# The Unintended Beneficiaries of Farm Subsidies

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**Abstract**: From 2011 to 2017, the U.S. government paid farmers annually \$6 billion in decoupled subsidies. Because around sixty percent of cropland is rented, if landlords raise rents in response to subsidy payments, the subsidies may not benefit the farmers as much as intended by policy. The Agricultural Act of 2014 linked subsidy payments to county characteristics and idiosyncratic yields. Instead of payments tied to farm-level productivity, which challenged identification under earlier programs, the programs offer a new path for identifying subsidy incidences. We find rents increase by approximately \$0.45-0.65 for every dollar received, roughly double what prior research found.

Appendix materials can be accessed online at:
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## 1. Introduction

From 2011 to 2017, the U.S. government provided roughly $6 billion per year in agricultural subsidies decoupled from production (Economic Research Service 2019).[1] Linked to historical planting decisions and given to actively engaged farmers, these subsidies are designed to provide income support to farmers without distorting market incentives. However, the Ricardian theory of rent hypothesizes that landlords can extract farm support payments indirectly through raising rental rates. With roughly sixty percent of cropland rented in the U.S. (USDA 2014; USDA 2017), this countervailing effect of higher rent caused by the subsidy payments, called the subsidy incidence, is a potentially economically meaningful transfer of wealth to nonfarmers.[2]

In this article, we estimate the size of the subsidy incidence using data from nationally representative cross-sections of farms for the 2015, 2016, and 2017 crop years. The Agricultural Act of 2014 changed how subsidies are determined and vary across farms. Instead of subsidies tied to farm-level characteristics like under the prior programs, the new Agricultural Risk Coverage-County (ARC-CO) program allocates payments by comparing the difference between realized county revenues and historical county average revenues. The new policy causes farms within a county to receive equal payments per acre conditional on base acre type, while farms in different counties could differ in payment levels. This policy means identical farms, with identical production histories, though residing in different counties could receive different subsidy payments due solely to differences in realized county yields. The cause of the variation in payments has important implications for identifying the subsidy incidence.

The challenge of identifying the subsidy incidence stems from how subsidies differ across farms. Before 2014, subsidy payments were determined mostly by the crops historically planted, their historical yield, and the crop's per-acre payment rate. Consequently, variation in subsidy payments across farms were in large part, conditional on each farm's historical productivity. Productivity confounds identifying the relationship between rent and subsidy payments because more productive land is more expensive to rent and receives higher subsidy payments.

To address this issue, Roberts, Kirwan, and Hopkins (2003) and Kirwan (2009) used first-differenced specifications to control for unobserved farm characteristics. However, time-variant variables like commodity prices influence subsidy payments and rent through unobserved productivity, confounding identification of the subsidy incidence. Kirwan and Roberts (2016) identify the subsidy incidence in a cross-sectional setting by directly controlling for productivity at the field-level. Their analysis, however, has limited generalizability due to small sample sizes for only three crops.

We use a regression kink design (RKD) to estimate the subsidy incidence under the new policy. Our approach uses the deterministic formulas for ARC-CO that creates variation in subsidy levels due to random variation of county revenues for a given year relative to a historical average benchmark. We find the subsidy incidence (\$0.45-0.65) is roughly double previous research has estimated (\$0.21 to \$0.34), indicating landlords benefit from subsidy payments by more than previously recognized.

## 2. Background on U.S. Farm Subsidies

Farmers have historically received subsidy payments through a system that has classified land based on the commodities historically produced on the land, called base acres. Under the subsidy programs leading up to 1996, the payments (officially called deficiency payments) were computed by multiplying a farm-specific program yield with the difference between the prices observed and a target price for each base acre type. The farmer would get income support if realized prices were lower than the target price. Program yields were calculated from historically observed yields for the farmer from 1981 to 1985.[3] This system of payments effectively paid higher payments to producers who were more productive in 1985. Eligibility to receive the subsidy payments required the farmer to plant the same crop as the base acre commodity.[4] This policy tied production decisions to subsidy payments, leading some to hypothesize the subsidies caused an overproduction of commodities and depressed prices (Bonnen and Schweikhardt 1998).

The Federal Agriculture Improvement and Reform Act of 1996 removed the planting restrictions for eligibility by replacing the program with production flexibility contracts (PFC). The PFC payments were tied to base acres but were computed by multiplying a new fixed commodity-specific amount by the same program yield as the prior program. The removal of the planting restrictions, in theory, led to a decoupling of subsidy payments and planting decisions. Hendricks and Sumner (2014), as well as Goodwin and Mishra (2006), show limited effects of the subsidies on production decisions, reinforcing the decoupling hypothesis. However, Devadoss, Gibson, and Luckstead (2016) theorized the payments are relevant to production decisions because of their impact on farmer's entry-exit decisions.

Farm subsidies notably changed again with the passing of the Agricultural Act of 2014 when Congress repealed direct payments, as well as smaller conditional programs, and replaced them with two conditional programs: the Agriculture Risk Coverage (ARC) and the Price Loss Coverage (PLC).[5] While decoupled from production decisions, but still tied to base acres, the programs switch the subsidy programs from mostly guaranteed payments to conditional ones. The general public's concern with farmers receiving large subsidy payments while simultaneously earning record high incomes may have motivated the change (Kilman 2011).

Farms were tasked with electing their base acres to either ARC or PLC for the five-year duration (2014 to 2018) of the Act to prevent redundancy in coverage. Within ARC, farmers had to choose between two different programs: Individual Coverage (ARC-IC) set payments based on individual farm-level outcomes, while County (ARC-CO) set payments conditional on base acre type and county average revenue. Less than 1 percent of farms chose ARC-IC for the 2014-2018 period, making ARC-CO the dominant ARC program.

**Commodity Income Support Programs: ARC-CO and PLC.**

For ARC-CO, farmers receive subsidy payments if average county revenue falls below a historical benchmark. For a given commodity, per-acre ARC-CO payment rates are calculated as:

$$\text{ARC-CO payment (\$ per acre)} = \begin{cases} 0, & \text{if AR} \geq \text{BR} \cdot 86\% \\ 86\% \cdot \text{BR} - \text{AR} & \text{if } 86\% * \text{BR} > \text{AR} > 76\% \cdot \text{BR} \\ 10\% \cdot \text{BR} & \text{if AR} \leq 76\% \cdot \text{BR}. \end{cases} \quad [1]$$

Benchmark revenue (BR) is calculated using an Olympic average of a county's average yield (using the five most recent crop years) multiplied by the Olympic average of the marketing year average national price.[6] Actual revenue (AR) is the Farm Service Agency (FSA) calculated average yield for the county multiplied by the marketing year average national price ($P$) (Farm Service Agency 2018). Per-acre payments are capped at ten percent of benchmark revenue.

Similar to the deficiency payments, the program in place before 1996, the per-base acre payments for PLC are:

$$\text{PLC payment (\$ per acre)} = \begin{cases} 0 & \text{if P} \geq \text{RP} \\ (\text{RP} - \text{P}) \cdot \text{PLC Yield} & \text{if RP} > \text{P} > \text{ML} \\ (\text{RP} - \text{ML}) \cdot \text{PLC Yield} & \text{if P} \leq \text{ML}. \end{cases} \quad [2]$$

Farmers receive a subsidy if the national marketing year average price (P) is less than the reference price (RP), which is set by policy. The difference between the two prices is multiplied by a fixed individual farm-specific reference yield called the PLC Yield. The PLC Yield is similar to the prior deficiency payments program yields, though updated with 2008-2012 yields. The payment is capped at the marketing loan rate (ML). If the price falls below the marketing loan rate, the farmer begins to receive additional government program benefits. In the first four years of program existence, prices never fell below the marketing loan rate for any commodity, thus PLC payments were never capped (Boussios and O'Donoghue 2019). The total subsidies a farm received is equal to the sum of the farm's base acres for each program multiplied by the per-acre payment for each respective base acre commodity, multiplied by eighty-five percent. O'Donoghue et al. (2016) and Boussios and O'Donoghue (2019) provide further background into these programs' policy rules.

Farmer's election preferences for ARC and PLC varied by crop and location. For the 2015, 2016, and 2017 crop years, ARC-CO was the preferred program at the national level at a ratio of three-to-one. Within Midwestern states, the ratio was higher for ARC-CO to PLC enrollment at a rate of seven-to-one. The disparity in program choice was due primarily to corn and soybean base acres, which made up sixty-two percent of all base acres and selected ARC-CO on ninety-two percent of those acres. Farmers' election choices outside of the Midwest split evenly between the two programs. This difference is driven heavily by the election choices of farmers with wheat, rice, sorghum, and peanut base acres, which account for larger shares of production outside of the Midwest. Seventy percent of payments nationally, and ninety percent in the Midwest, were from ARC-CO in 2015, 2016, and 2017. The differences in enrollment and payments are important considerations for identifying the subsidy incidence. The appendix provides a breakdown of the enrollment and payment disparities by region, base acre type, and program year.

