How Much Do Decoupled Payments Affect Production?

An Instrumental Variable Approach with Panel Data

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Selected Paper prepared for presentation at the Agricultural & Applied Economics Association's 2011 AAEA & NAREA Joint Annual Meeting, Pittsburgh, Pennsylvania, July 24-26, 2011

* The views expressed here are the authors' and should not be attributed to the Economic Research Service or the USDA.

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Abstract:

Agricultural support payments that cause no or minimal production distortions are exempt from World Trade Organization restrictions. If and how much decoupled payments, such as direct payments in the U.S., affect agricultural production remains an open empirical question with implications for policy. We use multiple years of the Census of Agriculture to estimate the aggregate supply response to changes in direct payments. To identify an exogenous source of variation in payments we exploit a provision of the 2002 Farm Act that departed from previous policy by making oilseeds eligible for direct payments, thus increasing payments to areas that historically produced more oilseeds. Using a sample of ZIP codes that accounts for more than eighty percent of the national value of production of program crops, our instrumental variable estimates, in contrast to OLS estimates, suggest that changes in payments over the period 2002 to 2007 had little effect on aggregate production.

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

Agricultural support payments that cause no or minimal production distortions can be categorized as “green-box” and are therefore exempt from World Trade Organization restrictions. In the United States, production flexibility contract payments under the 1996 Federal Agricultural Improvement Reform Act (the “1996 Farm Act”) and direct payments under the 2002 Farm Security and Rural Investment Act are considered “decoupled” because they are based on historical yields and acreages, not on current acreage, production or prices. The extent that such decoupled payments stimulate production and distort trade has emerged as a point of dispute in World Trade Organization negotiations (FAO, 2005; Sumner, 2005; USTR, 2004). In 2007, Canada used the WTO Dispute Settlement process to consult the U.S., charging that U.S. corn subsidies suppressed corn prices in Canadian markets over the period 1996 to 2006 (Schnepf and Womach, 2008). The charges were later postponed pending the Doha negotiations (Schnepf, 2010).

Estimating how much payments affect aggregate supply is challenging because areas with more acreage and higher yields of program crops in the past will receive more payments compared to less agriculturally productive areas. A correlation in yield growth over time would induce a correlation between government payments and production. We address the possibility of a spurious correlation between decoupled government payments and production by exploiting a provision of the 2002 Farm Act that allowed farmers to update their acres eligible for payments (their “base acreage”) to include soybeans and other oilseeds, which were historically excluded from payment programs. Areas growing more oilseeds from 1998 to 2002, therefore, experienced an increase in government payments after 2002 relative to similar areas with less historic oilseed acreage. Using historic oilseed production as an instrument for future changes in payments, we estimate how increases in payments affects growth in the value of production (the total supply response) and cropland harvested (the response on the extensive margin) at the ZIP-code level. We estimate the supply response to changes in payments across a sample of ZIP codes that accounts for 81 percent of the national production of program crops.

There are four main ways that decoupled payments could affect production: risk, credit constraints, labor participation, and expectations (for reviews, see OECD, 2005 and Bhaskar and Beghin, 2009). Hennessy (1998) articulated the risk mechanism and showed that decoupled payments could stimulate production by reducing the absolute level of risk aversion (a wealth effect) of farmers with decreasing absolute risk aversion or, if payments are linked to shocks (e.g., price floors), by decreasing the variability of farm profits (an insurance effect). Several studies have incorporated the link between decoupled payments and risk in simulation models to estimate how payments affect supply (Young and Westcott, 2000; Anton and Le Mouel, 2004; Sckokai and Moro, 2006). This approach has also been extended to allow for both input and output price risk (Serra, et al., 2006). Using data from Kansas farms, Serra, Goodwin, and Featherstone (2011) find empirical evidence of decreasing absolute risk aversion but that the elasticity between decoupled payments and output is very small (.00043). This is consistent with Just (2011), who finds that decoupled payments would have to increase operator wealth substantially to have a large effect on production through the risk aversion mechanism alone.

Even if the effect of payments on risk aversion is negligible, under imperfect credit markets, payments could increase output by increasing a farmer's financial capital, either for short-term liquidity needs or longer-term investment. Goodwin and Mishra (2006) find little evidence of an interaction effect between payments and farm debt-to-asset ratios in determining acreage, but payments may also ease credit constraints by increasing collateral for loans by increasing land values, and in turn permit greater investment and output (Roe, Somwaru and Diao, 2003). Roberts, Kirwan, and Hopkins (2003); Goodwin, Mishra, and Ortalo-Magne (2003); and Kirwan (2009) estimated econometrically the extent that decoupled payments are capitalized into land values. Model-based empirical research has considered how payment-induced increases in land values affect agricultural production (Dewbre, Anton, and Thompson, 2001; Gohin, 2006).

The third way that payments could affect production is by changing how farm households allocate their labor. Ahearn, El-Osta and Dewbre (2006) and El-Osta, Mishra and Ahearn (2004) estimated econometrically the effect of decoupled payments - which raise household wealth thereby influencing labor-leisure tradeoffs - on household labor supply on and off the farm. The studies find that farm operators receiving more payments tend to supply less labor off the farm and work more hours on the farm, a finding consistent with Key and Roberts (2009), who show how farmers with preferences for farm (versus off-farm) work could respond to higher decoupled payments by decreasing off-farm labor and increasing farm labor.

Even when current payments are decoupled from production, farmers may respond to anticipated but uncertain policy changes by making production decisions to maximize future payments (Lagerkvist, 2005; Sumner, 2003). The 2002 Farm Act, which extended the fixed decoupled payments of the 1996 Act, gave producers an opportunity to update their base acreage and yields and allowed them to include acreage in common oilseeds like soybeans and rapeseed in their base. Hence, prior to 2002, farmers may have altered their acreage decisions in anticipation of the base updating, even though current payments were decoupled from current production. Using a small sample of cotton and soybean farmers in Mississippi and corn and soybean farmers in Iowa, Coble, Miller, and Hudson (2007) found that on average, farmers thought that there was a 40 percent chance of updating base acres in the next farm bill, though only 17 percent said that they adjusted acreage or yields in anticipation of updating.

