# Agricultural Subsidy Incidence: Evidence from Political Favoritism

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**Abstract**

We estimate the incidence of direct payments in the United States on cash rental rates. Our econometric model exploits variability in direct payments due to variation in the proportion of cropland with base acres of crops produced in the South while controlling for expected market returns. Southern crop base acres received substantially larger direct payments because Southern agriculture historically received political favoritism. Estimates from two-stage least squares indicate that roughly \$0.80 of every dollar of direct payments accrues to landlords through higher rental rates in the long run and we cannot reject the null hypothesis of full incidence.

*Keywords*: Incidence, agricultural subsidies, decoupled payments, rental rates.
*JEL codes*: Q18, H22.

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Political support for government intervention in the market often depends as much on the distribution of benefits and costs as the overall change in social welfare. In recent years, the beneficiaries of agricultural subsidies in the United States have come under increased scrutiny due to the pressure to reduce budgetary expenditures in the Farm Bill. The United States spent roughly $7.6 billion annually between 2000 and 2013 on agricultural commodity subsidies (U.S. Department of Agriculture 2016).[1] One concern is that non-operator landowners may benefit from these agricultural subsidies—even though the subsidies are generally paid directly to farm operators. Non-operator landowners may capture a portion of the subsidies by adjusting rental rates.

Economists have long recognized that the economic incidence of government subsidies differs from the initial recipient of such subsidies. Standard economic theory predicts that non-operator landowners capture all of a purely decoupled subsidy but only capture a portion of a subsidy directly tied to production (Floyd 1965; Alston and James 2002). Direct payments in the United States, effective during the period 2002–2014, were one example of a fixed subsidy that was not tied to current production or price.[2] There are, however, several reasons why landowners may not capture the entire direct payment. First, tenants are often related to the landowner (Schlegel and Tsoodle 2008), so some rental rates may not reflect the competitive rate (Perry and Robison 2001; Tsoodle, Golden, and Featherstone 2006).[3] Second, direct payments are not purely decoupled (e.g., Hennessy 1998; Just and Kropp 2013; Hendricks and Sumner 2014). Third, tenants may exercise market power in the rental market (Kirwan 2009; Kirwan and Roberts 2016).

Most studies examining the impact of government payments on rental rates find that less than $0.50 of every dollar of subsidies is captured by changes in the rental rate (Kirwan

[1] In this calculation, we only include production flexibility contract, fixed direct, Average Crop Revenue Election (ACRE), counter-cyclical, and loan deficiency payments. Expenditures are much larger after accounting for crop insurance subsidies, ad hoc disaster assistance, and conservation programs.

[2] Note that we refer to direct payments in this paper as the specific type of subsidy implemented in the U.S. between 2002 and 2014, rather than referring to direct payments more broadly as any payment made directly to farmers.

[3] However, Bryan, Deaton, and Weersink (2015) do not find a strong impact of family relations on rental rates.

2009; Breustedt and Habermann 2011; Hendricks, Janzen, and Dhuyvetter 2012; Ciaian and Kancs 2012; Kilian et al. 2012; Herck, Swinnen, and Vranken 2013; Michalek, Ciaian, and Kancs 2014; Kirwan and Roberts 2016). There are a few exceptions in the literature that find larger impacts on rental rates (Lence and Mishra 2003; Patton et al. 2008; Goodwin, Mishra, and Ortalo-Magné 2011), but these studies are subject to concerns that unmeasured variability in productivity bias their coefficient estimates upward (see Kirwan and Roberts (2016) for a critique). One unresolved puzzle is that previous literature usually finds a large impact of government payments on land values (Latruffe and Le Mouël 2009) even though the estimated impact on rental rates is usually small. For example, Ifft, Kuethe, and Morehart (2015) find that an additional dollar of direct payments increases land values by about $18. Given that rents are a major determinant of land values (Alston 1986; Burt 1986), it seems odd that non-operators would be willing to pay a premium for land with greater government payments but not extract the government payments through higher rental rates.

Our paper makes two important contributions. First, we exploit a new source of plausibly exogenous variation in direct payments. We exploit the difference in direct payments in counties that have base acres of Southern crops (i.e., cotton, rice, and peanuts) and argue that the difference in payments has arisen due to political favoritism towards Southern crops. Identification using this source of variation helps to reduce concerns that our estimate is biased upward due to unmeasured productivity and exploits large, persistent differences in farm subsidies. Second, we provide an explanation for the difference in our estimates of a large incidence and much of the previous literature that estimates a small incidence. We argue that rents adjust more to large, persistent differences in direct payments than small within-region variability due to within-region customary arrangements and round-number rental rates.

Intuitively, our empirical strategy compares cash rental rates in counties that have similar market returns, but that have different direct payments due to the favoritism shown to areas that historically produced Southern crops. Our econometric model uses county-level data and regresses cash rental rates on direct payments, expected market returns, and the proportion of cropland enrolled in the Average Crop Revenue Election (ACRE) program. We instrument direct payments with the share of cropland with Southern crop base acres. We argue that the favoritism shown to Southern crops is primarily due to political favoritism which should have no direct impact on rental rates except through government payments. Since production of these crops is concentrated in a particular region, there could be concerns that our instrument is correlated with differences in unmeasured expected returns between regions. We use the framework of Conley, Hansen, and Rossi (2012) to construct revised standard errors that allow for a potential violation of the exclusion restriction.

According to the OECD Producer Support Estimates, the 2000–2014 average commodity-specific government transfers as a percent of total gross commodity receipts was only 5% for corn and soybeans and 7% for wheat, while it was 20% for cotton and 12% for rice.[4] Data that we construct for this paper also indicate that counties with Southern crop base acres received substantially larger direct payments than counties with similar market returns but no Southern crop base acres. Southern commodities were favored politically in farm legislation of the 1930s due to the significant political influence of large Southern landowners (Winders 2009). Furthermore, one-party rule in the Southern U.S. up to 1960 resulted in Southern lawmakers holding powerful political positions (Gardner 1987). By the 1970s, Southern landowners had lost their control of the rental market so that leases in the South were similar to those in the North (Winders 2009). However, the favoritism of subsidies towards Southern crops has persisted due to the capitalization of the benefits into asset values and the large incentive for Southern landowners to maintain de facto power in agricultural policy (Barkley 1996; Acemoglu and Robinson 2006).

We estimate that roughly $0.80 of every dollar of direct payments accrues to non-operator landlords, but we cannot reject the null hypothesis of full incidence. Our estimate is larger than other articles that exploit exogenous variation through changes in government payments

[4]OECD does not provide Producer Support Estimates for peanuts in the United States.

between time periods (e.g., Kirwan 2009; Hendricks, Janzen, and Dhuyvetter 2012; Michalek, Ciaian, and Kancs 2014) or differences in government payments between fields (Kirwan and Roberts 2016). We argue that the incidence with these two types of variations in subsidies are different. Rental rates between fields within a particular geographic region may not fully reflect differences in field-specific direct payments if rates are established by customary arrangements in the region so that rents depend on average conditions within the region and not just field-specific conditions (see Young and Burke 2001). However, rental rates between different regions may fully reflect direct payments as the customary arrangements in each region reflect the typical direct payments of that region. Similarly, small changes in direct payments over time may have a negligible impact on rental rates if rents tend to be established at round numbers. Both types of incidence are relevant for policy analysis, but understanding the incidence of large, persistent differences in subsidies is most relevant for understanding how rental rates would change if subsidies were eliminated.

Even though direct payments were eliminated in the 2014 Farm Bill, our estimate of the incidence is relevant to current and future farm programs for two reasons. First, understanding the incidence of fixed payments not tied to production in real-world rental markets provides an important baseline for understanding the incidence of more complex programs. If direct payments are not fully reflected in rental rates, then economic theory under perfectly competitive rental markets may not provide realistic estimates of the long-run incidence of other types of programs. Second, Agriculture Risk Coverage (ARC) and Price Loss Coverage (PLC) payments, which were introduced in the 2014 Farm Bill, are both tied to base acres and base yields rather than current production.[5] Therefore, the incidence of ARC and PLC payments is likely similar to the incidence of direct payments, although the incidence could be smaller for ARC and PLC due to uncertainty about the payments.

[5]ARC provides payments when county-level revenue falls below a trigger and PLC provides payments when price falls below a trigger.

## Model

We assume that cash rental rates differ across counties according to differences in the returns to land from market returns to crop production and government subsidies. This relationship is approximated using a linear model:

$$(1) \quad Rent_i = \beta_1 + \beta_D DirectPmts_i + \beta_R MktReturns_i + \beta_A ACRE_i + \varepsilon_i,$$

where $Rent_i$ is the average cash rental rate per acre for cropland in county $i$, $DirectPmts_i$ is the average direct payment subsidy per acre, $MktReturns_i$ is the true expected market return for cropland, $ACRE_i$ is the true expected payments from the ACRE program, and $\varepsilon_i$ is the variation in rental rates from other unobserved factors. The objective of our paper is to estimate $\beta_D$, which represents the proportion of direct payments captured in rental rates.

*Bias of OLS*

If we could observe the true expected market returns and true expected ACRE payments, then we could obtain an unbiased estimate of $\beta_D$ by estimating equation (1) with OLS. However, we are unlikely to perfectly measure the true expected market returns and ACRE payments and this measurement error may be correlated with direct payments and bias OLS results. We observe $MktReturns_i^* = MktReturns_i + \eta_i^R$ and $ACRE_i^* = ACRE_i + \eta_i^A$, where variables with a $^*$ superscript denote observed variables and $\eta_i^R$ and $\eta_i^A$ denote the measurement errors. We assume classical measurement error so that the measurement errors are uncorrelated with the true variables.

Measurement error in the control variables results in attenuation bias of their coefficients, but also contaminates the coefficient on direct payments.[6] The bias can be written as[7]

$$\text{plim}\hat{\beta}_D - \beta_D = (\beta_R - \text{plim}\hat{\beta}_R)\pi_R + (\beta_A - \text{plim}\hat{\beta}_A)\pi_A, \tag{2}$$

where $\pi_A$ and $\pi_R$ are defined from the following equations

$$MktReturns_i = \pi_R DirectPmts_i + \delta_R ACRE_i^* + \mu_i^R, \tag{3}$$

and

$$ACRE_i = \pi_A DirectPmts_i + \delta_A MktReturns_i^* + \mu_i^A. \tag{4}$$

Equation (2) is helpful for understanding the likely direction of the bias. $\beta_R$ is expected to be positive and the probability limit is expected to be smaller than the true value due to attenuation bias. Therefore, imperfectly controlling for expected market returns biases the coefficient on direct payments upwards, *ceteris paribus*, since market returns are positively correlated with direct payments ($\pi_R > 0$). The bias from imperfectly controlling for expected ACRE payments could be upwards or downwards. Since enrollment in ACRE likely increased total expected government payments ($\beta_A > 0$) and decreased direct payments ($\pi_A < 0$), we may expect downward bias. However, it could also be the case the ACRE was a more

[6] Another way to think about the source of bias is that $MktReturns_i$ and $ACRE_i$ are proxy variables for the true controls, expected market returns and expected ACRE payments. OLS is unbiased only if the true controls are uncorrelated with direct payments after partialling out $MktReturns_i$ and $ACRE_i$ (Wooldridge 2010, pp. 67–69).

[7] The derivation of the bias follows by noting that OLS effectively omits the terms $(\beta_R - \text{plim}\hat{\beta}_R)MktReturns_i$ and $(\beta_A - \text{plim}\hat{\beta}_A)ACRE_i$. Substituting equations (3) and (4) into these omitted terms and collecting the terms on $DirectPmts_i$ gives the bias in equation (2). Griliches (1986) derives the same formula in the case of a single control variable measured with error.

appealing program in areas with greater direct payments ($\pi_A > 0$) so the bias could be upwards. Therefore, the overall sign of the bias of OLS is indeterminate.

