# Stripping Because You Want to Versus Stripping Because the Money is Good: A Latent Class Analysis of Farmer Preferences Regarding Filter Strip Programs

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Abstract:

We examine the stated preferences of Ohio corn and soybean farmers in the Maumee watershed regarding government grass filter strip programs. Our analysis utilizes a quantitative measure of farmers' perceptions of how effective filter strips are at reducing runoff and controls for potential endogeneity through a control function approach. We additionally estimate two models, one that assumes preference homogeneity and a latent class model that allows for preference heterogeneity. The latent class model is a superior model based on fit of the data. Additionally, we find that the homogeneous preference model produces marginal effects and willingness to accept estimates that are drastically different from those produces using latent class analysis.

Nutrient pollution has inflicted substantial damages to vital ecosystems both in the United States and worldwide, resulting in a reduction of deliverable ecosystem services (Kemp *et al*. 2005; Huisman *et al*. 2005; Rabalais *et al*. 2007; Lotze *et al*. 2006; Diaz and Rosenberg 2008). In recent decades, nonpoint source pollution, especially from agriculture, is responsible for an increasing share of nutrient loading (OEPA 2010). The increased incidence of severe storm events that are predicted with climate change (Milly *et al*. 2005, Moberg *et al*. 2006) are likely to exacerbate this problem (Jeppeson *et al*. 2009, Joseph *et al*. 2009). Governments have responded to these issues primarily by establishing voluntary programs that compensate farmers for adopting agricultural best management practices (BMPs). These are also called payment for ecosystem services (PES) programs.

There is a large literature dedicated to understanding farmer adoption of BMPs. Much of this research has focused on farmers adopting conservation practices without monetary incentives (Erven and Erven 1982; Gould *et al.* 1989; Bosch *et al*. 1995; Wu and Babcock 1998; Soule *et al*. 2000; Roberts *et al*. 2006; Davey and Furtan 2008). In addition to this literature, some work has examined farmer preferences for paid BMP programs. Zbinden and Lee (2005) and Lambert *et al*. (2006) consider actual program participation, while Purvis *et al*. (1989) and Ma *et al*. (2012) examine stated preference responses for hypothetical programs. This paper provides an analysis of farmer stated preferences for hypothetical filter strip programs and extends the previous literature in two important ways. First, our analysis allows for farmer preference heterogeneity through latent class analysis (LCA). We compare the heterogeneous model to a standard conditional logit model that assumes preference homogeneity and find that, for both marginal effects and willingness to accept (WTA) estimates, the assumption of preference homogeneity can bias the results. As a second contribution, this research examines how farmer perceptions of program efficacy in reducing runoff influence their preferences toward filter strip programs. While previous work has attempted to control for these perceptions, this paper presents a novel approach that controls for variable endogeneity.

It is common practice to assume that farmers are purely profit maximizers. We endeavor to focus on farmers as utility, rather than profit, maximizers. Our theoretical model treats farmer utility as a function of consumption, which is dependent on income and motivates the profit-maximization inclinations of farmers, and environmental services[1], which are influenced by environmental quality rather than profit or income. It is reasonable to posit that for some farmers, who we call environmental stewards, environmental services and environmental quality will have a greater impact on utility compared to other farmers (profit-maximizers). LCA accounts for this type of preference heterogeneity by assuming that the farmer population is comprised of several unobserved, or latent, classes. All farmers in a particular class possess homogeneous preferences, while preferences are allowed to vary across classes. We adopt a semi-parametric estimation of LCA in which the number of classes is not assumed *ex ante*, but is instead derived from the data through the use of the Bayesian Information Criterion (BIC). This methodology is also being applied to farmer best management practices by Konar *et al.* (2012), although this working paper considers the impact of farmer and field characteristics on actual tillage choice, while the present paper considers the impact of program characteristics on preferences toward hypothetical government filter strip programs.

Latent class analysis is one of several methods capable of addressing farmer preference heterogeneity. Perhaps the simplest method to model preference heterogeneity is the inclusion of

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[1] This could be considered the consumption of environmental services, the provision of environmental services, or a combination of the two.

interaction terms, which allows for heterogeneity based on observable characteristics. Unfortunately this method is of limited value when the source of heterogeneity is unobserved. In the absence of a variable that clearly delineates farmers into classes or otherwise specifies heterogeneity, the heterogeneity is latent or unobserved and thus controlling for heterogeneity through the use of interaction terms is insufficient. Random parameters models (also called random coefficients, mixed, or mixture models) also allow for heterogeneity by estimating variable coefficients and an individual-specific standard deviation parameter for each coefficient (McFadden and Train 2000, Train 1998, Columbo, Hanley, and Louviere 2009). Peterson *et al*. (2012) use a random parameters model to estimate the transaction costs associated with hypothetical PES programs using farmers in a lab setting. The advantage of latent class analysis for the current study lies in the discrete rather than continuous nature of preference heterogeneity we anticipate in the farmer population. We suspect that a portion of farmers are environmental stewards, and the preferences of farmers within this group will be relatively homogeneous and will differ from the preferences of farmers who do not identify as environmental stewards but are instead pure profit maximizers. In addition, by avoiding the assumption of a continuous distribution of preferences, we are not forced to make assumptions regarding the shape of this distribution. Latent classes can also improve communication of results to policy makers, who often find discussion of distinct classes of farmers more intuitively appealing than discussions of random parameters output or complex interaction terms. Previous work has argued that latent class approaches provide a more robust and tractable approach to representing preference heterogeneity than the random parameters approach (Greene and Hensher 2003; Hess *et al*. 2011; Shen 2009), although there is no consensus on this issue (Allenby and Rossi 1999). Indeed, we find that model preference varies by field, with some fields emphasizing random parameters models (e.g. marketing) and others emphasizing latent class models (e.g. transportation economics).