While understanding the mechanisms through which payments may affect production can inform policy decisions and improve modeling efforts, the marginal effect of payments on output has clear implications for trade policy. Using survey data to econometrically estimate the effect of decoupled payments on agricultural production, however, presents conceptual and practical challenges. Decoupled payments originate from agricultural programs open to all program crop farmers and program participation is often nearly universal. This makes it difficult or impossible to distinguish between a treatment and control group – a prerequisite for a standard program evaluation. In most instances it is also unclear what causes variation in payments across observationally similar farms, opening the possibility that unobservable factors could be associated with both program participation (or payment levels) and supply response. Further complicating the identification of the effect of payments, changes in agricultural subsidy policies generally occur simultaneously across the nation. This makes it difficult to distinguish the effects of a policy change from changes in prices, technology or other time-varying factors. Consequently, econometric analyses should address concerns about omitted variables correlated with both agricultural supply and government payments. Furthermore, the most detailed source of data on U.S. farms, the Agricultural Resource Management Survey (ARMS), is applied to a different sample of farms each year, thus precluding panel data approaches that aid in separating the effect of payments from confounding factors.

Goodwin and Mishra (2006) contribute to the econometric literature by estimating the effect of coupled and decoupled payments on acreage using multiple years of the cross-sectional ARMS survey. The researchers estimated a linear relationship between current payments per acre and current crop acreage conditioning on other farm-level and county variables. The authors recognize the standard critique of ignoring unobservable variables correlated with the outcome of interest. The greater concern, however, is the endogeneity of payments (total or per acre) caused by the mechanical relationship between payments and land operated. Payments are tied to the land and will be sent to the farmer cultivating it. Leasing or buying land enrolled in the farm program will therefore increase the total payments that a farmer receives. If only a portion of the acreage that a farm operates is enrolled in a payment program, then renting or buying more program land will also increase payments per acre which could cause a spurious correlation between payments and production.

O'Donoghue and Whitaker (2010) attempt to address payment endogeneity by using changes in payments caused by the 2002 Farm Act. They use multiple years of the ARMS survey to construct cohorts of similar farms to create a pseudo panel. Their paper makes an important contribution to the literature, but the small sample size and the aggregation of farms into cohorts leaves room for improvement. It is also important to note that their analysis captures the short run response to a change in payments since their post-policy observations come from 2003 and 2004 and most of the payment flow from the 2002 Farm Act only started in 2003.

Gardner, Hardie, and Parks (2010) take a county-level approach to estimate the relationship between payments and land use in 1987, 1992, and 1997. They find that decreasing program payments by half from their observed levels would have decreased cropland acreage by 89 million acres (22 percent). Instrument validity and measurement error in land use data notwithstanding, the result suggests large effects of payments on production. This link is unsurprising since the policies in place in 1987 and 1992 explicitly linked commodity payments to production decisions. It remains to be seen whether the decoupling that accompanied the 1996 Farm Act, and largely maintained in the subsequent 2002 Farm Act, had a similar effect.

The 2002 Farm Act provided significant continuity of payments from the historical 1996 Act, which departed from previous farm bills by introducing decoupled payments. Under the 2002 Farm Act, payments continued to go to producers of wheat, corn, barley, grain sorghum, oats, upland cotton, and rice, though under the 1996 Farm Act, producers signed a 7-year production flexibility contract while under the 2002 act producers signed an annual agreement to receive direct payments. Payment rates specified in 2002 were similar to the rates applied in 1997 under the 1996 Farm Act. A major change in 2002, however, was the inclusion of oilseeds as program crops.[1] The program specified a payment of $.44 per bushel for soybeans and $.008 per pound for other oilseeds (USDA, 2002). Under the 2002 Farm Act, payments would be made for oilseeds based on plantings and yields from the period 1998 to 2002.[2] Hence, the 2002 Farm Act increased payments for some farmers after 2002 that was exogenous to their 2007 planting decisions. This exogenous variation in decoupled payments allows us to identify the effect of payments on the value of production and acres of cropland harvested and makes it credible to assert that the estimated association is causal.

We offer several contributions to the empirical literature on decoupled payments. First, the exhaustive nature of the Census data allows us to aggregate farm-level observations to the ZIP-code level, which permits estimating the aggregate supply response across the nation using a large number of geographic units. Second, by examining the value of production of program crops and cropland harvested, we can identify the total supply response for program crops and the acreage response for crops in general, an improvement on existing studies that only look at the acreage effects and often only acres in program crops. Third, constructing a panel from multiple Census years allow us to control for growth trends that could be correlated with payments and production that could bias estimates. Finally, and perhaps most importantly, the policy change in 2002 that changed payments to farmers based on past planting history provides clear guidance for selecting an instrument for the change in decoupled government payments. The use of an instrumental variable reduces the possibility that temporally correlated unobservable variables bias our results.

Empirical Model

It is often assumed that farm operators make production decisions to maximize their expected utility. As noted in our discussion of the literature, attitudes towards risk, expectations about future policy changes, or preferences for farm versus non-farm work could all enter an agent's optimization problem. In addition, imperfections in credit, labor, land or other markets could constrain production decisions. The complexity of the optimization problem precludes deriving a feasible structural production response equation. Instead, we posit a reduced-form equation describing the change in production across time. Our model supposes observing a ZIP code in three distinct periods: t, t-1, and t-2. For the total production effect of payments we examine the value of production of program crops and for the effect on the extensive margin we look at acres of cropland harvested.

Let y_{it} be the outcome for ZIP code i in year t. The outcome y_{it} varies based on its past values y_{it-1} and y_{it-2}, changes in government payments since the previous period (ΔGP_{it}), covariates X_{it-1} from the previous period, a region-specific term $\mu_{r(i)}$, and an idiosyncratic shock ε_{it}. Supposing that the relationship between y_{it} and the other variables can be approximated with a linear functional form, we have

(1) $y_{it} = \delta_0 + \delta_1 y_{it-1} + \delta_2 y_{it-2} + \delta_3 X_{it-1} + \theta \, \Delta \, GP_{it} + \mu_{r(i)} + \varepsilon_{it}$.

where $\Delta GP_{it} = GP_{it} - GP_{it-1}$. Assuming that ΔGP_{it} is uncorrelated with ε_{it} and estimating (1) with OLS would likely be an improvement upon cross-sectional models since it uses variation in payments over time while controlling for past realizations of the variable of interest. Still, the exogeneity of ΔGP_{it} is a tenuous assumption given the mechanical relationship between production and payments. If a policy change allows farmers to update program acres and yields, then areas with higher yield or acreage growth would receive a larger increase in payments compared to areas with less growth. Identification of the effect of payments is confounded if areas with higher yield and acreage growth in the past experience more growth in the future compared to other areas – a very plausible scenario.

