Submitted Article

Do Direct Payments Distort Producers’ Decisions? An Examination of the Farm Security and Rural Investment Act of 2002

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Abstract  Do direct payments alter operators’ acreage decisions? The authors use an event study with individual-level data in a panel-data setting to examine how an exogenous change in direct payments affects individual farmers’ production decisions. They track changes in acreage across time and examine whether an exogenous, government implemented Act that allowed farmers to update base acres altered individual operators’ acreage decisions. Results suggest that direct payments do change individual acreage decisions, ranging from approximately 44 to 78 acre increases (9 to 16% changes). Their results have no implications for aggregate production impacts.

Key words: acreage response, direct payments.

JEL Codes: Q12, Q18.

Introduction

Decoupled payments are generally assumed not to distort market outcomes.1 If nondistortionary, a government could use them in domestic policies without affecting either domestic or international markets. The United States uses direct payments, an agricultural payment almost entirely divorced from current production decisions, to help subsidize farmers.2 A burgeoning literature has arisen exploring the extent to which decoupled payments can be truly nondistortionary.

While decoupled payments have been explored in general, little research has examined the effect that U.S. direct payments have on production decisions. Previous research has drawbacks associated with the methods and data used, generally entailing endogeneity problems and not

1“Decoupled payment” here refers to a payment independent of current production decisions, outcomes, or prices.

2“Direct payment” here refers to the 2002 Farm Act payment that replaced the Production Flexibility Contract payment (previously known as the Agricultural Market Transition Act payment).
properly identifying the micro-level effects of direct payments on farmers' production decisions. We believe we are the first to use an event study with individual level data in a panel data setting to study the effects of changes in direct payments on individual farmers' production decisions. We track changes in acreage across time and use an exogenous event, the Farm Security and Rural Investment Act of 2002 (hereafter the 2002 Farm Act) that allowed farmers to update base acres, to impute causation and assess the impact that an exogenously driven change in direct payments had on operators' acreage decisions.

While our findings cannot shed any light on aggregate acreage supply response, our results suggest that direct payments alter the individual operator's acreage decision. Holding all else constant, estimates range from approximately 44 to 78 acre increases (approximately 9 to 16% changes) due to changes in direct payment receipts.

It is generally assumed that since direct payments are essentially lump sum payments, their use will not produce any market distortions. However, several theoretical avenues exist through which direct payments could alter behavior and, by extension, market outcomes. Excluding explicitly imposed restrictions, direct payments can affect agricultural production in three ways. First, if farmers face credit constraints, a direct payment may increase their access to borrowed capital. Second, direct payments that serve to increase wealth may lead to changes in the risk preferences of operators. Third, expectations about future payments could alter current production. If farmers believe future payments will be based on current production levels, they may increase production of the crops for which they expect to receive future payments (Young and Westcott 2000). While these theories help motivate the importance of studying the effects of direct payments, we focus on whether the payments cause changes in farmers' production decisions, not how.

Young and Westcott (2000) argue that government programs could result in a variety of outcomes, including aggregate land use changes, crop mix changes, or a combination of the two. Roberts and Key (2008) posit another potential outcome: changes in industry structure. Farm programs might help some farms to consolidate while other farms exit, thereby altering industry structure while potentially having minimal (if any) aggregate land use or crop mix effects.

If direct payments affect production, they could affect prices. If so, a portion of direct payment subsidies could get passed on to consumers (Adams et al. 2001; Westcott 2005). Additionally, if direct payments are price distorting they may also be trade distorting. Direct payments may also provide incentives to keep marginally productive land in agriculture (Adams et al. 2001; Anderson and Parkhurst 2004). The Farm Act required that base acreage remain in agriculture, even when not being farmed. This could prevent land from leaving the agricultural sector, even though efficiency would otherwise dictate its departure (Young and Westcott 2000). Alternatively, even if direct payments do not affect prices directly, they could promote changes in the structure of industry. Payments could encourage consolidation in agricultural markets, which could alter the way farming takes place in the United States.

While aggregate effects are important to understand, our approach takes the first step required to obtain a better understanding of the individual decisions farmers make. We cannot stress enough that we do not claim to study the aggregate effects of individual behavior distortions here.
Background

Under the Uruguay Round of the General Agreement on Trade and Tariffs, participating nations agreed to move away from trade distorting agricultural subsidies. Production Flexibility Contract (PFC) payments introduced in the 1996 Federal Agricultural Improvement and Reform Act met the specific criteria set forth in the Uruguay Round, allowing their classification as minimally distorting agricultural subsidies. The criteria established to “decouple” the subsidy from current production decisions required that: 1) historical production levels would determine the subsidy size; 2) current or future production decisions would not affect the subsidy level; and 3) payment receipts would not require production (Burfisher and Hopkins 2003).

The 2002 Farm Act continued commodity payments, renaming the PFC payments and acreage as “direct” payments and “base” acreage. The act increased the payment rates and gave operators three main options to update their PFC acreage to base acreage. Option one simply changed the name—base acres would equal the contract acreage that would have been used for 2002 PFC payments. Option two, with several variations, allowed farmers to add oilseed acreage—based on their average historic 1998–2001 plantings, with total base acreage not to exceed the average sum of acres planted—to program crops during these years. Option three allowed farmers to recalculate their direct payments using updated (1998–2001) acreage histories for all program crops (Young et al. 2005).

As agricultural support moves away from coupled support to decoupled support systems, more evidence of the effects of decoupled support systems become available. Some researchers have found little evidence of decoupled payments influencing production (Burfisher and Hopkins 2003; Goodwin and Mishra 2005). However, in a recent literature review, Bhaskar and Beghin (2007) argue that decoupled payments can influence farmers’ decisions by altering expectations about future payments, easing potential credit constraints, altering land values, affecting the risk faced by farmers, and/or altering labor markets.

Some economists have argued that expectations of future program payments may influence farmers’ current production decisions (Tielu and Roberts 1998; Sumner 2003; McIntosh, Shogren, and Dohlman 2007; Coble, Miller and Hudson 2008; Bhaskar and Beghin 2008).

Tielu and Roberts (1998) also suggested that wealth-increases due to decoupled payments may lead to increased access to capital. Roberts, Kirwan, and Hopkins (2003), Goodwin, Mishra, and Ortalo-Magné (2003), Roe, Somwaru, and Diao (2003), and Kirwan (2009) all concluded that direct payments caused an increase in agricultural land values, which would increase farm wealth and operators’ access to capital if credit constraints existed.

Several models examining the effects of direct payments on land use and land values have also been created. Gohin, Guyomard, and Le Mouel (2000) argued that direct payments do alter land use and land values, noting that the size of the effect depended on other support measures and production technology used. Gohin (2006) examined the 2003 Common Agricultural Policy (CAP) Midterm Review, allowing for different capitalization assumptions of direct payments. Simulating a drop in direct

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3Base acres for peanuts were designated separately from the other program commodities.
payments induced a substantial decrease in land rents under both scenarios and resulting drops in production. Frandsen, Gersfelt, and Jensen (2003) used a computable general equilibrium model to examine how decoupling income supports in the European Union might affect production and trade outcomes. When modeling the conversion of domestic support into a region-specific decoupled payment to land, land prices increase substantially, resulting in a decrease in agricultural production.

