRP in 1992. The maps show that CRP participation was not evenly distributed across all farmland: some areas that are known to be very productive (such as land in California, Iowa, Illinois, Ohio, Michigan, and along the Mississippi River) are underrepresented in the CRP.

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[15] Roberts and Lubowski (2007) find that land is more likely to exit CRP if it is planted in grass or legumes as cover, and also find that tendency to exit is correlated with rent proxies for several uses and with some interactions between land quality and rent proxies. In the data set I use for this study, land that exits the CRP is better along several dimensions as compared to land that stays in the program, and land that exits is slightly more likely to be planted in grass or legumes than land that stays in.

[16] Control-Unfarmed land, as noted, could be kept out of farming so that urban development can be done on it. This land is in counties with higher population densities and smaller decadal drops in returns to urban/built-up land, as compared to the other subsets of land; however, 1996 returns to urban development are lower in Control-Unfarmed. All land that becomes urban/built up in 1997 is excluded from the analysis.

FIGURE 3 ABOUT HERE

FIGURE 4 ABOUT HERE

Figure 5 shows a map of land in the CRP-Exit group. This group is a sparse subset of the greater CRP sample. Figure 6 shows land in the Control-Unfarmed group. This land is also quite sparse, but is a better match for the spatial distribution of CRP-Exit land than is the great mass of US farmland shown in Figure 3. The spatial distributions still differ, and therefore spatial controls and robustness will be important in the analysis.

FIGURE 5 ABOUT HERE

FIGURE 6 ABOUT HERE

Results

To seek evidence of temporal spillovers, I study the relationship between CRP experience and land use outcome. Here I can infer causality on the part of the CRP because I have matched parcels with similar observable and, I argue, unobservable characteristics (similar *z*). I then examine conservation practice takeup, further restricting the subsamples to land that is farmed in 1997. There are competing reasons why ex-CRP land may adopt conservation practices at a higher rate, so I cannot infer causality for this result.

Land Use Results

The regression analysis uses observations at the parcel level to estimate the relationship between an indicator for whether the parcel enters cultivation in 1997 ($farm_{i,1997}$) and an indicator for whether the parcel had been in the CRP ($CRP_{i,1992}$). Regressions control for observable parcel-level (subscript *i*) and county-level (subscript *c*) characteristics: $\mathbf{land}_{i,1997,1982}$ contains land quality measures (from 1997 and 1982), $\mathbf{rent}_{c,1996}$ and $\mathbf{rentchg}_{c,1996}$ are quasi-rents and decadal changes in quasi-rents, and $\mathbf{demogs}_{c,1996}$ contains county-level demographics. Regressions also include spatial fixed effects, usually at the level of USDA Farm Production Regions (ν_r).[17] Equation 6 is the OLS regression equation; the probit equation is the parallel probit construction.

$$\begin{aligned} farm_{i,1997} &= \beta_0 + \beta_1 CRP_{i,1992} + \boldsymbol{\gamma}_1\mathbf{land}_{i,1997,1982} + \boldsymbol{\gamma}_2\mathbf{rent}_{c,1996} + \boldsymbol{\gamma}_2\mathbf{rentchg}_{c,1996-1986} + \boldsymbol{\gamma}_3\mathbf{demogs}_{c,1996} \\ &\quad + \boldsymbol{\gamma}_4\left(\mathbf{land}_{i,1997,1982} \times \mathbf{rent}_{c,1996}\right) + \boldsymbol{\gamma}_5\left(\mathbf{land}_{i,1997,1982} \times \mathbf{rentchg}_{c,1996}\right) + \nu_r + \varepsilon_i \end{aligned} \tag{6}$$

It is clear from Table 1 that CRP land is worse quality and unconditionally less likely to be farmed as compared to the general body of American farmland. Any analysis that ignores selection into CRP will find that CRP participation is associated with a lower tendency to be in cultivation later. As argued in the Methods section, this selection bias is eliminated if analysis is restricted to CRP-Exit and Control-Unfarmed. Results of OLS and probit regressions with region fixed effects are shown in Table 2 (specifications 1 and 2, respectively), along with results of an OLS regression with state fixed effects (specification 3). Given the dimensionality of the analysis and the survey structure of the data, probit cannot compute marginal effects with state fixed effects. County fixed effects regressions would provide more careful spatial controls, and Figures 5 and 6 demonstrated that there are spatial differences between the groups; however, again, the survey structure of the data and the large number of counties render county fixed effects problematic (since there are more counties than NRI sampling clusters in the relevant sample, an *F* statistic cannot be computed and standard errors may be incorrect). However, the coefficient

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[17] The USDA divides the United States into ten farm production regions: Pacific (AK, CA, HI, OR, WA), Mountain (AZ, CO, ID, MT, NM, NV, UT, WY), Southern Plains (OK, TX), Northern Plains (KS, NE, ND, SD), Lake States (MI, MN, WI), Corn Belt (IA, IL, IN, MO, OH), Delta States (AR, LA, MS), Appalachia (KY, NC, TN, VA, WV), Southeast (AL, FL, GA, SC), and Northeast (CT, DC, DE, MA, MD, ME, NH, NJ, NY, PA, RI, VT).

on CRP participation from a regression with county fixed effects is 0.200, nearly identical to that from the state fixed effects specification.[18]

TABLE 2 ABOUT HERE

All specifications in Table 2 tell very similar stories, indicating that these results are not driven by functional form or spatial variation.[19,20] The coefficient on CRP participation is positive and highly statistically significant: CRP-Exit land is *more likely* to be farmed than the non-CRP land. At 22-27%, this effect is economically significant. This result could not have been obtained without pre-limiting the CRP and counterfactual data to address the issues of unobservable land quality and unobservable contract end date. Analysis using broader samples of CRP and non-CRP land finds that CRP land appears less likely to be farmed. However, this biased result obtains because CRP land is likely to stay in a CRP contract and because it is of lower quality than non-CRP land. My sample specification step eliminates these biases.