To indentify the causal relationship between payments and production, we instrument for the change in payments using the value of oilseed production averaged over periods t-1 and t-2. Because the policy change allowed farms to receive payments based on past oilseed acreage and yields, ZIP codes associated with greater oilseed production in previous periods would have experienced a greater increase in payments from the policy. A good instrument is one that is sufficiently strongly correlated with the endogenous regressor, in this case ΔGP_{it}, and uncorrelated with the error term, ε_{it}. We show in a following section that because of the policy change, past production of oilseeds is a strong predictor of future changes in payments. We also perform a diagnostic that casts light on the extent that possible correlation with the error term may affect our results.

To aid in identification, we include in the vector of control variables X_{it-1} the acres of idle agricultural land (land in the Conservation Reserve Program plus land in fallow or other idle states) and the total amount of tillable land (land out of production plus harvested land and pasture), corn yields averaged over the previous two periods, the median farm size in the ZIP code measured by acres harvested, and a linear and quadratic term for the median age of farm operators in the ZIP code. Acres of tillable land and acres of idle agricultural land capture the land constraints in a ZIP code while the corn yield reflects land quality. The median farm size captures the scale of the typical operation, which could be important given economies of scale and trends towards larger but fewer farms. The age terms control for the possibility that growth is linked to the life stage of the typical farm operator.

Including the region term $\mu_{r(i)}$ is important in a national analysis covering landscapes with different agro-climatic conditions and crop mixes. The region variable is based on a classification provided by the USDA/Economic Research Service that groups counties according to crop reporting districts and farm characteristics like crop mix.[3] The region term is preferable to a state-level term since states, whose boundaries were created based on political considerations, often include ZIP codes and counties with very distinct types of agriculture.

We estimate two outcome equations: one relating the change in total government payments to the value of production of program crops and another relating changes in payments to cropland harvested.

(2) $$VP_{it} = \delta_0 + \delta_1 VP_{it-1} + \delta_2 VP_{it-2} + \delta_3 X_{it-1} + \theta \Delta GP_{it} + \mu_{r(i)} + \varepsilon_{it}$$

(3) $$CH_{it} = \delta_0 + \delta_1 CH_{it-1} + \delta_2 CH_{it-2} + \delta_3 X_{it-1} + \theta \Delta GP_{it} + \mu_{r(i)} + \varepsilon_{it}$$

We instrument for ΔGP_{it} using the reduced form equation:

(4) $$\Delta\, GP_{it} = \alpha_0 + \alpha_1 y_{it-1} + \alpha_2 y_{it-2} + \alpha_3 X_{it-1} + \beta Oilseeds_{it-1,t-2} + \mu_{r(i)} + \nu_{it}.$$

where y_i is the lagged dependent variable, which differs between equations (2) and (3).

Data

The ZIP-code values for each Census year are calculated by aggregating farm-level data from the Census of Agriculture administered and maintained by the USDA National Agricultural Statistics Service (NASS).[4] The Census collects data on farm and operator characteristics every five years from most farms in the country.[5] Response rates are generally high (more than 80 percent) and each farm receives a non-response weight, which we use when aggregating farms in each census year.

We define "program crops" as corn, soybeans, wheat, oats, barley, sorghum, canola, flaxseed, safflower, and sunflower. All of these crops were officially program crops after the 2002 Farm Act, but only corn, wheat, oats, barley, and sorghum were program crops in 1997. We calculate the value of production of program crops for each Census year holding prices constant at 2002 levels[6]. Cropland harvested includes all acreage from which crops where harvested, including forages. This measure permits measuring the effect of payments on the true extensive margin because it counts each acre only once, even if two crops were harvested on the acre in the same growing season. Furthermore, because it includes all land in crops, it is not affected by rotation or substitution among crops. Thus, while the value of production of program crops captures the total supply response of program crops, cropland harvested captures whether payments affect land in cultivation in general.

We define government payments as total payments received for participation in Federal farm programs (excluding Commodity Credit Corporation loans or crop insurance payments) net of payments received for participation in the Conservation Reserve Program and the Wetlands Reserve Program. In 2002, these federal payments net of conservation payments would have been derived mostly from Production Flexibility Contracts, which were tied to historically enrolled contract acreage, not current plantings and were therefore considered to be decoupled payments. In 2007, payments net of conservation payments consisted of direct payments, which are the decoupled payments that replaced the PFC payments, and some loan deficiency and counter cyclical payments, but these latter two types of payments were only paid for cotton and peanuts, which are excluded from the analysis. Thus, the change in government payments from 2002 to 2007 largely reflects changes in decoupled payments between the 1996 and 2002 Farm Acts.

Our ZIP code-level analysis includes all operations that responded to the Census. An analysis of aggregate outcomes could be conducted at the ZIP code, county, or state level. ZIP codes are used because they are the smallest geographic unit where farms can be located with the data, thus providing the maximum number of observations and cross-sectional variation in the dependent and independent variables. In the Heartland, for example, there are 9,718 ZIP codes compared to 544 counties. Although a very small fraction of ZIP codes change over time, most changes have occurred in relatively urban areas with population growth and where agriculture is less prevalent, which mitigates this potential problem.

To focus on areas with significant production of program crops, we only include ZIP codes that contributed a non-negligible amount to the national production of program crops. To trim the sample, we first calculate the value of program crops produced in the nation averaged across 1997, 2002, and 2007 and then sort ZIP codes by their average value of production of program crops for these years and calculate a cumulative sum of production for each ZIP code. We focus on ZIP codes with a cumulative sum of five percent or greater. The fifth percentile of the cumulative sum corresponds to the ZIP code where 95 percent of the production of program crops occurs in ZIP codes with more program crops and five percent occurs in ZIP codes with less program crops. Taking only the ZIP codes associated with 95 percent of the value of program crops leaves 8,467 ZIP codes. We also require that each ZIP code has a positive value for each covariate used in the analysis and that they produced at least some soybeans – the most common oilseed – in all Census years. This leaves a total sample of ZIP codes of 6,634 that together accounted for 81 percent of the total U.S. value of production of program crops for the years 1997, 2002, and 2007.

Of the 6,634 ZIP codes used in the national analysis, 3,526 are located in the "Heartland" – a relatively homogenous geographical region defined at the county level by the USDA.[7] The Heartland includes all counties in Illinois, Indiana, and Iowa, and some bordering counties in Kentucky, Minnesota, Missouri, Nebraska, Ohio, and South Dakota. To see if our results are driven by regional heterogeneity, we also conduct our analysis for the subset of ZIP codes located in the Heartland – a region that accounts for a large share of the national production of program crops and, naturally, a large share of total direct payments.

