ECONOMICS OF THE GREENSEEDER HAND

PLANTER, DISCRETE CHOICE MODELING, AND

ON-FARM FIELD EXPERIMENTATION

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Title of Study: ECONOMICS OF THE GREENSEEDER HAND PLANTER, DISCRETE CHOICE MODELING, AND ON-FARM FIELD EXPERIMENTATION

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Abstract: Corn yields in developing countries are lower than in developed countries partly due to planting methods that involve hand dropping of multiple seeds per hill. Researchers from Oklahoma State University (OSU) recently developed the Greenseeder Hand Planter (GHP) to replace such methods. The first essay determines economic breakeven levels of seed and labor savings, increases in corn yield, and reduced loss of N through reduced ammonia volatilization. Results suggest a $50 GHP used to plant 3 hectares per year would need to increase corn yields on average by about 1.12%, use 12.19% fewer seeds, or reduce labor man-days by 38.66% to equal expected net returns from traditional methods.

In the second essay, I conduct Monte Carlo experiments to measure bias in the conditional logit (CL) and independent availability logit (IAL) when there is no choice set formation and when choice sets are stochastically formed. I also compare the performance of the two models using empirical data on paddlefish angler preferences collected in Oklahoma. Both the CL and IAL work well when their own assumptions hold, but not under the alternative's assumptions. However, the IAL produces unbiased and less efficient parameter estimates when individuals actually choose from the full set of alternatives. Empirical results suggest the IAL is able to predict the attribute-cutoff.

To avoid limitations from small-scale agronomic trials, there has been a movement toward large-scale, on-farm field trials but questions remain as how best to conduct them and when it is most profitable to quit them. The third essay addresses these questions by using a fully Bayesian decision-theoretic approach. Data are from Monte Carlo simulations assuming a corn-input stochastic plateau production function. Results suggest the best way to conduct such experiments is to allocate to each of the 10% of the plots, 0 lb. of N, half of N*, and 150% of N* under a 30-plot experimental design. Results further indicate that it optimal to quit such trials in year 2. Sensitivity analysis confirms the optimal quit period but suggests such experiments are most profitable by allocating unalike N levels to all of the 30% of experimental plots.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER I

ECONOMICS OF THE GREENSEEDER HAND PLANTER

Abstract

Corn (*Zea mays* L.) yields in developing countries are lower than in developed countries in part due to planting methods that involve hand dropping of multiple seeds per hill. The Greenseeder Hand Planter (GHP) was developed to reduce seeding rates and long-term health risks from using bare hands to drop pesticide-treated seeds. When used to apply fertilizer, it can prevent loss of nitrogen (N) from ammonia volatilization. This research determines economic breakeven levels of seed and labor savings, increases in corn yield, and reduced loss of N through reduced ammonia volatilization. A GHP used to plant 3 hectares per year that costs $50 would need to increase corn yields on average by about 1.12%, use 12.19% fewer seeds, or reduce labor man-days by 38.66% to equal expected net returns from traditional methods. Using the GHP to apply fertilizer would on average increase corn yields up to 10.82% per hectare due to reduced N loss from ammonia volatilization and thus fertilization alone could be enough to pay for the planter.

Key Words: Corn, Greenseeder Hand Planter, planting by hand, linear mixed effects, partial budgeting.

JEL Codes: Q13, Q1

Introduction

Corn *(Zea mays* L.) is one of the most cultivated crops in the world. Corn originated from Mesoamerica and its production has spread throughout the world. Corn can be grown over a wide range of altitudes and latitudes (Shiferaw et al*.,* 2011). Plant breeders have developed varieties that grow well under different biophysical environments. Thus, global corn production has increased over the years. Between 1961 and 2010, area allocated to corn production increased by more than 50% with about 73% of this growth in developing countries (Shiferaw et al., 2011). In 2010, corn was planted on about 73%, 44%, and 46% of the cultivated land in Africa, Latin America, and South Asia, respectively (Shiferaw et al., 2011) and on 35 million U.S.A. hectares (USDA, 2016*).*

While demand for corn in developing countries remains high (Borlaug, 2007; Shiferaw et al., 2011), its yields in developing countries are lower than in developed countries (Cairns et al., 2013; Chim et al., 2014). For example, since 1961 corn yields in the top five corn producing countries in the world (U.S.A., China, Brazil, Mexico, and Indonesia) have increased three-fold (from 1.84 Mg ha^{-1} to more than 6.10 Mg ha^{-1}) while in developing regions of Africa, Asia, and Latin America, corn yields have stagnated at less than 2 Mg ha^{-1} (FAO, 2011; Cairns et al., 2013). These yield differences are attributed to a number of factors including access to and use of localized seed genetics, fertilizer, pest management, inefficient irrigation systems, and differences in seeding practices (Adjei et al., 2003; FAO, 2007; Aikins et al., 2010; Masasi and Ng'ombe 2019). In developed countries, mechanized planters that deliver and cover single seeds per drop at relatively precise depths and precise within row spacing, enhances yield potential (Omara et al., 2016; Mukembo et al., 2016). But, about 60% of corn area (29 million hectares) in developing countries is

planted with multiple seeds per hill by hand (Mukembo et al., 2016; Chim et al., 2014; Fisher, 2016; Dhillon et al., 2017).

Planting by hand usually involves using a heavy Stick Seeder Planter (SSP) and/or hand-hoe. Workers use the SSP to open a shallow hole about 5 cm deep; drop two to three seeds in the hole; cover the seeds with soil forming a small hill; and step on the hill enhancing soil to seed contact (Adjei et al., 2003). The typical SSP is composed of a wooden shaft and a pointed metal tip that can be used to penetrate the soil and open a slot for seed placement (FAO, 2010). Aikins et al. (2010) explain that the whole process is labor intensive and results in non-uniform plant stands often with multiple plants emerging from each hill and competing for nutrients. For equivalent seeding rates, non-uniform spacing of seeds, has been found to result in lower yields than uniform spacing (Epplin et al., 1996; Rutto et al., 2014). Whereas several hand planters have in the past been developed for corn farmers in developing countries, few of them drop one corn seed with a single strike (singulation) (Aikins, Plange, and Baffour 2010; Dhillon et al., 2017, 2018). Researchers at Oklahoma State University (OSU) developed a singulating corn Greenseeder Hand Planter (GHP) hypothesized to reduce optimal seeding rates (Omara et al., 2016). Theoretically, use of a GHP relative to a SSP could result in equivalent or greater yields from fewer seeds purchased and planted per hectare.

The GHP includes a seed box that eliminates the need for the operator to handle each seed (see Figure 1). Prior to planting, corn seeds are commonly coated with one or more pesticides such as imidacloprid (trade name Gaucho), permethrin (trade name Kernel Guard Supreme or Profound), thiamethoxam (trade name Cruiser), as well as with biological agents (Paulsrud et al., 2001). Careless handling of coated seeds may result in deleterious health

consequences. Thus, the GHP can reduce long term health risks because it reduces operator exposure to treated seeds (Fisher, 2016; Dhillon et al., 2017). By changing the GHP's internal drum, the GHP can serve as a mid-season fertilizer applicator where the operator places fertilizer underneath the soil surface (Dhillon et al., 2017; Dhillon et al., 2018). The GHP is hypothesized to prevent loss of N from ammonia volatilization from urea fertilizers because it allows the operator to place fertilizer beneath the soil surface which reduces urea's exposure to direct heat from the atmosphere (Dhillon et al., 2017). Developing countries experience higher loss of nitrogen (N) via ammonia volatilization than industrialized countries due to high temperatures and widespread use of urea and ammonium bicarbonate (Bouwman and Boumans, 2002).

The GHP is designed to release a single seed per location, which is intended to improve homogeneity of plant growth, decrease inter-plant nutrient competition, improve yield potential, and reduce seed cost per hectare (Chim et al., 2014; Fisher, 2016). To keep manufacturing costs low, the GHP does not yet meet this target. However, the most recent design comes closer than the version used in the experiments reported here.

This study seeks to determine the labor savings, seed savings, and the quantity of corn yield increase required for the GHP to be an economically viable alternative to the SSP. This study also determines the quantity of corn yield increase that would be realized due to reduced loss of N from ammonia volatilization if the GHP was used to apply urea fertilizer. In addition to these main objectives, we also determine the effect of using the GHP on corn yield per hectare relative to an ideal standard of near perfect seed singulation. Evaluation of the GHP technology could show whether the GHP would pay or not, which would be an important finding for farmers producing a vitally important food crop. These objectives are

achieved by employing partial budgeting techniques and estimating a linear mixed effects

model to data from designed field trials in Stillwater, Oklahoma, U.S.A.

Methodology

Theory

Farmers are expected to choose the planting method that maximizes expected net

returns and improves their welfare. Biermacher et al. (2009) suggest that the expected profit

maximizing framework is suitable to model behavioral decision and choice of farmers before

the onset of the planting season. Assume that one of the farmer's objectives is to adopt a

planting method that maximizes expected profit π by comparing profit that is yielded by m

alternative methods. The farmer chooses a planting method j over any alternative package m

such that

(1) $$\pi_j > \pi_m, m \neq j.$$

The adoption decision D^* and the optimal expected profit π_j^* from choosing a given

planting method would be:

(2) $$D^* = \begin{cases} 1 \text{ iff } E(\max E(\pi_j^*)) > E(\max_{m \neq j} E(\pi_m^*)) & \text{or } \eta_{i1} > 0, \text{ for } \forall m \neq j \\ 0 \text{ otherwise} \end{cases}$$

where $\eta_j = E(\max_{m \neq j}(\pi_j^* - \pi_m^*)) > 0$ (Bourguignon et al., 2007; Biermacher et al., 2009). By

eqn. (2), a farmer whose objective is to maximize expected profit is expected to adopt a

planting method whose expected profit is greater than all alternatives. The GHP considered

here is attempting to drop a single seed per planting station as opposed to an SSP in which

two or more seeds are dropped per hill. Thus, if the same number of seeds are planted per

hectare, the theoretical expected yield would be greater for the GHP given the expected

agronomic benefits of uniform plant spacing. Alternatively, if fewer seeds are planted per

hectare with the GHP, total seed costs would be lower. Ignoring the potential value of farmer

health benefits from using a GHP relative to a SSP, the farmer's optimization problem is

mathematically

(3) $\quad\quad\quad \max_D E(\pi_i|\boldsymbol{x}) = ((D)(pE(y_{GHP}|\boldsymbol{x}) - c_{GHP}) + ((1-D)(pE(y_{SSP}|\boldsymbol{x}) - c_{SSP}))$

subject to

$$y_k = f(\boldsymbol{x}), \, k = \{GHP, SSP\}, \quad D \in \{1,0\},$$

where p is the price of corn, $E(\pi_i)$ is expected profit (\$) per hectare, D is the discrete choice

variable that equals 1 if the farmer uses GHP, 0 otherwise, y_{GHP} is corn yield from plots

where the GHP was used, y_{SSP} is corn yield from plots where the SSP was used, c_{GHP} is cost

of production from plots where the GHP was used, c_{SSP} is cost of production from plots

where the SSP was used, y_k is corn production function and $\boldsymbol{x}$ denotes a vector of inputs used

in corn production.

Data and Procedures

Agronomic data

Plot-level agronomic data were generated from experiments conducted at the Efaw,

Lake Carl Blackwell, and Stillwater Agronomy Research Stations in Payne County,

Oklahoma, USA. Efaw has an Ashport silty clay loam soil. The Lake Carl Blackwell plots

have Pulaski fine-sandy loam soils. Stillwater Agronomy Research Station has mostly

Kirkland silt loam soils (Omara et al., 2016). These experiments were designed as

randomized complete blocks. Each experiment comprised three replications and four plots

per replication in each site year. The experiments were conducted at the Stillwater site in 2014, at Efaw in 2014 through 2016 and at Lake Carl Blackwell in 2015 and 2016.

Treatments consisted of planting methods: GHP, SSP, and a tractor-drawn John Deere Planter (JDP). The GHP has an internal drum that can hold up to 1 kg of seed. It was designed to deliver a single seed per hill at a planting depth of about 5cm (Omara et al., 2016). The SSP has a metal tip like those typically used in Central and South America. Its only function is to open a planting hole into which seeds are dropped and covered by foot (Chim et al., 2014). The SSP used in these field experiments managed 100% singulation (planter delivers a single seed with every strike) which implies that the SSP in this experiment did not simulate its actual applications in developing countries. Despite this severe limitation, the experimental dataset is used because it still provides helpful information.

Hybrid corn variety Pioneer P1498HR was planted on all plots with plant population of 74,000 seeds per hectare. Inter-row spacing at all the stations was 76 cm while plant spacing was uniform at 18 cm. Plot size varied ranging from 1.5 m by 6 m to 3 m by 6 m. Summary statistics of corn yield from each research station are shown in table 1.

In addition, summary statistics from the research stations according to planter type are shown in table 2. Other details of the field trials are in Dhillon et al. (2017).

Economic analysis

Partial budgeting was used to determine the economics of the GHP. Adopting a GHP would result in incremental changes at the farm and a partial budget is a useful tool for a farmer when such a situation arises (Nuthall, 2011). Partial budgeting computes the overall

impact by netting out the negative effects from positive effects. Positive effects include the monetary value of activities that would increase revenue and/or decrease costs while negative effects are those that would decrease revenue and/or increase costs. In our partial budget, the added returns were the additional revenue that would result from using the GHP and reduced costs included seed and labor costs for SSP. The added costs included the GHP's annual operating costs whose computation relied on depreciation, interest on average value, repairs, taxes, and insurance (also called the DIRTI-5 by Lessley and Holik, 1987). Reduced revenues were zero.

The following assumptions were used in our partial budget. The market price of the GHP is assumed to range between $40 and $100 per unit. The $100 is about what it costs now and the $40 is what we hope it will cost under mass production. Omara et al. (2016) posit that if the market price of the GHP was $40 per unit, it would be more marketable among smallholder farmers in the developing world.

We assume a useful life of 3 years and that the GHP would be used to plant corn seed on up to 5 hectares per year. Following Haggblade and Tembo (2003), Ng'ombe et al. (2017), and Ng'ombe (2017), peasant farmers in Zambia and across sub-Saharan Africa (SSA) plant up to 5 hectares of land annually – the typical holding size of land for farming by most farmers. An annual market interest rate of 6% was assumed while the repairs, taxes, and insurance for the GHP are assumed to be zero. Price of corn is assumed to be $175 per Mg while labor cost was set at $2.5 per man-day. A farmer is assumed to plant a hectare of corn in 5 days while 25 kg of corn seed is assumed to be planted on one hectare of land. These assumptions and variable values were pulled from the standard nationally representative smallholder corn enterprise budget from Zambia. The corn enterprise budget was prepared by

the Zambia National Farmers' Union (ZNFU) based on production practices by representative Zambian smallholder corn farmers in 2015 (ZNFU, 2015). Zambia is a developing country in SSA where planting by hand is common (Haggblade and Tembo, 2003). In addition, Zambia is one of the countries where the GHP has been distributed (see Figure 2).

Bouwman and Boumans (2002) find that N loss from ammonia volatilization of urea fertilizers on average amounts to 18% and 7% in developing countries and industrialized countries, respectively. Funderburg (2009) reports a 20% N loss from ammonia volatilization to be common when urea fertilizers are applied on the soil surface. Jama et al. (2017) determine corn yield response to nitrogen use from 940 on-farm trials and demonstration sites consisting of at least 3,220 site-year treatment combinations in southern Africa. Jama et al. (2017) classified the applied N rates as "half N", and "full N" based on recommended rates for each site. The "half N" and "full N" rates imply applying fertilizer containing N less than or equal to 50% and more than 50% of the recommended N rates, respectively (see Jama et al., 2017 for more details). Though Jama et al. (2017) did not estimate the traditional linear response stochastic plateau (Tembo et al., 2008; Boyer et al., 2013), their results corroborate the idea of the linear response stochastic plateau. They find that without N, farmers would on average realize 1.6 Mg of corn per hectare while the expected corn plateau is 4 Mg per hectare and that marginal physical productivity for corn is 0.025 Mg per kg.

Considering that agricultural producers are financially constrained and so the quantity of urea is limited, studies by Bouwman and Boumans (2002) and Jama et al. (2017) allow us to estimate the corn yield increase that would be realized due to reduced loss of N from ammonia volatilization if the farmer used the GHP to apply fertilizer. In our budget, the GHP

potentially increases the amount of N by up to 18% (because it places fertilizer underneath the soil) and the amount of urea that a producer has is assumed to be constrained. Following Bouwman and Boumans (2002) and Jama et al. (2017), the percentage that the GHP would increase corn yield due to reduced N loss from ammonia volatilization $y*$ is:

(4)
$$y^* = [4 - (1.60 + 0.0259 * Nrem)]/4$$

where *Nrem* is amount of N available after 18% loss from ammonia volatilization: *Nrem* = (*Nplat* – *Nplat**0.18), *Nplat* is the amount of N required to produce corn at its plateau. Based on averages of Table 2 in Jama et al. (2017), *Nrem* is 75.95kg of N per hectare while *Nplat* is 92.66 kg of N per hectare.

Statistical analysis

The effect of using GHP on corn yield was estimated using the plot-level agronomic data. Dhillon et al. (2017) conducted the experiment for other experimental objectives. For this study, it would have been preferred to compare the GHP with actual farmer practices rather than an ideal situation under which the SSP was used. The data are used here because they are the only experimental data available and they do show how well the GHP compares versus ideal planting methods. The data are cross-sectional time series and therefore could be prone to problems of non-spherical errors across seasons. Thus, the R-package lme4 (Bates et al., 2015) is used to estimate the linear mixed effects model. The R-package lme4 uses restricted maximum likelihood estimation (REML). For estimation of linear mixed effects models, REML is preferred to maximum likelihood estimation (MLE) because it yields unbiased covariance parameters by accounting for the loss of degrees of freedom that results from parameter estimation of fixed effects (West et al., 2007). To determine the statistical

significance of treatment main effects, we used the R-package lsmeans developed by Lenth (2015). Our linear mixed effects model's data generating process is

(5) $$y_{itk} = \mu + \tau_i + s_t + \varepsilon_{itk}$$

where y_{itk} is corn yield with the the ith planting method, from year t, and site k, μ is the overall mean, τ_i is the effect of the ith planting method, $s_t \sim N(0, \sigma_s^2)$ is the site-year random effect, $\varepsilon_{itk} \sim N(0, \sigma_\varepsilon^2)$ is a random error, and σ_s^2 and σ_ε^2 are mutually independent.

Results

Statistical analysis

Several diagnostics were conducted to determine the plausibility of the linear mixed effects model selected. Based on the Shapiro-Wilk test, the null hypothesis of normality of the distribution of corn yield was not rejected at a 10% significance level (P = 0.200). Based on results from the Levene test, the null hypothesis of equal error variances across the treatments was not rejected (P = 0.567). The likelihood ratio test was used to determine significance of the fixed effects (based on the ANOVA function in R software) in the model. The null hypothesis of no fixed effects was rejected (P < 0.001). Parametric bootstrap of the p-value based on 1000 replications was used to determine statistical significance of site-year random effects. There was strong evidence to support the inclusion of site-year random effects in the model (P < 0.001). The estimated linear mixed effects regression model is shown in table 3. The linear mixed effects model with seed size fixed effects was also estimated, but the results differed little from those reported above and therefore they are omitted.