The other coefficients in Table 2 tell a sensible story. Land that is prime and that is less sloped is more likely to be farmed. (Good land classification is negatively correlated with farming, but that effect disappears if other land characteristics are omitted from the regression.) Counties with growing population see land less likely to be farmed, and higher incomes and lower unemployment rates are associated with more farming. These results are sensible because this land is mostly quite rural, where farming is a large source of employment and income.

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[18] Analyses reported here use errors clustered on the NRI “cluster” level, which is finer than the county level. Coefficient estimates remain highly significant ($p < 0.001$) when errors are clustered at the county or state level; however, to perform this higher-level clustering, regressions must ignore the stratified structure of the NRI survey.

[19] Results from propensity score matching are similar, with a point estimate of 0.224 that can be compared to the estimates in Table 2, and point estimates within 15% of those in Table 3. There are no known methods of correctly calculating standard errors for propensity score matching when data has sample weights (Leuven and Sianesi, 2003).

[20] Results from a logit specification are similar to those from OLS and probit. A multinomial logit specification shows that land that leaves CRP is more likely to be farmed (with a similar point estimate) and less likely to enter other uses such as non-cultivated cropland and pasture.

To restate the main result, the CRP seems to have made land 22-27% 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, including unobservable quality *z*. (Control-Unfarmed land should have also improved while it was not farmed, although not as much, as argued above.[21]) Again, this is an estimate of a local effect on only the land that exited the CRP. Nonetheless, it demonstrates the existence of temporal spillovers. I address selective exit in robustness checks below.

Robustness and Potential Biases

I have shown that CRP seems to cause at least some land to be more likely to be farmed after exiting the program as compared to what would happen if it had never participated.

These results may be biased downward for several reasons. First, Control-Unfarmed land may be unobservably better (higher *z*) than CRP-Exit land; after all, it did leave farming without a subsidy, and may have chosen not to enter CRP to retain the option of farming (recall, the data on which parcels applied to enter CRP but were rejected is not publicly available). Second, there may be a delay in transitioning into farming, so some transitions from CRP-Exit into farming may not be observed. While non-CRP land can move into farming in any year, CRP land can only transition after contracts end. Thus, ex-CRP land transitions may be under-recorded relative to non-CRP land transitions, making Control-Unfarmed land seem more likely to be farmed.

The results may be biased upward for several reasons. First, Control-Unfarmed land may be unobservably worse (lower *z*) than CRP-Exit land. After all, Control-Unfarmed was willing to

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[21] Control-Unfarmed land that is unfarmed for a longer period (both 1987 and 1992) should improve more than land that is unfarmed for a shorter period (1992 only), and thus should be more like CRP land; regressions using Control-Unfarmed land as the counterfactual to CRP land could therefore show a smaller effect of CRP on later farming. However, they actually show a larger effect of CRP on farming. This may be because Control-Unfarmed land that was unfarmed in 1987 and 1992 was worse-quality land, as indicated by the fact that the landowner chose to retire it for a long period without financial incentive.

retire from farming without a subsidy while some CRP land probably would not have retired without being paid. However, Table 1 shows Control-Unfarmed to be slightly better on observable qualities as compared to CRP-Exit. Second, some Control-Unfarmed land may be in a long fallow cycle. However, the NRI tries to classify land that is in a fallow cycle as cultivated cropland. Land classified as non-cultivated cropland (which is mostly hay) or pasture is verified to have not been cultivated cropland for the last three years. If land is in a fallow cycle of five years or longer, it may be misclassified as pasture or non-cultivated cropland, in which case I would include it in Control-Unfarmed. 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 make Control-Unfarmed appear less likely to be farmed as compared to ex-CRP land. Third, the CRP-Exit group may overrepresent landowners who have had shocks pushing them to farm the land. This selective exit issue is addressed in part by the demographic controls I include in my regressions and in part by the robustness checks below.

The results may be biased up or down because of differential plans to put Control-Unfarmed or CRP-Exit land into development. CRP-Exit made a transition out of conservation, which may indicate a desire to develop that land, perhaps driven by economic shocks. On the other hand, Control-Unfarmed was kept out of farming, which may indicate similar motivations. My regressions control for urbanization measures (population density and change in population density), and I exclude land classified as urban or built-up in 1997 to mitigate these biases.

TABLE 3 ABOUT HERE

These results are robust to a number of alternative specifications, shown in Table 3.[22] First, I noted above that selective exit from CRP limits the analysis, since I only address selective entrance. Selective exit means that I can only conclude that the CRP causes *some* land to be more likely to be farmed, and the magnitude of this effect may be diminished for CRP land that has not exited. If only the best CRP land exits, then CRP-Exit may contain the upper envelope of CRP land (say, $z_{mid} \leq z \leq \bar{z}$ for a z_{mid} such that $\underline{z} < z_{mid} < \bar{z}$) . Control-Unfarmed would then contain a wider range of land quality ($\underline{z} \leq z \leq \bar{z}$). Since observable land quality measures have a relationship with land use and thus perhaps with unobservable land quality z, I restrict the subsamples to only the observably best land for four robustness checks. In specifications 1 and 2, I restrict both the CRP and non-CRP land to only the observably best land; in specifications 3 and 4, I thusly restrict only the non-CRP subsample. In each case, I define the best land according to the presence of a good (unrestricted) land classification (specifications 1 and 3) and the prime farmland indicator (specifications 2 and 4). Results are robust to these checks, with coefficients still significant and in nearly all specifications increased in magnitude. In specification 5, to address the concern that Control-Unfarmed may overall be better than CRP-Exit land, Control-Unfarmed is restricted to only highly erodible land (with erodibility index 8 or more). These results are also robust and are again stronger than the original specification. This implies that Control-Unfarmed land as a whole is higher-quality than CRP-Exit land as a whole. Again, this would imply that my results understate the effect of CRP participation.

