Does Federal Crop Insurance Make Environmental Externalities from Agriculture Worse?

Jeremy G. Weber, Nigel Key, Erik O'Donoghue

Abstract: Farmers dramatically increased their use of federal crop insurance in the 2000s. From 2000 to 2013, premium subsidies increased sevenfold and acres enrolled increased by 77%. Although designed for nonenvironmental goals, subsidized insurance may affect the use of land, fertilizer, and agrochemicals and, therefore, environmental externalities from agriculture. Using novel panel data, we examine farmer responses to changes in coverage with an empirical approach that exploits program limits on coverage that were more binding for some farmers than for others. Estimates indicate that expanded coverage had little effect on the share of farmland harvested, crop specialization, productivity, or fertilizer and chemical use. More broadly, we construct and describe a new nationwide, farm-level panel data set with nearly 32,500 farms observed at least twice over the 2000–2013 period, a resource that should enrich US agro-environmental research.

JEL Codes: Q12, Q15, Q18

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THE DECISIONS OF CROP FARMERS, such as how much fertilizer and pesticide to use, can affect biodiversity and water quality. Hendricks et al. (2014), for example, find that increased demand for corn-based ethanol expanded the so-called dead zone in the Gulf of Mexico by encouraging farmers to plant more corn and use more fertilizer. US federal crop insurance may have similar unintended effects. Although

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designed to reduce farm income variability, crop insurance may cause farmers to take more risks and apply more fertilizer, plant crops on erodible lands, or specialize in fewer crops, thereby exacerbating environmental externalities from agriculture.

The growth in federal crop insurance warrants greater study of the program's unintended consequences (Goodwin and Smith 2013). Crop insurance has expanded significantly since 2000 and with the 2014 Farm Act is now the main conduit of financial support to farmers. Between 2000 and 2013, acres enrolled beyond the most basic coverage increased by 77%. The corresponding premium subsidies paid by the federal government also increased. Before 2000 the subsidies never exceeded a billion dollars in real terms; for the years 2011–13, they ranged between \$6 and \$7 billion annually (fig. 1).

The empirical literature on crop insurance provides a generally weak foundation for distinguishing the effect of insurance apart from confounding factors. There are no farm-level empirical studies of crop insurance and input use that use a sample of national scope and control for farm fixed effects. Studies have commonly relied on cross-sectional variation (e.g., Horowitz and Lichtenberg 1993; Smith and Goodwin 1996), even though time-invariant unobservable variables such as land quality and risk attitudes are likely correlated with crop insurance participation and input

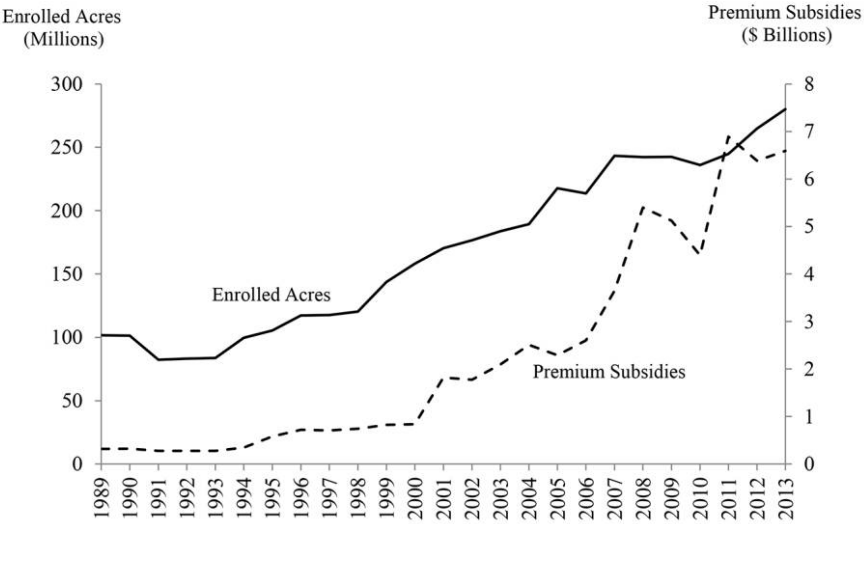

Figure 1. Enrolled acres and total premium subsidies, 1989–2013. The figure was elaborated by the authors using data from the US Department of Agriculture, Risk Management Agency, Summary of Business. Enrolled acres correspond to the number of acres enrolled in a plan beyond the basic catastrophic level. Premium subsidies refer to those subsidies applied to acres enrolled in a plan beyond the basic catastrophic level. Premium subsidies are in 2009 dollars.

use. O'Donoghue, Roberts, and Key (2009) is an exception, but it only considers crop diversification, not input use.

Almost all published studies also assume that crop insurance participation is exogenous to farm decisions. This is unlikely. Farmers may shift land from low-input, noninsurable uses to high-input, insurable crops (or vice versa) for reasons unrelated to insurance, such as changing crop prices or farm finances. With the new land use eligible for subsidized insurance, participation in insurance would increase along with input use, inducing a spurious correlation between the two. Here, Cornaggia (2013) is a notable exception in his treatment of the endogeneity of insurance adoption. He exploits the introduction of new insurance policies in some counties and not others. However, his data only permit examining the effect of insurance on yields, and his identification strategy rests on 14 insurance policy events, with all but one occurring before the beginning of our study period.

We study how changes in insurance coverage over the 2000–2013 period affected farm-level crop choice and fertilizer and chemical use while controlling for farm-fixed effects and the endogeneity of crop insurance participation. Controlling for farm-fixed effects is possible by our creation of a panel data set constructed from 14 years of the annual USDA Agricultural Resource Management Survey. For plausibly exogenous variation in coverage, we exploit the insurance program's limit on how much coverage farmers could purchase. As the incentive to have insurance grew, farmers who initially had little coverage could greatly expand coverage; farmers already close to the maximum level could not. Instrumenting the change in coverage with each farm's initial coverage ratio—its actual coverage relative to its farm-specific maximum coverage possible—allows us to identify the effect of coverage on production decisions under plausible assumptions. Moreover, the nonlinear relationship between the initial coverage ratio and the subsequent change in coverage allows us to control (linearly) for the initial level of crop insurance coverage in our regressions, making our approach robust to a linear relationship between initial coverage and changes in production decisions.

In addition to providing plausible estimates of the effects of insurance coverage, we create and document a new farm-level panel data set that will enrich future research in agricultural and environmental policy. Empirical research related to US farms has been limited by the lack of nationwide panel data at the farm level. Outside of the Census of Agriculture, which has a limited scope of questions and occurs once every 5 years, there has been no comprehensive panel data for US farms. Our panel data set is based on the Agricultural Resource Management Survey—the only annual nationwide data source on the finances, production practices, and resource use of US farms and the households operating them. Despite its design as a cross-sectional survey, nearly 32,500 farms have been surveyed at least twice over the 2000–2013 period, thereby providing a rich resource to study dynamic issues and account for time-invariant farm heterogeneity.

Applying the data to study the effects of crop insurance, our ordinary least squares (OLS) estimates from a first-differenced model show a positive relationship between coverage and fertilizer and chemical use, although smaller than some prior estimates using cross-sectional data. Our instrumental variable estimates, however, show that coverage has little effect on crop specialization or input use. The estimates are sufficiently precise that even the upper bounds of a 95% confidence interval represent environmentally negligible effects. Thus, it does not appear that a more generous crop insurance program by itself encourages specialization or greater fertilizer and chemical use, as several prior studies have found.

1. AGRICULTURE, THE ENVIRONMENT, AND CROP INSURANCE

1.1. Agriculture and the Environment

Farmers are the chief managers of arable lands around the world, and their decisions affect environmental quality on their lands and beyond (Tilman et al. 2002). Switching marginal land from passive uses into cultivation reduces its value as wildlife habitat. Marginal lands are also more prone to soil erosion when cultivated, leading to the sedimentation of lakes and streams (Shortle, Abler, and Ribaudo 2001). For land already in cultivation, a less diverse crop mix reduces biodiversity and increases insect and disease problems (Sulc and Tracy 2007; Landis 2008).

Fertilizer nutrients or pesticides running into surface water or leaching into groundwater can be extensive. The US Environmental Protection Agency has identified agricultural non-point-source pollution as a leading source of impairment of the country's water resources (US EPA 2015). Studies have shown that 30%–40% of nitrogen fertilizer applied to crop fields seeps into ground or surface water, with losses of 70% on the margin (Cambardella et al. 1999; Randall and Mulla 2001; Li et al. 2006). A 10-year study by the US Geological Survey found widespread occurrences of pesticides in streams and groundwater, often at concentrations deemed harmful to aquatic life and fish-eating wildlife (Gilliom 2007). A 1990 nationwide survey by the EPA found that 10% of community water systems and 4% of rural domestic wells contain at least one pesticide (US EPA 1990).

1.2. The Federal Crop Insurance Program and Incentives to Participate

The Risk Management Agency (RMA) of the US Department of Agriculture oversees federal crop insurance by operating and managing the Federal Crop Insurance Corporation. RMA sets the terms in which private insurance companies provide insurance to farmers, including the total premiums associated with each policy. The federal government encourages participation in crop insurance by paying a share of the premium for farmers.

