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Subsidy Incidence and Inertia in Farmland Rental Markets: Estimates from a Dynamic Panel

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Recent econometric studies indicate that the effect of government farm subsidies on farmland rental rates may be smaller than once thought. This literature has corrected for bias due to expectation error in measured subsidy payments. We suggest two additional sources of bias—inertia and tenancy arrangements—that may explain the discrepancy between theoretical predictions and empirical estimates of subsidy incidence. We identify a model that accounts for these issues, employ panel data from Kansas to estimate it, and find that an additional dollar per acre of government subsidy increases rental rates by $0.12 per acre in the short run and $0.37 per acre in the long run.

Key words: cash rent, farm subsidies, incidence

Introduction

The economic incidence of a government subsidy refers to the distribution of subsidy benefits after allowing for changes in decisions it causes. Economists have long noted that the economic incidence of a subsidy is likely to differ from the statutory incidence; the named recipient of the subsidy payment is unlikely to capture all of the benefits, because subsidy-induced changes in input use are likely to result in changes in some input prices. In the case of agricultural subsidies such as those provided by the U.S. Farm Bill, many economists and other pundits argue that subsidy payments to farmers are likely to be capitalized into land values such that landowners garner the benefits of the programs. Since nonfarming landowners own about 42% of farmland, a substantial portion of subsidy payments may be captured by nonfarmers.

Agricultural subsidies in the United States transfer a relatively large amount of government support to a small proportion of the population. Government budget cuts threaten to eliminate these transfers. The political feasibility of subsidy reform and the welfare impact of subsidy elimination depend on who ultimately benefits—farm operators or landowners. If farm operators capture most of the subsidy benefit, then subsidy removal implies a considerable welfare loss to farmers, especially small farmers who are more reliant on subsidies as a share of farm revenue (Kirwan, 2007). On the other hand, a substantial entitlement exists when most of the subsidy benefit is reflected in land values. Removing this entitlement is politically difficult because current landowners believe they have acquired the right to collect these subsidies via the purchase of subsidy-inflated farmland. The entitlement creates considerable incentive for rent-seeking and rent-preserving behavior.

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In a series of articles, Alston has shown that restrictive theoretical assumptions about the supply elasticity of land, the demand elasticity of agricultural output, and the nature of agricultural production technology are required for the incidence of an output subsidy to be entirely captured in changing per period prices for land (Alston and James, 2002; Alston, 2007, 2010). Since output subsidies form at least part of U.S. Farm Bill programs, the relevant challenge for empirical analysis of the incidence of these programs is not determining whether landowners benefit but measuring the shares of program benefits captured by landowners, farmers, and others. These articles suggest that \$0.40-\$0.60 of a per acre subsidy dollar is reflected in increased rental rates for farmland.

As a test of these theoretical predictions, the magnitude of subsidy incidence on farmland rental rates has been estimated econometrically using reduced-form regressions of rental rates on determinants such as market earnings, government payments, and land productivity measures. Kirwan (2009) suggests that the subsidy incidence on rental rates may be as low as 20%. This considerable discrepancy between theoretical predictions and empirical results suggests that existing models relating government subsidy payments to changes in farmland rents may be incomplete.

Expectation error is a widely recognized problem in identifying the subsidy incidence: actual government payments are observed, but rental rates are determined by (unobservable) expectations about uncertain future payments. This creates measurement error in the data and requires the identification of valid instrumental variables to address the resulting bias it causes for econometric estimation.

We suggest two additional sources of bias—inertia and tenancy arrangements—that may explain the discrepancy between theoretical predictions and empirical estimates of subsidy incidence. First, static models may not account for the possibility of "inertia" in rental rates; landowners and farmer operators may not adjust rents contemporaneously with changes in returns to crop production, because rates are subject to multiyear contracts or because of social norms, such as family ties between the landowner and operator. Inertia implies that the incidence in the long run will be greater than the incidence in the short run. Second, currently available data do not contain enough information on rental agreements between operators and landowners. Ideally, we would measure the effect of our explanatory variables on the amount of cash rent per cash-rented acre. However, available data, both ours and others', do not separately identify acres rented under cash or share arrangements.

We contribute to the literature on the incidence of farm subsidy payments by addressing bias from these two additional sources with a panel dataset of Kansas farmers during the period 1990-2008. We use a dynamic panel GMM estimator to account for bias in the estimates of the coefficient on the lagged dependent variable and bias from expectation error in government payments and farm profits by using lagged levels as instruments. We analytically derive the bias implied by mismeasurement of rental rates in our data, which is shown to depend on parameters that we can measure using survey data external to our main dataset.

We compare results from the GMM estimator to fixed-effects and ordinary least squares (OLS) estimates that are biased but more efficient. Estimates of the short-run impact of government subsidy payments on rental rates are similar across specifications, but fixed effects and OLS are more efficient. Fixed-effects estimates of the long-run incidence of subsidy payments are biased downward and OLS estimates are biased upward, providing bounds on reasonable estimates of the long-run impact. Our GMM estimator produces a point estimate of the long-run incidence that lies between these bounds.

After adjusting our estimates for the bias caused by mismeasured rental rates, we find that a marginal subsidy dollar increases land rent by \$0.12 per acre in the short run. Our incidence estimate rises when we allow rental rates to adjust to their long-run values. Using the GMM estimate of rental inertia, the incidence rises to \$0.32 per acre when allowing for a five-year adjustment period, and to \$0.37 per acre in the long run. This lies within the long-run incidence bounds from the fixed-effects and OLS models of \$0.17-\$0.48 per acre. Accounting for inertia and measurement error in rental rates increases our incidence estimate substantially. After addressing these sources of bias,

our estimate of long-run subsidy incidence is close to the predictions made by Alston and James (2002) and Alston (2010) that \$0.40-\$0.60 of a per acre subsidy dollar is captured by landowners.

Policy Background

Much of the theoretical analysis of subsidy incidence necessarily assumes relatively simplistic program structures. These models assume that payments are either coupled or decoupled. If payments are coupled, then they are linked directly to output levels; if payments are decoupled, then they are tied to fixed factors of production, namely land. However, U.S. Farm Bill programs are not purely coupled or purely decoupled. Theory would thus indicate that the incidence of these subsidies as a whole is somewhere between the prediction for purely coupled subsidies and purely decoupled subsidies.

Three Farm Bills are relevant for our sample period: the 1990 FACT Act, 1996 FAIR Act, and 2002 FSRI Act. Three major commodity programs were in place over this period: loan deficiency payments (LDPs), countercyclical payments (CCPs), and direct payments. Other government payments included *ad hoc* disaster payments, crop insurance subsidies, and conservation payments. However, this study focuses on the major commodity programs because they have accounted for the majority of agricultural subsidy expenditures. The major commodity programs apply only to specific program crops. For our analysis of the state of Kansas, program crops make up roughly 85% of cropland acreage.

