Do Decoupled Payments Stimulate Production? Estimating the Effect on Program Crop Acreage Using Matching

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Abstract. This study uses matching to evaluate the effect of decoupled payments on the acreage response of Iowa farmers who were in business in 1997 and 2002. Using farm-level panel data from the U.S. Agricultural Census, we examine whether farmers receiving high levels of 1997 agricultural payments per acre had a greater increase in program crop acreage between 1997 and 2002 than farmers receiving low levels of payments. The panel data set allows for conditioning current acreage on past individual acreage and operator characteristics. The large and exhaustive sample allows for comparisons across similar farms. The matching methodology avoids distributional and functional form assumptions about the relationship between the treatment and outcome. Results are consistent with other recent empirical estimates that suggest small but statistically significant effects of decoupled payments on production.

Key words: decoupled payments, supply response, government payments, program crops, trade policy

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1. Introduction

Some agricultural policy reforms, including the 1996 Federal Agricultural Improvement and Reform (FAIR) Act, seek to minimize production distortions by giving farmers lump-sum payments that are not tied to production decisions or prices. The extent to which these lump-sum or "decoupled" payments actually affect production has been a significant dispute among academics and in recent World Trade Organization negotiations (FAO, 2005; Sumner, 2005). Some developing nations contend that the large level of agricultural payments in the United States significantly influences production and trade. The U.S. has maintained that these payments are minimally trade distorting (USTR, 2004).

Economic theory suggests that lump-sum payments have no effect on production when markets are complete. However, under imperfect labor, credit, or insurance markets, decoupled payments could influence supply (Burfisher and Hopkins, 2003; Chau and de Gorter, 2000; Hennessy, 1998; Roe, Somwaru, and Diao, 2003). Recently, there have been several econometric studies examining the link between lump-sum payments and production (For a review see OECD, 2005). Some of these studies have examined the production responses to post-FAIR Act payments. Though post-FAIR Act payments may not be truly lump sum, the studies generally find a positive association between these mostly decoupled payments and production, though the magnitude of this association varies substantially (Adams, et. al, 2001; Goodwin and Mishra, 2005; Goodwin and Mishra, 2006; Key, Roberts, and Lubowski, 2005).

In this paper we take a new empirical approach to evaluate the effect of the farm-level supply response to decoupled payments. We use panel data from the 1997 and 2002 U.S. Agricultural Census to explore whether 1997 agricultural payments induced farmers to increase land allocated to program crops. Census data includes information on the amount of land allocated to particular crops and total government payments. A matching estimator based on the propensity score is used to compare the change between 1997 and 2002 in program crop acreage of farms with a high level of 1997 payments per acre to similar farms with a low level of payments. This allows us to measure how much the essentially "decoupled" 1997 agricultural payments influenced production at the farm level. The findings should help inform debates on future agricultural policy reforms and trade negotiations.

The paper offers several contributions to the empirical literature. First, the exhaustive nature of the Census data used - which includes essentially all U.S. producers of program crops – minimizes measurement errors associated with sample design and response rates. The large sample size and the fact that we limit the sample to a relatively homogenous group of farmers in Iowa, allow for comparisons across very similar farms. Second, most previous studies have relied on a single survey to estimate a cross-sectional relationship between payments and plantings. If factors correlated with both payment levels and planted acreage are unobservable, estimates could be biased. The farm-level panel data set used in this study allows us to estimate how different payment levels affect subsequent changes in planting for individual farms. While payment levels are likely correlated with contemporaneous plantings, there is no reason to expect payments to be correlated with future changes in plantings. Third, the matching methodology provides a way to estimate the effect of payments on cropland allocation that does not require distributional and functional form assumptions about the relationship between the treatment and outcome.