The CRP-Exit / Control-Unfarmed result also persists nearly unchanged (now in the range 21-24%) when Control-Unfarmed parcels are chosen only from counties where CRP enrollment might have been near the countywide enrollment cap (detailed results available on

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[22] Table 3 shows OLS estimates, but all coefficient estimates are similar for logit or probit regressions.

request). In these counties, Control-Unfarmed land is more likely to have made a bid for a CRP contract and been rejected, and thus this land could be better counterfactual land.[23]

Finally, the use of Control-Unfarmed land in the intervening years can also tell us about that land's unobservable quality *z*. When that subsample is restricted to only parcels that were farmed in 1987, which may have a higher *z*, the CRP land still appears more likely to be farmed although the magnitude is slightly reduced to 17-21% (detailed results available on request).

Conservation Practice Results

Conservation practices reduce the environmental impact of farming, generally by conserving water or reducing erosion. Variation in conservation practices can be thought of as variation on the intensive margin of farming. The NRI contains data on whether any of 22 different conservation practices was adopted on cultivated cropland in 1997 (but not in earlier years). This data is summarized in Table 1 for the populations of interest. Of all land cultivated in 1997, 22.77% used a 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%). In some cases, these practices are strongly recommended or required for sensitive (e.g., highly erodible) land. Government programs at various levels promote and subsidize conservation practices.

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

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[23] Another interesting robustness check would be to study counties that had particularly high CRP bid rejections in the early rounds, since this may imply that large amounts of unfarmed CRP land was rejected from CRP and thus may be particularly like the CRP land. Correspondence with USDA staff reveals that rejections those first waves are unlikely to be random, being largely caused by high bids.

The conservation practice analysis can be performed in much the same way that the land use outcome analysis was performed. The restriction of data to CRP-Exit and Control-Unfarmed is sensible again here, because it ensures that all land faces transition costs when implementing a conservation method. However, since some low-quality land is required to adopt conservation practices and these requirements are not evident in the data, the sample must be further restricted 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). Even then, causality cannot be attributed to the CRP. There is a theoretical reason why CRP participation may cause less intensive farming later (because richer land needs fewer environmentally harmful inputs to farming), but there are competing explanations. It could be that landowners concerned with stewardship (high-σ farmers) are drawn to both CRP and the use of conservation practices if they farm. It could also be that landowners who are more familiar with government programs are more likely to enter CRP and to take up a conservation practice promoted by local farm office officials.

TABLE 4 ABOUT HERE

Table 4 shows the results of regressions of conservation practice usage on CRP participation, using only non-highly erodible land in CRP-Exit and Control-Unfarmed that was cultivated in 1997.[24] CRP land is roughly 4% more likely to adopt a conservation practice. This is large relative to the baseline adoption rate of conservation practices of 18.1% for this land. Conservation practice take-up is also associated with more erodible land, less sloped land, and land in counties with more unemployment and fewer young people. To restate the result, CRP land that exits the program and enters cultivation is more likely to use a conservation practice as compared to non-CRP land, although this may not be causal.

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[24] Results are similar for logit and propensity score matching.

Conclusion

Policies designed to influence land use have unintended consequences. Temporal spillovers can reduce or increase the net benefits of payments for environmental services programs with finite contract lengths if participation causes later activity on the land to be more environmentally harmful or beneficial, respectively. Estimates of the net benefits of such programs that ignore effects outside the time window of the conservation contract will overestimate or underestimate these net benefits if such spillovers are important. I describe this phenomenon generally, and I find evidence for the existence of substantial harmful temporal spillovers in land conservation in the largest American conservation program, the Conservation Reserve Program. The temporary contracts used in the CRP are particularly vulnerable to temporal spillovers since land improves while in the program and thus becomes more productive to farm. Program participation could cause enduring conservation, on the other hand, if landowners who see their land flourish in conservation develop a taste for conservation.

On the extensive margin, I find that CRP participation causes at least some land to be 22-27% more likely to be farmed than it would have been had it never been in the program. This result is local in the sense that the estimate only applies directly to land that voluntarily exited the program, but it demonstrates temporal spillovers as an item of concern in land conservation. Additionally, robustness checks provide suggestive evidence that selective exit is not driving this result. The CRP may act as a long, subsidized fallow period for some landowners. However, on the intensive margin, CRP participation is associated with greater use of conservation practices on land that is later farmed. While this offsets some of the environmental harm caused on the extensive margin, I cannot claim that this was caused by CRP participation.

An innovation of my analysis is the use of a sample re-specification step. I use observed land use to infer the range of an unobservable land quality factor I call *z*, since land most taken out of cultivation either for conservation or for other uses is likely to be marginal. This process essentially “matches” samples of conserved and non-conserved land with similar values of *z*. Without such a step, differences in unobservable land quality create biased results that falsely imply persistent benefits of conservation because conserved land is unobservably worse (and thus less likely to be farmed) than counterfactual land.

These results speak more broadly to the potential for temporal spillovers to afflict other conservation programs. Spatial spillovers (slippage) have already been raised as a concern because of their ability to reduce benefits of payment for environmental services programs. We must also consider the temporal effects of these programs. These effects may be more insidious because they are hard to detect. Temporal spillovers may also be fundamentally more damaging because they do not simply shift activity but actually increase intensive use because the characteristics of the land being conserved changes while in the program.

To reduce undesired temporal spillovers, it may be wise to change policy design. Conservation payments may currently be higher than necessary, since land quality improvements provide private benefits to landowners when the later intensive use is undertaken. (This might not be the case if lowered conservation participation bids reflect these benefits.) Other changes to conservation contracts might reduce these spillovers. For example, longer contracts might provide more in-contract environmental improvements relative to the out-of-contract reduction in environmental benefits. However, if such changes were made, programs could see reduced program participation or requests for higher payments.

