

Agricultural Subsidy Incidence: Evidence from Commodity Favoritism

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Abstract

We use county-level data in the United States to estimate the incidence of direct payments on cash rental rates. Direct payments were fixed subsidies not tied to price or production—thus, standard theory suggests direct payments should be fully reflected in rents. Our econometric model exploits variability in direct payments due to variation in the proportion of cropland with cotton or rice base acres while controlling for expected market returns. Cotton and rice base acres received substantially larger direct payments, arguably because cotton and rice—historically produced in the South—are politically favored compared to commodities produced in other regions. Estimates from two-stage least squares indicate that roughly \$0.81 of every dollar of direct payments accrues to landlords through higher rental rates in the long run. We also construct revised standard errors that account for potential violations of the exclusion restriction. Most previous literature exploits changes in subsidies over time or differences in subsidies across areas producing the same set of commodities. Our estimate of the incidence of direct payments on rental rates is larger than most previous literature because we exploit large, persistent differences in subsidies.

Keywords: Incidence, agricultural subsidies, decoupled payments, rental rates.

JEL codes: Q18, H22.

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

10 Economists have long recognized that the economic incidence of government subsidies
11 differs from the initial recipient of such subsidies. Standard economic theory predicts that
12 non-operator landowners capture all of a purely decoupled subsidy but only capture a por-
13 tion of a subsidy directly tied to production (Floyd 1965; Alston and James 2002). Direct
14 payments in the United States (2002–2014) were one example of a fixed subsidy that was not
15 tied to current production or price.² There are, however, several reasons why landowners
16 may not capture the entire direct payment. First, tenants are often related to the landowner
17 (Schlegel and Tsoodle 2008), so some rental rates may not reflect the competitive rate (Perry
18 and Robison 2001; Tsoodle, Golden, and Featherstone 2006).³ Second, direct payments are
19 not purely decoupled (e.g., Hennessy 1998; Just and Kropp 2013; Hendricks and Sumner
20 2014). Third, tenants may exercise market power in the rental market (Kirwan 2009; Kir-
21 wan and Roberts Forthcoming).

22 Most studies examining the impact of government payments on rental rates find that less
23 than \$0.50 of every dollar of subsidies is captured by changes in the rental rate (Kirwan 2009;

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

²Note 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.

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

24 Breustedt and Habermann 2011; Hendricks, Janzen, and Dhuyvetter 2012; Ciaian and Kancs
25 2012; Kilian et al. 2012; Herck, Swinnen, and Vranken 2013; Michalek, Ciaian, and Kancs
26 2014; Kirwan and Roberts Forthcoming). There are a few exceptions in the literature that
27 find larger impacts on rental rates (Lence and Mishra 2003; Patton et al. 2008; Goodwin,
28 Mishra, and Ortalo-Magné 2011), but these studies are subject to concerns that unmeasured
29 variability in productivity inflates their coefficient estimates.

30 One unresolved puzzle is that previous literature usually finds a large impact of gov-
31 ernment payments on land values (Latruffe and Le Mouél 2009) even though the estimated
32 impact on rental rates is usually small. For example, Ifft, Kuethe, and Morehart (2015) find
33 that an additional dollar of direct payments increases land value by about \$18. Given that
34 rents are a major determinant of land values (Alston 1986; Burt 1986), it seems odd that
35 non-operators would be willing to pay a premium for land with greater government payments
36 but not extract the government payments through higher rental rates. The most plausible
37 explanation of the puzzle is that either the land value or the rental rate literature exploits
38 variability in the data that over or underestimates the true effect.

39 Intuitively, our empirical strategy compares cash rental rates in counties that have similar
40 market returns, but that have different direct payments due to the favoritism shown to areas
41 that historically produced cotton or rice. Our econometric model uses county-level data and
42 regresses cash rental rates on direct payments, expected market returns, and the proportion
43 of cropland enrolled in the Average Crop Revenue Election (ACRE) program. We instrument
44 direct payments with the share of cropland with cotton or rice base acres. We argue that the
45 favoritism shown to cotton and rice is primarily due to political favoritism which should have
46 no direct impact on rental rates except through government payments. Since cotton and rice
47 production is concentrated in a particular region, there could be concerns that our instrument
48 is correlated with differences in unmeasured expected market returns or differences in the
49 rental market for this region. We use the framework of Conley, Hansen, and Rossi (2012)

50 to construct revised standard errors that allow for a potential violation of the exclusion
51 restriction.

52 According to the OECD Producer Support Estimates, the 2000–2014 average commodity-
53 specific government transfers as a percent of total gross commodity receipts was only 5% for
54 corn and soybeans and 7% for wheat while it was 20% for cotton and 12% for rice. Data that
55 we construct for this paper also indicate that counties with cotton or rice base acres received
56 substantially larger direct payments than counties with similar market returns but no cotton
57 or rice base acres. There are several potential explanations for political favoritism towards
58 cotton and rice. Gardner (1987) argues that farm programs are primarily a means of income
59 redistribution and a commodity receives greater support if income can be redistributed more
60 efficiently for that commodity. Thus, government support depends on supply and demand
61 elasticities and the cost of political lobbying specific to each commodity (Gardner 1987).
62 Another explanation for cotton and rice favoritism is that one-party rule in the Southern
63 U.S. up to 1960 resulted in Southern lawmakers holding powerful positions (Gardner 1987).⁴

64 Exploiting this large, persistent difference in direct payments gives a more plausible esti-
65 mate of the long-run incidence on rental rates compared to other articles that exploit changes
66 in government payments between time periods (e.g., Kirwan 2009; Hendricks, Janzen, and
67 Dhuyvetter 2012; Michalek, Ciaian, and Kancs 2014) or between fields with the same crop
68 planted (Kirwan and Roberts Forthcoming). Rental rates within a particular geographic
69 region may not fully reflect differences in direct payments if rates are established by the
70 customary arrangements in the region (see Young and Burke 2001). However, rental rates
71 between different regions may fully reflect direct payments as the customary arrangements
72 in each region reflect the typical direct payments of that region. Similarly, small changes
73 in direct payments over time may have a negligible impact on rental rates if rents tend to

⁴From 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.

74 be established at round numbers.⁵ The most relevant parameter for understanding the ulti-
75 mate beneficiaries of agricultural subsidies is to understand how rental rates would differ if
76 subsidies were eliminated—a large, persistent shock.

77 We estimate that roughly \$0.81 of every dollar of direct payments accrues to non-operator
78 landlords, but we cannot reject the null hypothesis of full incidence. Exploiting the variation
79 in payments due to cotton and rice favoritism is critical to our results. If we restrict our
80 analysis to only counties that have negligible cotton or rice base acres, then our estimate of
81 the incidence has severe upward bias because we cannot perfectly control for expected market
82 returns between counties in the same region. However, our two-stage least squares empirical
83 strategy only requires that our estimates of expected market returns are not systematically
84 over or underestimated for counties with cotton or rice base acres and we also allow for
85 potential violations of the exclusion restriction.

86 Even though direct payments were eliminated in the 2014 Farm Bill, our estimate of the
87 incidence is relevant to current and future farm programs for two reasons. First, under-
88 standing the incidence of fixed payments not tied to production in real world rental markets
89 provides an important baseline for understanding the incidence of more complex programs.
90 If direct payments are not fully reflected in rental rates, then economic theory under perfectly
91 competitive rental markets may not provide realistic estimates of the long-run incidence of
92 other types of programs. Second, Agriculture Risk Coverage (ARC) and Price Loss Coverage
93 (PLC) payments, which were introduced in the 2014 Farm Bill, are both tied to base acres
94 and base yields rather than current production.⁶ Therefore, the incidence of ARC and PLC
95 payments is likely similar to the incidence of direct payments although the incidence could
96 be smaller for ARC and PLC due to uncertainty about the payments.

⁵For example, if rent is \$100/acre and direct payments decrease by \$2.27/acre, then rent may not change in order to keep the rental rate at a round number. However, if direct payments decrease by \$10/acre, then rent may decrease to \$90/acre.

⁶ARC provides payments when county-level revenue falls below a trigger and PLC provides payments when price falls below a trigger.

97 **Identification Challenges**

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

101 *Measuring the Rental Rate*

102 The first challenge is to obtain data on the cash rental rate for the dependent variable. Several
103 previous studies estimate the relationship between government payments and land values
104 (Goodwin and Ortalo-Magné 1992; Just and Miranowski 1993; Weersink et al. 1999; Barnard
105 et al. 1997; Ifft, Kuethe, and Morehart 2015). Translating these results into estimates of
106 the proportion of subsidies reflected in land values, however, requires assumptions about
107 the discount rate and expected stream of government payments (Kirwan 2009; Hendricks,
108 Janzen, and Dhuyvetter 2012). Identifying the impact on rental rates provides a cleaner
109 identification strategy since rental rates presumably depend on the current expected returns
110 from agricultural production.