With low initial participation by farmers, the government encouraged greater adoption by increasing premium subsidies and plan options in the Agricultural Risk Protection Act of 2000 and the 2008 Farm Act. The 2000 Act increased premium subsidies from an average of 33% of total premiums (across all coverage levels) to an average of 57% (O'Donoghue 2014). O'Donoghue (2014) shows that the subsidies led farmers to adopt policies with higher coverage levels, with a 1% increase in the subsidy rate increasing total premiums and premiums per acre by 1%. The 2008 Farm Act maintained subsidies for traditional policies and introduced a new option for enterprise units, which came with even higher subsidies (80% for most coverage levels).

For both the 2000 and 2008 Acts, the additional subsidies and options would have been available in the year following the Act's authorization. The full effect of the changes on coverage, however, likely took several years to occur as farmers learned about the new options in a way that is analogous to the adoption of new product or innovation. The rate of adoption of a new product or technology often follows the "S-shaped curve" described by the Diffusion of Innovations model (Rogers 2010). The rate of adoption is initially slow and then accelerates before leveling off when only a few of those remaining have not adopted. This pattern seems to apply to crop insurance adoption. For example, revenue-based policies were first introduced in 1996, but adoption expanded most quickly during the 1998–2001 period (Dismukes and Coble 2006). Following this pattern, the subsidy increase in 2000 (and 2008) resulted in temporal variation in crop insurance coverage over the next several years as more and more farmers adopted insurance in response to the subsidy increases.

In addition to changes in subsidies and insurance options, the Renewable Fuel Standard and macroeconomic factors also made insurance more attractive by increasing price levels, price variability, and consequently profit variability. Increases in energy prices helped increase fertilizer and other input prices while US biofuel policy and rising global demand contributed to higher crop prices in the second half of our study period (Trostle et al. 2011; Beckman, Borchers, and Jones 2013). Higher input and output prices, in turn, generally increase profit variability by magnifying the effect of yield shocks.[1] Moreover, the US Renewable Fuel Standard increased the volatility of corn prices by strengthening the linkage between energy and corn markets (Du and McPhail 2012; McPhail and Babcock 2012; McPhail, Du, and Muhammad 2012).

1. Consider crop profits as $\pi = p_y \cdot \text{yields} - p_x \cdot x$. For simplicity, assume that only yields are stochastic, in which case the variance of profits is $\text{Var}(p_y \cdot \text{yields} - p_x \cdot x) = p_y^2 \cdot \text{Var(yields)}$, indicating that variability increases exponentially with the crop price. If both input and output prices increase proportionally, average profits will increase by the same proportion, which could discourage insurance use. Empirically, Coble et al. (1996) find that the elasticity of crop insurance with respect to expected marginal revenue is similar in magnitude to the elasticity with respect to the variance of marginal revenue (though they have different signs). If their finding holds for recent years, it implies that a proportional increase in input and output prices would increase the demand for insurance because it would increase profits linearly and profit variability exponentially.

Figure 1 shows that acres enrolled beyond the basic coverage level increased by 77% over the study period. Acres enrolled expanded consistently from 2000 to 2005, in part reflecting the delayed effect of changes made in the 2000 Farm Act. Rising crop and input prices and increased volatility likely played a larger role in increasing crop insurance adoption in the later 2000s. Enrolled acres increased by 14% in 2007 alone, then remained steady following the 2008 Farm Act, but saw strong growth over the 2011–13 period when corn prices were dramatically higher than in most prior years. Premium subsidies show a roughly similar pattern, though the percentage increase was larger, reflecting rising production values, which increased expected indemnities and premiums.

1.3. Crop Insurance and Farm Decisions

There are several reasons why insurance coverage could influence decisions like how much fertilizer to apply. According to the moral hazard argument, greater coverage encourages riskier production choices, causing farmers to use more risk-increasing inputs and fewer risk-decreasing inputs (Pope and Kramer 1979; Leathers and Quiggin 1991; Horowitz and Lichtenberg 1993; Babcock and Hennessy 1996). Sheriff (2005) argues that farmers overapply nitrogen fertilizer to reduce the risk of very low yields, in which case subsidized crop insurance would reduce nitrogen use. Whether an input is risk increasing or risk decreasing, and consequently how insurance affects input use, becomes an empirical question.

A similar logic applies to other production decisions that affect profit variability. With greater coverage, a risk-averse producer could shift to riskier crops or specialize in one or two crops (O'Donoghue et al. 2009). For farm households, less farm income risk may encourage households to spend less time at off-farm jobs and more time on the farm (Key, Roberts, and O'Donoghue 2006). Shifting time or money to the farm could result in cultivation of marginal land and more fertilizer used per acre (Chang and Mishra 2012).

However, the potential effects of crop insurance on production via moral hazard should not be overstated. Deductibles and premiums depend on yield histories and should therefore attenuate moral hazard. The premium a farmer pays also depends on his claim history. A claim in one year increases the premium for following years and reduces the guarantee at which insurance pays, effectively increasing the deductible.

Federal crop insurance might alter production decisions for other reasons. Because it is heavily subsidized, the program increases the risk-adjusted returns to insured crops. By encouraging farmers to shift to insured crops, which may require more inputs, additional insurance could increase input use at the farm or regional level, even if it lowered input use on individual crops (Wu 1999; Wu and Adams 2001; Young, Vandeveer, and Schnepf 2001; Goodwin, Vandeveer, and Deal 2004; Walters et al. 2012). In addition, banks may lend to insured farmers at more favorable terms, relaxing financial constraints and making it cheaper to buy equipment or inputs to increase yields or plant more acres (Cornaggia 2013).

1.4. Empirical Approaches and Findings

Much of the earliest empirical work examining the production effects of crop insurance used cross-sectional data and focused on fertilizer and pesticide application rates. Horowitz and Lichtenberg (1993) show large, input-increasing effects of adopting crop insurance, with federally insured farms applying 19% more nitrogen and spending 21% more on pesticides than uninsured farms. Two other empirical studies around the same time find that insurance reduced chemical use: Quiggin, Karagiannis, and Stanton (1993) for Midwestern corn and soybean farmers and Smith and Goodwin (1996) for Kansas wheat farmers. Babcock and Hennessy (1996) take a different approach and use data from field experiments to estimate how fertilizer use affected crop yield distributions. In a simulation with their parameterized model they find that insurance would cause small reductions in nitrogen fertilizer use.

Later empirical work estimated the effect of insurance on a wider range of farm outcomes. Wu (1999) shows that in Nebraska crop insurance shifted land away from hay and pasture into corn, which increased chemical use. Goodwin et al. (2004) simultaneously estimate the effect of insurance on output and input intensity and find that increased participation in insurance programs caused modest changes in acreage and mixed effects on fertilizer and chemical expenditures per cropped acre. More recently, Walters et al. (2012) use insurance contract data and find acreage responses to insurance for some crops and regions but not others. Looking only at crop yields, Cornaggia (2013) exploits the exogenous expansion of insurance to new crops and finds that county-level yields increased after the expansion.

The generally weak foundation for distinguishing the effects of crop insurance from confounding factors may explain the diverse findings in the literature. Many studies use cross-sectional data or assume that insurance decisions are unrelated to unobserved factors that affect crop choices or fertilizer use. As noted earlier, this is a tenuous assumption: it is easy to imagine a scenario where, for reasons unrelated to crop insurance, a farmer decides to plant more acres of corn, which then affects decisions about fertilizer use and insurance coverage.

2. EMPIRICAL ANALYSIS

2.1. Empirical Approach

To quantify how crop insurance coverage affects farm decisions and therefore the environment, our empirical approach uses a novel unbalanced panel data set (described in the next section) with rich farm-level information. The base model relates changes in various outcomes to changes in crop insurance premiums per acre while controlling for initial farm characteristics, county fixed effects, and the years when the farm was observed:

$$y_{i,t} - y_{i,s} = \beta_0 + \beta_1(\ln PA_{i,t} - \ln PA_{i,s}) + \mathbf{X}_{i,s}\boldsymbol{\theta}_x + \mathbf{T}_{i,s}\boldsymbol{\theta}_1 + \mathbf{T}_{i,t}\boldsymbol{\theta}_2 + v_{c(i)} + \eta_{it}, \quad (1)$$

where $y_{i,t} - y_{i,s}$ is the change in the production variable for farm i between the first year the farm was observed s and the second year t (or in the case of farms observed three or more times, the second and third time and so forth). To measure the allocation of land to crops, we look at the share of total acres operated that are harvested; to capture crop specialization, we use the share of total acres harvested accounted for by the most harvested crop. For fertilizer and chemical use, we look at the log of fertilizer expenses per acre, the log of chemical expenses per acre, and the log of the sum of fertilizer and chemical expenses per acre. To capture overall intensity of land use, we use the log of the value of production per acre.