Previous work has found that, while homogeneous preference models fail to capture many details that are illuminated in Latent Class and other heterogeneous models, they are often good approximations of average coefficients, marginal effects, and WTP/WTA estimates. Many studies have found that parameter estimates for homogeneous models fall between the values found if different classes of Latent Class models (Campbell et al. 2011; Shen and Saijo 2009; Wallmo and Edwards 2008). Our estimations find that, while this is often the case, it is also possible for homogeneous preference models to provide marginal effects and WTP/WTA estimates that lie outside the range found in the Latent Class model. The result is that a single homogeneous preference model can provide relatively unbiased estimates for some variables of interest while providing severely biased estimates for other variables. To our knowledge this is the first research to highlight this troubling result.

In addition to larger payments, farmers are more likely to engage in conservation practices when they have higher incomes, larger farms, more education, lower quality soil, and when they express greater levels of concern for the environment (Lambert *et al*. 2006, Bosch *et al*. 1995, and Gould *et al*. 1989). However, to our knowledge, no studies have been able to adequately isolate the degree to which farmers believe these practices will reduce runoff. It is reasonable to expect that farmers who believe BMPs to be effective would be more likely to adopt these practices, whether incentivized or not. Furthermore, this effect may differ from one latent class to another, thus indicating the need to control for preference heterogeneity. Ma *et al*. (2012) include controls in their estimation that capture farmer perceptions of ecosystem benefits from the considered BMP program. This measure is qualitative (a 5 point Likert scale), and as such is slightly different from the continuous quantitative measure undertaken here. More importantly, this variable is likely to be endogenous, as there are unobserved farmer attributes (social networks, for example) that are correlated with both perceptions of BMP efficacy and program choice. We test our measure and reject the null hypothesis of exogeneity (p-value 0.005) and so utilize a control function approach to control for this endogeneity.

We find that farmers do exhibit preference heterogeneity regarding filter strip program selection. Our sample of corn and soybean farmers in the Maumee watershed falls into two latent classes. Both classes prefer programs that offer higher payment per acre, lighter paperwork burdens, and require narrower filter strips. The smaller class exhibits a strong status quo preference, suggesting that members of this class are generally less likely to enroll in conservation programs, while the larger class displays no statistically significant preference for or against PES programs. Furthermore, the class with a strong status quo preference shows no significant effect of increased perceptions of filter strip efficacy, while increases in these perceptions increase the probability of selecting a program for the class without a status-quo preference. We compare these results to a standard conditional logit model that assumes preference homogeneity. We find that the standard and latent class models produce similar estimates of total WTA in the Maumee watershed for most variables, but the estimates are remarkably disparate for the perceived efficacy variable, with the latent class model producing estimates more than an order of magnitude larger than the homogeneous preference model.

The rest of the paper proceeds as follows. The next section describes the survey and data from which our results derive. Subsequent sections outline the theoretical and empirical models utilized, results, and concluding remarks.

## Data

Data are from a 2012 mail survey of farmers in the Maumee watershed, located primarily in northwest Ohio. We received 817 responses from a total of 2000 surveyed corn and soybean farmers (40.85% response rate). Of these, 596 indicated that they operated a farm in 2011. Many of those who responded did not complete the entire survey, so our analysis is limited to 364 farmers for whom we have no missing variables of interest. Table 1 compares demographic information for the sample of 364, the broader sample of up to 596, and the entire farmer population for counties in the Maumee watershed (USDA, 2009). Our sample is skewed toward large farms with high gross sales and farmers who additionally earn off-farm income.[2] Addresses for the targeted sample were provided by a private vendor, and were pulled from lists of farmers receiving government payments and from farming magazine rolls. The survey was conducted using a variation of the tailored design method (Dillman 2007). The total set of mailings included an announcement letter, a survey packet, a reminder letter and a replacement packet for non-responders. Those who completed the survey were entered into a raffle for one free pair of tickets to an Ohio State Buckeyes home football game. Several months before the initial mailing of the survey a pilot test was conducted with farmers recruited by local extension professionals.

The survey contained a section in which respondents were asked to "Consider one of your fields where runoff is a potential problem and where no filter strip exists." The survey then asked a series of questions regarding basic field attributes, including the field's distance from the

[2] The analysis presented in this paper uses the unweighted sample. Additional analysis using weights that produce the demographics in the final column of Table 1 was undertaken. The results using the weighted sample are qualitatively the same as those using the unweighted sample, and so are not presented here. They are available upon request from the authors.

nearest surface water, slope, soil type, and whether the field had working drainage tile. After these questions, respondents were asked, “How likely is it that a 1-inch rainfall during a 30-minute storm event in mid-June would cause soil to run off into nearby surface water?” under three scenarios: 1) the field as it currently is with no filter strip, 2) the field with a 25-foot filter strip, and 3) the field with a 75-foot filter strip. Respondents were prompted to report this likelihood as a probability from 0% to 100%.

Following this section, the survey read,

> “Consider a situation where there is a voluntary program to establish *filter strips*. Sufficient state and federal funds are available to ensure that all applicants will be enrolled. Two options are available. Both options feature *100% reimbursement of the costs* for establishing the entire filter strip plus an annual rental payment detailed below.”