The farm receives a loan-deficiency payment when realized market price falls below the loan rate, which is set by legislation. This payment applies to current production, which is current yield multiplied by current acreage. Thus when market prices are low, the LDP functions essentially as an output subsidy. Because of the explicit link to realized production in the current period, LDPs are said to be coupled to production.

Countercyclical payments are based on program-eligible yields and acres, determined using historical data on yields and acreage. The CCP is calculated as payment acreage multiplied by payment yield multiplied by the target price minus the maximum of the market price or the loan rate. Direct payments are issued to the farmer regardless of current production or market prices. They are calculated based on program-eligible yields and acres. The key feature of both CCPs and direct payments is that they are based on historical acreage and yields rather than current production. Economists, however, have argued that these programs are not fully decoupled, because they may have production effects through risk reduction (Hennessy, 1998) and expectations that base acreage and yields may be updated (Sumner, 2003).

Government payments issued for a specific acre of farmland depend on the program eligibility of that acre. The economic incidence of these payments is unlikely to be similarly restricted to specific parcels of land. Gardner (2003, p. 93) suggests that farmland prices benefit "more uniformly" across all cropland acres from the existence of commodity programs than is usually reflected in theoretical models of payments tied to specific acres. In other words, receiving payments on program-eligible acres may affect the rental rate the farmer is willing to pay on both program-eligible and non-program-eligible acres. Therefore, we estimate the general effect of all commodity-program payments on land rental rates using rents and government payments at the farm level.

We form expectations about the effect of these programs by first considering the theoretical effect of purely coupled and purely decoupled subsidies. The effect of a purely coupled subsidy can be analyzed as a positive shock to the output price due to a shift in demand. Using a two-factor equilibrium displacement model similar to Floyd (1965) and applying realistic assumptions about theoretical parameters, Alston (2010) calculates that \$0.39 per dollar of a per acre coupled subsidy would accrue to landlords. Theory suggests that purely decoupled payments should be fully reflected in land rents. Alston (2010) suggests that the per acre incidence of U.S. Farm Bill subsidy payments as a whole on land rental rates is \$0.40-\$0.60 per subsidy dollar.

We also form an expectation of the estimated incidence of subsidies relative to other estimated parameters. For an individual farmer, the marginal effect of a coupled subsidy is equivalent to a change in the output price. Since variation in non-subsidy crop revenue is largely due to changes in output prices, we expect the effect of changes in non-subsidy crop revenue on rental rates to be similar to the incidence of a fully coupled subsidy. We expect that the effect of all program payments on land rents will be larger than the effect of crop revenue but not fully reflected in rental rates.

Previous Literature

The canonical economic model of farmland value is the discounted stream of expected benefits from the land. Schmitz and Just (2003) specify this net present value capitalization formula 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 net benefit and the value of farmland.

Several studies have estimated the effect of government payments on land values (e.g., Goodwin and Ortalo-Magne, 1992; Just and Miranowski, 1993; Weersink et al., 1999), but there are several challenges to this approach. First, we do not know the appropriate discount rate, which may vary over time. Second, although agricultural income is the primary source of expected benefits for landowners (Alston, 1986), Y_{t+i} may also include the option value of converting land to commercial or residential use (Barnard, Wiebe, and Breneman, 2003). Third, current and past subsidy payments may provide little information about the expected stream of future government subsidies that affect land values.

The challenge of estimating the effect of government payments on land values is simply that we do not have enough data to identify all of the parameters in the net present value model. If instead we consider the current period price, the cash-rental rate, then these complications are avoided. A direct measure of subsidy incidence on land—the share of government payments captured by landowners—is the change in the annual per acre rental rate for a $1 change in expected government payments. One major identification challenge that remains when using data on cash rents is that the relevant variable for the analysis is the expected government payment rather than the observed government payment.

Empirical Literature Review

Our empirical model of subsidy incidence builds upon three recent articles that apply a similar reduced-form approach to data on rental rates rather than land values. Kirwan (2009), Patton et al. (2008), and Goodwin, Mishra, and Ortalo-Magne (2011) each regress some measure of per acre cash rent on per acre measures of market returns and government payments. These articles differ mainly in terms of the data used and how they address the bias caused by the difference between observed and expected government payments. The use of instrumental variables techniques is common across these articles, though the instruments used vary. Goodwin, Mishra, and Ortalo-Magne (2011) also consider the direct use of proxy measures of expected government payments to address attenuation bias.

Kirwan (2009) assembles a two-year panel of nearly 60,000 farms from the 1992 and 1997 U.S. Census of Agriculture, estimating the effect of subsidy payments on rents using a cross-section of the difference between these periods. The differenced observed government payments are subject to expectation error. High price levels in 1997 give rise to a set of valid instruments: observed government payments in 1997 are unlikely to differ from expected payments because LDPs are not triggered when prices are high and the direct payments were known in advance. This allows Kirwan to use 1997 subsidy levels to instrument for the difference between observed government payments in 1997 and in 1992.

Goodwin, Mishra, and Ortalo-Magne (2011) measure subsidy incidence using a pooled cross-section of farms from USDA survey data collected between 1998 and 2005. They compare proxy and instrumental variables methods to remove expectation bias in government payments and market returns. As a proxy variable for expected payments, they use a four-year average of per acre payments in the county. In their instrumental variables specification, they use lagged payments, futures prices, and lagged county-level returns as instruments. However, their use of external instruments may be problematic; futures prices are unlikely to make good instruments because they are likely correlated with the expectation error in returns and in government payments.

Patton et al. (2008) consider the case of coupled and decoupled European Union livestock-program payments on agricultural land rents in Northern Ireland using panel data from 1994 to 2002. Their dataset is unique in that rental rates are set on an annual basis because of legislation that restricts the length of tenure. They employ a panel system GMM estimator, using lagged values of market revenues and coupled government subsidies as instruments to remove bias resulting from expectation error.

The estimates of the incidence of U.S. farm subsidies from these studies vary widely. Kirwan (2009) estimates that the marginal subsidy dollar increases rents by $0.21 per acre. Goodwin, Mishra, and Ortalo-Magne (2011) estimate the incidence is $0.32 per acre using proxy variables and $1.01 per acre using instrumental variables. In a much different context, Patton et al. (2008) find that the rate of incidence varies greatly for coupled program payments; it may be between $0.40 and $1.00 per acre. They find that decoupled payments are fully reflected in rental rates.

One caveat to these estimates suggested in Alston (2007, 2010) and acknowledged by Kirwan (2009) and Goodwin, Mishra, and Ortalo-Magne (2011) is that these models may capture a short- or intermediate-run effect that is different from the long-run incidence, because rental rates may be slow to adjust to changes in government subsidies. Put differently, rental rates may be subject to inertia, unable to adjust immediately to external shocks. Kirwan attempts to address this by extending the length of difference in his model to assess long-run incidence. He finds results similar to those using the five-year difference.