111 However, data on rental rates have not been as widely available as land value data. Some
112 studies use cash rent calculated as total rent divided by total rented acres (Kirwan 2009;
113 Hendricks, Janzen, and Dhuyvetter 2012), but this underestimates the true cash rental rate
114 since total rented acres include acres rented by cash and crop-share agreements.⁷ Hendricks,
115 Janzen, and Dhuyvetter (2012) show how this measurement error biases the coefficient on
116 government payments downward with their data and use secondary data to correct for the
117 bias.

118 In this paper, we use data on the average cash rental rate for cropland at the county
119 level. These data are obtained from NASS surveys of the cash rental rate for irrigated and

⁷Furthermore, 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.

120 nonirrigated cropland, rather than constructing the rental rate from total rent divided by
121 rented acres. Other studies that use data on actual cash rental rates include Kirwan and
122 Roberts (Forthcoming) and Goodwin, Mishra, and Ortalo-Magné (2011).

123 *Expectation Error*

124 The second challenge is to accurately measure *expected* government payments. Farm subsidy
125 programs often depend on the harvest price—and more recently yield. Cash rental rates are
126 negotiated before harvest, and thus government payments are uncertain. The econometri-
127 cian, however, only observes data on the realized government payments. Regressing rent on
128 realized government payments results in classical measurement error since the observed vari-
129 able has a larger variance than the true variable. Therefore, the coefficient on government
130 payments is likely to be biased towards zero, *ceteris paribus*.

131 Kirwan (2009) provides a creative solution to the measurement error problem. He argues
132 that government payments in 1997 were known with certainty due to the introduction of
133 production flexibility contracts that did not depend on price or current production. There-
134 fore, Kirwan (2009) uses the 1997 government payments as an instrument for the difference
135 in 1997 and 1992 government payments. Several other studies use lagged or future govern-
136 ment payments as an instrument for current government payments (Lence and Mishra 2003;
137 Hendricks, Janzen, and Dhuyvetter 2012; Kilian et al. 2012). Goodwin, Mishra, and Ortalo-
138 Magné (2011) consider different specifications where they use the previous 5-year average of
139 government payments to approximate expected payments or various instruments. Kirwan
140 and Roberts (Forthcoming) include direct payments—which were known with certainty—in
141 their regression and also include a dummy variable for whether or not the farmer expected
142 to receive a counter-cyclical payment. Kirwan and Roberts (Forthcoming) use data from
143 2006 and 2007 when counter-cyclical and loan deficiency payments comprised a significant

144 portion of total government payments but the amount of payments was uncertain at the
145 time rents were established.⁸

146 We use rent data from 2012 when prices were so high above the triggers that farmers
147 arguably perceived a negligible probability of receiving counter-cyclical and loan deficiency
148 payments.⁹ Direct payments, on the other hand, provided a fixed per acre payment for the
149 life of the Farm Bill that did not depend on price or current production. One potential
150 concern with our analysis, however, is that the 2008 Farm Bill also introduced the Average
151 Crop Revenue Election (ACRE) Program. ACRE was a voluntary program that provided
152 farmers with payments when state-level revenues fell below a trigger. Farmers that enrolled
153 in ACRE lost 20% of their direct payments. Therefore, direct payments decreased in counties
154 with greater ACRE enrollment. Farmers, however, did not likely anticipate receiving less
155 government payments in these counties, or else they would not have enrolled in the ACRE
156 program. We include the proportion of cropland enrolled in ACRE as a control.

157 *Omitted Variable Bias*

158 The third challenge is to control for expected returns other than direct payments. Not
159 completely controlling for market returns biases the coefficient on direct payments upwards,
160 *ceteris paribus*, since the unobserved variability in market returns is likely positively corre-
161 lated with cash rent and direct payments. Another potential omitted variable is the expected
162 payments from ACRE since the proportion of cropland enrolled in ACRE is not likely to
163 completely control for expected ACRE payments. Expected ACRE payments are positively
164 correlated with rent but negatively correlated with direct payments since farmers sacrificed

⁸Counter-cyclical and loan deficiency payments totaled \$1.2 billion for production in 2006 and \$0.8 billion for production in 2007 compared to \$5.1 billion of direct payments (U.S. Department of Agriculture 2016). And for production in 2005, counter-cyclical and loan deficiency payments totaled \$4.8 billion. Counter-cyclical and loan deficiency payments are usually paid in the year following production so we use data from government payments in the following year.

⁹Counter-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).

165 direct payments to enroll in ACRE. Therefore, the bias from omitting expected ACRE pay-
166 ments is likely downward.

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

178 Kirwan and Roberts (Forthcoming) argue that they control for differences in expected
179 market returns across fields by including farmers' "yield goal" as a control. The yield goal
180 represents an expectation of yields rather than actual yields. One disadvantage of their
181 approach is that the data are crop specific. Kirwan and Roberts (Forthcoming) have data
182 on the rent of land planted to soybeans, for example, and the yield goal for soybeans but
183 the yield goal for other crops planted in the rotation may have an even larger impact on
184 the rental rate. Kirwan and Roberts (Forthcoming) argue that after controlling for the yield
185 goal, the variation remaining in subsidies is due to random variability in historical yields used
186 to calculate base. Such random variability is likely small since the base yield is calculated
187 from a multi-year average and farmers had the option to update base yield in 2002 if yields
188 from a recent period represented an improvement.

189 We take great effort to construct a control for market returns that accounts for variation
190 in returns across space and across crops. However, we recognize that we are unlikely to
191 perfectly control for expected market returns and expected ACRE payments so we propose

192 an instrumental variable approach. Our approach and assumptions are described in detail
193 in the next section.

194 *Long-Run Incidence*

195 The fourth challenge is to estimate the long-run incidence, allowing for adjustments in rental
196 rates. Rental rates are likely to have substantial inertia to changes in government payments
197 and market returns due to multi-year contractual agreements and customary rates may not
198 adjust to small changes in expected benefits.

199 Using panel data with fixed effects exploits year-to-year changes which only capture
200 short-run rental rate adjustments (Ciaian and Kancs 2012; Herck, Swinnen, and Vranken
201 2013; Michalek, Ciaian, and Kancs 2014). Kirwan (2009) uses long (five-year) differences.
202 Hendricks, Janzen, and Dhuyvetter (2012) rely on the partial adjustment framework to
203 estimate long-run impacts. The year-to-year variation in subsidies exploited by these studies
204 is often small so rental rates may be slow to adjust or not adjust at all to maintain rent
205 at a round number. The more relevant counterfactual is how rents adjust in the long run
206 to large changes in subsidy rates given adjustments in contracts and customary rents. We
207 exploit large cross-sectional variation in subsidy rates which inherently captures a long-run
208 effect without having to explicitly specify the dynamic process (Pesaran and Smith 1995).¹⁰

209 *Aggregation*

210 The fifth challenge is to have data at the appropriate level of aggregation. Kirwan and
211 Roberts (Forthcoming) assume that rents are established at the field-level. Estimates with
212 aggregate data (i.e, at the farm or county level) are biased if fields with above-average rental
213 rates also have above-average subsidies or if rent is averaged across subsidized and unsub-
214 sidized farmland and subsidies are averaged across all rented and owner-operated cropland.

¹⁰Lence 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 commodity favoritism.

215 Kirwan and Roberts (Forthcoming) find that farm-level estimates of the incidence are roughly
216 twice as large as field-level estimates.

217 An important assumption made by Kirwan and Roberts (Forthcoming) is that rental
218 rates are field specific. We argue, however, that a single rental rate is likely to be established
219 for all acreage within a tenant-landlord relationship so that the relevant unit of analysis is
220 all acreage within the tenant-landlord agreement.¹¹ Under this alternative assumption, field-
221 level subsidy rates vary more than the average tenant-landlord subsidy rate creating classical
222 measurement error and attenuated coefficients with field-level data. Furthermore, Kirwan
223 and Roberts (Forthcoming) find that the effect of subsidies on rental rates is smaller for larger
224 farms which is consistent with tenant market power or consistent with more measurement
225 error for larger farms that rent larger areas of land in each landlord relationship.

