Who *Really* Benefits from Agricultural Subsidies? Evidence from Field-level Data

Barrett E. Kirwan and Michael J. Roberts

If agricultural subsidies are largely capitalized into farmland values through their effect on rental rates, then expanding support for agriculture may not benefit farmers who rent the land they farm. Existing evidence on the incidence of subsidies on cash rental rates is mixed. Identification is obscured by unobserved or imprecisely measured factors that tend to be correlated with subsidies, especially land quality and time-varying factors like commodity prices and adverse weather events. A problem that has received less attention is the fact that subsidies and land quality on rented land may differ from owned land. Since most farms possess both rented and owned acreage, farm-level measures of subsidies, land values, and rental rates may bias estimated incidence. Using a new, field-level data set that, for the first time, precisely links subsidies to land parcels, we show that this bias is considerable: where farm-level estimates suggest an incidence of 42–49 cents of the marginal subsidy dollar, field-level estimates from the same farms indicate that landlords capture just 20–28 cents. The size of the farm and the duration of the rental arrangement have substantial effects. Incidence falls by 5–15 cents when doubling total operated acres, and the incidence falls by 0.1–0.8 cents with each additional year of the rental arrangement. Low incidence of subsidies on rents combined with the farm-size and duration effects suggest that farmers renting land have monopsony power.

Key words: Subsidy incidence, farmland, agricultural policy, aggregation error.

JEL codes: H22, Q14, Q15, Q18.

> "*The more the inhabitant was obliged to pay for the tax, the less he would be inclined to pay for the ground; so that the final payment of the tax would fall altogether on the owner of the ground.*"
>
> –*Adam Smith (1776)*

Agricultural subsidies are one of the largest per-capita transfer programs in the United States—subsidized farm operators have received, on average, \$8,824 annually in the twenty-first century.[1] But farmers who rent the land they farm will not fully benefit from modern-subsidy payments, which are decoupled from production, if, as is widely believed by economists and non-economists, decoupled subsidies ultimately are bid into rental rates, thereby benefiting farmland owners.[2] Farmers in the United States own only about 55% of subsidized farmland—

Barrett E. Kirwan is an assistant professor in the Department of Agricultural and Consumer Economics, University of Illinois at Urbana-Champaign. Michael J. Roberts is an associate professor in the Department of Economics, University of Hawaii Economic Research Organization (UHERO), and Sea Grant at the University of Hawaii Manoa. The authors sincerely appreciate the comments of the editor, James Vercammen, and two anonymous reviewers. We also appreciate the comments of seminar participants at Montana State University. We thank Robert Dubman for his help with data access. Correspondence may be sent to: bkirwan@illinois.edu.

[1] Authors' calculations from USDA Farm Services Agency administrative data, statistics published by the Social Security Administration, and the House Ways and Means Committee's Green Book (U.S. Congress, U.S. House of Representatives, Committee on Ways and Means 2014).

[2] Hal Varian, in his microeconomics textbook, teaches, "The market price of the rents depends on the generosity of the Federal subsidies. The higher the subsidies, the higher the equilibrium rent the large farmers receive. The benefits from the subsidy program still falls on those who initially own the land, since it is ultimately the value of what the land can earn—either from growing crops or farming the government—that determines its market value," (Varian 2010). The *Washington Post* echoed that sentiment when it reported, on p. 429, "The farm payments have also altered the landscape and culture of the Farm Belt, pushing up land prices and favoring large, wealthy operators," (Morgan, Gaul, and Cohen 2006). And Robert Reich echoed popular sentiment when he wrote in the *Wall Street Journal* that, "The lavish farm subsidies contained in the new farm bill won't make the nation more secure. They will only stimulate even more production, inflate land values, and make it more difficult for developing nations to export food to us, perpetuating world poverty," (Reich 2001).

Amer. J. Agr. Econ. 98(4): 1095–1113; doi: 10.1093/ajae/aaw022
Published online June 06, 2016

non-farmer landlords own the remaining 45%. If the subsidy is fully passed on to the land owners through higher rental rates, almost 43% of all farm subsidies end up in the pockets of non-farmers.[3]

This paper focuses on the effect that U.S. farmland-specific agricultural subsidies have on farmland cash rental rates.[4] Inasmuch as the cash rental rate reflects the returns to farmland that become capitalized into the land value, we contribute to a large body of literature on the relationship between farmland values and farm subsidies (e.g., Melichar 1979; Robison, Lins, and VenKataraman 1985; Clark, Klein, and Thompson 1993; Weersink et al. 1999; Schmitz and Just 2003; Sherrick and Barry 2003; Latruffe and Le Mouël 2009). The main novelty of the present study is that it is the first to obtain and analyze data that precisely connects subsidy payments to the land being subsidized.

Roberts, Kirwan, and Hopkins (2003) lay out the basic theory of subsidy incidence on farmland rental rates. The theory distinguishes between coupled and decoupled subsidies. The conditions for coupled payments to be fully reflected in the rental rate are extreme: they do so "if the supply curve were perfectly inelastic and prices for inputs besides land did not change" (Roberts, Kirwan, and Hopkins 2003). These conditions were first formalized by Floyd (1965) and further developed by Gardner (1987) and Alston and James (2002). The theory for decoupled subsidies is more straightforward. According to Alston (2010), "a pure decoupled transfer should have little (if any) effect on input use or output and, if that transfer is tied to land, it should be reflected in land rents and should accrue entirely to landowners."

Here we focus on direct payments in the United States, which ostensibly are decoupled from production and theoretically should be fully reflected in higher farmland rental rates. Many studies have shown, however, that a small share of the marginal decoupled-subsidy dollar is reflected in farmland rental rates. In the United States, Roberts, Kirwan, and Hopkins (2003), Kirwan (2009), and Hendricks, Janzen, and Dhuyvetter (2012) find that an additional direct-payment dollar per acre causes rental rates to increase by 12–37 cents. Similar results have been found in the European Union (EU). Breustedt and Habermann (2011), Ciaian and Kancs (2012), Kilian et al. (2012), and Michalek, Ciaian, and Kancs (2014) find rental rate incidence in the range of 6–38% in various EU countries.

Unless these estimates contain some heretofore undiscovered bias, the divergence of the empirical results from the theoretical predictions suggests either that so-called decoupled payments are not actually decoupled from producer behavior, or the theory's simplifying assumptions fail to hold in reality. If decoupled subsidies affect production, theory suggests that the subsidy will be divided among the various factors of production (e.g., see Alston and James 2002), which would be consistent with the low rental-rate incidence findings. Bhaskar and Beghin (2009) and Weber and Key (2012) review the literature on the production effects of decoupled subsidies. Five pathways between decoupled subsidies and production effects have been examined: farm investment due to bankruptcy risk (cf. Vercammen 2007), risk mechanisms (cf. Hennessy 1998; Femenia, Gohin, and Carpentier 2010; Serra, Goodwin, and Featherstone 2011), credit constraints (cf. Goodwin and Mishra 2006; Roe, Somwaru, and Diao 2002; Gohin 2006), labor participation (cf. El-Osta, Mishra, and Ahearn 2004; Key and Roberts 2009), and policy expectations (cf. Lagerkvist 2005; Coble, Miller, and Hudson 2008). The evidence reveals that although decoupled payments appear to affect production, the effects are small. Using credibly exogenous variation in decoupled payments due to the 2002 farm bill, Weber and Key (2012) examine the cumulative effects through all five channels and fail to reject the null hypothesis that decoupled payments do not affect production. In other words, the empirical evidence does not support the idea that decoupled subsidies have substantial production effects. Thus, the low-incidence findings cannot be attributed to the production effects of

[3] Farmers rarely rent land to other farmers. According to the 1999 Agricultural Economics and Land Ownership Survey (the most recent source of information on landlord characteristics), 94% of landlords are retired or employed in non-agricultural industries. The 1996 Agricultural Resource Management Survey (ARMS) provides the only information on the percentage of *subsidized* acres that are rented: 45%. If the share of subsidized, rented land owned by non-farmers is the same as all rented farmland (94%) then 42.5% of subsidized land is owned by a non-farmer.

[4] Farmland-specific agricultural subsidies are paid to the farm operator, who is the tenant in a cash lease, on a fixed number of qualifying acres—in the United States, these are called *base* acres. Some examples of farmland-specific subsidies are Direct Payments and Counter-Cyclical Payments under the 2002 and 2008 farm bills and Price Loss Coverage under the 2014 farm bill.

decoupled subsidies. Instead, we report evidence that the competitive-markets assumption in the theory of factor-specific incidence might not hold.

In this paper, we estimate the incidence of agricultural subsidies on land rents using a new nationally representative data set of field-level rental rates and subsidies, which contrasts with well-identified estimates of agricultural subsidy incidence on rental rates that typically rely on farm-level data. When it comes to agricultural-subsidy incidence on rental rates, the primary unit of analysis is the field-level tenant-landlord contractual agreement, and this study is the first to estimate the incidence on rental rates at this unit of analysis. Previous state-of-the-art research has employed farm-level data. Michalek, Ciaian, and Kancs (2014) employ four consecutive years (2004–2007) of farm-level data from the European Commission's Farm Accountancy Data Network (FADN), and find a 6–10% incidence on rental rates of the European Union's Single Payment System. Ciaian and Kancs (2012) use a two-year panel (2004–2005) to examine the incidence on rental rates of EU Area Payments in new member countries and find 10% incidence on rental rates.