We estimate the subsidy incidence by strictly considering payments from ARC-CO. Motivating this choice is that ARC-CO is the preferred program by base acres, especially for corn and soybeans, and for farmers in the Midwest. Moreover, ARC-CO was the biggest program by expenditures in all three sample years used here. Most importantly for identifying the subsidy incidence, we use the fact that payments under the ARC-CO program are not tied to farm-level characteristics, which may confound the relationship between rental rates and subsidy payments.

3. Identifying the subsidy incidence

To motivate our identification strategy, first consider the identification problem under prior subsidy programs in the context of the following linear model:

$$r_i = \beta_0 + \beta_1 s_i + \varepsilon_i, \qquad [3]$$

where $r_i$ is the per-acre rental rate for farm $i$, $s_i$ is the per-acre subsidy, and $\varepsilon_i$ is the error term. $\beta_1$ is the incidence of government subsidies on rents. The Ricardian theory of rent argues the landlord can raise rental rates equivalent to the value of the subsidy payment if decoupled from production, thus $\beta_1$ is predicted equal to one. Conversely, we can estimate how much of a subsidy dollar the farmer receives by subtracting $\beta_1$ from one. With higher values of $\beta_1$, landlords benefit more.

While this short-run subsidy incidence is predicted equal to one by the Ricardian theory of rent, previous empirical results have found $\beta_1$values are closer to zero than one. Roberts, Kirwan, and Hopkins (2003) found a range of subsidy incidence values between 0.21 and 0.34, while Kirwan (2009) found an incidence of 0.21, and Kirwan and Roberts (2016) found a range conditional on crop type from 0.20 to 0.28. Market power in rental markets (Kirwan 2009) and the rigidity of contracts (Hendricks, Janzen, and Dhuyvetter 2012) have been hypothesized for explaining why the estimated values differ from the Ricardian theory.

In the model above, the subsidy incidence $\beta_1$ is identified if we assume that subsidy payments are uncorrelated with the error term ($\varepsilon_i$). Under prior subsidy programs (e.g., Direct payments), which tied the size of the payment to historical productivity, this assumption is untenable. More productive land commands higher rents and subsidy payments; thus naïve OLS estimates of the model above would be upwardly biased.

Research has tried to address this endogeneity issue in two ways. Roberts, Kirwan, and Hopkins (2003) and Kirwan (2009) estimate their models in first-differences to control for unobserved time-invariant heterogeneity across farms, such as the farm's productivity. Time-varying factors, like commodity prices, however, pose problems for this identification since they are correlated with changes in subsidy payments and rental rates. Notably, as well, Kirwan (2009) used observations on farm subsidies in 1992 and 1997. These sample years measured farms under two different subsidy programs, with the earlier year requiring planting restrictions for eligibility (deficiency payments), while the latter year did not (Production Flexibility Contracts). The costs associated with eligibility in one period but not the other would likely further challenge identification. Kirwan and Roberts (2016) identify the subsidy incidence under the Direct Payments program by directly controlling for productivity, using the farmer's ex-post "yield goal" as a proxy for productivity. With field-level data, their survey samples were limited to a small number of crops and observations.

**Identification Strategy**

To see our identification strategy, and how ARC-CO program rules affect identifying the subsidy incidence, consider a simplified case where only one crop is produced, and farmers only receive ARC-CO payments. Also, assume that all farmland is enrolled in ARC-CO. First, we re-write the model above so that now *c* indexes counties:

$$r_{i,c} = \beta_0 + \beta_1 s_c + \varepsilon_{\mathrm{i,c}} \quad [4]$$

where farm $i$ remains the observational unit, but now subsidy payment rates are the same within counties. The fact that subsidy payment rates are determined at the county level is by itself no solution to our identification problem since unobserved variables, such as farm productivity, may correlate with rent and the variables at the county-level that determine subsidy payments.

[Insert Figure 1]

To identify the subsidy incidence, we want to control for the factors that determine subsidy payments. Subsidy payments from the ARC-CO program are a deterministic function of county-level AR and BR, shown in equation [1] and illustrated in Figure 1. If AR is less than 86 percent of BR, the farmer receives a subsidy payment equal to the two values' difference. If AR is below 76 percent of BR, the farmer receives 10 percent of BR For all values of AR above 86 percent of BR, the farmer receives nothing. The kinked assignment for subsidy payments provide variation in the effect of AR and BR on rent that is uncorrelated with farm-level attributes, thus allows us to identify the subsidy incidence, as follows:

$$\mathrm{r}_{\mathrm{i,c}} = \beta_0 + \beta_1 s_c + \beta_2 BR_c + \beta_3 AR_c + \epsilon_{\mathrm{ic}}, \quad [5]$$

where county-specific benchmark revenue ($BR_c$) and actual revenue ($AR_c$) are defined as in equation [1]. To further detail how the deterministic formulas allow us to identify the effect of payments, we replace the subsidy variable $\mathrm{s_c}$ in [5] with the formulas from [1] and include dummy indicator variables, as follows:

$$\mathrm{r}_{\mathrm{i,c}} = \beta_0 + \beta_1 \cdot 0.85 \cdot [\mathrm{D}_1 \cdot (0.86 \cdot BR_c - AR_c) + \mathrm{D}_2 \cdot (0.1 \cdot BR_c)] + \beta_2 BR_c$$
$$+ \beta_3 AR_c + \epsilon_{\mathrm{ic}}, \quad [6]$$

The dummy indicator ($\mathrm{D}_1$ and $\mathrm{D}_2$) variables are defined as:

$$D_1 = 1 \text{ if } 86\% \cdot BR_c > AR_c > 76\% \cdot \mathrm{BR}, 0 \text{ otherwise}$$
$$D_2 = 1 \text{ if } AR_c \leq 76\% \cdot \mathrm{BR_c}, 0 \text{ otherwise.} \quad [7]$$

The subsidy formulas in (6) are multiplied by 0.85 because farms receive payments on only 85 percent of eligible base acres.

The subsidy incidence ($\beta_1$) in either (5) or (6) is identified equivalently, assuming $\mathrm{E}(r_{i,c}|s_c, AR_c, BR_c)$ is linear in the explanatory variables. In equation [6], however, rental rates are modeled entirely by AR, BR, and dummy indicators, which allows for the estimating equation to identify treatment effects in the spirit of the sharp regression kink design (RKD) as explained by Card et a. (2012) and applied elsewhere (Landais 2015; Lundqvist, Dalhberg, and Mork 2014; Nielsen, Sørensen, and Taber 2010; Card et al. 2016). Intuitively, if subsidy payments causally affect rent, we should observe a kink in AR and BR's relationship on rent where changes in subsidies are observed. Landais (2015) more formally phrases the RKD technique, "… RKD estimation only relies on the estimation of the numerators of the estimand, which is the change in the slope of the conditional expectation function of the outcome given the assignment variable at the kink (p.261)."

Applied to the ARC-CO setting here, the RKD identifies the subsidy incidence by the change in AR and BR's marginal effect where subsidy formulas change. As an example, consider farms in different counties with a given level of BR values. One set of farms resides in counties where AR fell below the subsidy threshold, thus triggering payments. The other set of farms resides in counties where AR was high enough not to trigger payments. Assuming the standard RKD assumptions, a causal effect of subsidy payments is identified by a change of the slope AR and BR where subsidy payments are triggered. Farms in the counties where low AR values caused subsidy payments to be triggered have a different slope $((-\beta_1 \cdot 0.85 + \beta_3) \cdot AR_c)$ than farms where payments were not triggered $(\beta_3 AR_c)$.[7] By measuring the slope on each side of the formula kink(s), the incidence $(\beta_1)$ is recovered by the difference between the two marginal effects on either side of the kink. The deterministic relationship between subsidy payments and AR and BR allows us to identify the subsidy incidence with the RKD techniques.

One challenge of applying the techniques standard in the RKD literature to the setting here is that subsidy payments are a deterministic function of not one but two variables, and thus the kink(s) where subsidies are triggered is a continuum of points that are conditional on the values of both AR and BR. For this reason, we had to preface the prior paragraph with a given level of BR. This is important not just in terms of estimation but also in using graphical methods to help visualize the shape of the regression function and assess the magnitude of the effects (Lee and Lemieux 2010). Visually examining an assignment variable's distribution also allows for assessing whether treatment manipulation could pose a problem. Graphs are naturally more challenging to interpret with two assignment variables, as it requires three-dimensional figures.

Alternatively, we can reformulate the problem in terms of a single assignment variable by dividing all the variables by BR. $S/\mathrm{BR}$ is a deterministic function of a $\mathrm{AR}/\mathrm{BR}$, with kinks. This can be seen similarly graphically by dividing the variables on the x- and y-axes in Figure 1 by BR, where they are now $\mathrm{AR}/\mathrm{BR}$ and $\mathrm{S}/\mathrm{BR}$, respectively. The estimating equation is then

$$\frac{r_{i,c}}{BR_c} = \alpha_0 + \sum_{\mathrm{p}=1}^{\bar{\mathrm{p}}} \gamma_{\mathrm{p}} \left(\frac{AR_c}{BR_c}\right)^{\mathrm{p}} + \delta \frac{AR_c}{BR_c} D_1 + \sum_{\mathrm{j}=1,2} \mu_{\mathrm{j}} D_j + \omega_{\mathrm{i,c}} \qquad [8]$$

, where p is the order of the polynomial with maximum size $\bar{p}$. Estimation and graphical presentation of the analysis are now of a model with just a single assignment variable $AR/BR$ with kinks at 0.76 and 0.86. The subsidy incidence is recovered by dividing $\delta$ by -0.85 to scale the change $S/BR$ with changes in $AR/BR$ at $D_1$. Figure 2 presents a visualization of a hypothetical case of a positive subsidy incidence and linear effect of $AR/BR$ on $r/BR$. The kinks in the line highlight how a subsidy effect can be observed by just plotting the assignment and outcome variables. If subsidies were to have no impact on rent, the line would be straight, or at least smooth, for all values of the assignment variable.