2. Empirical Model

Sources of Payment Variation

The empirical approach is to compare the change in program crop acreage of observationally similar farms with different levels of government payments per acre. A key assumption of this approach is that after controlling for observable differences between farms, differences in the payments per acre across farms are not associated with unobservable factors that influence the change in program crop acreage over time – in other words there is no sample selection bias.[1] We maintain that this assumption is reasonable since much of the variation in payments across observationally similar farms (having the same acres of program crops, located in the same county, the same-age operator, etc.) results from a largely exogenous ex-ante variation in government payments resulting from differences in base acres and base yields.

Payments per acre harvested (including base and nonbase acres) vary randomly across similar farms in part because of random variation in how a farm's land is classified – base versus nonbase and which type of base. In the 1980's, program participation came with many restrictions: it required farmers to limit their plantings to a share of acres historically planted to program crops (called 'base acres') and required a certain portion to be set-aside (left fallow). These costly restrictions caused some farmers not to participate. In addition, some farmers may have strategically chosen not to participate in order to 'build base' (payment-qualifying) acres in anticipation of higher future payments. Because payments are tied to historical plantings, and participation required farmers to limit plantings, some may have chosen not to participate in order to expand acreage and increase expected future payments. Because payments in future

[1] More specifically, there is no selection bias among the sample of continuing operations. The sample does not include farms that exited or entered between 1997 and 2002, as discussed in the following section.

years were tied to historical plantings, historical plantings were tied to participation decisions, and participation varied somewhat across producers, so did payments. Base acres could also vary due to chance variation in rotations between 1981 and 1985, particularly since soybeans were not considered a program crop at that time. It is unlikely that program participation decisions in the early 1980's should be systematically associated with changes in acreage between 1997 and 2002, especially since the cropland could have changed hands over that period of time.

A second source of variation in payments per acre for similar farms is differences in base yields, which affect payment levels on base acres. Base yields were determined by realized yields between 1981 and 1985. While yields are clearly tied to land quality, they also vary widely from year-to-year and across space, due to weather outcomes. Indeed, summary statistics reported by Roberts, Key and O'Donoghue (2006) indicate that field-level yields are typically 30% to 50% above or below their mean, and the county-level yield shock accounts for only about half the field-level variability. Thus, some variation in base yields, even locally, is likely random.[2]

We reduce the likelihood that there are differences in relevant unobservable factors between the treatment and control groups by choosing a homogenous sample of farmers. Even before matching, the characteristics of the operations in our sample with high and low payments per acres are very similar, as we would hope for in an ideal study.

Supply response

This study compares the effect of largely decoupled government payments in 1997 on subsequent (2002) program crop acreage. A general farm-level model of program crop acreage response over a five-year period can be expressed as:

$$A_t = f\left(A_{t-5}, AC_{j,t-5}, P^*_{t-1}, G_{t-5}, W_{t-5}, z_{t-5}\right)$$

Where A_t is program crop acreage in year *t* (2002) and A_{t-5} is program crop acreage in the previous census year (1997). $AC_{j,t-5}$ is acreage of crop *j* in 1997, which is included as a proxy for land quality. Output is a function of the expected output and input prices at the time of planting, P^*_{t-1}. In the econometric specification used in this paper we include county fixed effects, which should capture the effect of price variation across counties. Price variation within a county is likely to be small. In addition, the response of the dependent variable, the *aggregate* program crop acreage, to within-county price variation is likely to small. Wealth in the lagged period W_{t-5}, is included to allow for the possibility that risk preferences vary with wealth, which in turn could affect production decisions. As a proxy for wealth we include total agricultural sales, which should be correlated with agricultural income.[3] Other operator and operation characteristics z_{t-5} that might influence acreage decision include the operator age and age-squared, and a measures of land quality.

[2] We do not observe base yields or base acres in the Census data so we cannot use these as instrumental variables.
[3] The census of agriculture does not provide a good measure of wealth. One possible proxy is the value of land and building on the operation, but this is only available on the "long form" census questionnaire, which was distributed to only about one-third of all operators.