We measure crop insurance coverage using the premium paid per acre of land operated ($PA_{i,t}$). Many prior studies used a binary variable to indicate whether a farmer had any acres enrolled in crop insurance (Horowitz and Lichtenberg 1993; Smith and Goodwin 1996; Wu 1999). Conditional on having some acres enrolled, this approach does not capture increases in the number of acres enrolled or the level of coverage chosen for enrolled acres. More recent work has used the share of total acres enrolled in crop insurance (Chang and Mishra 2012; Walters et al. 2012). But as Goodwin et al. (2004) note, such a measure ignores changes in coverage levels on enrolled acres.

Our measure of coverage, in contrast, captures changes in acres enrolled and coverage levels. Because coverage is expressed as premiums per acre operated by the farm, the measure increases with the share of acreage enrolled in crop insurance. It also increases with the level of coverage chosen for enrolled acres since farmers pay higher premiums for higher coverage levels. The measure is similar to that of Goodwin et al. (2004), who use a measure of total liabilities, since premiums are proportional to the liabilities covered by the insurance policy. Our premium-based measure of coverage is well suited to our empirical goal of quantifying how an increase in crop insurance coverage—whether from enrolling more acres or selecting higher coverage levels or both—affects farm decisions.

The vector $X_{i,s}$ contains farm-specific characteristics observed in the first year of the difference in the dependent variable (subscript s in eq. [1]). Controlling for initial characteristics allows farms managed by young versus old farmers, for example, to have different growth trends, and avoids the potential for reverse causality that comes with using changes in characteristics. We control for the initial level of crop insurance coverage as measured by (unlogged) premiums per acre. To capture farm size and life cycle effects we include a linear and quadratic term for both the farm operator age and the initial total value of production. To account for differences in crop specialization, we control for the initial share of harvested acres accounted for by soybeans, corn, and wheat, all separately. Including the share of acres in each of these major crops helps control for any effect that crop rotation patterns may have on changes in insurance coverage and input decisions. As we will show, our conclusions are robust to excluding the farm-level control variables.

The vector T_{is} contains binary variables indicating the first year of the differenced dependent variable; the variables in T_{it} indicate the second year. These year dummy variables control for shocks unique to those years and that affect the change observed over the time spanned by the two years. This controls for confounding macroeconomic factors correlated with crop insurance coverage and our outcomes. For example, coverage generally increased over time along with corn prices, which would lead to an increase in the value of production per acre, or, similarly, an increase in fertilizer use per acre.

The term $v_{c(i)}$ is a county fixed effect. It captures any change in behavior common to all sample farms from the same county. It therefore controls for local unobserved conditions such as the possibility that changing crop prices encouraged agricultural intensification in some areas more than others because of differences in land suitability. On average, there are about six sample farms per county.

2.2. Identification

Prior studies of crop insurance and production decisions that do not control for farm fixed effects (e.g., Horowitz and Lichtenberg 1993; Smith and Goodwin 1996) likely suffer from omitted variable bias caused by unobserved farm characteristics correlated with farm decisions and insurance participation. By relating differences in farm-level outcomes with differences in coverage, the specification in (1) accounts for time-invariant farm characteristics that affect the outcome in an additive manner.

Controlling for farm fixed effects may nonetheless be inadequate to identify the causal effect of crop insurance participation on farm decisions. Any factor causing a shift in land use toward input-intensive insurable crops could create a spurious correlation between input use and insurance coverage. OLS would give biased results in other plausible scenarios as well.[2] Appropriately estimating the effect of insurance coverage requires temporal variation in coverage unrelated to the decision to expand or intensify crop production.

2. Another example where OLS would give biased results is where in absence of insurance farmers use few inputs on marginal land and many inputs on high-quality land. With an insurance program, farmers might only get coverage for marginal land, which could encourage them to use as much inputs on the marginal land as they do on the high-quality land. With the insurance program, suppose that much of the variation in premiums over time is based on farmers replacing marginal land with high-quality land, which causes premiums to decrease, or vice versa, which causes premiums to increase. In every instance where such replacing occurs, premiums change in a way that is uncorrelated with input use on the average acre. The first difference model estimated with OLS uses all of this variation in premiums in the estimation, which would bias the coefficient on premiums toward zero.

Our instrumental variable identification strategy leverages two facts: first, as previously discussed, the incentive for farmers to adopt more insurance increased over time and, second, the federal crop insurance program has always had a maximum coverage level (85% for an individual level policy; 90% for an area-based policy). The growing incentive to expand coverage and the presence of a maximum coverage level suggests a negative nonlinear relationship between (*a*) a farmer's initial coverage relative to the maximum coverage possible and (*b*) the change in coverage in response to incentives to have more insurance. This nonlinear relationship is because farmers who initially had coverage close to the maximum coverage were substantially more limited in how much they could expand coverage compared to farmers who initially had less coverage.

To illustrate, consider an increase in the demand for insurance from period 1 to period 2 caused perhaps by a drop in the price of insurance. Greater demand will result in more coverage for most farms, and premiums per acre in period 2 will increase relative to premiums per acre in period 1 (fig. 2). How much premiums increase in the second period depends on the farmer's initial coverage. A farmer paying the maximum premium in the first period cannot increase coverage in response to the lower price of insurance, which is why the ratio of the second and first period premium equals one when the first period premium equals the maximum premium. A farm with a low premium in the first period, in contrast, may double or triple

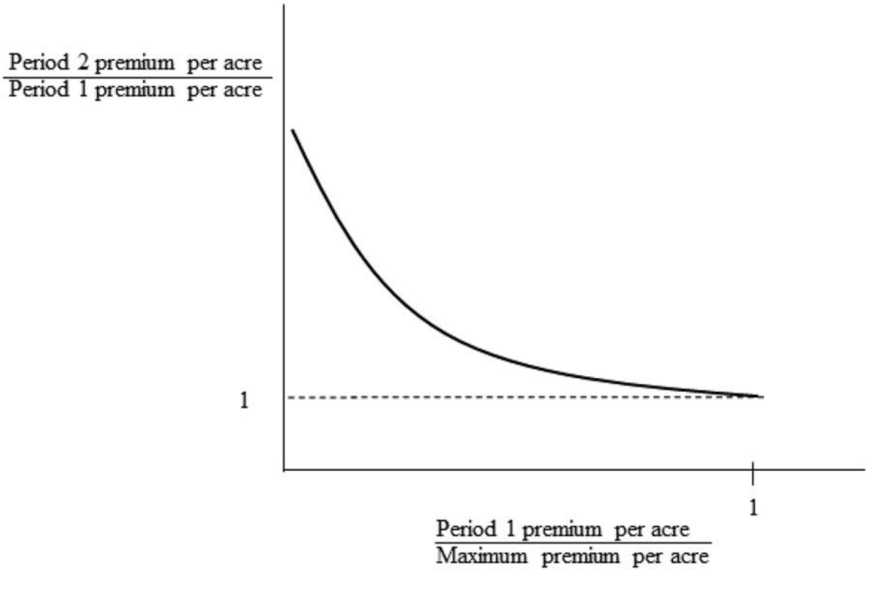

Figure 2. The initial coverage ratio and the response to greater incentives for insurance

coverage. This is shown in figure 2 by the negative nonlinear relationship between the ratio of the period 1 premium to the maximum premium possible in period 1 (horizontal axis) and the ratio of the second-period premium to the first-period premium (vertical axis).

The relationship in figure 2 has a firm microeconomic foundation. Consider two risk-averse farmers who seek a specific level of risk exposure. The farmers can reach their desired risk exposure through a combination of risk-reducing practices ("diversification") and insurance. The combinations of diversification and insurance that yield the same risk exposure constitute an indifference curve for each farmer. The price of insurance relative to the price of diversification (e.g., the cost of risk-reducing practices) determines the cost-effective mix of diversification and insurance. Further assume that the maximum coverage constraint binds for one farmer and not the other (perhaps because of a greater aversion to risk and therefore a greater demand for insurance).

Consider an increase in premium subsidies, which causes the price of insurance to decline relative to the price of diversification. The constrained farmer cannot increase the quantity of insurance and, assuming that his preferred risk exposure has not changed, he has no incentive to change his use of diversification. In contrast, the change in relative prices causes the unconstrained farmer to use less diversification and more insurance, a shift associated with an increase in premiums relative to the constrained farmer. Moreover, the percentage change in coverage increases exponentially the further the farmer's initial insurance level is from the maximum level (see the appendix, available online, for a detailed explanation, a graphical illustration, and a treatment of the case where increased profit variability increases the demand for a reduction in risk).