[Insert Figure 2]

Equation [8] relaxes the assumption of a linear relationship of the assignment variable. Estimation with different polynomials is used to rule out specifications that may inaccurately attribute a change in the marginal effect of the assignment variable at the kink to treatment effects. Akaike Information Criterion (AIC) evaluate which polynomial specification best explains the relationship between the assignment and outcome variable (Lee and Lemieux 2010).

The validity of the RKD identification strategy relies in part on the two assumptions from the RKD literature (Nielsen, Sørensen, and Taber 2010; Card et al. 2012); that both the marginal effect of the assignment variable on the outcome and the density of the assignment variable is smooth around the kink. The first assumption means the marginal effect of $AR/BR$ is smooth across either side of the kink, and a discontinuity in the marginal effect at an assignment kink is due to subsidy payments. The second assumption relates to whether we can credibly believe the assignment of individuals along the assignment variable is as good as random, or at the very least, individuals are unable to precisely manipulate assignment locally around the kink (Lee and Lemieux, 2010).

In many instances found in the RKD literature (Landais, 2015; Nielsen, Sørensen, and Taber 2010; Card et al. 2015), manipulation concerns stem from the individual's ability to determine both the assignment (e.g., income) and treatment variable (e.g., unemployment benefits). In the papers that consider unemployment as the treatment variable, the authors argue that the mechanics of the unemployment formulas and the uncertainty of receiving future unemployment benefits make it unlikely an individual would have enough incentive and foresight to precisely alter their income today to influence future benefit amounts. In our setting, manipulation concerns appear even less likely. An individual farm has no direct ability to change program enrollment or county-level variables' values.

## 4. Data

We use farm-level data from the USDA's Agricultural and Resource Management Survey (ARMS) Costs and Returns Report (CRR).[8] The CRR portion of ARMS is an annual nationally representative survey that attempts to collect business and household information from 30,000 farms in 48 contiguous states. RKD estimation requires estimating the assignment variable's slope on each side of the kink(s), making it particularly sensitive to outliers (Simonsen, Skipper, and Skipper 2016). For that reason, our core results pool sample years 2015, 2016, and 2017 to increase the number of farms observed around the subsidy assignment kink(s). As a robustness check we also estimate [5], which includes the subsidy variable. If the effects are linear in the explanatory variables and the variables measured without error, the two estimation results would be equivalent. With the robustness check, we estimate each cross-section separately and find qualitatively similar results for each sample year.

ARMS CRR records data at the farm-level. We transform our variables to a per-acre basis to match our model specification. Table 1 presents summary statistics for the variables used in this study, computed for 2015, 2016, and 2017, using ARMS population weights. Most of the acreage in the samples are devoted to planting either corn or soybeans, and each farm averages about 1,100 acres. We also find roughly 60 percent of operated land is rented, highlighting the research question's relevance. The subsidy incidence represents a more economically significant transfer of wealth with higher proportions of rented land.

[Insert Table 1]

We measure farmland rental rates as the average per-acre cash rental rate, calculated by dividing total land rented for fixed cash rent by the total amount of cash rent paid.[9] Per-acre subsidy payments are constructed from the surveys by dividing total ARC-CO payments as reported in ARMS CRR by total acres in operation.[10]

Farms typically manage the production of multiple commodities across multiple base acre types. Consequently, a farm's average per-acre AR and BR values are calculated across multiple commodities, distorting the clear discontinuities in the mapping of AR and BR to subsidies. Moreover, ARMS does not record base acre types or program enrollment for the farm. Due to the ARC-CO program's decoupled nature, we cannot perfectly infer a farm's base acre types or program enrollment from any variable in the dataset. This unobserved variable creates issues even with other field-level collection phases of the ARMS survey. Unobserved base acre crop type and program enrollment can confound our estimates if correlated with the error term.

Our solution to the unobserved aggregation of base acres within a farm is to construct the variables $\widetilde{\mathrm{AR}}_{\mathrm{c}}$ and $\widetilde{\mathrm{BR}}_{\mathrm{c}}$ by weighting AR and BR by the county's base acre average. This limitation of the data introduces measurement error into our control variable(s), which could pose problems for our identification strategy (Davezies and Le Barbanchon 2017). Though if the measurement error is uncorrelated with treatment, we can still identify average treatment effects under the RKD (Battistin et al. 2009). We believe our solution to this data issue is supported because the same county-level values would construct the farm's per-acre AR and BR values, the approach here is simply weighted according to the county average instead of the farm-level.

We separate the national sample into Midwest and non-Midwest farms to confront the unobserved program enrollment decision. Midwest is defined by the following states: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, and Wisconsin. Midwest farms enrolled in ARC-CO versus PLC at a seven-to-one ratio, while non-Midwest farm's enrollment ratio was closer to one-to-one. Ninety percent of decoupled subsidies in the Midwest during the three years included here were delivered from ARC-CO. We would expect no discontinuity in the assignment variable's relationship on rent at the kink if the farm is not enrolled in ARC-CO.

To increase the precision of our estimates and potentially control for unobserved factors that correlate with the assignment variable's measurement error, we include additional covariates in most models. Ando (2017) showed the inclusion of additional covariates in RKD can reduce bias. In addition to estimating the subsidy incidence with RKD techniques, we also estimate the effects of subsidy payments using model [5] with additional covariates. The additional control variables, farm-level revenue, crops produced, and crop-specific yields, are shown in Table 1 and calculated as follows: farm-level average per-acre actual revenue is the total revenue across all crops divided by the total land operated; crop proportions are the quantity of crop produced over total land operated, and crop-specific yields are the quantity of the crop produced divided by the area it was produced on.

Another relevant component to understanding the data and the subsidy incidence is the timing of cash rental agreements and the ARC-CO program. For spring-planted crops, such as corn or soybeans, farmers plant their crops around April through May. Farmers must make rental agreements before yields and prices are known, requiring using expectations of outcomes rather than realized outcomes to make decisions. Paulson and Schnitkey (2013) and Bigelow, Borchers, and Hubbs (2016) provide detailed backgrounds on agricultural land rental markets.

The implementation of ARC-CO requires that the government collects two data points each year, county average yield and the average national marketing year price. County yields are reported near the beginning of the crop year (winter-spring), while average annual prices require the completion of the marketing year to calculate and report. This timing means for an example year of the 2015/16 crop year (referenced as the 2015 crop year), the farmer and landlord set their rental contract in winter-2014/spring-2015, plant their crop in April-May 2015, harvest in the fall-2015, realize county-average yields in spring-2016, and finally realize national marketing year prices in September-2016.

ARC-CO payments for a given marketing year are contingent on outcomes from the prior marketing year. For example, the 2016 crop-year payments depend on 2015 outcomes. Due to this timing, expectations of future production and subsidy payments from the 2015 crop-year influence rental rates for 2016. As opposed to the actual amount of subsidy received, the expectations create a measurement error problem (Roberts, Kirwan, and Hopkins 2003; Kirwan 2009). This type of measurement error has been deemed random, thus creates attenuation bias, causing downward biased incidence estimates.

## 5. Graphical Evidence of Subsidy Payments on Rent

Figure 3 provides visualizations of the assignment (AR/BR) and treatment variable (S/BR). The variables are graphically presented to examine the assignment variable's frequency and the relationship between assignment and reported treatment. The x-axes of the figures in the second and third columns of figure 3 were constructed from the median values of observations within equidistant bins of the assignment variable. The S/BR variable is the average value from farms within each bin. The percent reporting zero payments in the y-axis of the third column of graphs is the percent of farms within each bin reporting having received zero ARC payments.

The histograms indicate that farms' assignment along AR/BR appears to evolve smoothly across thresholds and levels, showing that producers do not self-sort into receiving payments. The figures also highlight the frequency of AR/BR values below the payment thresholds, suggesting a high frequency of farms receiving payments. We expect that assignment along AR/BR to stem from the random variation of county yields for a given year relative to a historical average benchmark.