To minimize issues of measurement error we define a discrete treatment variable D_{t-5}: indicating payments per acre of program crops harvested in 1997 above the sample median. The control group receives payments per acre in 1997 below the median. We first estimate a model where the functional relationship between the independent variables is assumed linear (OLS):

$$A_t = f\left(A_{t-5}, AC_{j,t-5}, D_{t-5}, W_{t-5}, z_{t-5}\right) + \varepsilon$$

A problem with the linear regression model is it imposes strict distributional and functional form assumptions about the relationship between the treatment and outcome. Matching provides a more flexible approach.

Matching

We want to estimate the average effect of a binary treatment (high government payments per acre) on a continuous scalar outcome (program crop acreage harvested). Dropping the time notation for simplicity, let A_{0i} and A_{1i} denote the two potential outcomes such that A_{0i} is the outcome (acreage) of individual *i* not exposed to the treatment, and A_{1i} is the outcome if exposed to the treatment. We are interested in the sample-average treatment effect for the treated (what was the additional crop acreage harvested for those receiving high payments compared to what it would have been had they received low payments), which is given:

$$\tau_T^S = \frac{1}{N_T} \sum_{i:D_i=1} \left(A_{1i} - A_{0i}\right),$$

where N_T is the number of treated individuals and the where $D_i = \{0,1\}$ is the treatment indicator.

In most policy analysis settings, the sample-average treatment effects cannot be computed because we only observe one of the two possible outcomes for each individual. For example, if an individual was exposed to the treatment, then we observe A_{1i}, but we do not observe what the outcome would have been had the individual not received the treatment (A_{0i}). The basic idea behind the matching estimator is to estimate A_{0i} using the average outcomes of similar individuals who were not exposed to the treatment. Analogously, if we observe the outcome for an individual who did not receive a treatment, then we can estimate A_{1i} using the average of outcome of similar individuals who were exposed.

Matching estimators compare outcomes across pairs of "similar" treated and control units. Ideally, pairs would be matched on all relevant observable variables. In practice, matching subjects on a vector of characteristics is not computationally feasible with a large sample when the number of characteristics is large. Propensity score matching is a method that summarizes the characteristics of each observation into a single index (the propensity score) to make matching feasible. Since Rosenbaum and Rubin (1983) proposed matching individuals based on their propensity score – that is, on their probability of receiving the treatment – these methods have become increasingly popular in the evaluation of economic policies and medical trials.

The propensity score is defined as the conditional probability of receiving a treatment given pre-treatment characteristics *X*:

$$P(X) \equiv \Pr(D = 1 | X) = E[D | X],$$

Rosenbaum and Rubin (1983) show that if exposure to treatment is random within cells defined by *X* then it is also random within cells defined by the values of $P(X)$. Hence, if the propensity score is known, then the average treatment effect can be estimated as the expected difference in outcomes between individuals receiving the treatment and not receiving the treatment, conditional on the having the same propensity score. The average effect of treatment on the treated (ATT) can be estimated as:

(1) $$\tau^T = E\left[E\left[A_{1i} \middle| D_i = 1, P(X_i)\right] - E\left[A_{0i} \middle| D_i = 0, P(X_i)\right] \middle| D_i = 1\right]$$

A probit (or other standard probability model) can be used to estimate the propensity score:

$$\Pr\left[D_i = 1 \middle| X_i\right] = \Phi(h(X_i)),$$

where Φ denotes the normal c.d.f. and $h(X_i)$ is a function of the covariates.

Since the propensity score is a continuous variable, the probability of having two observations with the same value of $P(X)$ is zero. Consequently, an estimate of the propensity score is not sufficient to estimate (1). We use two methods to overcome this problem: stratification matching (or blocking) and nearest neighbor matching. With stratification matching the range of the propensity score is divided into intervals such that within each interval the treated and control observations have the same average propensity score. Then, within each interval the difference between the average outcomes of the treated and controls is computed.

The ATT is weighted average of the average differences in each block, with the weights given by the frequency of the treated observations.

The nearest neighbor method matches each treated observation with the control unit with the closest propensity score. Once each treated unit is matched, the difference between the outcome of the treated and untreated units is computed, and the ATT is the average of these N_T differences.