Following the logic of figure 2, let the relationship between the rate of increase in coverage and the initial coverage level be described with an exponential function of the form:

$$\frac{PA_{i,t}}{PA_{i,s}} = \left(\frac{PA_{i.s}}{\mathrm{Max}\,PA_{i,s}}\right)^{\phi}, \tag{2}$$

where ϕ is presumably negative. Taking logs of both sides gives

$$\ln(PA_{i,t}) - \ln(PA_{i,s}) = \phi \ln\left(\frac{PA_{i.s}}{\mathrm{Max}\,PA_{i,s}}\right). \tag{3}$$

Equation (3) motivates using an instrumental variable (IV) approach to estimate (1), where the log of the initial premium divided by the maximum premium, which we call the coverage ratio, is used as an instrument for the log difference in coverage as measured by premiums per acre. The first stage in this IV regression is then:

$$\ln(PA_{i,t}) - \ln(PA_{i,s}) = \gamma + \phi \ln\left(\frac{PA_{i.s}}{\text{Max}PA_{i,s}}\right) + \mathbf{X}_{i,s}\boldsymbol{\delta}_x + \mathbf{T}_{i,s}\boldsymbol{\delta}_1 + \mathbf{T}_{i,t}\boldsymbol{\delta}_2 + v_{c(i)} + \varepsilon_{it}. \tag{4}$$

We calculate the coverage ratio by dividing the initial per acre premium paid by the farmer by his maximum premium. The maximum premium—and therefore maximum coverage—varies by county and crop mix. We calculate the maximum using producer premium data from the Risk Management Agency's Summary of Business data, which are county-level data aggregated from all individual policies issued in the county. We find the crop-specific plan and coverage level with the highest per-acre premium in each year and each county. Then we multiply this maximum per-acre premium by the number of harvested acres of each crop for every farm. This gives the total premiums each farm would have paid, had it enrolled each crop in the most expensive plan observed in the county. We refer to this amount as the farm's maximum premium.[3]

The nonlinear relationship between the coverage ratio and changes in coverage allows for estimating ϕ while controlling for the initial premium per acre, $PA_{i.s}$, which is included in $\boldsymbol{X}$. The instrument—the log of the coverage ratio—is not perfectly predicted by a linear relationship with the initial premium per acre. Note that the log of the ratio can be written as $\ln(PA_{i,s}) - \ln(\text{Max}PA_{i,s})$. Both terms have variation that is not fully predicted by a linear function of the farm's initial premium per acre. First, the log of the maximum coverage, $\ln(\text{Max}PA_{i,s})$, varies by county and year, causing the coverage ratio to differ for two farmers despite having the same initial level of coverage. Second, the nonlinearity introduced by taking the natural logarithm provides another source of variation, since $\ln(PA_{i.s})$ and $PA_{i.s}$ are not perfectly correlated. This nonlinearity is a product of the constraint imposed by policy. As shown by figure 2, the maximum coverage level introduces a constraint that binds exponentially more for farmers as their initial coverage level approaches the maximum level. This nonlinearity is confirmed by the data in figure 3, which show

3. While we call this a maximum premium it is calculated as the average premium per acre associated with the most expensive plan and coverage level observed in the county. For example, two farmers with the most expensive plan in the county may pay different premiums because of different claim histories. If these were the only two farmers with the most expensive plan, we would use the average of the two for the per acre premium associated with the most expensive plan and coverage level. Also, note that this maximum is based on the most expensive insurance option chosen in a county, which may be different than the most expensive option available if that option is not selected by anyone in the county.

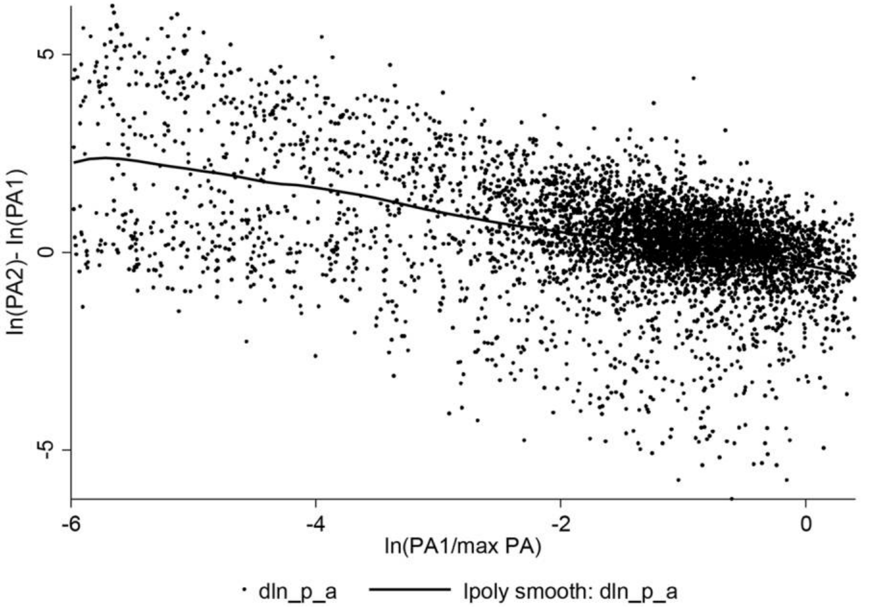

Figure 3. The log of the initial coverage ratio is negatively related to the change in coverage. The line represents the results of a kernel-weighted local polynomial regression of the log difference in coverage on the log of the initial coverage ratio. For the figure, the log of the coverage ratio is truncated at –6, and only observations with nonzero premiums are used.

a negative linear relationship between the log of the coverage ratio and the log difference in premium per acre.

Our instrumental variable is plausibly exogenous to changes in farm decisions because it is statistically related to changes in coverage because of a policy constraint (the coverage limit) and changing policy and market conditions (more subsidies, program options, and higher crop and input prices). Moreover, the inclusion of the farm's initial coverage level makes the model robust to a correlation between a farm's initial coverage level and changes in the outcomes studied.

More specifically, the IV estimator is likely to give more credible estimates than the OLS estimator because the change in premiums per acre predicted nonlinearly by the coverage ratio should strip out much of the endogenous changes in premiums per acre. Consider a scenario where changing crop prices or farm finances encourage some farmers to shift land from low-input, noninsurable uses to high-input, insurable crops. With the switch, farmers increase insurance coverage, so premiums per acre and input use per acre both increase. Because OLS uses all the variation in premiums for identification, every time such a switch happens, premiums change in a way that is spuriously correlated with input use. As long as the log of the coverage ratio imperfectly predicts such switching, the predicted change in premiums will contain less endogenous variation then the actual change.[4]

2.3. Creating a Panel Data Set from the Agricultural Resource Management Survey

Empirical research on the causal effects of US agricultural and environmental policy has been constrained by a lack of farm-level panel data. The only nationwide source of detailed and comprehensive farm-level data is the Agricultural Resource Management Survey (ARMS), which is a cross-sectional survey. The National Agricultural Statistics Service (NASS), which administers the survey, draws a new sample of farms each year, sampling roughly 30,000 farms out of a population of 2.1 million.[5]

Although the survey is designed as a repeated cross-sectional survey, farms surveyed more than once over the years can be identified and their records linked. If a simple random sample were drawn, the probability of observing the same farm twice would be very low. This is not the case with ARMS. The USDA definition of a farm is broad and, as a result, many farms in the population have little production. Because ARMS seeks to be an annual snapshot of the state of agriculture every year, small farms are undersampled while larger farms, where most production occurs, are oversampled. As the surveys have been conducted annually since 1996, the many years of ARMS samples combined with the oversampling of large farms has caused many farms to be surveyed two or more times.

Using the unique principal operator identifier, a number assigned to each farm that does not change over time, we identified all farms appearing at least twice in the ARMS. Because the survey questions necessary for our study were not present prior to 2000, we focus on the data sets from 2000 to 2013. Over this period, 202,127 distinct farms were sampled and responded to the survey, of which 16%, or 32,498 farms, appear at least twice (table 1). Roughly 4% of farms appear at least three times.

Farms appearing at least twice in ARMS, which we label repeat farms, are quite different, on average, from the typical ARMS respondent farm. Because larger farms are sampled with a higher probability, repeat farms tend to be larger farms. For each year of ARMS we compare the median value of production and acreage operated

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4. It is possible for IV to be more biased than OLS even if the instrument is less correlated with the error term than the endogenous variable that it is instrumenting for. However, this becomes increasingly less likely to hold with a stronger instrument (more correlated with the endogenous variable). We note that our instrument is extremely strong, with a first-stage F-statistic in excess of a thousand.

5. For an overview of ARMS along with detailed documentation, visit www.ers.usda.gov/data-products/arms-farm-financial-and-crop-production-practices.

Table 1. How Often Is the Same Farm Observed in ARMS?

Number of Times Observed	Farms	Percentage of Distinct Farms Observed
1	169,629	84
2	25,548	13
3	5,449	3
4	1,239	1
5	230	<.1
6	24	<.1
7	8	<.1
Total	202,127	100

Note.—The data are from the USDA, Economic Research Service, and USDA, National Agricultural Statistics Service, Agricultural Resource Management Survey (ARMS), 2000–2013. The percentages in the third column do not add to 100 because of rounding.

of all respondent farms with that of repeat farms observed for the first time in that year. We calculate the unweighted median since we are interested in comparing repeat farms with the typical ARMS respondent farm, not repeat farms with the general population. As expected, the median repeat farm consistently has more acres and production than the median respondent farm (table 2).

The oversampling of large farms arguably suits our purposes better than a sample representative of the US farm population as defined by the USDA. We are not interested in observing the typical farm in the population, which—because of the broad USDA farm definition—has little agricultural production and is unlikely to participate in federal crop insurance. For environmental and land use issues we are most interested in what happens to the typical acre. Because large farms account for most acres enrolled in crop insurance, a sample reflecting the large farm population provides more information on how crop insurance affects practices on the typical acre.