[Insert Figure 3]

The binned scatterplots show the S/BR and AR/BR relationship is similar in shape to ARC-CO rules, suggesting a reasonable job on average of reflecting the deterministic variables for each farm in the Midwest. Less so for the non-Midwest sample. However, in both the Midwest and non-Midwest samples, the figure shows the S/BR values fall far below the policy's true cap (0.085). We believe the discrepancy could be due to unobserved differences in enrollment, measurement error in the assignment variable, and error in the reported subsidy amount. The RKD approach assumes all acreage is enrolled in ARC-CO, which we, however, know is untrue. By selecting farms in the Midwest alone, we reduce this enrollment discrepancy for a large portion of the sample, as eighty-four percent of the planted acreage in the Midwest chose ARC-CO. We believe, for this reason, the S/BR variable is observed higher in the Midwest sample, but still below the cap.

The scaled-down but similar shape of the relationship between the Midwest sample variables and that from the deterministic relationship indicates that reported subsidy values are likely unrelated to assignment values. If the measurement error were strictly in the assignment variable, one would suspect variation in the bins would include values above and below the cap. However, we only observe bin values far below the cap.

We believe the discrepancy is in large part due to an under-reporting of payments by farms. Under-reporting of payments in surveys, particularly the reporting of zero payments, has been well-documented in surveys that include government transfer programs (Meyer, Mok, and Sullivan 2009; Meyer and Mittag 2019; Meyer, Mok, and Sullivan 2015). Using the population weights of the ARMS survey to aggregate to a national sum, we found ARMS reported payments totaled $2.5, $3.6, and $2.4 billion, compared to actual receipts from Farm Service Agency of $4.4, 5.9, and $3.7 billion for 2015, 2016, and 2017, respectively.[11] Implying, farms, on average, are only reporting about sixty percent of what is paid. With an inability to cross-reference survey data with receipts, we are unable to determine when under-reporting occurs. On inspection of the data, we speculate that under-reporting is in large part due to the reporting of zero payments. The third column of sub-figures in figure 3 presents the percent of farms reporting zero payments for bins of AR/BR. The figure indicates noise and variability in the treatment variable along $AR/BR$, with a slight shift of the kinks to the right, but it generally follows the formulas' pattern. However, the large discrepancy from the formula is that bins that would be expected to have received positive payments still included over forty percent of farms reporting zero payments.

The underreporting of subsidy payments suggests the error in the relationship between S/BR and AR/BR is in part due to the misreporting by farms. Notably, with the RKD technique, the subsidy incidence is estimated by the change in the assignment variable's marginal effect and does not include the farmer reported subsidy amount in estimation. This provides the RKD technique one advantage over prior identification approaches that rely on farmer reported payments, as under-reporting of payments would downward bias traditional incidence estimates.

Figure 4 presents a binned scatterplot of the assignment and outcome variable, as well as a locally weighted regression of the two variables for the Midwest sample. The scatterplot was constructed from the median $AR/BR$ value of farms with bins of 0.02 width and their corresponding average value of $r/BR$. The scatterplot and predicted values from the locally weighted regression provide semiparametric visualizations of the effect of the discontinuous relationship of the assignment variable on $r/BR$. Similar to the example in figure 3, there are two kinks in the effect of the assignment variable on the outcome variable. The discontinuous kinks would only be expected in the relationship of the assignment variable on the outcome variable if subsidies had a causal effect.

[Insert figure 4]

Curiously, the kinks are to the right of ARC-CO formula thresholds. We believe the rightward shift is due to aggregation error in the assignment variable. Consider a farm in a county with two base acre types; corn and soybeans. Subsidy payments could be triggered for soybean base acres, but high AR/BR values for corn base acres could cause the average AR/BR value for a county to appear above a payment threshold. Aggregating multiple crops and payments being strictly positive, a rightward shift of the kink would be expected if subsidies caused higher rent. The double kinks, and the rightward shift, are supportive evidence that subsidy payments cause higher rent.

Though figure 4 indicates a positive effect of subsidies on rent, the shift in the kinks caused by the aggregation error and the reported subsidy values' error present challenges to using some RKD techniques. Locally weighted polynomial regressions, a staple of RKD techniques (Lee and Lemieux 2010), reduce the range of observations within the assignment variable in which researchers are required to assume randomization occurs. Rather than assuming the treatment variable is random across the entire sample, restricting the sample and estimation to narrow bandwidths around the treatment kink provides a more reasonable randomization assumption and more flexible specifications of marginal effects. Locally weighted polynomial regressions also allow for permutation tests to assess if the change in the marginal effect at the kink is due to functional form assumptions or causal effects (Ganong and Jager 2018). Fuzzy RKD techniques are helpful in the case of imperfect compliance in a kinked treatment setting (Card et al. 2015), but are challenged by measurement error in the reported treatment variable. The locally weighted polynomial regressions and the fuzzy RKD assume there is no measurement error in the assignment or reported treatment variable. Further, the aggregation of the county-level variables to the farm creates “mass points”, as farms within a county have the same assignment variable value. The mass points, combined with the measurement error, create issues for local regression techniques (Cattaneo, Idrobo, and Titiunik 2019).

6. RKD Estimates of the Effect of Subsidy Payments on Rent

We measure the subsidy incidence using what is often referred to as a global, parametric RKD approach (Cattaneo, Idrobo, and Titiunik 2019). Rather than estimate on narrow samples around the kink(s), we estimate equation [8] across the entire sample. The limitation from including the entire sample is that rather than assuming individuals cannot manipulate treatment in proximity to the kink precisely, we must assume this is true across the entire sample. This stronger assumption is likely more plausible for ARC-CO payments than in other RKD settings, such as unemployment benefits (Landais 2015; Nielsen, Sørensen, and Taber 2010; Card et al. 2015). However, the advantage with the global parametric approach is that it is less sensitive to individual observations, which have been shown to be problematic in RKD approaches (Ganong and Jager 2018; Simonsen, Skipper, and Skipper, 2016).

A second challenge in applying RKD techniques in our setting is that the graphs presented above (figures 3 & 4) show that kinks in subsidy payments and rent given the assignment variable appear not to align perfectly with the program rules (figures 1 and 2). The kinks on the subsidy figure appear to be at 0.76 and 0.95, while the kinks on the rent figure are at 0.85 and 1.10. We believe the discrepancy between the kinks in the figures and the policy rules is due to poorly measured farmer-reported subsidy payments and measurement error in the assignment variable caused by the aggregation of multiple base acres into a single base acre type. One advantage of RKD is that it does not require the measurement of the subsidy variable for estimation. However, measurement error of the assignment variable presents challenges to estimation of the subsidy incidence as the kink(s) locations are not known. Specifying the kink(s) at incorrect points, even if suggested by the policy rules, could lead to biased estimates. Our RKD estimator exploits the slope change in mean rents as a function of the assignment variable and the change in the subsidy formulas. If subsidy formulas do not change at the specified kink(s), it is unclear exactly what the RKD would estimate.

Accordingly, we use a grid search technique that selects the kink point values based on model fit, similar to Porter and Yu (2015), and as applied to RKD by Hansen (2017). The technique selects the points that best explain the kinked relationship between the assignment and outcome variables. This approach does not rely on farmer-reported subsidy data to select the kink points. We also examined the model fit without dummy variables as a placebo test to examine whether the kinked specification meant an improved model fit. AIC values indicate a greater fit of the model with the kinked assignment variable, suggesting a kinked relationship between the assignment and outcome variable.

Table 2 presents the estimated subsidy incidence from RKD models with optimally selected kink points (0.845 and 1.010) across different polynomials and the inclusion of control variables. The model-selected kink points are similarly shifted to the right of the policy levels, with the positive subsidy payment kink point slightly further to the right. Estimating RKD specification as presented in [8] with a linear polynomial, the incidence was found to be 0.537. The inclusion of the additional controls reduced the incidence to 0.378. Quadratic and cubic polynomials of the assignment variable caused the incidence estimate to vary only slightly. AIC values suggest the linear specification was optimal. We additionally estimated fifth and sixth specifications that estimate the subsidy incidence separately at each kink. Estimating each kink individually allows for more flexible parametric specifications, as it does not require the marginal effect of AR/BR to the left of the left kink and to the right of the right kink to be the same. Despite slightly different subsidy incidences, the results from estimation of each kink were similar, with the marginal effects of AR/BR where payments were capped not statistically different from where no payments were distributed.[12] The lack of evidence of a difference provides support to the global estimation technique, as local estimation provided similar results, but it also supports the identifying assumptions behind the RKD. Similar marginal effects of AR/BR at the extreme values suggest marginal effects are smooth and linear across the sample, thus the discontinuity observed in the visualization can credibly be explained by a positive causal effect of subsidies on rent.

[Insert table 2]

The reduction in incidence with the inclusion of controls suggests a correlation between the assignment and farm-level variables. Measurement error in the assignment variable is the expected culprit. We also suppose the RKD subsidy incidence found here is biased downward due to fewer acres enrolled in ARC-CO than planted. Similar to the need to scale the RKD coefficient by -0.85, as farmers are only eligible to receive payments on eighty-five percent of base acres, only eighty-four percent of acres in the Midwest were enrolled in ARC-CO. Scaling the coefficients to account for enrollment differences would suggest a range of incidence values from 0.45 to 0.64. In comparison, Kirwan and Roberts' (2016) estimates ranged from 0.210 to 0.285.

