The Incidence of Agricultural Subsidies on Farmland Rental Rates: Overcoming Bias From Inertia, Expectations, and Tenancy Arrangements

Nathan Hendricks
Ph.D Candidate
Department of Agricultural and Resource Economics
University of California, Davis
nathan@primal.ucdavis.edu

Joseph Janzen
Ph.D Candidate
Department of Agricultural and Resource Economics
University of California, Davis
janzen@primal.ucdavis.edu

Kevin Dhuyvetter
Professor and Extension Specialist
Department of Agricultural Economics
Kansas State University
kcd@ksu.edu

Selected Paper prepared for presentation at the Agricultural & Applied Economics Association 2010 AAEA, CAES, & WAEA Joint Annual Meeting, Denver, Colorado, July 25-27, 2010

The Incidence of Agricultural Subsidies on Farmland Rental Rates: Overcoming Bias From Inertia, Expectations, and Tenancy Arrangements

Nathan Hendricks, Joseph Janzen*, and Kevin Dhuyvetter

April 28, 2010

Abstract: Recent studies indicate that the effect of government subsidies on rental rates for farmland may be lower than once thought and lower than predictions from theory. However, there are still a number of unresolved issues in estimating subsidy incidence econometrically. We identify three such issues, inertia, expectations, and tenancy arrangements, and employ panel data from the state of Kansas to resolve them. Our econometric model suggests that subsidy incidence on rental rates is low in the short run, but consistent with predictions from theory in the long run.

Keywords: Farm subsidies, incidence, cash rents.

JEL Classification Numbers: Q1, H5.

Note: Working paper. Please do not cite without permission from the authors.

*Corresponding Authors' Address: 2167 Social Sciences and Humanities, University of California, Davis, 1 Shields Ave, Davis, CA, 95616, USA, telephone: +1-530-848-1401, e-mail: janzen@primal.ucdavis.edu.
The authors are grateful to Julian Alston and David Rapson for advice and suggestions and to Michael Langemeier for data assistance.

1 Introduction

The incidence of government subsidy payments, in an economic sense, refers to the distribution of the benefits of the subsidy after accounting for the behavioral changes it causes. Because of the limited availability of agricultural land, some economists have suggested that the incidence of an agricultural subsidy payment will fall entirely on land values. Using the well-known model of Floyd (1965), Alston (2007) has shown that restrictive assumptions about the nature of agricultural production technology and the supply elasticity of land are required for the incidence of an output subsidy to fall entirely on land prices. Using rough elasticity estimates, his model suggests that about 39 to 58 percent of an output subsidy dollar is reflected in changing rental rates for farmland.

To test this theory, the magnitude of subsidy incidence on rental rates has been directly estimated using reduced-form regressions of rental rates on determinants such as market earnings, government payments, and land productivity measures (Lence and Mishra, 2003; Goodwin, Mishra, and Ortalo-Magne, 2010). Roberts, Kirwan, and Hopkins (2003) and Kirwan (2009) suggest that estimates can be biased due to expectation errors in government payments and farm-level heterogeneity which is correlated with government payments and market earnings. Taking these biases into consideration, Kirwan (2009) estimates that the subsidy incidence on rental rates may be as low as 25 percent, which is relatively small compared to predictions from theory.

Data limitations and econometric specification problems may explain the difference between theoretical and empirical estimates of subsidy incidence. Our paper directly addresses three complications. First, many farmland rental agreements set rental rates over multiple years. Government policy changes that affect subsidy levels do not immediately cause farmers and landlords to adjust rates. This inertia in setting rental rates may lead to significant differences between the short-run and long-run incidence of a subsidy dollar.

Second, rental rates are generally set prior to planting and based, in theory, on expectations of market revenue and government payments, as mentioned above. The data on farm profitability and government payments used in econometric estimation of subsidy incidence are observed after output is harvested and sold. This creates errors-in-variables and attenuation bias.

Finally, tenancy arrangements may vary greatly from farmer to farmer and landlord to landlord. Many rental agreements contain some provision for crop sharing. In these cases, the incidence of the subsidy payment may be a parameter of the rental agreement. If crop share rented acres cannot be distinguished from cash rented acres at the farm level, measures of rental rates may be biased and this bias may vary significantly across farms or regions.

This paper is organized as follows. The next section briefly considers the mechanism by which government subsidies are capitalized and the nature of government payments. Then, Section 3 reviews previous estimates of the incidence of government payments on land rental rates. Section 4 presents a dynamic econometric model that uses panel data to overcome problems related to inertia and expectation error, as discussed above. Section 5 uses a second dataset to address estimation problems posed by unobserved tenancy arrangements. Concluding remarks are offered in Section 6.

2 Capitalization of Government Payments

The value of farmland today is the discounted stream of expected benefits from the land. Schmitz and Just (2003) model the net present value of farmland as

$$V_t = \sum_{i=0}^{\infty} \delta^i E_t(Y_{t+i}), \tag{1}$$

where δ is the discount rate and $E_t(Y_{t+i})$ is the expectation at time t of the net benefit from farmland at time $t+i$. Government payments will increase the expected cash flow and increase the value of farmland. The magnitude of this effect depends on expectations about future government subsidy policy and factors that affect the discount rate. While rental income is the primary source of expected benefits (Alston, 1986), Y_{t+i} may also include the option value of converting land to commercial or residential use (e.g. Barnard, Wiebe, and Breneman, 2003).