In the results section, we estimate equations (3) and (4) with the observed variables as the dependent variable to give insights how the magnitude of the bias in OLS differs across subsamples of our data. An important caveat to those regressions is that estimates of $\pi_R$ and $\pi_A$ are unbiased only if the measurement errors are uncorrelated (i.e., $Cov(\eta_i^R, \eta_i^A) = 0$). Furthermore, we cannot measure the magnitude of attenuation bias in $\beta_R$ and $\beta_A$.

*2SLS Identification Strategy*

We employ 2SLS to resolve the bias of OLS due to measurement error of the control variables. We assume that direct payments are determined by market returns of the land and political influence. Thus, the key source of exogenous variation in direct payments that we seek to exploit is due to political influence. We measure political influence as the proportion of cropland with Southern crop base acres ($SouthBase_i$) giving the following first stage equation

$$(5) \quad DirectPmts_i = \alpha_1 + \alpha_{SB} SouthBase_i + \alpha_R MktReturns_i + \alpha_A ACRE_i + u_i.$$

Agricultural subsidy programs have favored commodities grown in southern counties (i.e., cotton, rice, and peanuts) due, at least in part, to the influence of southern planters in early farm policy (Winders 2009).[8] Southern planters were large landowners as remnants of the plantation system in the South that had significant political influence in the early 1900s (Key 1949; Winders 2005). The Agricultural Adjustment Act (AAA) passed in 1933 was the first major national policy to implement price supports and production controls for agricultural

[8] Another argument for the political favoritism of southern commodities is that farm programs are primarily a means of income redistribution and Southern commodities receive greater support because income can be redistributed more efficiently for these commodities (Gardner 1987). Under these arguments, Gardner (1987) shows that government support depends on supply and demand elasticities and the cost of political lobbying specific to each commodity.

commodities. One-party rule in the South in the early 1900s resulted in southern lawmakers holding powerful political positions. In agriculture, southern Democrats held key positions as the chair of House and Senate Agriculture committees and Senate majority leader when the AAA was passed, giving southern planters significant influence over the policy.[9]

The influence of southern lawmakers continued after the AAA was passed. From 1931 to 1995, the chairman of the House Committee on Agriculture was from a Southern state for all but 10 years. From 1933 to 1995, the chairman of the Senate Committee on Agriculture was from a Southern state for all but 12 years. The influence of southern planters in the initial AAA and southern lawmakers in later legislation resulted in favoritism towards Southern commodities. We exploit this political favoritism by using the proportion of cropland with base acres of Southern commodities as an instrument for direct payments.

Southern planters also exerted substantial control in the landowner-tenant relationship in the early 1900s. Figure 1 shows that in 1910 and 1920 over 60% of farms in the South were operated by tenants that owned no land (Haines and ICPSR 2010). In the early 1900s, rental agreements in the South were sharecropper arrangements that indebted the tenant to the landowner and were in general oppressive towards the tenant (Conrad 1965). The exclusion restriction would be violated if the market power of southern landowners has persisted and affects current rental rates. However, the rate of tenancy fell dramatically after 1920 so that by 1974 there was little difference in the share of farmers that were tenants in the South from other production regions (figure 1). Winders (2009) argues that the type of rental arrangements changed also. By the 1960s the common arrangement in the South was a traditional farmland lease. This leads Winders (2009, p. 113) to conclude:

> "Thus, the plantation system no longer characterized southern agriculture by the late 1960s. The class structure of the rural South came to reflect closely that of the Midwest: the majority of farms were smaller and owner-operated (that is,

[9] Winders (2009) also argues that southern planters influenced the AAA to ensure that all payments were sent directly to landowners. In 1938, legislation was passed that required payments to be shared between landowners and tenants. In response, many planters hired tenants as wage laborers instead so that landowners could continue to capture all of the payments.

> neither tenant nor plantation), and tenancy meant leasing land and comprised a small proportion of farms."

But the influence of southern lawmakers and, in particular, the favoritism of subsidies towards southern commodities has persisted. One explanation for the persistence of the policies is that they may be viewed as an entitlement. Barkley (1996) develops a dynamic political economy model that illustrates the persistence of agricultural subsidies due to the capitalization of the benefits into asset values. The model of Acemoglu and Robinson (2006) also illustrates that even though the role of southern planters diminished, the policies can persist because the remaining landowners in the South had a large incentive to maintain their de facto power in agricultural policy.[10]

Consistency of 2SLS requires two assumptions: (i) the first stage relationship between the instrument and the endogenous regressor exists, and (ii) the exclusion restriction holds. The first assumption requires that $\alpha_{SB} \neq 0$. Furthermore, finite sample bias can exist if the relationship between the instrument and endogenous regressor is not sufficiently strong (Bound, Jaeger, and Baker 1995). In our case, the relationship between the share of cropland with Southern crop base acreage and direct payments is strong as we show in our results.

The exclusion restriction in our model requires that the proportion of cropland with Southern crop base acreage only affects rental rates through the effect on direct payments after parsing out market returns and enrollment in ACRE (i.e., $Cov(SouthBase_i, \varepsilon_i) = 0$).[11] We argue that the proportion of cropland with Southern crop base acreage is plausibly exogenous because Southern crops are politically favored which affects the direct payments, but there is no reason that land with Southern crop base should have systematically different rental rates conditional on the same expected market returns. In other words, the proportion of cropland with Southern crop base acreage cannot be correlated with other

[10]The Renewable Fuels Standard has resulted in large benefits to corn and soybean producers (Moschini, Lapan, and Kim 2017), so one could argue that current government policy favors corn and soybeans. However, the emphasis of our analysis is on the favoritism of direct payments in the 2008 Farm Bill.

[11]To put this assumption within the context of customary arrangements, we argue that the customary arrangements differ in areas with Southern crops because direct payments are larger for Southern crops, conditional on the same market returns.

factors not included in our model that affect rental rates. This assumption requires that any measurement error in our estimate of market returns is not systematically related to the amount of Southern crop base acres. The consistency of OLS requires that any measurement error in market returns is uncorrelated with direct payments—a more stringent assumption. The exclusion restriction also requires that there is nothing systematically different about counties with Southern crop base acres apart from direct payments, market returns, and ACRE enrollment that would affect the rental rate such as differences in the competitiveness of the rental markets.

*Local Average Treatment Effect*

Our 2SLS estimate gives a Local Average Treatment Effect (LATE). In particular, our 2SLS strategy isolates the effect of large, persistent differences in direct payments. To see this, note that 2SLS uses the direct payments predicted from the first stage regression so 2SLS only exploits the variation in direct payments explained by the proportion of cropland with Southern crop base acres. Therefore, 2SLS exploits the large difference in direct payments due to favoritism towards Southern crops. Alternatively, fixed effects methods estimate the impact of small changes in subsidies over time. We expect that the true impact of direct payments on rental rates is larger due to large, persistent differences in direct payments rather than small changes over time or small differences within a region. So a LATE interpretation indicates that our estimate is most relevant to policy scenarios that would make large changes to direct payment rates.

*Relaxing the Exclusion Restriction*

The exclusion restriction is unlikely to hold perfectly in most applications and there are reasons to think that it might be violated in our model. Following Conley, Hansen, and Rossi (2012), equation (1) can be rewritten as

$$(6) \quad Rent_i = \beta_1 + \beta_D DirectPmts_i + \beta_R MktReturns_i + \beta_A ACRE_i + \gamma SouthBase_i + \varepsilon_i,$$

where the exclusion restriction imposes $\gamma = 0$. Intuitively, $\gamma$ represents the expected value of the difference in cash rent in a county where all cropland had Southern crop base acres and the cash rent in a county that had no Southern crop base acres—controlling for differences in direct payments, expected market returns, and ACRE enrollment. The difference in cash rental rates represented by $\gamma$ could occur because we have not completely controlled for differences in expected market returns between counties with Southern crop base acres and those without Southern crop base acres.

When $\gamma \neq 0$, then the probability limit of 2SLS is written as $\hat{\beta}_D \xrightarrow{p} \beta_D + \gamma/\alpha_{SB}$ in our case where $\beta_D$, $\gamma$, and $\alpha_{SB}$ are scalars (Conley, Hansen, and Rossi 2012). The probability limit of 2SLS makes clear that the bias from violations of the exclusion restriction depends on the strength of the first stage relationship (see also Bound, Jaeger, and Baker 1995). Small deviations from the exclusion restriction can induce large bias when the first stage relationship is weak and conversely relatively large deviations from the exclusion restriction may have a smaller effect on bias when the first stage relationship is strong. In practice, there is often a tradeoff between the plausible exogeneity of an instrument and the strength of the first stage relationship. We choose an instrument that has a strong first stage relationship but where the exclusion restriction is unlikely to hold perfectly.

To account for potential deviations from the exclusion restriction, we construct revised standard errors using the framework of Conley, Hansen, and Rossi (2012). We do not know the true value of $\gamma$ but we make an assumption about likely values, essentially imposing a prior distribution for $\gamma$. We assume that $\gamma \sim N(0, \delta^2)$, where $\delta$ is the standard deviation of likely values of $\gamma$. We do not have any prior beliefs about whether $\gamma$ is more likely to be positive or negative so we assume $\gamma$ has mean zero. Imposing prior beliefs about the distribution of $\gamma$ is more general than the standard 2SLS approach that imposes the prior belief that $\gamma = 0$. When $\gamma$ is assumed to be normally distributed, Conley, Hansen, and Rossi (2012) show how to easily calculate a revised variance matrix by using a large sample approximation that assumes uncertainty about $\gamma$ is of the same order of magnitude as sampling uncertainty. Conley, Hansen, and Rossi (2012) refer to this approach as a local-to-zero approximation.[12] In the results section, we discuss our specific prior beliefs about $\gamma$.

## Identification Challenges

In this section, we review the main challenges in identifying the incidence of agricultural subsidies. We also describe approaches of previous literature and compare them to our approach described in the previous section.

### *Measuring the Rental Rate*

The first challenge is to obtain data on the cash rental rate for the dependent variable. Several previous studies estimate the relationship between government payments and land values (Goodwin and Ortalo-Magné 1992; Just and Miranowski 1993; Weersink et al. 1999; Barnard et al. 1997; Ifft, Kuethe, and Morehart 2015). One challenge with using land values

[12] Another approach proposed by Conley, Hansen, and Rossi (2012) is to use Bayesian analysis that incorporates prior information about $\gamma$. A full Bayesian analysis also requires priors about other model parameters though. Conley, Hansen, and Rossi (2012) suggest that the Bayesian and local-to-zero approaches are likely to give similar results in large samples so we simply use the local-to-zero approach. Another alternative approach proposed by Conley, Hansen, and Rossi (2012) is to use only a support assumption for $\gamma$ and construct the union of confidence intervals. The disadvantage of this approach is that the confidence intervals are likely to be large since it gives equal weight to all potential values of $\gamma$, even those at the extremes that seem unlikely. The local-to-zero approach gives tighter confidence intervals by assuming a normal distribution for the potential values of $\gamma$.

as the dependent variable is that land values depend on factors other than agricultural returns that must also be controlled for in the regression such as urban development, amenities, and mineral rights (e.g., Plantinga and Miller 2001; Ifft, Kuethe, and Morehart 2015). Another challenge is to translate the effect of subsidies on land values into estimates of the proportion of subsidies reflected in land values, which requires assumptions about the discount rate and expected stream of government payments (Kirwan 2009; Hendricks, Janzen, and Dhuyvetter 2012). Identifying the impact on rental rates provides a cleaner identification strategy since rental rates presumably depend on the current expected returns from agricultural production.

However, data on rental rates have not been as widely available as land value data. Some studies use cash rent calculated as total rent divided by total rented acres (Kirwan 2009; Hendricks, Janzen, and Dhuyvetter 2012), but this underestimates the true cash rental rate since total rented acres include acres rented by cash and crop-share agreements.[13] Hendricks, Janzen, and Dhuyvetter (2012) show how this measurement error biases the coefficient on government payments downward with their data and use secondary data to correct for the bias.