In contrast, no nationally representative data in the United States follows the same farm or field over time. Hendricks, Janzen, and Dhuyvetter (2012) employ annual panel data on Kansas farmers from 1990–2008 and find a 38% incidence on rental rates. Kirwan (2009) creates a nationally representative panel of U.S. farms from the micro-files of the Census of Agriculture and finds a 21% incidence on rental rates. The Census of Agriculture, however, only occurs every five years, so the sample is necessarily limited to farms that neither entered nor exited during the intervening years, which excludes about 25% of the farms in each census.

The only annual, farm and field-level nationally representative data available in the U.S. come from the Agricultural Resource Management Survey (ARMS). The ARMS sampling procedure, however, is explicitly designed to prevent farms from being surveyed in multiple years (Perry, Burt, and Iwig 1994). Even with this limitation, ARMS is a widely used dataset. To capitalize on the strength of ARMS, we worked directly with the USDA to add questions to the 2006 and 2007 ARMS to elicit the expected subsidy, the rental rate, and the underlying productivity of specific plots of land growing soybeans, rice, or cotton across the United States. This paper works to use these nationally representative, explicitly cross-sectional, *field-level* data in a way that reduces bias and accurately estimates subsidy incidence on rental rates.

The primary concern with using cross-sectional data to estimate subsidy incidence on rental rates is the difficulty of accounting for land productivity, which is commonly unobserved. Unobserved land productivity, however, confounds the estimated relationship between subsidies and the rental rate because, as we explain below, both directly depend on the land's productivity.

The problems of not accounting for land productivity can be seen in the approaches taken by Goodwin, Mishra, and Ortalo-Magné (2011) and Patton et al. (2008). Goodwin, Mishra, and Ortalo-Magné (2011) employ multiple cross sections from ARMS Phase III to estimate the subsidy incidence on rental rates, but they do not account for land productivity. Patton et al. (2008) employ farm-level panel data from 1994–2002 in Northern Ireland, but decoupled farmland payments—Less-Favored Land Payments—are not introduced until 2001. By treating these payments as zero until 2001, and by taking first-differences to account for unobserved heterogeneity, these authors include the *level* of the 2001 subsidy in their analysis and effectively undo the benefits of having panel data. Among the burgeoning literature on the subsidy incidence on rental rates, the findings from these studies stand apart. Patton et al. (2008) find an incidence on rental rates of 1.2, and Goodwin, Mishra, and Ortalo-Magné (2011) find 0.73. Both estimates are substantially larger than the findings cited above. But since we know subsidy rates derive from land productivity, these estimates have an unknown degree of upward bias.

We deal with this problem using field-level data. The ARMS collects practices and underlying productivity, which allows us to directly control for this confounding characteristic. Once we control for field productivity, we estimate a subsidy incidence on rental rates of 0.249 for soybeans, 0.200 for rice, and 0.275 for cotton. In other words, landlords extract 24.9 cents of the marginal subsidy dollar per acre from soybean fields, 20 cents of the marginal subsidy dollar per acre on rice fields, and 27.5 cents of the marginal subsidy dollar per acre on cotton fields. These results are in line with those found in the United States and the EU, even though they are obtained with different data and a different identification strategy than those currently in the literature (Kirwan 2009; Hendricks, Janzen, and Dhuyvetter 2012; Ciaian and Kancs 2012; Michalek, Ciaian, and Kancs 2014).

We examine the robustness of these findings by exploring the variation in the incidence estimates over the farm-size distribution and the length of the landlord-tenant relationship. We find that the incidence falls by an *additional* 5–15 cents of the marginal subsidy dollar per acre when doubling total operated acres. This is a substantial effect. For example, a soybean farm at the 25th percentile of the farm-size distribution faces an already-low incidence of 0.377; when its size increases to the median size the incidence falls by 45%, to 0.203. The subsidy incidence also falls by 0.1–0.8 cents for each additional year of the landlord-tenant relationship. This effect is economically important. For instance, the incidence for a soybean farm at the median tenancy length (10 years) faces a 22% lower incidence than a farm at the first year of tenancy. These findings are consistent with tenants who have bargaining power through size and informational advantages.

We develop these arguments further below, but first we explain the institutional setting and provide the relevant details needed to appreciate the identification strategy. Before we elaborate on the sources of identification, we provide more detail about the unique data set we employ. We then explain our identification strategy and illustrate it by contrasting our estimates with the estimates one would obtain in the absence of our identification strategy. Finally, we use the data to shed light on the reasons for low incidence rates.

The Setting

In this section we provide an overview of the farmland rental market, and we explain the institutional details that inform our identification strategy.

The Farmland Rental Market

The farmland rental market is large and important for subsidized crop producers. In the United States, farmers rent 355 million acres of farmland, an area equal to 38% of all farmland and comparable to all the farmland in the Midwest. Table 1 reports statistics on the farmland rental market from the 2002, 2007, and 2012 Censuses of Agriculture. While commodity prices rose between 2007 and 2012, the total amount paid in cash rent increased from $13.27 billion to $21 billion, a 58% increase, and the number of acres rented increased by 15.3%. Farmland cash-rent expense accounted for 10.2% of total production expenditures by renters. At the same time the number of tenants fell 0.8%, while their farm sizes increased 3.6%.

Examining the incidence of subsidies on rental rates is extremely relevant because a large share of *subsidized* land is cash rented. Data from the 1996 Agricultural Resource Management Survey (ARMS) indicate that 45% of *subsidized* acres are rented.

U.S. Farmland-specific Agricultural Subsidies

Although farmers have received support through many mechanisms, we focus our analysis on direct payments (DP) and countercyclical payments (CCP) because these were farmland-specific subsidies paid directly to the tenant under a cash-lease agreement.[5] Both DP and CCP policies have their roots in the 1973 farm bill, which was "designed to encourage increased production of basic food and fiber crops," (Youde 1974). This began the reversal of 40 years of supply-control policies. The 1973 farm bill introduced an "income support" program designed to reward farmers by providing a guaranteed income based on their "typical production of the targeted crops." The 1973 farm bill defined the income guarantee as the product of the farmer's acreage allotment for the subsidized crop and the *projected yield* established for the farm. A similar formula exists today. To effectively estimate the incidence of the subsidy, we must understand the acreage allotment and projected yield parameters.

Acreage allotment. Throughout the 1950s and 1960s the Secretary of Agriculture used a tool called the *acreage allotment* to limit the supply of a crop. Each year the USDA

[5] A landlord who cash leases farmland is legally ineligible for the subsidy payments. "If the lease is a cash lease, the landlord is not eligible for direct or counter cyclical payments," (Sharing of contract payments, 7 C.F.R. §1412.504, 2006). Although landlords are eligible to receive a share of the subsidy if the land is leased on a share basis, since we focus on cash lease arrangements we avoid this complication.

Table 1. Farmland Rental Market Descriptive Statistics

		2002	2007	2012
Number of Renters	Total	719,143	686,437	680,952
Number of Farms (1,000)	Total	2,174	2,197	2,109
Farm size—All Producers (acres)	Mean	435	419	434
Farm size—Renters (acres)	Mean	816	844	849
Acres Rented by Renter	Mean	501	517	529
Proportion of Farm Rented	Mean	63.3%	63.1%	60.7%
Cash Rent Expenditures	Total ($billion)	11.20	13.27	21.00
	Share of Total	10.0%	6.4%	5.5%
Number of Rented Acres	Total (million)	323	308	355
Proportion of U.S. Farmland Rented		38.4%	38.8%	38.8%

Note: 2002 and 2007: authors' calculations from Censuses of Agriculture. 2012: Census of Agriculture, Table 70. Summary by Tenure of Principal Operator and by Operators on Farm: 2012.

determined a farmer's crop-specific allotment—the number of acres he could plant to a specific crop and still qualify for subsidies. If land was sold (or rented), the seller's (landlord's) allotment was reduced commensurately while the buyer's (renter's) was increased. In that way, the allotment was a characteristic of the land. Since the income support was limited to pre-defined acres and because the right to receive the subsidy payment transferred with the land, the income support was a de facto land-specific subsidy. Today acreage allotments are known as "base acres."

Projected/program yield. The policy rewarded farmers if they were more productive by connecting the subsidy to a farmer's "projected yield." The projected yield—renamed *program payment yield* in the 1977 farm bill—was usually determined by a farmer's average productivity in previous years. Congress began to divorce income-support subsidies from production in the 1985 farm bill, which defined the program yield as an Olympic average of the five previous years' (1981–1985) program yields. After 1985, the program yield was frozen at the value calculated in 1985. Today, subsidies continue to be calculated as a function of the 1985 program yield. Because more productive land commands a higher rental rate *and* receives higher subsidies, the subsidy's effect on the rental rate will be confounded with the land's underlying productivity. To accurately estimate the incidence, it is crucial to disentangle the subsidy effect from the productivity effect.

Decoupled subsidies. Congress' drive to end supply controls, which began in 1973, culminated in the 1996 "Freedom to Farm" bill. As specified in the bill, farmers were no longer limited by their base acres; they could plant as much or as little of the subsidized crops, without penalty. After 1996, a farmer received direct payments equal to a legislated subsidy rate multiplied by the farm's 1996 crop-specific base acres and the 1985 crop-specific program yield.[6] This had the effect that farms could completely change their crop mix but continue to be subsidized for the crops grown in 1996.