As this is the first study to construct a true panel data set using ARMS, there is value to documenting how repeat farms might differ from the typical respondent farm. We know that repeat farms are larger than the typical respondent farm, but if we control for farm size are the repeat farms similar to the typical respondent farm? To make this comparison, we draw a random subsample of ARMS respondent farms that is stratified to match the farm size distribution of repeat farms. We compare the two groups for a variety of characteristics other than farm size (provided in the appendix). In considering the comparability of treatment and control groups, Imbens and Wooldridge (2009) suggest that linear regression may be misleading when the normalized difference in group means is larger than 0.25 standard deviations. The

Table 2. How Do Repeat Farms Compare to the Typical ARMS Respondent Farm?

	Farms (Number Of)		Acres Operated (Median Acres)		Value of Production (Median $)	
Year	Repeat	All	Repeat	All	Repeat	All
2000	2,862	9,863	748	440	382,148	151,126
2001	1,999	7,343	840	416	474,014	131,190
2002	2,925	11,926	720	397	367,400	114,503
2003	4,398	17,782	620	395	320,628	142,233
2004	4,376	19,468	445	300	369,739	133,307
2005	4,213	21,564	412	250	339,560	105,583
2006	3,584	20,351	466	264	355,012	125,529
2007	2,314	17,465	650	360	560,727	239,878
2008	2,126	20,469	576	340	435,519	153,940
2009	1,700	19,877	450	300	292,288	111,103
2010	1,242	20,661	400	250	258,473	100,000
2011	661	19,441	300	280	300,694	181,221
2012	98	20,561	555	323	159,123	147,634
All years	32,498	243,378	550	310	369,834	135,293

Note.—The data are from the USDA, Economic Research Service, and USDA, National Agricultural Statistics Service, Agricultural Resource Management Survey (ARMS), 2000–2013. "All years" contains 2013 data in the "All" categories while there is no row for 2013 since any repeat farms would, by definition, have to have been observed prior to 2013.

largest difference we observe is 0.23 and the average absolute difference is 0.04, indicating substantial comparability across the two groups. We also make comparisons among farms meeting our sample criteria and find substantial comparability across the groups, with only one normalized difference exceeding 0.25.

2.4. Sample Farms

We narrow our sample of repeat farms to those most relevant for studying the effects of crop insurance. We focus on farms that participated in federal crop insurance in at least one of the years observed and whose primary outputs are insurance eligible, which we define as farms where at least half of the value of production in the first year observed came from crop insurance-eligible crops.[6] This gives

6. We calculate the value of production of insurance-eligible crops by using the market value of the farm's production of corn (for grain or silage), soybeans, cotton, sorghum (for grain or silage), barley, oats, wheat, and canola. These crops represented over 91% of all insured crops (excluding forage as a crop) in crop year 2014 according to RMA Summary of Business data.

a sample of 6,681 repeat observations, the majority of which reflect unique farms because most sample farms are observed only twice and therefore account for one repeat observation (table 1). To take full advantage of our panel data, we also included observations from farms observed three or more times. A farm observed three times contributes two observations to our sample, the difference from the first and second year observed and the difference between the second and third year observed. Excluding these farms has little effect on the results.

In the first year of each year-to-year difference, the average farm in the sample was operated by a 52-year-old whose farm had nearly $854,000 in production or about $380 per acre (table 3). The farm had 23% of its acres planted to corn, another 30% to soybeans, and 20% to wheat. It also harvested close to 85% of the acres it operated and had fertilizer expenses of $51 per acre and chemical expenses (e.g., herbicides and insecticides) of $45 per acre. All monetary amounts are in 2011 dollars.

A threat to our instrumental variable approach is that time-varying factors affected land and input use in a way that was nonlinearly related to the initial cov-

Table 3. Descriptive Statistics for the Sample Used in Estimation

Variable	Mean	SD	Median
Farm characteristics:			
Operator age	52	11	52
Off-farm income	44,600	116,000	26,250
Value of production	854,000	1,508,000	489,000
Wheat acres to total acres harvested	.2	.31	.01
Corn acres to total acres harvested	.23	.25	.14
Soybean acres to total acres harvested	.3	.27	.32
Change in premium per acre	2.48	11.2	1.2
Change in log premium per acre	.31	3.8	.28
Premium per acre in 2000	6.17	7.86	3.7
Premium per acre in 2013	11.3	11.59	8.64
Farm outcomes:			
Share of acres harvested	.84	.25	.92
Max share accounted for by one crop	.42	.36	.35
Value of production per acre	382	281	331
Fertilizer expenses per acre	51	47	40
Chemical expenses per acre	45	42	32
Fertilizer and chemical expenses per acre	96	77	78

Note.—The data are from the USDA, Economic Research Service, and USDA, National Agricultural Statistics Service, Agricultural Resource Management Survey (ARMS), 2000–2013. The descriptive statistics are for farms meeting the sample criteria as described in the text. The farm-level statistics are based on the first year the farm was observed. There are a total of 6,681 observations in the full sample used in our analysis. The premium per acre statistics are based only on farms observed for the first time in the reference year (n = 752 for 2000, and n = 1,199 for 2013). Monetary amounts are in 2011 dollars.

erage ratio. We cannot dismiss such a situation, but we can compare the beginning-period values for land and input use for farms across coverage-ratio terciles. Farms with initially similar practices are arguably more likely to experience similar time-varying factors than farms with differing practices. Table 4 shows that farms in the first tercile (low coverage ratios) harvested a larger share of their land and used more inputs per acre than farms in the second and third terciles. The differences, however, are surprisingly small. As mentioned earlier, a normalized difference in means of 0.25 standard deviations or less is often interpreted as indicating reasonably comparable groups (Imbens and Wooldridge 2009). In comparing the first and the second tercile, the normalized difference is 0.13 on average and is always less than a quarter. When comparing the first and the third terciles, the average normalized difference is 0.14 and is greater than 0.25 for only one variable–the share of harvested acres.

Figure 4*A*, 4*B* illustrates the geography of our sample and shows that sample farms are spread throughout the major row-crop regions of the United States and that increases in insurance coverage were not confined to a particular region. Figure 4*A* shows counties shaded based on their quartile for the number of sample farms. The distribution of sample farms generally matches where substantial production of key row crops occurs (to use the Economic Research Service Farm Resource Regions: the Southern Seaboard, the Mississippi Portal, the Heartland, and the Northern Great Plains). Figure 4*B* depicts counties based on the average log difference in premiums per acre, with counties again shaded by quartiles. Counties with large increases in coverage are spread across the regions where sample farms are present. In another map, we also show that the distribution of low-coverage-ratio farms generally follows the distribution of all sample farms (see appendix).

Given the unique nature of our panel, we assess the distribution of sample farms across years. We provide the number of farms observed in each year, pairing, for example, the number of farms observed for the first time in 2000 and for the second time in 2003 (see appendix). Farms are well distributed across years, with a farm most commonly observed two or three years apart. More than 11% of the sample was observed for the first time in 2000, prior to the implementation of the 2000 Farm Act that increased subsidies. Similarly, 68% were observed for the first time prior to the large increase in crop prices in 2007 (and therefore also before the implementation of the 2008 Act).

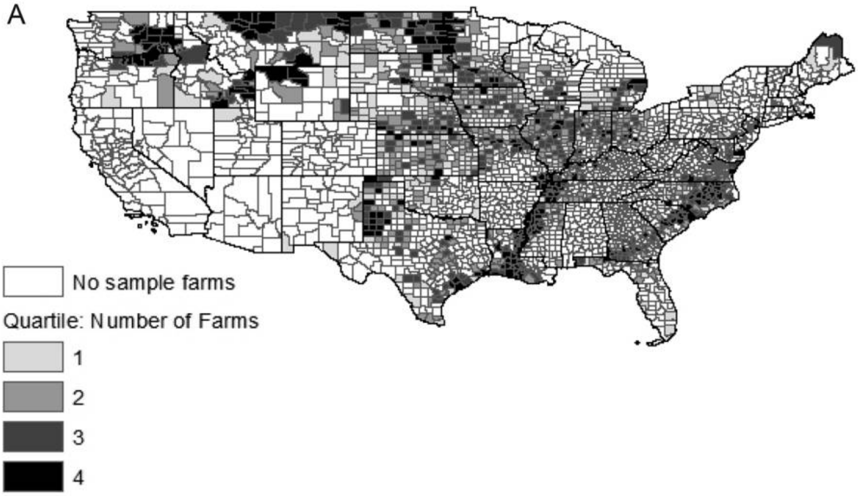
2.5. Weighting, Standard Errors, and Zeros

The sample statistics are based on unweighted data. The ARMS uses a stratified sampling design, and each observation has a weight based on its probability of selection. In the typical cross-sectional use of ARMS data, the weights permit using sample data to estimate population values such as the income of the average US farm. Because ARMS is designed to create a nationally representative cross-section