Patton et al. (2008) suggest that time-series variation in rental rates may help solve econometric identification problems associated with estimating subsidy incidence, including the identification of the long-run incidence. In a panel of sufficient length, lagged variables internal to the dataset are plausibly predetermined and can be used as instruments. This can address bias problems and increase the efficiency of coefficient estimates. The time-series dimension of panel data also allows for the construction of a model that explicitly considers the dynamics of farmland rental rates. Although Patton et al. (2008) had panel data, they did not need to consider rental-market dynamics, because institutional idiosyncrasies in the farmland market in Northern Ireland effectively disconnected observed rental rates across time periods. In the United States, where contracts may set rents for multiple years and landowner-operator relationships are often long-lived and familial, estimates of subsidy incidence may suffer from omitted-variables bias if rental-market dynamics are not included in the model.

Kansas Farm Management Association Data

To address rental-market dynamics among heterogeneous farmers and land types, we require panel data. The dataset we use for our analysis is a farm-level unbalanced panel collected by the Kansas Farm Management Association (KFMA). Members provide detailed accounting information to this association, so the data contain variables on farm revenues and costs similar to the USDA census and survey data used in Kirwan (2009). The full dataset covers thirty-six years with approximately 2,000 farm business units observed each year.

Data prior to 1990 were eliminated because less detailed records were kept during that period. Farms that did not report cash rent paid were removed from the sample. Cash rent and government payments per acre were winsorized at the upper 99th percentile, and revenue and costs per acre were

Table 1. Summary Statistics

Variable	Description	Mean	Std. Dev.	Min.	Max.
r_{it}	Cash rent paid ($/ac)	14.37	15.38	0.001	85.60
g_{it}	Government payments ($/ac)	21.53	14.65	0	73.64
rev_{it}	Crop revenue received ($/ac)	161.41	84.63	27.18	475.04
$cost_{it}$	Production costs ($/ac)	118.23	60.12	21.63	340.70
irr_{it}	Proportion of acres irrigated	0.06	0.16	0	1
$pasture_{it}$	Proportion of acres in pasture	0.28	0.29	0	0.99

winsorized at the upper and lower 99th percentiles. Prior to this, the data were manually cleaned to identify and exclude any obvious data-entry errors. The final dataset is an unbalanced panel from 1990 to 2008 containing 27,439 observations.

A total of 3,251 separate farms are observed throughout the sample period. Observing approximately 1,400 farms per year implies that half of the sample farms are observed for seven years or fewer. Therefore, "small T" econometric methods should be applied to the panel. Nevertheless, since the panel spans nineteen years and three different farm bills, the data offer substantial variation in government payments across time and farms, which can be used to identify the incidence of subsidy payments.

Summary statistics for the variables used in our econometric model are shown in table 1. Similar to Kirwan (2009), our dataset does not contain the actual per acre rental rate for cash-rented acres. 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. Note that we address this issue using a postestimation adjustment discussed below. The government payments variable is the total government payments received divided by total crop acres (i.e., owned and rented). Insufficient data prevented us from subtracting Conservation Reserve Program payments from government payments. Crop revenue is the total value of production divided by total crop acres. The total value of production includes the landowner's share of production and crop insurance proceeds and excludes government payments. Production costs include total crop expenses and the opportunity cost of property and machinery divided by total crop acres. Production costs do not include value of unpaid labor, interest, or cash rent.

KFMA only collects data for farms that voluntarily join the association. These may not be a representative sample of farms in the state. Discussion with those who maintain the database suggests that the data may exclude the smallest farms, which have less incentive to keep detailed financial records, and the largest farms, which may not require the financial analysis services provided by KFMA. The direction of this sample-selection bias is unclear.

We augment the annual farm financial data with additional information about farmland rental markets collected in surveys of Kansas farmers. The survey data describe rental-contract choice, length of tenure, and relationships between landowners and operators that may help explain the incidence of government subsidies on land. Certainly, this additional information lends credence to the idea that farmland rental markets may be subject to inertia. Among farmers in Kansas, tenancy is long-lived and many operators have familial relationships to the landowner. Average length of tenancy is seventeen years, and approximately 40% of landowners are related to the operator.

While the KFMA data are not ideal, they have several advantages over alternative data sources used in previous studies, such as data from the Census of Agriculture and ARMS (Agricultural Resource Management Survey). While the Census of Agriculture provides a comprehensive set of observations, it suffers the same measurement error problem in the dependent variable as the KFMA data and does not provide annual observations from which to estimate inertia. The ARMS data do not suffer from measurement error in the dependent variable but are not a true panel, so that one cannot estimate inertia or control for fixed effects.

The Empirical Model

Our empirical model exploits the availability of panel data but recognizes the challenges of inertia and expectation error in the data. The reduced-form equation that we seek to estimate is:

$$r_{it} = r_{i,t-1}\gamma + \mathbf{X}^*_{it}\beta + f_i + d_t + \varepsilon_{it}, \tag{2}$$

where r_{it} is the average rental rate for farm i in year t and $\mathbf{X}^*_{it}$ is a row vector of covariates including expected subsidy payments, crop revenue, production costs, proportion of acres irrigated, and proportion of acres in pasture. The asterisk is used to emphasize that the "true" covariates in the model are expected government payments, revenue, and costs, even though we only observe realized values of these variables.

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. ε_{it} is the idiosyncratic portion of the error term and we assume $E\left[\varepsilon_{it}|\mathbf{X}^*_{i,1},\ldots,\mathbf{X}^*_{i,T},f_i,d_t\right]=0$. The unobserved heterogeneity of farmers, f_i, is likely correlated with government payments, revenue, and costs. To avoid biased estimates, we remove the fixed effects in equation (2) by taking first differences:

$$\Delta r_{it} = \Delta r_{i,t-1}\gamma + \Delta\mathbf{X}^*_{it}\beta + \Delta d_t + \Delta\varepsilon_{it}. \tag{3}$$

Let β_g denote the element of the vector β that corresponds to government payments. Since β_g is the increase in rental rates due to a \$1 increase in expected government payments in the current year, therefore we define β_g as the measure of subsidy incidence in the short run. We use β_g and γ, the measure of the speed of rental rate adjustment, to define a measure of subsidy incidence over the long run.