Table 1 presents descriptive statistics of key variables for the national sample of ZIP codes and for the subset located in the Heartland. Except for Operator Age and Farm Size, which are medians for the ZIP code, all variables are calculated by aggregating all farms in the same ZIP code. Monetary amounts are in 2002 dollars.[8]

From 2002 to 2007, the average ZIP code saw the value of production increase by around 20 percent relative to the 2002 level. At the same time, there was almost no change in the average acres of cropland harvested, and the median ZIP code even had a small decrease. The large increase in production without an increase in area suggests that farmers replaced non-program crops with program crops and/or increased the intensity of program crop cultivation through higher yielding varieties, greater input use, and possibly more double-cropping. With respect to government payments, the average ZIP code saw about an 11 percent decrease in payments in real terms relative to the 2002 level of around $450,000.

The descriptive statistics for ZIP codes in the Heartland follow similar patterns. The value of production increased by about 17 percent for the mean ZIP code while area harvested decreased slightly. Payments also decreased in real terms. Understandably, corn yields are slightly higher in the Heartland than in the national sample – the median yield in the heartland is 125 bushels per acre compared to 117 for the full sample. The median farm size is also larger in the Heartland than in the national sample.

For both the national and Heartland sample, the mean values for variables involving production, payments, and land exceed the median values, suggesting a skewness in the distribution of these variables among ZIP codes. Still, skewness is limited across the variables – for both samples, the largest difference between the mean and median is in idle land for the national sample where the mean is 2.5 times greater than the median.

Estimation and Instrumental Variable Diagnostics

To reduce the influence of large ZIP codes and to be able to interpret coefficients as elasticities, we use the natural log to transform all the variables except Operator Age, Corn Yields, and Farm Size. In log terms, the key explanatory variable of interest, the change in payments from 2002 to 2007 is defined as $\Delta\, GP_{it} = \ln(GP_{it}) - \ln(GP_{it-1})$.

Payments for oilseeds under the 2002 Farm Act were based on the acres and yields of oilseeds for the period 1998 to 2002. We use the value of production of oilseeds averaged using the years 1997 and 2002 ($Oilseeds_{it-1,t-2}$) as our instrument for the change in payments between t and t-1, where the notation t, t-1, and t-2 corresponds to 2007, 2002, and 1997. To be a valid instrument, $Oilseeds_{it-1,t-2}$ must be relevant – strongly correlated with the change in payments and valid – unrelated to the outcomes in equations (2) and (3) and therefore uncorrelated with ε_{it}.

The policy change in 2002 to make payments based on historic oilseed production provides a clear reason to expect the value of production of oilseeds averaged for the 1997 and 2002 years to be correlated with changes in payments. It is nonetheless important to test for statistical relevance. Tests for relevance often involve testing the null hypothesis that the coefficients on the excluded instruments (in this case β) are jointly equal to zero. Rejecting the null of zero coefficients, however, is a low bar for relevance and does not distinguish between instruments that are only weakly correlated with the endogenous regressor and those that are strongly correlated. Studies have shown that with weak instruments, the Two-Stage Least Squares estimator is biased towards the probability limit of the OLS estimator, with the bias occurring because of randomness in the first-stage fitted values (Bound, Jaeger, and Baker, 1995; Angrist and Pischke, 2009).

To test for weak instruments (little correlation with the endogenous regressor), Staiger and Stock (1997) argue that for one endogenous regressor and one or two instruments, the F-stat for the null hypothesis that the instrument coefficients are jointly equal to zero should exceed 10. Stock and Yogo (2005) provide a formal interpretation of this rule of thumb. For one endogenous regressor and one or two instruments, an F-stat of 10 roughly corresponds to the five percent critical value of the hypothesis that the bias of the IV estimate is less than 10 percent of the bias of the OLS estimate.

The 2002 Farm Act was signed into law in May of 2002, and its changes to payments were to take effect immediately. USDA data show that most direct payments from the 2002 Farm Act came in the 2003 calendar year and would therefore not have been reported in the 2002 Census of Agriculture (USDA, 2010). Direct payment outlays then remained stable over the next five years. Thus, payments received in 2002 largely reflect the pre-policy payment level while payments in 2007 reflect the level under the new policy. Accordingly, we use the change in payments from 2002 to 2007 as the dependent variable in equation (4). Note that the level of payments in 2007 is a measure of the annual flow of payments from the new policy – a flow that began reaching most producers early in the 2003 calendar year and continued to 2007. Changes in production or acreage from 2002 to 2007 attributable to the change in payments, therefore, captures the accumulated effect of a higher annual flow of payments for several years.

The IV models using past oilseed production as an instrument for changes in payments are implemented using Two-Stage Least Squares. Robust standard errors allowing for heteroskedasticity are calculated.[9]

Results

We check the strength of our instrument by estimating the first stage equation (4). As expected, oilseed production is strongly correlated with the change in total payments from 2002 to 2007 (Table 2). Using the national sample, the F-test of a zero coefficient on the oilseed production variable is 121 for the equation controlling for the lagged value of production and 76 for the equation controlling for the lagged cropland harvested. The corresponding F-tests for the Heartland are 54 and 29. We therefore dismiss concerns about weak instrument bias. The coefficients from the national sample suggest that a one percent increase in historic oilseed production is associated with a 0.10 to 0.14 percent increase in the growth in government payments from 2002 to 2007. For outcome equations (2) and (3), the OLS estimates suggest a strong effect of payments on the value of production and cropland harvested. For the national sample, the coefficients imply that a one percent greater increase in payments leads to a 0.20 percent increase in the value of production and a similar increase in cropland harvested. The OLS estimates for ZIP codes in the Heartland are about a third larger than the estimates from the national sample. In all cases, the OLS estimates are precisely estimated with point estimates being 10 to 20 times larger than their standard errors.

In contrast, the IV estimates at the national level suggest that government payments had little effect on the value of production of program crops or on the acres of cropland harvested, with the point estimates being negative and statistically indistinguishable from zero. A Durbin-Wu-Hausman test for the exogeneity of the change in payments variable rejects the null hypothesis of exogeneity at the one percent level. The test is performed by regressing the outcome variable on the instrument, the suspected endogenous variable, and the control variables and obtaining the residuals. Then the outcome variable is regressed on the suspected endogenous variable, the control variables, and the residuals from the previous regression. The test consists of testing whether the coefficient on the residual is different from zero.

For the Heartland sample, the IV estimates are slightly closer to the OLS estimates: 0.29 compared to 0.06 for the value of production and 0.28 compared to 0.17 for cropland harvested. In the first case, the exogeneity of the change in payments is rejected at the 10 percent level, however, it is not rejected when looking at cropland harvested. Thus, the national and Heartland results do not indicate that increases in payments are associated with increases in the value of production. There is weak evidence that payments may have increased cropland harvested in the Heartland but not for the nation as a whole.