11

Furthermore, actual mean differences among treatments were determined by conducting a post-hoc analysis and results are reported in table 4. The SSP is the base treatment. Results in table 4 indicate the GHP had significantly lower corn yield than the SSP. We find no statistically significant differences between mean corn yields from using the SSP and JDP. These findings corroborate with descriptive statistics in table 1 for years 2014 and 2015, although in 2016 the GHP resulted in higher average corn yield than the JDP. Dhillon et al. (2018) document efforts to refine use of the hand planter and the improved performance in 2016 may partly result from learning in prior years.

The idealized SSP would result in about 0.742 Mg more corn yield per hectare than the GHP. The estimated JDP advantage over GHP of 0.611 Mg per hectare is not statistically different from zero.

Economic Analysis

Using the partial budgeting approach, breakeven values for corn yield, labor costs, the price of corn seed, and the purchased price of the GHP are discussed next. Results suggest that for a GHP priced at $50 to be an economically viable alternative to the SSP, it should be able to increase corn yields by about 1.12% (equivalent to 28 kg) per hectare. If the GHP can achieve the 20% yield increase projected by Omara et al. (2016) then it would unambiguously pay to adopt the hand planter. In terms of seed savings, results indicate that such a GHP would be an economically viable planting method if it reduced seeds by about 12.19% per hectare. This finding also implies that for the GHP that costs $50 to result in equivalent net returns as the SSP, it should enable the smallholder farmer to save corn seeds valued at about $5.0 per hectare (assuming seeds are valued as $1.5/kg).

In terms of labor savings, results suggest that for the GHP valued at $50 to generate equal net returns as the SSP, it is required to reduce labor man-days for planting by 38.66%. Stated differently, this implies that for the GHP to enable a farmer to break-even, it should reduce the amount of labor required for planting by at least about 39%. Since planting is done in one motion with the GHP, it does have some potential for labor saving. Our experience, however, is that there is little or no labor saving and certainly nothing close to 38.66%, so labor saving does not appear to be a sufficient motivation for adopting the GHP. In addition, the main part of the GHP is metal and contains the seeds so that it weighs more than the SSP, which makes it being a labor saving technology even more unlikely.

Since the value of the GHP depends on its production and transactions costs, its market price would perhaps be different from the one assumed above, which would ultimately alter our partial budgeting results. Considering such potential disparity and holding other factors fixed, breakeven values of corn yield, labor, and seed at varying market prices of the GHP are presented in table 5. As mentioned before, it is assumed that the GHP's market price would range between $40 and $100 per unit. If the price of the GHP was $95 per unit, for it to produce the same net returns as the SSP, the GHP would need to increase corn yields by about 2.08% per hectare or result in seed savings of 23.16% per hectare, *ceteris paribus*.

Similarly, it would have to reduce labor man-days required for planting corn by about 74%. Whereas if the market price of the GHP was $40 per unit, the breakeven values for seeds, labor and corn yields would be 9.75% less seeds per hectare, about 30.94% less man-days per hectare, and 0.88% more corn yield per hectare.

The smallholder farmer is assumed to be cash constrained and thus only able to purchase and apply a fixed amount of fertilizer per hectare. In terms of added corn yields due to 18% reduced N loss from ammonia volatilization, our findings show that a farmer would realize about 10.82% of additional corn per hectare (about 0.432 Mg per hectare) if the GHP was used to apply fertilizer. Thus using the GHP to apply fertilizer would provide about $75.74 per hectare, assuming fertilizer is limited. Thus, using it to apply fertilizer on only one hectare is sufficient to pay for the full cost of the GHP priced anywhere between $40 and $70 per unit.

Clearly from the linear mixed effects regression model, the GHP resulted in lower corn yields per hectare than the SSP, a plausible reason for its lower corn yields could be due to the way it was designed. The GHP like the SSP is not designed to ensure or enhance seed to soil contact (see video https://www.youtube.com/watch?v=VisKBsqcCWA). The SSP's operator used his/her foot to enhance soil to seed contact whereas this was not done with the GHP. Another limitation of the experiment is that unlike conventional practice in developing countries, only one seed was dropped per hill with the SSP. A third limitation is that within row spacing was uniform for all treatments. Thus, the seeding rate was held constant and findings from the experiment cannot be used to address the potential for seed savings with the GHP relative to the SSP.

Following Martin et al. (2005) and Rutto et al. (2014), lack of attention to seed to soil contact when the GHP plots were seeded or failure by the GHP to drop the seed may have contributed to lower emergence rates for GHP relative to SSP and JDP and the resultant lower crop yield on the GHP plots. As shown in table 6, the GHP had the lowest corn emergence rates among the three treatments in all the years and possibly it did not always

place a seed. As Dhillon et al. (2018) note, these findings have already been used to modify both the design and use of the hand planter.

Conclusion

The GHP has the potential to improve yields and reduce costs for planting corn in developing countries. In terms of seed savings, a GHP valued at $50 would be a breakeven investment if it increased corn yields by 1.12% per hectare or saved about 12.19% of seeds per hectare. If labor reduction was its only benefit, a reduction of labor man-days by 38.66% would be required for it to be economically as viable as the SSP. Since the GHP's market price would vary, if the GHP sold at $95, breakeven would require a 2.08% increase in yield, a 23.16% seed savings, or a 74% reduction in labor. In terms of added corn yields due to reduced loss of N from ammonia volatilization, the GHP seems to be a profitable venture as it would result in about 10.82% increase of corn per hectare which is a staggering $75.74 additional corn returns per hectare. With about $74 more added revenue, the farmer would be able to pay for the GHP from using on a single hectare. This result suggests that the economics of the GHP are more favorable for using it to apply fertilizer than for planting corn.

The GHP was compared to two ideal planting techniques. The GHP had lower corn yields than an SSP with perfect seed singulation. The GHP had lower corn emergence which may be due to the GHP failing to drop a seed or incomplete seed and soil contact. The SSP was used in an ideal situation (up to 100% seed singulation), which is different from how farmers use it. Further research is needed to evaluate the GHP versus actual farmer practice. Given the potential for the GHP to reduce seed costs, increase corn yield due to reduced loss

of N from ammonia volatilization, and reduce potential health risk relative to the SSP, it is recommended that additional field trials be conducted with the following changes. First, either the GHP should be modified to enhance seed to soil contact when seeding, or the GHP operator should cover and step on the soil above each placed seed. Second, within row distance between seed drops should be doubled in the SSP plots relative to within row distance between seed drops in the GHP plots to more nearly simulate farmer practice. Third, two seeds should be dropped at each location in the SSP plots relative to one seed in GHP plots.

One limitation of our study is that consequences of physical contact between treated seed and SSP and GHP laborers were not determined. The GHP may reduce the negative consequences to operator health resulting from handling treated seed. Additional research would be required to quantify this potential benefit from using a GHP rather than SSP. Furthermore, due to uncertainty, producers may require a higher rate of return in order to adopt. Micro-dosing of fertilizer looks promising, but as Jama et al. (2017) argue, research is needed to demonstrate results in actual farmers' fields rather than only on field trials.

Table 1.1: Descriptive Statistics of Corn Yield (Mg ha^{-1}) According to Planter Type Obtained in 2014, 2015, and 2016

Planter Type	2014		2015		2016	
	Mean	SD	Mean	SD	Mean	SD
SSP	6.683	0.511	4.506	2.140	5.462	2.320
GHP	5.583	0.909	3.421	2.313	5.326	2.291
JDP	6.433	1.357	4.488	1.845	5.221	2.268

Table 1.2: Descriptive Statistics of Corn Yield (Mg ha^{-1}) by Planter Type from Efaw, Lake Carl Blackwell and Stillwater Agronomy Research Stations

Planter Type	Efaw		Lake Carl Blackwell		Stillwater	
	Mean	SD	Mean	SD	Mean	SD
SSP	6.313	2.268	4.067	1.511	4.050	0.750
GHP	5.582	2.200	3.106	2.173	3.577	0.985
JDP	5.940	1.677	3.706	2.029	5.233	0.451

Table 1.3: Linear Mixed Effects Regression Results of Corn Yield (Mg ha^{-1}) Response to Planter Type

Variable name	Coefficient	Std. Error
Intercept	5.262**	0.776
GHP	-0.742**	0.346
JDP	-0.130	0.456
Site-year random effect	2.536**	1.262
Error variance	3.388**	1.357
Log likelihood ratio	-408.344	
Number of observations		193

**Statistically significant at 1%,

Table 1.4: Least Squares (LS) Means (Mg ha^{-1}) of Corn Yield by Planter Type

Planter *I* vs Planter *j*	Difference in Least Squares Means (Mg ha^{-1})
SSP vs JDP	0.130
SSP vs GHP	0.742**
JDP vs GHP	0.611

**Statistically significant at 1%

Table 1.5: Breakeven Corn Yield (Mg/ha), Corn Seed (kg/ha) and Labor Savings (man-days)

Price of GHP ($/unit)	Breakeven Corn Yield (Mg/ha)	Breakeven Amount of Seed (kg/ha)	Breakeven Amount of Labor (man-days/ha)
40	0.022	2.578	1.547
45	0.025	2.900	1.740
50	0.028	3.222	1.933
55	0.030	3.544	2.127
60	0.033	3.867	2.320
65	0.036	4.189	2.513
70	0.039	4.511	2.706
75	0.041	4.833	2.900
80	0.044	5.156	3.093
85	0.047	5.478	3.287
90	0.050	5.800	3.480
95	0.052	6.122	3.673
100	0.055	6.444	3.867

Table 1.6. Average Emergence Rates (%) According to Planter Type Obtained in 2014, 2015, and 2016

Planter Type	2014		2015		2016	
	Mean	SD	Mean	SD	Mean	SD
SSP	89.333	17.282	96.565	5.010	91.167	5.734
GHP	82.056	21.421	80.372	14.372	86.020	9.411
JDP	87.333	14.224	92.288	10.461	95.583	4.641

Figure 1.1: The Greenseeder Hand Planter

Source: Oklahoma State University's Nitrogen Use Efficiency website (www.nue.okstate.edu)

Note: Countries marked in red had some farmers that received a GHP in previous by 2016.

Figure 1.2: Distribution of the GHP across the World by 2016.

Source: Oklahoma State University's Nitrogen Use Efficiency website (www.nue.okstate.edu)

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CHAPTER II

THE EFFECT OF INCLUDING IRRELEVANT ALTERNATIVES IN DISCRETE CHOICE

MODELS OF RECREATION DEMAND

Abstract

I extend research into choice set formation in random utility maximization (RUM) models by

conducting Monte Carlo experiments to compare the performance of two plausible choice set

specifications. I measure bias in the conditional logit (CL) and independent availability logit

(IAL) models when there is no choice set formation and individuals choose from the full set of

alternatives, and when choice sets are stochastically formed and individuals choose from a subset

of all alternatives. I also compare the performance of the two models using empirical data on

paddlefish angler preferences and catch-and-release regulations in Oklahoma. Both the CL and

IAL work well when their own assumptions hold, but not under the alternative's assumptions.

The CL produces biased parameters when the researcher ignores choice set formation and

includes irrelevant alternatives in the choice set. However, the IAL produces unbiased and

considerably less efficient parameter estimates when individuals actually choose from the full set

of alternatives while empirical results show that the IAL predicts a distance cutoff that is closer

to the true one. Since neither the IAL nor CL is universally preferred, researchers must choose

between them based on knowledge about the probability of irrelevant alternatives considered.

Introduction

Choice set misspecification is an important problem in discrete choice models. Choice set misspecification occurs when irrelevant alternatives are mistakenly placed into the choice set, rather than being discarded by the researcher. Decision makers may naturally exclude certain alternatives because they are unaware of them or because one of the attributes generates substantial disutility. In practice, it is well known that parameter and welfare estimates are sensitive to choice set definitions (Peters and Adamowicz 1995; Swait and Ben-Akiva 1987). Parameter sensitivity is especially of concern in applications with a huge number of feasible alternatives, which includes models of location choice in applications to real estate and recreation demand, because the researcher may be compelled to make a subjective judgement about the composition of the true choice set (McFadden 1978; Guevara and Ben-Akiva 2013; von Haefen and Domanski 2018).

The literature on choice set misspecification offers several approaches for identifying and dealing with irrelevant alternatives. One practice common in recreation demand applications is to exclude alternatives that exceed a certain distance or price (determined by the researcher) from each decision-makers' home. Prior work shows including distant sites affects parameter estimates, although this influence may diminish past some threshold (Parsons and Hauber 1998; Whitehead and Haab 1999). Several papers examine parameter sensitivity to the inclusion of various substitute site/activity combinations in discrete choice models of recreation demand (Haab and Hicks 1997; Jones and Lupi 1999; Parsons, Plantinga and Boyle 2000; Pramono and

Oppewal 2012). These papers treat choice set definitions as exogenous—i.e. the decision maker's choice set is assumed to be known deterministically. However, there is also a growing number of papers that treat choice set definition endogenously by modeling choice set formation in one stage and the selection of an alternative in a second stage (Manski 1977; Swait and Ben-Akiva 1987; Haab and Hicks 1997; Li, Adamowicz and Swait 2015; Thiene, Swait and Scarpa 2017). These papers conclude that ignoring choice set formation leads to biased parameters and welfare estimates.

This article extends research into choice set formation in discrete choice models by measuring the bias from two plausible forms of choice set specification. My motivation comes from the practice of excluding recreational sites that the researcher thinks are too far for a decision-maker to reasonably access (for example, day-trip sites more than 200 one-way miles from an individual's home) or due to the difficulty in assessing travel costs for long distances. Specifically, unlike previous research, I examine two types of choice set formation: (1) every choice set includes the full set of alternatives and (2) each choice set is formed from a stochastic selection process that is partly a function of a choice attribute level, thereby rendering choice set formation endogenous. The two cases are used to assess the properties of two estimators of discrete choice models, one of which accounts for stochastic choice set formation. Thus, I measure the bias from including irrelevant alternatives in discrete choice models when choice set formation follows a stochastic process and when the choice set formation process is not followed.

The primary objective is to measure the bias when an observable attribute (e.g. distance to recreation site) affects the probability that an alternative appears in the choice set. In doing so, I estimate two models: 1) the independent availability logit (IAL) model that incorporates choice

set formation as a stochastic process and probabilistically imputes all possible choice sets and 2) the conditional logit (CL) model which assumes the choice set includes all alternatives. The approach used is illustrated in figure 2.1.

While actual data is often used to measure potential bias in recreation demand models, I conduct Monte Carlo experiments that generate artificial data based on a known data generation process as well as empirical analyses using real data. The experiment simulates a random utility maximization (RUM) problem in which individuals choose from many recreational sites with varying attributes. Monte Carlo analysis allows us to measure the true bias when irrelevant alternatives are included in the choice set while considering stochastic choice set formation, and test how well recreational choice set definition heuristics can do in minimizing this bias. Monte Carlo-based methods are desirable in this study because (i) the two data generating mechanisms to which I compare the parameter estimates would be known, and (ii) a greater number of replications allows one to evaluate the robustness of the results. Monte Carlo experiments build on previous research by comparing the performance of the IAL to recreational choice set definition heuristics. My empirical analyses mainly compare parameter estimates of the two models using real data without any known parameter estimates.

Competing Models

In this section, a description of the models estimated using both simulated and actual data. In the traditional RUM setting, choices among discrete alternatives are studied to model choice problems such as those in recreation, transportation, marketing, and agriculture. A researcher chooses the alternatives to include in the model, typically using a list of all possible alternatives, a list of the alternatives chosen by decision makers, observed levels of the attributes, or some combination of the above. However, this process has been criticized as naïve; particularly in the

marketing literature where it has been argued that individuals have unique choice sets that are formed as part of a multistage choice process (Manski 1977; Horowitz and Louviere 1995). Haab and Hicks (1997) provide evidence that choices in recreation demand are made in a two-stage, consider-then-choose discrete choice process.

In typical discrete choice models, typically, the random utility of the nth individual for choosing the ith alternative is

$$(1) \qquad\qquad U_{ni} = V_{ni} + \varepsilon_{ni}$$

where V_{ni} is the deterministic utility component (i.e., systematic utility influenced by observable covariates), and ε_{ni} is an independent and identically distributed stochastic component reflecting unobservable, individual specific heterogeneity. An nth individual would choose an ith alternative if and only if it provides the highest utility among all the feasible alternatives, A. Formally, utility maximization implies that the probability that the nth individual chooses the ith alternative is

$$(2) \qquad\qquad P_n(i) = Prob(V_{ni} + \varepsilon_{ni} \geq V_{nk} + \varepsilon_{nk}, \forall k \in C).$$

Where V_{nk} is the deterministic utility component from the kth alternative, ε_{nk} is an independent and identically distributed stochastic component reflecting unobservable individual and specific heterogeneity for choosing every k^{th} alternative in set C, and V_{ni} and ε_{ni} are as defined in equation (1).

The Conditional Logit Model

Assuming ε_{ni} is an independent and identically distributed as a Gumbel distribution, yields the standard CL (Greene 2003). Given these assumptions, the probability $P_n(i)$ in equation (2) can be written as

$$(3) \qquad\qquad P_{ni} = \frac{\exp(V_{ni})}{\sum_{k \in C} \exp(V_{nk})}$$

Equation (3) is crucial in discrete choice modeling and the deterministic part of utility V_{ni} is assumed to be a linear function of I attributes known to the researcher. Formally, $V_{ni} = x'_{ni}\beta$, where x_{ni} is the vector of attributes of the ith alternative for the nth individual (e.g., distance d, and perceived site quality, q) and β a parameter vector. Through parameter estimates of β, probability of selection and marginal effects for each alternative can be easily obtained by maximum likelihood estimation. If the choice set C is incorrectly specified, parameter estimates would be biased (Manski 1977).

The Independent Availability Logit Model

Applications of the CL assume the set of alternatives in the choice set is known by the analyst. Individual-specific choice sets are allowed, but the composition of these choice sets must still be known with certainty. In some cases, however, this assumption may be inappropriate, and the choice set developed by the analyst may either include alternatives that were never considered by the decision-maker or may exclude those considered by the decision-maker but overlooked by the analyst, rendering C with either too many alternatives or too few alternatives. Both cases would render misspecification of the likelihood function and could lead the CL to estimate biased parameters (Swait and Ben-Akiva 1987; Li, Adamowicz and Swait 2015). When choice sets are endogenously determined it can be very difficult for the analyst to determine the exact choice set, because endogenous behavior is not typically observed. Although it may be possible to collect data about the relevant alternatives through survey methods, it has also been argued that choice set formation warrants probabilistic modeling of the choice set formation process (Wichmann, Chen and Adamowicz 2016) that prompted Manski (1977) to extend the CL model to a two-stage consider-then-choose model.