The survey then detailed two filter strip programs and asked respondents to rank these two programs and their current program (i.e., a status quo option, which featured no filter strip program) as “best,” “middle,” and “worst.” All filter strip programs allowed for mowing and specified that inspections will be annual and announced. The programs were allowed to differ, however, in filter strip width (25 or 75 feet), paperwork burden (two, five or ten hours per year), annual rental payment (125, 175, 200 or 250 dollars per acre), and program length (five or ten years). Given the mail-survey format allowed for limited survey length, each respondent was presented two choice sets, each featuring two filter strip programs along with the status quo option. Each choice set featured one program with a 25-foot filter strip and one with a 75-foot filter strip with the order of appearance (first or second program presented) randomized.

Program length was identical for the two programs within each choice set, but each respondent saw one pair of choices where both featured a 10 year length and one pair of choices where both featured a 5 year length with the order of appearance (first or second set) randomized. Finally, paperwork burden and annual rental payment levels were chosen such that each program within a choice set featured levels different from one another. We employ a full factorial experimental design, given the restrictions that program length was always the same for both programs in the choice set and both programs in the choice set could not be identical[3]. The survey also collected basic demographic information as well as less common farmer- and farm-level attributes, including risk tolerance and enrollment in current government-sponsored conservation or BMP programs.

## Theory and Econometric Method

### *Theoretical Model*

The theoretical specification of this paper assumes that farmers rank filter strip programs through a process of utility maximization in which utility is a function of consumption goods ($C$) and environmental services ($E$). The model is an adaptation of Dupraz *et al.* (2003) and Ma *et al.* (2012). Broadly speaking, consumption is bounded by income ($I$). Income is a function of farm profits ($\pi$) and off-farm income ($R$). Farm profits are influenced by farm output ($Y$), the level of variable inputs ($X$) (hired labor and planted land, for example), inputs like management and

[3] The full factorial experimental design should allow us to accurately estimate all main effects and interaction effects (Louviere et al. 2000). However, because we do not receive responses from every farmer solicited, we most likely do not have data on the full factorial. This would likely lead to inefficient experimental design, and could potentially bias estimates. However, we argue that given the random nature of choice set assignment to each potential respondent, the gaps in our full factorial design should also be random. This will probably lead to an inefficient design, but is not likely to systematically bias our estimates in any particular direction.

machinery that are fixed over the considered time period ($F$), environmental services ($E$) and prices ($p$). Without loss of generality, the cost of fixed inputs is assumed zero. This setup yields the following utility maximization problem:

$$\max_{X,E} \quad U(C, E) \tag{1}$$

$$where\ C \leq I = \pi + R$$

$$and\ \pi = p_y Y(E,X, F) - p_x X.$$

Off-farm income is treated exogenously in the model. The farm production function $Y(.)$ is increasing in $X$ and non-increasing in $E$.[4] The levels of environmental services and inputs that maximize utility are given by ($X^o$, $E^o$). Implementing a filter strip program reduces runoff, but requires land and labor be diverted from farming, both to establish and maintain the filter strip and to complete paperwork and other compliance activities. In this way, a filter strip can be seen as a pair of changes ($\Delta E$, $\Delta X$), where $\Delta E$ is nonnegative and $\Delta X$ is nonpositive.

Noting that consumption can be written as a function of income, which is itself a function of inputs and environmental services, $C(I(X,E))$, a farmer's minimum willingness to accept ($WTA$) for the installation of a filter strip ($\Delta E$, $\Delta X$) satisfies the following equation:

$$U(C[I(X^o, E^o)], E^o) = U(C[I(X^o + \Delta X, E^o + \Delta E) + WTA], E^o + \Delta E). \tag{2}$$

A filter strip program offer is modeled as a trio of changes ($\Delta E$, $\Delta X$, $Z$), where the first two terms capture the effects of the filter strip and $Z$ is the program payment to the farmer. The farmer will accept the offered program provided $Z \geq WTA$, or

[4] There may be instances where further provision of environmental services can increase farm production, but this is ignored for simplicity.

$$U(C[I(X^{o} + \Delta X, E^{o} + \Delta E) + Z], E^{o} + \Delta E) \geq U(C[I(X^{o}, E^{o})], E^{o}). \tag{3}$$

*Empirical Model*

We now assume that utility obtained from individual *n* choosing alternative *i* is comprised of a systematic or observable element, denoted *V*, and a random error term $\varepsilon$, so the following equation holds:

$$U_{ni} = V_{ni} + \varepsilon_{ni} \tag{4}$$

Assuming the error terms are i.i.d. with a type 1 extreme value distribution and homogeneity of farmer preferences, the probability that a farmer will choose policy alternative *i* as the best (or highest ranked) from a set of policy alternatives $\{1,\ldots, I\}$ is given by

$$Pr_n(i) = \frac{exp(\beta' X_{ni})}{\sum_{i=1}^{I} exp(\beta' X_{ni})}, \tag{5}$$

where $X_{ni}$ is a vector of attributes associated with program *i* for farmer *n* and $\beta$ is the vector of estimated coefficients associated with these attributes. This is the standard conditional logit model. Farmer responses in our survey gave a ranking, but for the purposes of this model we convert this ranking to an indicator variable equal to one if the program is considered the best and zero otherwise.

The assumption of farmer homogeneity is likely to be impractical. Potential heterogeneity of farming goals (environmental stewardship vs. profits) suggests that changes in program attributes may have different marginal impacts on program adoption for different farmer groups. For instance, farmers who place their emphasis exclusively on making a profit may be greatly influenced by changes in program payment while caring little about the environmental benefits of the program. Conversely, farmers who value environmental stewardship may care less about program payment and more about the environmental benefits of the program.