226 It may also be the case that rental rates depend on customary arrangements within a
227 particular region. For example, Young and Burke (2001) note that cropshare agreements
228 have different splits across different regions as would be predicted by conventional theory,
229 but the agreements rarely vary within a geographic region even though soil quality clearly
230 varies within a region. Young and Burke (2001) suggest that this occurs because contracts
231 tend to cluster around a few discrete values and because contracts tend to conform to the
232 customary local arrangements. In this case, the cash rental rate depends on the average direct
233 payments within the region and the field-level direct payment is a noisy approximation of
234 direct payments in the region resulting in attenuation bias.

¹¹Aggregate statistics indicate that it is likely that a large portion of tenant-landlord relationships include multiple fields. According to the 2014 TOTAL (Tenure, Ownership, and Transition of Agricultural Land) survey, landowners that rent more than 200 acres represent 70% of all land rented in the South, Plains, and Midwest states. Landowners that rent more than 500 acres represent 45% of all land rented in this region. Furthermore, landowners renting to a single tenant represent 62% of all land rented in this region.

235 **Econometric Model**

236 Our identification strategy uses two-stage least squares (2SLS) to estimate the effect of direct
237 payments on rental rates. Our second stage equation of interest is

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

238 where $Rent_i$ is the average cash rental rate per acre for cropland in county i , $DirectPmts_i$ is
239 the average direct payment subsidy per acre, $MktReturns_i$ is the expected market returns
240 for cropland, $f(\cdot)$ is a function of expected market returns that is potentially nonlinear, β_R
241 is a vector of parameters in the nonlinear function of expected market returns, $ACRE_i$ is the
242 proportion of cropland enrolled in the ACRE program, and ε_i is the variation in rental rates
243 from other unobserved factors. The objective of our paper is to estimate β_D , which represents
244 the proportion of direct payments captured in rental rates. The first stage equation is

$$(2) \quad DirectPmts_i = \alpha_1 + \alpha_{CR} CottonRice_i + f(\alpha_R, MktReturns_i) + \alpha_A ACRE_i + u_i,$$

245 where $CottonRice_i$ is the proportion of cropland with cotton or rice base acres.

246 First, consider why ordinary least squares (OLS) estimates of equation (1) are likely
247 biased. For OLS to estimate the causal parameter β_D , direct payments per acre must be
248 uncorrelated with the variation in rental rates not explained by our measure of market
249 returns and ACRE enrollment (i.e., $Cov(DirectPmts_i, \varepsilon_i) = 0$). Given that we are unlikely to
250 perfectly measure expected market returns and expected ACRE payments, this assumption
251 is unlikely to hold. Any variability in returns not captured by our controls is included in the
252 error term (i.e., an omitted variable) and is likely correlated with direct payments. The bias

253 of OLS could be upwards or downwards depending whether the bias from omitted market
254 returns or omitted ACRE payments dominates.

255 The bias of OLS may not be large when the sample includes counties that have differing
256 amounts of base acreage in cotton or rice. Angrist (1998) shows that regression estimates
257 an average coefficient where more weight is given to observations with a greater variance
258 of direct payments conditional on the controls. The variance of direct payments is greatest
259 between counties that have different amounts of cotton or rice base acreage. Therefore, OLS
260 identifies the incidence of direct payments on rents primarily using the variation in direct
261 payments due to commodity favoritism.

262 To further alleviate concerns about omitted variable bias, we consider 2SLS. Consistency
263 of 2SLS requires two assumptions: (i) the first stage relationship between the instrument and
264 the endogenous regressor exists and (ii) the exclusion restriction holds. The first assumption
265 requires that $\alpha_{CR} \neq 0$. Furthermore, finite sample bias can exist if the relationship between
266 the instrument and endogenous regressor is not sufficiently strong (Bound, Jaeger, and Baker
267 1995). In our case, the relationship between the share of cropland with cotton or rice base
268 acreage and direct payments is strong as we show in our results.

269 The exclusion restriction in our model requires that the variation in rental rates left over
270 after parsing out expected market returns and enrollment in ACRE cannot be correlated with
271 the proportion of cropland with cotton or rice base acreage (i.e., $Cov(CottonRice_i, \varepsilon_i) = 0$).
272 This assumption requires that our estimates of expected market returns are not system-
273 atically over or underestimated for counties with cotton or rice. The consistency of OLS
274 requires that expected market returns are measured perfectly, which is a much more strin-
275 gent assumption. For example, the exclusion restriction does not require that we perfectly
276 measure the difference in market returns between two neighboring counties, but simply that
277 on average we correctly measure the difference in market returns between counties with and
278 without cotton and rice base acres.

279 The exclusion restriction also requires that there is nothing systematically different about
 280 counties producing cotton or rice apart from direct payments, expected market returns, and
 281 ACRE enrollment that would affect the rental rate. The exclusion restriction would be
 282 violated if, for example, there were cultural differences such that counties with cotton or rice
 283 base acres had more or less competitive rental markets.

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

$$(3) \quad Rent_i = \beta_1 + \beta_D DirectPmts_i + f(\beta_R, MktReturns_i) + \beta_A ACRE_i + \gamma CottonRice_i + \varepsilon_i,$$

287 where the exclusion restriction imposes $\gamma = 0$. Intuitively, γ represents the expected value of
 288 the difference in cash rent in a county where all cropland had cotton or rice base acres and
 289 the cash rent in a county that had no cotton or rice base acres—controlling for differences
 290 in direct payments, our measure of expected market returns, and ACRE enrollment.¹² The
 291 difference in cash rental rates represented by γ could occur because we have not completely
 292 controlled for differences in expected market returns or due to differences in the rental
 293 markets between counties with cotton or rice base acres and those without cotton or rice
 294 base acres.

295 When $\gamma \neq 0$, then the probability limit of 2SLS is written as $\hat{\beta}_D \xrightarrow{p} \beta_D + \gamma/\alpha_{CR}$ in our
 296 case where β_D , γ , and α_{CR} are scalars (Conley, Hansen, and Rossi 2012). The probability
 297 limit of 2SLS makes clear that the bias from violations of the exclusion restriction depends
 298 on the strength of the first stage relationship (see also Bound, Jaeger, and Baker 1995).
 299 Small deviations from the exclusion restriction can induce large bias when the first stage

¹²Let ρ_i represent the variation in rent not explained by $DirectPmts_i$, $MktReturns_i$, and $ACRE_i$,

$$Rent_i = \beta_0^p + \beta_D^p DirectPmts_i + f(\beta_R^p, MktReturns_i) + \beta_A^p ACRE_i + \rho_i.$$

Then we can write $\gamma = E[\rho_i | CottonRice_i = 1] - E[\rho_i | CottonRice_i = 0]$.

300 relationship is weak and conversely relatively large deviations from the exclusion restriction
301 may have a smaller effect on bias when the first stage relationship is strong. In practice, there
302 is often a tradeoff between the plausible exogeneity of an instrument and the strength of the
303 first stage relationship. We choose an instrument that has a strong first stage relationship
304 but where the exclusion restriction is unlikely to hold perfectly.

305 To account for potential deviations from the exclusion restriction, we construct revised
306 standard errors using the framework of Conley, Hansen, and Rossi (2012). We do not know
307 the true value of γ but we make an assumption about likely values, essentially imposing a
308 prior distribution for γ . We assume that $\gamma \sim N(0, \delta^2)$, where δ is the standard deviation
309 of likely values of γ . We do not have any prior beliefs about whether γ is more likely
310 to be positive or negative so we assume γ has mean zero. Imposing prior beliefs about
311 the distribution of γ is more general than the standard 2SLS approach that imposes the
312 prior belief that $\gamma = 0$. When γ is assumed to be normally distributed, Conley, Hansen,
313 and Rossi (2012) show how to easily calculate a revised variance matrix by using a large
314 sample approximation that assumes uncertainty about γ is of the same order of magnitude
315 as sampling uncertainty. Conley, Hansen, and Rossi (2012) refer to this approach as a local-
316 to-zero approximation.¹³ In the results section, we discuss our specific prior beliefs about
317 γ .

318 Data

319 First, we describe our data sources and the construction of variables and then show summary
320 statistics and data visualizations.

¹³Another approach proposed by Conley, Hansen, and Rossi (2012) is to use Bayesian analysis that incorporates prior information about γ . 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 γ 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 γ , 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 γ .

321 *Data Description*

322 We restrict our analysis to counties in four farm resource regions as defined by U.S. Depart-
323 ment of Agriculture (2015): the Northern Great Plains, Prairie Gateway, Heartland, and
324 Mississippi Portal. Altogether, the four regions in our analysis account for roughly 66% of
325 U.S. cropland area.