Multiple-year contracts and other rigidities in farmland markets prevent contemporaneous adjustment of rental rates to shocks to government subsidy programs. Partial adjustment (Nerlove, 1958) is an approximation to the unknown process by which these shocks are incorporated into rental rates. While in practice operators and landowners may make sharp, discrete adjustments in response to shocks, King and Thomas (2006) suggest that partial adjustment is a good approximation of an aggregate adjustment process. In our case, operators and landowners may adjust rental rates for individual land parcels in discrete jumps, but the rental-rate process for the entire farm may be approximated by partial adjustment. If rental markets follow a partial adjustment process and shocks to rental rates have an infinite time frame to adjust, then the long-run subsidy-incidence measure derived from our empirical model is simply $\beta_g/(1-\gamma)$. We can also use our model to estimate an intermediate-run effect T periods in the future. This effect is $\beta_g(\sum_{t=1}^{T}\gamma^{t-1})$.

Inertia

If we omit lagged rent from equation (3), we will underestimate the subsidy incidence because we only obtain a short-run estimate. But what is the effect on the short-run incidence estimate due to omitting lagged rent from the specification? To simplify the analysis, assume briefly that the true model is $\Delta r_{it} = \Delta r_{i,t-1}\gamma + \Delta g^*_{it}\beta_g + \Delta\varepsilon_{it}$. In other words, current rents are determined only by lagged rent and expected government payments, g^*_{it}. Then the omitted variable bias formula when we omit lagged rent is:

$$\text{plim}\left(\hat{\beta}_g - \beta_g\right) = \text{plim}\left[\left(\Delta\mathbf{g}^{*\prime}\Delta\mathbf{g}^*\right)^{-1}\Delta\mathbf{g}^{*\prime}\Delta\mathbf{r}_{t-1}\gamma\right], \tag{4}$$

where $\mathbf{r}_{t-1}$ and $\mathbf{g}^*$ are vectors of $r_{i,t-1}$ and g^*_{it} stacked across all i and t. Since γ and $\left(\Delta\mathbf{g}^{*\prime}\Delta\mathbf{g}^*\right)^{-1}$ are positive, the sign of the bias depends on the sign of $\text{plim}\left[\left(g^*_{it} - g^*_{i,t-1}\right)(r_{i,t-1} - r_{i,t-2})\right]$.

Expanding the expression in the brackets, we cannot definitively sign each of the terms. However, the contemporaneous variables $r_{i,t-1}$ and $g^*_{i,t-1}$ are almost certainly positively correlated. Thus, differencing the variables induces the potential for the sign of the overall bias to be negative. Because we cannot theoretically predict the bias due to omitting the dynamic term, we leave this as an empirical issue.

Regression estimates of the dynamic specification avoid omitted-variables bias but are inconsistent because $\Delta r_{i,t-1}$ and $\Delta\varepsilon_{it}$ are necessarily correlated through the common component $\varepsilon_{i,t-1}$. This is the "dynamic panel bias" problem first identified in Nickell (1981). The advantage of the first-difference transformation 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 for $\Delta r_{i,t-1}$.

Expectation Error

The availability of instrumental variables from within the dataset can also address endogeneity problems in other variables attributable to expectation error. Rental rates are determined by expected government subsidies, revenues, and costs, but we observe only realized values after harvest. There is no expectation error in the direct-payments portion of government payments. However, countercyclical payments depend on market prices and loan-deficiency payments depend on market prices and production, which are not known with certainty when a rental rate is negotiated. The difference between expected government payments and realized payments is a form of measurement error. Realized crop revenue and production costs have the same problem because production and prices of inputs and outputs are uncertain. While some input quantities are determined prior to planting, other inputs are adjusted throughout the crop year in response to agronomic and economic conditions.

If expected values differ from their realized values by a linear error term, we can rewrite equation (3) as:

$$\Delta r_{it} = \Delta r_{i,t-1}\gamma + \Delta(\mathbf{X}_{it} + \eta_{it})\beta + \Delta d_t + \Delta\varepsilon_{it}, \tag{5}$$

where η_{it} is the linear expectation error term. Angrist and Pischke (2009) suggest two options to address measurement error in variables in a panel data context with fixed effects: employ instrumental variables or use external information on the extent of the measurement error to adjust naïve coefficient estimates. We have limited information about the extent to which expected and realized government payments vary for specific farms, so we use the instrumental variables approach. So long as there is no serial correlation in η_{it}, then $\mathbf{X}_{i,t-2}$ and further lags are valid instruments for $\Delta\mathbf{X}_{it}$.

Lence and Mishra (2003) argue that lagged levels are valid instruments since they are part of the farmer's information set when rental rates are determined. Government payments from previous years are known when forming an expectation of the current government payments and will influence the expectation. However, government payments from previous years are not likely to be correlated with the difference in current expected government payments and current realized government payments.

Improving Efficiency

We have argued that lagged levels are valid instruments to remove dynamic panel bias and bias due to expectation errors in our differenced equation. Anderson and Hsiao (1982) propose the use of two-stage least squares with one period of appropriately lagged levels as instruments. 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. The

Arellano-Bond estimator incorporates additional lags as instruments without shrinking the sample size available for estimation by constructing a separate instrument for every available lag in each time period.

Blundell and Bond (1998) show that if r_{it} is close to a random walk (i.e., γ close to 1), then lagged levels are weak instruments for differences; that is, past lags do not convey much information about future changes. Arellano and Bover (1995) proposed a system GMM estimator that estimates the model in levels as well as in differences, using lagged differences as instruments for the levels equation. However, the system GMM estimator requires the additional assumption that deviations from the long-run equilibrium rent in the initial period are uncorrelated with the level of the long-run equilibrium rent for each farmer.

Whereas valid instruments are usually in short supply, the Arellano-Bond estimation procedure can generate instruments prolifically because any lagged level is a valid instrument. While this may yield efficiency gains, Roodman (2009a) notes that a large number of instruments can overfit endogenous variables. In the extreme, where the number of instruments equals the number of observations, the R^2 in the first-stage regression is one and the second-stage coefficients are the ordinary least squares estimates, which we know to be biased. This points to a bias-efficiency trade-off in using additional instruments.

Additional work by Roodman (2009b) suggests two strategies to avoid the problem of too many instruments. First, the instrument matrix can be collapsed by only constructing instruments for each additional lag—substituting zeros where those lags are not available—rather than constructing an instrument for each lag in each period. The moment conditions implied by the collapsed instrument matrix are weaker than the uncollapsed moment conditions. The moment conditions corresponding to the collapsed instrument matrix state that the lagged level is on average orthogonal to the differenced error term across the estimation period.

Second, Roodman (2009b) suggests excluding longer lags as instruments so that the number of lags used as instruments in any period is capped. He suggests varying the number of lags chosen and analyzing the sensitivity of the coefficient estimates, as the econometric literature gives little guidance about selecting the optimal number of instruments in a panel data setting. Standard diagnostic tests for overidentification restrictions and weak instruments must be used with caution for panel GMM estimation.