Additionally, studies by Hennessy, Ridier, and Jacquet (1998), and Anton and Le Mouël (2004) suggested that decoupled income transfers could affect production decisions through farmers’ risk preferences. Serra et al. (2005) incorporated price uncertainty and showed that the presence of direct payments can reduce risk and increase production. Modeling output and price risk, Serra et al. (2006) found that direct payments can alter the use of inputs, thereby altering production outcomes. Burfisher, Robinson, and Thierfelder (2000) found small positive effects of decoupled income transfers on production decisions, but argued they were not likely to significantly impact overall production. Makki, Somwaru, and Vandevene (2004) argued that marginal wealth increases from decoupled payments are small, meaning that marginal changes in risk preferences will also be small. Additionally, they argued that operators would likely mitigate risk using insurance, hedging, and other management strategies, rather than altering production decisions.

Economists have also studied the effects of direct payments on labor markets (with mixed results), which could spill over to production effects. Ahearn, El-Osta, and Dewbre (2006) found that direct payments (called production flexibility payments, or PFC payments at the time) were negatively correlated with the farmer’s likelihood to work off the farm, while El-Osta, Mishra, and Ahearn (2004) found that PFC payments were positively correlated with on-farm labor hours and negatively correlated with off-farm labor hours. In contrast, Dewbre and Mishra (2002) concluded that PFC payments do not affect the farmer’s labor–leisure decision.

While many studies investigated the general effects of farm subsidies on agricultural production (Houck and Ryan 1972; Morzuch, Weaver, and Helmberger 1980; Chavas, Pope, and Kao 1983; Lee and Helmberger 1985; McIntosh and Shideed 1989; Choi and Helmberger 1993; Duffy, Shalishali, and Kinnucan 1994), fewer examined how decoupled payments might directly affect production decisions. Adams et al. (2001) found weak evidence of decoupled payments positively influencing the number of acres used in the production of major field crops in an aggregate setting. They presented the decision to plant acres as a function of per acre revenues and per acre decoupled payments (calculated by dividing PFC payments by total planted acres). If planted acres are not all linked to base acres, then per acre decoupled payments would be inversely related to the total acres, potentially underestimating the effects of decoupled payments on acreage decisions. Additionally, given their data constraints, they could not properly identify the causal effect of direct payments on production decisions.

Goodwin and Mishra (2006) used an acreage response model to conduct a similar investigation using individual operator data. They found that decoupled payments had a small but statistically significant positive effect on the quantity of acres used in the production of corn, soybeans, and wheat. In a similar piece, Goodwin and Mishra (2005) focused on the effects of direct payments under the 2002 Farm Act on micro-level acreage decisions. In this exercise, they found small but statistically insignificant positive effects.
Using 1998–2001 Agricultural Resource Management Survey (ARMS) data, Goodwin and Mishra constructed per acre decoupled payments in their 2006 paper. However, ARMS did not collect information on base acres in 1998–2001, meaning the authors must have divided total direct payments by total harvested acreage (since ARMS doesn’t collect information on planted acres either), regardless of whether the acreage was associated with base. This approach can lead to biased results.\(^4\) They also used various endogenous variables as explanatory variables, in particular with their key variable for the decoupled payments. Finally, most of their analyses relied on cross-sectional results, making it difficult to impute causality.

In a similar piece, Girante, Goodwin, and Featherstone (2008) examined how decoupled payments might affect production in the presence of credit constraints. They found that decoupled payments had little differential effect on various production decisions (including total crop acres, owned acres, and decisions to plant various crops) across farmers with different levels of creditworthiness (debt to asset ratio). Essential to the authors’ argument is the exogeneity of their measure of creditworthiness, which they point out is most likely endogenous, calling into question the validity of their results. Additionally, causality is also an issue, since an increase in acres could lead to an increase in direct payments and vice versa. However, overall, they concluded that decoupled payments have potentially distortionary effects on production.

Rather than focus on the acreage response of a small number of commodities or of farms located in one region, we investigate the effects of direct payments on total harvested acres for all crops while examining farmers’ decisions at the individual level.\(^5\) We include any farm that either produced program crops or received direct payments and we do not restrict our analysis to specific geographic regions. We derive an acreage response function from a representative farm operator’s profit maximization problem. Similar to previous studies, we present acres harvested as a function of input and output prices using per acre revenues and per acre costs and wealth. For robustness, we also present acres harvested as a function of the more fundamental input and output prices. In contrast to earlier work, we model acres harvested as a function of the change in the number of base acre dollars received due to the 2002 Farm Act, rather than as a function of the total direct payments received.

One major difficulty with previous work entails the endogeneity of the direct payments received. By purchasing or selling acres with base, the operator can alter payment receipts. Our approach eliminates the endogeneity of

\(^4\)Suppose, for example, that an operator increased his or her acreage of corn without base. This would increase the dependent variable (acres of corn harvested) and at the same time decrease the independent variable (direct payments per acre), which could easily lead to a spurious negative relationship. If an overall positive relationship did exist, it could bias the effect that direct payments per acre have on acreage decisions in a downwards fashion. See Table 4, specifications 1 and 2, to compare results using the direct payments variable versus the direct payments per acre variable.

\(^5\)Data limitations prevent us from using planted acres. However, correlations between planted and harvested acres are very high for all crops in all years, so we don’t believe that using harvested acres instead of planted acres introduces any biases in our results. To ensure that using harvested acres wouldn’t alter our results, we also constructed estimates of planted acres using state-level planted and harvested data for each crop (available from the National Agricultural Statistics Service). We created a ratio of planted to harvested acres and multiplied each farmers’ reported harvested acres by this ratio for each crop. Summing across all crops gave us an estimated level of planted acres for each farmer. Results remain unchanged and are available upon request.
this variable by focusing exclusively on the change in direct payments received by operators due to the implementation of the 2002 Farm Act.

Methodology

To identify how direct payments affect farmers’ production decisions, we examine the acreage decision as the production decision for two reasons: 1) it is the standard decision examined in earlier work; and 2) acreage changes are readily recognized and measured.

The 2002 Farm Act allowed operators to update their base acres. This updating was both exogenous and purely voluntary since the farmer’s choice to update cannot affect any current production decisions. The exogenous policy change therefore provides us with a way to measure and identify the impact of direct payments on acreage decisions properly.

To estimate the effect of payments on producers’ decisions, we explore the changes in the number of acres before and after the implementation of the 2002 Farm Act. We effectively use a differences-in-differences (DiD) approach to measure how the exogenous shock affected farmers, a widespread approach in economics used to understand better the effects of policy changes.

The basic DiD approach relies upon two groups of observations: a control group and a treatment group. The variable of interest is measured both pre- and posttreatment for both groups. The change in the variable is then calculated for each group, creating a measure of the overall change in the variable over time for each group. The difference calculated for the control group is then subtracted from the difference calculated for the treatment group, allowing researchers to obtain unbiased estimates of the actual treatment on the individuals.