326 Our dependent variable is the average cash rental rate for cropland in 2012. County-level
327 data on the cash rental rate (\$/acre) for irrigated and nonirrigated cropland are obtained
328 from National Agricultural Statistics Service (NASS) survey data. We construct the average
329 cash rental rate as irrigated rent times the share of cropland irrigated plus nonirrigated rent
330 times the share of cropland nonirrigated. The share of cropland irrigated for each county is
331 the ratio of harvested irrigated cropland to total cropland in 2012 obtained from the Census
332 of Agriculture.¹⁴ In some cases, we only have data on irrigated or nonirrigated rental rates.
333 Often this occurs because a large majority of the cropland is either irrigated or nonirrigated.
334 We use the nonirrigated rental rate as the county average when less than 10% of the county
335 is irrigated and use the irrigated rental rate when more than 75% of the county is irrigated.

336 Data on direct payments and base acres enrolled in farm programs are obtained from the
337 Farm Program Atlas from U.S. Department of Agriculture (2012). For our key explanatory
338 variable, we construct direct payments per cropland acre as total direct payments in 2009
339 divided by total cropland acres in 2012. The proportion of county cropland that has cotton
340 or rice base is calculated as the direct payment cotton and rice base acres divided by total
341 cropland acres in 2012. Base acres enrolled in the ACRE program are also obtained from
342 the Farm Program Atlas in order to calculate the proportion of cropland enrolled in ACRE.

¹⁴In 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.

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

$$(4) \quad MktReturns_i = (1 - \phi_i) \sum_c \frac{acres_{ci}}{\sum_c acres_{ci}} \left[\frac{1}{5} \sum_{t=2008}^{2012} (Revenue_{cit} - Cost_{crt}) \right],$$

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

355 For all crops, except cotton, expected revenue is calculated as $Revenue_{cit} = Price_{cst} \times$
 356 $Yield_{cit}$, where $Price_{cst}$ is the price for crop c in state s in year t and $Yield_{cit}$ is the trend yield
 357 for crop c in county i in year t . State-level marketing-year prices are obtained from NASS.
 358 If the state-level price for a crop is missing in a particular year, it is replaced by the average
 359 price in all states with data in that year. The trend yield is estimated from county-specific
 360 linear trend regressions using data from 1980 to 2012. We only estimate trend yield if there
 361 are 20 or more observations for a county and if there was at least one yield observation from
 362 2007 to 2012. We use trend yields rather than observed yields because cash rents depend on
 363 expected market returns and average realized returns in the five-year period could deviate
 364 substantially from expected market returns if weather was especially good or poor.

365 For cotton, expected revenue includes revenue from cotton lint and cottonseed production.
366 The revenue from cotton lint production is calculated the same as for other crops. Cottonseed
367 prices are also state-level prices. NASS does not, however, report county-level cottonseed
368 production. We assume cottonseed yield is 1.62 times the cotton lint trend yield based on
369 data in U.S. Department of Agriculture (2014).¹⁵

370 For all crops, production expenses are obtained by farm resource region from U.S. De-
371 partment of Agriculture (2014). We include all operating costs and allocated overhead but
372 exclude the opportunity cost of land (i.e., land rent). U.S. Department of Agriculture (2014)
373 provides cost estimates for the following regions for each commodity: soybeans in all regions,
374 corn, wheat, and sorghum in the Heartland, Prairie Gateway, and Northern Great Plains;
375 cotton in the Heartland, Prairie Gateway, and Mississippi Portal; and rice in the Mississippi
376 Portal.¹⁶ For corn, wheat, and sorghum expenses in the Mississippi Portal, we use expenses
377 from the Heartland. For rice expenses in the Heartland, we use expenses from the Mississippi
378 Portal.¹⁷ Using expenses from neighboring regions ensures that we have cost estimates in
379 every county where we have trend yield and acreage data for a commodity.

380 In equation (4), we average market returns across crops where we weight by the share of
381 acreage planted to each crop ($\frac{acres_{ci}}{\sum_c acres_{ci}}$). The acres planted to the crop is the 2008 to 2012
382 average planted acreage. If acreage data are missing for a particular crop in all years, then
383 we assume the crop is not produced in the county. If acreage data are available but trend
384 yield is not available for the crop, then we set acreage for that crop equal to zero.

385 Equation (4) assumes that the returns from summer fallowed land are zero. We obtain
386 2012 acres in summer fallow from the Census of Agriculture and divide it by cropland acres

¹⁵The 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 according to ERS costs and returns.

¹⁶ERS 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.

¹⁷There are only a few counties in the southern portion of the Heartland region where rice is produced.

387 to calculate ϕ_i . Annual data do not exist at the county level for summer fallow acreage so
388 ϕ_i is constant over time.

389 We drop observations from our sample if we have estimates of market returns from less
390 than 25% of total cropland.¹⁸ Counties that are dropped are likely those counties where
391 other crops comprise a major portion of cropland area and our measure of market returns
392 may not be representative for these counties. In the sample used for econometric analysis,
393 expected market returns accounts for more than 50% of cropland area for 81% of counties.

394 Our econometric analysis also excludes observations if market returns are greater than
395 \$325/acre. There is only one county with more than 1% cotton or rice base acreage that has
396 market returns greater than \$325/acre while there are 255 counties with less than 1% cotton
397 or rice base acreage. Including observations with market returns greater than \$325/acre cre-
398 ates a problem where—for this portion of the data—we have little overlap between counties
399 with and without cotton or rice base acres. In a later section, we explore the robustness of
400 our estimates to different specifications for dropping counties.

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

408 *Data Summary and Visualization*

409 Table 1 shows summary statistics for the variables used in our econometric analysis. Panel
410 A shows summary statistics for counties with less than 1% of cropland with cotton or rice
411 base acres (461 counties) and panel B for counties with more than 1% of cropland with

¹⁸That is, we add $\sum_c acres_{ci}$ and summer fallow acreage and divide by total cropland acres and drop the observation if the proportion is less than 0.25.

412 cotton or rice base acres (178 counties). The mean value for direct payments for the counties
413 with negligible cotton or rice base (\$10.92) is lower than for those counties with cotton or
414 rice base (\$19.35). The mean values for cash rent and market returns are higher in counties
415 with negligible cotton or rice base acreage. Enrollment in the ACRE program was greater
416 in counties with negligible cotton or rice base acreage. Among those counties with cotton
417 or rice base, the proportion of cropland with cotton or rice base acres differs substantially
418 among counties with a mean of 0.32 and a standard deviation of 0.23.¹⁹

419 Figure 1 shows maps for cash rent, market returns, direct payments, and the proportion
420 of cropland with cotton or rice base acres. The light grey area shows those counties that
421 are not included in one of the four farm resource regions included in our sample. The dark
422 grey area shows those counties that had missing data for one of the variables used in the
423 econometric analysis. Missing data usually occurred because county-level cash rent was not
424 reported or market returns could not be calculated because trend yield or acreage data were
425 missing. The light blue area shows those counties that were dropped from our analysis
426 because either market returns were calculated for less than 25% of cropland area or market
427 returns exceeded \$325/acre.

428 High cash rental rates are concentrated in the area surrounding the Corn Belt and Missis-
429 sippi Portal and rental rates are smaller moving west to the plains states (figure 1a). Market
430 returns generally follow a similar pattern as the cash rental rate (figure 1b). Direct payments,
431 however, are much larger in the Mississippi Portal region and portions of Texas compared
432 to the Northern regions (figure 1c). The larger direct payments are directly related with the
433 proportion of cropland with cotton or rice base acres (figure 1d).

434 Figure 2 shows a scatterplot of the data used in our econometric analysis for the relation-
435 ship between market returns and the average cash rental rate. Purple circles indicate counties
436 with less than 1% cotton or rice base and orange diamonds indicate counties with more than
437 1% cotton or rice base. The clear positive relationship between returns and the rental rate

¹⁹Cotton or rice base acres exceeded cropland acreage in one county. This may have occurred if cropland area decreased from the time base was established.

438 provides some support for the accuracy of our measurement of market returns—though not
439 necessarily eliminating omitted variable bias.

440 The most important observation from figure 2 is that conditional on the same market
441 returns, counties with cotton or rice base acres tend to have higher rental rates. Furthermore,
442 from figure 3, we see that conditional on the same market returns, counties with cotton or
443 rice base acres tend to have much larger direct payments. These simple observations from
444 the data provide suggestive evidence that direct payments are at least partially captured in
445 the rental rate.

446 **Econometric Results**

447 Next, we show the econometric results that conduct more rigorous tests than the graphical
448 evidence above and estimate the proportion of direct payments reflected in rental rates. We
449 first show OLS results which we argue are likely biased, then we show our preferred 2SLS
450 results and robustness checks.