Measurement Error in Rental Rates

The Kansas Farm Management Association data, like the Census of Agriculture data, do not differentiate between cash-rented and crop-shared acres. Thus we are forced to construct our dependent variable as Kirwan (2009): total cash rent divided by cash- and share-rented acres. Kirwan (2009) interprets the problem as a measurement error in the dependent variable, which only biases estimates if the measurement error is correlated with independent variables. He estimates the magnitude of the bias by regressing the measurement error on the independent variables using a cross-section from an alternative dataset, which distinguishes between cash- and share-rented acres. Kirwan (2009) concludes that the bias is small.

We take an alternative approach to addressing the measurement error problem by deriving the bias in the estimates analytically. The bias is shown to depend on parameters that can be obtained from survey data. Our formula for the bias allows us to correct our econometric estimates. To simplify the derivation, assume that we can observe expected values of the independent variables. Assume that the true model is:

$$\frac{R_c}{a_c} = \gamma \frac{R_{c,t-1}}{a_{c,t-1}} + \beta_\pi \frac{\Pi_c}{a_c} + \beta_g \frac{G_c}{a_c}, \tag{6}$$

where R_c is total cash rent, a_c is acres cash rented, Π_c is total net revenue on cash-rented acres, and G_c is total government payments on cash-rented acres. However, given our data limitations, we

estimate the following model:

$$\frac{R_c}{a_c + a_s} = \hat{\gamma}\frac{R_{c,t-1}}{a_{c,t-1} + a_{s,t-1}} + \hat{\beta}_\pi \frac{\Pi_c + \Pi_s + \Pi_o}{a_c + a_s + a_o} + \hat{\beta}_g \frac{G_c + G_s + G_o}{a_c + a_s + a_o}, \tag{7}$$

where the subscripts s and o denote values of the variables on crop-share-rented and owned land, respectively.

We assume that per acre net revenue is the same for rented and owned land, such that $\pi = \frac{\Pi_c}{a_c} = \frac{\Pi_s}{a_s} = \frac{\Pi_o}{a_o}$, and that government payments per acre are the same for owned and cash-rented land, $g = \frac{G_c}{a_c} = \frac{G_o}{a_o}$. However, under a crop-share-rental agreement, the statutory incidence of the subsidy payment dictates that the operator only receives a proportion, equal to the share rate, of the government payments. If ρ denotes the share of revenue received by the operator as specified in the rental contract, then $\frac{G_s}{a_s} = \rho g$. Then we can rewrite equation (7) as:

$$\frac{R_c}{a_c}\frac{a_c}{a_c + a_s} = \hat{\gamma}\frac{R_{c,t-1}}{a_{c,t-1} + a_{s,t-1}} + \hat{\beta}_\pi \pi + \hat{\beta}_g g \frac{a_c + \rho a_s + a_o}{a_c + a_s + a_o}. \tag{8}$$

Replacing $\frac{R_c}{a_c}$ with the true model in equation (6) and rearranging yields:

$$\begin{aligned} \gamma\frac{R_{c,t-1}}{a_{c,t-1}} + \beta_\pi \pi + \beta_g g \quad = \quad & \hat{\gamma}\frac{R_{c,t-1}}{a_{c,t-1}}\frac{a_{c,t-1}}{a_{c,t-1} + a_{s,t-1}}\frac{a_c + a_s}{a_c} + \hat{\beta}_\pi \pi \frac{a_c + a_s}{a_c} + \\ & \hat{\beta}_g g \left(1 + (\rho - 1)\frac{a_s}{a_c + a_s + a_o}\right)\frac{a_c + a_s}{a_c}. \end{aligned} \tag{9}$$

Using equation (10), we can write the true parameters in terms of the parameter estimates feasible with our data (denoted with a hat) and other parameters that can be obtained from available survey data. Let σ denote the fraction of rented acres cash rented and ϕ denote the fraction of acres rented, then the true parameters can be expressed as:

$$\gamma = \hat{\gamma}\frac{\sigma_{t-1}}{\sigma}; \tag{10}$$

$$\beta_\pi = \hat{\beta}_\pi \frac{1}{\sigma}; \tag{11}$$

$$\beta_g = \hat{\beta}_g \left(1 + (\rho - 1)(1 - \sigma)\phi\right)\frac{1}{\sigma}. \tag{12}$$

Since the fraction of cash-rented acres has been increasing over time, $\hat{\gamma}$ is biased upward. However, $\hat{\beta}_\pi$ and $\hat{\beta}_g$ are biased downward, where the bias of β_g is smaller, since the term in parentheses in equation (12) is less than one.

Estimation Results

Point estimates and standard errors are reported in table 2 for the dynamic GMM, fixed-effects, and OLS specifications. The dynamic GMM specification is a two-step difference GMM estimator of equation (3) that uses a maximum of four lags as instruments, where we collapse the instruments as described previously. The Arellano-Bond test finds evidence of first-order autocorrelation in the error term, so the first valid instrument for the lagged difference of rent is the third lagged level. Thus, our set of excluded instruments for the dynamic GMM specification includes the third, fourth, fifth, and sixth lagged levels of rent and the second, third, fourth, and fifth lagged levels of government payments, revenue, and costs. For GMM, fixed effects, and OLS, we weight the estimation by the

Table 2. Coefficient Estimates with Alternative Estimators

Variable	Dynamic GMM	Fixed Effects	OLS
Lagged Rent	0.675***	0.408***	0.805***
	(0.199)	(0.025)	(0.011)
Government Payments	0.071	0.060***	0.056***
	(0.090)	(0.010)	(0.009)
Crop Revenue	0.057**	0.005**	0.004***
	(0.029)	(0.002)	(0.002)
Production Costs	−0.024	0.036***	0.022***
	(0.066)	(0.004)	(0.002)
Percentage Irrigated	1.666	1.952	−3.745***
	(4.125)	(1.877)	(0.679)
Percentage Pasture	−17.888***	−10.855***	−2.100***
	(3.495)	(0.925)	(0.267)
Sample Size	16,501	22,352	22,352

Note: The dependent variable is cash rent paid divided by total acres rented. Standard errors are in parentheses. Double and triple asterisks (* and **) indicate significance at the 5% and 1% levels. Coefficient estimates on year fixed effects are not reported.

level of crop acres rented so that our coefficient estimates are interpreted as average effects across acres rather than across farms. We report panel-robust standard errors for each specification.[1]

Figure 1 displays the coefficient estimates and 95% confidence intervals for the variables of primary interest across alternative specifications. Note that all results are interpreted on a per acre basis. It is also interesting to compare the magnitude of the coefficients across variables since we expect their absolute values to be between 0 and 1. Note that we reverse the horizontal axis for the cost variable since we expect the coefficient to be negative on costs. In addition to the variables in the figure, we also include percentage of land irrigated, percentage of land in pasture, and year fixed effects as independent variables.