**Estimates of subsidy incidence using reported payments**

We also estimate the subsidy incidence similar to equation [5] with additional covariates as a robustness check to specification choice and measurement error in the county-level controls. We estimate each sample year separately using both the national and Midwest samples, weighting the regressions by the ARMS sample weights. Table 3 presents the estimated coefficients of these models, including a second specification that only includes farms that harvested corn. The second specification is expected to better control for any unaccounted for productivity differences. However, the stronger productivity control provides a tradeoff in that the restricted subsample could be less representative of the national or Midwest subsidy incidence.

[Insert Table 3]

The second set of models, which include the reported subsidy variable as a covariate, find higher estimates than the prior RKD models. Coefficient estimates are slightly larger for the national sample relative to the Midwest, except for the 2017 sample. The inconsequential difference between the national and Midwest sample estimates is likely due to the large population weights of farms in the Midwest.

The coefficient estimates from the set of models that include corn yields found slightly lower subsidy incidence levels. Due to the aggregation of multiple crops, farm revenues may less accurately represent the land's productivity than yields. The subsidy incidence was smaller with the inclusion of a stronger control for productivity, though notably remains above 0.5 across all specifications and bandwidths. A third set of model results are presented in the appendix, where we estimated the subsidy incidences by combining all three sample years to construct a dynamic county-cohort pseudo-panel. The approach found a slightly smaller incidence (0.44) but is similar in magnitude to the main results.

## 7. Conclusions

Various economic theories of land rent postulate subsidies are passed through the farmer to the landlord. However, prior research has shown relatively small subsidy incidence values. Here, we find estimates for the subsidy incidence that meaningfully surpass those found in prior research. Our results suggest between $0.45 and $0.65 of every subsidy dollar is passed through the farmer to the landlord through higher rent. With sixty percent of farmland rented and roughly $6 billion in subsidy payments, the estimates project the programs equate to transferring about $1.6 and 2.3 billion from taxpayers to landlords each year.

We hypothesize the magnitude of the subsidy incidence found here relative to prior results is due to a new identification approach that uses the kinked assignment for subsidy payments to identify subsidy effects. Payments are no longer strictly determined by individual farm characteristics but variations in county-level variables that are uncorrelated with farm-level attributes. Importantly, RKD identifies the subsidy incidence by the discontinuous change in the slope of the assignment variable at the kink(s), meaning it is not reliant on self-reported subsidy payments to identify impacts. Underreporting of the payments could downward bias estimates from non-RKD estimation techniques. Though the RKD technique and new subsidy program provide two possible explanations for the higher subsidy incidence found here, it could also be that the changing structure of farms, which has caused subsidy payments to increasingly go to larger farms with higher incomes (McFadden and Hoppe 2017), has increased the incidence over time.

While the subsidy incidence estimate is higher than that found in prior research, it is still below that hypothesized by the Ricardian theory of rent. Prior research has speculated the difference between the estimates and theory could be due to rigidity in rental contracts and that the long-run subsidence is even greater (Hendricks, Janzen and Dhuyvetter 2012). It is also possible that due to measurement error in the subsidy payment variable, as well as county-level aggregated variables, our results are still attenuated (Kirwan 2009). The difference from $1 could also be due to a fairness equilibrium (Rabin 1993), as both farmers and landlords often reside in similar geographical regions and communities and thus might value a perceived fairness component. Related research in land values has shown price discounts for buyers when they are acquaintances with the sellers (Perry and Robison 2001; Kostov 2010), though this may not hold in rental agreements (Tsoodle, Golden, and Featherstone 2006). The results we present only examine the short-run subsidy incidence related to rental rates. It is also possible that each subsidy dollar could translate into higher prices of other inputs as well.

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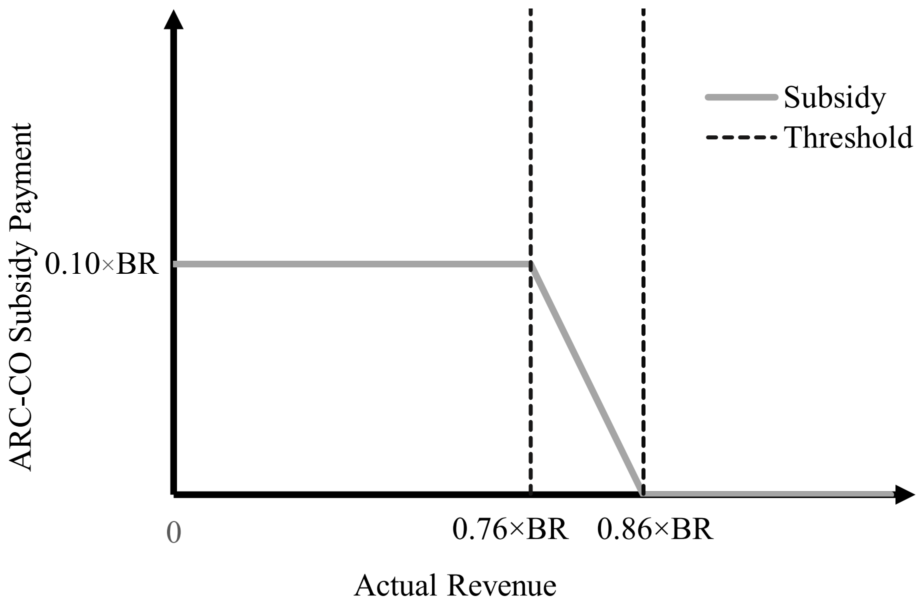



Figure 1. Subsidy Payment Thresholds

Note: All subsidy payments are further scaled by 85% due to program rules.

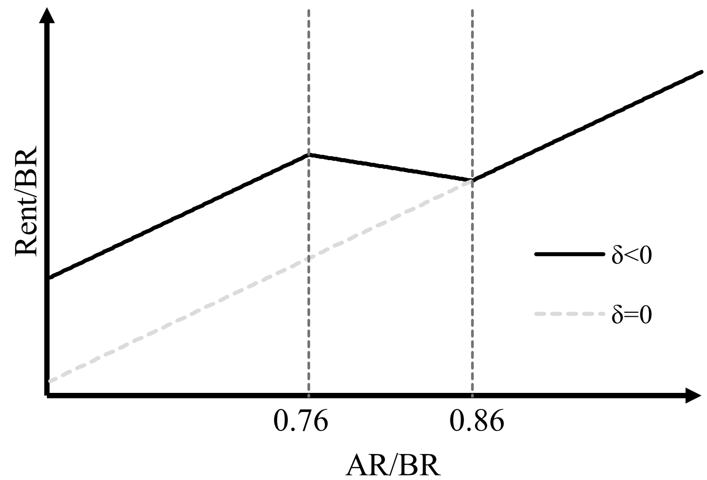
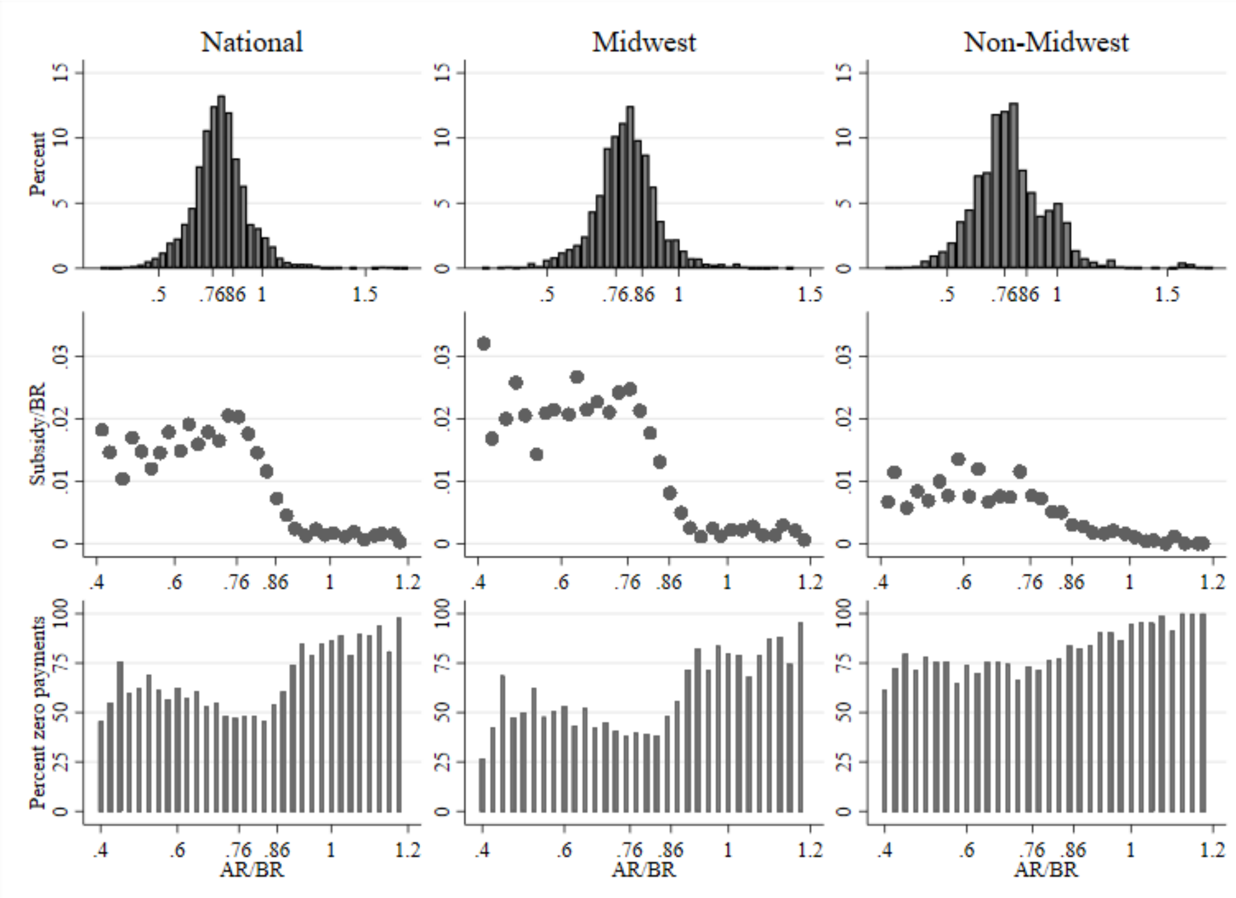