To simplify the analysis, we consider farmland rental rates directly. This reduces the complications of discounting, inflation, urbanization, and expectations of future government programs. A direct measure of the share of government payments captured by landowners is the change in the rental rate for a \$1 change in government payments.

Government payments are comprised of coupled and decoupled payments. Theory suggests that decoupled payments, which are not tied to current production, will be fully capitalized into rental rates. The extent to which coupled payments are capitalized depends on supply and demand elasticities and the nature of the agriculture production technology (Floyd, 1965; Alston, 2007). There are three relevant farm bills during our sample period: the 1990 FACT Act, 1996 FAIR Act, and 2002 FSRI Act. Government programs are complex, especially previous to 1996, but we try to simplify the programs to present the essential features. Payments for the major program crops have essentially consisted of three components: counter-cyclical payments (CCPs), loan deficiency payments (LDPs), and direct payments.

Counter-cyclical payments are calculated as payment acres times payment yield times the target price minus the maximum of the market price or the loan rate. The 1990 FACT Act referred to such payments as deficiency payments. Under FACT, the payment acres were the minimum of planted acres or a calculation based on historical planted acreage and the Acreage Reduction Program acreage. Thus if the farmer planted too many acres of the program crop he was not eligible for deficiency payments on the excess acres, but planting too few acres reduced payments. The payment yield was based on historical yields. The 1996 FAIR Act eliminated deficiency payments and the Acreage Reduction Program in favor of direct payments. However, in the period 1998-2001 crop prices fell and Congress issued ad hoc payments based on historical acreage and yields. The 2002 FSRI Act formalized these ad hoc payments as counter-cyclical payments which were tied to a specific target price, but still based on historical acreage and yields.

Loan deficiency payments were authorized under each of the three farm bills. The basic calculation of the LDP received by the farmer is current production times current yield times the difference of the loan rate and the realized market price, when that price is less than the loan rate. Thus, CCPs and LDPs are only issued when market prices fall below a certain threshold.

Direct payments are issued to the farmer regardless of current production or market prices. They are calculated based on historical acreage and yields. The 1990 FACT Act did not include any provisions for direct payments. The 1996 FAIR Act referred to direct payments as production flexibility contract payments because farmers received the payment regardless of the crop acres currently in production, which was a dramatic shift from previous farm bills. The 2002 FSRI Act continued the direct payments.

Even though direct and counter-cyclical payments under the 1996 and 2002 farm bills are not tied to current production, economists have argued that they may also have production effects through reducing risk (Hennessy, 1998) and expectations of updating their base acreage and yields (Sumner, 2003). However, econometric evidence suggests the effect on production may be relatively small (Goodwin and Mishra, 2006). To the extent that these payments have production effects, they may not be fully capitalized.

Loan deficiency payments are coupled in the sense that they are tied to current production. However, uncertainty about market conditions creates uncertainty about the magnitude of LDPs and CCPs because they are only issued when market prices are sufficiently low. Therefore, the capitalization of LDPs and CCPs into rental rates depends not only on elasticities and technology parameters, but also on the degree of uncertainty about the magnitude of the payments when rental rates are determined.

3 Empirical Literature Review

There is a considerable literature on subsidy incidence in agriculture. We focus on three recent papers that directly estimate the incidence of government payments on farmland rental rates. Kirwan (2009), Patton et al. (2008), and Goodwin, Mishra, and Ortalo-Magne (2010) each regress some measure of per-acre cash rents on measures of market returns and government payments. Each paper uses novel data that allows for a unique estimation strategy. All papers consider the problem posed by farmer expectations. Because only realized cash flows are observed, market revenues and government payments are measured with error. Familiar problems of attenuation bias result.

Kirwan (2009) is probably the most comprehensive incidence study to date in terms of the data econometric identification. Kirwan creates a two year panel of nearly 60,000 farms from the 1992 and 1997 U.S. Census of Agriculture. He uses subsidy levels from a year in which producers only received direct payments to instrument for a payment difference that is subject to expectation error. Kirwan recognizes the problem of inertia and uses alternative datasets to estimate the incidence over two years and nine years. He finds a subsidy incidence of about 25% on rental rates.

The dependent variable in Kirwan (2009), cash rent payments divided by total rented acres, is measured with error for farms that also lease on a crop share basis. Kirwan concludes the bias is small and positive by using an external dataset to regress the measurement error on government payments. However, the external dataset is only for a single year. If the number of share leases decreased (increasing the dependent variable) and government payments decreased from 1992 to 1997, then his estimate will be biased downward.