In this paper, we use data on the average cash rental rate for cropland at the county level. These data are obtained from NASS surveys of the cash rental rate for irrigated and nonirrigated cropland, rather than constructing the rental rate from total rent divided by rented acres. Other studies that use data on actual cash rental rates include Kirwan and Roberts (2016) and Goodwin, Mishra, and Ortalo-Magné (2011).

*Expectation Error*

The second challenge is to accurately measure *expected* government payments. Farm subsidy programs often depend on the harvest price—and more recently yield. Cash rental rates are negotiated before harvest, and thus government payments are uncertain. The econometri-

[13] Furthermore, the Census and Kansas Farm Management Association data include rent for pasture which does not receive government payments. The Farm Accountancy Data Network (FADN) used by Michalek, Ciaian, and Kancs (2014) and Ciaian and Kancs (2012) also only reports total rent and total rented acres but it is not clear to us how crop-share acreage is treated in their data.

cian, however, only observes data on the realized government payments. Regressing rent on realized government payments results in attenuation bias since the observed variable has a larger variance than the true variable. Therefore, the coefficient on government payments is likely to be biased towards zero, *ceteris paribus*.

Kirwan (2009) provides a creative solution to the measurement error problem. He argues that government payments in 1997 were known with certainty due to the introduction of production flexibility contracts that did not depend on price or current production. Therefore, Kirwan (2009) uses the 1997 government payments as an instrument for the difference in 1997 and 1992 government payments. Several other studies use lagged or future government payments as an instrument for current government payments (Lence and Mishra 2003; Hendricks, Janzen, and Dhuyvetter 2012; Kilian et al. 2012). Goodwin, Mishra, and Ortalo-Magné (2011) consider different specifications where they use the previous 5-year average of government payments to approximate expected payments or various instruments. Kirwan and Roberts (2016) and our work include direct payments—which were known with certainty—as the key variable of interest so that expectation error is not a concern.

### *Omitted Variable Bias*

The third challenge is to control for expected returns other than direct payments. Several articles exploit panel data and include fixed effects to control for time-invariant productivity (Kirwan 2009; Hendricks, Janzen, and Dhuyvetter 2012; Ciaian and Kancs 2012; Herck, Swinnen, and Vranken 2013; Michalek, Ciaian, and Kancs 2014). Patton et al. (2008) include fixed effects but Kirwan and Roberts (2016) argue that unobserved heterogeneity still biases their results since payments not tied to production were implemented in the last year of their sample, so Patton et al. (2008) effectively include the level of payments as the explanatory variable. Lence and Mishra (2003) and Patton et al. (2008) use lagged market returns as an instrument for current market returns to reduce attenuation bias of the effect of market returns. Goodwin, Mishra, and Ortalo-Magné (2011) use a historical average of agricultural sales minus production costs at the county-level as a control, but this includes returns from livestock production.

Another potential omitted variable is the expected payments from government programs others than direct payments. We use rent data from 2012 when prices were high above the triggers so that farmers arguably perceived a negligible probability of receiving counter-cyclical and loan deficiency payments.[14] The 2008 Farm Bill also introduced the Average Crop Revenue Election (ACRE) Program. ACRE was a voluntary program that provided farmers with payments when state-level revenues fell below a trigger. Farmers that enrolled in ACRE lost 20% of their direct payments. Therefore, direct payments decreased more in counties with greater ACRE enrollment. Farmers likely anticipated receiving payments from ACRE in counties with enrollment, or else farmers would not have enrolled in the ACRE program.

We take great effort to construct a control for market returns that accounts for variation in market returns across space and across crops. However, we recognize that we are unlikely to perfectly control for expected market returns and expected ACRE payments so we propose an instrumental variable approach that exploits plausibly exogenous variation in direct payments due to political favoritism of Southern commodities.

*Long-Run Incidence*

The fourth challenge is to estimate the long-run incidence, allowing for adjustments in rental rates over time. Hendricks, Janzen, and Dhuyvetter (2012) find substantial inertia in farm-level rental rates. One reason for inertia in rents is the long-lived relationship between tenants and landowners—the average length of tenancy is 17 years in Kansas (Hendricks, Janzen, and Dhuyvetter 2012). Multi-year contractual agreements also create inertia.

[14] Counter-cyclical and loan deficiency payments were essentially zero for 2012 crop production. Furthermore, counter-cyclical and loan deficiency payments were less than $22 million from production in the previous two years (U.S. Department of Agriculture 2016).

Using panel data with fixed effects exploits year-to-year changes which only capture short-run rental rate adjustments (Ciaian and Kancs 2012; Herck, Swinnen, and Vranken 2013; Michalek, Ciaian, and Kancs 2014). Kirwan (2009) uses long (five-year) differences. Hendricks, Janzen, and Dhuyvetter (2012) and O'Neill and Hanrahan (2016) rely on the partial adjustment framework to estimate long-run impacts. The year-to-year variation in subsidies exploited by these studies is often small, so rental rates may be slow to adjust or not adjust at all to maintain rent at a round number.

We exploit large cross-sectional variation in subsidy rates which inherently captures a long-run effect without having to explicitly specify the dynamic process of rental rate adjustment.[15] That is, the difference in rents between counties with and without Southern crop base acres is due to the persistent difference in direct payments, conditional on the same market returns. An alternative approach is to use the partial adjustment framework with a dynamic panel model of rents. With this approach, we would need to control for county fixed effects to account for unobserved heterogeneity that is correlated with the lagged dependent variable and use an Arellano-Bond type estimator (Cameron and Trivedi 2005). Two problems with this approach are that it only exploits year-to-year changes within counties and the effect of direct payments is not identified since direct payments did not change from 2008 to 2012.

*Aggregation*

The fifth challenge is to have data at the appropriate level of aggregation. Kirwan and Roberts (2016) find that farm-level estimates of the incidence are roughly twice as large as field-level estimates. Estimates with aggregate data (i.e, at the farm or county level) are biased if rented land has systematically different subsidy rates than owner-operated land and the aggregate subsidy rate is averaged across rented and owner-operated cropland (Kirwan and Roberts 2016). One reason this bias could occur is if rented land had systematically

[15]Lence and Mishra (2003) also exploit cross-sectional variation in rents but only in Iowa, so they do not exploit large differences in subsidy rates due to political favoritism.

different productivity than owner-operated land. We examined responses from a survey of farmers in Kansas where farmers were asked to separately estimate the average market value of cropland that they owned and rented.[16] A majority of farmers (67%) indicated no difference in value between owned and rented and the average difference in value across all farms was not significantly different from zero. Estimates with aggregate data may also be biased if rent is averaged across subsidized and unsubsidized land while the subsidy rate is averaged only across subsidized land. To avoid this problem, we calculate the average subsidy rate across subsidized and unsubsidized land since we divide total subsidies by total cropland.

An alternative explanation for the difference in estimates with field-level and aggregate data is that rental rates depend on customary arrangements within a particular region. For example, Young and Burke (2001) note that cropshare agreements have different splits across different regions as would be predicted by conventional theory, but the agreements rarely vary within a geographic region even though soil quality clearly varies within a region. Young and Burke (2001) suggest that this occurs because contracts tend to cluster around a few discrete values and because contracts tend to conform to the customary local arrangements. Under this argument, the cash rental rate depends on the average direct payments within the region.

## Data Description

We restrict our analysis to counties in five farm resource regions as defined by the U.S. Department of Agriculture (2015): the Northern Great Plains, Prairie Gateway, Heartland, Mississippi Portal, and Southern Seaboard. This region is the area of the United States where most production of field crops occurs.[17] Within the Southern Seaboard region we

[16] The survey was conducted as part of grant project funded by the National Science Foundation under Award No. EPS-0903806. There are 567 survey responses that provided an estimated market value for both owned and rented cropland.

[17] See the Cropland Data Layer for an overview of the location of field crop production available at `https://nassgeodata.gmu.edu/CropScape/`.

exclude South Carolina, North Carolina, and Virginia because tobacco comprises a major portion of crop sales and ERS has not recently published cost of production estimates for tobacco.[18] There is also substantial cotton and rice production in California and Arizona, but we exclude these areas from our analysis because most crop sales in this region are from fruit, nut, and vegetable production which we do not include in our estimate of market returns.

Our dependent variable is the average cash rental rate for cropland in 2012. County-level data on the cash rental rate ($/acre) for irrigated and nonirrigated cropland are obtained from National Agricultural Statistics Service (NASS) survey data. We construct the average cash rental rate as irrigated rent times the share of cropland irrigated plus nonirrigated rent times the share of cropland nonirrigated. The share of cropland irrigated for each county is the ratio of harvested irrigated cropland to total cropland in 2012 obtained from the Census of Agriculture.[19] In some cases, we only have data on irrigated or nonirrigated rental rates. Often this occurs because a large majority of the cropland is either irrigated or nonirrigated. We use the nonirrigated rental rate as the county average when less than 10% of the county is irrigated and use the irrigated rental rate when more than 75% of the county is irrigated.

Data on direct payments and base acres enrolled in farm programs are obtained from the Farm Program Atlas from U.S. Department of Agriculture (2012). For our key explanatory variable, we construct direct payments per cropland acre as total direct payments in 2009 divided by total cropland acres in 2012. Since we divide total direct payments by total cropland acres, it represents the average across irrigated and nonirrigated land and represents the average across subsidized and unsubsidized land.[20] The proportion of county cropland that has base acres of Southern crops is calculated as the direct payment cotton and rice

[18] We also exclude small areas in Maryland and Delaware since they are not contiguous with the rest of our study region.

[19] In many cases the Census does not report irrigated acreage in a county because it could risk disclosing an individual respondent's data. If irrigated acreage was not reported for 2012, then we use the average irrigated acreage from 2002 and 2007. If irrigated acreage was not reported for 2002, 2007, or 2012 then we assume zero irrigated acres.

[20] The Farm Program Atlas did not separately report direct payments on irrigated and nonirrigated land.

base acres plus 1999–2001 average peanut planted acres divided by total cropland acres. The Farm Program Atlas reports base acres for cotton and rice, but not peanuts. The 2002 Farm Bill eliminated the peanut quota program and peanuts were added as a commodity to receive direct payments where base acres were established by average planted acres in the period 1999–2001 (Brown, Lamb, and Marra 2002). Base acres enrolled in the ACRE program in 2009 are also obtained from the Farm Program Atlas in order to calculate the proportion of cropland enrolled in ACRE.[21]

We use the following equation to calculate the expected market returns at the county level:

$$(7) \quad MktReturns_i = (1-\phi_i)\sum_c \frac{acres_{ci}}{\sum_c acres_{ci}}\left[\frac{1}{5}\sum_{t=2008}^{2012}(Revenue_{cit} - Cost_{crt})\right],$$

where $MktReturns_i$ is the average expected market returns for county $i$, $\phi_i$ is the proportion of cropland in summer fallow in county $i$, $Revenue_{cit}$ is the expected market revenue for crop $c$ in county $i$ in year $t$, $Cost_{crt}$ is the cost of production for crop $c$ in ERS farm resource region $r$ in year $t$, and $acres_{ci}$ are the average acres planted to crop $c$ in county $i$. The crops considered for calculating expected market returns are corn, cotton, peanuts, rice, soybeans, sorghum, spring wheat, and winter wheat. We use average expected market returns over the past 5 years—but including 2012—to approximate the market returns relevant for setting cash rental rates in 2012. An alternative would be to calculate a measure of expected market returns for 2012 only; however, we expect that cash rents are fairly sticky and do not fully adjust each year in response to different prices so market returns in previous years affect the current cash rental rate.

[21]We do not have data on ACRE enrollment in later years at the county level but do have enrollment at the state level from the Farm Service Agency (FSA). In our study region, base acres enrolled in ACRE only increased by 5% from 2009 to 2012. Enrollment in the ACRE program changed little over time since farmers had to make a one-time enrollment decision for the life of the 2008 Farm Bill.