Table 4. Farms in Different Coverage-Ratio Terciles Had Similar Initial Levels of the Dependent Variables

	Coverage Ratio Tercile (Mean Ratio)						Tercile 2 vs. Tercile 1		Tercile 3 vs. Tercile 1	
	Tercile 1 (.04)		Tercile 2 (.28)		Tercile 3 (.82)					
Variables at Initial Period Values	Mean	SD	Mean	SD	Mean	SD	Diff.	Norm. Diff.	Diff.	Norm. Diff.
Share of acres harvested	.88	.23	.84	.25	.78	.28	−.04	−.11	−.10	−.29
Max share accounted for by one crop	.42	.34	.38	.36	.47	.37	−.05	−.10	.04	.08
ln(Value of production/acre)	5.80	.76	5.67	.83	5.57	.95	−.13	−.11	−.23	−.19
ln(Fertilizer expenses/acre)	3.67	.92	3.56	.95	3.55	1.01	−.11	−.08	−.12	−.08
ln(Chemical expenses/acre)	3.60	1.03	3.28	1.06	3.30	1.11	−.32	−.22	−.30	−.20
ln(Fertilizer and chemical expenses/acre)	4.38	.89	4.17	.93	4.17	.99	−.21	−.16	−.21	−.16

Note.—The initial period values are based on the first year of each year-pair observation. The coverage ratio terciles are based on each farm's initial coverage ratio (= initial premiums/premiums associated with maximum coverage). Monetary amounts are in 2011 dollars.



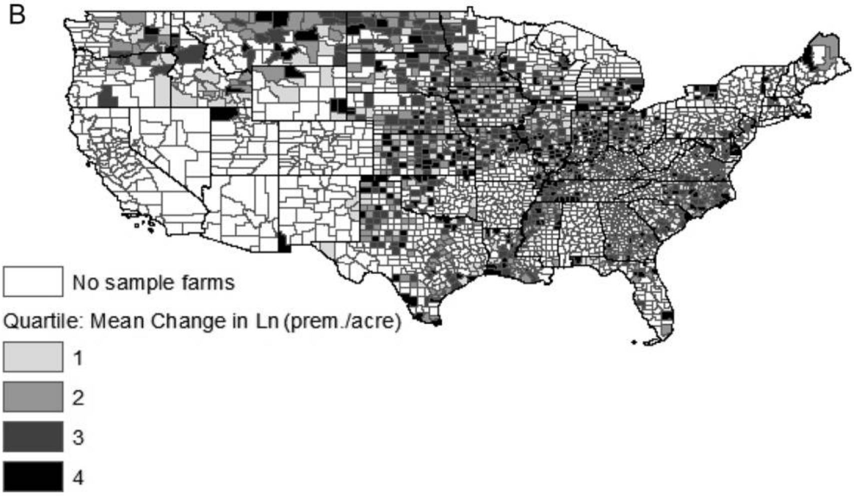


Figure 4. *A*, Sample farms are distributed across the major row-crop regions. *B*, Counties with high growth in coverage are spread across regions.

of farms rather than a panel of farms, the weights associated with repeat farms do not expand to a meaningful population. We therefore ignore the weights in estimation.

Researchers using ARMS normally account for sample design in estimating variances using a jackknife method with replicate weights provided by the USDA/NASS (e.g., Katchova 2005; Ahearn, El-Osta, and Dewbre 2006). This is an unattractive option because the replicate weights (like the base weights) are designed uniquely for each cross-sectional sample, not for the subsample of repeat farms. Facing a similar problem of needing to account for sample design without using weights, Weber and Clay (2013) cluster standard errors by each farm's survey stratum or location. The intuition is clear—clustering by stratum amounts to summing variances from mutually exclusive and exhaustive subpopulations. They show that clustering by strata or by location gives standard errors of similar magnitude, both of which are about two-thirds larger than the unclustered errors. Because we use county fixed effects, we cluster our standard error by county. The robustness section considers using crop reporting district fixed effects and clustering errors by district.

Because farms sometimes have zero insurance coverage in one of the years observed, our key dependent variable, the log difference in premiums per acre, is undefined for about a quarter of sample of farms. We take the common, though arbitrary, approach of adding a very small number to observations with a zero premium. To allow for a discrete effect of this arbitrary fix, we include in all models a dummy variable for whether the farm had a zero premium in the first year observed and another one for whether it had a zero premium in the second year observed. In the robustness section we present results for when these observations are excluded.

3. RESULTS

3.1. Ordinary Least Squares

Estimating equation (4) with OLS suggests that greater insurance coverage encourages farms to cultivate and harvest a larger share of their acres and use more fertilizer and chemicals per acre (table 5). A 10% increase in insurance coverage (measured by premiums per acre) is associated with a 0.11 percentage point increase in the share of acres harvested and a 0.44% increase in fertilizer and chemical expenses. Unsurprisingly, the value of production per acre also increases with greater coverage.

Qualitatively, these first-differenced OLS results fit the farm-level cross-sectional findings of Horowitz and Lichtenberg (1993) and Chang and Mishra (2012) as well as the county-level panel data findings from Goodwin et al. (2004), all of which show a positive association between insurance and fertilizer and chemical use. Yet, as highlighted before, such correlations may reflect unobserved factors that encourage a farmer to both intensify production and expand coverage.

3.2. Instrumental Variable Approach

Figure 2 depicts the hypothesized nonlinear relationship between the initial coverage ratio and the ratio of the second and first period premium. Using the sample data, we plot the actual linearized relationship as described by equation (3) (fig. 3). The slope of the line corresponds to ϕ in equation (3). It is negative as predicted: farmers with a larger log coverage ratio had a smaller proportional change in premiums

Table 5. OLS Estimates of the Effect of Crop Insurance Coverage

	Δ Share of Acres Harvested	Δ Max Share Accounted for by One Crop	Δ ln(Value of Prod./Acre)	Δ ln(Fertilizer Expenses/Acre)	Δ ln (Chemical Expenses/Acre)	Δ ln(Fert. + Chemical Expenses/Acre)
Δ log premium per acre	.011***	.003	.033***	.039***	.044***	.044***
	(.002)	(.002)	(.006)	(.009)	(.009)	(.008)
Initial premium per acre	.000	−.000	.000***	.000	.000***	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Control variables	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
County fixed effects	Y	Y	Y	Y	Y	Y
Observations	6,681	6,543	6,574	6,368	6,341	6,574
Adjusted R-squared	.024	.189	.112	.092	.030	.085

Note.—Robust standard errors clustered by county are in parentheses. County and year fixed effects are included as well as all the control variables mentioned in the text (dummy variables indicating zero premiums in the first or second year observed; linear and quadratic terms for the farm operator's age and the farm's total value of production; the initial share of harvested acres accounted for by soybeans, corn, and wheat, all separately). Other than the share variables, the dependent variables are per acre operated by the farm. The different number of observations across regressions is from some farms not having positive values for the outcome variable in at least one year.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

per acre. The line runs through the point (0, 0), which corresponds to the point (1, 1) in the hypothesized nonlinear relationship in figure 2.

We more formally establish the strength of the excluded instrument, the log of the coverage ratio, by estimating equation (4). A first-stage regression for the full sample shows that a 1% increase in the logged ratio was associated with 0.73% less growth in premiums per acre (coefficient of 0.724, standard error of 0.022) (table 6). The *F*-statistic for whether the coefficient on the logged ratio is zero is above 1,100, far higher than the thresholds provided in Stock and Yogo (2005) for the reliability of *t*-tests based on IV estimates and for a sufficiently low probability that the bias of the IV point estimates is less than 10% of the bias of OLS.

In contrast to the OLS estimator, the instrumental variable (IV) estimator shows that crop insurance slightly decreases the share of acres harvested and has little effect on input use (table 7). Compared to the statistically significant coefficients in the OLS regressions, the IV coefficients are multiple times smaller and yet with standard errors of roughly similar magnitude. OLS, for example, gives a coefficient of 0.044 on the change in premiums when looking at total fertilizer and chemical expenditures while the IV estimate is only 0.011.

The one case where both OLS and IV give similar results is for crop specialization. In both cases the coefficient is positive, but with point estimates that indicate economically insignificant effects: a 10% increase in crop insurance coverage leads to a 0.03 (OLS) to 0.05 (IV) percentage point increase in the share of acres harvested dedicated to the most harvested crop. Only the IV estimate is statistically distinguishable from zero.

3.3. Robustness

We perform nine robustness checks. The first six checks concern the general robustness of the results. The seventh and eight checks relate to heterogeneous effects and external validity. The ninth check addresses concerns about measurement error in premiums per acre.