451 *OLS Results*

452 Table 2 reports OLS results for the effect of direct payments on rental rates. The different
453 columns report estimates where we control for market returns with different polynomial
454 specifications. The R^2 indicates that our regression is able to explain roughly 73% of the
455 variation in cash rents.

456 Each of the specifications in table 2 give similar estimates of the incidence. For the linear
457 functional form (column 1), for example, the coefficient on direct payments indicates that
458 cash rents increase by \$0.51 for every dollar of direct payments. For all three specifications
459 in table 2, we reject the null hypotheses of $\beta_D = 0$ and $\beta_D = 1$ at the 5% level. Our standard
460 error of the coefficient on direct payments (≈ 0.15) is similar in magnitude to the standard
461 error on direct payments in Kirwan and Roberts (Forthcoming) for soybeans (≈ 0.11).

462 The coefficient on market returns in column (1) of table 2 indicates that cash rents
463 increase by \$0.36 for an additional dollar of market returns. In theory, this coefficient
464 should also represent the amount that landowners would capture from a purely coupled
465 subsidy. Our result is consistent with Alston (2010) who finds that standard economic
466 theory suggest landowners receive about \$0.39 from a pure output subsidy under plausible
467 parameters with a range from \$0.19 to \$0.62 under alternative parameter assumptions. An
468 important caveat, is that our coefficient on market returns could be biased downward to the
469 extent that we have measurement error in expected market returns. However, our coefficient
470 is much larger than estimated by Kirwan (2009) and Hendricks, Janzen, and Dhuyvetter
471 (2012)—0.03 and 0.11, respectively.²⁰ Goodwin, Mishra, and Ortalo-Magné (2011) estimate
472 a coefficient on markets returns of about 0.12–0.16 depending on their specification. The
473 estimate of Goodwin, Mishra, and Ortalo-Magné (2011) is likely biased downwards given
474 that they use an historical average of actual returns from crop and livestock production.
475 Our coefficient on market returns is similar to Lence and Mishra (2003).

476 As expected, the coefficients on the proportion of cropland enrolled in ACRE indicate
477 that cash rents are larger in counties with more land enrolled in ACRE, *ceteris paribus* (table
478 2). Direct payments per cropland acre within a county decrease as more area is enrolled in
479 ACRE because farmers had to reduce their direct payments in order to enroll in the ACRE
480 program. However, farmers may have still expected to receive some subsidy payments from
481 ACRE and so the coefficient on ACRE reflects this value.

482 Table 3 reports OLS results with alternative specifications. Column (1) in each of the
483 panels shows a simple bivariate relationship between cash rent and direct payments with
484 different samples. Columns (2)-(4) show results with linear, quadratic, and cubic controls
485 for market returns.

²⁰Kirwan (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.

486 Panel A in table 3 shows results that omit the proportion of cropland enrolled in ACRE
487 as a control. The coefficient in column (1) shows that OLS is biased upwards substantially
488 when controls for market returns and ACRE enrollment are omitted. It is not surprising that
489 the coefficient on direct payments exceeds 1 in the simple bivariate regression. Cash rental
490 rates are larger than direct payments per acre and direct payments are positively correlated
491 with market returns. So the coefficient on direct payments in the bivariate regression reflects
492 the impact of subsidies and market returns on rental rates. Consistent with our discussion
493 in the model section, results in columns (2)-(4) show smaller estimates of the incidence of
494 direct payments on rents when we omit the control for ACRE enrollment.

495 Panel B in table 3 shows regression results using data from only those counties with
496 negligible cotton or rice base acreage. These results do not exploit the variability in direct
497 payments due to commodity favoritism. The coefficients on direct payments in columns
498 (2)-(4) are much larger than those in table 2, consistent with a large omitted variable bias
499 when we do not exploit the variability from commodity favoritism.

500 Panel C in table 3 shows results using only counties with more than 1% cotton or rice
501 base acreage. Since the proportion of cropland with cotton or rice base acreage varies across
502 these counties, OLS exploits—at least in part—the variability in direct payments due to
503 variation in cotton and rice base acreage. Therefore, estimates in panel C should have less
504 bias than those in panel B. Indeed, OLS estimates in columns (2)-(4) of panel C are much
505 smaller than in panel B and are slightly larger than OLS estimates for the entire sample in
506 table 2. The main disadvantage of the OLS estimates in panel C is that the standard errors
507 increase to about 0.25 compared to 0.15 in table 2 since we only have 178 observations in
508 panel C.

509 The main concern with OLS estimates in table 2 and panel C of table 3 is that there could
510 still be some remaining unobserved heterogeneity affecting rental rates that is also correlated
511 with direct payments. For example, if we have omitted some variability in market returns,
512 then OLS estimates are biased upwards. Alternatively, if we have not sufficiently controlled

513 for expected ACRE program payments, then OLS estimates are likely biased downwards.
514 Next, we consider an instrumental variables approach to exploit the variability in direct
515 payments due to favoritism for cotton and rice.

516 *2SLS Results*

517 Table 4 reports our first-stage regression results. Not surprisingly, the share of cropland with
518 cotton or rice base acreage has a large impact on direct payments even after controlling for
519 market returns and ACRE enrollment. The results indicate that direct payments are roughly
520 \$37/acre larger if all of the cropland in a county has cotton or rice base acreage relative to
521 a county with no cotton or rice base acreage. This is a large difference in payments, given
522 that the average direct payments in counties with less than 1% cotton or rice base is only
523 \$11/acre in our sample (see table 1).

524 Our first-stage results also indicate no evidence of a weak instrument problem. The F-
525 statistics for the coefficient on our instrument exceed 300 for all specifications. This suggests
526 minimal finite sample bias for instrumental variables (Staiger and Stock 1997). The strong
527 relationship between the instrument and direct payments also means that violations of the
528 exclusion restriction have a relatively smaller impact on our estimate of the incidence than
529 if we had a weak instrument.

530 Table 5 reports estimates of the incidence using 2SLS. Heteroskedasticity-robust standard
531 errors are reported in parentheses under each coefficient. Standard errors that allow for a
532 potential violation of the exclusion restriction are reported in brackets under each coefficient.
533 We place asterisks next to the standard errors to indicate the statistical significance for each
534 type of standard errors.

535 We relax the exclusion restriction using the local-to-zero approximation proposed by
536 Conley, Hansen, and Rossi (2012) and impose the prior distribution $\gamma \sim N(0, \delta^2)$. We
537 assume γ has mean zero because we do not have a prior on whether cash rents are likely to
538 be systematically higher or lower in counties with cotton or rice base acreage after accounting

539 for subsidies and our measure of market returns. We assume $\delta = 5$. This assumption implies
540 that we have 95% confidence that the value of γ is between -9.8 and +9.8. This allows for the
541 possibility that cash rents in counties with cotton or rice base acres on all cropland could be
542 \$9.80/acre greater (or less) than in counties with no cotton or rice base acres due to factors
543 not accounted for in our regression. The mean cash rental rate for counties with more than
544 1% cotton or rice base acres is \$62/acre, so our prior on γ allows for a substantial violation
545 of the exclusion restriction. Of course, our assumption of a normal distribution assumes that
546 γ is most likely close to zero.

547 Column (1) in table 5 indicates that cash rents increase by \$0.81 for every dollar of direct
548 payments. The coefficient on direct payments is larger with 2SLS than with OLS indicating
549 that the bias from omitted variables in OLS was downward. The most likely explanation is
550 that our control for ACRE enrollment does not sufficiently control for the expected payments
551 from the ACRE program and these unobserved payments bias OLS downwards. The p-value
552 for a test for endogeneity that is robust to heteroskasticity is reported near the bottom of
553 table 5 (see Wooldridge 2010). The test rejects the null hypothesis of exogeneity for each
554 specification.

555 The heteroskedasticity-robust standard error for the coefficient on direct payments is 0.17,
556 only slightly larger than 0.15 from the OLS model. Accounting for a potential violation of
557 the exclusion restriction, the standard error increases to 0.22 (standard error in brackets in
558 column 1). With either type of standard error, we reject the null hypothesis that $\beta_D = 0$
559 but fail to reject the null that $\beta_D = 1$ at the 5% level.

560 Including quadratic and cubic controls for market returns does not dramatically alter the
561 coefficient on direct payments (columns 2-3). The coefficient on market returns is similar
562 with 2SLS compared to OLS (compare tables 5 and 2).

564 In the supplementary appendix, we report results from several different robustness checks
565 and describe the specifications for the robustness checks in more detail. Table A1 shows
566 results if we use different thresholds for the proportion of cropland area that is accounted
567 for in our estimate of market returns or different thresholds for market returns to maintain
568 overlap in our sample. We also consider estimating the model for all observations with
569 nonmissing data. The coefficient on direct payments in these specifications varies between
570 0.76 and 0.83, so these assumptions make little difference to our results.