For both methods, the ATT estimator is essentially the difference between two sample means, so the variance is calculated using standing methods for differences in means. Details on the implementation of the stratification and nearest neighbor methods used in this study are given in Becker and Ichino (2002).

3. Data

Farm-level data are from the Agricultural Census maintained by the USDA National Agricultural Statistics Service. Census data on farm and operator characteristics are collected every five years from essentially all farms in the country. Since every farm operator must respond to the survey (by law), we can track operations across time, as long as they remain in business. Each respondent receives a unique Census File Number (CFN) to track the farm, ranch, or other agricultural entity controlled or operated by the individual filing the census.

To reduce heterogeneity, this study examines only operations in Iowa that harvested at least 50 acres in program crop in 1997 that also remained in business in 2002. In 1997, a total of 80,402 farmers responded to the census in 1997 in Iowa. Of these 44,269 harvested at least 50 acres of "program crops" (corn, wheat, barley, oats, rice, cotton, and sorghum) in 1997. Because of the special rules that applied to soybeans in 1997, we also separately analyze the effect of payments on program crop acres plus soybean acres. Of these operators, 30,068 remained in business after five years, as indicated by their responding to the 2002 census. To further reduce heterogeneity, and to allow us to use corn yields as a measure of land quality, we dropped the 16 farms that did not grow corn for grain, leaving a final sample of 30,052 farms.

Government payments are defined as total payments received for participation in Federal farm programs (not including Commodity Credit Corporation loans or crop insurance payments) net of payments received for participation in the Conservation Reserve Program and the Wetlands Reserve Program.[4] In 1997, these payments derived almost entirely from Production Flexibility Contracts (PFC), which were tied to historically enrolled contract acreage, not current plantings (USDA, 2008). For the program acreage analysis, the treatment group consists of those that received payments that were above \$33.03 per acre of program crops harvested in 1997. For the program acreage plus soybeans acreage analysis, the cutoff is \$18.44 per acre of program crops plus soybeans.

Table 1 provides a comparison of the mean values of variables used in the analysis for the payments per program crop acre categories. Note that for most of the operator and operation characteristics, differences between the averages of the treatment and control groups are generally not large, and in most cases would not be considered economically significant (though some are statistically significantly different). The first six rows suggest that payments could play an economically significant role in acreage response. The treatment (high payments per acre)

[4] In the 1997 census respondents were asked for the "total amount received for participation in Federal farm programs (not including CCC loans)." Respondents were also asked to provide "how much was received for participation in the Conservation reserve program and Wetlands Reserve Program (CRP and WRP)?" The latter was subtracted from the former to obtain the measure of payments used in this study.

group increased the amount harvested by 17.5 acres compared to 2.6 acres for the treatment group. In terms of program crop plus soybean acreage, the treatment group increased their acreage by 36.8 acres compared to 25.2 acres for the control.

4. Results

The first two columns of Table 2 present the results of the linear regression model with 2002 program crop acres as the dependent variable. Since we control for 1997 program crop acres, the positive (negative) coefficients can be interpreted as increasing (decreasing) program crop acreage between 1997 and 2002. Farmers that had more land in farms, and that harvested more acres of soybeans, oats, and silage, and fewer acres of hay in 1997 were significantly more likely to increase their program crop acres between 1997 and 2002. The age of the operator was also important. Program crop acreage expanded with age, but at a decreasing rate. Corn grain yields, the proxy for land quality, was also positively correlated with an expansion of program crop acreage. Of greatest interest for this analysis, the results indicate that being in the high payments per acre category was associated with 12.0 additional acres in program crops over five years.

The third and fourth columns of table 4 give the results for program crop plus soybean acres as the dependent variable. The results of this analysis are very similar: being in the high payments per acre category (where the acreage now includes soybeans) was associated with 16.3 additional acres of program crops and soybeans over five years.