First, we compare estimates of the coefficient on the lagged dependent variable among dynamic GMM, fixed effects, and OLS. Roodman (2009a) states that the fixed-effects estimate of the coefficient on lagged rent should be biased downward and the OLS estimate should be biased upwards. Thus, fixed effects and OLS provide bounds on a reasonable estimate of the coefficient on the lagged dependent variable. The dynamic GMM estimate of the coefficient on the lagged dependent variable (0.675) provides a point estimate that lies between fixed-effects (0.408) and OLS (0.805) estimates but has a much larger confidence interval than fixed effects or OLS. The dynamic GMM estimate of γ indicates that there is substantial inertia in the land rental market.

Next, we compare estimates of the coefficient on government payments among dynamic GMM, fixed effects, and OLS. The fixed-effects estimate of the coefficient on government payments, β_g, is 0.060 and the OLS estimate is 0.056; both estimates are significantly different from zero. We know from theory that expectation error should bias fixed-effects and OLS coefficient estimates downward, so a theoretically valid estimate of β_g should be larger than 0.06.[2] The estimate of the coefficient on government payments from the dynamic GMM specification is 0.07, but the coefficient is not statistically different from zero. The use of instruments to remove the bias from expectation errors and dynamic panel bias leads to a large loss in efficiency, such that the fixed-effects and OLS estimates of the coefficient on government payments may be preferred in terms of root-mean-squared error. Nevertheless, our estimates indicate that expectation errors do not lead to substantial bias for

[1] For the dynamic GMM estimator, we report panel robust standard errors with the Windmeijer (2005) correction since two-step GMM estimates of standard errors are biased downward in finite samples.

[2] Simulation studies such as Judson and Owen (1999) have found that the impact of dynamic panel bias on coefficients other than γ is likely to be small. Therefore, we expect any bias in β_g to be primarily from expectation errors rather than dynamic panel bias.

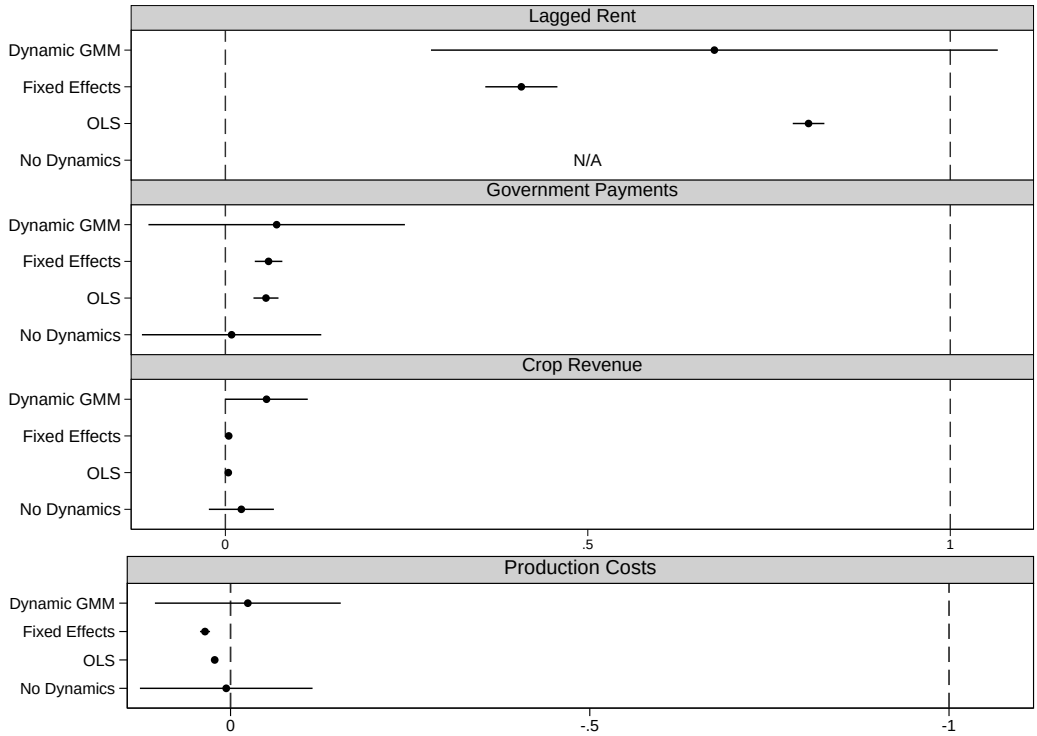

Figure 1. Coefficient Estimates and 95% Confidence Intervals for Alternative Estimators

the coefficient on government payments, because the point estimate from dynamic GMM is similar in magnitude to fixed-effects and OLS estimates.

Last, we compare dynamic GMM estimates and a GMM estimate that does not include a lagged dependent variable to assess the effect of omitted variable bias for the coefficient on government payments. As noted above, we have no *a priori* expectations about the sign of this omitted-variable bias. The specification without dynamics is exactly the same as the dynamic GMM specification except that lagged rent is not included as an independent variable and its lagged levels are not excluded instruments. In figure 1, we find that excluding dynamics from the model creates downward bias in our coefficient estimate for government payments. The point estimate for the coefficient on government payments in the dynamic GMM specification is 0.07 compared to 0.01 in the model with no dynamics.

Table 2 reports estimates for the coefficients on percentage irrigated and percentage in pasture. For dynamic GMM and fixed effects, the coefficient on percentage irrigated is only identified when a farm changes the percentage of acres irrigated. We found that this occurred for a small portion of our sample; usually the change was small. There is little variation in our sample to identify the coefficient, so it is unreasonably small and not statistically different from zero. For OLS, the coefficient on percentage irrigated is negative because irrigation is more prominent in western Kansas where rents also tend to be lower due to a drier climate. Pasture rent is much lower than nonirrigated rent and there was more variation in this variable, so the dynamic GMM and fixed-effects estimates are much larger in absolute terms.