First, we drop the farm characteristic control variables and only control for county and year fixed effects and the zero-premium indicator variables. If our instrument, the log of the coverage ratio, were substantially correlated with farm characteristics that affect our outcomes, we would expect large changes in our estimates. Sensitivity to controlling for observed characteristics, in turn, would suggest that estimates may also be sensitive to unobservable characteristics correlated with our instrument. Second, we exclude farms that had a zero premium in one year and for which we added a small number to the premium to permit taking the log. Third, instead of including a dummy variable for the first year observed and another dummy variable for the second year observed, we include a dummy variable for each unique year pairing (e.g., 2002 for year 1 and 2007 for year 2). This provides a general robustness check on

Table 6. The Initial Coverage Ratio Is Negatively Correlated with Increases in Coverage

	Δ Log Premium per Acre
Initial coverage ratio	−.724***
	(.022)
Initial premium per acre	−.000*
	(.000)
Wheat acres to total acres harvested	.189
	(.135)
Corn acres to total acres harvested	.019
	(.138)
Soybean acres to total acres harvested	.535***
	(.142)
Operator age	.010
	(.010)
Operator age squared	−.000
	(.000)
Total off-farm income	−.000
	(.000)
Total value of production	−.000
	(.000)
Total value of production squared	.000
	(.000)
Zero premium, first year	−4.166***
	(.135)
Zero premium, second year	8.363***
	(.071)
Intercept	−5.577***
	(.446)
County fixed effects	Y
Year fixed effects	Y
Observations	6,681
Adjusted *R*-squared	.90
F-statistic on the coverage ratio	1,130

Note.—Robust standard errors clustered by county are in parentheses.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table 7. Instrumental Variable Estimates of the Effect of Crop Insurance Coverage

	Δ Share of Acres Harvested	Δ Max Share Accounted for by One Crop	Δ ln(Value of Prod./Acre)	Δ ln(Fertilizer Expenses/Acre)	Δ ln (Chemical Expenses/Acre)	Δ ln(Fert. + Chemical Expenses/Acre)
Δ log premium per acre	−.007**	.005**	.014	−.001	.006	.011
	(.003)	(.003)	(.009)	(.014)	(.013)	(.010)
Initial premium per acre	−.000	−.000	.000***	−.000	.000**	.000
	(.000)	(.000)	(.000)	(.000)	(.000)	(.000)
Control variables	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
County fixed effects	Y	Y	Y	Y	Y	Y
Observations	6,681	6,543	6,574	6,368	6,341	6,574

Note.—Robust standard errors clustered by county are in parentheses. County and year fixed effects are included as well as all the control variables mentioned in the text (dummy variables indicating zero premiums in the first or second year observed; linear and quadratic terms for the farm operator's age and the farm's total value of production; the initial share of harvested acres accounted for by soybeans, corn, and wheat, all separately). Other than the share variables, the dependent variables are per acre operated by the farm. The different number of observations across regressions is from some farms not having positive values for the outcome variable in at least one year. The first-stage F-statistic for the excluded instrument (Δ log premium per acre) is 1,130.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

our approach to controlling for time shocks. It also controls for a modification in the calculation of premiums made in the 2008 Farm Act, which could cause a shift in premiums for farms observed before and after 2008.[7] Fourth, we control for the number of years between the first and the second observation. This helps address concerns about survivor bias as it allow for a correlation between years of survival and changes in farm decisions. Fifth, to address concerns that varying time lengths used in differencing may affect our results, we limit estimation to farms observed 3–5 years apart, which reduces our sample size by about two-thirds. Sixth, we use crop reporting district fixed effects and cluster our standard errors at the district level. Crop reporting districts are groupings of roughly 10 agriculturally similar counties.

The results are surprisingly stable across the first six robustness checks (table 8). Controlling for farm characteristics provides economically small and statistically insignificant coefficients for all outcomes. There is a change in statistical significance compared to the main results for the share harvested and the maximum share in one group, but even these main results were economically small. Excluding farms with zero premiums gives point estimates very similar to the main estimates. Likewise, including dummy variables for year pairings, including a length of time elapsed variable, limiting estimation to farms observed 3–5 years apart, or using crop reporting district fixed effects all provide results similar to the main results.

The seventh and eight checks address the applicability of the estimates of the response to greater crop insurance (β_1 in eq. [1]) to the broader population of crop farms growing insurance-eligible crops. Almost all farm-level empirical studies of crop insurance assume that conditional on covariates, the behavioral response to crop insurance coverage is the same for all sample farms (Horowitz and Lichtenberg 1993; Quiggin et al. 1993; Smith and Goodwin 1996; Wu 1999; O'Donoghue et al. 2009). Endogeneity issues aside, assuming a homogeneous behavioral response, and therefore the same coefficient across farms, OLS gives a consistent point estimate of this coefficient when using a simple random sample as well as when using a sample where large farms are oversampled, such as the normal ARMS sample or the repeat sample that we have constructed. If there are heterogeneous responses, weighted or unweighted OLS will not provide a consistent estimate of the populated-share-weighted average response, regardless of the sample design (see Deaton 1997, 67–79). The only solution to estimate interpretable coefficients is to simply estimate different equations (or at least different coefficients) based on each farm group for which the behavioral response is unique.

7. Prior to 2008, total premiums were mandated to be priced to generate a loss ratio of 1.075. The 2008 Farm Act mandated that total premiums should be priced to generate a ratio of 1.0 (actuarially fair). Including dummy variables for each unique year pairing will control for this change in policy. For information on the change, see Shields (2010, 2, n. 16).

The key issue for applying our results more broadly, then, is whether small and large farms, for example, have a fundamentally different response to crop insurance, in which case a distinct coefficient should be estimated for each type of farm. A reasonable test of the assumption of a homogeneous response is to estimate separate equations for different types of sample farms. We do this on two dimensions: crop specialization and farm size. For crop specialization, we split the sample based on how much corn a farm had in its original crop mix, with corn farms categorized as those where 25% or more of the value of production comes from corn. For farm size, we split the sample based into small and large farms based on having more or less acreage than the median farm.

When splitting the sample, the estimated coefficient on the change in premiums is statistically indistinguishable from zero 22 out of the 24 times (four samples and six outcomes) (table 8). The actual coefficients are generally statistically similar across subsamples as well. For farms specialized in corn, the 95% confidence interval for the coefficient on the change in premiums contains the coefficient estimated for farms not specialized in corn five out of six times. Similarly, for large farms, the confidence interval contains the point estimate for small farms five out of six times. Thus, the overall results suggest that greater crop insurance coverage has little effect on farm behavior and that this is true for different types of farms.

The one statistically strong and economically large result from the sample is for the value of production per acre on small farms. However, this was not associated with greater input use. One interpretation is that insurance encourages smaller farmers to switch to higher value crops that require more investment but not more fertilizer or pesticide.

The final robustness check addresses concerns about measurement error in our measure of crop insurance coverage, the change in premiums per acre. Although our measure has advantages over past measures, it is still open to improvement. Ideally we would use the total premium per acre (the combined farmer and government-paid premium), which is not collected in the Agricultural Resource Management Survey. The total premium would provide a more precise measure of the change in coverage for farms observed before and after the 2008 Farm Act, which changed subsidy rates for farms selecting policies involving enterprise units.

For farms where both observations occur in the 2001–7 window, the percentage change in the farmer premium equals the percentage change in the total premium since the relationship between the two was fixed by law during this period. We exploit this fact to explore the possibility of attenuation bias from measurement error and find little indication that measurement error is a problem. If it were a problem, OLS estimates for the change in premiums in equation (1), β_1, should be larger when using only farms observed for the first and second time between 2001 and 2007. Yet, we find estimates of β_1 that are similar to or smaller than those from the full sample where measurement error is presumably greater (see appendix).

Table 8. The Robustness of the Estimates of the Effect of Insurance Coverage

Sample/Specification	Δ Share of Acres Harvested (1)	Δ Max Share Accounted for by One Crop (2)	Δ ln(Value of Prod./Acre) (3)	Δ ln(Fertilizer Expenses/Acre) (4)	Δ ln (Chemical Expenses/Acre) (5)	Δ ln(Fert. + Chemical Expenses/Acre) (6)
Main results (for comparison)	–.007**	.005**	.014	–.001	.006	.011
	(.003)	(.003)	(.009)	(.014)	(.013)	(.010)
Excluding farm covariates	–.002	–.000	–.002	–.002	.001	.004
	(.001)	(.001)	(.003)	(.005)	(.005)	(.004)
Farms with positive premiums	–.009**	.005	.006	–.011	.009	.001
	(.004)	(.003)	(.010)	(.017)	(.015)	(.012)
Year-pair dummy variables	–.007**	.005*	.014	.002	.007	.013
	(.003)	(.003)	(.009)	(.014)	(.013)	(.010)
Add (year 2 – year 1) variable	–.007**	.005**	.014	–.001	.006	.011
	(.003)	(.003)	(.009)	(.014)	(.013)	(.010)
Periods 3–5 years apart	–.003	.003	.018	.000	–.028	–.007
	(.005)	(.006)	(.018)	(.028)	(.028)	(.024)

Crop reporting district	−.008**	.004	.005	.001	.001	.008
	(.003)	(.003)	(.011)	(.016)	(.012)	(.010)
Farms specialized in corn	−.004	−.001	.026	.052*	.021	.033
	(.006)	(.004)	(.019)	(.029)	(.028)	(.021)
Farms not specialized in corn	−.008	.006	.017	−.025	−.012	−.004
	(.005)	(.004)	(.012)	(.021)	(.016)	(.014)
Large farms	−.002	.003	.013	−.005	.006	.015
	(.004)	(.004)	(.011)	(.018)	(.019)	(.013)
Small farms	.001	.003	.077***	.003	−.017	.010
	(.007)	(.007)	(.019)	(.027)	(.028)	(.024)