This approach is most easily implemented in laboratories, while real world constraints often prevent empirical economists from constructing such parsimonious experiments. Our “treatment” (the exogenous policy shock) could affect all farmers that grew program crops or owned base acres. However, the degree to which each farmer was exposed to the policy shock differed, making the treatment a continuous variable rather than a dichotomous one. The variation in exposure to the treatment across observations is what allows us to measure these differences and properly identify the effect of the policy change on farmers’ decisions.

For producer $i$ ($i = 1, \ldots, N$) in time period $t$ ($t = 1, 2$), let $Y_{it}$ represent the number of acres of cropland harvested. Let $Y_{it}$ be a function of a set of factors $X_{it}$ that characterize the farm and the producer that influence the propensity to alter acreage levels. Let $DP$ measure the level of direct payments the producer received. This gives us an equation for estimating, one that looks very similar to earlier cross-sectional studies:

$$Y_{it} = \alpha + \beta X_{it} + \gamma DP_{it} + \epsilon_{it}$$

(1)

where $\alpha$, $\beta$, and $\gamma$ are all coefficients to be estimated, and $\epsilon_{it}$ represents the random error term.

The implementation of the 2002 Farm Act allowed us to create an event study where we can estimate the difference between period 1 (pre-Farm Act) and period 2 (post-Farm Act). However, this means that DiD cannot be used with cross-sectional data since there is no way to track the same individual(s) pre- and posttreatment.
Act) and period 2 (post-Farm Act). Differencing gives us the following:

\[ \Delta Y_i = \alpha + \beta \Delta X_i + \gamma \Delta D P_i + \epsilon_i. \quad (2) \]

While Equation (2) works for a panel dataset, the available data (the ARMS datasets) did not allow us to create a panel. The ARMS data get collected every year from a different sample of farmers, giving us a series of cross-sections. Following labor economists who worked with similar data constraints, we constructed a pseudo-panel dataset by creating cohorts of observations within each cross-section, whose characteristics were most likely to be constant, in order to examine changes across time (Deaton 1985; Verbeek and Nijman 1992). Since we are modeling linear functional forms, mean cohort behavior should reproduce the form of individual behavior, meaning that the cohorts can effectively be treated as individuals and we can handle them as a panel dataset (Browning, Deaton, and Irish 1985).

Within a cohort, the “constituents” should remain as homogenous as possible, while they should be heterogeneous across different cohorts. Most importantly, however, farms cannot jump from one cohort to another over time. We used geographic location (state) and farm production specialty categories (defined as having at least 50% of revenues coming from a particular source) to create our cohorts. Those farms that did not produce a single commodity that provided at least 50% of revenues for the farm were lumped into one of the categories of “Other Crop” or “Other Livestock.” While using geographic location and farm production specialty categories to define our cohorts makes sense in terms of minimizing within-cohort heterogeneity while maximizing between-cohort heterogeneity, these choices also take into account the nonrandom stratified sampling technique used to create the ARMS datasets. Farms are selected into the ARMS datasets, and weights are subsequently designed, to ensure that at the state and regional levels estimates of production of the major commodities and the distribution of farms reflect the “true” population estimates.

We expect that if we tracked a single cohort over time, the changes the cohort experienced would be similar to the changes an individual farm within the cohort would experience over the same time frame. Furthermore, if we could track an individual farm over time, we would expect that the farm would remain in the same cohort due to the difficulties of changing either location or major production specialty. The first farm production specialty groupings we used, following Jinkins’s work on entropy, is shown in table 1. 7

We lumped together two years for each period (period 1 consisted of 2000 and 2001 respondents, while period 2 consisted of 2003 and 2004 respondents) to increase the number of observations and gain more precise estimates. 8 We grouped all farms into their respective periods and cohorts and took the mean of each variable (using the ARMS weights, which effectively measure how representative a particular farm is in a given year), which can be thought of as constructing representative farms for each cohort. Denoting a cohort average by \( \bar{c} \), our estimation equation

7 We created several groupings for robustness checks. Recall that we only include those producers who either collected direct payments or who produced program crops.
8 Using only 2001 and 2003 data obtains similar results with higher standard errors.
Finally, it is worth noting that in constructing cohort samples, there is a trade-off between the size and number of cohorts. Using an errors-in-variables technique allows one to determine the optimal cohort size to obtain population cohort means, following Deaton (1985). However, this is a very costly process and instead we follow Browning, Deaton, and Irish (1985) and treat the sample cohort means as if they were population cohort means. However, we do not think this is an unreasonable assumption, given our criteria for cohort construction (using state and production specialty) designed to take advantage of the sampling and weighting techniques used to create the ARMS database. These techniques were devised to ensure that the weighted state and regional means of variables do, in fact, reflect the true population means of the variables at the state and regional levels.

### The Endogeneity Issue

We can use a series of cross-sections to create a pseudo-panel dataset and estimate how changes in direct payments are correlated with changes in harvested acres. However, operators might have changed the size of their farm by buying or selling acres with base, which would make the payments endogenous (which previous studies failed to recognize).

The 2002 ARMS questionnaire presents a solution to this endogeneity problem. Each operator was asked, given the acres he or she had in 2002 of each crop, to disclose the number of these acres that would have counted as base under the previous 1996 Farm Act and how many do count as base under the newly implemented 2002 Farm Act. To illustrate our point: imagine a farmer with 50 acres of corn, 25 acres of soybeans, and 25 acres of vegetables in 2002. Suppose that he or she had bought 25 of his or her corn acres at the end of 2001 and that it came with base (we don’t actually know this from the survey, but assume we do), while he or she already owned the other 25 acres of corn that had base. Additionally, due to the 2002 Farm Act, he or she could update the 25 soybean acres to base acres, while the last 25 acres (of vegetables) remained ineligible for base.

<table>
<thead>
<tr>
<th>Table 1 Cohort Commodity Groupings</th>
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<tr>
<td><strong>Group</strong></td>
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</table>

Now looks like the following:

\[ \Delta Y_c = \alpha + \beta \Delta X_c + \gamma \Delta DP_c + \epsilon_c. \]  