Finally, temporal spillovers may interact with spatial spillovers. Imagine that as parcel A enters into a temporary conservation contract, marginal parcel B is pushed by spatial slippage to enter intensive use. When parcel A then exits the contract, this increases the supply of land available for intensive uses and pushes down prices. That may push some land that had previously been in intensive use into nonuse. Parcel B may then "reverse slip" out of its intensive use. Of course, if parcel A was actually improved while in the program, these effects should not net out since parcel A will be more productive than it was before it enrolled.

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Figures and Tables

Figure 1. Land use and unobservable land quality

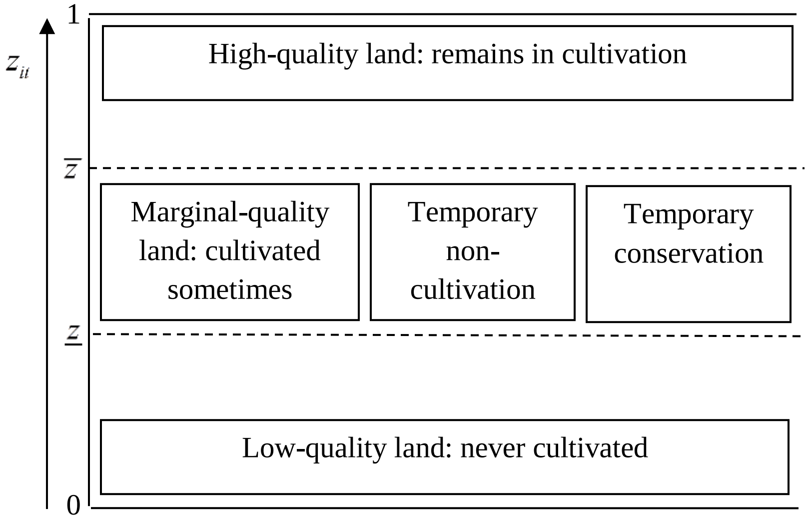
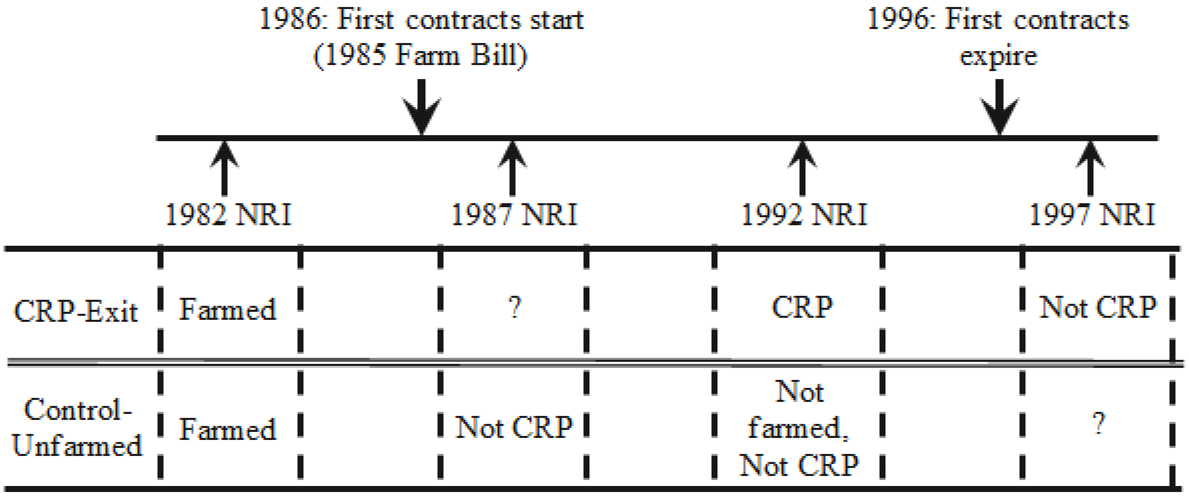
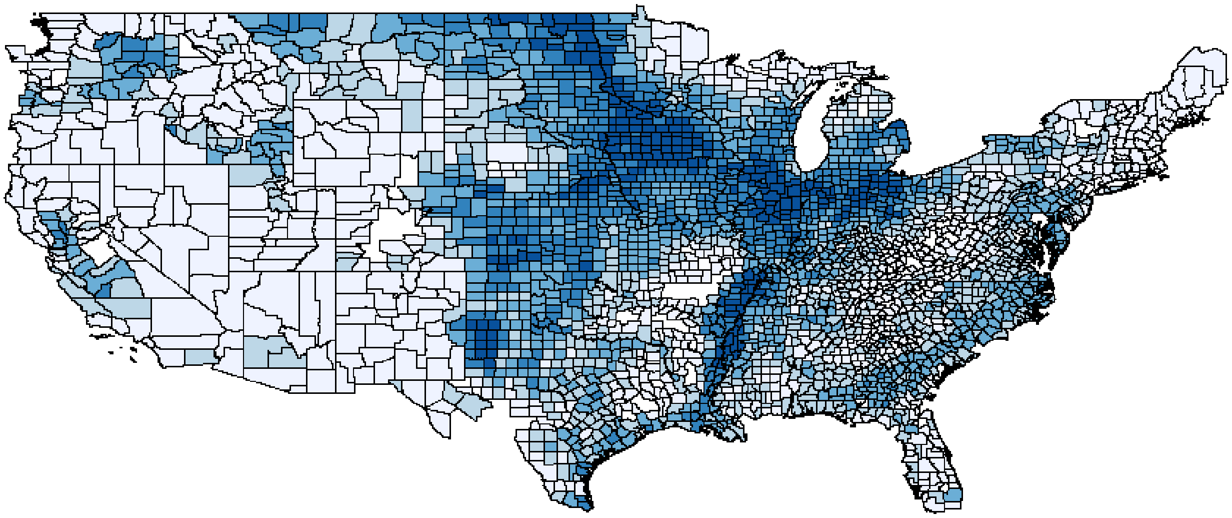