571 Table A2 in the supplementary appendix shows results if we calculate the variables in our
572 analysis differently. The first column shows results if we use cropland used for crops (i.e., the
573 sum of harvested, failed, and summer fallowed cropland) rather than total cropland area to
574 derive per acre estimates. The coefficient on direct payments is 0.75. Our results are also not
575 highly sensitive to using a 4 or 3-year historical average of market returns instead of a 5-year
576 average. If we calculate market returns over the period 2009–2012 instead of 2008–2012,
577 then the coefficient on direct payments is 0.75 and the coefficient on market returns is 0.33.
578 If we use the period 2010–2012 for market returns, then the coefficient on direct payments
579 is 0.89 and the coefficient on market returns is 0.28.

580 Table A3 in the supplementary appendix shows results using the rental rate from different
581 years. Using rental rates from 2011 and market returns from 2007–2011, the coefficient on
582 direct payments is 1.02. Using rental rates from 2010 and 2009 and the respective five year
583 periods for market returns, the coefficients on direct payments are 1.37 and 1.31, but the
584 difference from 1 is not statistically significant. The coefficient on market returns in each of
585 these specifications ranges from 0.34 to 0.40. Using rental rates from earlier years gives a
586 larger estimate of the incidence. Estimates from earlier years could be problematic because
587 there was a sharp increase in agricultural returns in 2008 due to an increase in commodity
588 prices so a five-year historical average of expected market returns is less likely to represent
589 market returns in these prior years than for 2012 used in our main specification.

590 Discussion and Conclusion

591 Our preferred estimate of the incidence of direct payments on rental rates is 2SLS with a
592 linear control for market returns (column 1 of table 5) and assuming the instrument is only
593 plausibly exogenous (standard error in brackets). This specification isolates the variability
594 in direct payments due to commodity favoritism, but without strictly imposing the exclusion
595 restriction.

596 Our preferred specification indicates that \$0.81 of every dollar of direct payments is
597 captured by landowners through adjustments in the rental rate in the long run. Standard
598 economic theory suggests that subsidies not tied to production should be completely reflected
599 in rental rates ($\beta_D = 1$) and our econometric estimates are not able to reject this null
600 hypothesis, though the evidence suggests slightly less than full incidence on rental rates.
601 We also estimate that about \$0.36 of every dollar of expected market returns accrues to
602 landowners through higher rental rates in the long run, which is also consistent with economic
603 theory.

604 According to the 2012 TOTAL Survey, about 46% of cropland in the United States is
605 rented by non-operator landlords.²¹ Assuming that the incidence of direct payments is similar
606 across different types of rental rate agreements, our estimate indicates that of the annual \$4.7
607 billion of direct payments in the 2008 Farm Bill, about \$1.75 billion ($1.75 = 4.7 \times 0.46 \times 0.81$)
608 was captured by non-operator landlords.

609 Kirwan (2009) and Kirwan and Roberts (Forthcoming) estimate that only about \$0.25 is
610 captured by landowners and Hendricks, Janzen, and Dhuyvetter (2012) estimate about \$0.37
611 is captured by landowners in the long run. Many other studies also estimate a small incidence
612 (Breustedt and Habermann 2011; Ciaian and Kancs 2012; Kilian et al. 2012; Michalek,
613 Ciaian, and Kancs 2014; Herck, Swinnen, and Vranken 2013).

²¹About 57% of cropland in the United States is rented and roughly 81% of rented cropland is rented by non-operator landlords. Note that the percent of cropland rented (57%) is larger than the percent of all agricultural land rented (39%).

614 Overall, we argue that exploiting large differences in subsidy rates across regions with
615 different commodities provides a more plausible estimate of the incidence of direct payments
616 on rental rates in the long run. It could be that rental rates capture little of the difference in
617 direct payments that occur over time or across areas with similar commodities. But rental
618 rates may capture most of the large, persistent difference in direct payments that occurs
619 between regions. A rationale for this distinction in incidence for different types of changes
620 in subsidies is that rental rates may be set by customary local arrangements that are slow
621 to adjust and tend toward rounds numbers. The impact of persistent differences in direct
622 payments is arguably most relevant for policy analysis that seeks to understand the ultimate
623 beneficiaries of these programs.

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

630 We began this paper by noting that the politics of government interventions depend as
631 much on the distribution of benefits and costs as the overall change in social welfare. Our
632 empirical results indicate that there is a tradeoff between reducing trade distortions (i.e.,
633 transferring with less deadweight loss) and transferring benefits to farm operators. Subsidies
634 tied directly to production are trade distorting, but non-operator landlords only capture
635 roughly 36% of the benefits on rented land. Subsidies not tied to production are less trade
636 distorting, but non-operator landlords capture roughly 81% of the benefits on rented land.

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Tables

Table 1: Summary Statistics

	Observations	Mean	Std. Dev.	Min	Max
Panel A. Counties with Less than 1% Cotton or Rice Base					
Cash Rent (\$/acre)	461	98.28	50.30	10.50	237.13
Direct Payments (\$/acre)	461	10.92	3.91	1.21	22.20
Market Returns (\$/acre)	461	168.59	105.76	-76.94	324.44
Proportion ACRE	461	0.10	0.12	0.00	0.62
Panel B. Counties with More than 1% Cotton or Rice Base					
Cash Rent (\$/acre)	178	62.09	36.35	10.50	145.00
Direct Payments (\$/acre)	178	19.35	12.41	2.44	60.88
Market Returns (\$/acre)	178	60.73	94.86	-143.22	315.29
Proportion ACRE	178	0.03	0.09	0.00	0.59
Proportion Cotton or Rice	178	0.32	0.23	0.01	1.10

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

	(1)	(2)	(3)
Direct Payments	0.509 (0.151)**	0.546 (0.148)**	0.545 (0.149)**
Market Returns	0.360 (0.010)**	0.270 (0.017)**	0.247 (0.011)**
Market Returns ²		0.000 (0.000)**	0.001 (0.000)**
Market Returns ³			-0.000 (0.000)**
Proportion ACRE	27.857 (8.628)**	26.442 (8.632)**	26.667 (8.505)**
Intercept	29.245 (2.038)**	29.989 (1.907)**	28.170 (2.074)**
Observations	639	639	639
R^2	0.729	0.737	0.739

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

Table 3: OLS Results with Alternative Specifications

	(1)	(2)	(3)	(4)
Panel A. Omit ACRE Control				
Direct Payments	1.443 (0.249)**	0.426 (0.148)**	0.468 (0.144)**	0.467 (0.146)**
Market Returns	No	Linear	Quadratic	Cubic
Proportion ACRE	No	No	No	No
Observations	639	639	639	639
R^2	0.058	0.725	0.734	0.735
Panel B. Counties with Less than 1% Cotton or Rice Base				
Direct Payments	7.690 (0.424)**	1.534 (0.396)**	1.377 (0.429)**	1.507 (0.441)**
Market Returns	No	Linear	Quadratic	Cubic
Proportion ACRE	No	Yes	Yes	Yes
Observations	461	461	461	461
R^2	0.358	0.748	0.749	0.752
Panel C. Counties with More than 1% Cotton or Rice Base				
Direct Payments	1.749 (0.230)**	0.712 (0.245)**	0.724 (0.251)**	0.622 (0.252)**
Market Returns	No	Linear	Quadratic	Cubic
Proportion ACRE	No	Yes	Yes	Yes
Observations	178	178	178	178
R^2	0.357	0.594	0.599	0.613

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

Table 4: Proportion of Base Acres Cotton or Rice and Direct Payments (First-Stage)

	(1)	(2)	(3)
Proportion Cotton or Rice	36.653 (2.036)**	36.635 (2.023)**	36.875 (2.055)**
Market Returns	0.029 (0.002)**	0.030 (0.004)**	0.036 (0.004)**
Market Returns ²		-0.000 (0.000)	-0.000 (0.000)**
Market Returns ³			0.000 (0.000)**
Proportion ACRE	2.061 (1.447)	2.072 (1.451)	2.117 (1.404)
Intercept	5.742 (0.307)**	5.734 (0.316)**	6.159 (0.335)**
F-Statistic ($H_0 : \alpha_{CR} = 0$)	324	328	322
Observations	639	639	639
R^2	0.687	0.687	0.691

The dependent variable is direct payments per acre. Standard errors in parentheses represent heteroskedasticity-robust standard errors.

Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

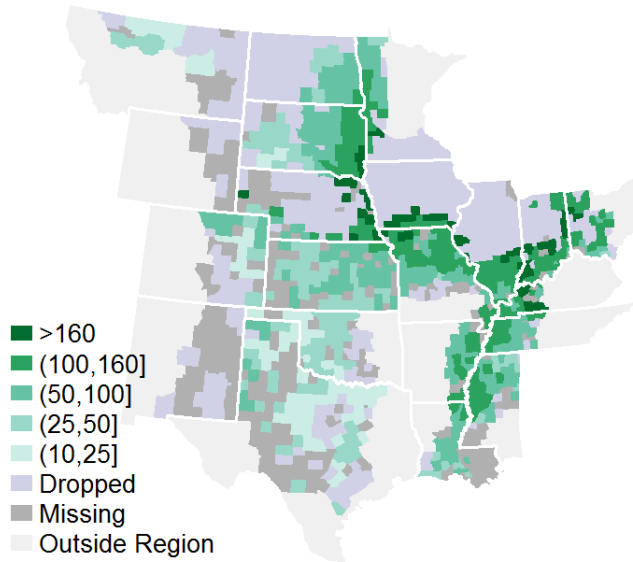
Table 5: Two-Stage Least Squares Results for the Incidence of Direct Payments on Cash Rental Rates

	(1)	(2)	(3)
Direct Payments	0.807 (0.174)** [0.221]**	0.861 (0.171)** [0.219]**	0.835 (0.176)** [0.222]**
Market Returns	0.355 (0.010)** [0.011]**	0.262 (0.017)** [0.018]**	0.240 (0.011)** [0.012]**
Market Returns ²		0.000 (0.000)** [0.000]**	0.001 (0.000)** [0.000]**
Market Returns ³			-0.000 (0.000)** [0.000]**
Proportion ACRE	32.173 (8.624)** [8.629]**	30.927 (8.611)** [8.616]**	30.802 (8.448)** [8.453]**
Intercept	25.595 (2.037)** [2.183]**	26.178 (1.942)** [2.094]**	24.665 (1.981)** [2.149]**
P-value for test of endogeneity (H_0 =exogeneity)	0.003	0.001	0.005
Observations	639	639	639

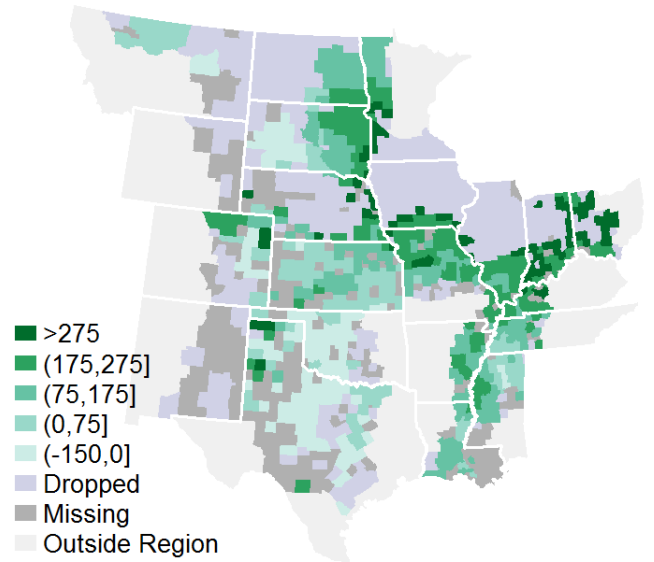
Standard errors in parentheses represent heteroskedasticity-robust standard errors imposing the exclusion restriction. Standard errors in brackets are calculated allowing for a potential violation of the exclusion restriction (Conley, Hansen, and Rossi 2012). The test for endogeneity is conducted using the results that impose the exclusion restriction and the test is robust to heteroskedasticity.

Asterisks * and ** denote significance at the 10% and 5% levels, respectively.