Patton et al. (2008) consider the case of coupled and decoupled European Union livestock program payments on agricultural land rents in Northern Ireland. Their dataset is unique in that rental rates are set on an annual basis due to special legislation that restricts the length of tenure in Northern Ireland. They use lagged values of market revenues and coupled government subsidies as instruments to remove bias from expectation errors. Citing Lence and Mishra (2003), Patton et al. (2008) argue that lagged values are valid instruments since they are part of the farmer's information set when rental rates are determined while clearly the expectation error is not part of the information set. They find a rate of incidence of approximately 40% for the two main livestock direct payment programs that they consider.

Goodwin, Mishra, and Ortalo-Magne (2010) place considerable emphasis on the impacts that different government program payments have. They use a pooled cross-section of observed farms from the USDA ARMS survey. They compare proxy and instrumental variables methods to remove expectations bias. As a proxy variable for expected payments they use a four-year average of per-acre payments in the county. They use lagged payments, futures prices, and lagged county-level returns as instruments. Their use of external instruments may be problematic; futures prices are unlikely to make good instruments because they are likely to be correlated with the expectation error in returns and in government payments. Goodwin, Mishra, and Ortalo-Magne (2010) do have the considerable advantage of being able to assess cash rents and share rents separately. This allows a unique analysis in which the authors test whether landlords who lease on a share basis are able to "extract additional benefits through higher share rates." They find that $1 in subsidy payments is associated with increases in rental rates of between $0.50 to $1.64, depending on the subsidy program and the tenancy arrangement.

4 Empirics: Kansas Farm Management Data

The first dataset is a farm-level unbalanced panel from Kansas covering 36 years with about 2,000 farm business units observed each year,[1] containing similar variables on farm revenues and costs as in Kirwan (2009). The data were collected by the Kansas Farm Management Association. Farmer members provide detailed accounting information to this association. While the dataset is attractive because of the relatively long panel, there are potential problems. There is potential for selection bias; KFMA only collects data for farms who voluntarily join the association. These may not be a representative sample of farms in the state. Discussion with those who maintain the database suggest that the data may exclude the smallest farms, who have less incentive to keep detailed financial records, and the largest farms, who may not require the financial analysis services provided by KFMA.

We performed the following steps in cleaning the data: eliminated all data prior to 1990 because it introduced too many potential policy regimes and it represented a period in the dataset where less detailed records were kept, dropped all farms with only a single year observed, dropped all cases where the farm did not report cash rent paid or government payments received, dropped observations which reported renting fewer than 40 crop acres, and winsorized the data to eliminate any remaining outliers. Prior to this we also manually cleaned the data, dropping any farms where implausible or unexplainable values were observed. The final dataset is an unbalanced panel containing observations on about 1,400 farms per year from 1990 to 2008, with observations on a total of 3,228 separate farms throughout the sample period.

Summary statistics for the variables used in our econometric model are shown in table 1.

[1] Most of the farms are only observed for a portion of the sample period. A total of 8,634 separate farms were observed throughout the entire sample.

Table 1: Summary Statistics

Variable	Description	Mean	Std. Dev.	Min.	Max.
r_{it}	Cash rent paid ($/ac)	14.289	15.265	0.001	84.663
g_{it}	Gov't payments ($/ac)	21.53	14.589	0	74.407
rev_{it}	Crop revenue received ($/ac)	161.692	84.756	0	473.832
$cost_{it}$	Variable production cost ($/ac)	117.488	59.313	0	331.892
irr_{it}	Proportion of acres irrigated	0.056	0.158	0	1
$pasture_{it}$	Proportion of acres in pasture	0.278	0.284	0	0.993
N	27,049				

For these data, our unit of analysis is the farm. Similarly to Kirwan (2009), our dataset does not contain the per-acre rental rate. We construct our measure of per-acre rental rates by dividing total cash rent paid by the number of rented acres farmed. The number of rented acres farmed includes cash rented acres and crop share rented acres.

4.1 The Empirical Model

Our empirical model is designed to exploit the availability of panel data, subject to the limitations mentioned above. The empirical model is based on the reduced form equation of Kirwan (2009). Consider his reduced form estimation equation:

$$r_{it} = \alpha + g^*_{it}\delta + X'_{it}\beta + f_i + d_t + \varepsilon_{it}, \tag{2}$$

where r_{it} is the average rental rate for farm i at year t. The independent variable g^*_{it} represents the expected subsidy anticipated by the farmer at the time of planting. The covariates in the matrix X are sales revenue, variable production expenses, proportion of acres irrigated, and proportion of acres in pasture. An individual fixed effect, f_i, is present due to unobserved heterogeneity across farms. The source of this heterogeneity may be management ability, land quality, or other productivity differences. Year fixed effects, d_t, control for shocks common to all farms in any given year. The presence of fixed effects argues for the use of panel data, because one can use variation over time to identify parameters.