Figure 2: NRI and CRP timeline with land uses of specified subsamples



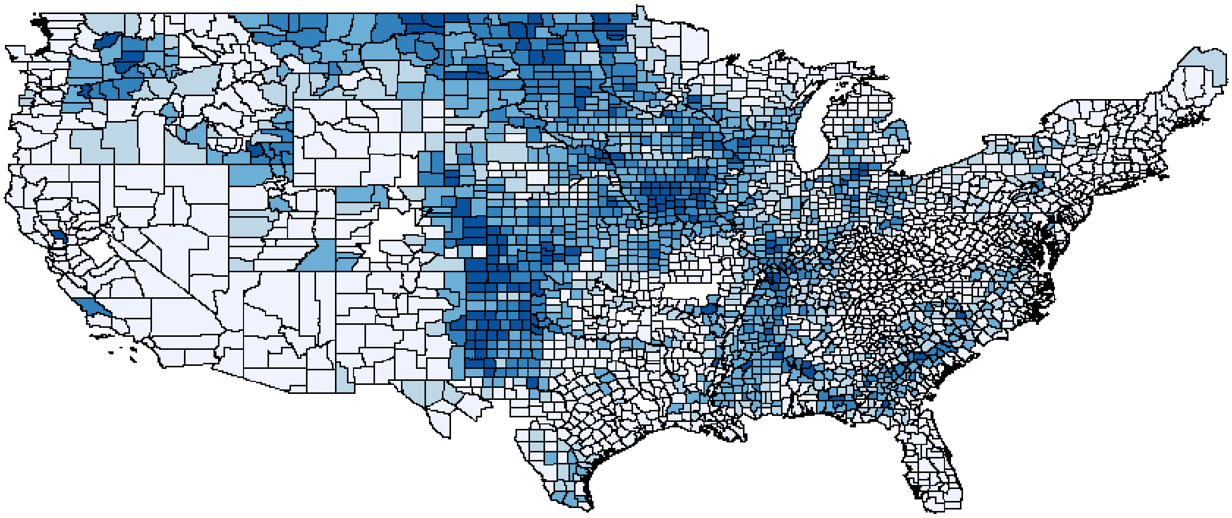
	1982 NRI	1987 NRI	1992 NRI	1997 NRI
CRP-Exit	Farmed	?	CRP	Not CRP
Control-Unfarmed	Farmed	Not CRP	Not farmed, Not CRP	?

Figure 3: Spatial distribution of land that was farmed in 1982



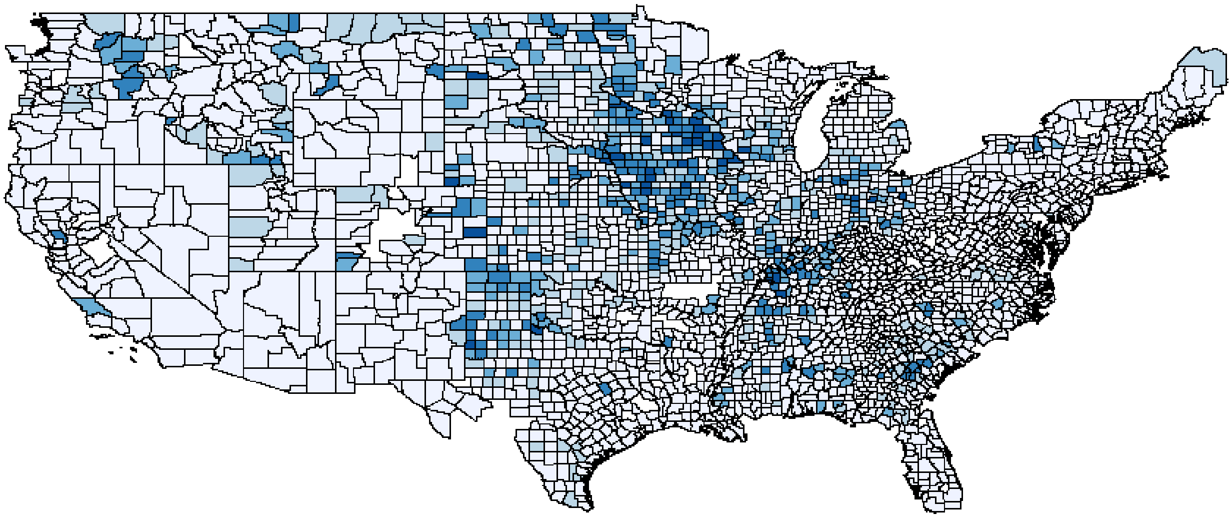
Shadings represent percent of county land area that was cultivated cropland in 1982. Shading levels from light to dark: 0-0.05, 0.05-0.2, 0.2-0.5, 0.5-0.75, 0.75-1. The maximum value is 0.9412.