For all crops, except cotton, expected market revenue is calculated as $Revenue_{cit} = Price_{cst} \times Yield_{cit}$, where $Price_{cst}$ is the price for crop $c$ in state $s$ in year $t$ and $Yield_{cit}$ is the trend yield for crop $c$ in county $i$ in year $t$. State-level marketing-year prices are obtained from NASS for 2008–2011. For the state-level price in 2012 we used the projected price used for revenue insurance by the Risk Management Agency plus the average state-level basis for the previous 3 years.[22] If the state-level price for a crop is missing in a particular year, it is replaced by the average price in all states with data in that year. The trend yield is estimated from county-specific linear trend regressions using data from 1980 to 2012. Yield data reflect total production, so they represent average yields across irrigated and nonirrigated cropland. We only estimate trend yield if there are 20 or more observations for a county and if there was at least one yield observation from 2007 to 2012.

We use the projected price in 2012 rather than the actual price received since farmers did not know the price they would receive at the time rents were negotiated for 2012. We use trend yields rather than observed yields because cash rents depend on expected market returns and average realized market returns in the five-year period could deviate substantially from expected market returns if weather was especially good or poor. In particular, there was a major drought in 2012 that decreased yields and increased prices. The drought was not known at the time rents were negotiated for 2012 and our calculation of expected market returns does not reflect the effect of the drought since we use projected price and trend yield for 2012.

For cotton, expected market revenue includes revenue from cotton lint and cottonseed production. The revenue from cotton lint production is calculated the same as for other crops. Cottonseed prices are also state-level prices. NASS does not, however, report county-

[22]The projected price used by the Risk Management Agency represents an average futures price before planting. We calculate the basis for each state by calculating the difference between the monthly average futures price and monthly state-level cash price from NASS. Since there is no futures market for peanuts and Risk Management Agency did not publish a projected price in our data period, we use the state-level price from NASS in March (i.e., prior to planting). The average difference (2004–2014) between the March price and the marketing year average price was negligible so the March price gives a reasonable prediction of prices after harvest given opportunities for arbitrage through storage.

level cottonseed production. We assume cottonseed yield is 1.62 times the cotton lint trend yield based on data from U.S. Department of Agriculture (2014).[23]

For all crops, production expenses are obtained by farm resource region from U.S. Department of Agriculture (2014). We include all operating costs and allocated overhead but exclude the opportunity cost of land (i.e., land rent). U.S. Department of Agriculture (2014) provides cost estimates for the following regions for each commodity: soybeans in all regions, corn in the Heartland, Prairie Gateway, Northern Great Plains, and Southern Seaboard; wheat and sorghum in the Heartland, Prairie Gateway, and Northern Great Plains; cotton in the Heartland, Prairie Gateway, Mississippi Portal, and Southern Seaboard; rice in the Mississippi Portal; and peanuts in the Prairie Gateway and Southern Seaboard.[24] For corn, wheat, and sorghum expenses in the Mississippi Portal, we use expenses from the Heartland. For wheat and sorghum expenses in the Southern Seaboard, we use expenses from the Heartland. For rice expenses in the Heartland and Southern Seaboard, we use expenses from the Mississippi Portal.[25] Using expenses from neighboring regions ensures that we have cost estimates in every county where we have trend yield and acreage data for a commodity.

Alternatively, we could estimate expenses using county level data from the Census of Agriculture similar to the approach taken by Goodwin, Mishra, and Ortalo-Magné (2011). One problem with using Census data is that the Census does not differentiate expenses for crop production. For example, expenses for machinery rent and utilities also account for expenses for livestock production. Therefore, expenses from the Census will be systematically biased estimates of crop production expenses depending on the amount of livestock production in the county.

[23] The ratio of cottonseed yield to cotton lint yield is equal to 1.62 for every year between 2007 and 2012 in the Prairie Gateway and Mississippi Portal.

[24] ERS only provides cost estimates up to 2010 for sorghum in the Heartland. We calculate the average ratio of sorghum costs from 2003 to 2010 between the Prairie Gateway and Heartland to impute costs in the Heartland for 2011 and 2012. From 2003 to 2010, costs ranged 8–15% larger in the Heartland. On average, costs are 10% larger in the Heartland for sorghum.

[25] There are only a few counties in the southern portion of the Heartland region where rice is produced.

In equation (7), we average market returns across crops where we weight by the share of acreage planted to each crop ($\frac{acres_{ci}}{\sum_c acres_{ci}}$). The acres planted to the crop is the 2008 to 2012 average planted acreage. If acreage data are missing for a particular crop in all years, then we assume the crop is not produced in the county. If acreage data are available but trend yield is not available for the crop, then we set acreage for that crop equal to zero.

Equation (7) assumes that the market returns from summer fallowed land are zero. We obtain 2012 acres in summer fallow from the Census of Agriculture and divide it by cropland acres to calculate $\phi_i$. Annual data do not exist at the county level for summer fallow acreage so $\phi_i$ is constant over time.

Our calculation of market returns does not include any expected returns from crop insurance indemnities in excess of premiums. We include a robustness check where we add average premium subsidies to the expected market returns. However, we omit premium subsidies in our main specification since it is not clear that farmers perceive the full premium subsidy as an expected net benefit since crop insurance demand was historically low with smaller subsidies (Glauber 2004).

We drop observations from our sample if we have estimates of market returns from less than 25% of total cropland. Counties that are dropped are likely those counties where other crops comprise a major portion of cropland area and our measure of market returns may not be representative for these counties or where crop acreages are small so that NASS rarely reports yield data. In the sample used for econometric analysis, expected market returns accounts for more than 50% of cropland area for 86% of counties. We also drop 12 counties where fruit and nut sales comprise greater than 10% of total crop sales according to the Census and drop 3 counties in Louisiana where sugarcane is greater than 10% of crop acres.[26]

[26] We drop these counties because they are in the South and we omit a high value commodity from our calculation of market returns. Including these counties would bias our estimate of the effect of direct payments upward.

### Data Visualization and Wald Estimates

Figure 2 shows a scatterplot of the data used in our econometric analysis for the relationship between market returns and the average cash rental rate. Purple circles indicate counties with less than 1% Southern crop base and orange diamonds indicate counties with greater than 1% Southern crop base. The data contain few counties with Southern crop base acres that have market returns greater than \$200/acre but many counties with no Southern crop base. Therefore, it is important to make comparisons conditional on the same market returns rather than aggregate summary statistics.

The most important observation from figure 2 is that conditional on the same market returns, counties with Southern crop base acres tend to have higher rental rates. Furthermore, from figure 3, we see that conditional on the same market returns, counties with Southern crop base acres tend to have much larger direct payments. Table A1 in the supplementary appendix provides summary statistics for all variables used in the regression and figure A1 shows maps of the key variables.

Next, we calculate Wald estimates of the effect of direct payments on rental rates that follow the logic of the previous paragraph (e.g., see Angrist 1990). The Wald estimates provide an intuitive and transparent estimate of the incidence from our data. Wald estimates calculate how much larger the rental rates are in counties with Southern crop base acres divided by how much large the direct payments are in counties with Southern crop base acres for those counties that have similar expected market returns:

$$(8) \quad \beta_D^{Wald} = \frac{\overline{Rent}^{SB} - \overline{Rent}^{0}}{\overline{DirectPmts}^{SB} - \overline{DirectPmts}^{0}},$$

where $\overline{Rent}$ is the average rent, the superscript $SB$ denotes the average for counties with greater than 1% of cropland with Southern crop base acres, and 0 denotes the average for counties with less than 1% of cropland with Southern crop base acres. The Wald estimate is identical to 2SLS with no controls when the instrument is a binary variable.

Table 1 reports the numerator (column 1), denominator (column 2), and Wald estimate (column 3) from equation (8) conditional on market returns within different intervals. We only report estimates for market returns between -\$50/acre and \$200/acre because there are few observations with no Southern crop base below -\$50/acre and few observations with Southern crop base above \$200/acre (see figure 2). Rent and direct payments tend to be higher in counties with Southern crop base giving positive estimates—in most cases—of the effect of direct payments on rental rates. On average, the Wald estimates indicate that \$0.89 of every dollar of direct payments are reflected in rental rates.

## Econometric Results

The observations in the previous section provide suggestive evidence that direct payments are mostly captured in rental rates. In this section, we show econometric results that pool the data to improve the precision of the estimates, control for the proportion of land enrolled in ACRE, and use the proportion of land with Southern crop base as an instrument rather than a simple binary variable. We first show OLS results which we argue are likely biased, then we show our preferred 2SLS results and robustness checks.

### *OLS Results*

Table 2 reports OLS results for the effect of direct payments on rental rates. The coefficient in column (1) is from a simple bivariate regression and shows that OLS is biased upwards substantially when controls for market returns and ACRE enrollment are omitted—in theory, the coefficient on direct payments should not exceed one. The coefficient on direct payments in the bivariate regression reflects the impact of subsidies and market returns on rental rates where cash rental rates are larger than direct payments per acre and direct payments are positively correlated with market returns.[27] This illustrates the importance of controlling for market returns.

Results in column (2) of table 2 control for market returns and ACRE enrollment and use data from the entire sample. The coefficient on direct payments indicates that cash rents increase by \$0.55 for every dollar of direct payments. We reject the null hypotheses of $\beta_D = 0$ and $\beta_D = 1$ at the 5% level. The $R^2$ indicates that our regression is able to explain roughly 82% of the variation in cash rents.

As a point of comparison, results in column (3) of table 2 show regression results using data from only those counties with negligible Southern crop base acreage and the coefficient on direct payments is much larger. The standard error is also large because there is little variation in direct payments independent of the variation in market returns. Results in column (4) use only counties with greater than 1% Southern crop base acreage. In this case, OLS exploits the variability in direct payments due to political favoritism since the amount of Southern crop base acreage varies across these counties.[28] Figure 3 illustrates that there are large differences in direct payments conditional on the same market returns for these counties. The estimates in column (4) are larger than those in column (2) but smaller than those in column (3).

The difference in the coefficients on direct payments in columns (2), (3), and (4) in table 2 can be explained by differences in the magnitude of bias from measurement error in the control variables.[29] Table 3 shows the coefficients on direct payments from auxiliary

[27]As a point of comparison, Kirwan and Roberts (2016) estimate bivariate regressions and the coefficients on direct payments are 0.77, -0.09, and 0.60 for soybeans, rice, and cotton.

[28]Angrist (1998) shows that regression estimates an average coefficient where more weight is given to observations with a greater variance of direct payments conditional on the controls. The variance of direct payments is greatest between counties that have different amounts of Southern crop base. Therefore, OLS in column (4) identifies the incidence of direct payments on rents primarily using the variation in direct payments due to political favoritism.

[29]The difference in the coefficient on market returns between columns (2), (3), and (4) in table 2 arises in part from a nonlinear relationship between rent and market returns. Many counties with negligible Southern base have large market returns. If we restrict the sample to counties with market returns less than \$125/acre, then the coefficient on market returns is 0.34 with negligible Southern base and 0.20 with Southern base. In our robustness checks, we estimate 2SLS with nonlinear functions of market returns and do not find any substantial impact on the estimated coefficient for direct payments.

regressions defined in equations (3) and (4). However, it is important to note that we use $MktReturns_i^*$ and $ACRE_i^*$ as the dependent variables since we cannot observe the true values.

The bias from measurement error in market returns is positive in all three subsamples since the coefficient on market returns ($\beta_R$) is positive in table 2 and $\pi_R$ in table 3 is positive. But $\pi_R$ is roughly 4 times larger in the sample with negligible Southern base so the upward bias in the coefficient on direct payments is much larger for this sample. Intuitively, the large upward bias for this sample occurs because direct payments have little variation independent of market returns (e.g., due to political influence).

Comparing the coefficients on ACRE enrollment ($\beta_A$) in table 2 and $\pi_A$ in table 3 indicates that measurement error in expected ACRE payments creates downward bias in the coefficient on direct payments for the entire sample and upward bias in the two subsamples. The estimates of $\pi_A$ also indicate that the bias is smallest for counties with Southern base. Overall, the results in table 3 provide a rationale why the coefficient on direct payments is largest in table 2 with negligible Southern base. The coefficient on direct payments is likely smaller with the entire sample compared to the subsample with Southern base due to bias of measurement error in expected ACRE payments.