(3)
The farmer needed to compute the number of currently operated acres (100) that qualified as base under the 1996 Farm Act. In our example, he or she would add the previously owned 25 acres of corn base to the 25 acres of corn base purchased at the end of 2001: a total of 50 acres that qualified as base acres under the 1996 Farm Act. To compute the number of currently operated base acres he or she had under the 2002 Farm Act, he or she would add the 50 corn acres that qualified as base under the 1996 Act (since it remained as base under the 2002 Farm Act) to the 25 soybean acres updated to base. Together, this means the farmer had 75 acres of total base under the 2002 Act. The change in base acres (25) has eliminated the impact of purchases or sales of acres, making the change due exclusively to the exogenous policy change. Furthermore, since ARMS contains detailed information for each program crop, we can calculate a completely exogenous dollar-value change due to updating. We calculate the direct payments accruing to each producer by multiplying the payment rate by the farmer’s payment yield for the applicable crop, and multiply that by 85% of the base acres under the respective Farm Acts (1996 and 2002) that the operator has of that crop. We then sum across all applicable program-eligible crops to obtain the total level of direct payments under each Farm Act that the farmer receives. This allows us to calculate the levels of the direct payments for both the pre- (1996 Farm Act) and the post-updating (2002 Farm Act) that occurred. Differencing them gives us the (exogenous) change in direct payments received by farmer $i$ due to updating, $\Delta Base^i_s$, which can then be converted to cohort mean changes (as outlined above) to produce $\Delta Base^c$. Therefore, we substitute the change in base acre dollars ($\Delta Base^c_s$) due to the implementation of the 2002 Farm Act for the change in direct payments in Equation (3), giving us:

$$\Delta Y_c = \alpha + \beta \Delta X_c + \gamma \Delta Base^c_s + \epsilon_c.$$ (4)

This approach also removes a large degree of the potential for biases attributable to unobservable heterogeneity and omitted variables. Important variables we cannot measure easily or effectively (e.g., land productivity, location based technology, etc.) could bias our results if not included. To the extent that these variables remain constant through time but vary spatially, our differencing approach eliminates any biases they might have introduced.

Identification Based on Differences

To identify the effect of the change in direct payments on farmers’ production decisions properly, various cohorts must have been affected differently by the exogenous policy change. This would give us the variation required to compare the acreage responses between cohorts to different changes in direct payment receipts (the second difference in the DiD technique).

Since the various program crops received different levels of payments per acre (e.g., in 2002 corn received $0.28 per bushel, while oats received $0.024 per bushel), and separate cohorts had dissimilar amounts of base (e.g., crop versus livestock cohorts), different cohorts did not equally
value the 2002 Farm Act. This differential impact of the exogenous policy change on diverse cohorts allows us to identify the effect that direct payments have on farmers’ acreage decisions properly.

Controls

We now have a methodology that 1) utilizes an exogenous source of variation (the introduction of the 2002 Farm Act) of direct payments that can be measured (through the change in base acre dollars); 2) allows us to identify the effect of the change in base acres (the 2002 Farm Act had a different value for operators producing different agricultural products); and 3) permits us to control for unobservable heterogeneity and omitted variables (by differencing).

We still need to control for characteristics of the cohort’s operations and general environment. In the matrix $X$ we included the cohort size (measured by a lagged sales category) and farm type of the operation. Since access to capital markets might also affect how many acres a producer utilizes, we included the producers’ wealth. We also included the change in government payments, excluding direct and conservation payments, accruing to producers, since these payments might also affect access to capital markets. Finally, we used two different specifications to control for the relevant prices that producers would use to assess the feasibility of production changes. In the first set of specifications, we used the revenues and costs per acre to control for prices of inputs and outputs. In the second set of specifications, we used individual prices of inputs and outputs. The wealth levels, the government payments, the revenues and costs per acre, and input and output prices were all measured using lagged values to prevent endogeneity biases from entering our results. At the beginning of the current year, the previous years’ values are all known and can be considered exogenous. These variables should control for most, if not all, of the issues farms faced between 2000 and 2004. We then used ordinary least squares to estimate Equation (4) to assess the effect of the exogenous policy change on farmers’ production decisions. The coefficient “gamma” gives us an unbiased estimate of how direct payments alter producers’ acreage decisions, having controlled for endogeneity, omitted variable, and unobservable heterogeneity concerns.

Data

We used ARMS data collected by the National Agricultural Statistics Service (NASS) of the U.S. Department of Agriculture. Each year NASS collects data from a cross-section consisting of thousands of farmers, conducted using random stratified sampling techniques. A set of weights gets constructed for each observation to enable expansion of the sample up to national-level estimates.

To create the lagged values for the years 2000 and 2001, we used 1999 and 2000 data. These datasets had 10,251 and 10,309 observations respectively. Combining the two datasets and deleting all operations that had no harvested acres of program crops or no direct payments left us with 12,374 observations. We combined these observations into the state-commodity cohort combinations using the individual year-specific ARMS weights (which effectively measure how representative a particular farm is in a year), which left us with 698 (cohort) observations. Eliminating cohorts with less
than 20 observations left us with 138 lagged observations for period one. Repeating this process for period two left us with 160 lagged observations.

Using the same procedures to create current values of the variables, we ended up with 133 observations for period one and 160 observations for period two. The 2002 ARMS survey supplied us with data to calculate the change in base acre dollars and, observing the same process as above, left us with 67 observations. Our final balanced pseudo-panel dataset combined all five datasets, consisting of 64 observations (cohorts) in each time period.

We focused on crop production and, since our data does not contain planted acres, we used acres harvested as our measure of acres used in production for the current period (our dependent variable). We used lagged per acre revenues to control for prices and expected revenues per acre, calculated by dividing a cohort’s lagged mean gross farm income by its lagged mean acres harvested. Lagged per acre costs controlled for input costs and were calculated by dividing the total costs of the operation by the number of acres in production. We computed the level of government payments accruing to the operation as the total government payments received minus the level of direct payments and level of Conservation Reserve Payments and Wetland Reserve Payments received in the previous period. Profits per acre were therefore the sum of the revenues and government payments per acre, minus the costs per acre. Finally, we used the lagged mean net worth of each operation to proxy for each cohort’s expected wealth for each period.

Size categories were calculated using the period one average value of production for each cohort. We created three categories. A cohort fell in the first category if it produced an average of less than $100,000 worth of agricultural products. If it produced an average of between $100,000 and $200,000, it fell in the second size category. Cohorts with farms producing an average of $200,000 or more of agricultural products comprised the third category. Each category held approximately one-third of the cohorts in our sample.

We also calculated the difference in base due exclusively to the 2002 Farm Act. Using the payment yields for each program crop produced and the payment rates for each crop, we calculated the total change in base acre dollars. We included fixed effects in the model for general farm-type specialties (crop vs. livestock).

Table 2 contains descriptive statistics for the cohorts we generated. The average change in profits per acre was a drop of just under five dollars per acre, while government payments decreased by an average of $0.77 per acre. Net worth, however, increased by an average of over $70,000. On average, the number of harvested acres increased by approximately 28 acres. Meanwhile, due to the 2002 Farm Act, base acre dollars increased by an average of just over $1,620.