Figure 4: Spatial distribution of land that was in CRP in 1992



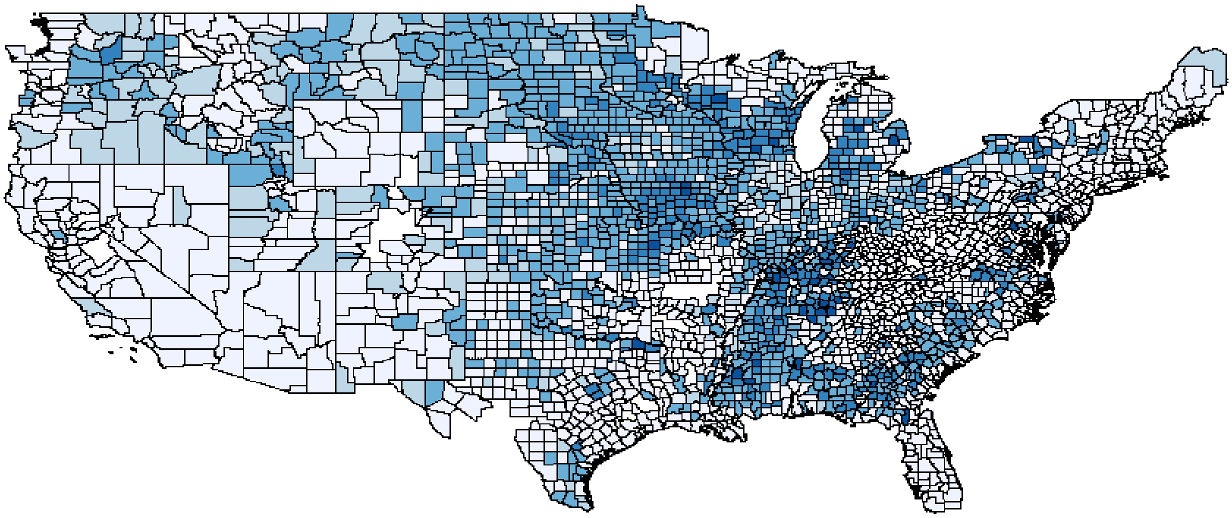
Shadings represent percent of county land area that was in CRP in 1992. Shading levels from light to dark: 0-0.001, 0.001-0.01, 0.01-0.05, 0.05-0.1, 0.1-0.3235 (the maximum value).

Figure 5: Spatial distribution of land in CRP-Exit



Shadings represent percent of county land area that was in CRP in 1992. Shading levels from light to dark: 0-0.001, 0.001-0.005, 0.005-0.01, 0.01-0.03, 0.03-0.0681 (the maximum value).

Figure 6: Spatial distribution of land in Control-Unfarmed



Shadings represent percent of county land area that was in CRP in 1992. Shading levels from light to dark: 0-0.001, 0.001-0.01, 0.01-0.05, 0.05-0.1, 0.1-0.1712 (the maximum value).

Table 1a. Parcel level land characteristics

	Farmed in 1982	CRP in 1992	CRP-Exit	Control-Unfarmed
Prime farmland? [a, b]	56.610	28.577	34.526	43.806
	(0.155)	(0.240)	(0.870)	(0.579)
Good land class? [a, b]	95.165	87.540	88.780	89.488
	(0.075)	(0.167)	(0.482)	(0.392)
Erodibility index [a, b]	7.551	12.672	12.441	11.101
	(0.026)	(0.071)	(0.208)	(0.169)
Slope [a, b]	2.777	4.232	4.736	4.048
	(0.009)	(0.019)	(0.086)	(0.042)
Early CRP signup wave (percent)	-	61.700	82.073	-
		(0.176)	(0.509)	
CRP in 1992 (percent)	8.170	1	1	-
	(0.028)	(0)	(0)	
CRP in 1997 (percent)	7.821	89.493	0	0.519
	(0.025)	(0.115)	(0)	(0.034)
Farmed in 1992 (percent)	82.127	-	-	-
	(0.088)			
Farmed in 1997 (percent)	79.018	5.299	52.771	27.135
	(0.098)	(0.105)	(0.702)	(0.520)
Conservation practice if farmed in 1997 (percent)	23.297 (0.161)	25.053 (1.291)	25.774 (1.259)	18.110 (0.864)
Observations	232,953	19,782	2,400	14,972
Average acres per sample point	16.117	17.208	13.090	14.115
	(0.025)	(0.087)	(0.169)	(0.099)
Hundreds of acres	3,754,446	340,400	31,417	211,329

Standard errors in parentheses

[a] Land characteristic data: 1997 prime farmland dummy, 1997 good land classification dummy for land capability classification of four or less, 1982 erodibility index (maximum of wind and water erodibility values), 1982 slope (Universal Soil Loss Equation (USLE) slope percent)

[b] Land characteristic data only available for certain land uses, so these are means over available data: erodibility not available for pasture; erodibility and slope not available for pasture, range, forest, other rural; erodibility, slope, prime, and land classification not available for urban, water, and federal land. For the rightmost two columns, this Table’s summaries in these columns include only observations with non-missing 1997 land classification and non-missing county rent data.

Table 1b. County level land characteristics

	Farmed in 1982	CRP in 1992	CRP-Exit	Control-Unfarmed
Population density (1996) (people per square mile)	76.066	29.152	40.340	64,681
	(0.421)	(0.218)	(1.536)	(1.033)
1986-1996 Δ population density	7.246	1.962	2.921	6.372
	(0.065)	(0.030)	(0.221)	(0.163)
Median household income (1997) (thousands of dollars)	32.781	30.843	32.009	31.898
	(0.016)	(0.010)	(0.088)	(0.097)
Poverty rate (1997) (percent)	14.255	15.297	13.886	14.806
	(0.017)	(0.010)	(0.073)	(0.078)
Unemployment rate (1996) (percent)	5.213	5.055	5.212	5.537
	(0.010)	(0.005)	(0.046)	(0.034)
Percent black (1996)	6.313	4.618	4.251	8.597
	(0.026)	(0.030)	(0.214)	(0.169)
Percent white (1996)	91.123	92.310	93.619	88.907
	(0.035)	(0.032)	(0.224)	(0.184)
Median age (1996)	35.837	36.322	36.139	35.520
	(0.011)	(0.006)	(0.059)	(0.048)
Percent under 18 (1996)	27.225	27.548	27.180	27.140
	(0.011)	(0.006)	(0.037)	(0.049)
Percent over 65 (1996)	15.645	16.352	16.019	15.127
	(0.012)	(0.006)	(0.068)	(0.061)
Observations	232,953	19,782	2,400	14,972
Average acres per sample point	16.678	17.208	13.090	14.115
	(0.028)	(0.087)	(0.169)	(0.099)
Hundreds of acres	3,754,446	340,400	31,417	211,329