The upshot is that in 2006 and 2007, the years of our analysis, direct payments were pre-determined, farmland-specific subsidies, which could directly impact farmers' willingness to pay to rent subsidized land.

Data

This paper uses novel, field-level data from the USDA's Agricultural and Resource Management Survey (ARMS). According to the USDA, "ARMS is a nationally representative survey administered using [three] phases—sample screener, field-level, and farm-level phases—targeting about 5,000 fields and 30,000 farms each year," (Morehart 2014b). We employ data from the second phase of the 2006 and 2007 ARMS. This phase is dubbed Phase II—Production Practices and Costs (PPC). The ARMS Phase II questionnaires are "only directed to

[6] Direct and counter-cyclical payments were extended to soybeans and other oilseeds in 2002 with the acreage allotment and program yield based on the land's 1998–2001 yields scaled by the ratio of the national-average 1981–1985 yield and the national-average 1998–2001 yield (Direct Payment Yield for Soybeans and Other Oilseeds 2002).

operations producing the survey year's target crop(s)," (Morehart 2014a). A limited number of crops are targeted each year. In 2006 the target crops were soybeans and rice, and in 2007, apples and cotton were targeted. The analysis consequently employs three Phase II data sets, the set of soybean fields in 2006, the set of rice fields in 2006, and the set of cotton fields in 2007 (apples are unsubsidized).[7] These crops represent three of the eight primary, subsidized crops.[8] To limit the influence of outliers we winsorize the variables at the 99th percentile and we drop fields with subsidies per acre more than four standard deviations above the mean. The soybean-field sample has 424 cash-rented fields in 19 states. The rice-field sample has 81 cash-rented fields in five states—primarily Texas and the Mississippi Portal.[9] The cotton-field sample has 215 cash-rented fields in 11 states. To account for the diverse environments in which these crops are grown we include state fixed effects in the empirical model below. In addition, the state fixed effect accounts for statistical anomalies caused by the sampling scheme, which stratifies at the state level.[10]

To collect production practices and costs for the target crop, the ARMS Phase II enumerator randomly chooses only one of the respondent's fields where the target commodity is growing, and the survey focuses on the costs and production practices on *that field only*. This procedure ensures that a respondent reports on a single target commodity even if they produce more than one of the ARMS-targeted crops on the whole farm.

The advantage of these data for the current analysis is that in 2006 and 2007 we added questions to the ARMS Phase II survey that elicited information on rent paid and subsidy payments received on the randomly chosen field. These data are unique because they link subsidies to the *specific* cash-rented parcels being subsidized.

Importantly, the ARMS Phase II survey obtains production practices and costs for the same field. This allows us to carefully and accurately control for the field's underlying productivity, which is often mis-measured or omitted altogether (e.g., Goodwin, Mishra, and Ortalo-Magné 2011). To account for the land's underlying productivity we use the producer's *fertilizer-decision yield goal* as reported in the Fertilizer section of the survey. Rather than an aspiration to be achieved, the fertilizer-decision yield goal is a common parameter used to determine fertilizer application rates. According to the USDA, "recommended fertilizer application rates are often based on the *yield goal* of the producer," (USDA 2006), which is why the question is asked in the Fertilizer section of the survey along with early season field-specific production costs and indicators for crop-rotation pattern.[11]

Producers have an incentive to set an accurate yield goal; an overly optimistic goal, for instance, leads to higher costs due to greater fertilizer use, but not higher revenue. We explore the accuracy of the yield goal by regressing realized yield on yield goal. This analysis results in the following intercepts and slopes (respectively): -4.86 & 1.02 (soybeans (bu/acre)), -152.89 & 1.01 (cotton (lbs/acre)), and 35.73 & 0.47 (rice (cwt/acre)). The slope coefficient of 1 for soybeans and cotton show the yield goal is an extremely good predictor of actual yield, although the rice coefficient, 0.47, shows a high but less than perfect correlation between yield goal and realized yield. The negative intercepts suggest that farmers have optimistic yield goals. The intercept term for the soybean-yield relationship is 11.8% of the average realized yield. For cotton it is 18.6% of average realized yield and 50% of average realized yield for rice.

Optimistic yield goals threaten our identification strategy only if the optimism is systematically correlated with our identifying variation, that is, subsidies after accounting for the yield goal. We investigate this by

[7] The survey questionnaires are available at the following url: http://www.ers.usda.gov/data-products/arms-farm-financial-and-crop-production-practices/questionnaires-and-manuals.aspx.

[8] The remaining subsidized crops are barley, corn, oats, sorghum, and wheat. Minor oilseeds also receive direct payments.

[9] The Mississippi Portal is one of nine "resource regions" defined by the USDA, ERS (U.S. Department of Agriculture 2000).

[10] The exact states in each of the data sets can be seen at the following USDA, ERS website: http://www.ers.usda.gov/data-products/arms-farm-financial-and-crop-production-practices/documentation.aspx#Scope.

[11] From the 2006 Agricultural Resource Management Survey (ARMS) Phase II Field Crop Chemical Usage and Production Practices Interviewer's Manual: "Recommended fertilizer application rates are often based on the yield goal of the producer. Estimates of producer yield goal compared with actual yield gives some indication of how realistic producers expectations are. It also gives an indication of how unexpected conditions, such as droughts or pest infestations, may have affected yields. Furthermore, assessing the impact of adopting different nutrient practices requires information on the producers pre-season expected yield or yield goal which can be much different from actual yield."

stratifying the data on the subsidy after "partialling out" the yield goal, and comparing the optimism of those with low identifying variation and those with high variation. We find a statistically insignificant difference in optimism of 2.5% between the two groups, and conclude that the optimism is not systematic and thus does not adversely affect our identification strategy.

Early season seed, fertilizer, and chemical costs reveal another dimension of soil productivity; more productive fields are those with higher marginal product, which leads to higher input use. Information on historical rotations also serves as a powerful control for land quality and production practices that influence rental rates.

In the analysis below we contrast the field-level analysis with results obtained by analyzing farm-level data from the farms associated with each field. To accomplish this, we merge the field-level data with data from the ARMS Phase III—Farm Business and Farm Household Information. Each of the Phase II respondents is asked to complete an expanded Phase III survey, which collects detailed farm and household-level information.

Table 2 contains summary statistics for the soybean, rice, and cotton fields in our dataset, and table 3 reports the corresponding farm-level summary statistics. For both tables, columns 2 and 3 report the means and standard deviations, respectively, for fields/farms in the soybean data set. Columns 4 and 5 report means and standard deviations for rice fields/farms, and columns 6 and 7 report the means and standard deviations for cotton fields/farms.

The relevant rental rate for the analysis is the field-specific cash rental rate. The first row of each table reports the cash rental rate at each level of aggregation. In table 3 the reported farm-average cash rental rate is constructed by dividing total rent expenditures by total rented land. The farm-average cash rental rate is lower than the field cash rental rate for soybeans and rice, while the cotton field cash rental rate is slightly below the farm average. Since only cropland is subsidized, the farm-average rental rate will be too low for farms that also rent less valuable, unsubsidized pasture and rangeland. And it will be too low for farms that have crop-share agreements in addition to cash leases. We discuss the influence this has on the incidence estimates in the Measurement Error section below.

The second row of each table reports direct payments per acre, which are greater at the field level than the farm level for rice, but not for soybeans and cotton. The relatively low field-level direct payments for soybeans likely reflects that soybeans receive lower subsidies than other crops produced by the farm. For rice, the average field-level subsidy is $5.94 greater than the average farm-level subsidy. The soybean field-level subsidy is $6.82 less than the average farm-level subsidy, and for cotton the difference is $8.35.

Table 2 reports statistics on several field characteristics that influence the amount a renter would be willing to pay to rent the land. The cost of seed, fertilizer, and chemicals measure the operating cost of the field. A field's environmental sensitivity has been found to be correlated with its productivity (Claassen, Lubowski, and Roberts 2005), so we capture these effects with variables indicating whether the field is highly erodible and if it is in a conservation program. Expected returns of other crops in the crop rotation also matter. If these unobserved crop yields respond to land attributes in a way that is correlated with direct payments, even after accounting for current crop yield and field operating costs, then the incidence estimate may be too large. Although yields of rotated crops are unavailable, we account for this somewhat with variables indicating the crops being rotated with the currently grown crops.

The financial variables listed in table 3 are whole-farm gross revenues, whole-farm nonland expenses, net returns, assets, and debt. The summary statistics for these variables reveal similar financial situations for rice and soybean farms. Both farms generated similar revenue per acre, $383.63 for soybeans and $397.53 for rice, and have similar net returns per acre, $85.09 and $82.92 for soybeans and rice, respectively. Farms with cotton fields in the data fared substantially better, generating $626.40 per acre in revenue and net returns of $132.14 per acre.