Table 3. Unadjusted and Adjusted Parameter Estimates

		Unadjusted		
Variable	**Description**	**Dynamic GMM**	**Fixed Effects**	**OLS**
γ	Inertia	0.68	0.41	0.81
β_g	Initial Incidence	0.07	0.06	0.06
β_g^{5yr}	Intermediate (5-year) Incidence	0.19	0.10	0.19
β_g^{∞}	Long-run Incidence	0.22	0.10	0.29
β_{rev}	Crop Revenue	0.06	0.01	0.00
β_{costs}	Production Costs	−0.02	0.04	0.02
		Adjusted		
		Dynamic GMM	**Fixed Effects**	**OLS**
γ	Inertia	0.68	0.41	0.81
β_g	Initial Incidence	0.12	0.10	0.09
β_g^{5yr}	Intermediate (5-year) Incidence	0.32	0.17	0.32
β_g^{∞}	Long-run Incidence	0.37	0.17	0.48
β_{rev}	Crop Revenue	0.11	0.01	0.01
β_{costs}	Production Costs	−0.05	0.07	0.04

Using External Data to Adjust Parameter Estimates

To interpret the coefficients of primary interest, we employ the parameter-adjustment formulas in equations (10)-(12) to correct for measurement-error effects in our dependent variable, cash rent per acre. Estimates of ρ and σ are obtained from surveys of Kansas farmers (Schlegel and Tsoodle, 2008a,b). Recall that our sample for estimation only includes farms that cash rented some land. Therefore, the parameters used for adjustment are determined conditional on farms that have at least one cash-rent tenancy arrangement. The most common crop-share lease splits revenues with two-thirds going to the operator and one-third to the landowner, $\rho = 2/3$.[3] Responses from the survey data show that 53% of leases were fixed cash leases of those farms that had at least one fixed cash lease, $\sigma = 0.53$. We use our main dataset to find that, among farms that cash rent some land, 70.7% of crop land is rented, $\phi = 0.707$. Finally, we assume that the proportion of acres cash rented remains constant over time, so that our initial estimate of γ remains valid.[4]

The results of our parameter adjustments are observed in table 3. For the dynamic GMM estimator, the adjustment increases our estimate of the short-run subsidy incidence from \$0.07 to \$0.12 per acre. After scaling the standard error by the adjustment factor, we conduct hypothesis tests on the short-run incidence. The hypothesis of full incidence (i.e., $\beta_g = 1$) is rejected at the 95% confidence level. We cannot reject the null hypothesis that the short-run subsidy incidence on rental rates is zero, but this is not equivalent to accepting the hypothesis that landowners capture none of the benefits of subsidy payments; more precise coefficient estimates are needed. We considered a system GMM estimator and using additional lags as instruments to improve efficiency, but both strategies resulted in small efficiency gains.[5] However, we can compare the dynamic GMM estimates

[3] Survey data indicate that the crop-share arrangement varies across regions of Kansas depending primarily on the land quality in different regions. In most regions of the state, more than 80% of the crop-share leases are a 1/3-2/3 split. In any case, our adjustment is not sensitive to this parameter. If we assume a 1/2-1/2 split, our estimate of the incidence is 0.112 in the short run and 0.344 in the long run compared to 0.119 and 0.367 reported in table 3.

[4] The adjusted γ coefficient depends on the change over time in σ, the share of rented acres that are cash rented. We know that this share has been increasing over time, but we do not have annual data necessary to calculate the adjustment. Available survey data suggests that the year-over-year change in σ would be small and that the implied adjustment would have a negligible effect on our results.

[5] Furthermore, the Hansen test rejects the validity of the additional differenced instruments used in system GMM. When we used all of the available lags as instruments, the coefficient on the lagged dependent variable was biased toward the fixed-effects estimate.

to fixed-effects and OLS estimates. Fixed effect and OLS estimates are significantly different from zero and, although they should be biased down due to expectation errors, they are not substantially different from GMM estimates.

Fixed-effects and OLS estimates indicate that the long-run incidence on rental rates is \$0.17 and \$0.48 per acre, respectively. These two estimates should provide reasonable bounds on the long-run incidence because the estimates of the short-run incidence are similar and the estimates of the coefficient on the lagged dependent variable should bound the true coefficient value. Dynamic GMM estimates indicate that, allowing for a five-year adjustment period, landowners capture \$0.32 of an additional per acre subsidy dollar, and landowners capture \$0.37 of an additional per acre subsidy dollar in the long run. Although the dynamic GMM estimates are imprecise, they are within the fixed-effects and OLS bounds and give some further evidence of a reasonable estimate of the incidence.

Without correcting for the measurement error in the dependent variable, our dynamic GMM and OLS estimates of the intermediate- and long-run subsidy incidence are similar to Kirwan's (2009) estimate. Correcting for the measurement error increases our estimates of the incidence by about 65-70%. Our point estimate from dynamic GMM of subsidy incidence on rents in the long run, \$0.37 per acre, is close to the range predicted by Alston (2010), \$0.40 to \$0.60.

As mentioned previously, we expect the incidence of government payments to be larger than market revenue because subsidies are not fully coupled to current production. Our dynamic GMM estimates are consistent with this theory; interestingly, the effect of government payments is only slightly larger than the effect of market revenue on rental rates. While an additional per acre subsidy dollar increases rent \$0.12, an additional dollar of per acre market revenue increases rent \$0.11 in the short run. Estimates of the effect of market revenue and production costs are substantially different with fixed effects and OLS compared to dynamic GMM, indicating that expectation errors cause a larger bias for these variables than for government payments.

Assessing Instrument Choice

As discussed in the model section, dynamic panel GMM estimators can be sensitive to the number of lags used as instrumental variables. The literature has found that there is a bias-efficiency trade-off in choosing the number of moments. Adding moment restrictions increases efficiency but moves coefficient estimates towards the fixed-effects results. We reduce the number of moments imposed by collapsing the instrument matrix, but we are still left to choose the number of lags used as instruments.

Figure 2 reports our estimates for alternative maximum lag lengths. The figure clearly illustrates the bias-efficiency trade-off. When we only use one lag as an instrument, the confidence intervals are so large that no meaningful conclusions can be made: these estimates cannot distinguish between full or zero incidence. However, when we include all available lags as instruments, the point estimates are nearly identical to the fixed-effects estimates with no instruments. Our preferred dynamic GMM specification sets the maximum lag length at four; using fewer lags resulted in unstable coefficient point estimates and using more lags yielded minimal efficiency gains. The Hansen J-test of the joint validity of the instrument set used in the preferred specification fails to reject the hypothesis of jointly valid instruments with a p-value of 0.279.

Discussion and Conclusion

This article examines the relationship between government subsidies to farms and the cash rental rate for farmland. The relationship is considered in the context of government commodity programs as they existed in the United States from 1990 to 2008 and in the context of rental markets for cropland. Our results confirm that the full incidence of government farm subsidies does not fall on cash-rental rates.