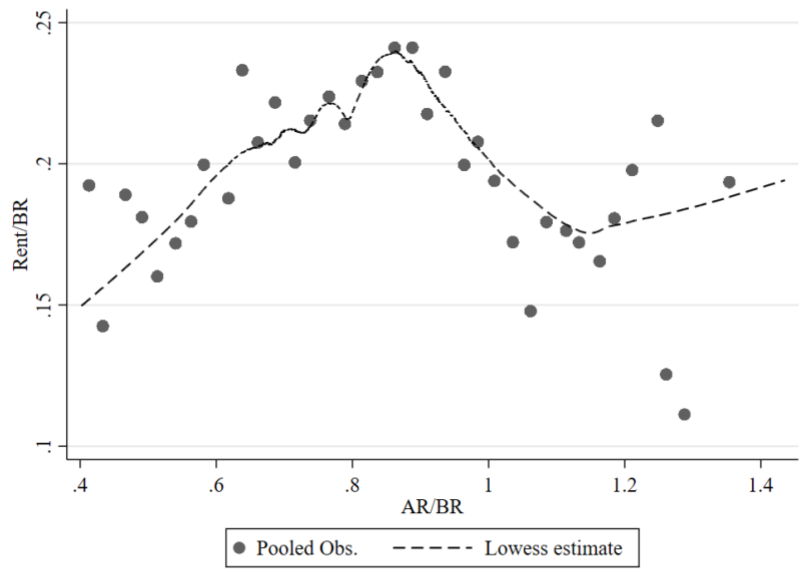

Figure 2. Visualization of a Positive Subsidy Incidence

Note: Figure assumes effect of AR/BR on Rent/BR is linear, additive, and positive. $\delta<0$ is a positive subsidy incidence. The vertical dotted lines signal changes in subsidy payment thresholds, moving from right to left, from zero to positive payments, and positive payments to capped, maximum payments.




Notes: First row: Histograms of assignment variable *AR/BR*. Second row: Binned scatterplots with observations of median AR/BR values for bin widths of 0.02 and average values of *Subsidy/BR*. Third row: The percent within each *AR/BR* bin of width 0.02 reporting zero payments. Data sources: pooled 2015, 2016, and 2017 ARMS surveys. *AR/BR* value is constructed by authors from data obtained from Farm Service Agency.

Figure 3. Visualizations of the Relationship Between AR/BR and Reported Subsidy Payments




Note: Binned scatterplot of median values of AR/BR and the average value of rent/BR for each bin. Bin width is 0.02. Lowess estimate is the predicted value of a locally weighted regression of AR/BR on rent/BR with bin widths of 0.25. Sample is restricted to only farms in the Midwest.

Figure 4. Visualization of Assignment Variable on Rent/BR

## Table 1. Survey-Weighted Mean Values of Samples

| Variables | National | | | Midwest | | |
|---|---|---|---|---|---|---|
| | 2017 | 2016 | 2015 | 2017 | 2016 | 2015 |
| Rental Rate ($/acre) | 124 | 127 | 126 | 141 | 142 | 142 |
| ARC Payments ($/acre) | 7 | 10 | 7 | 8 | 13 | 9 |
| Benchmark Revenue ($/acre)[1] | 602 | 637 | 635 | 631 | 667 | 670 |
| Actual Revenue ($/acre)[1] | 489 | 485 | 506 | 531 | 514 | 524 |
| Farmer Revenue ($/acre) | 340 | 344 | 325 | 375 | 371 | 355 |
| Land Owned (acres) | 421 | 389 | 492 | 393 | 383 | 395 |
| Land Rented (acres) | 698 | 662 | 699 | 644 | 617 | 617 |
| Land Farmed (acres) | 1096 | 1034 | 1167 | 1011 | 982 | 984 |
| Corn (acres planted) | 273 | 274 | 240 | 309 | 314 | 272 |
| Soybean (acres planted) | 276 | 250 | 243 | 306 | 277 | 258 |
| Sorghum (acres planted) | 14 | 17 | 19 | 9 | 10 | 13 |
| Wheat (acres planted) | 104 | 116 | 122 | 79 | 89 | 89 |
| **Observations** | 6,469 | 6,960 | 6,283 | 4,639 | 4,720 | 4,321 |

Note: All values are the survey weighted means. Midwest includes farms sampled from the following states: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, and Wisconsin. Source: Results are from ARMS surveys, unless noted by [1], which were constructed from Farm Service Agency program data.

## Table 2. Regression Kink Design Estimates of Subsidy Incidence

| | (1) Linear w/o controls | (2) Linear w/controls | (3) Quadratic w/controls | (4) Cubic w/controls | Linear w/ controls each individ. kink | |
|---|---|---|---|---|---|---|
| | | | | | (5) Left | (6) Right |
| Subsidy incidence | 0.537*** | 0.378*** | 0.396*** | 0.377*** | 0.372*** | 0.479*** |
| | (0.080) | (0.078) | (0.085) | (0.086) | (0.079) | (0.128) |
| AIC | -21,401 | -22,419 | -22,417 | -22,418 | | |
| Observations | 13,680 | 13,680 | 13,680 | 13,680 | 13,130 | 4,383 |

Notes: Presents the subsidy incidence after scaling δ by -1/.85 from equation [8]. Robust standard errors, additionally scaled, clustered by county. Asterisks denote statistical significance: *** p<0.01, ** p<0.05, * p<0.1. AIC statistic refers to the Akaike information criteria to measure model fit. Specifications 2-6 include the following control variables that were divided by BR: the proportion of land planted to corn, soybeans, wheat, and sorghum; farm-level per acre revenue; year dummy variables. Sample only includes Midwest farms. Kinks were estimated at 0.845 and 1.010. Specifications 5 and 6 were only estimated across a single kink. The left kink is where payments become capped. The right kink is the payment threshold. AIC is not presented for specifications 5 and 6 as they contain different samples.

## Table 3. Subsidy Incidence Estimates using Reported Payments

| | (A) Controls + Farm revenue | | (B) Controls + Corn yield | |
|---|---|---|---|---|
| **Sample years** | **National** | **Midwest** | **National** | **Midwest** |
| 2015 | 0.73*** | 0.68*** | 0.57*** | 0.54*** |
| | (0.13) | (0.13) | (0.13) | (0.13) |
| 2016 | 0.76*** | 0.68*** | 0.54*** | 0.50*** |
| | (0.14) | (0.15) | (0.14) | (0.15) |
| 2017 | 0.72*** | 0.76*** | 0.56*** | 0.63*** |
| | (0.11) | (0.12) | (0.11) | (0.12) |
| Observations | | | | |
| 2015 | 6,283 | 4,321 | 4,824 | 3,752 |
| 2016 | 6,960 | 4,720 | 5,606 | 4,300 |
| 2017 | 6,469 | 4,639 | 4,714 | 3,885 |

Note: Presents the coefficient estimate, robust standard errors, and sample year. Controls include following additional covariates: the proportion of land planted to corn, soybeans, wheat, and sorghum; state dummy variables. Model 1 includes farm-level per acre revenue. Model 2 is restricted to only farms with positive amounts of corn acres and includes per acre yield as additional control. Standard errors presented in parentheses. Asterisks denote statistical significance: *** p<0.01, ** p<0.05, * p<0.1.

[1] Calculation does not include the 2014 fiscal year due to the delay in the reauthorization of the Farm Bill in 2014.

[2] Based on the authors' calculations using total cropland (excluding pastured) from the National Agricultural Statistics Service (NASS) 2017 Census of Agriculture and cropland rented to others from the NASS 2014 Tenure, Ownership, and Transition of Agricultural Land (TOTAL) Survey.

[3] The timeline of yield updating is specific to the programs farmers enrolled in, though most programs did not allow updates until at least 2002.

[4] The farmer was allowed varying levels of flexibility at different times for planting crops on the base acres. However, by planting a different crop, they would be ineligible to receive any payments on those acres, making the choice of planting a different crop less appealing.