Table 3 reveals that farms in the data set are large. The average soybean farm has 1,589 acres, while the average cotton and rice farms operate 2,400 and 2,582 acres, respectively. Interestingly, although cotton farms are 900 acres larger than soybean farms on average, the amount of owner-operated acres are about the same, 325 acres and 393 acres for soybean and cotton farms, respectively. Rice farmers operate the most acres, but own the least, just 213 acres on average. The table shows that farmers' demographic characteristics are similar across farm types. Finally, the table reports

Table 2. Summary Statistics—Field Level

	Soybeans (N = 424)		Rice (N = 81)		Cotton (N = 215)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Cash rental rate	82.87	40.34	78.21	32.30	75.92	45.93
Direct payment (/acre)	10.64	11.07	52.39	49.27	19.53	20.73
Yield goal[a]	45.27	9.96	75.87	16.16	1,009.42	238.38
Expect a counter-cyclical payment	0.38	0.49	0.43	0.50	0.70	0.46
Total Seed Cost	33.14	10.16	30.05	21.11	68.89	29.46
Total Fertilizer Cost	16.32	19.87	73.41	38.94	86.10	39.58
Total Chemicals Cost	13.70	10.09	55.18	42.59	82.14	42.17
Irrigated	0.08	0.27	–	–	0.28	0.45
Classified as highly erodible	0.16	0.37	0.01	0.11	0.02	0.14
In a conservation program	0.08	0.27	0.09	0.28	0.06	0.24
Corn-soy rotation	0.65	0.48	–	–	–	–
Soy-soy rotation	0.18	0.39	–	–	–	–
Rice-rice rotation	–	–	0.21	0.41	–	–
Rice-soy rotation	–	–	0.41	0.49	–	–
Rice-idle rotation	–	–	0.35	0.48	–	–
Cotton-cotton rotation	–	–	–	–	0.56	0.50
Cotton-peanuts rotation	–	–	–	–	0.08	0.28
Cotton-corn rotation	–	–	–	–	0.10	0.30
Cotton-soy rotation	–	–	–	–	0.08	0.27
Cotton-wheat rotation	–	–	–	–	0.04	0.19

Source: Agricultural Resource Management Survey, Production Practices and Costs (Phase II): Soybeans (2006), Rice (2006), Cotton (2007). Subscript [a] indicates that yield goal units are bushels per acre for soybeans, hundred-weight per acre for rice, and pounds per acre for cotton.

Table 3. Summary Statistics—Farm Level

	Soybeans (N = 424)		Rice (N = 81)		Cotton (N = 215)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Farm-average cash rental rate	77.25	41.63	69.00	55.93	78.13	63.75
Farm-average direct payment (/acre)	17.46	11.53	46.45	35.51	27.88	17.67
Target-crop yield	41.19	13.09	70.14	12.12	821.49	347.04
Total acres operated	1,588.78	1,485.69	2,582.17	2,060.60	2,399.50	2,227.32
Total cropland	1,464.16	1,403.04	2,391.63	1,946.90	2,099.19	1,808.29
Total acres owned	325.25	444.12	213.15	427.05	393.44	544.66
Proportion of Acres Cash Rented	0.70	0.28	0.72	0.34	0.71	0.28
CCP per Acre	9.73	9.03	7.56	9.45	20.80	17.53
Loan Deficiency Payments (total)	2,257.80	7,079.01	2,318.52	8,646.62	3,536.73	19,310.50
Whole-farm gross revenue (/acre)	383.63	221.90	397.58	225.10	626.40	429.48
Crops' Share of Revenue	0.85	0.28	0.94	0.22	0.96	0.14
Whole-farm non-land expenses (/acre)	239.69	156.42	252.40	108.73	433.45	287.68
Net Returns (/acre)	85.09	135.87	82.92	165.42	132.14	226.87
Assets ($1,000)	1,866	2,041	1,285	1,396	2,536	2,598
Debt ($1,000)	309	436	335	586	319	535
Operator's age	51.22	11.33	51.57	12.43	52.75	11.35
Operator's gender	0.99	0.11	0.99	0.11	0.99	0.12
Operator has high school diploma or less	0.37	0.48	0.35	0.48	0.39	0.49
Operator has some college	0.31	0.46	0.30	0.46	0.31	0.46
Operator has a college degree	0.28	0.45	0.33	0.47	0.27	0.45
Operator's primary occupation is farming	0.90	0.31	0.96	0.19	0.94	0.24
Operator is retired	0.03	0.17	0.05	0.22	0.01	0.12

Data Source: Agricultural Resource Management Survey, Cost and Returns Report (Phase III): Soybeans (2006), Rice (2006), Cotton (2007).

statistics on the type of crop-rotation employed on the surveyed fields.

The Challenge of Identification

To identify the effect of the marginal, per-acre subsidy dollar on farmland rental rates we must address two sources of endogeneity: measurement error and omitted-variable bias. Our approach to the problem is extremely transparent; we employ unique field-level data collected to specifically address these challenges to identification.

Measurement Error

We address three sources of measurement error: aggregation error, measurement error in the dependent variable, and expectation error.

Aggregation error. An important advantage of using our data is the ability to contrast field-level estimates with farm-level estimates from the same farms. We can thus investigate potential bias in farm-level incidence estimates.

To clearly understand how aggregation to the farm level introduces bias, consider the aggregation in two steps. First, consider only the subsidized, cash-rented fields on the farm. Represent the per-acre subsidy for the i^{th} subsidized and rented field on the j^{th} farm with s_{ij}, and the rental rate for the same field with r_{ij}. Suppose the true incidence can be estimated by the following linear model:[12]

$$(1) \qquad r_{ij} = \alpha + \beta s_{ij} + \epsilon_{ij}.$$

Measurement error is introduced when the farm averages of these variables are used to represent the field-level data:

$$\bar{s}_j = \frac{1}{n}\sum_i s_{ij} = s_{ij} + u_{ij} \quad \text{and} \quad \bar{r}_j = \frac{1}{n}\sum_i r_{ij} = r_{ij} + v_{ij}$$

where $\bar{s}_j$ and $\bar{r}_j$ are the average per-acre subsidy and rental rate on *rented and subsidized fields* on farm j, and n is the number of rented and subsidized fields on farm j.

Unlike classical measurement error, where the errors are uncorrelated with the truth, the measurement errors u_{ij} and v_{ij} are uncorrelated with the averages, that is, $cov(\bar{s}_j, u_{ij}) = 0$ and $cov(\bar{r}_j, v_{ij}) = 0$. Thus, u_{ij} and v_{ij} are necessarily correlated with the true field-level values, s_{ij} and r_{ij}, and estimates using $\bar{r}_j$ and $\bar{s}_j$ will be unbiased.[13]

Unfortunately, data typically used to address this question do not report rent separately for subsidized and unsubsidized land or subsidies separately for rented and owner-operated land. Instead, farm-level data usually contain total subsidies and total cropland, which allow the investigator to calculate the average subsidy over all cropland, $\bar{\bar{s}}_j$. Likewise, the data usually contain total rental expenditures and acres of land rented, and the investigator can calculate the average rental rate over subsidized and unsubsidized land, $\bar{\bar{r}}_j$. These coarse measures introduce another type of measurement error:

$$\bar{\bar{s}}_j = \bar{s}_j + \eta_{s_j} \qquad \text{and} \qquad \bar{\bar{r}}_j = \bar{r}_j + \eta_{r_j}$$

where the measurement error η_{r_j} is a function of unsubsidized, rented farmland on farm j, and the measurement error η_{s_j} is a function of owner-operated subsidized farmland. These measurement errors are difficult to characterize, and may be correlated with $\bar{\bar{r}}_j, \bar{r}_j, \bar{\bar{s}}_j, \bar{s}_j$ and each other.

An important facet of estimating the incidence is that aggregation errors affect both the rental rate and subsidy variables, and these errors may be correlated. Correlated measurement error on the left and on the right can be a significant source of bias (see, e.g., Hyslop and Imbens 2001). This is likely to be a concern in this setting if, for instance, fields with above-average rental rates also have above average subsidies, $cov(u_{ij}, v_{ij}) > 0$, and if farms that have higher subsidies on

[12] For simplicity, consider the bivariate relationship; the full empirical model will be introduced in the next section.

[13] To see this, substitute for r_{ij} and s_{ij} in equation (1): $\bar{r}_j = \alpha + \beta\bar{s}_j - \beta u_{ij} + v_{ij} + \epsilon_{ij}$, and consider the ordinary least squares (OLS) estimator for $\beta_{\bar{r},\bar{s}}$:

$$\begin{aligned}
\hat{\beta}_{\bar{r},\bar{s}} &= \frac{cov(\bar{s},\bar{r})}{var(\bar{s})} \\
&= \frac{cov(\bar{s}, \beta\bar{s} - \beta u + v + \epsilon)}{var(\bar{s})} \\
&= \frac{cov(\bar{s},\beta\bar{s}) - cov(\bar{s},\beta u) + cov(\bar{s},v) + cov(\bar{s},\epsilon)}{var(\bar{s})} \\
&= \frac{\beta var(\bar{s})}{var(\bar{s})} \\
&= \beta.
\end{aligned}$$

owner-operated fields also rent more low-value unsubsidized farmland, $cov(\eta_s, \eta_r) > 0$.