*2SLS Results*

The bottom section of table 4 reports key information from our first-stage regression results. Not surprisingly, the share of cropland with Southern crop base acreage has a large impact on direct payments even after controlling for market returns and ACRE enrollment. The results indicate that direct payments are roughly \$35/acre larger if all of the cropland in a county has Southern crop base acreage relative to a county with no Southern crop base acreage. This is a large difference in payments, given that the average direct payments in counties with less than 1% Southern crop base is only \$13/acre in our sample (see table A1 in the supplementary appendix).

Our first-stage F-stastic of 681 indicates no evidence of a weak instrument problem. This suggests minimal finite sample bias for instrumental variables (Staiger and Stock 1997). The strong relationship between the instrument and direct payments also means that violations of the exclusion restriction have a smaller impact on our estimate of the incidence than if we had a weak instrument.

Table 4 reports estimates of the incidence using 2SLS. Heteroskedasticity-robust standard errors are reported in parentheses under each coefficient. Standard errors that allow for a potential violation of the exclusion restriction are reported in brackets under each coefficient. We place asterisks next to the standard errors in table 4 to indicate the statistical significance for each type of standard errors.

We relax the exclusion restriction using the local-to-zero approximation proposed by Conley, Hansen, and Rossi (2012) and impose the prior distribution $\gamma \sim N(0, \delta^2)$. We assume $\gamma$ has mean zero because we do not have a prior on whether cash rents are likely to be systematically higher or lower in counties with Southern crop base acreage after accounting for direct payments, market returns, and ACRE enrollment. We assume $\delta = 5$. This assumption implies that we have 95% confidence that the value of $\gamma$ is between -9.8 and +9.8. Of course, our assumption of a normal distribution assumes that $\gamma$ is most likely close to zero.

To help interpret our assumption about $\delta$, we estimate the reduced form equation that regresses $Rent_i$ on $SouthBase_i$, $MktReturns_i$, and $ACRE_i$. The exclusion restriction imposes the assumption that the effect of $SouthBase_i$ in this regression is only due to differences in direct payments. The coefficient on $SouthBase_i$ in the reduced form equation is 27.84.[30] Therefore, the average county with Southern crop base acres ($\overline{SouthBase} = 0.38$ from table A1) has rents \$10.58 larger than a county with no Southern crop base conditional on the same market returns and ACRE enrollment. Our assumption about $\delta$ allows for the possibil-

[30] Note that the 2SLS estimate of the effect direct payments on rent is simply the coefficient on $SouthBase_i$ in the reduced form equation divided by the coefficient in the first-stage equation. Therefore, the 2SLS estimate is 0.804=27.84/34.63, which corresponds to the result in table 4.

ity that the average county with Southern crop base acres has rents \$3.72 larger or smaller ($3.72 = 9.8 \times 0.38$) than a county with no Southern crop base acres. The magnitude of this violation is fairly large relative to the estimated difference in rents of \$10.58.

Table 4 indicates that cash rents increase by \$0.80 for every dollar of direct payments. The result is smaller than OLS results in column (4) of table 2 that exploit counties with Southern crop base acres, but is larger than OLS results in column (1) that use the entire sample. The p-value for a test for endogeneity that is robust to heteroskasticity is reported near the bottom of table 4 (see Wooldridge 2010). The test rejects the null hypothesis of exogeneity at the 5% level.

The heteroskedasticity-robust standard error for the coefficient on direct payments is 0.18, only slightly larger than 0.17 from the OLS model. Accounting for a potential violation of the exclusion restriction, the standard error increases to 0.23 (standard error in brackets). With either type of standard error, we reject the null hypothesis that $\beta_D = 0$ but fail to reject the null that $\beta_D = 1$ at the 5% level.

The coefficient on market returns in table 4 indicates that cash rents increase by \$0.44 for an additional dollar of market returns. An important caveat is that our coefficient on market returns could be biased downward to the extent that we have measurement error in expected market returns. However, our coefficient is much larger than estimated by Kirwan (2009) and Hendricks, Janzen, and Dhuyvetter (2012)—0.03 and 0.11, respectively.[31] Goodwin, Mishra, and Ortalo-Magné (2011) estimate a coefficient on market returns of about 0.12–0.16 depending on their specification.[32] Our coefficient on market returns is similar to Lence and Mishra (2003). The coefficient on market returns provides some evidence that the incidence of coupled subsidies is smaller than for direct payments; however, previous literature has found a different incidence of market returns and coupled subsidies (e.g., Goodwin, Mishra,

[31] Kirwan (2009) and Hendricks, Janzen, and Dhuyvetter (2012) both include revenues and costs as separate variables. Here we cite the coefficient on revenues from these articles which is larger in absolute magnitude than the coefficient on costs in both cases.

[32] The estimate of Goodwin, Mishra, and Ortalo-Magné (2011) is likely biased downwards given that they use a historical average of actual market returns from crop and livestock production.

and Ortalo-Magné 2011; Kropp and Peckham 2015). The coefficient on market returns is similar between 2SLS and OLS. As expected, the coefficient on the proportion of cropland enrolled in ACRE indicates that cash rents are larger in counties with more land enrolled in ACRE, *ceteris paribus*. Farmers would only have enrolled in ACRE if they expected to receive some subsidy payments from the program.

*Robustness Checks*

In the supplementary appendix, we report results from several different robustness checks and describe the specifications for the robustness checks in more detail. Table A2 shows 2SLS estimates if we include flexible nonlinear functions to control for market returns instead of a linear control. The coefficient on direct payments is only slightly smaller (0.70–0.74 depending on the specification).

Table A3 shows results if we calculate the variables in our analysis differently. In the first column, we add crop insurance premium subsidies to our calculation of market returns. Assuming that crop insurance premiums are set at actuarially fair rates, then the premium subsidy represents the expected net benefits (indemnities minus farmer paid premiums). Alternatively, we could calculate an average of indemnities minus farmer paid premiums over some historical period but this can differ dramatically from expected net benefits depending on weather realizations in the period. The coefficient on direct payments when we include insurance premium subsidies is smaller at 0.70.

The second column in table A3 shows results if we use cropland used for crops (i.e., the sum of harvested, failed, and summer fallowed cropland) rather than total cropland area to derive per acre estimates. The coefficient on direct payments is 0.72. If we calculate market returns over the period 2009–2012 instead of 2008–2012, then the coefficient on direct payments is 0.57 and the coefficient on market returns is 0.42. If we use the period 2010–2012 for market returns, then the coefficient on direct payments is 0.58 and the coefficient on market returns is 0.38.

Table A4 in the supplementary appendix shows results using the rental rate from 2011 or 2010. In both cases, we only use market returns calculated from 2008 to the year of rent data because there was a large change in market returns in 2008 due to the commodity price boom. Using rental rates from 2011, the coefficient on direct payments is 0.95. Using rental rates from 2010, the coefficient on direct payments is 1.31. The coefficient on market returns from these specifications is 0.37 and 0.41. Part of the reason for the difference in the estimate when we use 2011 or 2010 rental rates is that the sample of counties with rental rate data differs. If we restrict the sample to those counties that had rental rate data in 2012, then the coefficient on direct payments is 0.79 with 2011 rental rates and 1.17 with 2010 rental rates.

Table A5 shows results if we restrict the sample to levels of market returns where there are counties that have both Southern crop base acres and those without Southern crop base acres. We restrict the sample to counties with either market returns less than \$300/acre or market returns between -\$75/acre and \$300/acre. The coefficient on direct payments in these specifications is either 0.74 and 0.57. We prefer the estimates from our main specification that exploit all available data.

Another potential concern is that varying portions of cropland represented by our market returns calculation could bias our estimates. The assumption in our main specification is that the market returns on cropland not represented in our market returns calculation are similar to the calculated market returns. This assumption could be erroneous if the remaining cropland is of relatively high or low quality and different crops are produced on that land so that our calculation of market returns do not accurately represent the remaining cropland. The direction of any potential bias is not clear. We alleviate this concern in the main specification by only considering counties with greater than 25% of cropland represented by our market returns estimate and dropping counties with substantial fruit, nut, or sugarcane production. We assess the impact on our estimates by either restricting the analysis to counties where market returns are calculated for more than 0%, 25%, or 50% of cropland

and by including the portion of cropland represented by our market returns calculation as a control variable (table A6 in the supplementary appendix).[33] The coefficient on direct payments is smallest at 0.51 when we include all counties with market returns data and control for the proportion of the county with returns data and the coefficient is largest at 0.83 when we include all counties with returns data but omit the control for the proportion of the county with returns. The coefficient on direct payments is 0.72 when we include only counties where greater than 50% of cropland is represented by our returns calculation.

In summary, the coefficient on direct payments ranges from 0.51 to 1.31 across all of the robustness checks with most estimates between 0.7 and 0.8. While the estimates differ across specifications, there is general support for the main conclusion that large, persistent differences in direct payments are mostly reflected in the rental rate. The coefficient on market returns is more stable across the robustness checks, ranging from 0.37 to 0.45. The stability of the coefficient on market returns is likely due to the large amount of variation in market returns across counties, whereas there is a smaller amount of exogenous variation in direct payments.

We argue that our assumption of $\delta = 5$ allows substantial violations of the exclusion restriction, but our assumption is admittedly arbitrary. Figure A2 in the supplementary appendix shows the 95% confidence interval for different assumed values of $\delta$. Inference only becomes uninformative when $\delta$ is greater than 12.

## Discussion and Conclusion

Our preferred estimator of the incidence of direct payments on rental rates is 2SLS (table 4) and assuming the instrument is only plausibly exogenous (standard error in brackets). This

[33] We do not include the portion of cropland represented by our market returns calculation as a control in our main specification because cropland not represented in our market returns may be of higher or lower quality depending on the region so it is not clear what the coefficient captures. It seems more straightforward to restrict the analysis to counties where a majority of cropland is represented by our estimate of market returns. The specifications that further restrict the sample in columns (4) and (5) of table A6 give similar estimates to our main specification but with larger standard errors since the sample size is smaller.

specification isolates the variability in direct payments due to political favoritism towards Southern commodities, but without strictly imposing the exclusion restriction.

Our preferred specification indicates that \$0.80 of every dollar of direct payments is captured by landowners through rental rates in the long run. Standard economic theory suggests that subsidies not tied to production should be completely reflected in rental rates ($\beta_D = 1$) and our econometric estimates are not able to reject this null hypothesis, though the evidence suggests less than full incidence on rental rates. We also estimate that about \$0.44 of every dollar of expected market returns accrues to landowners through higher rental rates in the long run.

We find a larger effect of direct payments on rental rates than most previous literature.[34] Articles that find a small incidence exploit exogenous variation in subsidies by exploiting changes between time periods or differences in field-specific subsidies. We argue that there are at least two reasons why previous estimates of a small incidence exploiting this type of variation are consistent with our estimates that exploit large, persistent differences in subsidies across regions.

First, we assume that rents depend on customary arrangements within a region and thus depend—at least in part—on average conditions within the region in addition to field-specific conditions. For example, Young and Burke (2001) find that cropshare agreements rarely vary within Northern Illinois even though soil quality conditions vary within the region. But we assume that rents across different regions reflect the difference in average conditions between regions. Therefore, differences in direct payments between fields within the same region or over time are not mostly captured in rental rates, but large differences in direct payments across regions are mostly captured in rental rates.

[34] A few other studies find a large incidence (e.g., Lence and Mishra 2003; Patton et al. 2008; Goodwin, Mishra, and Ortalo-Magné 2011), but are subject to critique that unmeasured productivity biases their coefficients upwards (Kirwan and Roberts 2016). Here we attempt to resolve those concerns by exploiting plausibly exogenous variation in subsidies due to political favoritism towards Southern commodities.

Second, rents tend towards round numbers. Figure A3 in the supplementary appendix shows the number of farms in Kansas reporting different rental rates.[35] Most of the reported rates are in \$5 increments. There are also an especially large number of farms reporting rates at \$40, \$50, and \$60 compared to \$45 and \$55. Therefore, small differences in subsidies may not be reflected in changes in rental rates.