To allow for this potential preference heterogeneity, we also estimate a conditional logit using latent class analysis (Bhat 1997; Birol *et al*. 2006; Columbo *et al*. 2009). Preferences within a specific class are homogeneous, but preferences are allowed to vary across classes. Under these assumptions, the probability that a farmer *n* will choose a series of policy alternatives $\{i_{1 \dots} i_T\}$ from a set of policy alternatives $I \times I$, $I = \{1, \dots, I\}$, conditional on the farmer belonging to class *s*, is given by

$$Pr_n(i \mid s) = \prod_{t=1}^{T} \frac{exp(\beta_s{}'X_{nit})}{\sum_{i=1}^{I} exp(\beta_s{}'X_{nit})}, \tag{6}$$

where $\beta_s$ is the vector of estimated coefficients associated with attributes $X_{nit}$ in class *s*. We assume that one farmer's choice is independent of the choices of other farmers. However, because a farmer's choice is not independent of other choices made by the same farmer, equation (6) does not treat each choice as an isolated incident but instead describes the probability of a farmer making a series of *T* choices. The probability that farmer *n* belongs to class *s* is given by

$$Pr_n(s) = \frac{exp(\gamma_s Z_n)}{\sum_{s=1}^{S} exp(\gamma_s Z_n)}, \tag{7}$$

where $Z_n$ is a vector of farmer-specific characteristics and $\gamma_s$ is a vector of coefficients associated with class *s*. Equation (6) captures a conditional logit, where choice probabilities are determined by choice-specific attributes, while equation (7) captures a multinomial logit, where class probabilities are determined by farmer-specific attributes (McFadden 1973). Assuming independence of the probabilities outlined in equations (6) and (7), the unconditional probability that farmer $n$ will choose a series of policy alternatives $\{i_{1 \ldots}\ i_T\}$ is

$$Pr_n(i) = \sum_{s=1}^{S} \left[\frac{exp(\delta_s Z_n)}{\sum_{s=1}^{S} exp(\gamma_s Z_n)}\right] \times \left[\prod_{t=1}^{T} \frac{exp(\beta_s X_{nit})}{\sum_{i=1}^{I} exp(\beta_s X_{nit})}\right]. \qquad (8)$$

In our data, farmers are presented with two choice sets, so $T = 2$. The program attributes ($X_{nit}$) included in our estimations are outlined in Table 1. They include the annual per-acre payment given for land converted to filter strips, the required filter strip width, the annual paperwork burden associated with the program, the program length in years, and an alternative-specific constant denoting whether the choice was the status quo of not enrolling[5]. Additionally, we include a variable that captures farmers' perception of the efficacy of filter strips in reducing the likelihood of runoff. This variable is created by taking the difference of farmers' reported runoff probabilities with a filter strip on the field in question and without a filter strip. As an example, if a farmer reports the probability of runoff is $A$ in the absence of a filter strip, $B$ with a 25 foot filter strip, and $C$ with a 75 foot filter strip, the perceived efficacy variable is $A - B$ for a 25 foot filter strip program, $A - C$ for a 75 foot filter strip program, and zero ($A - A$) for the status quo of no program.

It is reasonable to question whether this perceived efficacy variable poses an endogeneity problem. Unobserved farmer characteristics that are captured in the error term (like farmer social networks) may influence both perceptions of filter strip efficacy and program preferences. We use the Durbin-Wu-Hausman test and reject the null of exogeneity (p-value = 0.005). The details of these results are located in the appendix. To address the endogeneity of perceived efficacy, we

[5] As there are three alternatives, it is possible to include two alternative specific constants. We chose to include only one on the basis that we don't expect any systematic difference between Program A and Program B, especially one that would influence the estimated coefficients of our variables of interest.

utilize an instrumental variable control function approach (Train 2009). The details of this approach, along with first stage results, are in the appendix.

**Results**

The estimations that follow are obtained using Latent Gold Choice 4.5 and Stata 11 statistical software. Results from the conditional logit estimations are presented in Table 3. The “pooled” column results assume preference homogeneity. In the language of LCA this is equivalent to assuming that all data fall into a single class. We will use this traditional estimation as a baseline for comparison with our LCA results. In the pooled estimation, increased payment per acre, decreased filter strip width, and decreased paperwork burdens all increase the probability of a program being selected. Program length and perceptions of filter strip effectiveness have no significant impact. Additionally, the alternative-specific constant for no program, which we call the status quo, has no significant effect. This means that farmers have no statistically significant preference for or against enrollment in programs beyond what can be attributed to measureable program attributes.

When allowing for preference heterogeneity via LCA, a different and more nuanced story arises from the data. We find a two class model provides the best fit for our data by virtue of minimizing the BIC.[6] Table 4 presents a breakdown of the two classes by farmer-specific covariates included in the model. One class, which we call the “Environmental Stewards,”

[6] The two class model has a BIC of 2188.22. The BIC for the one class model is 2286.77 and models with three or more classes fail to converge. Nylund et al. (2007) use Monte Carlo simulations to show that the BIC outperforms all other information criteria measures at predicting number of classes for LCA.

comprises 62% of the sample[7]. We label these farmers Environmental Stewards because they are less than half as likely to use conventional tillage practices (and so are more likely to engage in either conservation tillage or no-till) as the “Others” class. As one would predict for Environmental Stewards, they are also more likely to be enrolled in conservation programs (*Enrolled*), although the difference is not statistically significant. Farmers in the Environmental Steward class are also younger, have received more formal education, are more likely to recreate in Ohio rivers, lakes and streams, and are less likely to be first generation farmers, although of these additional variables only the difference in age is statistically significant. Lastly, “Environmental Stewards” are also more risk tolerant regarding farming practices but less risk tolerant in general.