In addition to the presence of fixed effects, we make the following assumptions about the underlying data-generating process. The motivation for these assumptions and the necessary adjustments required in econometric estimation are discussed below. First, the economic process that determines rental rates is dynamic; current realizations of the average cash rental rate paid by the farm depend on the rental rate in the previous year primarily due to contractual rigidities. This argues for adding a lagged dependent variable term to 2 as follows:

$$r_{it} = \alpha + r_{i,t-1}\gamma + g^*_{it}\delta + X'_{it}\beta + f_i + d_t + \varepsilon_{it}. \tag{3}$$

Second, observed government payments, revenue, and costs are subject to expectation errors which will cause attenuation bias in their coefficient estimates. Third, there are no available external instruments suitable for dealing with the endogeneity problems posed by the dynamic process and the expectation errors. Therefore, our panel data specification must be identified using instrumental variables drawn from the dataset itself. Finally, the idiosyncratic portion of the error term, ε_{it}, may have individual specific patterns of heteroskedasticity and autocorrelation, but is uncorrelated across individuals.

4.1.1 Contracting and Inertia

As mentioned above, rental rates are often fixed over multiple years by contracts. The inertia in rental rates represents in part a causal link between subsequent observations of r_{it}. If we estimated equation 2, intuition would suggest that our estimate of the effect of government payments would be small, because rental rates would not fully adjust because of contractual rigidities. Expected government payments could change greatly, but rental rates would not fully adjust simultaneously.

Cameron and Trivedi (2005, p. 764) note that estimating γ is difficult because correlation in the dependent variable can arise due to "true state dependence" or due to unobserved heterogeneity picked up by the individual specific coefficient f_i even when $\gamma = 0$. For the problem considered in this paper, it appears that there is indeed a causal link between observations of r_{it} and so the estimation of a dynamic panel data model is appropriate.

The problem with the inclusion of the dynamic term is that, under our assumptions about the data generating process, coefficient estimates of equation 3 including fixed effects are biased and inconsistent. This is because the lagged term is correlated with the error term $\varepsilon_{it} = \varepsilon_{it} - \bar{\varepsilon}_i$. This is what is commonly referred to as dynamic panel bias. The most straightforward method for dealing with this bias is to transform the estimating equation by taking first-differences. This eliminates the fixed effects term:

We remove the fixed effects in equation 3 by taking first-differences,

$$\Delta r_{it} = \Delta r_{i,t-1}\gamma + \Delta g^*_{it}\delta + \Delta X'_{it}\beta + \Delta d_t + \Delta\varepsilon_{it}. \tag{4}$$

However, regression estimates of equation 4 are inconsistent because $\Delta r_{i,t-1}$ and $\Delta\varepsilon_{it}$ are necessarily correlated through the common component $\varepsilon_{i,t-1}$. The advantage of the first-differences transform is that, unlike the within groups estimator, longer lags of the regressors are now available as instruments. As long as there is no serial correlation in $\Delta\varepsilon_{it}$, the twice lagged level of rental rates, $r_{i,t-2}$, and subsequent lagged levels are valid instruments. Such instruments can address bias in the lagged dependent variable and in other potential endogenous regressors, as shown next.

4.1.2 Expectation Error

The availability of instrumental variables from within the dataset can also address endogeneity problems in other variables due to expectation errors. The expected government payment variable is expressed separately because the coefficient estimate is of interest and because as analysts, we do not observe expected subsidy payments, only realized payments that occur after harvest. If expected government payments differs from its realized value by a linear error term, we can rewrite equation 4 as

$$\Delta r_{it} = \Delta r_{i,t-1}\gamma + \Delta(g_{it} + \eta_{it})\delta + \Delta X'_{it}\beta + \Delta d_t + \Delta\varepsilon_{it}, \tag{5}$$

where η_{it} is the aforementioned linear expectation error term for government payments. Angrist and Pischke (2009, p. 226) suggest that in the case of measurement error in variables in a panel data context with fixed effects, the econometrician has two options: employ instrumental variables or use external information on the extent of the measurement error to adjust naive coefficient estimates. We have limited information about the extent to which expected government payments and realized government payments vary for specific farms. To the extent that changes in prices between planting and harvest determine the measurement error due to expectations, we could employ futures prices as a proxy for expected prices and government-set loan rates to approximate η_{it}. Given the potential to introduce further error, we use the instrumental variables approach. So long as there is no serial correlation in η_{it}, then $g_{i,t-2}$ and further lags are valid instruments for Δg_{it}.

However, we should also consider the potential for expectations to introduce attenuation bias in other coefficients. Of the covariates listed, revenue is also likely to be measured with error because cash rental rates are agreed upon before sales revenue is realized. Variable production costs are also likely to be prone to expectation error problems. Uncertainty regarding inputs prices creates expectation errors. While some input quantities are determined prior to planting, other inputs are adjusted throughout the crop year in response to agronomic and economic conditions.

4.1.3 Arellano-Bond

We have demonstrated that the lagged levels are valid instruments in our estimating equation. In a short panel such as ours that can be characterized as "small T, large N", using two stage least squares to incorporate these instruments as in the Anderson and Hsiao (1982) estimators is problematic. If we want to increase the efficiency of our estimator by adding additional lags as instruments, our sample size shrinks. The Arellano-Bond estimator avoids this problem. As detailed by Holtz-Eakin, Newey, and Rosen (1988) and Arellano and Bond (1991), using a wider instrument set that incorporates all available lags as instruments increases the efficiency of estimation without losing observations.