Estimate Sensitivity to Instrument Endogeneity

For the IV estimates to provide unbiased estimates of the effect of payments for ZIP codes affected by the instrument, past production of oilseeds must be exogenous to future changes in the value of production or cropland harvested. Holding other key variables like past production or area constant, it is difficult to argue why farms in ZIP codes that previously produced more oilseeds would expand or intensify production faster than farms in ZIP codes with less oilseed production. However, the possibility of such a temporal correlation cannot be ruled out. In the case of instrument endogeneity, the true parameter value will be given by (see appendix for the derivation):

(5) $$\theta = \theta_{IV} - \frac{cov(\varepsilon_{07}, Oilseeds_{97,02})}{cov(Oilseeds_{97,02}, \Delta GP_{07})}$$

The term involving covariances is the bias term. In a sense, estimating $cov\left(\varepsilon_{07}, Oilseeds_{97,02}\right)$ requires estimating the direct effect of oilseed production on future expansion in production or cropland harvested independent of the indirect effect through payments. We cannot separate the two effects in the study period, but we can look to a previous period (1992-2002) when oilseed production would have been largely unrelated to changes in program payments, and see if it is statistically related to the outcomes in question. Formally, we estimate

(6) $$y_{i02} = \lambda_0 + \lambda_1 y_{i97} + \lambda_2 y_{i92} + \lambda_3 X_{i97} + \mu_{r(i)} + \eta_{i02}.$$

and use the results to calculate the term $cov(\eta_{02}, Oilseeds_{92,97})$. We calculate the denominator of (5) as the variance of $Oilseeds_{92,97}$ multiplied by the parameter $\hat{\beta}$

obtained by estimating equation (4). We then estimate the bias term in (5) by supposing that

$$(7) \qquad \frac{cov(\eta_{02}, Oilseeds_{92,97})}{cov(Oilseeds_{92,97}, \Delta GP_{07})} \approx \frac{cov(\varepsilon_{07}, Oilseeds_{97,02})}{cov(Oilseeds_{97,02}, \Delta GP_{07})},$$

which we use to recover a "bias-corrected" θ. A potential problem with this approach is that the covariance between our instrument and the error term in a previous period (1992-2002) may not carry forward to the study period (1997-2007). Nonetheless, the exercise should provide insight into the magnitude of a possible bias.

In absolute terms, the bias terms are small, with the largest being .069 (

Table 5). Because the initial IV estimates were small, the bias term ranges from being 17 to 104 percent of the original IV estimate. The magnitude and sign of the bias is the same for the national and Heartland samples – it is small and negative for the value of production and small and positive for cropland harvested. Incorporating the potential bias into the estimates therefore increases the estimated effect on the value of production and decrease the effect on cropland harvested. The exercise suggests that any correlation between past oilseed production and future outcomes is likely to exert modest influence on IV estimates.

Further robustness checks

We perform further robustness checks to see if our results are sensitive to using oilseed production in 1992 and 1997 as the instrument (as opposed to 1997 and 2002), adding another lagged dependent variable, and aggregating at the county level instead of the ZIP code level. The OLS and IV results for the coefficient on the change in payments are presented in Table 6.

Oilseed production in 1992 and 1997 is arguably more exogenous to outcomes in 2007 than oilseed production in 1997 and 2002. Possible correlations between oilseed production and future outcomes, perhaps induced by rotating crops of different value or by fallowing dynamics, are likely to weaken over time. As a robustness check, we use the average oilseed production for the years 1992 and 1997 as an instrument for changes in payments from 2002 to 2007. As before, we first test for instrument relevance and find that oilseed production averaged over the years 1992 and 1997 is strongly correlated with the changes in payments from 2002 to 2007. At the national level, the F-test of a zero coefficient is 162 when using the lags of the value of production as covariates and 114 when lags for cropland harvested are used. For the Heartland, the F-test results are 70 and 50.

Using an arguably more exogenous instrument leads to even smaller point estimates of the effect of payments on the value of production and cropland harvested at the national level. In both cases, the exogeneity of the change in payments is rejected at the ten percent level. Looking at just the Heartland, however, the estimates become more precise and suggest a positive effect of payments, though the IV model estimates a coefficient that is one half of the OLS estimate of 0.29. While both effects are positive and statistically significant, the Durbin-Wu-Hausman test rejects that the OLS and IV estimates are indistinguishable; with confidence we can say that the IV estimate is smaller than the OLS estimate. But in the case of cropland harvested, the OLS and IV estimates are closer (0.28 vs 0.24) and cannot be statistically distinguished from each other.

If cropping patterns and growth are driven by long term dynamics, it may be important to control for longer lags in the dependent variable. As another robustness check, we add the value of the dependent variable in 1992 as a covariate in the original models. Before re-estimating the outcome equations, we check if the instrument is still relevant after adding a third lag, which it is (the lowest F-stat is 27). When included in the outcome equation, the coefficient on the third lag is statistically different from zero in all models. Adding the third lag results in a negative effect of payments on the value of production for the national sample while it leaves the point estimate for the effect on harvested acres relatively unchanged. For the Heartland sample, adding a third lagged dependent variable pushes the point estimate for the effect on the value of production close to zero (-.007) while decreasing the estimated effect of payments on cropland harvested such that it is statistically indistinguishable from zero.

To test the sensitivity of the results to the level of aggregation, we replicate the analysis on counties instead of ZIP codes, of which there are 1,076 for the national sample and 493 for the Heartland sample. Though weaker, the instrument continues to be relevant in all cases except that of cropland harvested for the Heartland sample. For the national sample, the F-test for a zero coefficient on the instrument is 14.6 and 15.5 when controlling for the lagged value of production and lagged cropland harvested, respectively, while it is 18.9 and 3.5 for the same models for the Heartland sample.

At the national level, the OLS result of a positive effect of payments on the value of production disappears at the county level (point estimate of .010 and standard error of .025). Similarly, the OLS estimate for cropland harvested decreases from close to 0.20 in the ZIP code analysis to 0.068 in the county analysis. Instrumenting for payments reduces the point estimate of the effect on cropland harvested to -0.043, though it is not statistically different from the OLS estimate at the 10 percent level. Focusing on the Heartland, the OLS county-level estimates still show a strong effect of payments on production and cropland harvested, however, for the value of production, the point estimate decreases to -0.06 when instrumenting for payments. The IV point estimate for cropland harvested is large but is not reliable given the weakness of the instrument in that model.