According to the 2012 TOTAL (Tenure, Ownership, and Transition of Agricultural Land) Survey, about 46% of cropland in the United States is rented by non-operator landlords. Assuming that the incidence of direct payments is similar across different types of rental rate agreements, our estimate indicates that of the annual \$4.7 billion of direct payments in the 2008 Farm Bill, about \$1.73 billion ($1.73 = 4.7 \times 0.46 \times 0.80$) was captured by non-operator landlords.

Agriculture Risk Coverage (ARC) and Price Loss Coverage (PLC) payments are similar to direct payments, in that they are tied to base acres and base yields rather than current production. Our estimates indicate that non-operator landlords are likely to capture a large portion of ARC and PLC payments in the long run. One caveat is that ARC and PLC payments are uncertain because they depend on market prices and—for ARC—yields. Future research should explore the impact of payment uncertainty on the incidence of subsidies.

[35] The data are from a survey conducted as part of grant project not directly related to this paper funded by the National Science Foundation under Award No. EPS-0903806.

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## Tables

### Table 1: Wald Estimates

| | (1) Diff. Rent | (2) Diff. Direct Pmts | (3) Wald Estimate |
|---|---|---|---|
| $-50 \leq$ Returns $< -25$ | 10.577** | 8.044** | 1.315** |
| | (4.574) | (1.009) | (0.479) |
| $-25 \leq$ Returns $< 0$ | 13.961** | 8.238** | 1.695** |
| | (7.004) | (2.138) | (0.761) |
| $0 \leq$ Returns $< 25$ | 7.569 | 3.852** | 1.965* |
| | (5.796) | (1.354) | (1.068) |
| $25 \leq$ Returns $< 50$ | 25.359** | 9.235** | 2.746** |
| | (7.677) | (1.982) | (0.614) |
| $50 \leq$ Returns $< 75$ | 16.695** | 7.131** | 2.341** |
| | (7.544) | (1.624) | (0.781) |
| $75 \leq$ Returns $< 100$ | 10.403 | 17.036** | 0.611 |
| | (7.401) | (2.226) | (0.439) |
| $100 \leq$ Returns $< 125$ | 12.362* | 13.242** | 0.934** |
| | (6.633) | (2.441) | (0.472) |
| $125 \leq$ Returns $< 150$ | -9.577 | 23.186** | -0.413 |
| | (6.712) | (3.024) | (0.263) |
| $150 \leq$ Returns $< 175$ | -26.846* | 20.256** | -1.325 |
| | (14.209) | (8.199) | (1.098) |
| $175 \leq$ Returns $< 200$ | -21.158** | 21.357** | -0.991** |
| | (7.102) | (2.905) | (0.396) |
| Average | | | 0.888** |
| | | | (0.219) |

Standard errors in parentheses represent heteroskedasticity-robust standard errors. Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

**Table 2: OLS Results for the Incidence of Direct Payments on Cash Rental Rates**

| | (1) Bivariate | (2) Entire Sample | (3) Less than 1% Southern Base | (4) Greater than 1% Southern Base |
|---|---|---|---|---|
| Direct Payments | 3.089** | 0.552** | 1.481** | 0.998** |
| | (0.400) | (0.174) | (0.491) | (0.232) |
| Market Returns | | 0.444** | 0.458** | 0.175** |
| | | (0.010) | (0.020) | (0.023) |
| Proportion ACRE | | 50.293** | 36.230** | -54.400** |
| | | (9.095) | (8.605) | (14.094) |
| Intercept | 71.750** | 28.103** | 14.733** | 36.131** |
| | (5.648) | (2.169) | (3.444) | (4.188) |
| Observations | 971 | 971 | 746 | 225 |
| $R^2$ | 0.117 | 0.820 | 0.843 | 0.556 |

Standard errors in parentheses represent heteroskedasticity-robust standard errors. Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

**Table 3: Coefficients on Direct Payments in Auxiliary Regressions to Demonstrate Magnitude of Bias from Measurement Error in Control Variables**

| Dependent Variable | (1)<br>Entire Sample | (2)<br>Less than 1%<br>Southern Base | (3)<br>Greater than 1%<br>Southern Base |
|---|---|---|---|
| Market Returns ($\pi_R$) | 5.977** | 20.049** | 4.542** |
| | (0.480) | (0.547) | (0.484) |
| Proportion ACRE ($\pi_A$) | -0.003** | 0.005** | -0.001** |
| | (0.0005) | (0.0014) | (0.0005) |

Standard errors in parentheses represent heteroskedasticity-robust standard errors.
Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

**Table 4: Two-Stage Least Squares Results for the Incidence of Direct Payments on Cash Rental Rates**

| | |
|---|---|
| Direct Payments | 0.804 |
| | (0.183)** |
| | [0.233]** |
| Market Returns | 0.438 |
| | (0.010)** |
| | [0.011]** |
| Proportion ACRE | 53.253 |
| | (9.003)** |
| | [9.015]** |
| Intercept | 25.014 |
| | (2.097)** |
| | [2.254]** |
| Coefficient on Proportion Southern Base in First-Stage | 34.632 |
| First Stage F-Statistic ($H_0 : \alpha_{SB} = 0$) | 681.23 |
| P-value for test of endogeneity ($H_0$=exogeneity) | 0.017 |
| Observations | 971 |

Standard errors in parentheses represent heteroskedasticity-robust standard errors imposing the exclusion restriction. Standard errors in brackets are calculated allowing for a potential violation of the exclusion restriction (Conley, Hansen, and Rossi 2012). The test for endogeneity is conducted using the results that impose the exclusion restriction and the test is robust to heteroskedasticty.
Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