Building on the seminal work of Manski (1977), Swait and Ben-Akiva (1987) developed the IAL that models choice set formation as a two-stage process. In the first stage, a population of N individuals faces a set B with I alternative sites, the nth individual forms a non-empty choice set $B \in C$ and then chooses an alternative that maximizes utility in the second stage. Following Wichmann, Chen and Adamowicz (2016), the random utility function is defined on a feasible set B and the unconditional probability that the nth individual chooses an ith alternative is

$$(4) \qquad P_n(i) = \sum_{i \in B, B \in C} P_n(i|B) Q_n(B)$$

where $P_n(i|B)$ is the conditional probability that alternative I is chosen given that choice set B is the nth individual's true choice set, $Q_n(B)$ is the probability that B is in n^{th} individual's true choice set, I is an alternative in B, and C is a set of all possible non-empty choice sets. In the IAL model, $Q_n(B)$ is

$$(3) \qquad Q_n(B) = \frac{\prod_{h \in B} A_n(h) \prod_{k \in (C-B)} [1 - A_n(k)]}{1 - \prod_{i \in C} [1 - A_n(i)]}$$

where $h, I,$ and k index alternatives belonging to different choice sets, $A_n(.)$ is the probability that an alternative is included in the choice set. It is assumed that inclusion or exclusion of alternatives is independent of inclusion or exclusion of another alternative. For the nth individual, $A_n(i)$ denotes availability of the ith alternative. It is assumed that an alternative's availability is determined by a threshold for one variable (e.g. price or distance) such that

$$(6) \qquad A_n(i) = Prob(d_{ni} < \tau + \vartheta_{ni}) = \frac{1}{1 + \exp(\mu(d_{ni} - \tau))}$$

where d_{ni} is a choice attribute, τ is the threshold parameter for this attributes, and ϑ_{ni} is an identically and independently logistic distributed random variable with a location of zero and a scale of μ. In the context of recreation demand, equation (6) can be interpreted as the probability that the distance d_{ni} to reach alternative I exceeds the threshold $\tau + \vartheta_{ni}$ used by individual n to

determine which alternatives they will compare in making their final choice. In this case, the threshold is akin to the maximum distance an individual is willing to travel to access a recreation site, beyond which the site is not part of the choice set.

In the IAL model, the conditional probability that alternative I is chosen given that B is the nth individual's true choice set (included in equation (4)) is defined as

$$(7) \qquad P_n(i|B) = \begin{cases} \frac{\exp(V_{ni})}{\sum_{k \in B} \exp(V_{nk})} & \text{if } i \in B \\ 0 & \text{if } i \notin B \end{cases}$$

where V_{ni} is the systematic component of random utility $U_{ni} = V_{ni} + \varepsilon_{ni}$ where $V_{ni} = x'_{ni}\beta$ (defined in equation (3)), where ε_{ni} is identically and independently distributed Gumbel with scale fixed at unity and independent of the distance cutoff error ϑ_{ni} (Swait and Ben-Akiva 1987).

Equations (4)-(7) define the IAL model and its parameter estimates are usually obtained by maximum likelihood estimation. The model has been applied in several disciplines in both methodological and applied work. For example, Wichmann, Chen and Adamowicz (2016) used it to measure the importance of social networks on choice set formation, Haab and Hicks (1997) used it to measure the value of outdoor recreation sites, and Li, Adamowicz and Swait (2015) used it to examine the effects of choice set misspecification on RUM welfare measures.

Studies by Li, Adamowicz and Swait (2015) and Wichman, Cheng and Adamowicz (2016) used Monte Carlo simulations in their settings. Though flexible and often insightful, Monte Carlo studies are subject to between-simulation variability called Monte Carlo error (Koehler, Brown and Haneuse 2009). But most simulation-based studies ignore Monte Carlo error, which informs the analyst the amount of uncertainty in their finite simulation samples (Koehler, Brown and Haneuse 2009). This approach is partly motivated by considering this

uncertainty in simulation based results, and I report the Monte Carlo error in the mean parameter estimates.

Unlike previous studies, I also consider the performance of the CL and IAL for two scenarios shown in figure 1. In the first scenario, I estimate both the CL and IAL models from data simulated under a stochastic choice set formation (IAL model) by varying distance cutoffs to accommodate irrelevant alternatives in choice sets. In the second scenario, I estimate both models from data simulated based on the assumption that the choice set includes the full set of alternatives (CL model). I measure the bias and efficiency of parameter estimates from two discrete choice models commonly used in the literature when either choice sets are stochastically formed or when there is no choice set formation and individuals choose from the full set of alternatives. Using real databases, I vary the distances and estimate the CL model at all distance cutoffs but the IAL model is estimated once since its results are not expected to change.

Data and Procedures

In this section, I describe the data generating processes when no actual data were used. I also describe the actual data used. Specifically, I describe the Monte Carlo experiments employed besides summaries of variables from actual data set used.

Monte Carlo Experiments

Table 1 presents the basic setting of the data generation process. In both cases, there are 200 data sets (or replications, $R = 250$), 3,000 choice tasks (or observations, $O = 3,000$), and 10 alternatives in each choice set ($J = 10$). The alternatives are described by two attributes ($K = 2$) that reflect distance (d) to the site and a measure of perceived quality (q). The choice for $R=200$ and 3,000 choice tasks in both models was driven by time constraints imposed by the computational burden of estimating the IAL model rendering us not to vary the number of

observations. The two situations correspond to discrete choice recreation demand models where either each individual is assumed to face the same set of all recreation sites in a global choice set or different individuals consider different choice sets and may not automatically choose all options because of bounded rationality (Eliaz and Spiegler 2011).

Suppose that the two attributes are, in fact, distance (d) to access the alternative and the perceived environmental quality (q) of the alternative. One of the reasons alternatives may become irrelevant in a recreational site choice problem is when some people lack knowledge about the sites far from their homes and may thus, consider these sites as irrelevant. Since distance and quality vary across alternatives, the two variables are randomly and independently drawn from a uniform distribution between 0 and 1 for $n = 1, ..., 3,000$, and $i = 1, ..., 10$.

As earlier mentioned, the IAL model has a two-stage choice set formation built on work by Manski (1977). How I accomplish this process is summarized below. In the first stage, I draw independent errors, ϑ_{ni} (see equation (6)) from a logistic distribution with mean zero and scale parameter $\mu = 10$ in each replication. All alternatives or their combinations that satisfy equation (6) are included in the nth individual's choice set, where $\tau = 0.5$. The number of alternatives and their combinations is determined by $2^j - 1$ where j represents the number of alternatives in each choice set. In the current study, $j = 10$ to represent the ten categories. Thus, I considered 1023 non-empty choice sets where the universal choice set ranges from {1}, {2}, {3}, {4}, {5}, {6}, {7}, {8}, {9}, {10}, {1, 2}, {1, 3}, ... , and so on.

In the second stage, individuals compare the utility level from each alternative in their choice sets (as described in first stage) and select the alternative with the largest utility. I draw the random portion of utility ε_{ni} from a Gumbel distribution $\mathbf{G}(0, 1)$. For the sake of brevity, I assume a linear and additive form of $V_{ni} = x'_{ni}\beta$ where x includes a vector of alternative specific

constants for alternative 1 (*ASC*1) through alternative 9 (*ASC*9) where *ASC*10 is the base

category. The two attributes, distance and site quality, are included as regressors in both models.

Next, I estimate the CL and IAL models for 250 simulations, and I vary the distance

cutoffs after each set of simulations: first 0.2, then 0.4, then 0.6, and 0.8. The IAL model is

estimated when the cutoff is 0.2 while the CL is estimated at all distance cutoffs. Finally, I

simulate the data without considering a stochastic choice set (i.e., the CL being the true model)

as follows. Given the true vector of parameter estimates and an array of uniformly distributed

distance and site's quality, I generate the $X\beta$ matrix and compute probabilities for each

alternative based on equation (1). I then draw a Gumbel distributed random **G**(0,1) variable

added to the deterministic portion of utility (equation 1). The choice is made based on random

utility maximization from which individuals choose the recreation site with highest utility. I then

estimate both CL and IAL models once for 250 simulated datasets. The CL framework does not

assume distance cutoffs – its underlying assumption is that people would consider all recreation

sites presented to them unlike in the IAL. Thus, I estimate seven models altogether: 1) IAL at

distance cutoff of 0.2, 2) CL at distance cutoff of 0.2, 3) CL at distance cutoff of 0.4, 4) CL at

distance cutoff of 0.6, 5) CL at distance cutoff of 0.8, 6) CL, and 7) IAL when the choice set

formation is not considered and individuals choose from the full set of alternatives.

What follows next for the first scenario (i.e., IAL is true model) is a comparison of mean

parameter estimates from CL and IAL with the true parameter vector is 1.50, 5.00, 3.00, 2.50,

2.00, 0.75, 1.25, 2.70, 4.00, 4.50, -3.50, 0.50, and 10.00 for *ASC*1, *ASC*2, *ASC*3, *ASC*4, *ASC*5,

*ASC*6, *ASC*7, *ASC*8, *ASC*9, site quality, distance, τ, and μ, respectively. Notice that τ and μ

affect decision making via equation (6).

Empirical Data

Empirical data used in the current study are part of the revealed preference (RP) data used in Cha and Melstrom (2018). The study by Cha and Melstrom (2018) focus on paddlefish angler preferences and catch-and-release regulations in Oklahoma and used choice experiment data that this study benefits from. Thus, a more detailed description of the data can be found in Cha and Melstrom (2018). Table 2 shows descriptive statistics of the empirical data used. These summary statistics show that about 2% of the anglers decided to go fishing for paddlefish while about 20% of them preferred to go fishing to at least 2 fishing sites. The average number of paddlefish anglers were able to catch was about 3 fish per day while the maximum number was about 9 fish. Furthermore, table 1 shows that about 60% of anglers preferred to fish from the river bank to other alternatives. In terms of distance to the fishing site, the average distance was about 134 miles while the maximum distance anglers could travel was 2073 miles. While there are other variables in the data, this study only uses the described variables for brevity. Similar to Monte Carlo simulations, the following are distance cutoffs in miles that were employed for empirical data: 100, 200, 300, and 400. The CL is estimated at all these cutoffs while the IAL is estimated at distance cutoff of only 100 miles.

Evaluating Parameter Estimates

To assess the efficiency of the estimates, I measure the average differences between the parameter estimates $\hat{\beta}$ from values β. This is achieved by computing the square of the differences between the parameter estimates and their true values and taking their mean values. Thus, specifically, I compute the mean square error (MSE) as

$$(6) \qquad MSE(\hat{\beta}) = \frac{1}{R} \sum_{r=1}^{R} \sqrt{[\hat{\beta}_r - \beta]^2}$$

where $r=1,\dots,R$, R denotes replications, $R=250$ is the number of replications in each experiment, and $\hat{\beta}$ denotes an estimate of β. The MSE is a measure of how accurate and precise the estimate is and can thus also be decomposed as (Wackerly, Mendenhall, and Scheaffer 2014)

$$MSE(\hat{\beta}) = Bias^2(\hat{\beta}) + Var(\hat{\beta})$$

where square of bias measures the accuracy of the estimate while variance measures its precision.

As earlier mentioned, I also determine bias of mean parameter estimates. Thus, for each parameter β, the average value $\bar{\beta}$ and the standard error σ over 250 replications are computed and a hypothesis test to determine the significance of the difference between the two is conducted. I also report the Monte Carlo error of mean parameter estimates to measure the level of inter-simulation variability for all models. More details about the computation of the Monte Carlo errors can be found in Koehler, Brown and Haneuse (2009), White (2010), and Meaney and Moineddin (2014).

As mentioned before, I varied distance cutoffs at 0.2, 0.4, 0.6, and 0.8 for Monte Carlo simulations. Varying distance cutoff allows us to determine the bias when the analyst potentially misjudges the deterministic portion of the cutoff. Broadly, it represents varying maximum distances that individuals may face when choosing recreation sites, they tend to visit. For example, a distance cutoff equal to 0.40 would imply that recreation sites located less or equal to 40 miles away would be considered relevant to be accessible by individuals, otherwise, they would not. Thus, without loss of generality, increasing the cutoff to .80 could mean that recreation sites located up to 80 miles away are included in the choice sets. Empirical analyses make this clearer. A distance cutoff of 300 miles implies that recreational sites located more than

300 miles away from an individual would not be relevant in their consideration sets. The same implication holds for other empirical distance cutoffs used.

I used R Statistical Software version 3.5.0 (R Core Team 2018) for simulation of data and estimation of all the models. The *optim* function in R utilizing numerical optimization based on the Broyden-Fletcher-Goldfarb-Shanno (BFGS) algorithm was used to maximize the likelihood function for the IAL model. The BFGS algorithm was preferred because it nearly always works better than alternative algorithms (Train 2009). Additionally, the survival package in R (Therneau 2015) was used to estimate the CL model at the same time as the IAL model.

Results

In this section, first I present mean parameter estimates of the CL and IAL models obtained from the Monte Carlo experiments. I also present tests of parameter bias as well as the mean proportional root mean square errors. This is followed by empirical results.

Monte Carlo Simulation Results

Table 2.3 presents mean parameter estimates for CL and IAL models estimated from data simulated from a stochastic choice set formation process. The CL model is estimated on the complete set of alternatives, which therefore includes irrelevant alternatives in this set up. Column 2 shows the true parameter vector of the underlying data generating process while columns 3 and 4 present mean parameter estimates for IAL and their bias, respectively. The columns that follow present mean parameter estimates and respective bias of the CL model estimated at the three different distance cutoffs (i.e., at distance cutoffs of 0.4, 0.6, and 0.8)

Based on the *t*-statistics $(\bar{\beta} - \beta_{true})/se(\bar{\beta})$ and $\overline{\beta}$, where $se(\bar{\beta})$ is the standard error of $\bar{\beta}$, I test whether the mean parameter estimates are statistically different from their true ones.

The *t*-statistics comparing the true and model values are in parentheses of table 2.3. The null hypothesis is that the estimated value is equal to the true value.

Results in table 2.3 indicate that when choice set formation is stochastic, on average the IAL model recovers the true parameter estimates since the bias between the true parameters and mean parameter estimates is significantly different from zero. This is not surprising because the IAL model describes the real data generation process underlying these results. This is the reason I do not present results for IAL at other distance cutoffs because such results are expected. However, mean parameter estimates from the CL model are not free from significant bias at every distance cutoff. At the distance cutoff of 0.2, most parameter estimates under the CL are significantly biased. When the distance cutoff is raised to 0.4, 0.6, and 0.8, on average, mean parameter estimates from the CL are more negatively biased. Specifically, the mean parameter estimate for the coefficient of the variable *distance* is less negatively biased as the distance cutoff is increased – as more fishing sites are included in the choice sets. I attribute this bias to the endogenously determined distance cutoff which seems to have a significant impact on mean parameter estimate of the coefficient for the variable *distance*. However, a much closer look at the distance coefficient in the CL model suggests that changing the distance cutoff has little effect on the parameter estimates, at least over the range of cutoffs considered here. This suggests there is little evidence that performance of the CL model improves as the effect of the distance cutoff is quite less on choice set formation.

I present mean parameter estimates for the CL and IAL models estimated from choice data in which the decision makers consider the full set of recreation sites in table 2.4. The true model is shown in column 2 while the following columns, as before, present mean parameter

estimates and their bias for both CL and IAL models. Model performance statistics are presented in the lower panel.

The mean parameter estimates of the CL are very similar to the true parameter estimates. The mean parameter estimate of the coefficient of distance is -3.519 which is close to the true value of -3.500. Based on the reported *t*-statistics, I fail to reject the null hypothesis of no difference between the true value and mean parameter estimates. Similarly, the IAL model produces significantly unbiased parameter estimates of the CL. The results in table 4 indicate that when individual choice sets include the full set of alternatives (i.e. when the CL is the true model), the IAL model still successfully recovers the true parameter estimates.

A researcher modeling choice set specification problems would also be interested in knowing the proportional deviation of estimates from their true parameters, or the efficiency of the CL and IAL parameter estimates under the two scenarios I examined. As earlier mentioned, I evaluate efficiency based on the MSEs of the mean parameter estimates. Table 2.5 reports MSEs, proportion of MSEs by bias, and proportion of MSEs by variance of the estimates, of the CL and IAL models for the estimation results in table 2.3.

Focusing first on the MSE benchmark, for which lower values are preferred, I find that the IAL model has, on average lower MSEs (i.e., 0.582) than the CL model (i.e., 2.415). Thus, in addition to the CL failing to successfully recover true parameter estimates, its parameter estimates are less efficient than the IAL's. At the distance cutoff of 0.2, a comparison of the IAL with the CL indicates that that CL's distance parameter estimate deviates from its true value by more than 100% (indicated by the MSE). In terms of average MSEs across all distance cutoffs (shown in the third row from the last row of table 2.5), results indicate that mean parameter estimates from the CL lose more efficiency as distance cutoff increases. I attribute this result

45

from the fact that the CL does not have availability terms that are in the IAL which could hamper the former's performance. Additionally, a decomposition of MSE into variance and bias when the IAL is the true model shows that 95.8% of MSEs of IAL's mean parameter estimates emanates from variance of its estimates while 4.2% is from bias. As for the CL (assuming IAL is the true model), about 16% of its MSEs are from variance of its estimates while about 84% of the MSEs emanates from bias component. This is reasonable especially that under this scenario, IAL's mean parameter estimates are free from significant bias while majority of the mean parameter estimates from the CL are significantly inaccurate.

The MSEs for model parameters under CL and IAL when choice sets include the full set of alternatives are presented in table 2.6. Like in previous results, it turns out that the model that underlies the data generating process is characterized by more efficient mean parameter estimates than otherwise. On average, mean parameter estimates from the CL have MSE of 0.082 while those from the IAL model have on average MSE of 5.406. This suggests that the CL produces parameter estimates that are more than 100% more efficient than the IAL when there is no choice set formation and individuals choose from the full set of alternatives. Thus, even if the IAL model successfully recovers true parameter estimates of the CL model, it comes with the loss of efficiency rendering us not to recommend it for data characterized by deterministic choice sets if the alternative procedure, CL model can be easily implemented. After decomposing MSE into variance and square of bias, results suggest that variance of mean parameter estimates from the CL and IAL contribute 99.4% and 86.7% to their MSEs, respectively, the rest proportion of MSEs emanating from square of bias. This is unlike when the IAL is the true model. The reason for this could be that because both CL and IAL successfully recover the true parameter estimates when the CL is the true data generating mechanism.

Our finding that the model of interest produces mean parameter estimates that are clearly close to the true model if the data generation process is characterized by that model's assumptions are consistent with Li, Adamowicz and Swait (2015), and Wichamn, Chen and Adamowicz (2016) despite their respective studies being totally different from the current study. Additionally, thr finding that the CL model fails to successfully recover true parameters when individual decisions are made following stochastic choice set formation parallels the results of Li, Adamowicz and Swait (2015). Again, this similarity is in spite of the two studies focusing on divergent research questions—specifically, Li, Adamowicz and Swait (2015) study the effect of ignoring or misspecifying choice set formation on welfare measures in random utility models which is quite different from this study's focus.

Empirical Results

Estimation results of the CL and IAL models when the distance cutoff is varied empirically are shown in table 2.7. The variables *access to sites, catch* and *distance to site* are significantly different from zero at 1% significance level in the IAL model. At the distance cutoff of 100 miles, the IAL model attempts to estimate the true value using τ (distance threshold's estimate) in its availability function (in equation 6). As shown in table 2.7, the IAL model's distance threshold is significantly different from zero and close to the true value of 100. This implies that the IAL correctly predicts the empirical distance cutoff that is set by the researcher. These results corroborate Monte Carlo simulation results obtained in table 2.3. By magnitude, coefficients of the CL are close to those in IAL at the empirical distance cutoff of 100 miles except that the distance coefficient that is larger.

When the distance cutoff is increased to 200 miles, on average, results from the CL model change profoundly. Specifically, the coefficient of *distance to site* drops rapidly and

continues to do so as the distance cutoff increases to 400 miles. However, the difference between coefficients of *distance to site* is only marginal when the distance cutoff is 300 miles and when it is 400 miles. This might be because a substantial number of fishing sites were in most paddlefish anglers' choice sets rendering any changes of the distance cutoffs to have inconsiderable effects on the magnitude of *distance to site*. These results are consistent with simulation results in table 2.3 where the coefficient of the variable *distance* were seemingly closer by magnitude at higher distance cutoffs. In terms of model selection, the IAL records the lowest Akaike Information Criterion (AIC) which suggests that it remains a better fit for RUM models when the researcher has some idea about the supposed attribute cutoffs. However, the IAL model's advantage over the CL model could be structural because the IAL model has the availability terms which are absent in the CL even though the two models are non-nested.