A specification issue with the Arellano-Bond estimator is choosing the appropriate number of instruments. Whereas valid instruments are usually in short supply, the Arellano-Bond estimation procedure can generate instruments prolifically. While there are efficiency gains in using all available instruments, Roodman (2006) notes that the use of too many instruments rapidly increases the size of the estimated variance matrix to the point where it is near singular. This may result in poor inference. With no strong prior knowledge about the appropriate number of lags to employ, we tested numerous specifications and used econometric tests and economic intuition to choose the number of lags.

4.2 Tenancy Arrangements

The Kansas Farm Management data do not allow us to differentiate between cash rented and crop shared acres. This implies that the dependent variable is measured with error. Additional survey data from Kansas State University suggests that crop share arrangements are more prevalent in Kansas than in other agricultural regions of the United States. Measurement error in the dependent variable will only bias our coefficient estimates if the error is correlated with the regressors. Survey data indicates that cash rental arrangements have been increasing over time. The measurement error in Δr_{it} decreases when the number of cash rental arrangements increases. If there are only two years of data, then there is more potential for bias as the change in government payments may have some correlation with the trend in rental arrangements. However, from 1990 to 2008 there is not a clear trend in government payments so it is unlikely to be significantly correlated with the measurement error. Indeed, the expected government payments depends on market conditions which varied throughout the sample period.

4.3 Interpreting Coefficients in a Partial Adjustment Framework

The presence of the lagged dependent variable in our model makes coefficient interpretation different than in previous literature. Because rents are subject to inertia, there is an adjustment process whereby rents are determined in part by expectations of future revenues, both market and government, and by previous period rents, because of contract rigidity. This adjustment process recalls similar phenomena in the economic literature on supply response. Nerlove (1958) and Griliches (1967) show that if the agents can be thought of as seeking a long-run equilibrium level in the dependent variable, but can only make a partial adjustment towards this level in a given period, then the coefficients from a distributed lag econometric model can be used to calculate a long-run response. Using notation from equation (4), it follows from the partial-adjustment model that the short-run incidence of government subsidies on rental rates is δ and the long-run incidence is $\delta/(1-\gamma)$. The weakness of this method of interpretation is that it assumes that the adjustment parameter is fixed over time and across individuals.

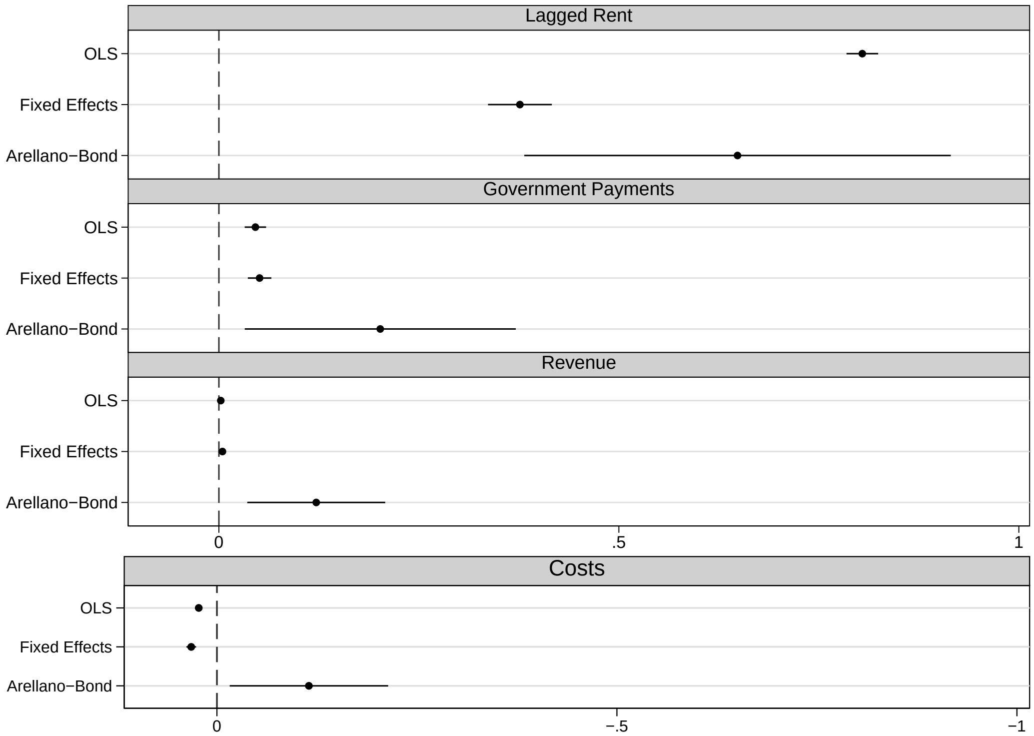
4.4 Estimation Results

The coefficient estimates and 95% confidence intervals for the variables we are primarily interested in are displayed in Figure 1. The confidence intervals are computed using standard errors clustered at the farm-level for all specifications. Coefficients on the proportion of rented acres irrigated and in pasture and year fixed effects are included in all the regressions but are not displayed in the Figure. We report the coefficient estimates and standard errors of the Arellano-Bond estimator in a table in the appendix.