## Figures

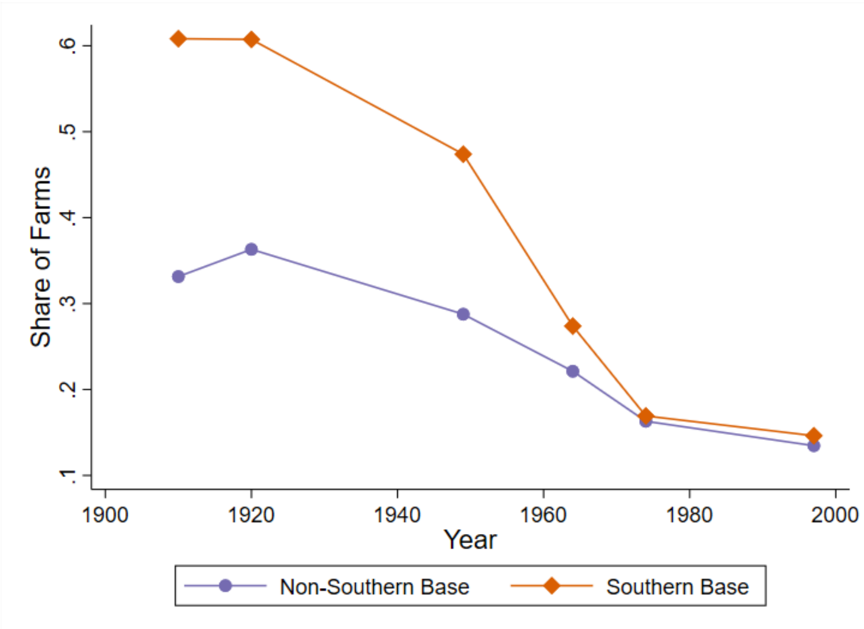



**Figure 1: Tenancy over Time by Region**

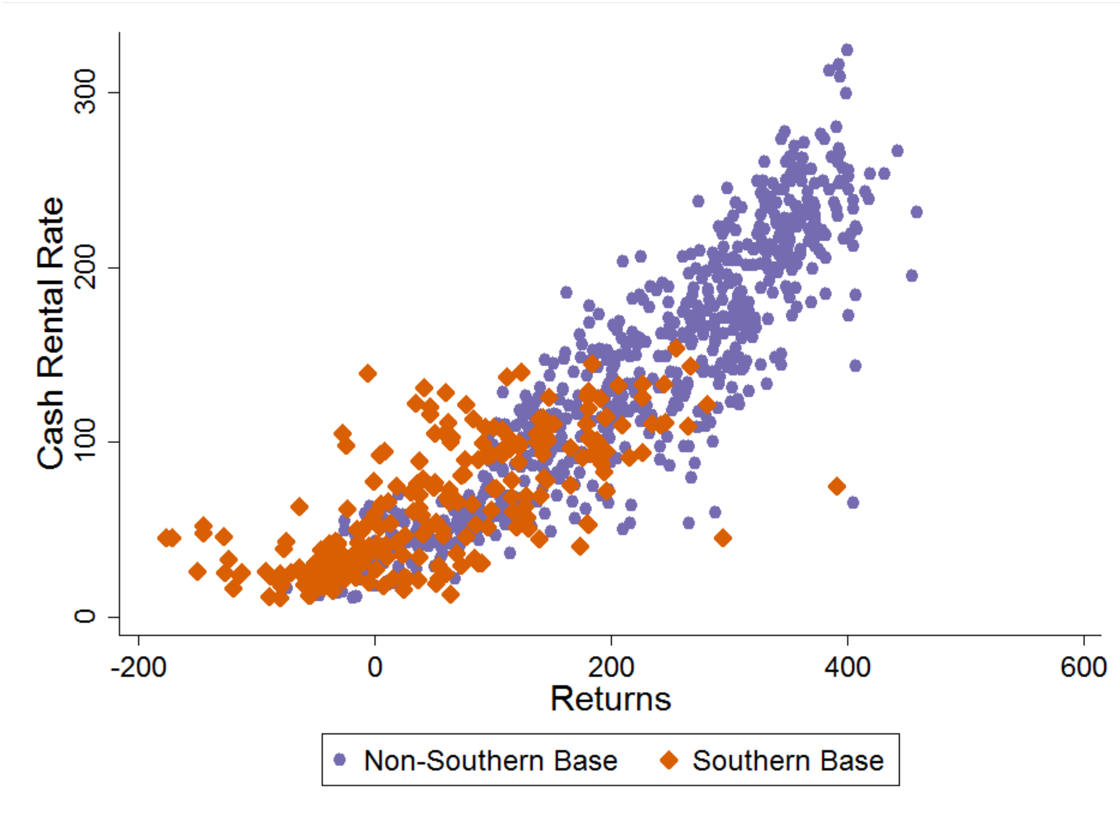



**Figure 2: Rents and Returns**

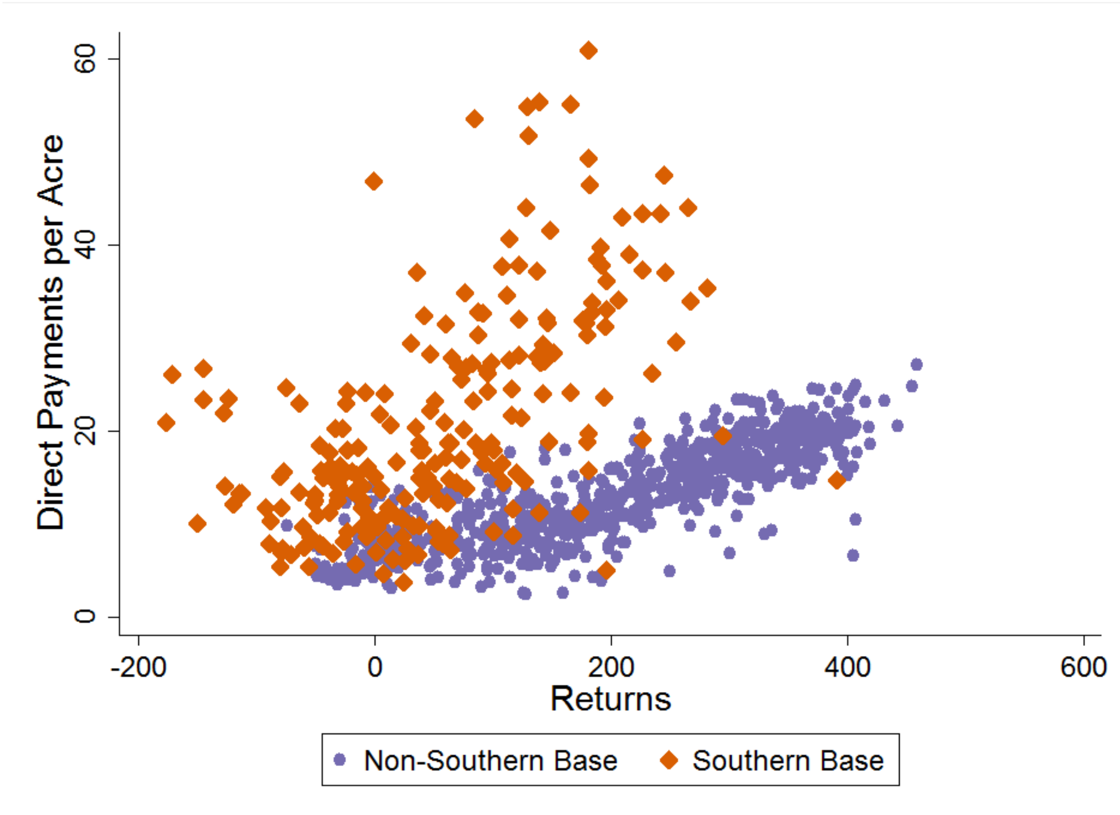



**Figure 3: Direct Payments and Returns**

# Online Supplementary Appendix to "Agricultural Subsidy Incidence: Evidence from Political Favoritism"

Nathan P. Hendricks and Krishna P. Pokharel

**Additional Tables and Figures**

Table A1 shows summary statistics for the variables used in our econometric analysis. Panel A shows summary statistics for counties with less than 1% of cropland with Southern crop base acres (746 counties) and panel B for counties with greater than 1% of cropland with Southern crop base acres (225 counties). The mean value for direct payments for the counties with negligible Southern crop base ($13.28) is lower than for those counties with Southern crop base ($20.91). The mean values for cash rent and market returns are higher in counties with negligible Southern crop base acreage. Enrollment in the ACRE program was greater in counties with negligible Southern crop base acreage. Among those counties with Southern crop base, the proportion of cropland with Southern crop base acres differs substantially among counties with a mean of 0.38 and a standard deviation of 0.24.[36]

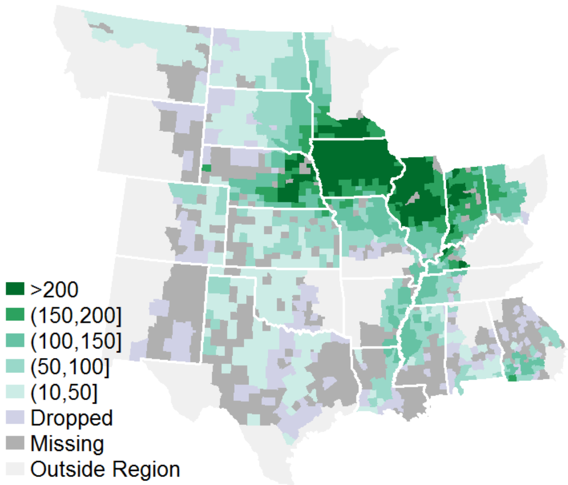
Figure A1 shows maps for cash rent, market returns, direct payments, and the proportion of cropland with Southern crop base acres. The light grey area shows those counties that are not included in one of the four farm resource regions included in our sample. The dark grey area shows those counties that had missing data for one of the variables used in the econometric analysis. Missing data usually occurred because county-level cash rent was not reported or market returns could not be calculated because trend yield or acreage data were missing. The light blue area shows those counties that were dropped from our analysis because market returns were calculated for less than 25% of cropland area or the county had significant fruit, nut, or sugarcane production.

[36]Southern crop base acres exceeded cropland acreage in one county. This may have occurred if cropland area decreased from the time base was established.

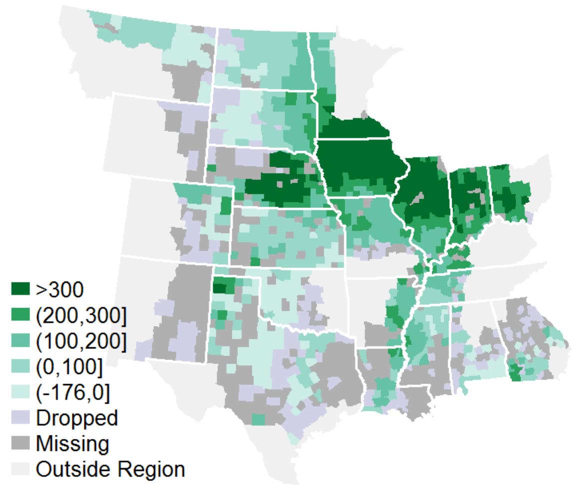
High cash rental rates are concentrated in the Corn Belt, Mississippi Portal, and Southeast and rental rates are smaller moving west to the plains states (figure A1a). Returns generally follow a similar pattern as the cash rental rate (figure A1b). Direct payments, however, are much larger in the Mississippi Portal region, the Southeast, and portions of Texas compared to the Northern regions (figure A1c). The larger direct payments are directly related with the proportion of cropland with Southern crop base acres (figure A1d).

**Table A1: Summary Statistics**

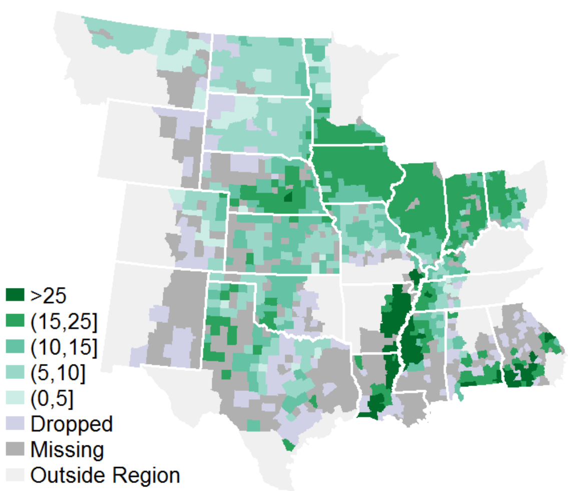
| | Observations | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| **Panel A. Counties with Less than 1% Southern Base** | | | | | |
| Cash Rent ($/acre) | 746 | 134.05 | 72.47 | 10.50 | 324.00 |
| Direct Payments ($/acre) | 746 | 13.28 | 5.16 | 2.36 | 27.07 |
| Market Returns ($/acre) | 746 | 207.42 | 127.91 | -73.65 | 458.48 |
| Proportion ACRE | 746 | 0.13 | 0.12 | 0.00 | 0.62 |
| **Panel B. Counties with Greater than 1% Southern Base** | | | | | |
| Cash Rent ($/acre) | 225 | 65.81 | 36.72 | 10.50 | 153.55 |
| Direct Payments ($/acre) | 225 | 20.91 | 11.87 | 3.68 | 60.88 |
| Market Returns ($/acre) | 225 | 57.11 | 100.64 | -176.08 | 391.39 |
| Proportion ACRE | 225 | 0.02 | 0.07 | 0.00 | 0.45 |
| Proportion Southern Base | 225 | 0.38 | 0.24 | 0.01 | 1.10 |



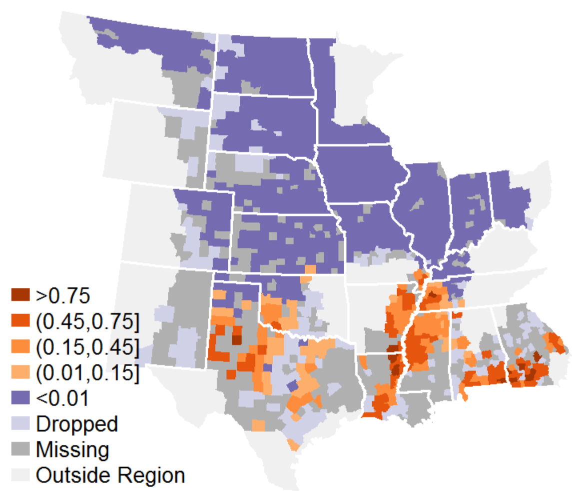

**(a)** Cash Rent




**(b)** Returns




**(c)** Direct Payments




**(d)** Proportion Southern Crop Base Acres

**Figure A1: Maps of Key Variables**

### Additional Information about Robustness Checks

Table A2 is the same as the main results in table 4 except that we control for market returns using flexible nonlinear functions. We use restricted (or natural) cubic splines to model flexible functions for market returns that do not have erratic behavior near the extremes of the data. Column (1) in table A2 uses 3 spline knots, column (2) uses 4 spline knots, and column (5) uses 5 spline knots. We do not report coefficient estimates on the market return variables in these specifications because they cannot be interpreted. These results also include the proportion of cropland enrolled in ACRE as a control, but we omit the coefficient from the table for conciseness.

Table A3 shows 2SLS results when we use different methods of constructing variables used in the econometric analysis. Column (1) calculates returns as in equation (7) but adds insurance premium subsidies. We calculate premium subsidies as the total crop insurance premium subsidies divided by total cropland acres. Data on premium subsidies at the county level are obtained from the Risk Management Agency (RMA). We divide by total cropland acres rather than insured acres to get an average for the county since not all acres are insured. Column (2) shows results when we use cropland used for crops rather than total cropland area to calculate variables. Cropland used for crops is calculated from the Census of Agriculture and is the sum of harvested, failed, and summer fallowed cropland. Cropland used for crops is then used (i) to calculate the share of cropland irrigated that affects the average rental rate, (ii) to calculate direct payments per acre, (iii) to calculate the share of cropland fallowed, (iv) to calculate the share of cropland with Southern crop base acres, and (v) to calculate the share of cropland enrolled in the ACRE program. The problem with using cropland used for crops is that one of the three components is often not reported at the county-level to avoid disclosing individual information. Therefore, we have to assume zero failed or zero summer fallowed acres when it is not reported, but this may underestimate cropland area used for crops. In several cases, total base acres greatly exceeds acreage of cropland used for crops which does not seem likely. Therefore, we drop counties where total base acres are more than 1.5 times as large as acreage of cropland used for crops. Columns (3) and (4) in table A3 use different periods to calculate the market returns. In the main paper, we average expected market returns over the 5-year period of 2008–2012. In column (3) we average expected market returns over the 4-year period of 2009–2012 and in column (4) we average over the period of 2010–2012.

Table A4 reports results if we use the rental rate from different years. In the main paper, we use rental rates from 2012. In column (1) we use rental rates in 2011 and market returns from the period 2008-2011. In column (2) we use rental rates in 2010 and market returns from the period 2008-2010.

Table A5 shows 2SLS results when we restrict our sample with market returns in certain ranges. Column (1) only considers counties with market returns less than $300/acre. Column (1) only eliminates one counties with more than 1% Southern crop base acreage (and this county only had 1.8% Southern crop base) but 223 counties with little Southern crop base (see figure 2 in the paper). Column (2) only considers counties with market returns between -$75/acre and $300/acre. Figure 2 in the paper shows that only counties with greater than 1% Southern crop base acres have market returns less than -$75/acre.

Table A6 considers the robustness of results if we use alternative approaches to account for the fact that our calculation of market returns does not include market returns from all cropland in the county. In column (1), we do not drop counties where less than 25% of cropland is included in our calculation of market returns. Results in column (2) are the same as column (1) except that we add the proportion of the county represented by our market returns calculation as an additional control variable. This control variable is the sum of planted acres across all crops used to calculate market returns divided by total cropland acres. Column (3) is the same as our main specification in the paper but we add the control variable. Results in column (4) restrict the sample to those counties where the crop acreage used to calculate the market returns is at least half as large as total cropland area for the county. This reduces the number of observations to 837 versus 971 in the main specification. Results in column (5) are the same as column (4) but add the control variable.