Both classes are qualitatively similar to each other and to the pooled estimation regarding program payment (positive and significant effect), paperwork (negative and significant effect), program length (no significant effect) and filter strip width (negative and significant effect). The difference between classes and between models is captured primarily in the status quo and filter strip efficacy variables. For the status quo variable, the “Environmental Steward” class is not significant while the “Other” class has a large positive and significant coefficient. A positive coefficient is interpreted as preference for the status quo, even after controlling for program attributes, while a negative coefficient would illustrate preference for enrolling in a filter strip program beyond what can be explained by program attributes. The efficacy variable is positive

[7] It is important to note that latent class models do not assign individual observations into particular classes. Each observation has a probability of being in each class, and while the probability of being in a particular class is often very close to 1, this is not universally the case. The claim that “the Environmental Stewards class comprises 62% of the sample” means that 62% of the total class probabilities fall in the Environmental Stewards class, while 38% fall in the Other Class. Our data is well sorted into two classes. 62.4% of farmers in our sample have a probability of being in one class or the other greater than 98%, while 82.4% have a probability of being in one class or the other greater than 95% and 88.7% have a probability of being in one class or the other greater than 90%.

and statistically significant for the “Environmental Steward” class and is insignificant for the “Other” class.

Reported marginal effects are the mean marginal effect in the sample, and significance is determined using the Krinsky Robb Procedure with 10,000 random draws (Krinsky and Robb 1986; Haab and McConnell 2002). Considering these marginal effects, we can identify further differences between classes. First, the marginal effect of increasing payment for the “Other” class is three times larger than for the “Environmental Steward” class. Increasing annual payment per acre by $10 will increase the probability of program adoption by 0.5% and 1.5% for the “Environmental Steward” and “Other” classes, respectively. The marginal effect of decreasing paperwork is also larger for the “Other” class, with a reduction of one annual paperwork hour increasing the probability of program selection by 1.6% (compared to 0.46% for the “Environmental Steward” class). The largest marginal effect disparities between classes can be found in the status quo and perceptions of runoff efficacy variables. For the “Other” class, the there is no significant marginal effect on increasing perceived efficacy, while the status quo program increases the probability of choosing the program by 31.5 percentage points even after accounting for program attributes. For the “Environmental Steward” class, the status quo variable has no significant effect, while increasing perceived filter strip efficacy by one percentage point increases the probability of program adoption by 4.1 percentage points. The results show that members of the “Environmental Steward” class who believe that filter strips are effective at reducing runoff are very likely to select into one of the PES programs. Members of the “Other” class, on the other hand, have a strong tendency to choose the status quo option, a tendency that can be mitigated by the adjustment of PES program attributes.

Using these results, we also derive static willingness to accept (WTA) measures for each program attribute, presented in Table 5. As with marginal effects, significance for WTA measures is determined using the Krinsky Robb Procedure with 10,000 random draws. These measures are interpreted as the change in per-acre rental payment that fully compensates a one unit increase in program attribute *X* and are calculated using the formula WTA = $\beta_X/\beta_P$, where $\beta_X$ is the coefficient for attribute *X* and $\beta_P$ is the coefficient for payment. We draw two striking conclusions from our analysis of static WTA. First, it is clear that there is substantial between-class variation in WTA measures. Additionally, a comparison of static WTA measures from the pooled and latent class models shows that assuming preference homogeneity when heterogeneity is present can lead to tremendous bias in parameter estimates.

Comparing the “Environmental Steward” and “Other” classes, we find that WTA for increases in annual paperwork hours is similar in both classes, between \$8.40 and \$10.90 per acre. While this difference is non-trivial, it is not striking. Similarly, static WTA for increases in program years are not significantly different from zero for either class. The largest between-class differences are in WTA for increases in filter strip and perceptions of efficacy. For both measures, the “Other” class has WTA that are both small and not significant. The “Environmental Steward” class, on the other hand, has large and significant WTA values (\$11.30 for filter strip width, -\$79.40 for efficacy perceptions). This suggests that members of the “Environmental Steward” class demand substantial compensation for wider filter strips but also require less compensation when they believe the program will deliver greater levels of ecosystem services.

Comparing WTA measures from pooled and latent class estimations in Table 5 allows us to highlight two important shortcomings of the homogeneity assumption. One might expect that

WTA from estimations assuming homogeneity of preferences may roughly approximate the WTA value that results taking the weighted average WTA across classes. This makes intuitive sense, but is not borne out in our data. Even when the pooled WTA estimate falls between estimates from the latent class model, as is the case for filter strip width and efficacy perception, the weighted average of the latent classes produces markedly different estimates than the pooled estimate ($7.04 vs. $2.58 for filter strip width, -$48.80 vs. -$2.56 for efficacy perception). Furthermore, when considering WTA for increased paperwork, the pooled estimate lies outside the range of latent class estimates, ensuring that the pooled estimate will not be a reasonable approximation of the weighted average across latent classes.

In addition to the traditional WTA estimate, we calculate a probabilistic WTA (which we denote PrWTA). PrWTA accounts for the heterogeneous probability that individuals will not select a particular program, or in the case of the current research the probability that farmers will choose to maintain the status quo of no program. Previous studies that have utilized probabilistic WTP/WTA have done so in the context of choice exercises with only two options (Darby et al. 2008; Lancaster and Savage (2004) initially have three options, but drop the 'no medication' option, leaving only two options for analysis). As a result, our calculation of PrWTA deviates slightly from that of previous studies. The PrWTA of an $N$-unit change in attribute $X$ is given by

$$\text{PrWTA} = (1 - \Pr(\text{SQ} \mid N \text{ change}))*N*\text{WTA}. \tag{9}$$

In this formulation, $\Pr(\text{SQ} \mid N \text{ change})$ is the probability a farmer will choose the status quo option when accounting for a marginal $N$-unit change in the program attribute of interest. Subtracting this value from one yields the probability of the farmer selecting either of the proposed PES programs. Equation (9) is calculated for each choice exercise (yielding two PrWTA calculations per farmer) and the mean PrWTA from the data is presented in Table 6.