We find evidence of first order autocorrelation in the error term. Therefore, the first valid instrument for the lagged difference of rent is the third lagged level of rent. The twice lagged level of government payments, revenue, and costs are valid instruments. We use up to 5 lags of rent, revenue, and costs as instruments while we only use up to 3 lags of government payments as instruments. The number of instruments was chosen based on econometric tests and our intuition. The null hypothesis of the exogeneity of the instruments was rejected using the Hansen test when too many instruments were included. An instrument is valid if it is uncorrelated with the error term and correlated with the variable it is instrumenting. We believe that further lags of rent, revenue, and costs are likely to contain information about their current realization more so than for further lags of government payments. Thus we include fewer lags as instruments for government payments.

We borrow a procedure from Roodman (2006) in order to frame our estimations. He notes that estimating (3) using Ordinary Least Squares (OLS) on the pooled cross section provides a theoretical upper bound on the lagged rent coefficient, because this coefficient is biased upward due to positive correlation between observed shocks and the unobserved fixed effect that is part of the error term. If the same model is estimated using fixed effects, then the coefficient on lagged rent is biased downward. The transformation implied by the within-groups estimator makes the lagged rent term correlated negatively with the contemporaneous

Figure 1: Coefficient Estimates and 95% Confidence Intervals from Kansas Farm Management Data



portion of the error term. As seen in Figure 1 our estimate of the coefficient with Arellano-Bond is 0.65 and lies between these bounds. The large coefficient on lagged rent suggests that inertia in the rental market is substantial.

Figure 1 also illustrates the attenuation bias from expectation error in government payments, revenue, and costs. The coefficients are all close to 0 and the coefficient on cost is actually of the opposite sign we would expect in the OLS and fixed effects regressions. After using previous values as instruments with the Arellano-Bond estimator, the coefficients all increase into a plausible range. We do not expect the coefficients on revenue and costs to equal 1 because these are short-run estimates, however in the long-run we would expect that revenues and costs should be nearly fully capitalized into rental rates. It is interesting to note that our estimates suggest that government payments are capitalized into rental rates about the same as revenue and costs, if not even more.

The coefficients from the Arellano-Bond results suggest that in the short-run a $1 increase in expected government payments increases land rent by $0.20, while in the long-run it increases land rent by $0.57. These values suggest that the incidence estimate of Kirwan (2009) is accurate in the short run and that the Note that these estimates are for the whole package of coupled and decoupled payments received from 1990 to 2008. We calculated the confidence interval for our long-run estimate using the delta method, but it was very large. It is difficult to get a precise estimate of long-run incidence because it is the ratio of two variables which are estimated with error and instrumental variables gives us large standard errors in our model.

The coefficients on proportion irrigated and proportion in pasture are 0.45 and -17.58. The coefficient on proportion irrigated is only identified when a farm changes the proportion of acres irrigated and we found that this occurred for a very small portion of our sample and usually the change was very small. Thus, there is very little variation in our sample to identify the coefficient so it is unreasonably small. Pasture rent is much lower than nonirrigated rent and there was more variation in this variable so the estimate is reasonable.

5 Empirics: U.S. County Data

Our second dataset is a national cross-section of cash rents for 1,236 counties obtained from the National Agricultural Statistics Service (NASS) for 2008. These data do not confound cash rent and crop shared acres. We construct a measure of direct government payment per acre using Economic Research Service (ERS) data on base acres and base yields for each county. Two indicators of expected revenue and costs are used. The first indicator of expected revenue is constructed by predicting yields in 2008 for each crop using a simple regression of yields on a time trend for county-level data from 1980 to 2007. Expected revenue is computed as a weighted sum across all crops of the 2008 marketing year average price times the predicted yield, where the weights are the acres planted to the crop divided by the sum of acres planted to all crops.[2] Expected costs of each crop were taken from the Cost Return Studies by the ERS and merged at the ERS Farm Resource Region level. We only consider counties in our analysis where the major crops are corn, soybeans, wheat, cotton, sorghum, barley, oats, peanuts, or rice. Alternative indicators of expected revenue and costs are the revenue and costs per acre obtained from the 2007 Census of Agriculture at the county level. The final dataset for analysis includes 999 counties.

5.1 Empirical Model

Identification with the dataset of county-level cash rents is different than with the Kansas farm-level panel. With the panel of Kansas farms we identify the change in rents due to changes in government payments over time. The data of county-level cash rents do not have a time dimension, so we identify the difference in cash rents across counties with different

[2] Similar results were obtained using the 2007 marketing year average price.

direct payments. The disadvantage of using the county-level data is that we cannot control for heterogeneity across farms.

The county-level data provide a unique opportunity to estimate subsidy incidence due to the price spike of commodities which occurred in 2007. Expected government payments are relatively easy to construct because farmers knew there would be no deficiency or counter cyclical payments.[3] The drastic increase in prices also led to renegotiation of many rental contracts.