**Table A2: 2SLS Estimates with Nonlinear Functions of Market Returns**

| | (1) | (2) | (3) |
|---|---|---|---|
| Direct Payments | 0.742 | 0.738 | 0.704 |
| | (0.158)** | (0.159)** | (0.165)** |
| | [0.214]** | [0.215]** | [0.219]** |
| P-value for test of endogeneity ($H_0$=exogeneity) | 0.009 | 0.008 | 0.012 |
| Observations | 971 | 971 | 971 |
| Spline Knots | 3 | 4 | 5 |

Standard errors in parentheses represent heteroskedasticity-robust standard errors imposing the exclusion restriction. Standard errors in brackets are calculated allowing for a potential violation of the exclusion restriction (Conley, Hansen, and Rossi 2012). The test for endogeneity is conducted using the results that impose the exclusion restriction and the test is robust to heteroskedasticty.
Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

**Table A3: 2SLS Estimates with Different Methods of Constructing Variables**

| | (1) Insurance in Returns | (2) Cropland Used for Crops | (3) Returns 2009-2012 | (4) Returns 2010-2012 |
|---|---|---|---|---|
| Direct Payments | 0.703 | 0.718 | 0.573 | 0.577 |
| | (0.181)** | (0.178)** | (0.178)** | (0.171)** |
| | [0.232]** | [0.229]** | [0.231]** | [0.226]** |
| Market Returns | 0.435 | 0.450 | 0.421 | 0.380 |
| | (0.010)** | (0.010)** | (0.009)** | (0.008)** |
| | [0.011]** | [0.011]** | [0.010]** | [0.009]** |
| Proportion ACRE | 49.844 | 43.863 | 53.563 | 51.812 |
| | (9.043)** | (7.890)** | (8.767)** | (8.758)** |
| | [9.053]** | [7.900]** | [8.782]** | [8.774]** |
| Intercept | 20.223 | 24.230 | 24.607 | 18.202 |
| | (2.058)** | (2.329)** | (2.078)** | (2.064)** |
| | [2.199]** | [2.547]** | [2.243]** | [2.212]** |
| Observations | 971 | 959 | 971 | 967 |

Standard errors in parentheses represent heteroskedasticity-robust standard errors imposing the exclusion restriction. Standard errors in brackets are calculated allowing for a potential violation of the exclusion restriction (Conley, Hansen, and Rossi 2012).
Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

**Table A4: 2SLS Estimates with Rental Rates from Different Years**

| | (1)<br>2011 | (2)<br>2010 |
|---|---|---|
| Direct Payments | 0.951 | 1.310 |
| | (0.156)** | (0.187)** |
| | [0.220]** | [0.241]** |
| Market Returns | 0.374 | 0.410 |
| | (0.008)** | (0.011)** |
| | [0.010]** | [0.013]** |
| Proportion ACRE | 43.764 | 33.417 |
| | (7.179)** | (7.374)** |
| | [7.195]** | [7.393]** |
| Intercept | 25.800 | 39.034 |
| | (1.770)** | (2.149)** |
| | [2.005]** | [2.435]** |
| Observations | 977 | 1004 |

Standard errors in parentheses represent heteroskedasticity-robust standard errors imposing the exclusion restriction. Standard errors in brackets are calculated allowing for a potential violation of the exclusion restriction (Conley, Hansen, and Rossi 2012).
Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

**Table A5: 2SLS Estimates with Different Samples**

| | (1) Returns < \$300/acre | (2) -\$75/acre < Returns < \$300/acre |
|---|---|---|
| Direct Payments | 0.740 | 0.568 |
| | (0.165)** | (0.165)** |
| | [0.220]** | [0.219]** |
| Market Returns | 0.384 | 0.408 |
| | (0.012)** | (0.012)** |
| | [0.013]** | [0.013]** |
| Proportion ACRE | 37.554 | 36.989 |
| | (8.669)** | (8.359)** |
| | [8.672]** | [8.362]** |
| Intercept | 30.720 | 29.357 |
| | (1.970)** | (1.914)** |
| | [2.148]** | [2.100]** |
| Observations | 747 | 727 |

Standard errors in parentheses represent heteroskedasticity-robust standard errors imposing the exclusion restriction. Standard errors in brackets are calculated allowing for a potential violation of the exclusion restriction (Conley, Hansen, and Rossi 2012). Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

**Table A6: 2SLS Estimates that Account for the Portion of Cropland Not Represented in Calculation of Returns**

| | Proportion of Cropland Represented by Returns Calculation | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | > 0% | > 0% | > 25% | > 50% | > 50% |
| Direct Payments | 0.827 | 0.512 | 0.642 | 0.722 | 0.729 |
| | (0.172)** | (0.201)** | (0.217)** | (0.200)** | (0.239)** |
| | [0.205]** | [0.255]** | [0.270]** | [0.250]** | [0.292]** |
| Market Returns | 0.426 | 0.406 | 0.428 | 0.447 | 0.447 |
| | (0.010)** | (0.012)** | (0.011)** | (0.011)** | (0.010)** |
| | [0.011]** | [0.012]** | [0.012]** | [0.012]** | [0.011]** |
| Proportion ACRE | 63.622 | 53.752 | 49.368 | 47.660 | 47.774 |
| | (9.618)** | (9.250)** | (8.933)** | (9.140)** | (9.188)** |
| | [9.636]** | [9.252]** | [8.933]** | [9.142]** | [9.188]** |
| Proportion with Returns | | 26.543 | 16.538 | | -1.023 |
| | | (6.599)** | (7.300)** | | (9.234) |
| | | [6.756]** | [7.497]** | | [9.500] |
| Intercept | 24.960 | 14.209 | 16.689 | 26.297 | 26.941 |
| | (1.877)** | (3.026)** | (3.755)** | (2.615)** | (5.773)** |
| | [1.969]** | [3.030]** | [3.755]** | [2.782]** | [5.790]** |
| Observations | 1053 | 1053 | 971 | 837 | 837 |

Standard errors in parentheses represent heteroskedasticity-robust standard errors imposing the exclusion restriction. Standard errors in brackets are calculated allowing for a potential violation of the exclusion restriction (Conley, Hansen, and Rossi 2012). Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

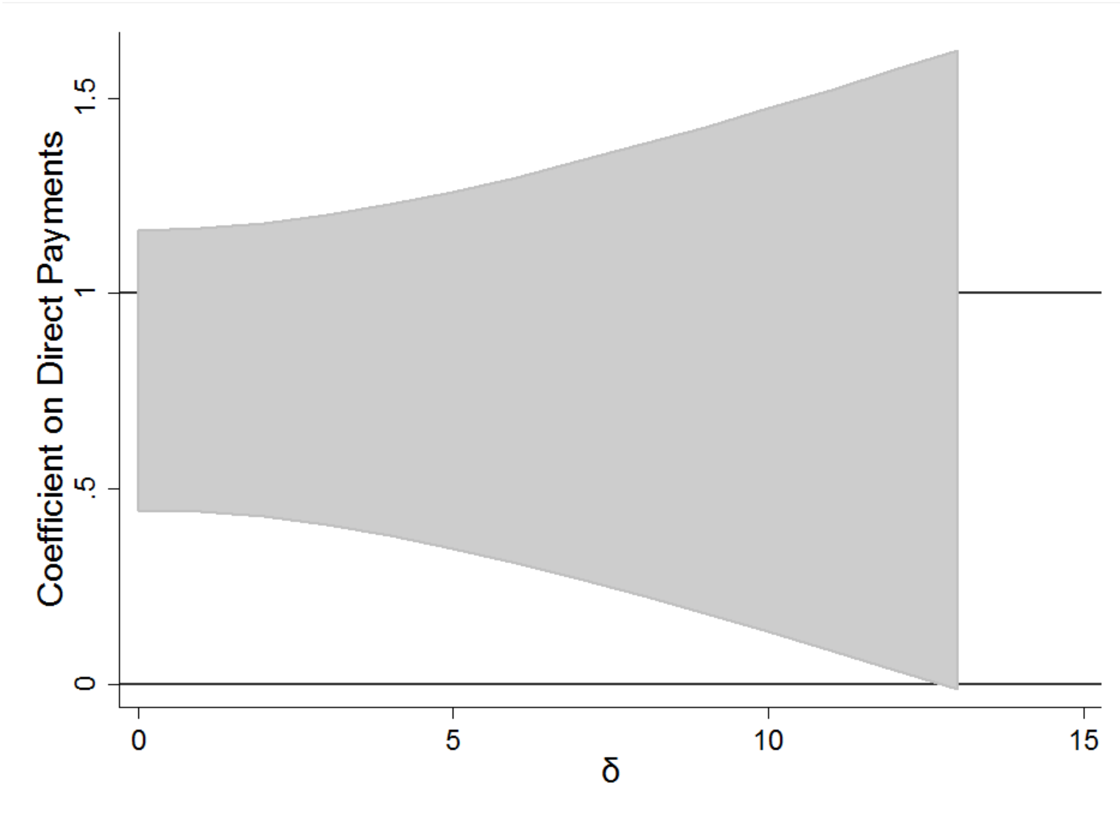



**Figure A2: 95% Confidence Interval for Alternative Assumptions about $\delta$**

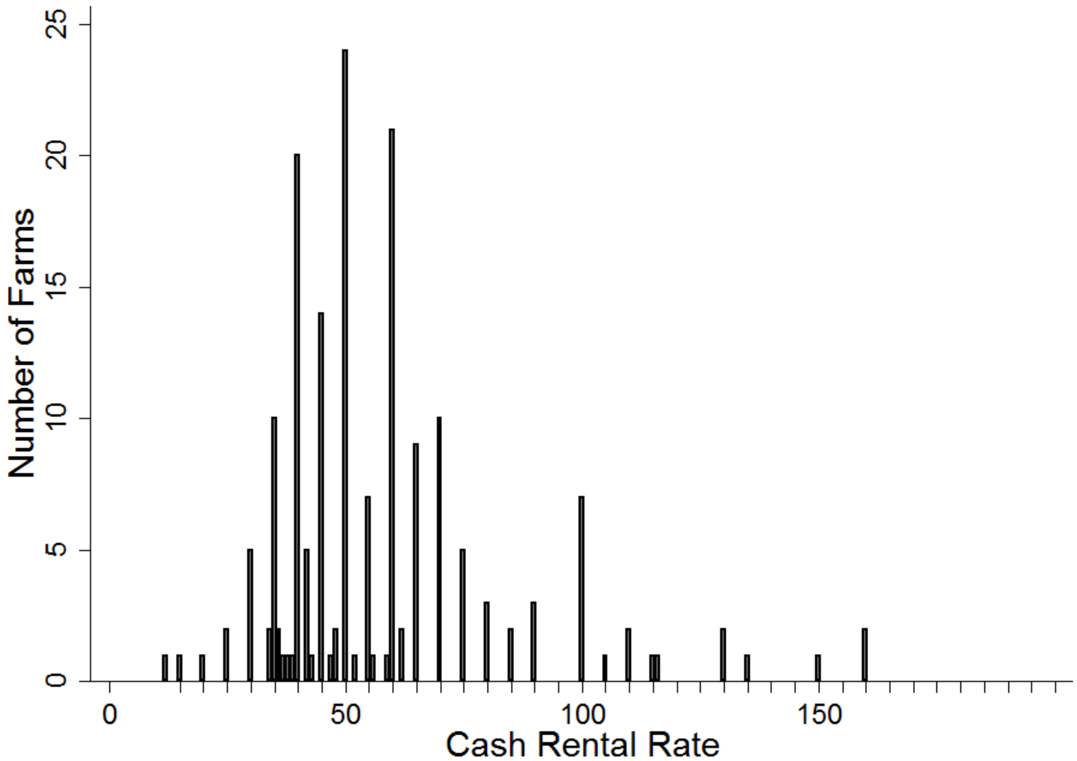



**Figure A3: Number of Farms in Kansas Reporting Different Cash Rental Rates**

Notes: Bin size is $1/acre. Ticks on the x-axis are in $5 increments.