Conclusion

Choice set misspecification is an important problem in random utility maximization (RUM) models of recreation demand and several papers conclude that ignoring choice set formation leads to biased parameters and welfare estimates. In this article, I extended research into distance-based choice set formation by measuring the bias from two RUM models. Unlike previous studies, I examined two possibilities: first, a subset of alternatives appeared in the choice set based on a stochastic process and, second, the choice set included the full set of alternatives. This is novel in that I simulate choice sets with ten alternatives, and using the two plausible discrete choice models functionally increases the choice beyond a binary decision, even when irrelevant alternatives are endogenously excluded. I achieved this by carrying out Monte Carlo experiments and estimated the conditional logit (CL) and the independent availability logit (IAL) models from simulated data. I also estimated the two models using real world

experimental data on paddlefish angler preferences and catch-and-release regulations in Oklahoma, to compare parameter estimates of the two models without any known parameter estimates.

The IAL model recovered true parameter estimates when a subset of the alternatives formed individual choice sets. I also showed that under the same conditions, the CL model failed to recover the true parameter values. Additionally, and most importantly, I found that under the CL model, the bias changed as one of the variables affecting choice set formation changed—specifically, as the distance cutoff was raised. I also found less efficient mean parameter estimates from the CL than the IAL when the underlying data generation process involved stochastic choice set formation. Thus, I recommend stochastic-choice-set-based methods such as the IAL when the analyst believes that individual choice sets do not include the full set of alternatives.

In the second scenario and as expected, I showed that the CL successfully recovers true parameter estimates when individual choice sets do include the full set of alternatives. Importantly, in this case the IAL model is also able to successfully recover the true parameter estimates. Despite relatively low Monte Carlo error, the IAL model produced considerably less efficient mean parameters than the CL. In both cases, a decomposition of the mean square error (MSE) into bias and variance components showed that biasedness of the mean parameter estimates from the CL contributed the most to their MSEs than the IAL's when the latter was the data generating process. Contrary, when the CL was the true data generating process, variance of the estimates under the IAL contributed more to MSEs since the IAL was able to the true parameter estimates of the CL. Thus. It is naturally better to use the CL model when there is no evidence of choice set formation and individuals choose from the full set of alternatives.

49

Nevertheless, relying on a stochastic choice set formation process under IAL and allowing for the possibility of a two-stage stochastic choice set formation is conceptually appealing. It's realistic that an individual's true consideration sets may not always be known with certainty and that the IAL assumes choice sets are latent is attractive. And based on empirical results, the IAL can predict the true distance cutoff while the magnitude of the CL model's results change meaningfully as the distance cutoff is increased. Although the researcher should be cautious in pursuing such an approach because they should be concerned about the precision of the estimate, results suggest that, in practice, the IAL model will on average recover the true parameters whether or not individual choice sets are formed stochastically.

This study considered a situation in which an alternative such as a recreation site is considered irrelevant partly based on one observable attribute, *distance to the site*. This problem naturally does not generalize to all contexts. There could be other attributes that affect the probability that an alternative appears in an individual's choice set, including environmental quality and the presence of built amenities. While I recognize that the absence of the availability terms in CL model could have contributed to its poor performance vs the IAL (in some cases), employing non-nested hypothesis tests of the two models represents an important challenge for future research.

Table 2.1: The Basic Setting of the Data Generation Condition

	Notation	Value
Number of data sets (replications)	R	250
Number of choice sets (observations)	O	3,000
Number of alternatives in each choice set	J	10
Number of attributes	K	2

Table 2.2: Descriptive Statistics of Empirical Data

Variable name	Variable Description	Mean	Std. Dev	Minimum	Maximum
Choice	Angler decides to fish for paddlefish, =1 if yes, 0 otherwise	0.019	0.139	0.000	1.000
Access to sites	Access to 2 or more fishing sites, = 1 if yes, 0 otherwise	0.200	0.400	0.000	1.000
Catch (daily)	Catch rate (number of fish caught per day)	3.349	2.150	1.400	9.210
River	Fishing site is river, =1 if yes, 0 otherwise)	0.600	0.489	0.000	1.000
Distance to site (miles)	Distance to river in miles	132.669	153.344	0.000	2073.100
Number of observations used					70,600

Table 2.3: Mean Parameter Estimates of CL and IAL Models when the IAL Model is true

		IAL		CL		CL		CL		CL	
Dist. Cutoffs	True			0.2		0.4		0.6		0.8	
Utility	model		Bias		Bias		Bias		Bias		Bias
ASC1	1.500	1.530	0.030	1.009	-0.491	1.042	-0.458	1.040	-0.460	1.045	-0.455
		(0.050)		(-1.281)		(-1.310)		(-1.346)		(-1.338)	
ASC2	5.000	5.251	0.251	3.683*	-1.317	3.527*	-1.473	-2.540*	-2.460	-4.127*	-9.127
		(0.255)		(-3.931)		(-3.161)		(-1.497)		(-3.538)	
ASC3	3.000	3.136	0.136	2.196*	-0.804	2.149*	-0.851	2.136*	-0.864	2.141*	-0.859
		(0.222)		(-2.935)		(-3.194)		(-3.231)		(-3.193)	
ASC4	2.500	2.605	0.105	1.858*	-0.642	1.824*	-0.676	1.794*	-0.706	1.800*	-0.700
		(0.181)		(-2.056)		(-2.186)		(-2.474)		(-2.448)	
ASC5	2.000	2.067	0.067	1.545	-0.455	1.498	-0. 502	1.501	-0. 499	1.499	-0.501
		(0.113)		(-1.331)		(-1.658)		(-1.625)		(-1.627)	
ASC6	0.750	0.778	0.028	0.579	-0.171	0.570	-0.180	0.559	-0.191	0.561	-0. 189
		(0.048)		(-0.380)		(-0.458)		(-0.497)		(-0.497)	
ASC7	1.250	1.448	0.198	0.183*	-0.167	0.114*	-1. 136	0.082*	-1.168	0.077*	-1.173
		(0.314)		(-10.042)		(-11.703)		(-12.198)		(-12.347)	
ASC8	2.700	2.898	0.199	1.633*	-0.168	1.564	-1.136	1.532*	-1.169	1.527*	-1.174
		(0.315)		(10.420)		(-11.704)		(-12.199)		(-12.348)	
ASC9	4.000	4.198	0.198	2.933*	-0.167	2.864*	-1.136	2.832*	-1.168	2.827*	-1.173
		(0.314)		(-10.042)		(-11.703)		(-12.198)		(-12.347)	
Quality	4.500	4.730	0.230	2.299*	-2.201	2.323*	-2.177	2.346*	-2.154	2.355*	-2.145
		(0.245)		(-7.536)		(-8.015)		(-7.659)		(-7.553)	
Distance	-3.500	-3.652	-0.152	-7.233*	-3.733	-7.039*	-3.539	-6.942*	-3.442	-6.933*	-3.433
		(-0.151)		(-7.701)		(-7.711)		(-7.763)		(-7.680)	
τ	0.500	0.502	0.02								
		(0.016)									
μ	10.000	10.246	0.246								
		(0.207)									
Average bias			0.118		-1.183		-1.206		-1.298		-1.532
Monte Carlo error		0.043		0.018		0.019		0.024		0.030	
Loglikelihood value		-258.8		293.9		-312.1		-305.3		-308.4	
AIC		543.6		609.8		646.2		632.6		638.8	

Notes: *t*-statistics comparing the true and model values are in parentheses.

*biased mean parameter estimate at 5% significance level

Table 2.4: Mean Parameter Estimates of CL and IAL Models when CL Model is true

Utility function	True model	CL	Bias	IAL	Bias
ASC1	1.500	1.477	-0.023	1.883	0.383
		(-0.071)		(0.152)	
ASC2	5.000	5.015	0.015	5.219	0.219
		(0.054)		(0.087)	
ASC3	3.000	2.993	-0.007	3.198	0.198
		(-0.026)		(0.079)	
ASC4	2.500	2.480	-0.020	2.693	0.193
		(-0.076)		(0.077)	
ASC5	2.000	2.023	0.023	2.217	0.217
		(0.090)		(0.086)	
ASC6	0.750	0.723	-0.027	1.025	0.275
		(-0.100)		(0.100)	
ASC7	1.250	1.256	0.006	1.620	0.370
		(-0.019)		(0.145)	
ASC8	2.700	2.687	-0.013	3.078	0.378
		(-0.045)		(0.151)	
ASC9	4.000	4.002	0.002	4.227	0.227
		(0.008)		(0.090)	
Quality	4.500	4.519	0.019	4.066	-0.434
		(0.066)		(-1.328)	
Distance	-3.500	-3.519	-0.019	-3.023	0.477
		(0.068)		(1.563)	
Tau				26.143	
				(12.795)[1]	
Mu				9.300	
				(-9.284)[1]	
Monte Carlo error		0.027		0.294	
Average bias			-0.004		0.228
Loglikelihood value		-339.1		-454.6	
AIC		700.2		935.2	

Notes: *t*-statistics comparing the true and model values are in parentheses.

*biased mean parameter estimate at 5% significance level

[1] These are standard errors for respective parameters and not t-statistics. Their respective parameters τ and μ are not part of the true model.

Table 2.5: Mean Square Error (MSE), and Proportion of MSE by Bias and Variance of Mean Parameter Estimates for CL and IAL Models when the IAL Model is true

		IAL	CL	CL	CL	CL
Distance cutoffs			0.2	0.40	0.60	0.80
ASC1	MSE	0.358	0.388	0.332	0.327	0.322
	Variance/MSE	0.997	0.378	0.367	0.355	0.357
	Bias square/MSE	0.003	0.622	0.633	0.645	0.643
ASC2	MSE	1.033	1.847	2.386	8.744	89.939
	Variance/MSE	0.939	0.061	0.091	0.308	0.074
	Bias square/MSE	0.061	0.939	0.909	0.692	0.926
ASC3	MSE	0.395	0.721	0.795	0.817	0.810
	Variance/MSE	0.953	0.104	0.089	0.087	0.089
	Bias square/MSE	0.047	0.896	0.911	0.913	0.911
ASC4	MSE	0.343	0.509	0.552	0.580	0.571
	Variance/MSE	0.968	0.191	0.173	0.140	0.142
	Bias square/MSE	0.032	0.809	0.827	0.860	0.858
ASC5	MSE	0.349	0.323	0.343	0.342	0.345
	Variance/MSE	0.987	0.360	0.266	0.274	0.273
	Bias square/MSE	0.013	0.640	0.734	0.726	0.727
ASC6	MSE	0.355	0.230	0.186	0.184	0.180
	Variance/MSE	0.998	0.873	0.826	0.801	0.802
	Bias square/MSE	0.002	0.127	0.174	0.199	0.198
ASC7	MSE	0.438	1.150	1.299	1.374	1.385
	Variance/MSE	0.910	0.010	0.007	0.007	0.007
	Bias square/MSE	0.090	0.990	0.993	0.993	0.993
ASC8	MSE	0.439	1.151	1.299	1.375	1.385
	Variance/MSE	0.910	0.010	0.007	0.007	0.007
	Bias square/MSE	0.090	0.990	0.993	0.994	0.993
A	MSE	0.438	1.150	1.299	1.374	1.385
	Variance/MSE	0.910	0.010	0.007	0.007	0.007
	Bias square/MSE	0.090	0.990	0.993	0.993	0.993
Quality of site	MSE	0.925	4.931	4.813	4.717	4.681
	Variance/MSE	0.943	0.017	0.015	0.017	0.017
	Bias square/MSE	0.057	0.983	0.985	0.983	0.983
Distance to site	MSE	1.033	14.168	12.733	12.042	11.985
	Variance/MSE	0.978	0.017	0.016	0.016	0.017
	Bias square/MSE	0.022	0.893	0.984	0.984	0.983
Tau	MSE	0.001				
	Variance/MSE	1.000				
	Bias square/MSE	0.000				

Continued…

55

Table 2.5. Continued

		IAL	CL	CL	CL	CL
Distance cutoffs		0.2		0.40	0.60	0.80
Mu	MSE	1.463				
	Variance/MSE	0.959				
	Bias square/MSE	0.041				
	Average MSE	0.582	2.415	2.367	2.898	10.271
	Average variance/MSE	0.958	0.185	0.169	0.184	0.163
	Average bias square/MSE	0.042	0.815	0.831	0.816	0.837

Table 2.6: Mean Square Error (MSE), and Proportion of MSE by Bias and Variance of Mean Parameter Estimates for CL and IAL Models when the CL Model is true

		CL	IAL
ASC1	MSE	0.116	6.463
	Variance/MSE	0.995	0.977
	Bias square/MSE	0.005	0.023
ASC2	MSE	0.064	6.300
	Variance/MSE	0.996	0.992
	Bias square/MSE	0.004	0.008
ASC3	MSE	0.056	6.344
	Variance/MSE	0.999	0.994
	Bias square/MSE	0.001	0.006
ASC4	MSE	0.079	6.358
	Variance/MSE	0.995	0.994
	Bias square/MSE	0.005	0.006
ASC5	MSE	0.069	6.438
	Variance/MSE	0.993	0.993
	Bias square/MSE	0.007	0.007
ASC6	MSE	0.069	7.595
	Variance/MSE	0.989	0.990
	Bias square/MSE	0.011	0.010
ASC7	MSE	0.124	6.631
	Variance/MSE	1.000	0.979
	Bias square/MSE	0.000	0.021
ASC8	MSE	0.082	6.402
	Variance/MSE	0.998	0.978
	Bias square/MSE	0.002	0.022
ASC9	MSE	0.085	6.325
	Variance/MSE	1.000	0.992
	Bias square/MSE	0.000	0.008
Quality	MSE	0.082	0.295
	Variance/MSE	0.996	0.360
	Bias square/MSE	0.004	0.640
Distance	MSE	0.080	0.320
	Variance/MSE	0.996	0.290
	Bias square/MSE	0.004	0.710
Average MSE		0.082	5.406
Average variance per MSE		0.996	0.867
Average bias square per MSE		0.004	0.133

Table 2.7: Empirical Parameter Estimates of CL and IAL Models

	IAL	CL	CL	CL	CL
Distance cutoff (miles)		100	200	300	400
Utility function					
Access to sites	1.029***	0.987***	1.412***	1.615***	1.636***
	(0.116)	(0.105)	(0.103)	(0.09)	(0.106)
Catch (daily)	0.102***	0.133***	0.072***	0.037***	0.037***
	(0.018)	(0.015)	(0.015)	(0.013)	(0.015)
River	0.074	0.020	-0.055	0.096	1.176***
	(0.102)	(0.143)	(0.096)	(0.091)	(0.147)
Distance to site (miles/100)	-32.960***	-36.477***	-28.288***	-22.551***	-22.030***
	(1.737)	(1.054)	(1.270)	(0.995)	(1.038)
τ	123.400***				
	(37.816)				
μ	10.028				
	(294.18)				
Model performance					
Loglikelihood value	-1697.1	-1860.6	-2499.5	-2890.9	-3023.6
AIC	3406.2	3729.2	5007.0	5789.8	6055.2

Notes: standard errors are in parentheses.
*** Statistically significant at 1% level
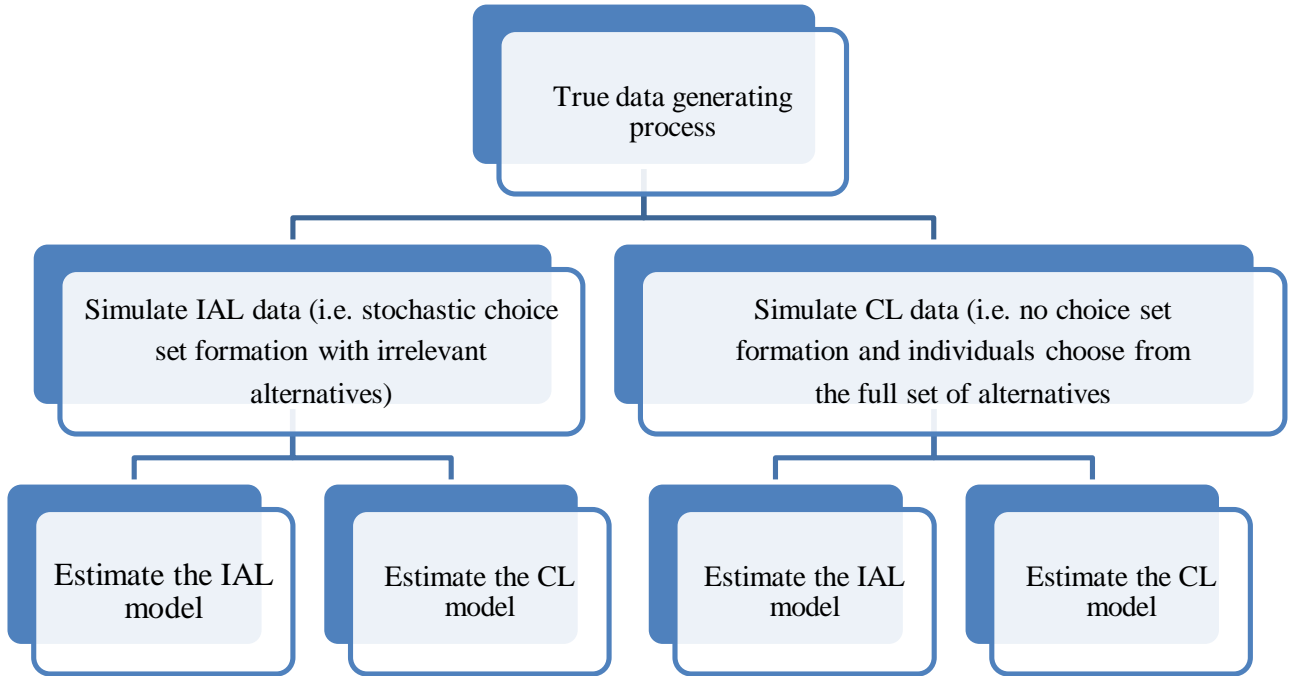
Figure 2.1: Modeling Strategy

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CHAPTER III

BAYESIAN OPTIMAL DYNAMIC SAMPLING PROCEDURE FOR ON-FARM FIELD
EXPERIMENTS

Abstract

For many decades, researchers have relied on small-scale agronomic experiment stations to provide input management recommendations to agricultural producers. But such experiments have most often provided production functions with large standard errors in addition to the uncertainty about how well the estimates apply to different fields. To avoid such limitations, there has been a movement toward large-scale, on-farm field experiments. But questions remain as how best to conduct large-scale, on-farm field experiments and when it is most profitable to quit them. This research addresses these questions by using a fully Bayesian decision-theoretic approach. Data are from Monte Carlo simulations assuming a corn-input stochastic plateau production function from one field. When corn and N prices are $2.95/bu. And 0.45/lb., respectively, results suggest the best way to conduct such experiments is to use a 30-plot experimental design by allocating to each of the 10% of the plots, 0 lb. of N, half of N*, and 150% of N*. Results further indicate that it is most profitable to quit such trials in year 2. Sensitivity analysis confirms the optimal quit period under similar designs but suggests such experiments are most profitable by allocating unalike N levels to all 30% of the experimental plots. Findings suggest designs used in large-scale, on-farm field trials should have N levels reduced in successive years as more information is gained.