[5] In addition to direct payments, Congress also provided a variety of additional income support programs. The Counter-Cyclical Payments (CCP) was created in 2002 to provide income support to farmers in the event of poor outcomes. CCP was similar to the deficiency payments program but placed no restrictions on planting. Average Crop Revenue Election (ACRE) and the Supplemental Revenue Assistance Program (SURE) were created in 2008. These programs paid farmers conditional on realized outcomes, as opposed to offering a guaranteed payment level. From 2010 to 2013, counter-cyclical and ACRE payments were only five percent of direct payments (Economic Research Service, 2019).

[6] The Olympic average is calculated by dropping the highest and lowest observations, then averaging the remaining three. In addition to this averaging, if historical yields or prices fall below

certain levels, there are yield (county t-yield) and price floors (reference price) that replace the lower values. Marketing year refers to the timing related to crop production.

[7] Due to the formula's two kinks, the subsidy incidence could also be measured by the difference in the intercept values for farms that received the maximum payments (0.085*BR) and those that did not receive any payments.

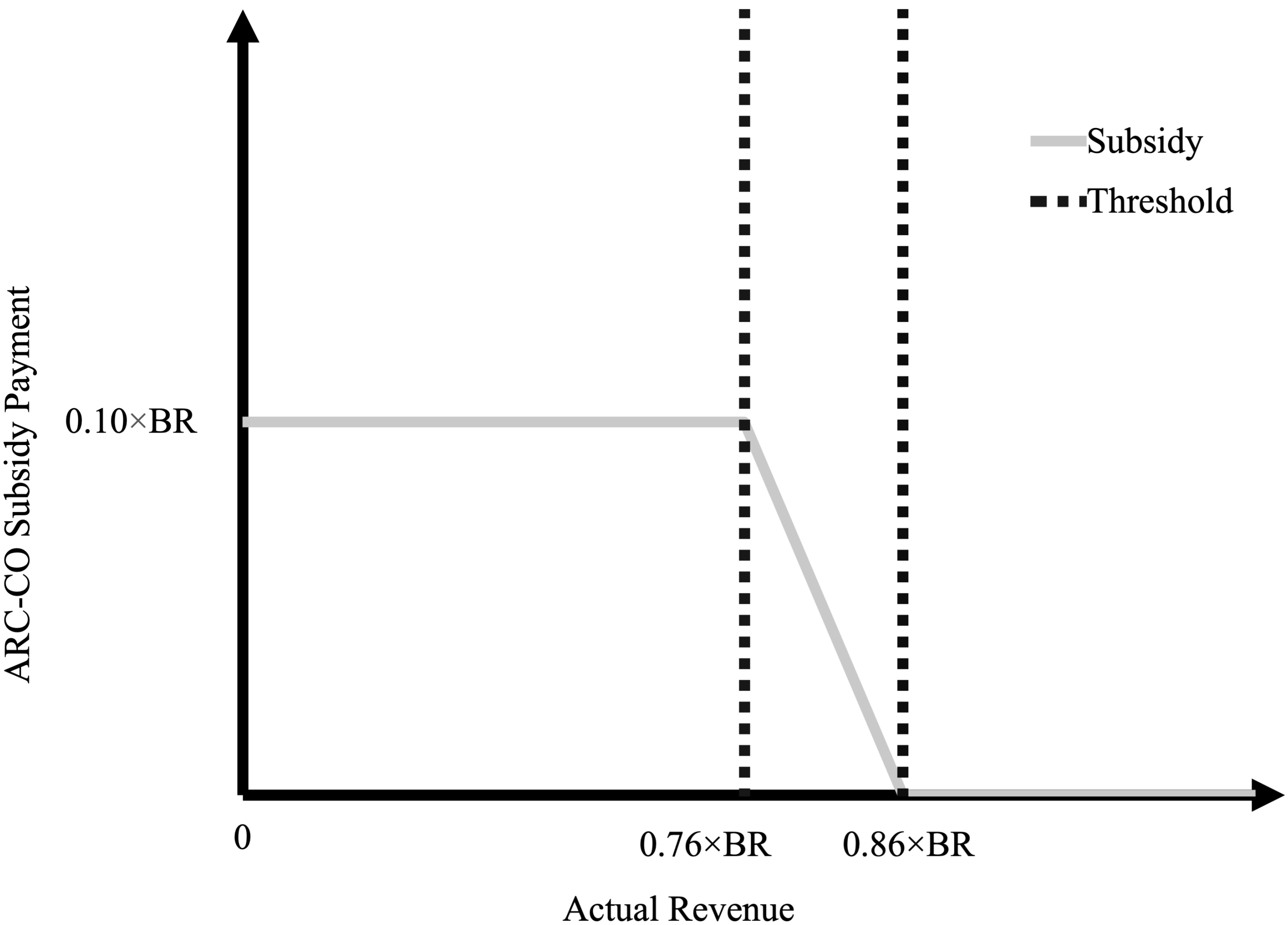
[8] Survey questionnaires are available online at https://www.ers.usda.gov/data-products/arms-farm-financial-and-crop-production-practices/questionnaires-and-manuals/

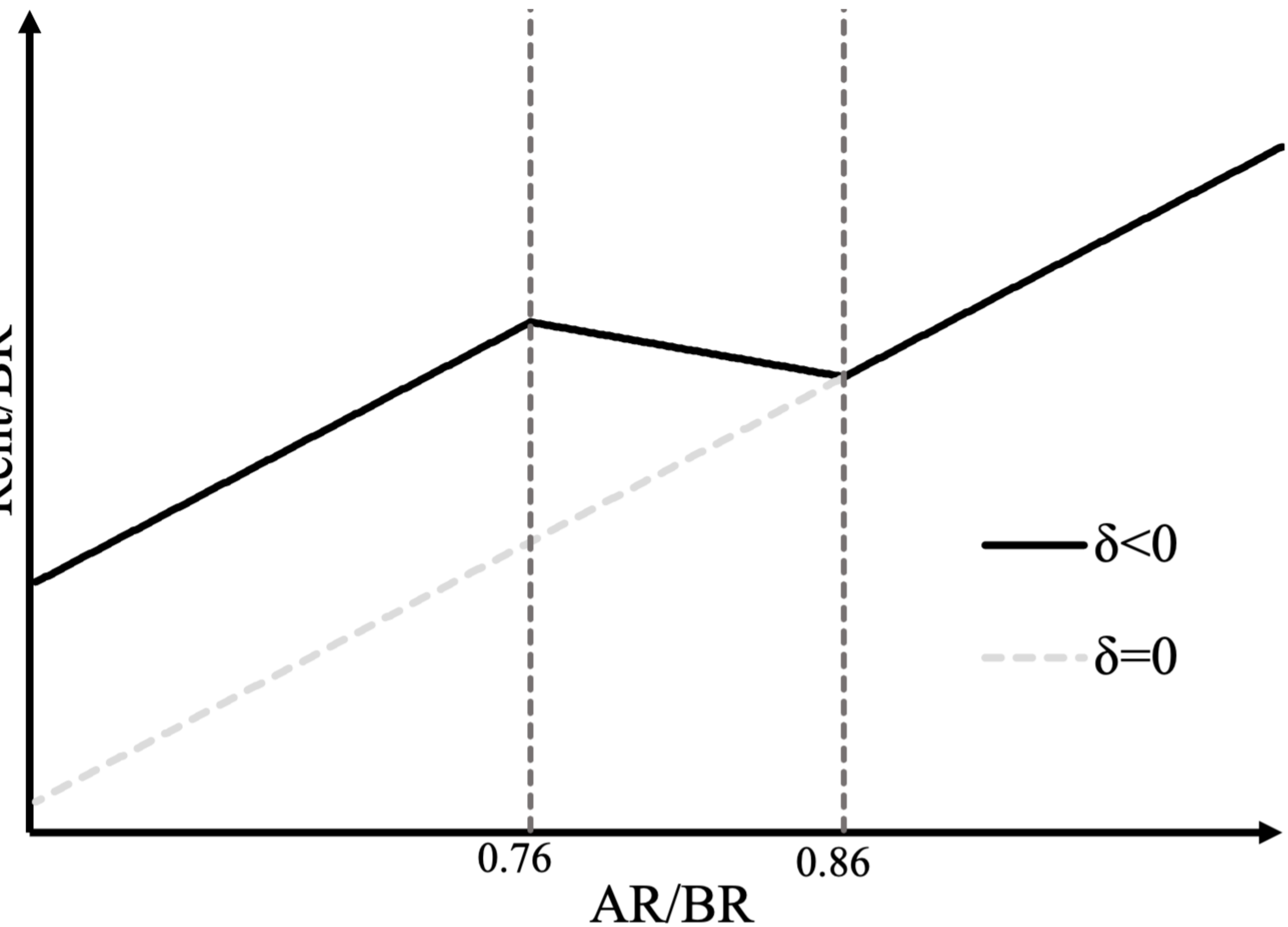
[9] Other types of land rental agreements include a flexible payment and sharecropping. Determining the per-acre costs and share of the farmer's subsidy payments is unknown under these forms of contracts.