To better understand how the independent variable, DBase$s, differs across cohorts, we examined different size categories and different farm production specialties (see table 3). For the cohorts containing the smallest

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9A negative change in profits might seem odd given that, over this time frame, farms were typically thought to have fared better in the later years, which appear to be substantiated by the lower government payments distributed and the higher net wealth of the farms. Note, however, that the change in profits is very close to zero with a large standard deviation and, when examined more closely, the median value of profits per acre for our sample of cohorts is a little over (positive) $5 per acre.
<table>
<thead>
<tr>
<th>Variable name</th>
<th>Definition</th>
<th>Mean</th>
<th>Std. dev.</th>
</tr>
</thead>
<tbody>
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<td>Harv_acres</td>
<td>Change in harvested acres</td>
<td>27.9</td>
<td>161.4</td>
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<tr>
<td>Prof_Acre</td>
<td>Lagged change in profits per acre</td>
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<td>224.1</td>
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<tr>
<td>Gov_Acre</td>
<td>Lagged change in government payments per acre (excluding direct, CRP, and WRP payments)</td>
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<td>35.3</td>
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<tr>
<td>Net_W</td>
<td>Lagged change in net wealth</td>
<td>73,465.1</td>
<td>194,612.3</td>
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<tr>
<td>Dbase$s</td>
<td>Change in base acre dollars due to implementation of 2002 Farm Act</td>
<td>1,624.1</td>
<td>2,129.7</td>
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<tr>
<td>VOP &lt; 100</td>
<td>Cohorts with farms averaging less than $100,000</td>
<td>0.33</td>
<td>n.a.</td>
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<tr>
<td>VOP100-200</td>
<td>Cohorts with farms averaging between $100,000 and $200,000</td>
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<td>VOP &gt; 200</td>
<td>Cohorts with farms averaging more than $200,000</td>
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<td>n.a.</td>
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<tr>
<td>Spec1</td>
<td>Cohorts predominantly producing program crops</td>
<td>0.63</td>
<td>n.a.</td>
</tr>
<tr>
<td>Spec2</td>
<td>Cohorts predominantly producing livestock</td>
<td>0.37</td>
<td>n.a.</td>
</tr>
<tr>
<td>N</td>
<td>Number of observations (cohorts)</td>
<td>64</td>
<td></td>
</tr>
</tbody>
</table>

Notes: n.a., not applicable.
farms, the base acre dollars increased an average of $1,482. For the next size category, farms experienced an average growth of $1,498 due to the base updating. Farms in the largest size category experienced a gain of over $1,918. Interestingly, the average number of harvested acres in this largest size category was smaller than the average in the middle size category, probably indicating that farms in the largest categories spent a larger portion of their activities producing high-valued commodities.

By studying the same sets of variables by farm specialty type, we see that farms primarily producing crops had much higher average changes in base acre dollars ($2,333) than farms engaged in primarily livestock activities ($442). Crop farms also had nearly twice as many harvested acres in each period.

Because our measure of profits per acre reflects all the revenues and costs associated with the farm rather than those incurred solely to run the crop portion(s) of the farm, we repeated the analysis using individual expected prices of inputs and outputs. To obtain expected prices for our crop commodities, we followed Goodwin and Mishra (2005) and Girante, Goodwin, and Featherstone (2008), using two methodologies, dependent upon whether futures prices were available from the Chicago Board of Trade (CBOT). For those that were, we used the crop’s planting month average daily close price of the crop’s harvest month’s future price, giving

**Table 3 How Direct Payments and Harvested Acres Change with Farm Size and Farm Type**

<table>
<thead>
<tr>
<th>Category</th>
<th>Definition</th>
<th>Mean</th>
<th>Std. dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Size</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VOP &lt; 100</td>
<td>(N = 21) Change in base dollars due to 2002 Farm Act</td>
<td>1,482.2</td>
<td>1,871.3</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre1 Average harvested acres in period 1</td>
<td>365.1</td>
<td>152.3</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre2 Average harvested acres in period 2</td>
<td>468.5</td>
<td>252.2</td>
</tr>
<tr>
<td>VOP100-200</td>
<td>(N = 23) Change in base dollars due to 2002 Farm Act</td>
<td>1,497.7</td>
<td>1,208.7</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre1 Average harvested acres in period 1</td>
<td>530.7</td>
<td>326.6</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre2 Average harvested acres in period 2</td>
<td>551.8</td>
<td>311.1</td>
</tr>
<tr>
<td>VOP &gt; 200</td>
<td>(N = 20) Change in base dollars due to 2002 Farm Act</td>
<td>1,918.4</td>
<td>3,087.0</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre1 Average harvested acres in period 1</td>
<td>524.8</td>
<td>318.0</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre2 Average harvested acres in period 2</td>
<td>481.0</td>
<td>293.9</td>
</tr>
<tr>
<td>Farm specialty</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Program crops</td>
<td>(N = 49) Change in base dollars due to 2002 Farm Act</td>
<td>2,333.2</td>
<td>2,378.3</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre1 Average harvested acres in period 1</td>
<td>570.4</td>
<td>310.2</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre2 Average harvested acres in period 2</td>
<td>619.3</td>
<td>291.8</td>
</tr>
<tr>
<td></td>
<td>VOP1 Value of production, period 1</td>
<td>138,577.3</td>
<td>103,701.8</td>
</tr>
<tr>
<td></td>
<td>VOP2 Value of production, period 2</td>
<td>185,045.8</td>
<td>95,029.0</td>
</tr>
<tr>
<td>Livestock</td>
<td>(N = 29) Change in base dollars due to 2002 Farm Act</td>
<td>442.2</td>
<td>707.6</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre1 Average harvested acres in period 1</td>
<td>314.8</td>
<td>127.2</td>
</tr>
<tr>
<td></td>
<td>Harv_Acre2 Average harvested acres in period 2</td>
<td>307.5</td>
<td>123.9</td>
</tr>
<tr>
<td></td>
<td>VOP1 Value of production, period 1</td>
<td>360,581.7</td>
<td>507,081.3</td>
</tr>
<tr>
<td></td>
<td>VOP2 Value of production, period 2</td>
<td>448,641.5</td>
<td>624,424.6</td>
</tr>
</tbody>
</table>

us a national-level futures price for the commodity. We then created a basis between local markets and the national level based on the one-year lagged ratios of realized local- and national-level prices for the commodities. Each Farm Act establishes loan rates for the major program commodities, creating an effective price floor for each commodity. Therefore, the expected price the farmer faces is the maximum of the futures price and the loan rate in effect at harvest time. For the commodities that had no futures prices available on CBOT, we calculated an expected price for each state by multiplying the average monthly price at planting time by the five-year average of the ratio between actual prices during the month of planting and the actual prices at the month of harvest. Again, the relevant expected price the farmer sees is the maximum of our calculated expected price and the federally established loan rates for the program commodities.

**Results and Discussion**

We ran regressions using several different specifications to enable us to compare our results to previous work done in the field (see table 4). We started with a typical cross-sectional approach found in the literature, using direct payments and contemporaneous explanatory variables on the right-hand side. Results are similar in nature to those reported in the literature, that is, almost all variables are highly statistically significant and have similar signs and magnitudes as those previously reported. In the literature, however, some have used direct payments per acre instead of total direct payments, without knowing which acres should be associated with the payments. Specification 2 illustrates how a “direct payments per acre” variable (constructed by dividing the payments by the acres harvested, rather than by base acres) could be misleading; the direct payments variable changed sign from the previous specification. Because of the issues raised earlier, direct payments per acre could cause incorrect inferences to be drawn at the cross-sectional level. We then ran the same cross-sectional regression replacing individual observations with the cohorts we generated to show that cohort construction did not drive our results. Specification 3 shows that all the signs and statistical significances of the estimated parameters remained the same, while almost all the magnitudes remained similar. In particular, the direct payments parameter remained very close in magnitude to that in specification 1. Therefore we can confidently state that using cohorts does not significantly alter our results.