Discussion

Using a sample of ZIP codes that account for more than eighty percent of the total U.S. production of program crops (as we define them), we conclude that there is little evidence that payments affect production. ZIP codes where farms on the whole received a greater increase in payments from the 2002 Farm Act because of greater historic oilseed production did not see larger increases the value of production of program crops compared to ZIP codes where farms had less favorable changes in payments. The same applies to the relationship between payments and cropland harvested.

Focusing on the Heartland, the results are less conclusive, though they generally concur with the findings from the national analysis. The results from certain specifications, namely using oilseed production in 1992 and 1997 as an instrument instead of oilseeds production in 1997 and 2002, shows an economically large, though statistically weak, effect of payments on production and cropland harvested. What farmers do with payments depends on the constraints and opportunities that they face. One possible reason for different effects between the Heartland and the rest of the U.S. is that the majority of ethanol plants are located in the Heartland and may have increased prices in local spot markets, thus encouraging farmers to use payments to finance expansion or intensification of production.

In all cases at the national level and most cases at the Heartland level, OLS gives large and precisely estimated positive effects of payments on production and cropland harvested, which is unsurprising given the mechanical correlation between growth in production and payments. In most cases, controlling for the endogeneity of payments produced coefficient estimates that were significantly smaller than the OLS estimates and also statistically different from them. While OLS estimates have smaller variances than IV, the efficiency losses are limited by the strength of the relationship between our instrument (past oilseed production) and the endogenous variable (changes in payments). The lack of a clear effect of payments on production or cropland harvested, therefore, cannot be readily attributed to a weak instrument.

Gardner et al. (2010) estimated that a 50 percent decrease in commodity payments would reduce cropland in the U.S. by 22 percent, implying an elasticity between payments and cropland of .44. Their estimate is about double the OLS estimate for the national sample in this study, and the IV estimates suggest that OLS is biased upwards.

There are many possible explanations for the difference, but perhaps the most likely reason is that commodity payments were explicitly linked to production in two of the three years covered by the Gardner et al. study (1987 and 1992).

There are no econometric studies of decoupled payments in particular that we are aware of with which we can compare the magnitude of our supply response estimates, so we discuss our results in light of farm-level estimates. Goodwin and Mishra (2006) studied the effects of payments per acre on acreage in the Heartland. They estimated an elasticity for corn of 0.031, for soybeans 0.020, and for wheat 0.042. O'Donoghue and Whitaker (2010) used an identification strategy similar to ours, albeit with a different data set and for a different time period. They find that the 2002 policy change increased payments to the average farm in their sample by about 40 percent which resulted in an increase in acreage of between 9 and 16 percent, implying an elasticity between payments and acreage in the range of .23 and .40. Our instrumental variable point estimates for cropland harvested in the Heartland lie between the acreage estimates of Goodwin and Mishra on the lower end and O'Donoghue and Whitaker on the higher end, though our estimates are statistically indistinguishable from zero in most cases.

There are, however, many reasons to expect farm level responses to differ from aggregate responses, especially in panel data situations where the farm-level analysis uses only farms that persist in business. Because of the pervasive entry and exit of farms, only an aggregate analysis can capture the true supply response of payments. We highlight two reasons why the aggregate supply response may be less than that suggested by a farm-level analysis. First, farms that receive more payments and expand production may simply acquire land from farms that receive fewer payments and exit the industry. This would create a strong positive correlation between payments and production at the farm level but a much weaker correlation in the aggregate. A second and perhaps more subtle reason concerns the well-documented finding that payments are capitalized in land prices and rental rates. Consider an analysis that looks at continuing farms over a period when there is a policy change that increases payments per acre. Assuming that continuing farms own much of the land that they operate, existing farms will benefit from the increased flow of cash and greater net worth. A farm that enters the industry following the policy change, on the other hand, will 'pay' for the increase in payments through higher land prices and rental rates and will therefore have less capital to finance production. Thus, in contrast to the farm-level analysis of continuing farms, the aggregate analysis includes farms that enter production after the policy change and who would experience smaller, if any, benefits from the increase in payments.

Conclusion

Using an identification approach that relies on the provision of the 2002 Farm Act that made oilseeds eligible for direct payments, we estimate the total supply response to changes in decoupled payments for ZIP codes that account for more than eighty percent of the total value of production of program crops. Our findings suggest that for the 2002-2007 period, decoupled government payments had little effect on the value of production of program crops. The results do not imply that government payments will always have such neutral effects on production. How farmers use the extra income from payments probably depends on market conditions and regional considerations. Indeed, we find some evidence that payments had positive effects on production and cropland harvested when the analysis is restricted to the Heartland.

While it is reasonable to expect government payments to affect production of the program crops in particular, our analysis allows for the possibility that payments affect production of non-program crops. We do this by looking at the effect of payments on total cropland harvested, which includes program and non-program crops. The findings for cropland harvested, however, are generally consistent with those when looking only at program crops.

Finding a weak link between decoupled payments and production suggests that claims about payments securing an abundant and stable food supply, which are sometimes used to justify them, may be overstated. At the same time, the finding does not support the critique that payments cause excess production and therefore distort world commodity prices and trade. Under current budget constraints, however, the most likely policy scenario is a decrease in government payments. Countries that are major producers of agricultural commodities would likely welcome a reduction in U.S. domestic support, especially countries like Canada and Brazil that in the past have lodged formal WTO complaints over U.S. agricultural subsidies. While welcomed by U.S. agricultural competitors, our findings imply that a reduction or removal of decoupled commodity payments would have modest effects on U.S. agricultural production and by extension world markets, though the exact effect would depend on how much payments decrease and how input markets respond, especially land markets.

The identification strategy employed in this paper could find useful applications in the study of other behavioral responses to agricultural policy. Agricultural policy, which is made at the national level in the U.S. and is applied in all states at the same time, often has provisions that affect farmers differently, based on observed characteristics like past behavior. Exploiting such provisions can help to improve the credibility of estimates of the effect of particular policies. One caveat, however, is that using variables based on past decisions as an instrument does not automatically ensure identification of the casual effects of the policy. Past decisions can be correlated with future outcomes, and researchers should explore whether such temporal correlations confound their estimates. Robustness checks such as replicating the analysis for periods prior to the policy change, similar to our efforts to estimate the bias of our instrument, is one approach to testing if and how much such temporal correlations may affect estimates.