To see the bias caused by aggregation errors, define the aggregation-induced measurement error as follows:

$$(2) \quad \bar{\bar{s}}_j - s_{ij} = \pi_{s_j}$$

and

$$(3) \quad \bar{\bar{r}}_j - r_{ij} = \pi_{r_j}$$

where $\pi_s = u + \eta_s$ and $\pi_r = v + \eta_r$.[14]

Examine the effects of using whole-farm averages by substituting equation (2) and equation (3) into equation (1):

$$\bar{\bar{r}}_j = \alpha + \beta\bar{\bar{s}}_j - \beta\pi_{s_j} + \pi_{r_j} + \epsilon_{ij}.$$

To assess the size of the bias, consider the OLS-estimator for $\beta_{\bar{\bar{r}},\bar{\bar{s}}}$

$$(4) \quad \hat{\beta}_{\bar{\bar{r}},\bar{\bar{s}}} = \frac{cov(\bar{\bar{s}},\bar{\bar{r}})}{var(\bar{\bar{s}})}$$
$$= \frac{cov(\bar{\bar{s}},\beta\bar{\bar{s}} - \beta\pi_s + \pi_r + \epsilon_{ij})}{var(\bar{\bar{s}})}$$
$$= \frac{cov(\bar{\bar{s}},\beta\bar{\bar{s}}) - cov(\bar{\bar{s}},\beta\pi_s) + cov(\bar{\bar{s}},\pi_r) + cov(\bar{\bar{s}},\epsilon_{ij})}{var(\bar{\bar{s}})}$$
$$= \left(1 - \frac{cov(\bar{\bar{s}},\pi_s)}{var(\bar{\bar{s}})}\right)\beta + \frac{cov(\bar{\bar{s}},\pi_r)}{var(\bar{\bar{s}})} + \frac{cov(\bar{\bar{s}},\epsilon_{ij})}{var(\bar{\bar{s}})}.$$

We can use equation (4) to estimate the bias empirically. The proportional bias can be calculated from the coefficient from regressing π_{s_j} on $\bar{\bar{s}}_j$. The second term in equation (4) is the coefficient from regressing π_{r_j} on $\bar{\bar{s}}_j$, while the last term represents bias due to omitted variables. We examine the magnitudes of these biases in the Results section below.

Furthermore, the difference between field- and farm-level subsidies per acre may well be systematic. For example, large farms with market power may successfully seek out land to rent that has particularly high subsidies relative to land productivity. Moreover, farms with greater market power may have lower incidence. If so, measurement error would be systematically correlated with heterogeneous effects. This kind of non-random measurement error could cause considerable unknown bias.

The farm-level rental rate. A second benefit of using field-level data is the ability to accurately measure the rental rate. Studies using farm-level data have calculated the farm-average rental rate by dividing the farm's total rental expenditure by its total acres rented. When farms have both cash and crop-share rental agreements, this approach results in a rental rate that is lower than the farm-average cash rental rate on subsidized fields—the measurement error is systematically negative for these farms. If farmers are more (less) likely to use cash contracts with higher subsidies, the measurement error will be positively (negatively) correlated with the subsidy, leading to an over (under) estimate of the subsidy incidence. Qiu, Goodwin, and Gervais (2011) find that as farmland-specific subsidies increase, farms switch from share contracts to cash contracts, which would result in positively-biased incidence estimates. Field-level cash rental rates avoid this type of right-hand-side measurement error-induced bias.

Expectation error. Another form of measurement error identified in the literature is measurement error in the expected subsidy measure, dubbed "expectation error" (see, e.g., Roberts, Kirwan, and Hopkins 2003). Direct payments were known with certainty beforehand and were not directly subject to expectation error. But DP are closely related to CCP because they both are proportional to the farm's program yield and paid only on base acres, and the CCP is subject to expectation error because it depended on how far below a fixed price—the Target Price—the market price fell. The influence this might have on the incidence estimate depends on the relationship between the DP and the expected price. Although we lacked the resources to collect the expected price distribution from every surveyed farmer, we did ask whether the farmer expected to receive a CCP. On average, farmers who expected to receive a CCP received a 22% higher-than-average DP, which suggests a positive relationship between the DP and the expected CCP. In other words, not accounting for the expected CCP would lead to upward bias of the incidence estimate.

Although we do not know the farmer's expected price, we do know whether they expected the price to be less than the Target Price. We use this measure, albeit crude, to control for the effect of expected subsidies on

[14] Because we can calculate π_s and π_r in our data but not u, v, η_s, or η_r, we decompose the bias in terms of π_s and π_r.

rental rates. But since we do not know the size of the expected CCP, the coefficient on this variable will be attenuated due to classical measurement error. This is an important control, however, because if DP and expected CCP payments are positively correlated, the coefficient on DP would pick up some of the effect of expected CCP payments. Including this indicator variable in the analysis below ameliorates some of this upward bias to the incidence estimate.

Omitted-variable Bias

Failure to account for the land's underlying productivity is an important source of bias in incidence estimates. The productivity of the land obscures the effect of subsidies on farmland rental rates because both the subsidies and the rental rate depend on the land's productivity. More productive land commands a higher rental rate; for example, fertile soil that retains moisture will be able to make greater productive use of inputs like fertilizer. Marginal productivity of inputs will tend to decline more slowly, leading to greater input use, but also greater returns to the land, and hence increase farmers' willingness to pay to farm the land. And since 1973, the land's historical productivity has determined farmland-specific subsidies. The farmland's productivity, therefore, is a fundamental reason that land commanding a high rental rate also receives high subsidies. Incidence estimates that fail to account for the underlying farmland productivity will be too large because they suffer from positive omitted-variable bias.

The econometrician rarely observes the underlying productivity of the farmland. Recent attempts to estimate the effect of subsidies on farmland values have tackled the problem with panel data using fixed effects to account for the farmland's presumably fixed underlying productivity (Ciaian and Kancs 2012; Hendricks, Janzen, and Dhuyvetter 2012; Michalek, Ciaian, and Kancs 2014). In this case, the identification of incidence comes from changes over time in subsidy rates, or because farms rent more, less, or different land over time. In the former case, some have argued that it may take time for changes in subsidies to be reflected in land rents (Alston 2010). In the latter case, even fixed effects may not fully account for land productivity, since land associated with the farm changes over time.

In the analysis below, we address omitted-variable bias by capitalizing on one of the strengths of the data we employ: the data contain the farmer's estimate of the field's underlying productivity. The farmer has much more information about the field's characteristics than does the econometrician, and is thus better equipped to accurately estimate the field's underlying productivity. And while farmers may be optimistic, or otherwise make systematic assessment errors, we see little reason why farmers' systematic errors ought to be associated with productivity or subsidy rates.

Empirical Strategy

In empirical work, the causal relationship between subsidies and rental rates tells us what farmland rent would be if we could either change the subsidy in a perfectly controlled environment or change the subsidy randomly so that fields with different levels of subsidy would be otherwise comparable.

The model we use to estimate the incidence of agricultural subsidies on farmland rental rates is

$$(5)\quad Rent_{jk} = \alpha_k + \beta Subsidy_{jk} + \gamma \boldsymbol{X}_{jk} + \epsilon_{jk}$$

where $Rent_{jk}$ is the rental rate for field or farm j, depending on the level of analysis, in state k. The per-acre DP subsidy is $Subsidy_{jk}$, while $\boldsymbol{X}_{jk}$ is a vector of field-level observable covariates. Differences in production practices and state policies—such as taxes, environmental regulations, and land-use regulations—are accounted for by α_k, a fixed effect for state k.

Since rental rates and direct payments are both a function of the field's productivity, simply regressing $Rent_j$ on $Subsidy_j$ would yield an upwardly biased estimate; $\widehat{\beta}$ would capture both the incidence and the positive relationship between $Rent_j$ and $Subsidy_j$ due to field productivity. To overcome this difficulty and identify the subsidy incidence parameter in equation (5), we attempt to exploit an element of randomness in how U.S. farmland subsidies are determined. U.S. farmland subsidies are based on an acre's historical, *realized* productivity (see the Projected/Program Yield section above). Because it is based on *actual* production, the program yield is partly determined by the land's underlying productivity, and partly determined by exogenous, idiosyncratic shocks (e.g., weather and pests) during the reference period. This random variation is a key component of the variation needed to identify the subsidy incidence.

To isolate the random component of the program yield, we use the fertilizer-decision yield goal as reported in the Fertilizer section of ARMS, with field-specific operating costs, and indicate the crop rotation to control for the land's inherent productivity. Armed with this information, we explicitly control for the fundamental characteristic that confounds the incidence analysis, namely each field's inherent productivity. The remaining cross-sectional variation in the subsidy, after we have "partialled out" the underlying productivity, is the random variation outlined above. We use the random component of subsidy variation to determine the causal effect of subsidies on farmland rental rates.

Results

Field-level Rental Rate Incidence

We report primary findings for soybean, rice, and cotton fields in tables 4, 5, and 6, respectively. Each table reports estimates from four different specifications. The first column reports the result of a simple bivariate OLS regression between the cash rental rate and per-acre DP. The second column adds the yield goal as a control variable, and the third column adds the remaining covariates: whether the respondent expected to receive counter-cyclical payments; seed, fertilizer, and chemical costs; dummy variables for reported crop rotations; whether the field is classified as highly erodible; and whether it is in a conservation program. Finally, the fourth column adds state fixed effects to the specification from column 3—this is our preferred specification.