While the cohort framework we established does not appear to alter our results, the methods we employ in this paper to deal with unobserved heterogeneity, potential omitted variable biases, and endogeneity biases could alter the previous results. To examine more carefully the impact of our methods on the results, we ran two more specifications to compare with the results currently reported in the literature. Specification 4 (see table 4) introduces lagged explanatory variables (such as lagged profits per acre, net wealth, government payments per acre, and farm sizes) to eliminate any endogeneity concerns with these variables that could bias results. The results show that the direct payments parameter increases, suggesting that endogeneity issues could have been a real concern. In specification 5 we used the cohorts in the panel format we used for our final results, taking the difference of each variable over time and thereby eliminating the potential for bias due to time invariant yet idiosyncratic variables not otherwise
<table>
<thead>
<tr>
<th>Variable†</th>
<th>Estimate</th>
<th>Std. error</th>
<th>Estimate</th>
<th>Std. error</th>
<th>Estimate</th>
<th>Std. error</th>
<th>Estimate</th>
<th>Std. error</th>
<th>Estimate</th>
<th>Std. error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td>442.2***</td>
<td>30.5</td>
<td>647.5***</td>
<td>35.9</td>
<td>281.2***</td>
<td>97.1</td>
<td>462.4***</td>
<td>125.9</td>
<td>0.30</td>
<td>34.1</td>
</tr>
<tr>
<td>DP‡</td>
<td>0.014***</td>
<td>0.0002</td>
<td>n.a.</td>
<td>n.a.</td>
<td>0.018***</td>
<td>0.002</td>
<td>0.028***</td>
<td>0.002</td>
<td>0.025***</td>
<td>0.004</td>
</tr>
<tr>
<td>DP_Acre</td>
<td>n.a.</td>
<td>n.a.</td>
<td>–0.16***</td>
<td>0.04</td>
<td>n.a.</td>
<td>n.a.</td>
<td>n.a.</td>
<td>n.a.</td>
<td>n.a.</td>
<td>n.a.</td>
</tr>
<tr>
<td>VOP</td>
<td>–441.7***</td>
<td>39.0</td>
<td>–639.1***</td>
<td>48.2</td>
<td>–85.0*</td>
<td>48.8</td>
<td>–140.5***</td>
<td>52.8</td>
<td>–30.4</td>
<td>73.9</td>
</tr>
<tr>
<td>VOP &lt; 100</td>
<td>–379.1***</td>
<td>65.7</td>
<td>–559.7***</td>
<td>76.1</td>
<td>–58.6</td>
<td>48.8</td>
<td>–59.8</td>
<td>50.3</td>
<td>5.6</td>
<td>52.0</td>
</tr>
<tr>
<td>VOP 100-200</td>
<td>–0.02***</td>
<td>0.002</td>
<td>–0.02***</td>
<td>0.002</td>
<td>–0.08***</td>
<td>0.02</td>
<td>–0.05</td>
<td>0.02</td>
<td>–0.06</td>
<td>0.07</td>
</tr>
<tr>
<td>Prof_Acre‡</td>
<td>0.006***</td>
<td>0.0001</td>
<td>0.008***</td>
<td>0.0001</td>
<td>0.003*</td>
<td>0.002</td>
<td>–1.15***</td>
<td>0.44</td>
<td>–0.80</td>
<td>0.52</td>
</tr>
<tr>
<td>Gov_Acre‡</td>
<td>0.0001***</td>
<td>2E–6</td>
<td>0.0001***</td>
<td>2E–6</td>
<td>0.0001***</td>
<td>2.5E–5</td>
<td>0.0001**</td>
<td>4.0E–5</td>
<td>0.0001</td>
<td>0.0001</td>
</tr>
<tr>
<td>Net Wealt</td>
<td>0.63</td>
<td>0.55</td>
<td>0.68</td>
<td>293</td>
<td>0.70</td>
<td>273</td>
<td>0.60</td>
<td>64</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>30,632</td>
<td>29,184</td>
<td>293</td>
<td>293</td>
<td>273</td>
<td>273</td>
<td>64</td>
<td>64</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* Significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Notes: n.a., not applicable.

† Specialty fixed effects (FE), their interactions with size category dummies, and year FE are all included in each regression, but remain unreported for brevity’s sake. Results are available upon request. Specialty FEs control for whether the majority of farms in a cohort fall into one of three categories: program crops, nonprogram crops, and livestock.

‡ These variables are in changes, not levels, for specification (5).
accounted for. However, as found in the literature, we continued to use the potentially endogenous variable “direct payments” on the right-hand side. Not surprisingly, due to the differencing over time (which eliminates a lot of the variation in the variables), many of the variable parameters lost their significance. Most notably, however, the direct payments variable retained its high level of significance, its sign, and its magnitude.

Overall, these results convey a couple of important points. First, using the cross-sectional specification with our data, we arrived at similar results to those achieved in previous work. This means that our general approach mirrors earlier studies. Second, our use of cohorts did not drive our results, as the results remain robust despite the method used. Our methodology to eliminate biases associated with endogenous and omitted variables, however, does appear to generate results that differ from those previously reported in the literature.

At this point, however, we still have not eliminated one of the largest drawbacks to recent studies concerning the effect of direct payments on production decisions: the potential for biased coefficients due to the endogeneity of the independent variable, “direct payments.” Table 5 contains the results where we substituted the change in base acre dollars for the change in direct payments while continuing to use the profits per acre to control for input and output prices (as used in previous studies). We used the unique questions asked in the 2002 ARMS survey to measure an exogenous change in direct payments due to the effect of the 2002 Farm Act. This allows us to identify the effect of a change in direct payments on producers’ acreage decisions properly.

Using the exogenous change in base acre dollars, we ran three different specifications to test the robustness of the construction of our cohorts. Recall that when constructing cohorts, the main tradeoff lies in the ability to jump between cohorts (which should be minimized since, if allowed, it would make comparisons of cohorts across time meaningless) and the homogeneity of the individuals within the cohort (which should be maximized to ensure that the effect can be accurately measured and attributed to the individuals in the cohort). Since we constructed the original cohorts using the state and nine commodity groups, we created two alternative cohort setups, one to minimize the ability to jump between cohorts by using only two commodity groups and the second to maximize the homogeneity within the group using 18 commodity groups.