Introduction

On-farm agronomic experiments are a source of input management recommendations to agricultural producers. For many years, researchers have relied on experiments conducted on experiment stations in search of generally appropriate crop-input response functions (Bullock and Mieno 2017). The technology has been available in the U.S. and other countries to conduct on-farm field experiments. However, agronomic, economic, and statistical research on how to apply on-farm trial data is limited. In the past, and since the 1950s when Earl Heady conducted numerous field trials, the tendency has been to conduct agronomic experiments on small plots without taking advantage of using large-scale farm machinery. Conducting experiments on small plots is not only labor intensive, but also generates data not adequate for "big data statistical analyses" that would assure more statistical confidence. Such agronomic experiments have most often provided crop-yield response functions with large standard errors in addition to the uncertainty about how well the estimates apply to different fields and possibly different agricultural production methods (Rodriguez 2014; Bullock and Mieno 2017). Thus, despite a long history of surveys and scientific research, there is still substantial variation of the amount of nitrogen (N) recommended to agricultural producers to apply to their crops (Hossain et al. 2004).

To achieve a scenario where limitations from small-scale agronomic trials are avoided, there has been a movement toward large-scale, on-farm field trials – where a whole field is turned into an experiment. On-farm experiment research has gained popularity because it allows agricultural producers to test a variety of agronomic questions using their equipment and

management practices on their own farm fields (Kyveryga, Mueller, and Mueller 2018). Whole-farm experiments are expected to generate huge amounts of data whose analyses and results could in the long run improve input management recommendations that farmers receive from small-scale field trials. Since the 1990s, large-scale, on-farm field trials have been conducted in the U.S. and Latin America to gather large amounts of data for analyses expected to improve scientific input recommendations to farmers. While using precision agricultural technology to run large-scale, on-farm field trials drops the cost of turning a whole field into a trial, the best way to conduct such experiments is not known. Bullock and Mieno (2017) report that a group of researchers conducting these trials in the U.S. and Latin America often vary N rates little for fear of lost yields while farmers are (or seem to be) reluctant to use check plots and replications. On the other hand, promoters of maximum return to N from Midwestern U.S. universities run strip trials for all of their whole-farm field experiments where they have zero yields (with zero inputs) but only experiment with a portion of the field (Bullock and Mieno 2017). Another way to conduct strip trials is where experimenters apply N along some strips but do not apply it on other strips. Thus, a simple but awkwardly difficult question that remains is – which way of conducting large-scale, on-farm field trials is most profitable moving forward?

Bullock and Mieno (2017) assess the value of information of conducting on-farm field trials. Their results suggest a potential profit boost exists by varying N rates little for large-scale, on-farm field trials relative to strip trials. But in their approach, Bullock and Mieno (2017) do not consider yearly random effects though they incorporate rainfall effects in Bullock and Mieno (2019). Including random effects could perhaps provide more realistic profit expectations for agricultural producers because such effects account for unknown events such as hail, weather, weed pressure and others (Tembo et al. 2008). The work by Bullock and Mieno (2017) is based

on a single experimental design and neither does it clearly establish the better one nor determine the best way these experiments should be conducted to provide relevant findings.

Additionally, questions remain as what aspects of large-scale, on-farm field experiments an experimenter should change as more information is gained in subsequent years. Simply put, there is no consensus as to how the optimal experimental design of on-farm field trials should change as more information is added from preceding trials. The objective of this article is to determine if conducting large-scale, on-farm experiments is cost-effective, and if so, to determine the tradeoff between gaining information from using levels of N far from the optimum and the lost yield or wasted N from using nonoptimal levels of N across years. Specifically, I determine optimal profits using corn-input response functions from on-farm field trials where N rates are varied across four levels in different consecutive years. The four levels include: zero N rates on few plots for fear of lost yields, half optimal N rates are applied (optimal is based on current recommendations for continuous corn: see; Boyer et al. 2013), optimal N rates are allocated to plots, and N rates above the optimal levels are allocated to fewer portions of the field.

The choice of the functional form of the crop yield response function is key for modeling production decisions when N rates are varied in large-scale, on-farm field trials. It is reasonable that different functional forms of crop yield response would give dissimilar estimates in the case of variable N rates. Additionally, scientific research has yet to reach a consensus about a universal crop-input response function. While Bullock and Mieno (2013) assume a quadratic functional form of corn yield response function, a considerable share of literature supports the plateau-type plant yield response to N (e.g., Ackello-Ogutu, Paris, and Williams 1985; Cerrato and Blackmer 1990; Paris 1992; Llewelyn and Featherstone 1997; Babcock and Pautsch 1998).

With the unrelenting criticisms by agronomists that crop yield potential is random, the univariate stochastic plateau model by Tembo et al. (2008) frequents the plateau-type literature and has therefore garnered wide applications in agricultural economics research measuring yield response to N (e.g., Tumusiime et al. 2011; Brorsen and Richter 2012; Boyer et al. 2013; Ouédraogo and Brorsen 2018; McFadden, Brorsen and Raun 2018; Dhaka et al. 2019). Thus, building on previous work by Bullock and Mieno (2013), the present study considers the stochastic plateau model by Tembo et al. (2008) as the underlying functional form.

Using simulated data, this study employs a fully Bayesian decision-theoretic approach to information gained from preceding trials. A fully Bayesian decision-theoretic approach is one in which the decision criterion is completely a function of the posterior distribution (Ryan et al. 2016). It is different than the classical framework where the decision is arrived at by averaging some classical criteria over the space that parameters occupy (Pronzato and Walter 1985; Ryan et al. 2016; Drovandi and Tran 2018). This article differs from Griffin et al. (2005), Bullock and Mieno (2017) and Bullock and Mieno (2019) because I model a stochastic plateau production with two random effects that typically characterize empirical agricultural production, in a fully Bayesian decision framework. This way, I am able to capture the value of field experimentation by using prior N recommendations in a given specific field, and update them as data from subsequent trials conducted from the same field are received. The approach used here is practically attractive because two single design years can be used to update the prior distribution and still arrive at valid inference.

A general framework of Bayesian decision theory is presented though the first step is to attempt to solve the problem analytically. But an analytical solution is not possible because of analytically intractable quantities from which I only derive first-order conditions. Thus, the

solution must be found numerically. Several simplifying assumptions are made about the experimental plots, on which different levels of N are allocated, thereby providing more general results. Sensitivity analysis is conducted as a robustness check to different and plausible alternative experimental designs.

Theoretical Model

Conducting agronomic experiments is costly and time consuming. Thus, knowing the best way to conduct them could be crucial to providing value from such experiments. Determining optimal ways of conducting experiments is a decision problem for which previous research has attempted to find solutions using classical and Bayesian methodologies. Bayesian decision methodologies have dominated the literature (e.g., Han and Chaloner 2004; Muller et al. 2006; Ryan et al. 2014; Zhang et al. 2015) because establishing an optimal experiment is more often associated with nonlinear models for which classical approaches are not well suited (Ryan et al. 2016).

Following Lindley (1972), determining an optimal experiment involves defining a decision criterion $U(\boldsymbol{d}, \boldsymbol{\theta}, \boldsymbol{y})$ that captures the worth of deciding to conduct such an experiment that lies in a decision space $\boldsymbol{D}$ for a set of designed sampling locations $\boldsymbol{d}$, yielding data $\boldsymbol{y}$ with model parameters $\boldsymbol{\theta}$. In short, the solution to finding an optimal experiment can be found by maximizing the posterior expected utility over the decision space $\boldsymbol{D}$ with respect to future data $\boldsymbol{y}$ and model parameters shown below

1) $$\text{argmax}_{\mathbf{d} \in \mathbf{D}} E[U(\boldsymbol{d}, \boldsymbol{\theta}, \boldsymbol{y})]$$

But the form of the utility function in equation (1) is mainly determined by the aims of the experiment. Thus, equation (1) incorporates a cost function (Overstall and Woods 2017).

67

Because the aims of on-farm field trials are more inclined to producer theory, the framework above can be extended to production economic theory by making it an expected profit maximization problem. However, large-scale, on-farm field trials are conducted over a planning horizon *T*, which suggests that determining the best way to conduct them would have to encompass a time dimension. Usually, the aims of large-scale, on-farm field trials are to determine optimal input levels – input levels that would maximize profits for agricultural producers. But large-scale, on-farm field experiments can be conducted over a given planning horizon, *T*. This way, the objective function is no longer maximization of expected profit. The goal is finding optimal net present values (NPVs) of conducting experiments over *T*. This is because there would now be a flow of expected profit in successive years in which experiments are conducted. The overall objective for an experimenter would thus be a selection of optimal NPVs higher than others. Assuming the experimenter is risk-neutral, such a decision can be arrived using equation (2)

2) $$\text{argmax}_{\mathbf{d} \in \mathbf{D}} NPV = \text{argmax}_{\mathbf{d} \in \mathbf{D}} \sum_{k=1}^{K} NPV_k$$

where $\mathbf{d}$ is the number of plots on which nonoptimal levels of inputs are applied, NPV is the *k*th NPV obtained from a series of NPVs, from which select only optimal NPVs are selected. Individual NPVs of experiments can be computed by considering a flow of expected profits for every replication. Thus, a given NPV can be computed using equation (3)

3) $$NPV = \sum_{t=1}^{T} \delta_t [P_t \times E(\mathbf{y}_{it}|\mathbf{x}_{it}, \mathbf{d}) - r_t \mathbf{x}_{it}]$$

such that: $\mathbf{y}_{it}|\mathbf{x}_{it}, \mathbf{d} = g(\mathbf{x}_{it}; \boldsymbol{\theta}_t) + \boldsymbol{\varepsilon}_{it}, \mathbf{x}_{it} \geq 0$

where $y_{it}|x_{it}, d = g(x_{it}; \theta_t) + \varepsilon_{it}$ is the production function estimated for a specific field,

y_{it} denotes yield amounts from the ith plot obtained in year t, x_{it} are input levels from the ith

plot used in year t, θ_t are parameters, $\delta_t = (1 + r)^{-1}$ is the discount factor where r is the interest

rate representing the agricultural producer's opportunity cost of time, and ε_{it} is a random error.

First-order conditions from equation (3) are shown in equation (4). For each period from 1

through t:

$$
4) \quad
\begin{cases}
\dfrac{\partial NPV}{\partial x_{i1}} = \dfrac{[P_1 \partial E(y_{i1}|x_{i1}, d)]}{\partial x_{i1}} - r_1 = 0, \\[2mm]
\dfrac{\partial NPV}{\partial x_{i2}} = \dfrac{[P_2 \partial E(y_{i2}|x_{i2}, d)]}{\partial x_{i2}} - r_2 = 0, \\
\quad . \\
\quad . \\
\quad . \\
\dfrac{\partial NPV}{\partial x_{it}} = \dfrac{[P_t \partial E(y_{it}|x_{it}, d)]}{\partial x_{it}} - r_t = 0
\end{cases}
$$

which imply that in each year, the value of the posterior marginal product be equivalent to the

cost of the input. Value of posterior marginal product is the value of marginal product evaluated

using posterior samples of the parameters.

Brorsen and Richter (2011) have shown that three different N levels are sufficient to

identify a plateau. Here I select one of these points to be the current estimate of the optimal

amount of inputs which should be determined. In order to maximize the value of information for

conducting large-scale, on-farm field experiments – whose goal is to provide more and sufficient

recommendations to agricultural producers, I use Bayesian decision theory techniques to recover

a series profits in a given year. It is such profits that flow in a given year and contribute to

individual NPVs. This implies that individual components of NPVs are computed by finding a

solution to the following expected profit maximization problem

5) $\text{argmax}_{\mathbf{x}_{it} \geq 0} \int_{\Theta} \pi_t[R_t(\mathbf{x}_{it}|\boldsymbol{\theta_t})] p(\boldsymbol{\theta_t}|\mathbf{x}_{it}) \, d\boldsymbol{\theta_t}$

where $\pi_t[R_t(\mathbf{x}_{it}|\boldsymbol{\theta_t})]$ is the profit function, R_t is the agricultural producers' expected revenue or wealth, $\boldsymbol{\theta_t}$ is a vector of relevant parameters, and $p(\boldsymbol{\theta_t}|\mathbf{x}_{it})$ is the posterior distribution of $\boldsymbol{\theta_t}$. The expected profit maximization problem in equation (5) has its first-order conditions. They are shown in equation (6) as

6) $\int_{\Theta} \pi_t'[R_t(\mathbf{x}_{it}|\boldsymbol{\theta_t})] R_t'(\mathbf{x}_{it}|\boldsymbol{\theta_t})] p(\boldsymbol{\theta_t}|\mathbf{x}_{it}) d\boldsymbol{\theta_t} = 0$

where $\pi_t'[R_t(\mathbf{x}_{it}|\boldsymbol{\theta_t})]$ is by the chain-rule the first derivative of the expected profit function, $R_t'(\mathbf{x}_{it}|\boldsymbol{\theta_t})]$ is the first derivative of the actual profit function. Equation (6) is behavioral because it dictates optimal choices as a function of $\pi_t(.)$, $R_t(.)$, and $p(\boldsymbol{\theta_t}|\mathbf{x}_t)$ reflecting the agricultural producers' expected, actual wealth or revenue motives, and associated risks, respectively. Equation (5) is at the heart of expected profit maximization problems but it faces a major hurdle: analytical solutions are quite difficult to derive especially for empirical estimation (Lindley 1972; Berger 2013; Ryan et al. 2016; Wu and Guan 2018). The posterior distributions in equations (5) and (6) are computed using Bayes' theorem as

7) $p(\boldsymbol{\theta_t}|\mathbf{x}_t) = \frac{p(\mathbf{x}_t|\boldsymbol{\theta_t})p(\boldsymbol{\theta_t})}{p(\mathbf{x}_t)}$

where $p(\boldsymbol{\theta_t})$ is the prior distribution about parameters $\boldsymbol{\theta_t}$ before collecting new data, $p(\mathbf{x}_t|\boldsymbol{\theta_t})$ is the likelihood, and $p(\mathbf{x}_t)$ is the marginal distribution or the evidence and can be regarded as a normalizing constant. In practice, due to the difficulty in computing the marginal distribution in equation (7), the posterior distribution is computed as

$$8) \quad\quad\quad p(\boldsymbol{\theta_t}|\boldsymbol{x_t}) \propto p(\boldsymbol{x_t}|\boldsymbol{\theta_t})p(\boldsymbol{\theta_t})$$

where all components in equation (8) are as previously defined. Because of the reasons put

forward for equation (5), alternative practical approaches exist that involve solving equation (4)

directly. They involve numerical integration of quantities such as in equation (4). These methods

include Gaussian or Gauss-Hermite quadrature, use of Simpson's rule, and Monte Carlo

integration (Berger 2013). Under Bayesian decision theory, the solution to equation (4) can be

found by evaluating the mean of the profit function evaluated at posterior draws of random

parameters in the profit function. Suppose I generate an identically and independent distributed

sequence of random variables $\{\theta_1, \theta_2, \theta_3, \dots\}$, with a common posterior density $h(\boldsymbol{\theta_t})$ on $\boldsymbol{\Theta}$.

Then

$$9) \quad\quad E\left[\frac{\pi_t(\boldsymbol{x_{it}}|\boldsymbol{\theta_t})]p(\boldsymbol{\theta_t}|\boldsymbol{x_{it}})}{h(\boldsymbol{\theta_t})}\right] = \int_\Theta E[\pi_t(\boldsymbol{x_{it}}|\boldsymbol{\theta_t})]p(\boldsymbol{\theta_t}|\boldsymbol{x_{it}})\, d\boldsymbol{\theta_t}$$

By the strong law of large numbers and under mild conditions (Berger 2013; Wackerly,

Mendenhall, and Scheaffer 2014), it follows that

$$10) \lim_{n\to\infty} \frac{1}{n}\sum_{i=1}^{N}\left[\frac{\pi_t(\boldsymbol{x_{it}}|\boldsymbol{\theta_{it}})]p(\boldsymbol{\theta_{it}}|\boldsymbol{x_{it}})}{h(\boldsymbol{\theta_{it}})}\right] = \int_\Theta E[\pi_t(\boldsymbol{x_{it}}|\boldsymbol{\theta_t})]p(\boldsymbol{\theta_t}|\boldsymbol{x_{it}})\, d\boldsymbol{\theta_t}$$

which can be used to approximate equation (4) as follows

$$11) \quad\quad\quad \cong \sum_{i=1}^{N} E\,\pi_t(\boldsymbol{x_{it}}|\boldsymbol{\theta_{it}})$$

where $\boldsymbol{\theta_{it}}$ is a vector of posterior estimates from an ith draw.

Data and Procedures

Data used for this study are simulated to represent different ways in which large-scale, on-farm field experiments are conducted at a typical farmer's field. Because of the complexity of the problem and lack of actual data, there is no actual field data available for this study. This approach only allows four levels of nitrogen (N) to a corn (y) field. The four levels are (i) the optimal based on the current recommendations and distribution – 156 lb./acre (Boyer et al. 2013), (ii) half the optimal – 78 lb./acre, and (iii) zero N level, and (iv) a level substantially above the optimum – 256 lb./acre.

Boyer et al. (2013) compare the stochastic plateau production functions with their deterministic counterparts to determine the most appropriate response function for continuous corn and rotation of corn with other crops. They find the stochastic response function the more appropriate response function than its deterministic counterparts in all cases. Since our study's focus is on continuous corn, and assumes the stochastic plateau function as the appropriate functional form, I use N rates found in Boyer et al. (2013) to guide the design of my experiments. For the sake of brevity, the total number of plots assumed are 100. Thus, the choice variable is how many plots use levels (ii), (iii), and (iv) described above. For simplicity, I assume that levels (ii), (iii), and (iv) are equal – each is equal to 10 plots while level (i) is 70. The response variable for corn is generated by Monte Carlo simulations using the stochastic plateau as the underlying data generating process for each round of replication. The procedure is illustrated in figure 1.