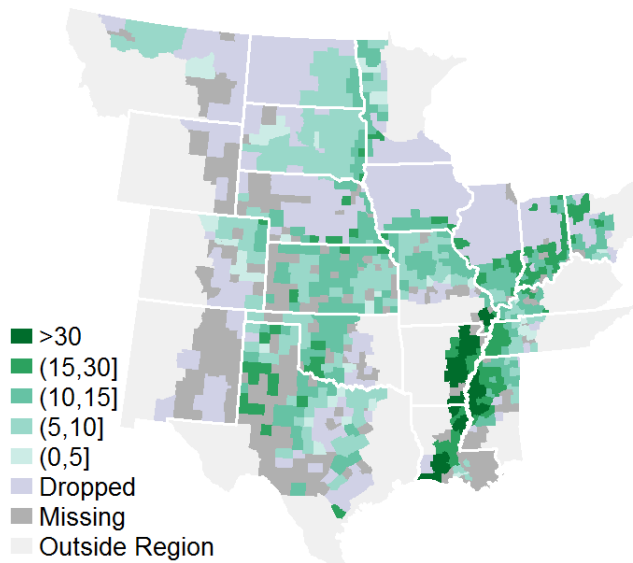
Figures



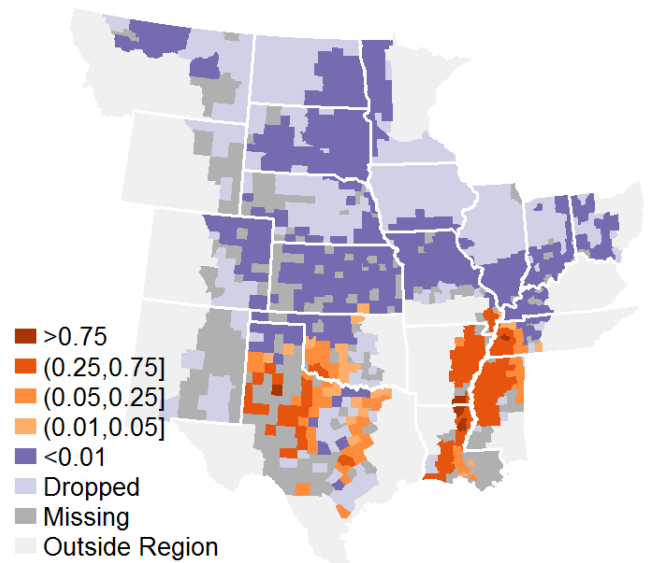
(a) Cash Rent



(b) Market Returns



(c) Direct Payments



(d) Proportion Cotton or Rice Base Acres

Figure 1: Maps of Key Variables

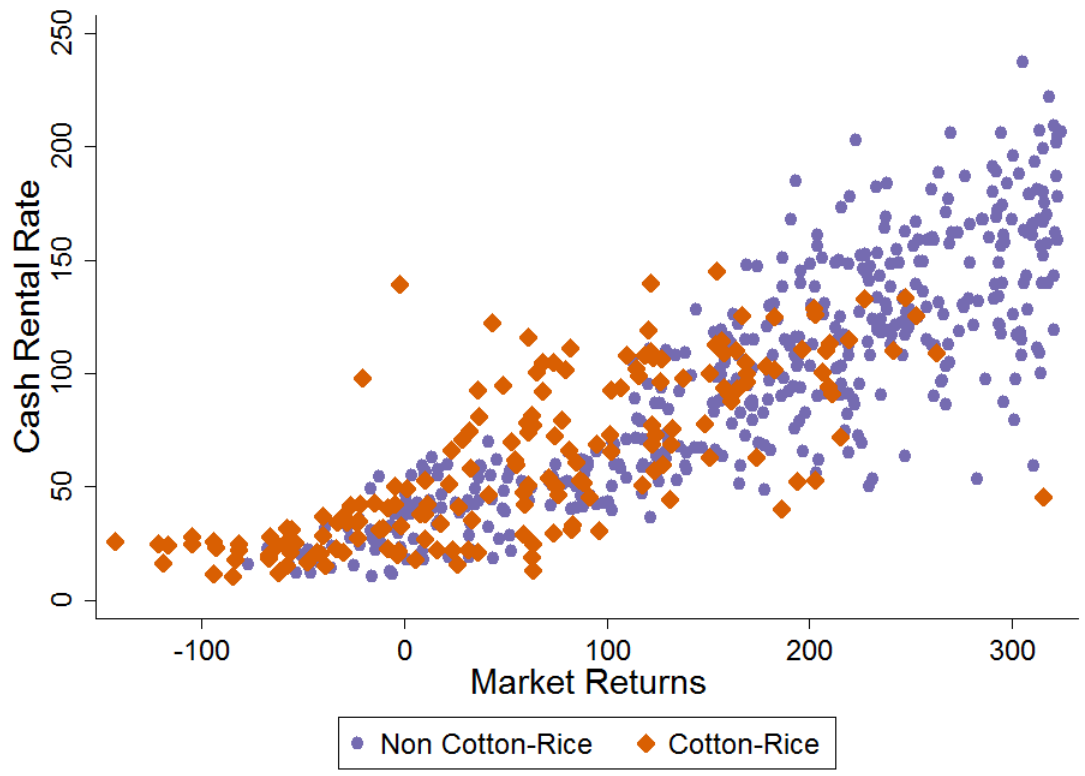


Figure 2: Rents and Market Returns

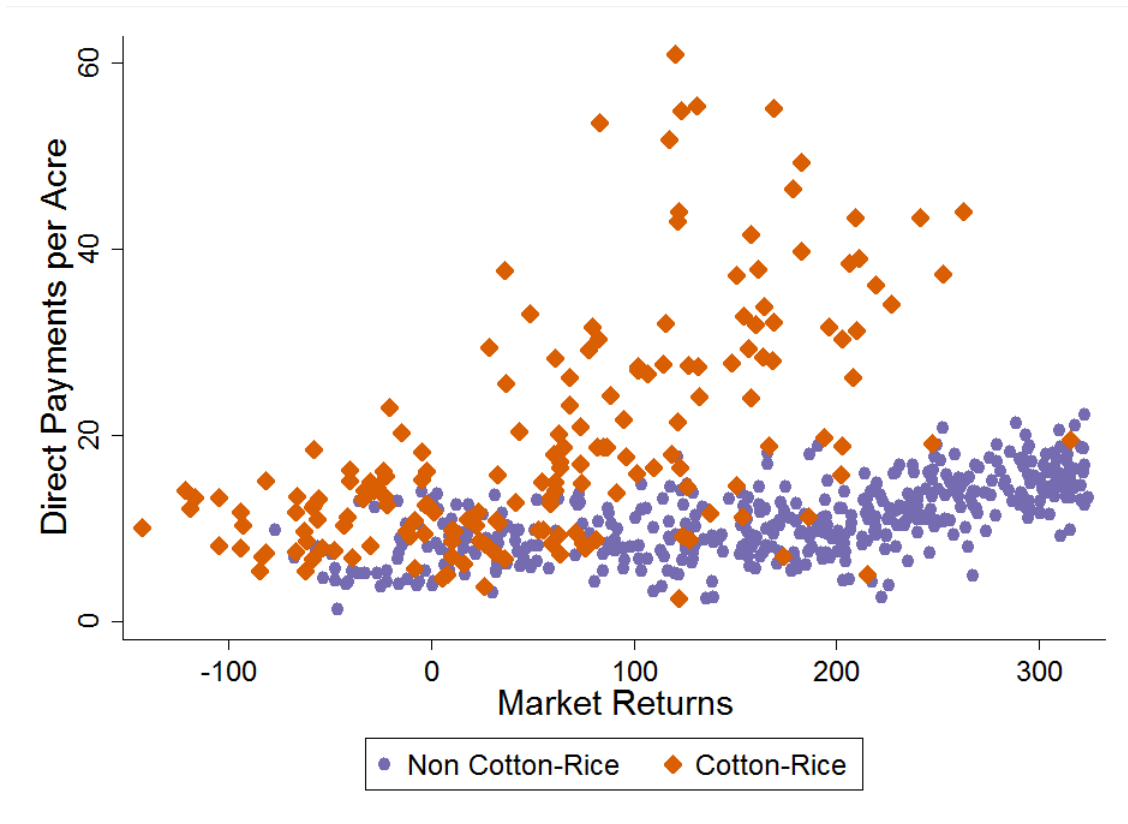


Figure 3: Direct Payments and Market Returns

Supplementary Appendix

Table A1 shows 2SLS results when we restrict our sample in different ways. Results in column (1) 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. In the paper, we include all counties where the crop acreage used to calculate the market returns is at least 25% as large as total cropland area. Column (1) uses 519 observations instead of the 639 observations in the main paper. Column (2) only considers counties with market returns less than \$275/acre instead of \$325 used in the paper. Column (2) only eliminates one county with substantial cotton or rice base acreage but 84 counties with little cotton or rice base (see figure 2 in the paper). Column (3) only considers counties with market returns between -\$80/acre and \$275/acre. Figure 2 in the paper shows that only counties with greater than 1% cotton or rice base acres have market returns less than -\$80/acre and mostly only counties with negligible cotton or rice base have market returns greater than \$275/acre. So column (3) considers the portion of the sample with the most overlap. Column (4) includes all counties with nonmissing data. That is, column (4) does not drop counties if crop acreage used to calculate market returns represents a small portion of cropland nor does it drop counties with market returns above a certain threshold. For brevity, we only report results with a linear control for market returns.

Table A2 shows 2SLS results when we use different methods of constructing variables used in the econometric analysis. Column (1) 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 cotton or rice 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 (2) and (3) in table A2 use different historical 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 (2) we average expected market returns over the 4-year period of 2009–2012 and in column (3) we average over the period of 2010–2012.

Table A3 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 2007-2011. Similarly, columns (2) and (3) use rental rates from 2010 and 2009 and market returns from the respective 5-year periods.

Table A4 reports 2SLS results if we do not control for ACRE enrollment. These are the exact same results as in table 5 of the main paper except that the results in table A4 omit ACRE enrollment.

Table A1: 2SLS Estimates with Different Samples

	(1)	(2)	(3)	(4)
	Returns	Returns	Returns	All Data
	>50% Cropland	<\$275/acre	<\$275/acre	
Direct Payments	0.789	0.827	0.758	0.826
	(0.188)**	(0.170)**	(0.171)**	(0.181)**
	[0.237]**	[0.218]**	[0.217]**	[0.224]**
Market Returns	0.350	0.344	0.357	0.395
	(0.011)**	(0.011)**	(0.011)**	(0.009)**
	[0.012]**	[0.011]**	[0.012]**	[0.010]**
Proportion ACRE	33.351	26.527	27.166	56.565
	(10.358)**	(8.392)**	(8.280)**	(9.380)**
	[10.358]**	[8.397]**	[8.284]**	[9.415]**
Intercept	28.410	26.501	25.238	19.971
	(2.640)**	(1.969)**	(1.966)**	(1.880)**
	[2.808]**	[2.121]**	[2.130]**	[2.001]**
Observations	519	554	541	1002

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 A2: 2SLS Estimates with Different Methods of Constructing Variables

	(1)	(2)	(3)
	Cropland Used for Crops	Returns 2009-2012	Returns 2010-2012
Direct Payments	0.753 (0.167)** [0.203]**	0.754 (0.158)** [0.210]**	0.893 (0.151)** [0.204]**
Market Returns	0.363 (0.010)** [0.010]**	0.325 (0.009)** [0.010]**	0.282 (0.008)** [0.008]**
Proportion ACRE	25.901 (7.411)** [7.413]**	31.875 (8.341)** [8.346]**	30.314 (8.289)** [8.293]**
Intercept	24.687 (2.196)** [2.354]**	23.912 (1.897)** [2.060]**	17.727 (1.890)** [2.050]**
Observations	635	626	594

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 A3: 2SLS Estimates with Rental Rates from Different Years

	(1)	(2)	(3)
	2011	2010	2009
Direct Payments	1.018 (0.169)** [0.221]**	1.366 (0.188)** [0.238]**	1.312 (0.189)** [0.233]**
Market Returns	0.341 (0.010)** [0.011]**	0.344 (0.011)** [0.013]**	0.398 (0.013)** [0.015]**
Proportion ACRE	22.409 (7.114)** [7.120]**	7.058 (6.729) [6.737]	11.647 (7.000)* [7.008]*
Intercept	26.742 (1.969)** [2.138]**	30.442 (2.082)** [2.297]**	37.744 (2.295)** [2.498]**
Observations	637	598	692

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 no Control for ACRE Program Enrollment

	(1)	(2)	(3)
Direct Payments	0.646	0.708	0.683
	(0.168)**	(0.166)**	(0.172)**
	[0.217]**	[0.216]**	[0.220]**
Market Returns	0.361	0.265	0.244
	(0.010)**	(0.017)**	(0.011)**
	[0.011]**	[0.018]**	[0.012]**
Market Returns ²		0.000	0.001
		(0.000)**	(0.000)**
		[0.000]**	[0.000]**
Market Returns ³			-0.000
			(0.000)**
			[0.000]**
Intercept	29.665	30.099	28.602
	(1.805)**	(1.731)**	(1.714)**
	[1.982]**	[1.914]**	[1.923]**
Observations	639	639	639

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.