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Table 1: Descriptive Statistics of Key Variables

	U.S. Sample (N=6,634)			Heartland Sample (N=3,526)		
	Median	Mean	S.D.	Median	Mean	S.D.
Value of Production 2002 ($1,000s)[1]	3,185	4,909	5,055	4,214	5,944	5,301
Change in Value of Production 2002-2007 ($1,000s)	523	995	2,259	670	1,041	1,884
Cropland Harvested 2002 (Acres) [2]	19,151	26,878	24,346	20,239	26,670	21,015
Change in Cropland Harvested 2002-2007	-193	18	6,713	-272	-286	4,594
Payments 2002 (1,000s)	314	453	482	308	411	345
Change in Payments 2002-2007 (1,000s)	-40	-54	242	-20	-29	156
Idle Land (Acres)	1,799	4,503	10,947	1,623	3,121	4,290
Tillable Land (Acres)	26,078	36,570	36,002	25,773	33,238	25,559
Corn Yield 1997, 2002 (Bushels/Acre)[3]	117	116	26	125	123	24
Farm Size 2002 (Acres)	97	175	218	128	180	171
Operator Age	54	54	4	54	54	4

Source: U.S. Census of Agriculture, multiple years.
[1]Value of Production includes only program crops and is calculated in every Census year using 2002 prices.
[2]Cropland Harvested includes all cropland harvested, including forages and non-program crops.
[3]This equals (corn yield 1997+corn yield 2002)/2.

Table 2: Oilseed Production and Changes in Payments[1]

	Entire U.S.		Heartland	
Variable	Lags for VP	Lags for CH	Lags for VP	Lags for CH
Oilseeds 1997, 2002	0.143***	0.096***	0.231***	0.180***
	(0.013)	(0.011)	(0.031)	(0.033)
Value of Production 2002	-0.049		-0.106**	
	(0.030)		(0.044)	
Value of Production 1997	0.064**		0.034	
	(0.026)		(0.031)	
Cropland Harvested 2002		0.278***		-0.124*
		(0.056)		(0.072)
Cropland Harvested 1997		0.138***		0.111***
		(0.033)		(0.036)
Idle Land 2002	0.046***	0.077***	0.011	0.008
	(0.008)	(0.009)	(0.009)	(0.010)
Total Tillable Land 2002	-0.220***	-0.600***	-0.208***	-0.210***
	(0.026)	(0.050)	(0.039)	(0.063)
Corn Yield 1997, 2002	0.001***	0.001***	0.001**	0.001
	(0.000)	(0.000)	(0.001)	(0.000)
Farm Size (100's of Acres)	0.003	-0.005	0.017***	0.017***
	(0.004)	(0.004)	(0.005)	(0.005)
Operator Age	0.035	0.016	-0.055	-0.053
	(0.055)	(0.054)	(0.049)	(0.049)
Operator Age Squared	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Intercept	-1.631	-0.633	3.020**	1.607
	(1.486)	(1.496)	(1.254)	(1.165)
Controls for region	yes	yes	yes	yes
Observations	6,634	6,634	3,526	3,526
Adjusted R Squared	0.215	0.227	0.075	0.077
F-stat of zero coefficient on Oilseeds	121.0	76.6	54.7	29.2

*** p-value < 0.001, ** p-value <0.01, * p-value <0.05. Robust standard errors in parenthesis.
[1]Only Farm Size, Corn Yield, and the terms with operator age and are not in log form.

Table 3: Payments and Value of Production and Cropland Harvested: Entire U.S.[1]

	Value of Production		Cropland Harvested	
Variable	OLS	IV	OLS	IV
Change in Payments 2002-2007	0.200***	-0.113	0.195***	-0.066
	(0.014)	(0.072)	(0.010)	(0.056)
Value of Production 2002	0.610***	0.633***		
	(0.022)	(0.025)		
Value of Production 1997	0.279***	0.320***		
	(0.021)	(0.027)		
Cropland Harvested 2002			0.635***	0.759***
			(0.024)	(0.040)
Cropland Harvested 1997			0.170***	0.215***
			(0.019)	(0.025)
Idle Land 2002	-0.022***	-0.008	-0.019***	0.003
	(0.005)	(0.007)	(0.004)	(0.006)
Total Tillable Land 2002	0.127***	0.040	0.215***	0.016
	(0.019)	(0.030)	(0.020)	(0.052)
Corn Yield 1997, 2002	-0.002***	-0.002***	0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Farm Size (100's Acres)	0.003	0.004	-0.006***	-0.008***
	(0.003)	(0.003)	(0.002)	(0.002)
Operator Age	-0.050	-0.032	-0.035	-0.029
	(0.032)	(0.037)	(0.023)	(0.028)
Operator Age Squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Intercept	2.555***	1.811*	0.975	0.910
	(0.829)	(0.991)	(0.619)	(0.743)
IV and OLS estimates are different?[1]	yes		yes	
Controls for region	yes	yes	yes	yes
Observations	6,634	6,634	6,634	6,634
Adjusted R Squared	0.875	0.848	0.930	0.905

*** p-value < 0.001, ** p-value <0.01, * p-value <0.05. Robust standard errors in parenthesis.

[1]Only Farm Size, Corn Yield, and the terms with operator age and are not in log form.

[2]This refers to the result of a Durbin-Wu-Hausman test for endogeneity. The test was conducted at the ten percent level.

Table 4: Payments and Value of Production and Cropland Harvested: Heartland[1]

	Value of Production		Cropland Harvested	
Variable	OLS	IV	OLS	IV
Change in Payments 2002-2007	0.287***	0.061	0.278***	0.165*
	(0.021)	(0.120)	(0.018)	(0.090)
Value of Production 2002	0.710***	0.717***		
	(0.035)	(0.035)		
Value of Production 1997	0.249***	0.279***		
	(0.030)	(0.042)		
Cropland Harvested 2002			0.694***	0.707***
			(0.034)	(0.036)
Cropland Harvested 1997			0.144***	0.165***
			(0.024)	(0.036)
Idle Land 2002	-0.026***	-0.024***	-0.022***	-0.020***
	(0.006)	(0.006)	(0.005)	(0.005)
Total Tillable Land 2002	0.065**	0.016	0.187***	0.146***
	(0.026)	(0.039)	(0.029)	(0.048)
Corn Yield 1997, 2002	-0.003***	-0.003***	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Farm Size (100's Acres)	-0.014***	-0.009**	-0.015***	-0.013***
	(0.003)	(0.004)	(0.003)	(0.003)
Operator Age	-0.037	-0.047	-0.045	-0.050
	(0.037)	(0.043)	(0.030)	(0.033)
Operator Age Squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Intercept	1.791*	1.963*	1.208	1.381
	(0.957)	(1.103)	(0.794)	(0.867)
IV and OLS estimates are different?[2]	yes		no	
Controls for region	yes	yes	yes	yes
Observations	3,526	3,526	3,526	3,526
Adjusted R Squared	0.909	0.898	0.932	0.928

*** p-value < 0.001, ** p-value <0.01, * p-value <0.05. Robust standard errors in parenthesis.

[1]Only Farm Size, Corn Yield, and the terms with operator age and are not in log form.