{Figure 3.1}

As mentioned before, posterior samples are used to determine the expected optimal N levels in the continuing period. The stochastic plateau crop response function (Tembo et al. 2008) is

$$12) \qquad y_{it} = \min(\beta_0 + \beta_1 N_{it}, P_t + u_t) + s_t + \varepsilon_{it}$$

where y_{it} is corn yield from the ith plot in year t, β_0 and β_1 are model parameters to be estimated, N_{it} is the amount of N applied on ith plot in year t, P_t is the plateau yield, $s_t \sim N(0, \sigma_u^2)$ is the plateau year random effect, $s_t \sim N(0, \sigma_u^2)$ is the intercept year random effect, $\varepsilon_{it} \sim N(0, \sigma_\varepsilon^2)$ is the random error term assumed to be independent from the plateau year random effect, and intercept year random effect. Thus, equation (11) is the assumed functional form for the production presented in equation (3). This implies that a vector of parameters of interest is actually: $\boldsymbol{\theta} = (\boldsymbol{\beta_0}, \boldsymbol{\beta_1}, \boldsymbol{\sigma_u^2}, \boldsymbol{\sigma_s^2}, \boldsymbol{\sigma_\varepsilon^2})'$. Following (Ouedraogo and Brorsen 2018), the Bayesian approach to providing the solution to equation (4) involves a maximization of equation (13)

$$13) \qquad E(\pi_t | N_{it}) = p[(1 - \Phi)(\theta_0 + \theta_1 N_{it}) + \Phi\left(P_t - \frac{\sigma_u \varphi}{\Phi}\right)] - r N_{it}$$

where p is corn price, r is price of N_{it}, $\varphi = [\varphi \theta_0 + \boldsymbol{\theta_1} N_{it} - P_t)/\sigma_u]$ is the standard normal probability density function, and $\Phi = \Phi[(\theta_0 + \theta_1 N_{it} - P_t)/\sigma_u$ is the standard normal cumulative distribution. Thus, under Bayesian decision theory, equation (4) is evaluated as an optimization problem using Monte Carlo integration whereby using posterior samples in the preceding Bayesian estimation, the resulting value is obtained as the average (as illustrated in equation (10)). Once the optimal value of N is known in a given period, I use levels of N as well as prior information about the distribution of parameters in the Monte Carlo simulations to

generate the values of corn, y_{it} to be used in the current period's Bayesian estimation. For brevity, the assumption of no spatial correlation between errors is retained. The true model assumed in all Monte Carlo simulations to generate corn levels is

$$14) \qquad y_{it} \sim \min(39.84 + 0.86 N_{it}, 157.93 + u_t) + s_t + \varepsilon_{it}$$

where y_{it}, N_{it}, and everything else is as defined in equation (11). Assuming corn price of 2.95/bu. And N price of \$0.45/lb., the true expected profit function is

$$15) \qquad E(\pi_t | N_{it}) = 2.95 \times E(y_{it}) - 0.45 N_{it}$$

$$\text{such that: } y_{it} \sim \min(39.84 + 0.86 N_{it}, 157.93 + u_t) + s_t + \varepsilon_{it}, N_{it} \geq 0$$

Once corn levels are generated, I estimate posterior means of the parameter vector $\boldsymbol{\theta} = (\boldsymbol{\theta_0}, \boldsymbol{\theta_1}, \sigma_u^2, \sigma_s^2, \sigma_\varepsilon^2)'$ following Bayes' theorem by using Markov chain Monte Carlo (MCMC) methods. Because the distribution of y_{it} is conditional on both plateau year and intercept random effects, equation (8) is estimated in a Bayesian hierarchical framework. All simulations of data are conducted in R software while all Bayesian estimations are carried out using *runjags*, an R software package that utilizes JAGS – Just Another Gibbs Sampler (Denwood 2016; R Core Team 2018). The following informative priors were imposed in all the model estimations. The assumption is that these priors are not accurate or else there would not be any value in experimenting

$$16) \qquad y_{it} \sim \min(\beta_0 + \beta_1 N_{it}, P_t + u_t) + s_t + \varepsilon_{it}$$

$$17) \qquad \beta_0 \sim N(40, \ 6.25), \ \ \beta_1 \sim N(1, 0.0625),$$

$$18) \qquad \sigma_u^2 \sim G(0.0025, 2.500), \sigma_s^2 \sim G(0.0025, 2.500), \sigma_\varepsilon^2 \sim G(0.00375, 2)$$

In each Bayesian estimation phase, the MCMC techniques that I employed involved two chains with a burn-in phase of 5,000 to enable the chains forget their initial regions, with the remaining iterations set at 20,000 per chain. I specifically use Gibbs Sampler because it is more efficient than Metropolis-Hastings especially that the latter generates potentially correlated sequences (Ntzoufras 2009; Ng'ombe and Boyer 2019). Autocorrelation and trace plots, and the Gelman-Rubin test of the remaining parts of each parameter's MCMC chains were used to check whether the chains converged successfully (Gelman and Rubin 1992; Gelman et al. 2013). Autocorrelation plots display the level of correlation between MCMC sequences while trace plots assess mixing of the chains. Additionally, the Gelman-Rubin test checks convergence of a parameter's Markov chain to its posterior distribution. In other words, the Gelman-Rubin test statistic checks whether parameter estimates are stationary, by comparing the within- and between-chain variation using equation (19)

$$19) \qquad \hat{R} = \sqrt{\frac{W + \frac{1}{n}(B - W)}{W}}$$

where $\hat{R}$ is the Gelman-Rubin test statistic – also known as the scale reduction statistic, W is within-chain variance, B is the variance between chains, and n is the size of the post-burn-in MCMC samples. Gelman and Rubin's idea is that if $B \rightarrow W$, then $\hat{R} \rightarrow 1$, which would be strong evidence that the Markov chains converged. In practice, $\hat{R}$ should be estimated across all parameters until all their respective $\hat{R}$'s satisfy $\hat{R} < 1.10$ (Gelman and Rubin 1992, Gelman et al. 2013; Lambert 2018; Plastina and Lence 2019).

When to Quit Experimenting

Following a Monte Carlo integration in which expected optimal N levels are determined, 10 plots are allocated zero of optimal N (expected profit maximizing level of N, labelled N*), while half of N* are allocated to 10 other plots, and 1.5×N* are allocated to other 10 plots while N* is allocated to 70 plots. Based on this, corn levels are generated in a Monte Carlo simulation discussed above. A trivial example below illustrates this point. Consider a field trial conducted in year k from which, a Monte Carlo integration using posterior samples from a preceding year's Bayesian estimation generates N* to be 194.50 lb./acre. This implies that:

20)   0 lb./acre of N^* would be allocated to 10 plots,

21)   half of N^* = 97.25 lb./acre would be allocated to other 10 plots,

22)   1.5×N*= 291.75 lb./acre would be allocated to other 10 plots, and

23) N^*= 194.50 lb./acre would be allocated to the remaining 70 plots.

As discussed above, corn levels would then be generated in a Monte Carlo simulation using the stochastic plateau function as the true data generating mechanism from which another Bayesian estimation to recover the true parameter vector is conducted. In this approach, a Bayesian estimation is conducted on a cumulative sample in every upcoming year so as to account for yearly random effects. For example, a Bayesian estimation for year 2 has 300 observations generated by appending each 100 observations from year 0, 1, and 2. This practice was maintained up to year 10. However, once corn levels are generated, actual profit is computed using 100 observations using corn and N levels from that particular year. It is assumed that corn and N prices are $2.95 per bushel (bu.) and 0.45 per pound (lb.), respectively. The actual cash flows for each year are discounted to year 1 and then used to compute the actual NPV for that year.

But there is another corn that is vital at determining when to quit experimenting – called "corn-quit". Corn-quit is the expected yield at a given year's optimal N*. If the experiment is quit in year k, corn-quit is the amount of corn simulated based on N_k^* in year k+1 through year 10. Using the previous example of year k's N* being 194.5 lb./acre, it means that corn-quit for years k+1 through 10 would be corn simulated at N_k^* = 194.5 by considering only plateau's random effects. This is plausible because once the experiment is quit in year k, the recommendation would be that agricultural producers use N_k^* in future years. Using corn-quit, another cash flow is computed named "quit-profit". Since it would be a series of profit flows from the year the experiment is quit to year 10, quit-profit is used to compute another component of NPV, I called "quit-NPValue".

All these procedures for each year are conducted 250 times. Thus, to determine the optimal period to quit experimenting, another quantity I called "Mean Quit-NPV" (MQNPV) is computed using equation (24)

$$24) \qquad MQNPV_k = Mean\ Actual\ NPV_k + (Mean\ Quit - NPValue_k)$$

where $Mean\ Actual\ NPV_k$ is the average actual NPV for year k from 250 replications, and $Mean\ Quit - NPValue_k$ is the average NPV from 250 replications if the experiment is quit in year k. $Mean\ Quit - NPValue_k$ is computed based on quit profit alone. I also compute the cost of experimentation to determine the additional average cost of an experiment. The kth year's cost of experimentation is computed using equation (25)

$$25)\ Cost\ of\ experimentation_k = Mean\ Actual\ NPV_k - (Mean\ Quit - NPValue_{k-1})$$

where $Mean\ Actual\ NPV_k$ is the average actual NPV for year k from 250 replications, and $Mean\ Quit - NPValue_{k-1}$ is the average NPV from 250 replications if the experiment is quit in the immediate year preceding the kth year. An interest rate of 6% is assumed in all NPV computations. A summary of scenarios considered in the sensitivity analysis is shown in table 3.1.

{Table 3.1}

Results

Diagnostics and Model Results

Because I estimated stochastic plateau functions for 250 times for each year, a snapshot of model results for the ninth year (out of preference) are presented in table 3.2.

{Table 3.2}

The last column in table 3.2, presents Gelman-Rubin test statistics for each parameter in the production function. The Gelman-Rubin test statistics for all parameters are smaller than 1.1, providing strong evidence that my chains converged successfully. A check on the 250th estimation results for the slope parameter using trace and autocorrelation plots shown in figure 3.2 also suggests MCMC sequences mixed well, which is in agreement with the Gelman-Rubin test statistics. I only show autocorrelation and trace plots for the slope's posterior mean to conserve space.

{Figure 3.2}

The posterior medians in table 3.2 show that the posterior median estimate for the intercept term is 40.815 bu./acre, which is close to the true value of 39.84 bu./acre. The posterior

median for the slope is 0.916 which is also close to the true value of 0.860. The slope value implies that an additional pound of nitrogen would result in 0.916 bushels per acre with 95% probability, when other factors are held fixed. The expected posterior plateau yield is 167.313 bushels of corn per acre, a value which is also close to the true one. Variance terms of the plateau year random effect, intercept random effects, and error terms are also on average close to their true values. All of these estimates are statistically significant since the credible intervals do not span zero. I also report Monte Carlo standard errors for the 250th estimation for all parameter estimates for year 9. They are presented in table 3.3. Results in table 3.3 indicate relatively low error from Monte Carlo methods employed because the average Monte Carlo standard error is around 0.018% which is less than 5%, the standard rate at which one would have to get concerned (Denwood 2016).

{Table 3.3}

Optimal Rates

Based on the main experimental design with 30 plots with each of the 10 plots allocated zero N, half of N*, and 1.5×N*, figure 3 shows average optimal N levels from years 0 through 10. As can be seen, both year 0 and 1 have the same expected optimal level of N (i.e., 208.741 lb./acre). I use the same level of N* in both years since I do not have any posterior samples yet to determine the new N*. However, this changes in year 1 when I have 200 observations with varying random effects associated with year 0 and 1. Thus, after a Bayesian estimation in year 1, new N* is generated for year 2 using posterior samples from year 1. From year 2 onwards, average optimal N levels decline substantially to 169.991 lb./acre and 167.256 lb./acre in years 9 and 10, respectively. In terms of variability, the 95% confidence interval of average N estimates are narrower as more information is gained over time. Average optimal N rates decline further.

In short, the estimates become more precise. This suggests that as experiments are conducted successively over the years, the expected optimal levels of N become less variable. In particular, the experimental design also changes because lower levels of N* are attained which then changes allocations to other 30 plots in subsequent years.

{Figure 3.3}

Notice that the original experimental design keeps on being updated and therefore changes each year. For example, each year has a different level of N* and other N levels depend on N* in that specific year. That is in year 10, the final experimental design would be 10 plots with 0 lb./acre allocated, 10 plots with 83.627 lb./acre, 10 plots with 250.883 lb./acre and 70 plots with 167.256 lb./acre. A clearer illustration of the changes in N* besides figure 3 is shown in table 3.4. Table 3.4 presents the expected profit maximizing N, corn-quit, quit-profit, actual profit and the expected cost of experimenting across years. The average corn-quit is around 156 bu./acre across the years while the average quit-profit is between $369.70 and $385.96/acre.

{Table 3.4}

Average actual profit realized for every field trial would be around $340/acre while the average cost of experimentation would be the lowest in year 2. After year 2, the average cost of experimentation would be higher across the years suggesting that the optimal year to quit experimentation would be year 2. This finding corroborates the results in figure 3.4. Figure 3.4 shows a plot of MQNPV across time. As shown in figure 3.4, the optimal time to quit conducting on-farm field trials is year 2 – the year with the highest MQNPV of $2,703.99/acre relative to other years. This value implies that, other things held fixed, the value of returns in the next 8 years if conducting field experiments for continuous corn is quit in year 2 would be $2,703.99/acre. This is the most that would be realized relative to quitting experimentation in

other years. Thus, for a rational experimenter, it would be most profitable to quit on-farm field

trials in year 2. After year 2, MQNPVs decline, which suggests that quitting experiments in later

years would be less profitable, *ceteris paribus*.

{Figure 3.4}

The MQNPV findings indicate that the best sampling procedure to conduct on-farm field

trials is one that involves varying different levels of N on 30% of the plots by equally

distributing the number of plots without N, half N^*, and $1.5 \times N^*$. While these results are

plausible in the given context, checking how robust they are, is of utmost importance for relevant

practical implications. This is because these results may largely depend on their basic parameter

values and assumptions. Thus, as explained before, a sensitivity analysis over a wider range of

potential scenarios was conducted.

Sensitivity Analysis

Moving away from the context discussed above, next I discuss results from sensitivity

analysis. Since I have twelve experimental designs presented in table 3.1, throughout the next

discussion, I have named them based on the total number of experimental plots allocated with

varying N levels out of a total of 100. An illustration makes this clearer: Results presented above

focus on the design with 10 plots, each allocated with 0 of N^*, half of N^*, and $1.5 \times N^{*2}$ while 70

plots were allocated with N^*. From now onwards in the present study, without loss of generality,

such a design is called a 30-plot design, the same classification is extended to other procedures.

Table 3.5 presents sensitivity analysis results for 24-plot (8 plots each with 0N, 0.5N*, and

1.5N*), 18-plot (6 plots each with 0N, 0.5N*, and 1.5N*), 12-plot (4 plots each with 0N, 0.5N*,

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[2] I will drop the multiplication sign from now onwards for simplicity. Thus, instead of $1.5 \times N^*$, I will use
1.5N*.

and 1.5N*), 6-plot (2 plots each with 0N, 0.5N*, and 1.5N*), 10-plot-new (10 plots each with

0.5N*, 0.75N*, and 1.25N*), and 12-plot-new (4 plots each with 0.5N*, 0.75N*, and 1.25N*)

designs. Results show that despite the change in the number of plots allocated to experiments,

average profit maximizing N and corn-quit levels do not differ considerably from results

associated with a 30-plot experimental design. Even over the years, profit maximizing mean N

levels converge to 167 lb./acre while their standard errors become smaller and smaller, as

indicated by narrower confidence intervals.

{Table 3.5}

While even average actual and quit-profit behave likewise, the average cost of

experimentation declines as the number of plots allocated to experiments declines. Based on

table 3.5 and among all designs that are based on 0N, half of N*, and 1.5N*, a 6-plot

experimental design would have least costs of experimentation (about $1.97/acre). This occurs in

year 3. For a 6-plot experimental design, unlike other designs, the optimal year to quit

experimenting would be year 6 because it is associated with the highest MQNPV (i.e.,

$2,757.2/acre). MQNPVs for these designs are shown in figure 3.5. Consistent with the 30-plot-

new experimental design, MQNPV and average costs of experimentation results from the 12-

plot, 18-plot, and 24-plot experimental designs suggest that the optimal quit period for

experimentation is year 2, a finding that is consistent with the 30-plot sampling procedure.

{Figure 3.5}

In general, assuming corn and N prices of $2.95/bu. And $0.45/lb., the best experimental

design is a 12-plot-new experimental design because it is associated with a higher MQNPV (i.e.,

$2,781.5/acre) than the rest. When I include 30-plot-new and 12-plot-new experimental designs

into analysis, a 12-plot-new would be the most profitable sampling procedure with MQNPV of

82

$2,782.9/acre. This is closely followed by a 30-plot-new design whose MQNPV value is $2,781.5/acre. As defined before, in a 30-plot-new or 12-plot-new design, not even a single plot is allocated with zero N, but rather N levels are varied substantially, that is 0.5N*, 0.75N*, and 1.25N*. This means that there are no corn plants or seedlings subjected to zero N in a field under these experimental designs, when other factors are held fixed. Even excess N levels that are applied (1.25N*) are less than the excess N (1.5N*) in other procedures. That is, plot-new designs involve 16.67% lower excess N than the other designs considered here. I attribute these reasons to why both the 30-plot-new and 12-plot-new designs would be more profitable than other designs. Consistent with each other, but fundamentally different from other designs, their most profitable quit period is year 6.

Random changes in economic conditions affect both corn and input prices. I therefore consider such stochastic economic behavior by adjusting corn and N prices as part of sensitivity analysis. I consider 30-plot and 12-plot experimental designs under three scenarios: when both corn and N prices double, when corn price alone doubles, and when N price alone doubles. Table 3.6 shows sensitivity analysis results for respective designs when corn and N prices double (i.e., corn price is $5.90/bu. And N price is $0.90/lb.).

{Table 3.6}

Results in table 3.6 suggest expected profit maximizing N and average corn-quit do not deviate from the main results presented for a 30-plot design at initial corn and N prices. Only monetary variables such as average quit-profit, actual profit, and cost of experimentation increase by 100%. This implies that both corn-quit supply and N demand functions are consistent with economic theory of being homogenous of degree zero in output and input prices. Similarly, the profit functions are homogenous of degree 1 in input and output prices (Jehle and Reny 2001,

83

Mas-Colell, Whinston, and Green 1995) since doubling corn-quit and N prices results in average

optimal quit-profit, actual profit, and cost of experimentation to double as well. Average corn-

quit is around 156 bu./acre across the years while the average quit-profit is between $739.41 and

$773.64/acre. In both cases, the average cost of experimentation is the lowest in year 2 while it is

higher in later years, which agrees with MQNPVs (shown in figure 3.6) for both experimental

designs.

{**Figure 3.6**}

A closer look at figure 3.6 suggests that if prices of both corn and N double (i.e., $5.90/bu. And

0.90/lb., respectively), a 12-plot experimental design would be more profitable than a 30-plot

design because it would have a higher MQNPV ($5,483.90/acre) than the latter ($5,407.30/acre).

This is possibly because the 12-plot design would result in lower yield losses from the 4 plots

without N. Also, smaller N levels would be used excessively under a 12-plot design because only

$1.25N^*$ is allocated to 4 plots while 10 plots are allocated $1.5N^*$ (16.67% more of excess N)

under the 30-plot design. These results are consistent with the 30-plot experimental design by

quit period and not by magnitude of their MQNPVs because of different price regimes in which

they are considered. Table 3.7 shows sensitivity analysis results when the price of either corn or

N doubles, holding the other's constant.

{**Table 3.7**}

As expected, average-corn-quit levels do not substantially differ from the main 30-plot

experimental design. However, when corn price doubles holding N price constant, profit

maximizing levels of N, average corn-quit levels increase by less than 100%. The average cost of

experimentation is generally lower under a 12-plot experimental design than a 30-plot design

perhaps because of the fewer experimental units under the 12-plot design. The MQNPVs for both designs are shown in figure 3.7.

{Figure 3.7}

Results suggest that a 12-plot experimental design would be more profitable (with MQNPV of $6,053.40/acre) than a 30-plot experimental design whose MQNPV is $5,972.70/acre. Results also indicate that under both designs, it would be more profitable to quit experimentation in year 2, a finding which is consistent with the 30-plot sampling procedure.