In the first column of the tables, we see that before controlling for any field characteristics, the field-level incidence estimate is relatively high for soybean and cotton fields, but not statistically significant for rice fields. For soybeans, column 1 reports an incidence of 0.765; in other words, rental rates increase by 76.5 cents with the marginal subsidy dollar. The estimate is a statistically significant 0.598 for cotton, and insignificant and negative for rice, -0.09.

Table 4. The Incidence of Field-level Subsidies on Field-level Rental Rates: Soybeans

Dependent Variable	Field Cash Rental Rate			
	(1) Bivariate	(2) Yield Goal	(3) Controls	(4) State FE
Direct Payments	0.765***	0.323*	0.398***	0.249**
	(0.244)	(0.194)	(0.144)	(0.112)
Yield Goal		2.455***	2.248***	1.308***
		(0.193)	(0.216)	(0.203)
CCP Expected			3.377	4.921
			(3.508)	(3.035)
Total Seed Cost			−0.569***	−0.266**
			(0.185)	(0.124)
Total Fertilizer Cost			−0.011	0.046
			(0.108)	(0.083)
Total Chemicals Cost			−0.397***	−0.082
			(0.149)	(0.111)
Corn-Soy Rotation			13.775***	5.116
			(4.911)	(4.186)
Soy-Soy Rotation			0.172	1.808
			(4.815)	(4.771)
Proportion Irrigated			−3.139	9.853
			(10.038)	(9.230)
Highly Erodible			2.830	−5.974
			(5.589)	(4.327)
In Conservation Pgm			−3.119	−3.126
			(4.818)	(4.249)
Observations	424	424	424	424
R^2	0.044	0.397	0.468	0.668
State FE	No	No	No	Yes

Note: Significance levels are as follows: * = 10%, ** = 5%, and *** = 1%. Standard error (in parentheses) clustered by crop reporting district. See the footnote in table 2 for data source.

Table 5. The Incidence of Field-level Subsidies on Field-level Rental Rates: Rice

Dependent Variable	Field Cash Rental Rate			
	(1) Bivariate	(2) Yield Goal	(3) Controls	(4) State FE
Direct Payments	−0.091	−0.098	0.072	0.200**
	(0.086)	(0.087)	(0.055)	(0.088)
Yield Goal		0.343*	0.391*	0.487**
		(0.165)	(0.215)	(0.209)
CCP Expected			−10.707	2.370
			(8.078)	(7.601)
Total Seed Cost			0.118	0.076
			(0.210)	(0.186)
Total Fertilizer Cost			−0.043	−0.016
			(0.075)	(0.076)
Total Chemicals Cost			0.132	0.136**
			(0.089)	(0.044)
First Crop			−11.714	−29.462*
			(23.607)	(14.334)
Rice-Rice Rotation			3.235	7.946
			(11.666)	(10.806)
Idle-Rice Rotation			−16.274	13.180
			(13.357)	(13.526)
In Conservation Pgm			−3.920	−0.139
			(17.316)	(11.405)
Open Discharge Irrigation			20.163	−16.718*
			(17.404)	(8.893)
Portal System Irrigation			−12.842	−15.524
			(15.781)	(9.069)
Poly Pipe Irrigation			21.077	−14.541*
			(14.198)	(7.859)
Observations	81	81	81	81
R^2	0.019	0.049	0.291	0.528
State FE	No	No	No	Yes

Note: Significance levels are as follows: * = 10%, ** = 5%, and *** = 1%. Standard error (in parentheses) clustered by crop reporting district. See the footnote in table 2 for data source.

Adding the yield goal reduces the point estimate for soybean and cotton fields. As we had suspected, the positive correlation between rental rates and direct payments is driven in part by the land's underlying productivity. Controlling for the yield goal reduces the incidence estimate from 0.765 to 0.323 for soybeans and from 0.598 to 0.490 for cotton, while the rice estimate remains virtually unchanged. The yield goal alone accounts for a substantial share of the variation in rental rates. The point estimate on the yield goal is strongly significant for soybeans and cotton, and the R^2 increases substantially between the Bivariate and Yield Goal specifications for soybeans and cotton and somewhat for rice.

Column 3 shows the importance of further adding covariates; the incidence estimate for all three crops is affected. The rice point estimate becomes positive, but still insignificant. The model's explanatory power continues to rise; each table reports an R^2 greater than 0.29.

Column 4 reports the incidence and covariate estimates after using a state fixed effect to account for state-level characteristics, such as state-specific policies and/or production practices. The tables report that adding one more dollar of direct payments per acre increases a soybean-field rental rate by 24.9 cents, increases rice-field rental rates by 20 cents, and increases the rental rate for cotton fields by 27.5 cents. These estimates are in line with what others have estimated, and they bolster the argument that farmland rental markets do not operate the way basic models of a competitive market would predict.

Aggregation and Measurement Error Biases

An advantage of using field-level data in the analysis is the ability to explore the bias

Table 6. The Incidence of Field-level Subsidies on Field-level Rental Rates: Cotton

Dependent Variable	Cash Rental Rate			
	(1) Bivariate	(2) Yield Goal	(3) Controls	(4) State FE
Direct Payments	0.598***	0.490***	0.302***	0.275**
	(0.118)	(0.155)	(0.119)	(0.120)
Yield Goal		0.077***	0.014	−0.002
		(0.022)	(0.015)	(0.013)
CCP Expected			−0.938	2.734
			(8.222)	(7.874)
Chemical Cost per Acre			0.211***	0.097
			(0.071)	(0.078)
Fertilier Cost per Acre			0.134**	0.097
			(0.064)	(0.055)
Seed Cost per Acre			0.014	−0.057
			(0.100)	(0.066)
Cotton-Cotton Rotation			13.549**	10.475
			(5.107)	(3.793)
Cotton-Wheat Rotation			19.133	13.674
			(14.714)	(12.632)
Cotton-Corn Rotation			−0.137	3.974
			(8.164)	(6.709)
Cotton-Soybeans Rotation			7.565	13.532**
			(5.732)	(6.154)
Proportion Irrigated			45.067***	43.387***
			(11.633)	(11.049)
Highly Erodible			−0.055	−3.064
			(19.314)	(8.523)
In Conservation Pgm			−9.010	2.811
			(8.692)	(8.462)
Observations	215	215	215	215
R^2	0.073	0.231	0.446	0.580
State FE	No	No	No	Yes

Note: Significance levels are as follows: * = 10%, ** = 5%, and *** = 1%. Standard error (in parentheses) clustered by crop reporting district. See the footnote in table 2 for data source.

induced when field-level data is aggregated to the farm level or higher. A general concern when using aggregated data (farm- or county-level) is whether the parameter estimates from the aggregated data accurately represent the fundamental, structural parameters that characterize individual behavior (see Blundell and Stoker (2005) for a general treatment of this problem). In the current problem, these aggregation issues are exacerbated by the subsidy-rate measurement error described above, where farm-level subsidy rates may not reflect subsidy rates on rented fields. When it comes to agricultural subsidy incidence, the primary unit of analysis is the field-level tenant-landlord contractual agreement. This level of analysis circumvents both aggregation and measurement error problems.

Table 7 illustrates the magnitude of bias caused by using aggregate data. To minimize the influence of omitted variables, all specification include farm-level covariates, such as the log of revenue and non-land expenses, in addition to the field-level covariates and state fixed effects. Column 1 contains the field-level incidence estimates for all three crops. The estimates are very similar to those previously reported in column 4 of tables 4, 5, and 6. Column 2 reports the subsidy incidence when the farm-average subsidy is used as a proxy for the field subsidy. This proxy variable appears to slightly bias the estimates upward—except for cotton, which loses precision. The farm-level incidence estimates, reported in column 3, appear to be substantially inflated. The differences between the biased farm-level estimates and the well-identified field-level estimates are 0.222, 0.276, and 0.130 for soybeans, rice, and cotton, respectively.

As noted earlier, one reason for upward bias in the farm-level estimates is the

Table 7. Incidence Estimates for Three Crops at Three Levels of Aggregation

	(1) Field Level	(2) Field-Level Rent & Farm-Average Subsidy	(3) Farm Level
Soybeans (N = 424)	0.237**	0.343***	0.459***
	(0.101)	(0.123)	(0.155)
Rice (N = 81)	0.210***	0.227**	0.486**
	(0.051)	(0.066)	(0.202)
Cotton (N = 215)	0.285**	0.137	0.415*
	(0.113)	(0.124)	(0.233)

Note: Significance levels are as follows: * = 10%, ** = 5%, and *** = 1%. Standard errors are in parentheses. All specifications include the field-level covariates reported previously, state fixed effects, and the following farm-level covariates: farm acreage (log), share of land owned, share of land cash rented, share of land in crops, total sales (log), non-land expenses (log), and operator demographics. Data source: Agricultural Resource Management Survey, Production Practices and Costs (Phase II) and Cost and Returns Report (Phase III): Soybeans (2006), Rice (2006), Cotton (2007).

correlation between the aggregation errors in the farm-average rent and subsidy. Here we use equation (4) along with the results reported in table 7 to determine the size of the aggregation bias. Since the dependent variable in column 2 is not aggregated, the estimates in column 2 do not suffer from bias due to correlated aggregation errors. Assuming negligible omitted-variable bias, the magnitude of the bias caused by correlated aggregation errors on the left and the right can be seen by subtracting the incidence estimate in column 2 from the farm-level estimate in column 3. Correlated aggregation errors account for about half of the farm-level soybean incidence estimate and nearly all of the farm-level rice estimate. Cotton appears to suffer the least aggregation error, but lack of precision in the column 2 estimate precludes determination of the role played by correlated errors.