Table 3 presents the coefficients of the probit models of the propensity scores. The results indicate many statistically significant variables, but the model explains only a small share of the variation in the dependent variable. This is consistent with our maintained assumption that after controlling for observables, differences in payments per acre in 1997 are caused largely by random factors that determined base yields and base acres in 1981-1985. In other words, if random events caused variation in payments per acre in observationally similar farms, then we could not expect to explain a farm's placement in the high or low payments category with observables. In other words, the explanatory variables in the probit model cannot explain a lot of the variation in the dependent variable because random error associated with the model is large.

Table 4 gives the estimated average treatment effect for the stratification and nearest neighbor matching models, for program crops and for program crops plus soybeans. The coefficients of the matching approach can be interpreted the same way as the indicator in the linear model. The estimates imply that the treatment (high payments per acre) caused farmers to increase their program crop acreage 13.3 – 16.8 acres (5.3 - 6.7 percentage points) more than they would have had they received the control (low payments per acre) between 1997 and 2002. Estimates also indicate that the treatment increased program crop plus soybean acreage by 17.2 - 18.9 acres or 3.6 - 4.0 percentage points.

These results can be compared to those of other studies. The treatment implies an average 246% increase in payments per harvested acre of program crops (from $15.64 to $54.12 per acre). This implies response elasticity of 0.022 – 0.027. In other words, a 10-percent increase in payments per acre would result in a 2.2 – 2.7% increase in farm-level supply (land in production) over five years. With soybeans, the elasticity estimates are 0.014 – 0.015. These findings are consistent with those of recent studies. Goodwin and Mishra (2006), who pooled several cross-sectional surveys of farms in the Heartland from 1998-2001, found acreage elasticities for corn and soybeans of about 0.01 to 0.03 (p.87) for decoupled AMTA payments.

5. Conclusion

This study used farm-level panel data from the US Agricultural Census to examine the acreage response over five years of continuing operations to payments that were largely decoupled from production. Specifically, it examined whether farmers receiving high levels 1997 agricultural payments per acre increased the quantity of land allocated to program crops in 2002 relative to those receiving lower levels of payments per acre. The panel data set allows for conditioning current acreage decisions on past individual acreage and operator characteristics. The matching methodology used does not require distributional and functional form assumptions about the relationship between the treatment and outcome. For continuing operations, results indicate that the growth of total program-crop acreage for farmers receiving a high level of payments would be 2.6 to 6.7 percentage points above that for farmers with low payments, depending on the model specification and definition of program crops. These results imply payment acreage elasticities of between 0.014 and 0.027. Our results are consistent with other empirical estimates that suggest small but statistically significant farm-level effects of the decoupled payments on production.

It is important to emphasize that this study did not account for the acreage decision of farms entering production or the effect of payments on farm exit rates, so it is not possible to ascertain how payments would influence total program crop acreage. Furthermore, to the extent that payments cause an increase in plantings of program-related crops, crop prices would fall, thereby attenuating an overall increase in aggregate plantings. However, for continuing farm operations, the results suggest that largely decoupled government payments had a small positive effect on the farm-level supply of program crops. Because the effect of payments on production is uneven across farms, the effect of farm payments on farm structure could be substantial. Recent studies have shown that government payments might contribute to increased concentration of production on large operations (Roberts and Key, forthcoming; Key and Roberts, 2007).

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Table 1. Descriptive Statistics and Test of Equality of Means by Payment Category, Iowa

Variables	Payments per program crop acre Low (control)	 High (treatment)	 t-statistic
1997 program crop (acres harvested)	249.3	237.8	4.42
2002 program crop (acres harvested)	251.9	255.3	-1.04
Change in program crop acres harvested	2.6	17.5	-7.36
1997 program crop + soybean (acres harvested)	446.9	441.6	1.09
2002 program crop + soybean (acres harvested)	472.1	478.4	-1.04
Change in program crop + soybean acres harvested	25.2	36.8	-3.24
Government payments ($)	4,055	12,288	-56.17
Gov. payments per program crop acre	15.64	54.12	-141.7
Gov. payments per program crop + soybean acre	9.34	30.55	-127.4
Sales ($)	204,024	222,088	-5.16
Operator age (years)	49.3	50.8	-10.16
Land in farm (owned + rented in - rented out, acres)	567.9	554.3	2.44
Corn (field corn for grain, acres harvested)	243.9	234.2	3.74
Soybeans (acres harvested)	197.6	203.8	-2.5
Oats (acres harvested)	4.8	3.2	11.62
Hay (all types, acres harvested)	20.5	18.8	3.53
Silage (corn or sorghum for silage, acres harvested)	3.8	4.9	-4.99
Hogs (hog and pig inventory, head)	219.0	235.1	-1.81
Cattle (cattle and calf inventory, head)	58.5	59.0	-0.28
Corn grain yield (bushels per acre harvested)	128.7	133.3	-17.76
Observations	15,026	15,026	