When only the price of N is doubled (shown in the last panel of table 3.7), average profit maximizing N levels substantially decline from 175.50 per lb. in year 1 to around 150lb./acre. Additionally, corn-quit levels for both 30-plot and 12-plot designs average around 154bu./acre while average actual and quit-profit decline by less than 100%, which is consistent with economic theory that the profit function is decreasing in input prices. As before, under other designs considered previously, the average cost of experimentation is lower under a 12-plot design than under a 30-plot design. In terms of their MQNPVs (shown in figure 3.8), I find that a 12-plot experimental design would be a better procedure than a 30-plot design since it has a higher MQNPV (i.e., $2,250.70/acre) than that of a 30-plot experimental design whose MQNPV is $2,217.9/acre. In terms of the optimal quit period, results still suggest both designs would be most profitable if they are quit in year 2, a result that is also consistent with those found when a 30-plot experimental design was investigated at initial prices.

{Figure 3.8}

Conclusion

Agronomic experiments have been a source of input management recommendations to both researchers and agricultural producers for many years. While such technology has been present in

in the U.S. and other countries across the world, agronomic experiments have largely been conducted on small plots without taking advantage of using large-scale farm machinery. Consequently, this has led to among other things, continuous search for appropriate crop input response functions, some of which are associated with large standard errors and uncertainty (Rodriguez 2014; Bullock and Mieno 2017). To avoid these drawbacks, a movement toward conducting large-scale, on-farm field experiments is underway. Such experiments are expected to be adequate for 'big data statistical analyses" which could lead to improved input management recommendations for agricultural producers in the long-run. But the best way to conduct such experiments is yet to be known. This article attempts to address this gap.

Assuming a stochastic plateau production function from one field, this article employs a fully Bayesian decision-theoretic approach to determine an optimal experimental design for large-scale, on-farm field trials. Data are from Monte Carlo simulations. The analysis spans over 10 years of experimentation with 250 replications. The main experimental design assumed is a 30-plot experimental design with each 10 plots allocated zero N, half optimal N (N*=156 lb./acre), 150% of N*, and the 70 plots are allocated with N*, which is computed sequentially. This procedure differs from previous related research (e.g., Bullock and Mieno 2013) that focus on determining the value of information from large-scale, on-farm experiments without explicitly accounting for the effect of year random and corn plateau random effects. My approach is theoretically sound and can be easily extended to other theoretical and applied economics settings.

Results indicate that when corn price is $2.95/bu. And N costs $0.45/lb., the best experimental design for conducting large-scale, on-farm field experiments for continuous corn is varying 30% of the plots, by equally distributing the number of plots with zero N, 96.882 lb./acre,

and with 290.64 lb./acre with the rest of the plots allocated the optimal level of N = 193.76 lb./acre. These results would be obtained by beginning in year 1 to conduct such experiments with a 30-plot experimental design consisting of 10 plots without N, 10 plots with half N* of 156lb./acre, 10 plots with 1.5×N* (i.e., 234 lb./acre), and the rest 70 plots allocated with 156 lb./acre. I find that the best way to quit these trials would be in year 2 – when the Mean Quit-Net Present Value (MQNPV) of conducting these experiments is maximum. But these results may change as shown in sensitivity analysis.

Sensitivity analyses indicate that the profitability of an experimental design for large-scale, on-farm field experiments depends on how many plots receive variable N. Experimental designs with less than 30 plots upon which N is varied would be more profitable than the main 30-plot experimental design considered here. For example, other factors held constant, I find a 6-plot experimental design to be the best design among those that consider zero N levels, half of N*, and 1.5N* to be allocated equally to experimental plots. On average, the optimal quit period for such designs was found to be year 2 though a 6-plot experimental design would be best quit in year 6 – the only finding different than those from a 30-plot experimental design. The reason put forward is that plots under a 6-plot experimental design would be subjected to lower loss of both yield and N than those with more than 6 plots subjected to experimentation in similar conditions.

But results discussed above are amenable to change once I consider designs that do not allocate zero N to field plots. For example, sensitivity analysis further shows that a 12-plot-new experimental design would be most profitable among all the sampling procedures considered in this article. A 12-plot-new design is the only design that allows allocation of 0.5N* to 4 plots, 0.75N* to 4 plots, and 1.25N* to other 4 plots – which is different than the rest of the designs

discussed above. Plausibly, a 12-plot-new sampling procedure performed better than other procedures because it would avoid substantial loss of both N and yield since each plot is allocated non-zero N compared to other previously discussed designs. Even the optimal quit period under the 12-plot-new design changes to 6 years, which is different from the optimal quit period associated with other designs with the exception of the 30-plot-new experimental design. Thus, the optimal period to quit experimenting basically depends on the experimental design chosen.

Doubling both corn and N prices showed that between a 12-plot and 30-plot experimental designs, a 12-plot design would be more profitable and that both corn-quit supply and N demand functions are homogenous of degree zero in corn and N prices. A 100% increase in either corn or N price still suggests a 12-plot experimental design would be more profitable than a 30-plot design. This provides further evidence that conducting field experiments on fewer plots is more profitable than otherwise. The idea holds true even when either the input or output prices change. In terms of when it is most profitable to quit experimenting, both the 12-plot and 30-plot designs would be most profitably quit in year 2 even when either corn or N price or both prices change. This implies that possible changes in input and output prices barely affect the optimal quit period of experimenting while the question as to when it is most profitable to quit experimenting depends on the design of the experiment one chooses

The finding that the optimal period to quit experimentation does not depend on the number of plots does not agree with Bullock and Mieno (2013) who find that the optimal length of on-farm experiments decreases with field size. This could be because Bullock and Mieno (2013) and the present study employ completely divergent assumptions and methods. Findings from both main and sensitivity analyses have consistently shown that large-scale, on-farm field trials are best conducted by varying N, but only over a small number of plots. That is, they are more profitable

when conducted using experimental designs with fewer number of plots upon which N levels are varied than otherwise. One other important finding consistent in the estimations is that the profit maximizing N levels decline in subsequent years while optimal profits remain afloat. This suggests that as more information is gained in successive years of experimentation, experimenters would have to reduce N levels in subsequent years while still being able to realize similar profit levels as before.

While I recognize the caveat in assuming the same stochastic plateau production function without considering spatial correlation between errors, this study makes a novel contribution by being the first to employ these rigorous techniques that have received little attention in agricultural economics research. Employing spatial correlation between errors in these methods is a significant challenge for future research.

Table 3.1: Experimental Designs Used in Sensitivity Analyses

Design	Price of corn $/bu.	Price of N $/lb.	Levels of N (lb./acre)			
1	5.90	0.90	10 plots with zero of N*	10 plots with half N*	10 plots with 1.5×N*	70 plots with N*
2	5.90	0.90	4 plots with zero of N*	4 plots with half N*	4 plots with 1.50×N*	88 plots with N*
3	2.95	0.45	8 plots with zero of N*	8 plots with half N*	8 plots with 1.5×N*	76 plots with N*
4	2.95	0.45	6 plots with zero of N*	6 plots with half N*	6 plots with 1.5×N*	82 plots with N*
5	2.95	0.45	4 plots with zero of N*	4 plots with half N*	4 plots with 1.5×N*	88 plots with N*
6	2.95	0.45	2 plots with zero of N*	2 plots with half N*	2 plots with 1.5×N*	94 plots with N*
7	2.95	0.45	10 plots with 0.75 of N*	10 plots with half N*	10 plots with 1.25×N*	70 plots with N*
8	2.95	0.45	4 plots with 0.75 of N*	4 plots with half N*	4 plots with 1.25×N*	88 plots with N*
9	2.95	0.90	10 plots with zero of N*	10 plots with half N*	10 plots with 1.5×N*	70 plots with N*
10	2.95	0.90	4 plots with zero of N*	4 plots with half N*	4 plots with 1.50×N*	88 plots with N*
11	5.90	0.45	10 plots with zero of N*	10 plots with half N*	10 plots with 1.5×N*	70 plots with N*
12	5.90	0.45	4 plots with zero of N*	4 plots with half N*	4 plots with 1.50×N*	88 plots with N*

Table 3.2: Bayesian Parameter Estimates of the Stochastic Plateau Corn Yield Response

	True	Median	SD	95% Credible Interval		Gelman-Rubin Test Statistic
Intercept	39.84	40.815	1.686	37.563	44.137	1.000
Slope	0.86	0.916	0.020	0.876	0.954	1.001
Plateau	157.93	167.313	8.062	150.501	182.399	1.002
Variance of plateau	625	526.315	934.579	236.6136	2714.072	1.002
Variance of year random effect	100	25.641	14.084	6.613	300.080	1.000
Variance of the error term	289	300.947	6678.028	276.176	329.435	1.001

Table 3.3: Monte Carlo Standard Errors

	MCSE	MCSE/SD (%)
Intercept	0.027	1.600
Slope	0.000	1.600
Plateau	0.080	10.000
Variance of plateau	0.000	2.000
Variance of the year random effect	0.001	1.700
Variance of the error term	0.000	0.700
Average	0.018	2.933

Table 3.4: Expected Profit Maximizing N, Corn-Quit, Quit and Actual Profit, and Mean Cost of Experimenting Across Years

Variable	Year										
	0	1	2	3	4	5	6	7	8	9	10
Expected profit maximizing N (lb./acre)	208.74	208.74	193.76	182.37	178.64	173.80	172.08	169.69	169.18	167.99	167.25
Mean corn-quit (bu./acre)	157.20	157.51	157.34	156.07	155.89	155.29	157.06	156.28	156.33	155.50	
Mean quit-profit ($/acre)	369.70	370.74	376.98	378.36	379.48	379.89	385.89	384.67	385.96	383.15	
Mean actual profit ($/acre)	335.14	335.38	340.25	345.95	340.30	343.15	342.73	344.40	349.48	342.76	346.85
Mean cost of experimenting ($/acre)		-34.32	-30.48	-31.02	-38.05	-36.32	-37.16	-41.49	-35.19	-42.29	-36.30

Table 3.5: Sensitivity Analysis Results for Different Experimental Designs When Corn Price is $2.95/bu. And N Price is 0.45/lb.

| Experimental Design	Variable	0	1	2	3	4	5	6	7	8	9	10
24-plot experimental design	Profit maximizing N (lb./acre)	208.74	208.74	193.67	183.11	178.54	173.53	172.18	170.08	169.18	168.10	167.33
	Mean corn-quit (bu./acre)	157.16	157.51	157.26	155.92	155.78	155,24	157.04	156.24	156.38	155.44	
	Mean quit-profit ($/acre)	369.70	370.74	376.76	377.56	379.21	379.87	385.78	384.37	385.20	382.92	
	Mean actual profit $/acre)	341.87	341.90	346.92	352.87	348.14	350.40	350.40	352.31	357.18	350.46	354.53
	Mean CE ($/acre)		-27.80	-23.82	-23.89	-29.41	-28.29	-29.46	-33.47	-27.19	-34.74	-28.39
18-plot experimental design	Profit maximizing N (lb./acre)	208.74	208.74	193.19	182.56	178.41	173.20	172.02	169.70	168.92	167.59	166.94
	Mean corn-quit (bu./acre)	157.16	157.51	157.18	155.99	155.84	155.13	157.02	156.14	156.25	155.29	
	Mean quit-profit ($/acre)	369.70	370.74	376.74	378.04	379.45	379.71	385.81	384.26	384.94	382.70	
	Mean actual profit ($/acre)	348.59	348.42	353.97	360.64	355.54	358.23	357.91	360.24	365.59	358.02	362.55
	Mean CE ($/acre)		-21.28	-16.77	-16.10	-22.49	-21.21	21.79	-25.57	-18.66	-26.92	-26.92
12-plot experimental design	Profit maximizing N (lb./acre)	208.74	208.74	191.39	181.95	177.51	172.36	171.58	169.53	168.70	167.52	166.98
	Mean corn-quit (bu./acre)	157.16	157.51	156.92	156.03	155.70	154.94	156.80	156.04	156.08	155.13	
	Mean quit-profit ($/acre)	369.70	370.74	376.81	378.42	379.43	379.52	385.37	384.04	384.52	382.25	
	Mean actual profit ($/acre)	355.32	354.95	361.67	368.76	362.61	366.24	365.05	367.48	373.70	365.83	370.17
	Mean CE ($/acre)		-14.75	-9.07	-8.04	-15.80	-13.19	-14.47	-17.88	-10.33	-18.69	-12.08
6-plot experimental design	Profit maximizing N (lb./acre)	208.74	208.74	193.73	182.05	179.10	173.78	172.27	169.80	168.86	167.51	166.55
	Mean corn-quit (bu./acre)	157.16	157.51	157.08	155.96	155.68	155.03	156.79	156.10	156.01	155.32	
	Mean quit-profit ($/acre)	369.70	370.74	376.22	378.16	378.68	379.16	385.03	384.09	384.25	382.81	
	Mean actual profit ($/acre)	362.04	361.47	367.49	374.25	370.26	372.77	371.96	375.90	381.29	373.69	377.96
	Mean CE ($/acre)		-8.23	-3.24	-1.97	-7.90	-5.90	-7.19	-9.13	-2.80	-10.55	-4.85
30-plot-new experimental design	Profit maximizing N (lb./acre)	208.74	208.74	189.48	182.61	179.09	174.14	172.70	170.30	169.45	168.56	167.64
	Mean corn-quit (bu./acre)	157.16	157.51	157.10	156.00	155.87	155.37	157.10	156.30	156.38	155.63	
	Mean quit-profit ($/acre)	369.70	370.74	378.17	378.04	379.22	379.99	385.73	384.45	385.09	383.26	
	Mean actual profit ($/acre)	364.52	364.39	368.92	373.36	367.05	370.15	369.19	371.45	376.29	368.98	373.45
	Mean CE ($/acre)		-5.31	-1.81	-4.81	-10.99	-9.07	-10.80	-14.27	-8.15	-16.11	-9.81
12-plot-new experimental design	Profit maximizing N (lb./acre)	208.74	208.74	185.67	180.06	177.65	174.20	171.86	169.50	169.08	168.13	167.54
	Mean corn-quit (bu./acre)		157.51	156.77	155.66	155.74	155.11	156.81	156.13	155.99	155.42	
	Mean quit-profit ($/acre)	369.70	370.74	378.93	378.17	379.50	379.18	385.24	384.31	384.09	382.84	
	Mean actual profit ($/acre)	367.07	366.55	374.57	379.20	373.70	376.96	375.70	378.26	384.16	376.06	380.88
	Mean CE ($/acre)		-3.15	3.83	0.27	-4.46	-2.54	-3.48	-6.98	-0.15	-8.03	-1.96

Table 3.6: Sensitivity Analysis Results for Experimental Designs When Corn and N Prices Double

| Experimental Design	Variable	Year										
		0	1	2	3	4	5	6	7	8	9	10
30-plot experimental design	Profit maximizing N (lb./acre)	208.74	208.74	194.77	182.02	178.31	173.91	171.69	170.25	169.23	168.20	167.68
	Mean corn-quit (bu./acre)	155.84	157.51	157.57	156.10	155.87	155.28	157.07	156.32	156.31	155.54	
	Mean quit-profit ($/acre)	739.41	741.48	754.42	757.29	759.18	760.00	771.95	769.55	770.16	766.31	
	Mean actual profit ($/acre)	670.29	670.76	680.73	692.17	680.53	686.24	685.31	688.92	698.81	685.64	693.79
	Mean CE ($/acre)		-68.65	-60.74	-62.25	-76.75	-72.93	-74.69	-83.03	-70.73	-84.51	-72.88
12-plot experimental design	Profit maximizing N (lb./acre)	208.74	208.74	191.05	182.43	177.77	172.91	171.94	169.275	168.67	167.66	166.79
	Mean corn-quit (bu./acre)	157.17	157.47	155.98	156.96	156.37	155.98	157.35	156.92	155.28	155.86	
	Mean quit-profit ($/acre)	739.41	741.24	748.35	761.91	762.62	764.66	773.64	773.48	764.35	768.72	
	Mean actual profit ($/acre)	710.64	709.90	723.887	736.37	726.11	732.27	730.07	735.39	747.52	731.53	740.68
	Mean CE ($/acre)		-29.51	-17.36	-11.97	-35.80	-30.35	-34.59	-38.24	-25.95	-32.81	-28.04

Table 3.7: Sensitivity Analysis Results for Different Experimental Designs When Either Corn Price or N Price Doubles