Results from these three specifications, however, show very few differences in the effect of a change in the base acre dollars on the number of acres harvested on the farm. Using the profits per acre controls, a change of one base acre dollar causes a change between a low of 0.027 acres and a high of 0.039 acres.

\[\text{For these regressions, we also included the government payments per acre (not including CRP, WRP, or direct payments) in our calculations of the profits per acre to simplify the results.}\]

\[\text{Alternative [1] used commodity groupings (a) livestock and (b) crops. Alternative [2] used (a) general cash grain (including oats and barley), (b) wheat, (c) corn, (d) soybean, (e) grain sorghum, (f) rice, (g) tobacco, (h) cotton, (i) peanut, (j) general crop, (k) fruits and tree nuts, (l) vegetables, (m) nursery and greenhouse, (n) beef cattle, (o) hogs, (p) poultry, (q) dairy, and (r) general livestock.}\]

\[\text{Recall that we used harvested acres as our dependent variable. We also constructed estimates of planted acres to explore whether using harvested versus planted acres altered our results. Our results did not change (and are available upon request), allowing us to interpret our results as if we had originally used planted acres.}\]
Note that we used the total profits of the farm to control (implicitly) for input and output prices. Profits per acre do not appear significantly different from zero and the sign is generally negative, which can appear surprising at first glance. However, our measure could (and likely does) include revenues and costs coming from a livestock (or other) portion of the operation, making it an imperfect measure for our analysis. Also note, however, that profits might not change a lot over time, so when differencing, a lot of variation gets removed, making statistical significance harder to achieve. Furthermore, when using the profit per acre variable to control for prices, the expected sign and significance of this variable are ambiguous.13

Because our measure of profits per acre reflects all the revenues and costs associated with the farm, rather than those incurred solely to run the crop portion(s) of the farm, we repeated the analysis in table 5 using the individual expected prices of inputs and outputs. We included the prices of all the major program crops, along with those of hay and oats and the major livestock commodities, as well as the price of natural gas, the median level of off-farm wages, and the rental price of land. We found

Table 5 Regression Results, Profits Per Acre

<table>
<thead>
<tr>
<th>Variable†</th>
<th>(6) Cohort 1 (state-farmtype[9]‡)</th>
<th>(7) Cohort 2 (state-farmtype[2]‡)</th>
<th>(8) Cohort 3 (state-farmtype[18]‡)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimate</td>
<td>Std. error</td>
<td>Estimate</td>
</tr>
<tr>
<td>Intercept</td>
<td>–29.2</td>
<td>38.8</td>
<td>–59.4</td>
</tr>
<tr>
<td>Δ Base Dollars</td>
<td>0.033***</td>
<td>0.009</td>
<td>0.027**</td>
</tr>
<tr>
<td>lag Prof_acre</td>
<td>–0.02</td>
<td>0.08</td>
<td>0.13</td>
</tr>
<tr>
<td>lag VOP &lt; 100</td>
<td>–16.6</td>
<td>89.4</td>
<td>15.2</td>
</tr>
<tr>
<td>lag VOP 100-200</td>
<td>2.0</td>
<td>63.5</td>
<td>19.6</td>
</tr>
<tr>
<td>lag Net Wealth</td>
<td>0.0002*</td>
<td>0.0001</td>
<td>0.0001</td>
</tr>
<tr>
<td>R²</td>
<td>0.38</td>
<td>0.32</td>
<td>0.34</td>
</tr>
<tr>
<td>N</td>
<td>64</td>
<td>62</td>
<td>57</td>
</tr>
</tbody>
</table>

* Significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.
† Specialty fixed effects (FE) and their interactions with size category dummies are included in each regression above, but remain unreported for brevity’s sake. Results are available upon request.
‡ [X] refers to the number of farm types used in the cohort setup.

Note that we used the total profits of the farm to control (implicitly) for input and output prices. Profits per acre do not appear significantly different from zero and the sign is generally negative, which can appear surprising at first glance. However, our measure could (and likely does) include revenues and costs coming from a livestock (or other) portion of the operation, making it an imperfect measure for our analysis. Also note, however, that profits might not change a lot over time, so when differencing, a lot of variation gets removed, making statistical significance harder to achieve. Furthermore, when using the profit per acre variable to control for prices, the expected sign and significance of this variable are ambiguous.13

Because our measure of profits per acre reflects all the revenues and costs associated with the farm, rather than those incurred solely to run the crop portion(s) of the farm, we repeated the analysis in table 5 using the individual expected prices of inputs and outputs. We included the prices of all the major program crops, along with those of hay and oats and the major livestock commodities, as well as the price of natural gas, the median level of off-farm wages, and the rental price of land. We found

13Consider farms operating on the increasing returns to scale portion of their cost curve (e.g., due to credit constraints, managerial ability, etc.). Regardless of the change in lagged profits per acre (excluding losses that force exit), long-run incentives exist to grow. Suppose, all else constant, that all output prices dropped over time. While (as a result) lagged profits per acre drop over time, incentives remain to grow, creating a negative coefficient for the profit variable. Given production risk across the country (e.g., weather, pests, diseases, etc.) farms in one part of the country could have positive profits while others (producing similar outputs) could have negative profits. With a mix of such types of farms, it can be seen that an ambiguous sign (and significance) on the lagged change in profits per acre could arise. Add the potential for farms to be operating on the decreasing returns to scale portion of their cost curve (which, assuming perfect competition, would create long-run incentives to reduce the size of the farm), and little, if anything, can be said a priori about either the sign or significance of the lagged change in profits per acre variable.
very similar results (see table 6), implying that our analysis appears robust to how we control for prices.\textsuperscript{14}

Our results suggest that direct payments matter to the individual decisions that operators make. The base updating allowed farmers to collect an average gain of $1,624 due to updating. Additionally, every dollar increase of direct payments was associated with an increase of 0.027 to 0.046 acres of harvested cropland. This translates into an average change of between 44 and 78 acres, holding all else constant, or an average increase of between 9 and approximately 16\% in total harvested acres on the farm. Overall, at the margin, direct payments appear to be causally associated with average farm-level increases in cropland harvested.\textsuperscript{15}

We wish to stress, however, that these results are not aggregate results, but rather the marginal results (holding all else constant) of those farmers that remained in the sample. We can say nothing about whether aggregate acreage increased or not. For example, operators may have started to bring in marginal land (for a potentially positive aggregate effect) or farms may have exited while, on average, the remaining farms may have all grown in size (for a potentially zero or potentially even negative aggregate effect). Our current results do not shed any light on these competing possibilities. Our results do, however, point out that direct payments appear to causally distort individual farmers’ decisions.

Although our findings appear to be significant and large, since our results are only at the margin while holding prices and other factors constant, the aggregate change in acres depends on many things and could easily be tempered (if not washed out entirely) by changes in prices or changes in the size of farms, etc. Therefore, we cannot say anything about the effect of direct payments on acreage response at the aggregate level, a topic that remains to be explored.

Our results vary rather substantially from those previously found in the literature, and there are several reasons why this might be the case. First, our methodological approach allows us to control for issues that were not controlled for previously, such as unobservable heterogeneity, omitted variable biases, and endogeneity issues, which could have seriously biased previous estimates. Second, our pseudo-panel data approach allows us to assign causation in a meaningful way that cannot be done using cross-sectional techniques (as used in the literature to date). Third, previous micro-level studies tended to focus on one region and a couple of crops while we examine many regions and many crops, which could lead to differences in our estimates.