However, simply regressing rents on direct payments, revenue, and costs yields inconsistent estimates because any indicator of expected revenue and costs is subject to measurement error. Consistent estimates are obtained by using alternative indicators of expected revenue and costs as instruments, so long as the measurement errors of the indicators are uncorrelated (Wooldridge, 2002, p. 107). We use expected revenue and costs which were constructed from yield trends and USDA cost studies as the primary indicators in our regression and use revenues and costs in 2007 from the census as instruments.

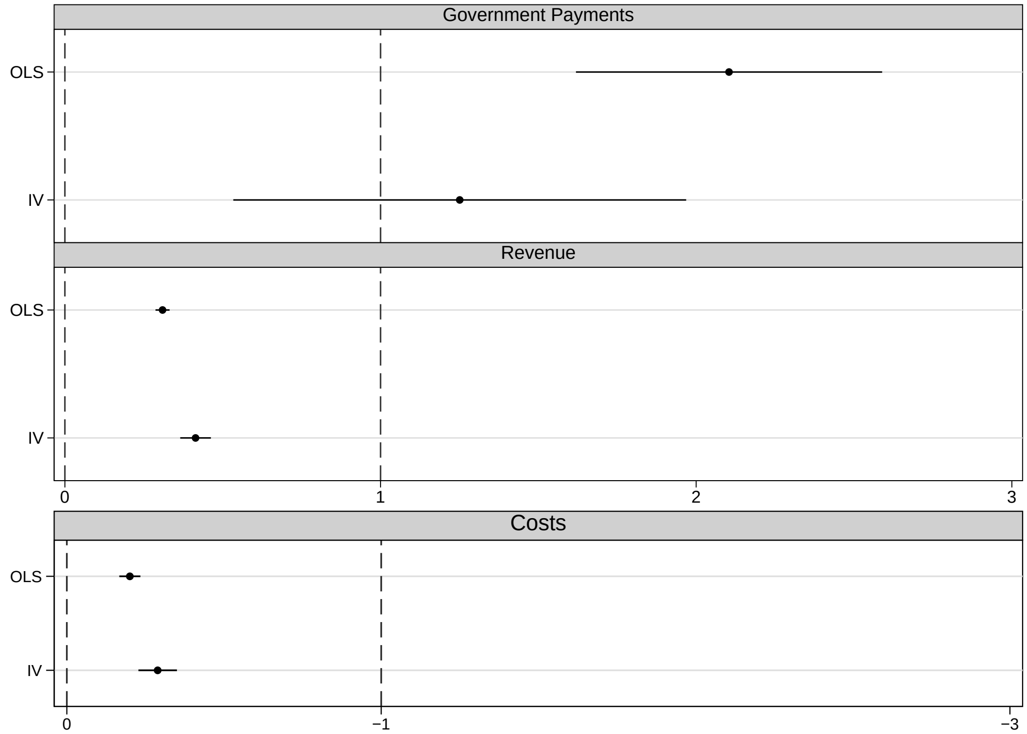
5.2 Estimation Results

As expected the coefficients on revenue and costs are biased towards 0 with simple OLS, visualized in Figure 2. The measurement error in revenue and costs also biases the coefficient on direct payments. The effect of direct payments is biased upward with OLS because direct payments are positively correlated with revenue and costs, so the coefficient on direct payments is also capturing some of the effect of revenue and costs. After instrumenting revenue and costs the coefficient on direct payments decreases. Our point estimate suggests that if a county has \$1 more of direct payments, cash rents in the county are \$1.25 higher, holding all else constant. Unfortunately, our estimate is not precisely estimated as instrumental variables increases the standard error and our sample is relatively small. Nevertheless, we find no reason to reject the assertion that direct payments are nearly fully capitalized into land rents.

It is a concern that the coefficients on revenue and costs are still less than 0.5 in absolute value even after instrumenting. In order to improve the estimates we suggest two directions for future research. First, uncertainty in expected revenue and costs should be incorporated into the analysis. Cash rents will be higher with higher expected revenue, but they will also be higher in counties with a lower variance of revenue. Second, the spatial nature of the data warrants consideration for future analysis.

[3] Payments from conservation programs (e.g., CRP, EQIP, and CSP) are not included. We anticipate any bias from omitting these payments to be relatively small.

Figure 2: Coefficient Estimates and 95% Confidence Intervals from US County Data



6 Concluding Remarks

This paper examines the relationship between government subsidy payments and farmland rental rates. The incidence is estimated with Kansas farm-level panel data from 1990 to 2008 using a differenced generalized method of moments estimator. Lagged realizations of the variables provide instruments to correct for expectation errors in government payments, revenues and costs. There is substantial inertia in the rental market as evidenced by a large coefficient on the lagged rental rate. We find that government payments are capitalized into rental rates at about the same rate as revenue and costs. In the short run another $1 of government payments increased rents by $0.20 and in the long run it increased rents by $0.57.