Notes: All variables are for 1997 except where noted. Sample described in text. Definition of program crops and government payments given in text. The t-statistics is for the test of the null hypothesis of equal means from a pooled sample.
Source: 1997 and 2002 Census of Agriculture.

Table 2. OLS Regression

Dependent Variable:	2002 program crop acres		2002 program crop + soybean acres	
	Coefficient	t-value	Coefficient	t-value
Intercept	197.1	11.5	392.0	12.68
Treatment indicator (high/low payments per acre of program crops)	12.01	6.0	-	-
Treatment indicator (high/low payments per acre of program crops + soybeans)	-	-	16.34	4.46
1997 program crop acres	0.063	0.3	-	-
1997 program + soybean acres	-	-	-0.584	-1.69
Sales	0.00003	6.0	0.00001	5.06
Age of operator	-6.798	-13.2	-12.764	-13.71
Age-squared	0.044	9.0	0.083	9.28
Land in Farm	0.040	5.7	0.093	7.24
Corn acres	0.617	3.2	1.582	4.58
Soybean acres	0.240	19.3	1.346	3.89
Oat acres	0.425	2.0	0.910	2.37
Hay acres	-0.145	-4.4	-0.321	-5.43
Silage acres	0.169	2.5	0.060	0.49
Hog inventory	0.003	1.6	0.001	0.45
Cattle inventory	-0.016	-1.6	-0.070	-3.85
Corn yield	0.270	5.8	0.448	5.3
County fixed effects	yes		Yes	
R-square	0.65		0.67	
Observations	30,052		30,052	

Table 3. Probit model of high/low 1997 government payments per acre

Dependent Variable:	Treatment indicator (high/low 1997 payments per program crop acre)		Treatment indicator (high/low 1997 payments per program crop + soybean acre)	
	Coeff.	z-value	Coeff.	t-value
Intercept	-1.87461	-14.4	-1.79704	-13.72
1997 program crop acres	-0.00522	-3.21	-	-
1997 program crop + soybean acres	-	-	-0.00221	-1.48
Sales	2.02E-07	4.9	2.02E-07	4.84
Age of operator	0.038229	9.68	0.035722	9.02
Age-squared	-0.0003	-7.9	-0.00027	-7.1
Land in Farm	0.000104	1.94	9.38E-05	1.76
Corn acres	0.003731	2.29	0.002206	1.48
Soybean acres	0.001095	11.29	0.001465	0.99
Oat acres	-0.00302	-1.7	-0.00056	-0.34
Hay acres	0.000788	3.11	0.000653	2.6
Silage acres	0.002564	4.95	0.002179	4.23
Hog inventory	3.61E-06	0.32	4.41E-06	0.39
Cattle inventory	-0.00011	-1.47	-5.9E-05	-0.79
Corn yield	0.004439	12.53	0.003622	10.19
County fixed effects	yes		yes	
Pseudo R-square	0.04		0.06	
Observations	30,052		30,052	

Table 4. Average treatment effect of high payments per acre for those with high payments

Dependent Variable:	2002 program crop acres		2002 program crop + soybean acres	
	ATT	t-value	ATT	t-value
Stratification matching	13.33	3.99	18.93	3.04
Nearest neighbor matching	16.76	3.56	17.22	1.94