Note.—Robust standard errors clustered by county are in parentheses. For the row "Full sample, excluding farm covariates," only the zero-premium indicator variables and the county and year fixed effects are controlled for. The other regressions include county and year fixed effects as well as all the control variables mentioned in the text. Other than the share variables, the dependent variables are per acre operated by the farm. Specialization in corn farming is based on having at least 25% of the farm's value of production coming from corn. The large and small farm categories are based on being above or below the sample median acres operated. For all regressions, the first-stage *F*-statistic on the excluded instrument is well above thresholds for weak instrument bias.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

4. DISCUSSION

4.1. Can We Reject Economically Important Effects?

In many instrumental variable applications, large standard errors can prevent a rejection of the null hypothesis of a zero effect but, at the same time, not allow researchers to rule out economically important effect sizes. This is not the case for our results, where even the upper bound estimated effects are economically small. Table 9 presents the upper bound on the 95% confidence interval for the effect of crop insurance on each outcome (col. 2). For sample farms, premiums per acre doubled from 2000 to 2013 in real terms, going from \$6 to \$12 dollars per acre. This is also true for all participating farms as calculated from the 2000 and 2013 cross-sections of the ARMS. A doubling of premiums would translate into a 0.7 increase in the log premium per acre (ln(12/6)). In column 3, we multiply this change in log premiums by our upper bound estimate.

The upper bound estimate of the effect of a doubling of crop insurance coverage on the share of land harvested is zero. The effect on the maximum share of land in one crop is larger: a 0.8 percentage point increase, which translates to less than one additional acre allocated to the most planted crop in a 100 acre farm. The upper bound estimate of the effect on the value of production is slightly larger, at 2.2%, though this is still an economically small effect.

For fertilizer and chemical use, we draw from existing studies to translate an upper bound estimate of use into a percentage increase in externality. For fertilizer, our upper bound estimate suggests that doubling coverage would cause a 1.3% increase in fertilizer nutrients leaving the field (col. 5 of table 8). This estimate comes from multiplying our upper bound estimate of the increase in fertilizer expenses (1.9%) with the estimate of fertilizer loss from Li et al. (2006). They found that a 1% increase in the fertilizer application rate on Iowa corn and soybean fields leads to a 0.7% increase in nutrients in water flowing in tiles that drain agricultural areas, which would likely result in an even smaller percentage increase in nutrients in larger streams (col. 4 in table 8).[8] It is reasonable to apply these numbers to our study, which has many Midwestern corn and soybean farms, and assume that a 1% increase in fertilizer expenses per acre would translate into a similar increase in the fertilizer application rate.

The implied (upper-bound) elasticity between crop insurance coverage and fertilizer loss is 0.013 (= 1.3/100). By comparison, Hendricks et al. (2014) find an elasticity between corn prices and nitrogen loss of 0.074, almost six times the elasticity associated with crop insurance coverage.

8. Gowda, Mulla, and Jaynes (2008) also conduct a farm-level study in the Midwest and find a similar result: a 1% decrease in the fertilizer rate was associated with a 0.85% decline in nitrate losses.

Table 9. The Economic Magnitude of Our Findings

	Point Estimate (1)	95% CI Upper Bound (2)	Change for a 100% Increase in Premiums per Acre (%) (3)	Elasticity of Contamination (4)	Increased Presence in Waterways (%) (5)
Share of acres harvested	−.007	−.000	−.0	. . .	
Max share in one crop	.005	.011	.8	. . .	
Value of production	.014	.032	2.2	. . .	
Fertilizer expenses	−.001	.027	1.9	.7	1.3
Chemical expenses	.006	.032	2.2	.5	1.1
Fertilizer and chemical expenses	.011	.031	2.2	. . .	

Note.—The doubling of premiums per acre is based on the observed change in premiums per acre from 2000 to 2013 (roughly \$6–\$12 per acre). The results in column 3 are from multiplying column 2 by .70 (= ln(12/6)). For the share variables, the numbers in column 3 refer to the percentage point increase in the share (e.g., a .8 percentage point increase in the max share of one crop). The elasticity of contamination for fertilizer (col. 4) is from Li et al. (2006) and represents the percentage increase in nitrogen in tile drainage water for a percentage increase in fertilizer application. The elasticity of contamination for chemical expenses refers to the percentage increase in atrazine in waterways resulting from a 1% increase in atrazine application as estimated by Tesfamichael et al. (2005). Column 5 comes from multiplying column 3 with column 4.

Our finding for chemical usage suggests an upper bound increase of pesticides in nearby waterways of 1.1%. The most common component of chemical expenditures is pesticides. Using data from the National Water Quality Assessment program, Tesfamichael, Caplan, and Kaluarachchi (2005) estimate that a 1% increase in the application rate of atrazine led to a 0.5% increase in the concentration of atrazine in streams. (Atrazine is a commonly used pesticides and was the second most commonly found pesticide in a nationwide survey by the EPA [US EPA 1990].) Our upper-bound estimate suggests that a doubling of crop insurance premiums would cause a 1.9% increase in chemical expenses. Supposing the increase in chemical expenses is associated with a similar increase in quantity of pesticide applied, our estimate multiplied by that of Tesfamichael et al. (2005) suggests a 1.1% (= 2.2% × 0.5%) increase in the concentration of pesticide in streams.

4.2. What Our Empirics Do and Do Not Capture

Our measures of fertilizer and chemical use are per acre operated by the farm. It is possible that insurance subsidies caused marginal lands to be brought into cultivation. If the land was originally part of the farm (e.g., in pasture) and crop insurance encouraged the farmer to convert it to cropland, we would observe increases in the value of production per acre, the share of land harvested, and fertilizer and chemical expenses per acre. If, however, crop insurance encouraged the farm to acquire the land, our outcome variables would only increase if the farmer used more fertilizer on it (or had a more specialized crop mix and so forth) than the average acre already operated by the farm. Otherwise, we would not capture the effect.

We do not know how land acquired between the first and second time observed may have differed from land already in the farm. But we can test if crop insurance was associated with farms acquiring more land. Using the log difference in the total acres operated as the dependent variable, we find that greater insurance coverage was not associated with an increase in acres operated (coefficient of −0.01, standard error of 0.007). This result combined with the lack of an effect on the value of production suggests that insurance did not cause participating farmers to intensify production on marginal lands.

Still, it is possible that both high- and low-coverage farms acquired land at similar rates, with farms with high coverage tending to acquire marginal lands (and intensify production on them) while low-coverage farms tended to acquire better lands. However, this would require that high-coverage farmers replaced high quality land with marginal land such that the total acres operated did not change, which seems unlikely.

Our empirics capture the effect of expanding crop insurance coverage in a period when other farm programs changed very little. We do not examine the effects of replacing any particular farm program with crop insurance. Over our study period, crop insurance premium subsidies and plans increased while the main farm income support program, the Direct Payment program, remained in place, paying around $5 billion each year to qualified farmers. Shortly after our study period, however, Congress passed the 2014 Farm Act, repealing the Direct Payment program in favor of strengthening crop insurance. The shift in programs will likely have minor environmental consequences. Weber and Key (2012) present evidence that the Direct Payment program did not affect production or harvested acreage. And although farmers had to comply with conservation provisions to receive payments, with the 2014 Act Congress transferred similar provisions to crop insurance. To be eligible for premium subsidies, the provisions require that farmers with highly erodible land or wetlands maintain conservation practices in line with National Resources Conservation Service guidelines.

5. CONCLUSION

Policies with nonenvironmental goals can cause unintended environmental harm. Using a novel data set and identification strategy, we find that federal crop insurance does not appear to fall into this category despite several past studies suggesting otherwise. Farmers who expanded crop insurance coverage during the 2000–2013 period had changes in land use, crop mix, and fertilizer and chemical use similar to farmers with smaller or no changes in coverage. Our finding is striking because the changes in crop prices over the period caused farmers to plant more corn, a high-value and input-intensive crop. One may have expected increasingly generous insurance subsidies to have accentuated this shift.

Although our results are based on the 2000–2013 period, they arguably hold under the 2014 Farm Act in which policy makers linked premium subsidies to conservation compliance on erodible lands or wetlands. Our findings of small effects of crop insurance coverage on farmer decisions combined with the recent linking to conservation requirements suggest that the federal crop insurance program should not have substantial negative environmental implications moving forward.

Looking beyond crop insurance, our panel data set of nearly 32,500 distinct farms in the 2000–2013 period lends itself to studying a wide range of agro-environmental issues and their links with program participation and farm household finances and characteristics. As the panel expands with each passing year, it will aid in studying the effects of the shifts in farm and conservation policy that occurred in the 2014 Farm Act, including the reduction of the Conservation Reserve Program, the re-linking of insurance premium subsidies to conservation compliance, and the elimination of the direct payment program.

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