We also estimate the incidence of direct payments using U.S. county-level data in 2008. Direct payments are a significant component of farm commodity program payments. Using national data from ERS we calculate that from 1996 to 2008 about 60% of commodity program payments (i.e., direct, counter-cyclical, or loan deficiency payments) were direct payments. High commodity prices remove expectation error about government payments, but there is still error in our measure of revenue and costs. Again, we use instrumental variables to correct for the measurement error using an alternative indicator of revenue and costs as instruments. We estimate that if a county has another \$1 of direct payments, rental rates are \$1.25 higher in that county, holding all else constant.

Unfortunately, the standard errors of our long run incidence estimate with the Kansas data and the incidence estimate of direct payments with the U.S. data are quite large. The large standard errors are due primarily to the use of instrumental variables techniques. However, overall we find no evidence to reject the notion that government payments are highly capitalized into land rental rates in the long run.

Appendix

Table 2: Arellano-Bond Estimates for Kansas Data

Variable	**Coefficient**
Lagged Rent	0.648** (0.136)
Gov't Payments	0.202* (0.086)
Crop Revenue	0.122** (0.044)
Production Cost	-0.115* (0.051)
Percent Irrigated	0.451 (4.203)
Percent Pasture	-17.581** (2.671)
Sample Size	18,368

Table 3: IV Estimates for U.S. County Data

Variable	**Coefficient**
Direct Payments	1.251** (0.366)
Crop Revenue	0.414** (0.025)
Production Cost	-0.289** (0.031)
Intercept	-18.918 (10.517)
Sample Size	999

References

Alston, J.M. 1986. "An Analysis of Growth of U.S. Farmland Prices, 1963-82." *American Journal of Agricultural Economics* 68:1–9.

—. 2007. "Benefits and Beneficiaries from U.S. Farm Subsidies." In *The 2007 Farm Bill and Beyond: Working Papers*. American Enterprise Institute.

Anderson, T.W., and C. Hsiao. 1982. "Formulation and estimation of dynamic models using panel data." *Journal of Econometrics* 18:47 – 82.

Angrist, J.D., and J.S. Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.

Arellano, M., and S. Bond. 1991. "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations." *The Review of Economic Studies* 58:277–297.

Barnard, C., K. Wiebe, and V. Breneman. 2003. "Urban Influence: Effects on US Farmland Markets and Value." In C. B. Moss and A. Schmitz, eds. *Government Policy and Farmland Markets*. Ames, IA: Iowa State Press, pp. 319–341.

Cameron, A., and P. Trivedi. 2005. *Microeconometrics: Methods and Applications*. New York, NY: Cambridge University Press.

Floyd, J.E. 1965. "The Effects of Farm Price Supports on the Returns to Land and Labor in Agriculture." *Journal of Political Economy* 73:148–158.

Goodwin, B.K., and A.K. Mishra. 2006. "Are "Decoupled" Farm Program Payments Really Decoupled? An Empirical Evaluation." *American Journal of Agricultural Economics* 88:73–89.

Goodwin, B.K., A.K. Mishra, and F. Ortalo-Magne. 2010. "The Buck Stops Where? The Buck Stops Where?: The Distribution of Agricultural Subsidies." In J. Perloff, ed. *Proceedings of the NBER Meeting on Agricultural Economics and Biofuels*. Cambridge, MA.

Griliches, Z. 1967. "Distributed Lags: A Survey." *Econometrica* 35:16–49.

Hennessy, D.A. 1998. "The Production Effects of Agricultural Income Support Policies under Uncertainty." *American Journal of Agricultural Economics* 80:46–57.

Holtz-Eakin, D., W. Newey, and H.S. Rosen. 1988. "Estimating Vector Autoregressions with Panel Data." *Econometrica* 56:1371–1395.

Kirwan, B.E. 2009. "The Incidence of U.S. Agricultural Subsidies on Farmland Rental Rates." *Journal of Political Economy* 117:138–164.

Lence, S.H., and A.K. Mishra. 2003. "The Impacts of Different Farm Programs on Cash Rents." *American Journal of Agricultural Economics* 85:753–761.

Nerlove, M. 1958. "Distributed Lags and Estimation of Long-Run Supply and Demand Elasticities: Theoretical Considerations." *Journal of Farm Economics* 40:301–311.

Patton, M., P. Kostov, S. McErlean, and J. Moss. 2008. "Assessing the influence of direct payments on the rental value of agricultural land." *Food Policy* 33:397 – 405.

Roberts, M.J., B. Kirwan, and J. Hopkins. 2003. "The Incidence of Government Program Payments on Agricultural Land Rents: The Challenges of Identification." *American Journal of Agricultural Economics* 85:762–769.

Roodman, D. 2006. "How to do Xtabond2: An Introduction to Difference and System GMM in Stata." Working paper 103, Center for Global Development, Washington, DC.

Schmitz, A., and R.E. Just. 2003. "The Economics and Politics of Farmland Values." In C. B. Moss and A. Schmitz, eds. *Government Policy and Farmland Markets*. Ames, IA: Iowa State Press, pp. 53 – 79.

Sumner, D.A. 2003. "Implications of the US Farm Bill of 2002 for agricultural trade and trade negotiations." *Australian Journal of Agricultural and Resource Economics* 47:99–122.

Wooldridge, J. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.