Standard errors in parentheses

Table 2. Effect of CRP on 1997 farming

Specification:	(1) OLS	(2) Probit (marginal effects)	(3) OLS
CRP 1992	0.220***	0.254***	0.199***
	(0.010)	(0.012)	(0.010)
Prime 1997	0.066**	0.082**	0.091***
	(0.032)	(0.040)	(0.033)
Good land class 1997	-0.082**	-0.106*	-0.061
	(0.041)	(0.063)	(0.042)
Erodibility index 1982	-0.001	-0.001	-0.001
	(0.001)	(0.002)	(0.001)
Slope 1982	-0.014***	-0.023***	-0.012***
	(0.004)	(0.006)	(0.004)
Population density (1996)	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
1986-1996 Δ population density	-0.001***	-0.002***	-0.001***
	(0.000)	(0.001)	(0.000)
Median household income (1997)	0.005**	0.006**	0.005***
	(0.002)	(0.003)	(0.002)
Poverty rate (1997)	0.002	0.004	0.003
	(0.002)	(0.003)	(0.002)
Unemployment rate (1996)	-0.009***	-0.014***	-0.009***
	(0.002)	(0.004)	(0.002)
Percent black (1996)	-0.077	-0.119	0.019
	(0.095)	(0.132)	(0.097)
Percent white (1996)	-0.086	-0.112	0.015
	(0.100)	(0.129)	(0.100)
Median age (1996)	-0.005**	-0.006*	-0.008***
	(0.002)	(0.003)	(0.003)
Percent under 18 (1996)	0.666***	0.798***	0.760***
	(0.204)	(0.247)	(0.219)
Percent over 65 (1996)	1.263***	1.474***	1.324***
	(0.224)	(0.313)	(0.248)
Constant	-0.135		-0.251
	(0.182)		(0.179)
1996 county-level rent estimates	Yes	Yes	Yes
1986-1996 county-level rent change estimates	Yes	Yes	Yes
Land characteristic x rent and rent change interactions	Yes	Yes	Yes
Fixed Effects	Region	Region	State
n (observations)	17,383	17,383	17,383
N (hundreds of acres)	242,928	242,928	242,928
F	72.41	31.09	71.73

Standard errors in parentheses; * 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. CRP land limited to land that exited in 1997; non-CRP land limited to land not farmed in 1992. Survey regressions performed with data points weighted (using pweight) by the NRI variable xfact and errors clustered at the NRI "cluster" (which is sub-county) level. Region fixed effects are USDA Farm Production Regions, as discussed in footnote 17. Parcels that are the single observation for an NRI sampling stratum are excluded.

Table 3. Robustness checks on effect of CRP on 1997 farming

Specification:	(1) Good Land Classification (All Land)	(2) Prime Land (All Land)	(3) Good Land Classification (Control Only)	(4) Prime Land (Control Only)	(5) Highly Erodible Land (Control Only)
CRP 1992	0.230***	0.255***	0.215***	0.170***	0.327***
	(0.011)	(0.016)	(0.010)	(0.013)	(0.010)
Prime 1997	0.075**		0.069**		0.022
	(0.033)		(0.033)		(0.032)
Good land class 1997					-0.098**
					(0.040)
Erodibility index 1982	-0.001	0.004	-0.002	-0.001	-0.003***
	(0.002)	(0.004)	(0.001)	(0.003)	(0.001)
Slope 1982	-0.015***	-0.012	-0.014***	-0.027***	0.001
	(0.005)	(0.014)	(0.005)	(0.007)	(0.004)
Population density (1996)	0.000	0.000	0.000	0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
1986-1996 Δ population density	-0.001***	-0.002***	-0.001***	-0.002***	-0.001*
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Median household income (1997)	0.004**	0.009***	0.006***	0.011***	0.003**
(thousands of dollars)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Poverty rate (1997)	0.002	0.006*	0.003	0.008***	0.003*
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Unemployment rate (1996)	-0.010***	-0.011***	-0.010***	-0.012***	-0.009***
	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)
Percent black (1996)	-0.119	-0.190	-0.127	-0.214	0.033
	(0.112)	(0.239)	(0.111)	(0.184)	(0.076)
Percent white (1996)	-0.091	-0.241	-0.111	-0.261	0.068
	(0.118)	(0.242)	(0.115)	(0.188)	(0.082)
Median age (1996)	-0.005**	-0.002	-0.005**	-0.001	-0.005**
	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)
Percent under 18 (1996)	0.675***	0.159	0.652***	0.360	0.674***
	(0.222)	(0.374)	(0.219)	(0.306)	(0.182)
Percent over 65 (1996)	1.220***	1.361***	1.319***	1.488***	1.015***
	(0.241)	(0.471)	(0.231)	(0.339)	(0.194)
Constant	-0.231	-0.283	-0.247	-0.406	-0.282*
	(0.200)	(0.312)	(0.198)	(0.265)	(0.159)
1996 county-level rent estimates	Yes	Yes	Yes	Yes	Yes
1986-1996 county-level rent change estimates	Yes	Yes	Yes	Yes	Yes
Land characteristic x rent and rent change interactions	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Region	Region	Region	Region	Region
n (observations)	15,625	7,796	15,862	9,326	13,300
N (hundreds of acres)	217,165	103,535	220,690	124,105	175,138
F	72.22	55.44	76.57	83.96	106.16

Standard errors in parentheses; * 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. CRP land limited to land that exited in 1997; non-CRP land limited to land not farmed in 1992. Survey regressions performed with data points weighted (using pweight) by the NRI variable xfact and errors clustered at the NRI “cluster” (which is sub-county) level. Region fixed effects are USDA Farm Production Regions, as discussed in footnote 17. Parcels that are the single observation for an NRI sampling stratum are excluded.