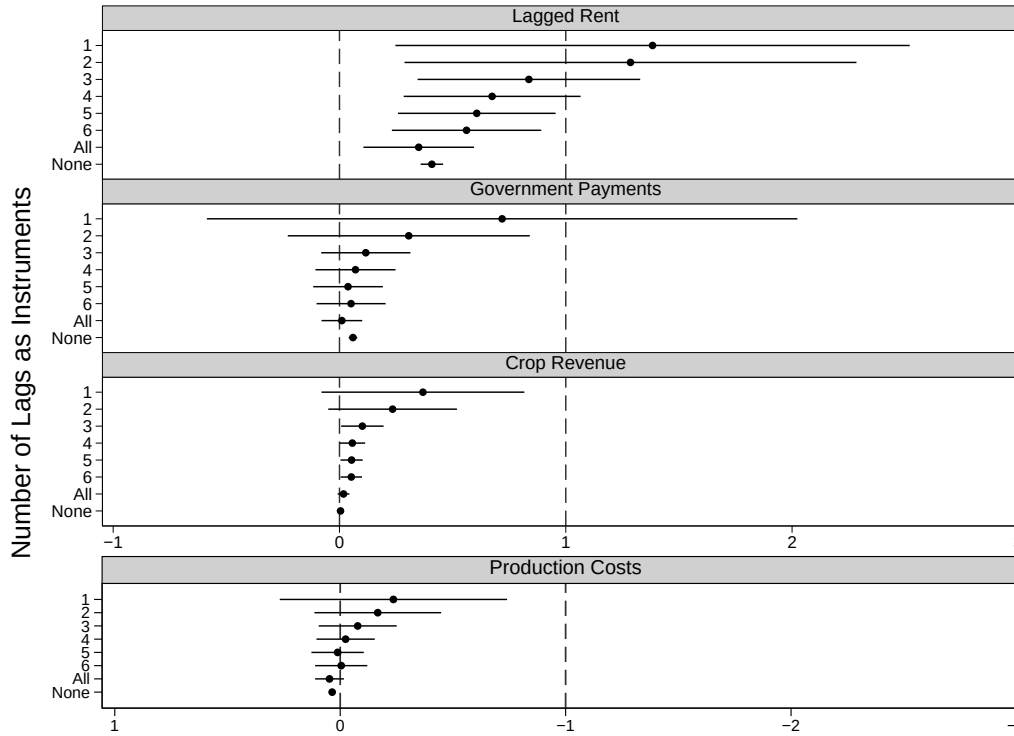

Figure 2. Results with Alternative Maximum Lag Lengths for Instruments

We use Kansas farm-level panel data from 1990 to 2008 to measure subsidy incidence in a dynamic framework using dynamic GMM, fixed-effects, and OLS estimators. For the dynamic GMM estimator, lagged rental rates provide instruments to correct for dynamic-panel bias, while lagged covariates in the dataset provide instruments to correct for expectation errors in government payments, crop revenues, and production costs. Our dynamic GMM results have much larger standard errors compared to fixed-effects and OLS results. The short-run incidence is similar with all three estimators, even though fixed effects and OLS are biased downward due to expectation errors. These results indicate that fixed effects and OLS may be preferred for an estimate of the short-run incidence in terms of root-mean-squared error. Our estimates of the long-run incidence differ more substantially between fixed effects and OLS but provide reasonable bounds on the long-run incidence because the coefficient on the lagged dependent variable is biased downward for fixed effects and upward for OLS.

Previous literature has shown that using a dynamic GMM panel data estimator presents a bias-efficiency trade-off in using additional instruments drawn from our existing dataset. We reduce the number of moment restrictions by collapsing the instrument matrix and using a subset of the lags available as valid instruments. We demonstrate that using all the available lags as instruments introduces substantial bias in to the estimates, while using only one or two lags as instruments fails to provide meaningful inference. Our results illustrate the need to exercise discretion when specifying these estimators.

Our model allows us to assess the extent of inertia in rental-rate adjustments. This inertia may occur due to contractual rigidities and social norms that prevent adjustment of rental rates in response to changes in agricultural returns. Estimating subsidy incidence in light of this inertia problem requires that we explicitly model rental-market dynamics. To evaluate the proposition that

dependence in rental rates across time is significant, we test whether the coefficient on lagged rental rates is significant and find that it is.

The observed dependent variable, cash rent per rented acre, is an incorrect measure of our desired dependent variable, cash rent per cash-rented acre. We derive a formula to scale our parameters appropriately due to this measurement error, and the correction increases our incidence estimate by 65-70%.

Our dynamic GMM estimates suggest that while the incidence of a dollar per acre in subsidy payments on rental rates is $0.12 per acre in the short run, after five years the marginal impact of the same subsidy dollar increases to $0.32 per acre, and in the long run the incidence increases to $0.37 per acre. The GMM estimate of the incidence is imprecise, but we formulate reasonable bounds on the long-run incidence of $0.17 to $0.48 with fixed-effects and OLS estimates. Accounting for inertia and measurement error in rental rates increases our incidence estimate substantially. After addressing these sources of bias, our estimate of the subsidy incidence in the long run is close to the prediction of Alston and James (2002) and Alston (2010) that $0.40-$0.60 of a per acre subsidy dollar is captured by landowners.

Our parameter estimates have implications for the ongoing debates on federal budget-deficit reduction and agricultural subsidy reform. On average the government paid approximately $9 billion per year in direct, countercyclical, and loan-deficiency payments between 1996 and 2008, according to ERS data.[6] While $9 billion is only a small fraction of the total federal budget, reductions in agricultural program spending are likely to be part of any comprehensive deficit-reduction measures.

The incidence of government subsidies will determine the effect of spending reductions on different parties who own or use farmland. If the incidence of subsidies on rental rates is large, then reducing subsidies will decrease benefits for landowners and lower the value of farmland. If the incidence of subsidies on rental rates is small, then farm operators will suffer losses in annual returns. As noted by Kirwan (2007), subsidies comprise a larger portion of income for small farmers than large farmers. Thus, if the incidence of subsidies on rental rates is small, then small farmers will be disproportionately affected by reductions in agricultural subsidies.

Nonfarming landowners own roughly 42% of farmland. Our estimates indicate that these nonfarming landowners capture 37% of the subsidies on their land in the long run, or roughly 15.5% of all agricultural subsidies. With annual subsidy expenditures of $9 billion dollars, our estimates suggest that nonfarming landowners capture $1.4 billion each year from agricultural subsidies through higher rental rates. In a time of budgetary austerity, such large transfers to nonfarming landowners are likely to come under greater scrutiny.

The remaining $7.6 billion is captured by farm operators who own land, farm tenants, consumers, and input suppliers. We do not estimate the incidence of subsidies on other input prices and consumer prices, but this is likely to be less than 20% of total subsidies (Alston, 2010; Kirwan, 2009). Therefore, a large portion of agricultural subsidies will benefit farm operators.

Both small farmers and nonfarming landowners have substantial stakes in policy reform. Small farmers may defend subsidy benefits on equity-based arguments: agricultural subsidies are necessary to prevent the exit of small farmers from production agriculture. Landowners are also likely to defend farm programs since a considerable portion of the benefits are reflected in rental rates, and thus also in land values. In spite of considerable momentum toward reforming subsidy programs, our estimate of the distribution of subsidy payments indicates that both small farmers and landowners have strong incentives to defend the status quo.

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[6] We did not include *ad hoc* payments made during this period. Production flexibility contract payments were included as direct payments in the calculation.

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