Heterogeneous Effects

Aggregation bias denotes more than just econometric inconsistency; it refers to whether estimates from aggregate data reflect the true micro parameters (Stoker 2008). In the case of "exact aggregation," every field experiences the same incidence, and consistent aggregate estimates represent the true effect—in terms of our econometric model in equation (5), $\overline{\beta} = \beta$. If the incidence effect is heterogeneous, however, an aggregate estimate would not represent the true effect even if it were consistently estimated, that is, $\overline{\beta} \neq \beta_j$. For example, large farms might experience lower incidence, or incidence may be lower in long-lived tenant-landlord relationships. We investigate incidence heterogeneity next.

We consider two potential sources of incidence heterogeneity. First, given the predominantly local nature of farmland rental markets, it is plausible that larger farms may have local market power. The second potential source of incidence heterogeneity stems from asymmetric information due to the length of time a farmer has rented a field. Private information about land quality and disposition is likely to increase with tenancy. This is likely to occur because the farmers have a better idea of the following: (a) how they are managing the land, and events that have affected it, including fertilizer applications, crop rotations, buffer strips, erosion events, pest problems, etc.; (b) prices of various crops; (c) changes in technology and how well they suit the particular field.

In all respects, the tenant farmer is likely to have an increasingly superior understanding of land quality and this understanding is likely to increase the tenant's bargaining position.

We examine these sources of heterogeneity by augmenting the empirical model in equation (5) by including the source of heterogeneity directly and by interacting it with the subsidy as follows:

$$(6) \quad Rent_{jk} = \alpha_k + \beta Subsidy_{jk} + \delta FarmsSize_{jk} + \phi Subsidy_{jk} * FarmSize_{jk} + \gamma \boldsymbol{X}_{jk} + \varepsilon_{jk}$$

$$(7) \quad Rent_{jk} = \alpha_k + \beta Subsidy_{jk} + \delta Duration_{jk} + \phi Subsidy_{jk} * Duration_{jk} + \gamma \boldsymbol{X}_{jk} + \varepsilon_{jk}.$$

In equation (6) $FarmSize_{jk}$ is measured as the log of the total number of acres operated,

Table 8. Heterogeneous Subsidy Incidence

Dependent Variable	Field Cash Rental Rate		
	Soybeans	Rice	Cotton
Direct Payments	1.678***	0.681*	1.785*
	(0.618)	(0.352)	(1.004)
Acres Operated (log)	5.352***	10.545	9.161**
	(1.657)	(6.251)	(4.094)
DP × Farm Size (log acres)	−0.210**	−0.064	−0.203
	(0.092)	(0.052)	(0.138)
Observations	424	81	215
R^2	0.677	0.553	0.593
State FE	Yes	Yes	Yes
Direct Payments	0.356**	0.218*	0.248
	(0.178)	(0.119)	(0.173)
Duration (years)	0.006	−0.397	−0.195
	(0.149)	(0.310)	(0.229)
DP × Duration (years)	−0.008	−0.001	−0.001
	(0.008)	(0.003)	(0.008)
Observations	424	81	215
R^2	0.669	0.546	0.592
State FE	Yes	Yes	Yes

Note: Significance levels are as follows: * = 10%, ** = 5%, and *** = 1%. Standard error (in parentheses) clustered by crop reporting district. Covariates are the same as in the main analysis.

and in equation (7) $Duration_{jk}$ is the number of years the current operator has farmed the cash-rented field. In both models δ captures the main effect on the rental rate and $\phi \times \ln(2)$ captures the change in the incidence from doubling the farm size (equation [6]) or one year longer duration (equation [7]). Total incidence would be calculated as $\beta + \phi FarmSize$ or $\beta + \phi Duration$ evaluated at a given level of *FarmSize* or *Duration*.

Table 8 reports the coefficients for the main effects and the interactions for the two sources of heterogeneity. The top panel reports the effect of farm size on the incidence. Two things stand out in this analysis. First, the main effect of Direct Payments is substantially larger than without the interaction term. For soybean fields, the coefficient on Direct Payments indicates that the subsidy incidence would be 1.678 on a one-acre farm (which is obviously outside the support of the data). Second, the interaction term is negative for all three crops, though imprecise for rice and cotton. Consider the interpretation of the coefficients in the results for soybean fields. At the mean soybean farm size, 803 acres, the incidence would be 1.678 − 0.210*ln(803) = 0.27, and just under 0.42 for a farm half this size. The interaction term indicates that doubling total operated acres reduces the subsidy incidence by roughly 15 cents—about 36%.[15] Rice farms appear to have less market power, with an estimated incidence of 0.20 for the average farm and 0.25 for a farm half the average size. Cotton farms have the largest farm-size interaction coefficient across the three crops (−0.203), but it also has the largest standard error (0.138) and the largest intercept (1.785). Incidence for an average-size cotton farm is an estimated 0.28, and 0.42 for a farm half the average size.

The lower panel of table 8 reports the effect of landlord-tenant relationship duration on the subsidy incidence. As with farm size, the interaction terms are all negative, although here they are all statistically insignificant. The main subsidy effect is larger than in the primary regressions—for soybeans it increases to 0.356, and, according to the interaction term, the rental-rate incidence falls by about 0.8 cents for every year of rental duration on soybean fields. In other words, on average, landlords extract about 36 cents of the marginal subsidy dollar from new tenants, but the incidence decreases by 0.8 cents with each additional year of the tenancy. At the median duration (10 years), the incidence is 8 cents (roughly 25%) lower than in the first

[15] According to these estimates, a 25-acre farm would experience full subsidy incidence.

year of the rental arrangement. The results are similar, but smaller, for rice and cotton.

The evidence presented in this section provides insight into the reason for low subsidy incidence. Larger farms experience lower subsidy incidence, potentially because they have market power in the local farmland rental markets. Although the estimates are imprecise, longer tenant-landlord relationships also appear to result in lower incidence.

Conclusions

In this paper we show that decoupled agricultural subsidies affect farmland rental rates, but the effect is mitigated by tenant monopsony power. We employ unique field-level data to precisely connect farmland rental rates with decoupled subsidies. The marginal subsidy dollar causes rental rates to increase by 24.9 cents on soybean fields, 20.0 cents on rice fields, and 27.5 cents on cotton fields. When used as a proxy for field-level subsidies, farm-average subsidies overestimate incidence by 10–50%. Notably, incidence estimates from farm-level data range from 50–100% larger than well-identified field-level estimates.

Larger farms appear to leverage their position to reduce the subsidy incidence. Subsidy incidence falls as farm size increases. Doubling the farm size decreases the incidence by about 10 cents. Although the point estimates are imprecise, they suggest that the duration of the tenant-landlord agreement might also reduce the incidence.

This paper improves on previous analysis by using data at the field level to overcome bias caused by aggregation and measurement error. This paper also overcomes omitted-variable bias by explicitly controlling for each field's fundamental productivity. Subsidies are a positive function of the subsidized land's underlying productivity. Hence, failure to account for this productivity results in an upward-biased incidence estimate. We explicitly control for the farmland's underlying productivity by using farmers' self-reported expected productivity of the field along with field-specific costs of production and information on crop rotations. Using field-level data, which is commensurate with the unit of analysis in standard incidence theory, we find that farmland rental rates for subsidized soybean fields increase by 25 cents, subsidized rice-field rental rates increase by 20 cents, and subsidized cotton-field rental rates increase by 28 cents with the marginal subsidy dollar.

Our findings about the incidence of subsidies on land rents could have broader implications. Many articles in the popular press, for example, have informally connected subsidies to land values and growth in large farms (Lynch and Bjerga 2013; Mitchell and Koopman 2013). We provide evidence that the subsidy incidence on rental rates declines as farms get larger. This evidence is consistent with evidence that subsidies contribute to farm-size growth and farm consolidation (Key and Roberts 2006). By capturing the majority of the subsidy, tenant farmers effectively pay less rent on subsidized land. Tenants who face a lower rental rate will rent more subsidized land and, consequently, have larger farms. This mechanism is self-reinforcing: the larger the farms, the greater their market power in the local land market, and the greater tenant farmers' incidence of subsidy benefits.

References

Alston, J.M. 2010. The Incidence of U.S. Farm Programs. In *The Economic Impact of Public Support to Agriculture*, ed. V.E. Ball, R. Fanfani, and L. Gutierrez, 81–105. New York, NY: Springer.

Alston, J.M., and J.S. James. 2002. The Incidence of Agricultural Policy. In *Handbook of Agricultural Economics*, ed. B.L. Gardner, and G.C. Rausser, 1689–749. Oxford, UK: Elsevier.

Bhaskar, A., and J.C. Beghin. 2009. How Coupled Are Decoupled Farm Payments? A Review of the Evidence. *Journal of Agricultural and Resource Economics* 34 (1): 130–53.

Blundell, R., and T.M. Stoker. 2005. Heterogeneity and Aggregation. *Journal of Economic Literature* 43 (2): 347–91.