PrWTA is preferable to WTA by virtue of its ability to account for the probabilistic nature of participation. Conditional on a farmer's participation in the program, WTA captures the tradeoff between payment and some other attribute. But the probability of participation is generally less than one and is certainly heterogeneous by farmer and choice exercise. Accounting for this heterogeneity produces a more accurate willingness to accept measure, one that may better reflect the challenges faced by program administrators. In addition, the probability of enrolling in a program changes as attribute levels change. Our measure of PrWTA uses the probability of opting into one of the PES programs in the event of an identical attribute change to both programs.

All PrWTA presented in Table 6 relate to a one unit change in the program characteristic in question. We generally find that any disparity between classes found using static WTA is maintained or exacerbated in the PrWTA measure. For example, the PrWTA for an increase in filter strip length is $8.71for the "Environmental Steward" class and $0.21 for the "Other" class. New differences are driven primarily by the large status quo preference in the "Other" class. This preference generates much larger probabilities of choosing the status quo in the Other class, which decreases PrWTA. In addition, our finding that homogeneous WTA is a poor estimate for the weighted average latent class WTA still holds for PrWTA.

## Conclusion

This study examines the stated preferences of Ohio corn and soybean farmers in the Maumee watershed regarding government grass filter strip programs. When assuming preference homogeneity, our estimations suggest there is no "status quo preference," meaning farmers do not demonstrate a preference for their current situation beyond what is explained by program characteristics. We also include a variable that captures how effective farmers believe filter strips are at reducing the probability of runoff. In models that do not allow for heterogeneity, we find that increasing this perception has no significant impact on the likelihood of program adoption. Using latent class analysis to allow for preference heterogeneity, we find that farmers fall into two distinct classes. A majority of farmers (62% of our sample) fall primarily into what we call the "Environmental Steward" class, while the rest of the sample falls primarily into the "Other" class. We identify that in the "Environmental Steward" class tend to be younger, more risk tolerant in farming practices, and already engaged in tillage BMPs when compared to their counterparts in the "Other" class.

In our latent class model, we find that the "Other" class of farmers possesses a strong and significant status quo preference, i.e., they appear hesitant to adopt programs featuring filter strips. The "Environmental Steward" class demonstrates no such status quo preference. We find a positive and significant effect of efficacy perceptions on filter strip choice for the "Environmental Steward" class. The effect is not significant for the "Other" class.

Our results have important policy implications. First, this information can help policymakers predict which farmers are likely to enroll in conservation programs. Our research suggests that adoption rates can be increased and costs can be reduced if policymakers target farmer populations that are likely to be in the "Environmental Steward" class when soliciting conservation program enrollment. Soliciting the "Environmental Steward" class has two advantages. First, these farmers lack the strong status-quo preference of farmers in the "Other" class. Additionally, the main tools with which policy makers can encourage enrollment for farmers in the "Other" class are to increase rental payment and decrease nonmonetary transaction costs (paperwork). With the "Environmental Steward" class, on the other hand, policy makers have an additional tool: educational extension programs. Demonstrating the value of PES programs is likely to improve enrollment for these farmers, and the data suggests that there are significant gains to be made in this area. The average farmer in the "Environmental Steward" class believes that a 25 foot filter strip will decrease the probability of runoff by 15.6 percentage points. Agronomic field research has found that such a filter strip can actually reduce sediment runoff by 70-90% and can reduce nutrient runoff by 50-70% (Schmitt et al. 1999; Robinson et al. 1996; Blanco-Canqui et al. 2004). Our study suggests that informing farmers in this class of the real benefits of agricultural BMPs may go a long way toward increasing program adoption. Secondly, the broader goal of this research is to determine which farmers are likely to enroll in conservation programs. This information, when combined with land-use and natural system models, can be used to generate improved estimates of how conservation programs influence nutrient pollution in the Maumee watershed.

This research can be extended by applying this methodology to other BMP programs and watersheds. Given that the main difference between classes boils down to a willingness or hesitancy toward enrolling in government conservation programs, it may also be useful to examine whether attitudinal variables (toward politics, the government, etc.) are effective predictors of class membership.

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## Appendix

### Durbin-Wu-Hausman Test for Endogeneity

To test for endogeneity of the perceived efficacy variable, we first estimate an OLS model with perceived efficacy as the dependent variable. The explanatory variables are program attributes, the status-quo constant, and a set of exogenous field-level variables that should influence the perceived efficacy of filter strips at reducing runoff. These variables include dummy variables for high- and low-slope fields, dummy variables for soil type, distance from the field to the nearest surface water (dummies for 25-75 feet and over 75 feet) and whether the field has working drainage tile. We then calculate the residuals from this regression and use these residuals as an explanatory variable in a subsequent regression with program choice as the dependent variable. The results from this estimation are presented in Table 8. The coefficient for our residual variable is significant, with a p-value below 0.01. A significant coefficient implies there are factors influencing efficacy perceptions (captured in the residual term) that also influence program choice which in the absence of the residual term would reside in the error term. This suggests endogeneity. As it stands, we can reject the hypothesis that our efficacy variable is exogenous.