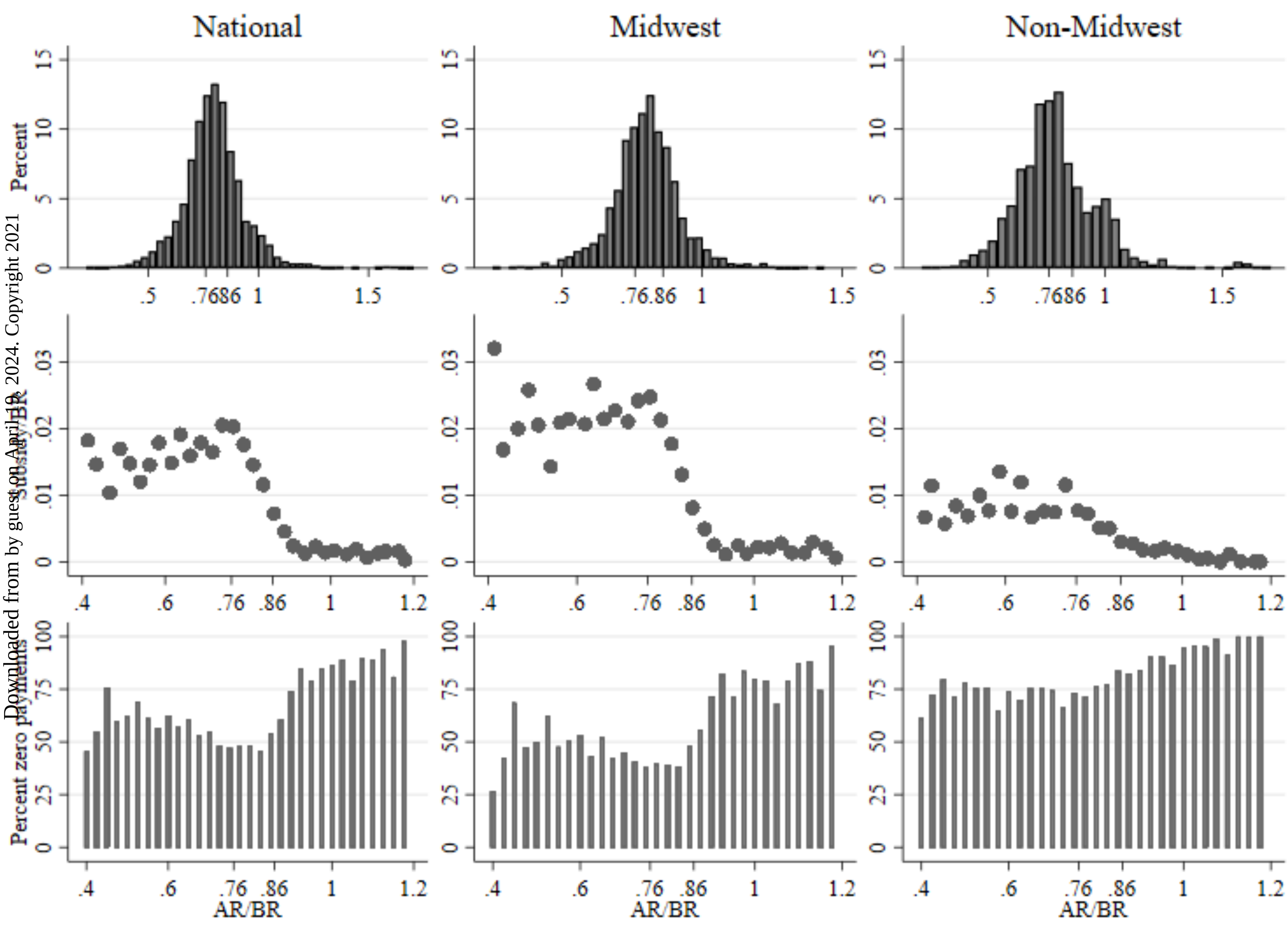
[10] ARMS does not differentiate for ARC-IC or ARC-CO, though given the size differences between the two programs, we assume they are ARC-CO for identification.

[11] The ARMS survey weights are not weighted based on reported subsidy payment weights, thus the aggregate sum should be taken with some caution. However, we believe the significant difference between the actual payment values reported in the ARMS survey and the FSA receipts is indicative of underreporting.

[12] The coefficients were tested by including a dummy interaction on value of AR/BR below 0.845 (the left kink) for a pooled model with both kinks. Technique provides equivalent results to estimating each specification individually but has the advantage that the extra interaction term allows for statistical testing of differences in slope for each threshold region of AR/BR.


ARC-CO Subsidy Payment
0.10×BR
Subsidy
Threshold
0
0.76×BR
0.86×BR
Actual Revenue


Rent/BR
δ<0
δ=0
0.76
0.86
AR/BR


National
Midwest
Non-Midwest
Percent
Percent zero payments
AR/BR
AR/BR
AR/BR
0
5
10
15
.5
.7686
.76.86
1
1.5
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.01
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.03
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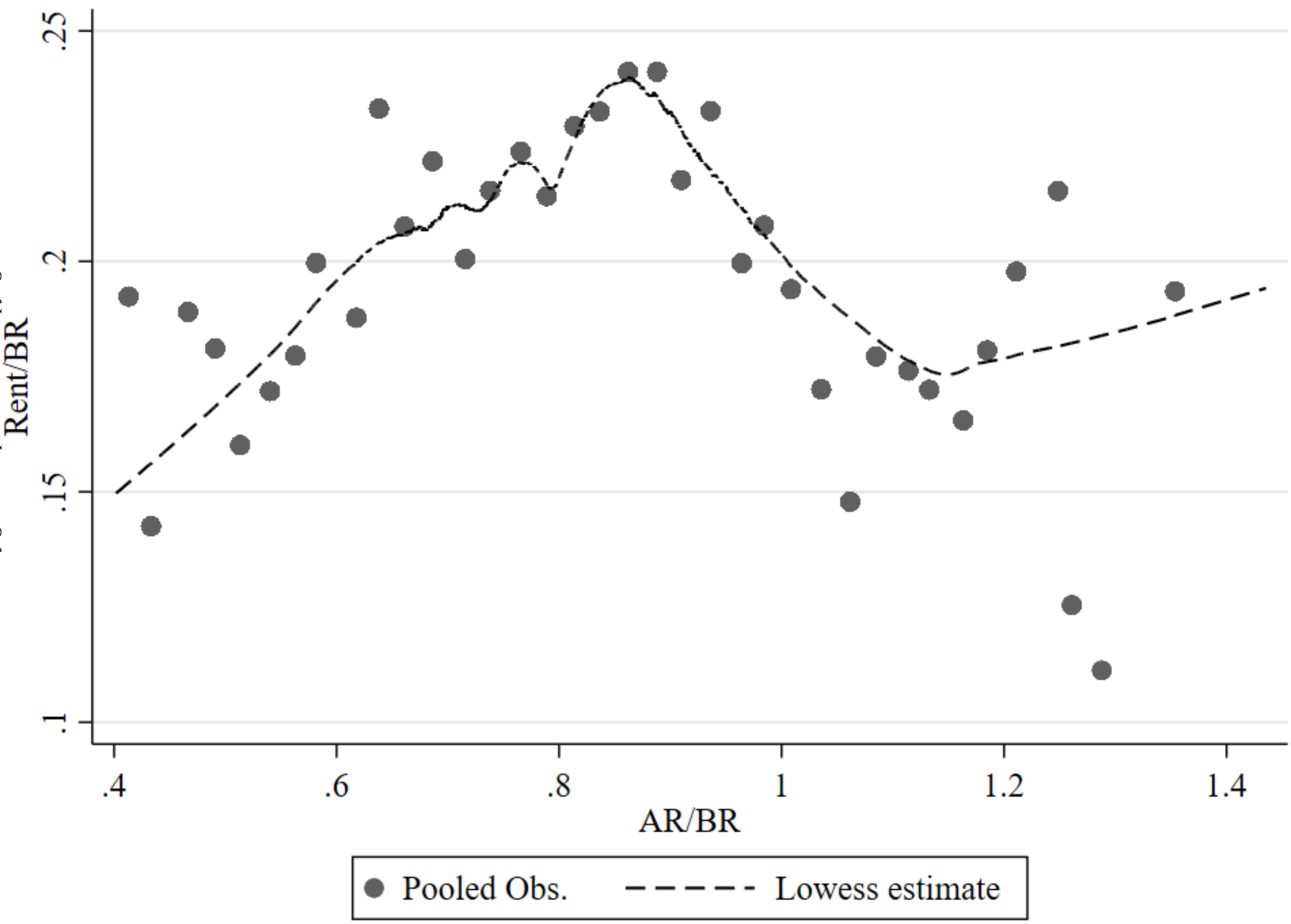
.6
.76
.86
1.2
0
25
50
75
100


.25
.2
.15
.1
Rent/BR
.4
.6
.8
1
1.2
1.4
AR/BR
Pooled Obs.
Lowess estimate