Breustedt, G., and H. Habermann. 2011. The Incidence of EU Per-hectare Payments on Farmland Rental Rates: A Spatial Econometric Analysis of German Farm-level Data. *Journal of Agricultural Economics* 62 (1): 225–43.

Ciaian, P., and A. Kancs. 2012. The Capitalization of Area Payments Into Farmland Rents: Micro Evidence From the New EU Member States. *Canadian Journal of Agricultural Economics* 60 (4): 517–40.

Claassen, R., R.N. Lubowski, and M.J. Roberts. 2005. Extent, Location, and Characteristics of Land Cropped Due to Insurance Subsidies. In selected paper presented at the Agricultural and Applied Economics Association annual meetings, Providence, RI.

Clark, J.S., K.K. Klein, and S. J. Thompson. 1993. Are Subsidies Capitalized into Land Values? *Some time series evidence from Saskatchewan. Canadian Journal of Agricultural Economics/Revue Canadienne d'Agroeconomie* 41 (2): 155–68.

Coble, K.H., J.C. Miller, and M.D. Hudson. 2008. Decoupled Farm Payments and Expectations for Base Updating. *Applied Economic Perspectives and Policy* 30 (1): 27–42.

Direct Payment Yield for Soybeans and Other Oilseeds 2002. 7 CFR §1412.302.

El-Osta, H.S., A.K. Mishra, and M.C. Ahearn. 2004. Labor Supply by Farm Operators under "Decoupled" Farm Program Payments. *Review of Economics of the Household* 2 (4): 367–85.

Femenia, F., A. Gohin, and A. Carpentier. 2010. The Decoupling of Farm Programs: Revisiting the Wealth Effect. *American Journal of Agricultural Economics* 92 (3): 836–48.

Floyd, J.E. 1965. The Effects of Farm Price Supports on the Returns to Land and Labor in Agriculture. *The Journal of Political Economy* 73 (2): 148–58.

Gardner, B.L. 1987. *The Economics of Agricultural Policies*. New York: McGraw-Hill Companies.

Gohin, A. 2006. Assessing Cap Reform: Sensitivity of Modelling Decoupled Policies. *Journal of Agricultural Economics* 57 (3): 415–40.

Goodwin, B., and A. Mishra. 2006. Are "Decoupled" Farm Program Payments Really Decoupled? An Empirical Evaluation. *American Journal of Agricultural Economics* 88 (1): 73–89.

Goodwin, B.K., A.K. Mishra, and F. Ortalo-Magné. 2011. *The Intended and Unintended Effects of U.S. Agricultural and Biotechnology Policies*. University of Chicago Press.

Hendricks, N.P., J.P. Janzen, and K.C. Dhuyvetter. 2012. Subsidy Incidence and Inertia in Farmland Rental Markets: Estimates From a Dynamic Panel. *Journal of Agricultural and Resource Economics* 37 (3): 361–78.

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

Hyslop, D.R., and G.W. Imbens. 2001. Bias From Classical and Other Forms of Measurement Error. *Journal of Business & Economic Statistics* 19 (4): 475–81.

Key, N., and M.J. Roberts. 2006. Government Payments and Farm Business Survival. *American Journal of Agricultural Economics* 88 (2): 382–92.

———. 2009. Nonpecuniary Benefits to Farming: Implications for Supply Response to Decoupled Payments. *American Journal of Agricultural Economics* 91 (1): 1–18.

Kilian, S., J. Antón, K. Salhofer, and N. Röder. 2012. Impacts of 2003 Cap Reform on Land Rental Prices and Capitalization. *Land Use Policy* 29 (4): 789–97.

Kirwan, B.E. 2009. The Incidence of U.S. Agricultural Subsidies on Farmland Rental Rates. *Journal of Political Economy* 117 (1): 138–64.

Lagerkvist, C.J. 2005. Agricultural Policy Uncertainty and Farm Level Adjustments: The Case of Direct Payments and Incentives for Farmland Investment. *European Review of Agricultural Economics* 32 (1): 1–23.

Latruffe, L., and Le, Le Mouël. 2009. Capitalization of Government Support in Agricultural Land Prices: What do we know? *Journal of Economic Surveys* 23 (4): 659–91.

Lynch, D.J., and A. Bjerga. 2013. Taxpayers Turn U.S. Farmers Into Fat Cats With Subsidies. *Bloomberg*. Available at: http://www.bloomberg.com/news/2013-09-09/farmers-boost-revenue-sowing-subsidies-for-crop-insurance.html.

Melichar, E. 1979. Capital Gains Versus Current Income in the Farming Sector. *American Journal of Agricultural Economics* 61 (5): 1085–92.

Michalek, J., P. Ciaian, and d'A. Kancs. 2014. Capitalization of the Single Payment Scheme Into Land Value: Generalized Propensity Score Evidence From the European Union. *Land Economics* 90 (2): 260–89.

Mitchell, M., and C. Koopman. 2013. The Growing 'Grain Drain.' *U.S. News and World Report*. Available at: http://www.usnews.com/opinion/blogs/economic-intelligence/2013/09/24/agriculture-subsidies-actually-hurt-young-farmers.

Morehart, M. 2014a. *ARMS Farm Financial and Crop Production Practices: Documentation*. Washington DC: U.S. Department of Agriculture, Economic Research Service. Available at: http://www.ers.usda.gov/data-products/arms-farm-financial-and-crop-production-practices/documentation.aspx.

——— 2014b. *What Is the Agricultural Resource Management Survey (ARMS)?* Washington DC: U.S. Department of Agriculture, Economic Research Service. Available at: http://www.ers.usda.gov/data-products/arms-farm-financial-and-crop-production-practices.aspx.

Morgan, D., G.M. Gaul, and S. Cohen. 2006. Farm Program Pays $1.3 Billion To People Who Don't Farm. *The Washington Post*. Available at: http://www.washingtonpost.com/wp-dyn/content/article/2006/07/01/AR2006070100962.html.

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 (5): 397–405.

Perry, C., J. Burt, and W. Iwig. 1994. *Redrawing the 1993 Farm Cost and Returns Survey List Frame Sample to Reduce Its Overlap with the 1992 FCRS and Three Other 1993 Surveys*. SRB Research Report No. SRB-94-10. USDA, NASS. Washington. D.C.

Qiu, F., B. Goodwin, and J. Gervais. 2011. An Empirical Investigation of the Linkages between Government Payments and Farmland Leasing Arrangements. *Journal of Agricultural and Resource Economics* 36 (3): 536–51.

Reich, R. 2001. Subsidies Aren't a Wartime Necessity. *Wall Street Journal*. Available at: http://online.wsj.com/article/SB1003191663686100520.html.

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 (3): 762–9.

Robison, L.J., D.A. Lins, and R. VenKataraman. 1985. Cash Rents and Land Values in U.S. Agriculture. *American Journal of Agricultural Economics* 67 (4): 794–805.

Roe, T., A. Somwaru, and X. Diao. 2002. *Government Policy and Farmland Markets*. Iowa State Press.

Schmitz, A., and R. Just. 2003. The Economics and Politics of Farmland Values. In *Farmland Markets*, ed. C. B., Moss, and Andrew Schmmitz, 53–79. Ames, IA: Iowa State Press.

Serra, T., B.K. Goodwin, and A.M. Featherstone. 2011. Risk Behavior in the Presence of Government Programs. *Journal of Econometrics* 162 (1): 18–24.

Sherrick, B., and P. Barry. 2003. Farmland Markets: Historical perspectives and contemporary issues. *Government Policy and Farmland Markets: The Maintenance of Farmer Wealth* (2003): 27–49.

Smith, A. 1776. *An Inquiry into the Nature and Causes of the Wealth of Nations*. London: printed for W. Strahan; and T. Cadell, in the Strand.

Stoker, T.M. 2008. *The New Palgrave Dictionary of Economics*. New York, NY: Palgrave McMillan.

U.S. House of Representatives, Committee on Ways and Means 2014. *2014 Green Book, Background Material and Data on Programs Within the Jurisdiction of the Committee on Ways and Means*. 113th Congress, 2nd Session, Washington, DC.

U.S. Department of Agriculture, Economic Research Service. 2000. *Farm Resource Regions, AIB-760*. Washington DC.

U.S. Department of Agriculture, National Agricultural Statistics Service. 2006. Agricultural Resource Management Survey (ARMS) Phase II - Field Crop Chemical Usage & Production Practices: Interviewer's Manual. Washington DC.

Varian, H.R. 2010. *Intermediate Microeconomics: A Modern Approach*. New York: WW Norton.

Vercammen, J. 2007. Farm Bankruptcy Risk As a Link Between Direct Payments and Agricultural Investment. *European Review of Agricultural Economics* 34 (4): 479–500.

Weber, J.G., and N. Key. 2012. How Much Do Decoupled Payments Affect Production? An Instrumental Variable Approach With Panel Data. *American Journal of Agricultural Economics* 94 (1): 52–66.

Weersink, Alfons, S., Clark, S., and Turvey, R., Sarker. 1999. The Effect of Agricultural Policy on Farmland Values. *Land Economics* 75 (3): 425–39.

Youde, J.G. 1974. Basic Provisions of the 1973 Farm Act. *California Agriculture* (February): 5–6.