Fourth, earlier studies focused on the effect of payments on the acreage response of particular crops, not on the overall acreage response of the

\textsuperscript{14}Consolidation trends could differ across cohorts over time that might bias our results. To control for this, we detrended the data by cohort and reran the regressions. Results did not change and are available upon request. Expected yields might alter acreage decisions, which could alter the amount of base the farmer operates (which would, in turn, alter the amount of direct payments the farmer receives). To control for this possibility, we included expected yields in separate regressions. Again, results remained robust and are available upon request.

\textsuperscript{15}Since we have a limited number of observations for each regression, an outlier could potentially drive our results. While the construction of our cohorts most likely curtailed this possibility, we reran our specifications using an MM estimation approach developed by Yohai (1987) that tested for outliers in both the response direction and the covariate space. If outliers are found, stable results are generated by limiting the influence of the outliers. Using this method does not substantively change our results and are available upon request.
Table 6 Regression Results, Prices

<table>
<thead>
<tr>
<th>Variable†</th>
<th>(9) Cohort 1 (state-farmtype [9]‡)</th>
<th>(10) Cohort 2 (state-farmtype [2]‡)</th>
<th>(11) Cohort 3 (state-farmtype [18]‡)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimate</td>
<td>Std. error</td>
<td>Estimate</td>
</tr>
<tr>
<td>Intercept</td>
<td>15.7</td>
<td>137.1</td>
<td>224.3</td>
</tr>
<tr>
<td>Δ Base Dollars</td>
<td>0.045***</td>
<td>0.013</td>
<td>0.039*</td>
</tr>
<tr>
<td>lag VOP &lt; 100</td>
<td>-12.1</td>
<td>114.3</td>
<td>132.6*</td>
</tr>
<tr>
<td>lag VOP 100-200</td>
<td>-15.6</td>
<td>86.5</td>
<td>72.8</td>
</tr>
<tr>
<td>lag Net Wealth</td>
<td>0.00019</td>
<td>0.00012</td>
<td>0.00005</td>
</tr>
<tr>
<td>R²</td>
<td>0.55</td>
<td></td>
<td>0.70</td>
</tr>
<tr>
<td>N</td>
<td>64</td>
<td></td>
<td>62</td>
</tr>
</tbody>
</table>

* Significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.
† Prices, specialty fixed effects (FE), and the FE interactions (with size category dummies) are included in each regression above, but remain unreported for brevity’s sake. Results are available upon request. Prices include expected commodity prices (using futures prices) multiplied by the output share of that commodity. Commodities include corn, wheat, soybeans, rice, cotton, barley, oats, sorghum, peanuts, tobacco, poultry, dairy, beef cattle, and hogs. We also include lagged prices of natural gas, land, and local off-farm wages along with specialty FEs that control for whether the majority of farms in a cohort fall into one of three categories: program crops, nonprogram crops, and livestock.
‡ [X] refers to the number of farm types used in the cohort setup.
individual farm. However, farmers receive direct payments independent of what crops they currently produce, making the payments perfectly fungible. In other words, the operator can use the direct payments in any manner desired. Previous studies essentially assumed that the payments would be funneled back into the farm business and into the major program crops that generated the direct payments in the first place (basically treating the payments as coupled payments). However, if farmers are profit maximizers, one would expect them to funnel direct payments into the activity that would, at the margin, increase profits the most. For some, it might mean increasing program crop acres; for others, it might mean increasing the number of livestock. It therefore does not seem surprising that previous results—where the authors effectively treated the (largely) decoupled payments as coupled—remained small. Essentially, prior studies found that farmers do not tend to funnel a meaningful amount of direct payments back into acres of a particular individual program crop (such as corn or wheat) and, as a result, they concluded that direct payments do not affect crop acreage decisions in a meaningful way. In contrast, we focused on overall harvested acreage to explore whether direct payments affected individual-level farm-wide production decisions—and our findings suggest that, at the individual level, direct payments do appear to causally affect overall crop acreage decisions in meaningful ways.

Fifth, we cannot compare our results to any macro-level studies, since our results cannot be interpreted as aggregate level results. Sixth, our study empirically examines the updating of the direct payments policies in the 2002 Farm Act. Almost all other empirical studies have examined years where the 1996 FAIR Act was in place. It is possible that direct payments had a larger impact on farmers’ decisions through the 2002 FSRI Act than did the PFC payments designated in the 1996 FAIR Act.

Conclusions

Do direct payments impact production decisions? In this paper we have explored how changes in direct payments might cause changes in individual operators’ acreage decisions. We have used ARMS data, spanning 1999 through 2004, and techniques borrowed from the labor economics literature to create a pseudo-panel from repeated cross-sectional data. Our results suggest that, even though not directly tied to production, the payments appear to alter farmers’ production decisions at the individual level.

Estimates range from approximately 44 to 78 acre increases in the size of the farm, or from 9 to 16% increases in acreage, due to increases in direct payment receipts, depending on the construction of the cohorts. These are rather significant marginal effects for a payment almost entirely decoupled from farmers’ current production decisions, and our results differ significantly from previous findings in the literature.

There are many reasons why our results might differ from previous research, which found minimal, if any, effects of direct payments on acreage decisions. Our methodology allows us to control for endogeneity issues, omitted variable biases, and unobservable heterogeneity, and it also allows us to assign causation (from direct payments to changes in individual producers’ acreage decisions), which previous research could
not. Additionally, our research has accommodated a broad array of crops across the United States while previous studies focused narrowly on a couple of crops in limited regions. Moreover, we focused on the effects of direct payments on overall acreage harvested, rather than the effect of payments on a single crop. Finally, our study empirically examined the updating of the direct payments policies in the 2002 Farm Act. Almost all other empirical studies have examined years where the 1996 FAIR Act was in place. These reasons might help explain why our findings differ significantly from previous research.

Regardless of the size of the effect of direct payments on individual producers’ acreage decisions, our results are robust across a wide range of specifications in both their sign and significance. Our consistently positive and significant coefficients imply that, ceteris paribus, farmers appeared to use direct payments to increase the size of their farm.

Our work provides the first step towards better understanding whether or not direct payments distort aggregate markets. While we believe our results are the first to show that direct payments significantly (both statistically and economically) affect individual farmers’ production decisions, at this point we wish to emphasize the fact that we can say nothing about aggregate acreage response effects.

Future research is required to better understand the implications of these micro-level distortions. Do direct payments have aggregate (national and/or international) effects? If so, what are they? Or do the direct payments promote changes in industry structure, as Roberts and Key (2008) seem to suggest? Over the time period we examined, more than 8% of all corn, soybean, wheat, cotton, peanut, and tobacco farms exited the marketplace (USDA, ARMS). If other farms grew by incorporating this land, it could help account for the consolidation trends that have been taking place in almost every agricultural industry. Future research could also address how, exactly, the payments distort farmers’ decisions—do they cause farmers to keep more land in production than efficiency otherwise would dictate? Or might the payments promote efficiency?

The answers to these questions require the aggregation of individual operators’ production decisions to examine the regional, national, and/or international effects of direct payments. While this analysis clearly lies outside the scope of the current paper, our results represent a vital first step necessary to begin thinking about these potential aggregate effects.

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