Table 4. Effect of CRP on 1997 conservation practice use

Specification:	(1) OLS	(2) Probit (marginal effects)
CRP 1992	0.044**	0.042*
	(0.022)	(0.022)
Prime 1997	-0.004	0.043
	(0.072)	(0.089)
Good land class 1997	0.012	-0.155
	(0.170)	(0.283)
Erodibility index 1997	0.054***	0.056**
	(0.020)	(0.027)
Slope 1997	-0.044***	-0.067***
	(0.014)	(0.024)
Population density (1996)	0.000	0.000
	(0.000)	(0.000)
1986-1996 Δ population density	-0.002*	-0.002
	(0.001)	(0.001)
Median household income (1997) (thousands of dollars)	-0.001	-0.004
	(0.004)	(0.005)
Poverty rate (1997)	-0.007	-0.010
	(0.005)	(0.007)
Unemployment rate (1996)	0.026***	0.022***
	(0.009)	(0.008)
Percent black (1996)	0.168	0.546
	(0.178)	(0.333)
Percent white (1996)	0.233	0.488
	(0.186)	(0.333)
Median age (1996)	0.000	0.002
	(0.006)	(0.006)
Percent under 18 (1996)	-1.156**	-1.279**
	(0.457)	(0.510)
Percent over 65 (1996)	-0.450	-0.713
	(0.578)	(0.640)
Constant	-0.048	
	(0.456)	
1996 county-level rent estimates	Yes	Yes
1986-1996 county-level rent change estimates	Yes	Yes
Land characteristic x rent and rent change interactions	Yes	Yes
Fixed Effects	Region	Region
n (observations)	2,386	2,386
N (hundreds of acres)	37,181	37,181
F	7.83	4.03

Standard errors in parentheses; * significant at 10%, ** significant at 5%; *** significant at 1%

All included land was not highly erodible (had erodibility index of 8 or lower).in 1997, was cultivated in 1982 and 1997, is in a county for which rent data is available, and does not enter 1997 land uses: urban/built-up, water, or federal. CRP land limited to land that exited in 1997; non-CRP land limited to land not farmed in 1992. Survey regressions performed with data points weighted (using pweight) by the NRI variable xfact and errors clustered at the NRI “cluster” (which is sub-county) level. Region fixed effects are USDA Farm Production Regions, as discussed in footnote 17. Parcels that are the single observation for an NRI sampling stratum are excluded.

Appendix: Additional Land Characteristic Information

Table A-1. Additional land characteristics

	Farmed in 1982	CRP in 1992	CRP-Exit	Control-Unfarmed
PARCEL-LEVEL CHARACTERISTICS				
CRP cover of grass in 1992 (percent)	-	90.568	91.982	-
		(0.067)	(0.382)	
Corn in 1982 (percent)	24.386	11.019	24.585	22.156
	(0.108)	(0.088)	(0.847)	(0.451)
Soy in 1982 (percent)	17.842	11.557	18.245	16.765
	(0.096)	(0.141)	(0.671)	(0.395)
Wheat in 1982 (percent)	23.726	31.519	24.277	17.407
	(0.130)	(0.117)	(0.676)	(0.472)
Summer fallow in 1982 (percent)	7.475	14.867	10.058	4.175
	(0.097)	(0.113)	(0.369)	(0.344)
COUNTY-LEVEL CHARACTERISTICS				
1996 return: crops [a]	$93.501	$80.743	$96.169	$98.626
	(0.149)	(0.097)	(0.754)	(0.820)
1996 return: gov't payments [a]	$10.973	$8.856	$9.364	$9.023
	(0.014)	(0.010)	(0.063)	(0.061)
1996 return: pasture [a]	$29.118	$27.054	$30.967	$31.005
	(0.064)	(0.024)	(0.181)	(0.308)
1996 return: range [a]	$10.886	$11.226	$8.937	$10.553
	(0.032)	(0.027)	(0.201)	(0.145)
1996 return: forest [a]	$11.772	$12.189	$12.662	$14.453
	(0.043)	(0.011)	(0.140)	(0.181)
1996 return: urban [a]	$2,339.625	$2,426.962	$2,450.342	$2,260.462
	(4.949)	(3.703)	(31.431)	(21.928)
1986-1996 Δ return: crops [a]	$81.313	$70.596	$87.583	$90.371
	(0.178)	(0.117)	(0.849)	(1.148)
1986-1996 Δ return: gov't payments [a]	-$19.879	-$17.631	-$21.141	-$17.096
	(0.027)	(0.015)	(0.200)	(0.114)
1986-1996 Δ return: pasture [a]	$33.179	$31.655	$36.209	$37.758
	(0.083)	(0.032)	(0.297)	(0.439)
1986-1996 Δ return: range [a]	$0.121	-$0.099	$0.056	-$0.194
	(0.009)	(0.004)	(0.063)	(0.037)
1986-1996 Δ return: forest [a]	$6.371	$6.693	$7.238	$8.188
	(0.032)	(0.007)	(0.114)	(0.130)
1986-1996 Δ return: urban [a]	-$71.557	-$171.874	-$80.735	-$15.242
	(1.272)	(0.604)	(6.363)	(6.343)
Observations	232,953	19,782	2,400	14,972
Average acres per sample point	16.678	17.208	13.090	14.115
	(0.028)	(0.087)	(0.169)	(0.099)
Hundreds of acres	3,754,446	340,400	31,417	211,329

Standard errors in parentheses

[a] 1996 land use returns and 1986-1996 changes in returns, in 2001 dollars (PPI adjusted)