**Control Function Estimation**

We utilize a control function estimation technique to control for endogeneity in the perceived efficacy variable. This is preferred to two stage least squares when using a linear first stage to generate predictions for a variable that will be used in a nonlinear estimation in the second stage (Train 2009). The first stage of the estimation is identical to the first stage described in the endogeneity test. Variables for soil type, slope, distance to the nearest surface water, and the existence of working drainage tile are used as instruments. When both the first and second stage estimations have normally distributed errors, the control function consists of just the residual terms from the first stage. However, our second stage errors have an extreme value distribution, which requires that the control function be comprised of the residual terms from the first stage and an additional normally distributed random variable. Results from the first and second stage of the control function estimation are in Tables 9 and 3, respectively.

**Table 1: Demographic Comparison**

| Variable | | Analysis Sample (364) | Full Sample | Maumee | Weighted Analysis Sample |
|---|---|---|---|---|---|
| Planted Acres (% in each Category) | 1-9 | 5.9 | 5.7 (456) | 10 | 10.7 |
| | 10-49 | 16.5 | 17.3 (456) | 28 | 30.3 |
| | 50-179 | 34.3 | 35.5 (456) | 31 | 33.0 |
| | 180-499 | 25.2 | 24.1 (456) | 18 | 17.7 |
| | 500 plus | 18.1 | 17.3 (456) | 8 | 8.3 |
| % With Off-Farm Income | | 84.8 | 88.3 (806) | 66 | 66.0 |
| Farm Gross Sales (% in each Category) | Less than 50k | 38.7 | 43.6 (438) | 64 | 64.0 |
| | 50k-100k | 16.5 | 16.0 (438) | 10 | 10.0 |
| | 100k plus | 44.8 | 40.4 (438) | 26 | 26.0 |

Notes: In the "Full Sample" column, the number of observations is given in parentheses. The analyses presented in the paper have a sample size of 364.

**Table 2: Program-, Individual-, and Field-Specific Variable Summary Statistics**

| Variable | Description | Mean | Standard Deviation | Min/Max |
|---|---|---|---|---|
| **Program-Level Attributes** | | | | |
| Payment | $US per acre | 126.45 | 96.12 | 0/250 |
| Width | Filter strip width in feet | 33.38 | 31.07 | 0/75 |
| Paper | Hours of paperwork per year | 3.77 | 3.77 | 0/10 |
| Years | Program length | 5.03 | 4.08 | 0/10 |
| StatusQuo | Alternative specific constant for = current field use | 0.33 | 0.47 | 0/1 |
| Efficacy | Reduction in probability of runoff | 11.37 | 18.29 | -90/90 |
| **Individual-Level Attributes** | | | | |
| StMarys | Awareness of algal issues at Grand Lake St. Marys (0 = not aware, 1 = somewhat aware, 2 = very aware) | 1.44 | 0.62 | 0/2 |
| HighRiskFarm | = 1 if risk tolerant in farming | 0.23 | 0.42 | 0/1 |
| HighSchool | = 1 if high school education or less | 0.43 | 0.50 | 0/1 |
| NormTill | = 1 if use conventional tillage | 0.25 | 0.43 | 0/1 |
| Age40 | = 1 if 40 or younger | 0.27 | 0.44 | 0/1 |
| Enrolled | = 1 if enrolled in other conservation programs | 0.58 | 0.49 | 0/1 |
| Recreate | = 1 if respondent recreates in Ohio rivers, lakes or streams | 0.39 | 0.49 | 0/1 |
| FirstGen | = 1 if first-generation farmer | 0.15 | 0.35 | 0/1 |
| GeneralRisk | Likert-scale variable for general risk tolerance (10 = risk tolerant) | 6.38 | 2.05 | 0/10 |
| Organic | =1 if part of the farm is certified or in the process of being certified organic | 0.02 | 0.15 | 0/1 |
| **Field-Level Attributes** | | | | |
| Drainage | = 1 if field possesses working drainage tile | 0.86 | 0.34 | 0/1 |
| 25-75ft | = 1 if nearest surface water is 25-75 feet from field | 0.20 | 0.40 | 0/1 |
| 75ft | = 1 if nearest surface water is more than 75 feet from field | 0.24 | 0.43 | 0/1 |
| SlopeLess2 | = 1 if slope is less than 2 degrees | 0.50 | 0.50 | 0/1 |
| SlopeMore5 | = 1 if slope is more than 5 degrees | 0.09 | 0.29 | 0/1 |
| ClayLoam | = 1 if soil type is clay loam | 0.48 | 0.50 | 0/1 |
| SiltyLoam | = 1 if soil type is silty loam | 0.15 | 0.35 | 0/1 |
| Loam | = 1 if soil type is loam | 0.05 | 0.21 | 0/1 |
| Sand | = 1 if soil type is sand | 0.02 | 0.14 | 0/1 |
| SandyLoam | = 1 if soil type is sandy loam | 0.08 | 0.27 | 0/1 |
| Width25 | =1 if filter strip is 25 feet wide | 0.34 | 0.47 | 0/1 |
| Width75 | =1 if filter strip is 75 feet wide | 0.33 | 0.47 | 0/1 |