[2]This refers to the result of a Durbin-Wu-Hausman test for endogeneity. The test was conducted at the ten percent level.

Table 5: Estimate of IV Bias

Sample	Outcome	Original IV Estimate	Bias Term Estimate	Bias as Percent of Original Estimate	"Bias-Corrected" Estimate
Entire U.S.	Value of Production	-0.113	-0.020	17%	-0.094
	Cropland Harvested	-0.066	0.069	104%	-0.135
Heartland	Value of Production	0.061	-0.037	60%	0.098
	Cropland Harvested	0.165	0.036	22%	0.129

Table 6: Summary of Robustness Checks

			Entire U.S.		Heartland	
Robustness Check			Value of Production	Cropland Harvested	Value of Production	Cropland Harvested
Using Oilseeds,1992-1997 as instrument	OLS	Coef.	0.200***	0.195***	0.287***	0.278***
		S.E.	(0.014)	(0.010)	(0.021)	(0.018)
	IV	Coef.	-0.053	-0.062	0.145*	0.240***
		S.E.	(0.064)	(0.046)	(0.086)	(0.071)
Controls for 1992 lagged dependent variable	OLS	Coef.	0.193***	0.192***	0.282***	0.275***
		S.E.	(0.013)	(0.010)	(0.021)	(0.018)
	IV	Coef.	-0.163**	-0.079	-0.007	0.140
		S.E.	(0.076)	(0.058)	(0.140)	(0.098)
Aggregates to county level	OLS	Coef.	0.010	0.068***	0.102**	0.136***
		S.E.	(0.025)	(0.010)	(0.044)	(0.021)
	IV	Coef.	-0.295	-0.043	-0.068	0.222
		S.E.	(0.210)	(0.087)	(0.208)	(0.151)

[1]The IV estimate for cropland harvested in the Heartland is not reliable due to the weakness of the instrument.

Appendix

The bias introduced by an endogenous instrument[10]

For simplicity we drop subscripts and work with scalar notation. Suppose we are interested in estimating θ in the equation

(a1) $y = \alpha + \theta x + \varepsilon$

where *x* is suspected of being endogenous, $(Cov(x, \epsilon) \neq 0)$. Now consider an equation relating *z* to *x*.

(a2) $x = \gamma + \beta z + \mu$

Plugging (a2) into (a1) gives

(a3) $y = \alpha + \theta(\gamma + \beta z + \mu) + \varepsilon$

which can be written as

(a4) $y = \tau + \theta\beta z + \theta\mu + \mu + \varepsilon$

Now consider the covariance of *z* and *y*.

$$cov(y, z) = cov(\tau + \theta\beta z + \theta\mu + \mu + \varepsilon, z)$$

$$cov(y, z) = cov(\tau, z) + \theta\beta cov(z, z) + cov(\theta\mu, z) + cov(\mu, z) + cov(\varepsilon, z)$$

$$cov(y, z) = \theta\beta var(z) + cov(\theta\mu, z) + cov(\mu, z) + cov(\varepsilon, z)$$

By construction $cov(\theta\mu, z)$ and $cov(\mu, z)$ both equal zero.

(a5) $\theta\beta = \frac{cov(y,z) - cov(\varepsilon,z)}{var(z)}$

Recognizing that $\beta_{OLS} = \frac{cov(z,x)}{var(z)}$ and that $\hat{\beta}_{OLS} \rightarrow \beta$ allows us to rewrite (a5) as

$$\theta = \frac{cov(y, z) - cov(\varepsilon, z)}{var(z)} * \frac{var(z)}{cov(z, x)}$$

$$\theta = \frac{cov(y,z) - cov(\varepsilon,z)}{cov(z,x)}$$

$$\theta = \frac{cov(y,z)}{cov(z,x)} - \frac{cov(\varepsilon,z)}{cov(z,x)}$$

$$\theta = \theta_{IV} - \frac{cov(\varepsilon,z)}{cov(z,x)}$$

Thus, IV will be biased by the term $\frac{cov(\varepsilon,z)}{cov(z,x)}$, which in our case is $\frac{cov(\varepsilon_{07}, Oilseeds_{97,02})}{cov(Oilseeds_{97,02}, \Delta GP_{07})}$.

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[1]The oilseeds included soybeans, sunflower seed, canola, rapeseed, safflower, mustard seed, flaxseed, crambe, and sesame.

[2] In practice, there were several options for updating base acreage under the 2002 Farm Act. The most common option allowed a soybean base to be added to base acres, where the soybean base was the minimum of 1) the average of program crop acreage 1998-2001 minus production flexibility contract base acres from the 1997 Farm Act and 2) average soybean acreage 1998 to 2002. For more details, visit: http://www.farmdoc.illinois.edu/manage/newsletters/fefo02_16/fefo02_16.html

[3] See www.ers.usda.gov/publications/aib760/aib-760.pdf for a map of the region and more details.

[4] More information about the Census of Agriculture can be found at: http://www.agcensus.usda.gov/.

[5] The census attempts to reach all agricultural operations that produce, or would normally produce and sell, $1,000 or more of agricultural products per year. Data are primarily collected through the mail, with supplemental reporting on the internet and non-response follow-ups by telephone and personal enumeration. The final response rate was 85.2 percent for the 2007 Census of Agriculture and 88.0 percent for the 2002 Census of Agriculture. NASS reports a probability weight for each observation to correct for undercoverage and non-response.

[6] For all commodities except corn silage, prices come from the USDA NASS QuickStats webtool (http://www.nass.usda.gov/QuickStats/Create_Federal_All.jsp). For corn silage we use a price of US$ 20/ton.

[7] See www.ers.usda.gov/publications/aib760/aib-760.pdf for a map of the region and more details.

[8] To put monetary amounts in real terms, we use the "CPI research series using current methods, 1978-98" provided by the Bureau of Labor Statistics: http://www.bls.gov/cpi/cpirsdc.htm

[9] Angrist and Pischke (2009) show that if heteroskedasticity is modest, the finite sample bias of the traditional formula for homoskedastic standard errors is less than the bias of the robust sandwich estimator. The large sample size and the likelihood of significant heteroskedasticity given the large range in farm sizes support using the robust estimator. However, we do include a finite sample adjustment by multiplying the covariance matrix by (N/N-K).

[10] The notes from Kumar Aniket were helpful in working through the derivation of the bias term. His notes are available at http://www.aniket.co.uk/teaching/devt2008/OLSbiasIV.pdf