**Table 3: Latent Class Analysis Estimates and Marginal Effects**

| Variable | Coefficients | | | Marginal Effects | | |
|---|---|---|---|---|---|---|
| | Pooled<br><br>[100%] | Environmental Stewards<br>[62%] | Others<br><br>[38%] | Pooled<br><br>[100%] | Environmental Stewards<br>[62%] | Others<br><br>[38%] |
| Payment | 0.0073***<br>(7.84) | 0.0087***<br>(8.39) | 0.0132***<br>(3.79) | 0.0014*** | 0.0005*** | 0.0015*** |
| Width | -0.0188***<br>(-4.32) | -0.0983***<br>(-5.33) | -0.0127<br>(-1.22) | -0.0037*** | -0.0059*** | -0.0014 |
| Paper | -0.0657***<br>(-4.29) | -0.0733**<br>(-2.07) | -0.1438***<br>(-2.65) | -0.0129*** | -0.0046** | -0.0162*** |
| Years | -0.0123<br>(-1.53) | 0.0033<br>(0.45) | -0.0506<br>(-1.11) | -0.0024 | -0.0002 | -0.0057 |
| StatusQuo | 0.3508<br>(0.71) | 0.8408<br>(1.10) | 2.7939**<br>(2.40) | 0.0688 | 0.0500 | 0.3148** |
| Efficacy | 0.0187<br>(0.60) | 0.6908***<br>(4.54) | -0.0149<br>(-0.26) | 0.0037 | 0.0411*** | -0.0017 |
| BIC | 2286.77 | 2188.22 | | 2286.77 | 2188.22 | |

Notes: *, **, and *** indicate significance at the 90%, 95%, and 99% confidence level, respectively. Numbers in brackets are the percent of the sample that falls in each given class. Values in parentheses are z-statistics. Marginal Effects are the mean marginal effect and significance is obtained using the Krinsky-Robb Procedure with 10,000 draws. BIC is the Bayesian Information Criterion statistic, for which a lower value implies a superior model fit. Variable definitions are provided in Table 1. The pooled column assumes one class in the data (preference homogeneity). Both components of the control function are not presented here, as they have no economic interpretation and were not significant in either estimation.

**Table 4: Mean Values of Farmer-Specific Covariates by Class**

| Variable | Class 1 (Environmental Stewards) | Class 2 (Others) | P-value (difference) |
|---|---|---|---|
| HighRiskFarm** | 0.259 | 0.165 | 0.02 |
| HighSchool | 0.391 | 0.482 | 0.34 |
| ConventionalTill*** | 0.181 | 0.360 | < 0.01 |
| Age40** | 0.314 | 0.183 | 0.03 |
| Enrolled | 0.612 | 0.500 | 0.12 |
| Recreate | 0.421 | 0.312 | 0.18 |
| FirstGen | 0.113 | 0.177 | 0.19 |
| Organic | 0.017 | 0.030 | 0.53 |
| HighRiskGen* | 0.335 | 0.364 | 0.07 |
| Efficacy*** | 12.727 | 8.807 | < 0.01 |

Notes: *, **, and *** indicate that differences in mean values between classes are significant at the 90%, 95%, and 99% confidence level, respectively. Variable definitions are provided in Table 2.

**Table 5: Static WTA Per-Acre Compensation Values**

| Variable | Pooled | Environmental Stewards | Others | Weighted Class Average |
|---|---|---|---|---|
| Width | $2.58*** | $11.30*** | $0.96 | $7.04 |
| Paper | $8.00*** | $8.43** | $10.89*** | $9.36 |
| Years | $1.68 | -$0.38 | $3.83 | $1.22 |
| Efficacy | -$2.56 | -$79.40*** | $1.13 | -$48.80 |

Notes: *, **, and *** indicate significance at the 90%, 95%, and 99% confidence level, respectively. Significance is obtained using the Krinsky-Robb Procedure with 10,000 draws.

**Table 6: Probabilistic WTA Per-Acre Compensation Values**

| | Pooled | Environmental Stewards | Others | Weighted Class Average |
|---|---|---|---|---|
| Width | $1.75 | $8.71 | $0.21 | $5.48 |
| Paper | $6.03 | $6.51 | $2.12 | $4.84 |
| Years | $1.15 | -$0.29 | $0.80 | $0.12 |
| Efficacy | -$1.76 | -$64.46 | $0.24 | -$39.87 |

**Table 8: Results from Durbin-Wu-Hausman Test**

| Variable | Coefficient | P-Value |
|---|---|---|
| Payment | 0.0131*** | < 0.01 |
| Width | -0.0248*** | < 0.01 |
| Paper | -0.1242*** | < 0.01 |
| Years | -0.0140 | 0.582 |
| StatusQuo | 0.3589 | 0.400 |
| Efficacy | -0.0038 | 0.777 |
| Residual | 0.0152 | 0.277 |

Notes: *, **, and *** indicate coefficients are significant at the 90%, 95%, and 99% confidence level, respectively. Variable definitions are provided in Table 2, and Residual variable is the residual from a "first stage" regression with Efficacy as the dependent variable and the inclusion of field-level instrumentals (soil type, dummy variables for high and low slope, distance to the nearest surface water, and whether the field has drainage tile).

**Table 9: First Stage Results, Control Function Estimation**

| Variable | Coefficient | t-statistic |
|---|---|---|
| Drainage | -3.8526*** | -3.77 |
| 25-75ft | -2.7262*** | -3.12 |
| 75ft | -4.2338*** | -5.24 |
| SlopeLess2 | -5.0229*** | -6.99 |
| SlopeMore5 | 0.2442 | 0.24 |
| Width | 0.1181*** | 6.60 |
| Payment | -0.0029 | -0.30 |
| ClayLoam | -1.6732* | -1.84 |
| SiltyLoam | 0.2914 | 0.25 |
| Loam | -8.2307*** | -4.68 |
| Sand | -4.4278* | -1.94 |
| SandyLoam | 0.3226 | 0.24 |
| Paper | -0.1912 | -1.44 |
| Years | -0.0161 | -0.10 |
| Current | -13.3091*** | -5.34 |

Notes: Estimation is OLS and the dependent variable is Efficacy. *, **, and *** indicate that coefficients are significant at the 90%, 95%, and 99% confidence level, respectively. Variable definitions are provided in Table 1. The excluded soil type is clay.