| Experimental Design	Variable	Year										
		0	1	2	3	4	5	6	7	8	9	10
Corn price doubles (5.90/bu.) but N price remains constant ($0.45/lb.)												
30-plot-new experimental design	Profit maximizing N (lb./acre)	243.47	243.47	225.20	206.98	198.50	191.58	188.52	185.14	183.37	182.10	181.00
	Mean corn-quit (bu./acre)		157.54	157.95	156.86	156.46	156.17	158.04	157.17	157.21	156.57	
	Mean quit-profit ($/acre)	820.38	819.94	830.58	832.37	833.79	835.20	847.60	843.98	845.03	841.86	
	Mean actual profit $/acre)	749.98	750.32	754.93	765.01	752.86	757.13	755.92	759.34	769.55	757.12	762.74
	Mean CE ($/acre)		-70.06	-65.00	-65.56	-79.51	-76.66	-79.27	-88.25	-74.43	-87.91	-79.12
12-plot-new experimental design	Profit maximizing N (lb./acre)	243.47	243.47	224.12	206.47	197.27	190.58	188.84	185.11	183.56	181.82	181.01
	Mean corn-quit (bu./acre)	157.61	157.54	157.96	156.78	156.49	156.00	157.81	157.11	157.10	156.27	
	Mean quit-profit ($/acre)	820.38	819.94	831.14	832.09	834.50	834.67	846.13	843.64	844.30	840.20	
	Mean actual profit ($/acre)	791.12	790.24	799.46	811.84	800.42	806.50	803.16	808.41	821.64	805.68	812.41
	Mean CE ($/acre)		-30.14	-20.48	-19.29	-31.67	-27.99	-31.51	-37.72	-22.00	-38.61	-27.78
Corn price is constant (2.95/bu.) but N price doubles ($0.90/lb.)												
30-plot-new experimental design	Profit maximizing N (lb./acre)	175.50	175.50	163.63	158.69	157.54	154.93	154.22	152.74	152.53	151.83	151.23
	Mean corn-quit (bu./acre)		156.79	154.96	153.85	153.74	152.93	154.63	153.71	153.91	152.76	
	Mean quit-profit ($/acre)	303.43	304.59	309.87	311.03	311.75	311.71	317.37	316.00	316.78	314.01	
	Mean actual profit ($/acre)	271.50	272.21	277.86	283.04	277.97	280.37	279.89	281.37	286.21	280.18	284.91
	Mean CE ($/acre)		-31.21	-26.73	-26.83	-33.06	-31.37	-31.82	-35.99	-29.78	-36.59	-29.09
12-plot-new experimental design	Profit maximizing N (lb./acre)	175.50	175.50	162.20	158.34	156.06	153.90	153.42	151.89	151.39	151.18	150.79
	Mean corn-quit (bu./acre)	156.40	156.79	154.50	153.55	153.28	152.50	154.16	153.26	153.45	152.28	
	Mean quit-profit ($/acre)	303.43	304.59	309.80	310.45	311.72	311.37	316.70	315.41	316.43	313.18	
	Mean actual profit ($/acre)	289.95	290.16	296.44	302.95	296.73	299.32	298.68	300.05	306.28	2.99.04	304.70
	Mean CE ($/acre)		-6.13	-8.15	-6.85	-13.72	-12.39	-12.69	-16.65	-9.12	-17.39	-8.47
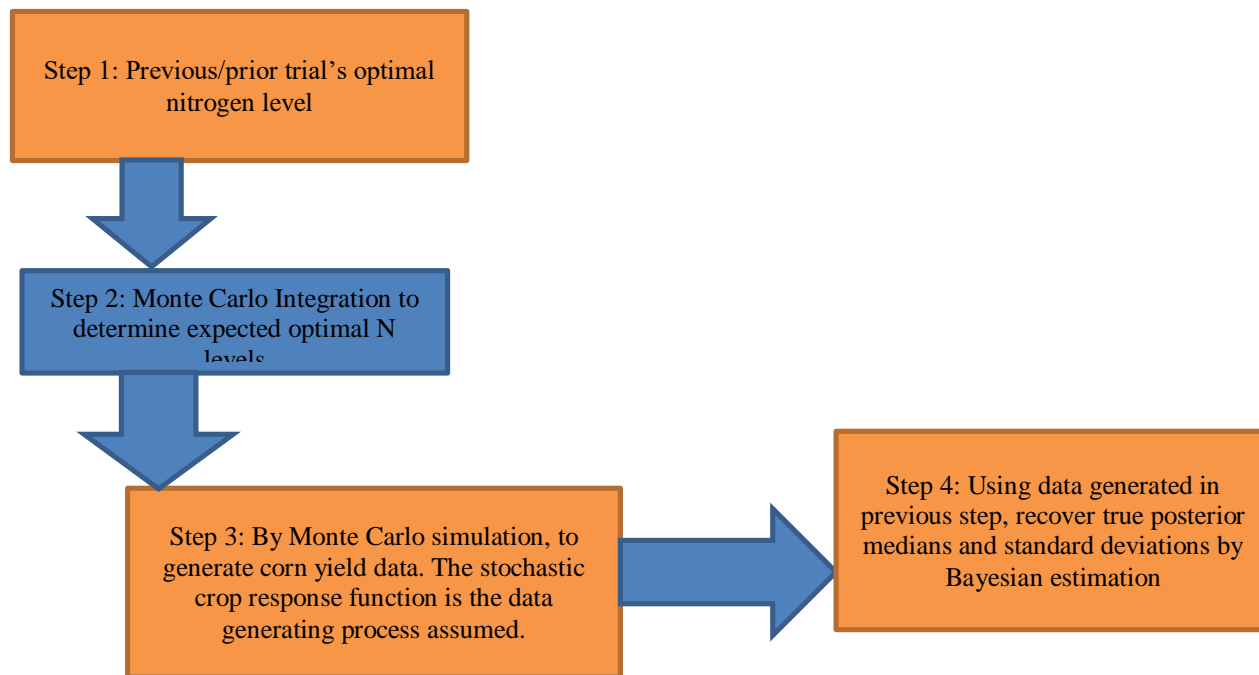
Figure 3.1: Flowchart summarizing the approach used
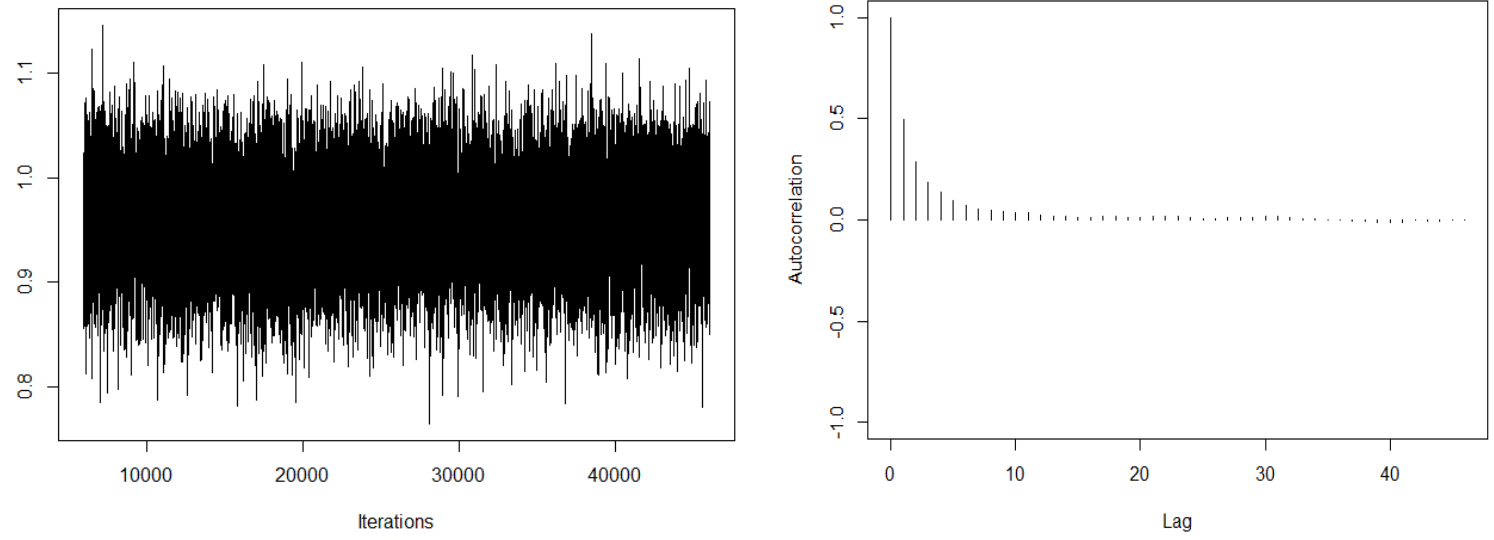
Figure 3.2: The trace and autocorrelation graphs for β_1.
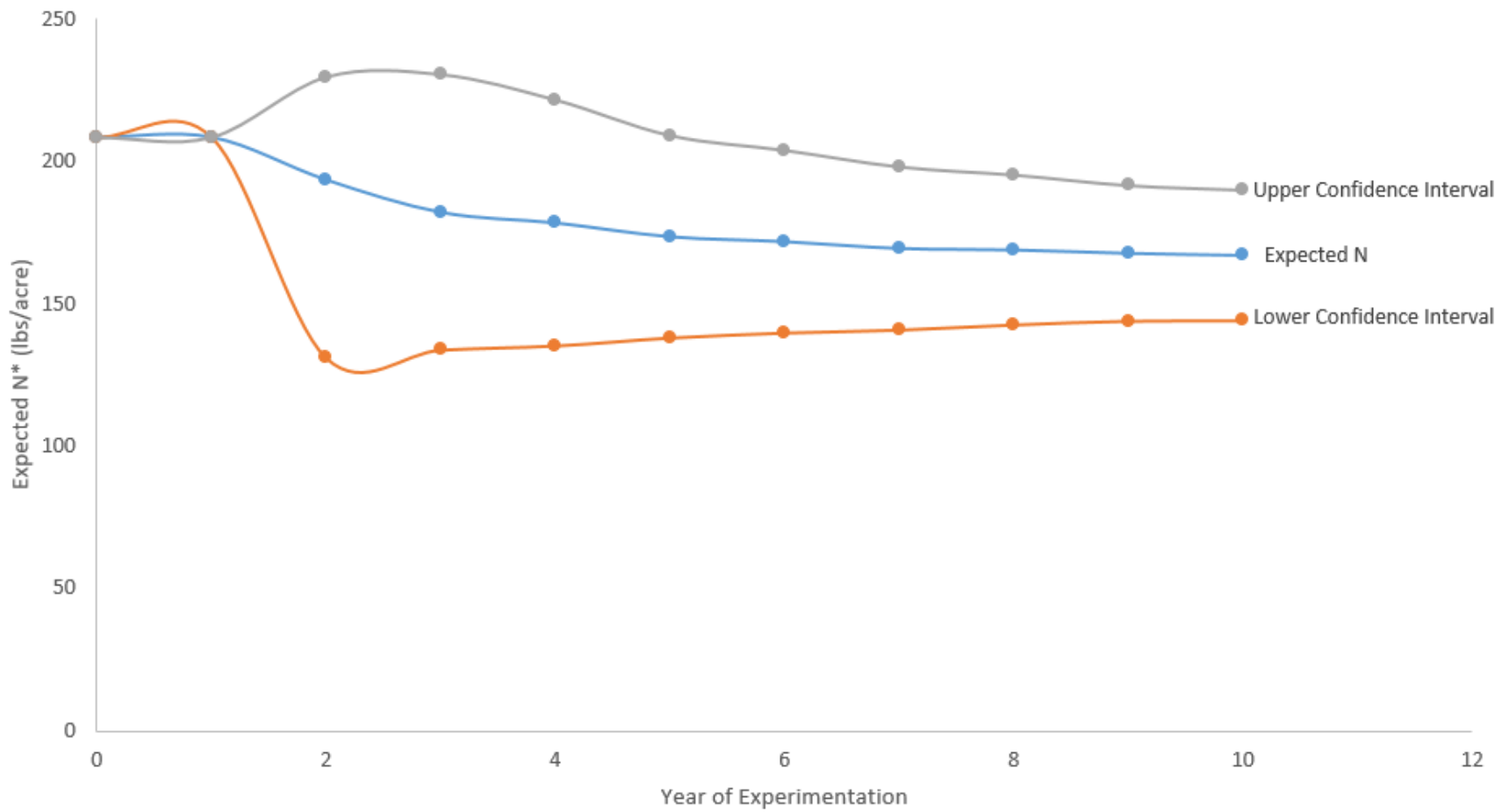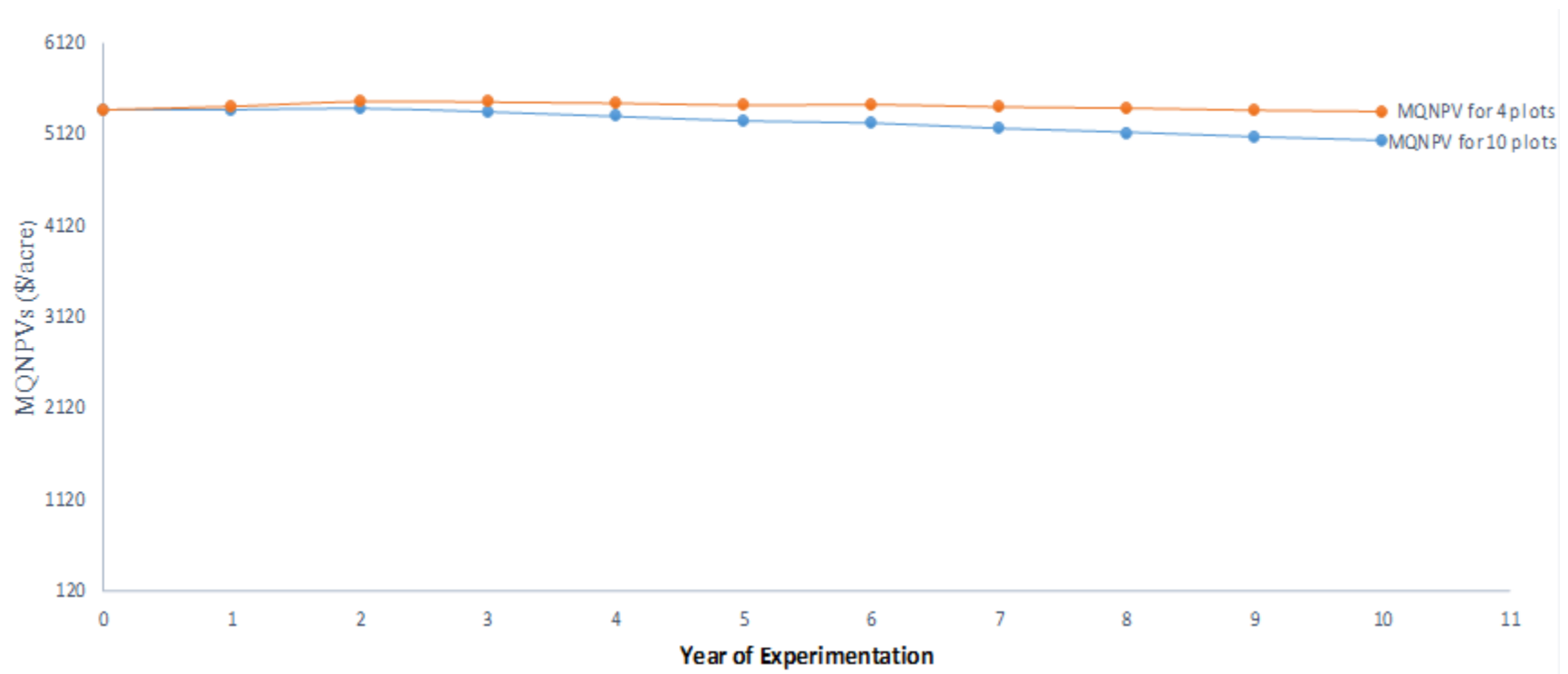
Figure 3.3: A Plot of Expected Profit Maximizing Levels of N in lb./acre in Each Year of Experimentation
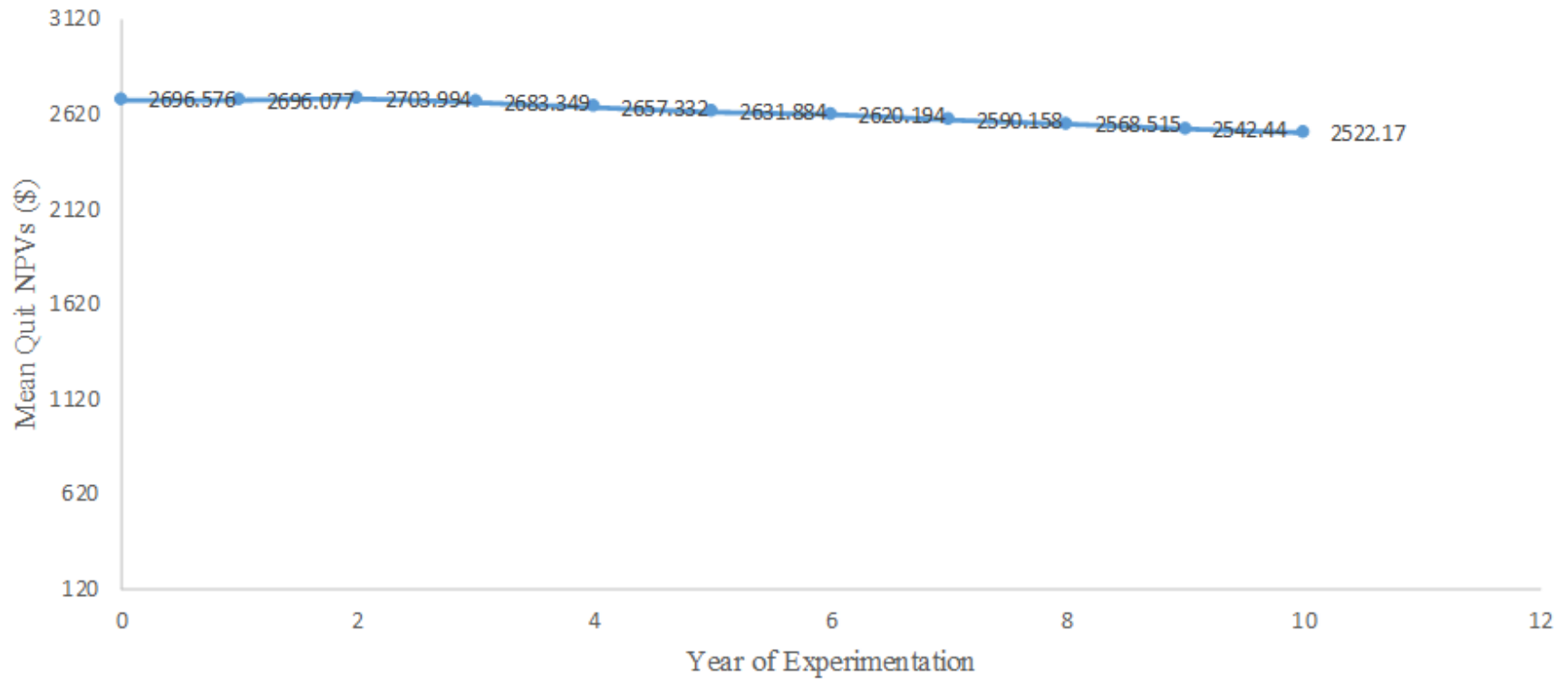
Figure 3.4: A Plot of Mean Quit NPV against Year of Experimentation
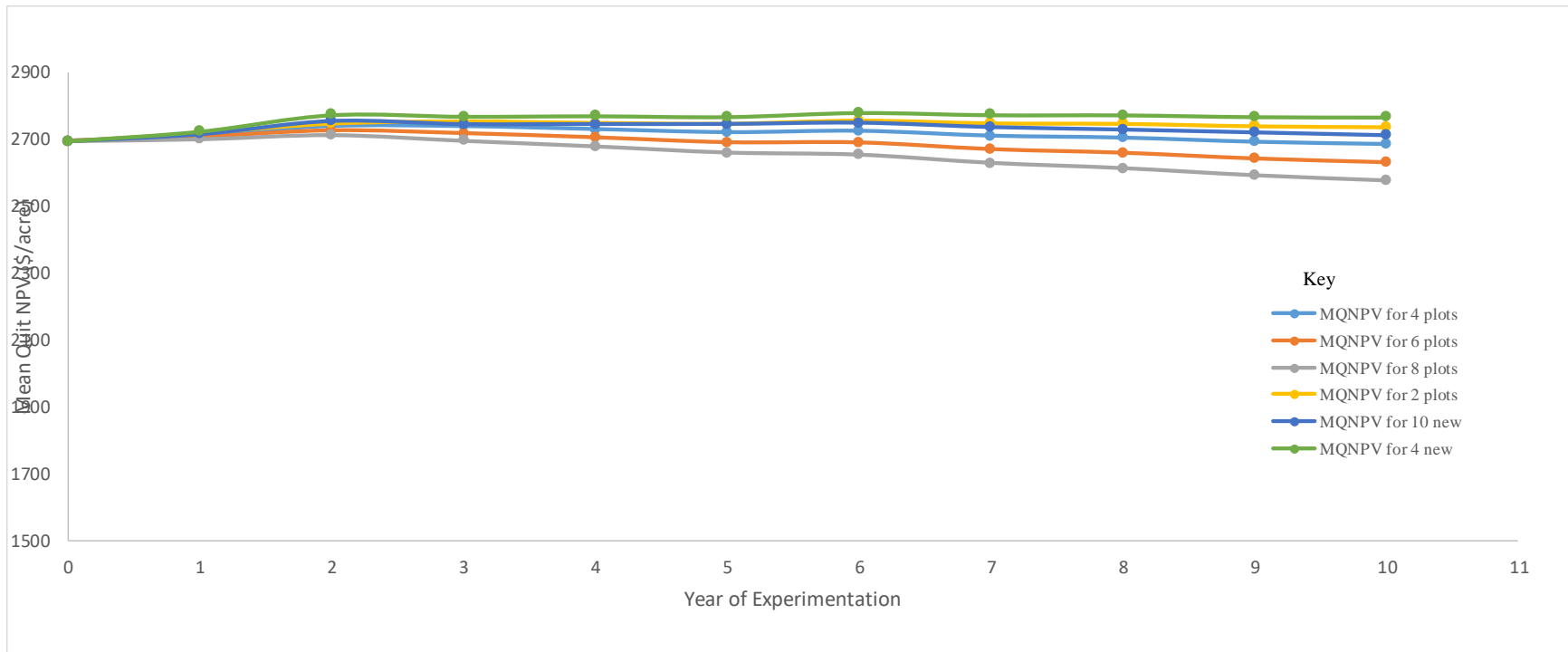
Figure 3.5: A Plot of Mean Quit NPV against Year of Experimentation for 2, 4, 6, 8 Plots, and 4 and 10 Plots with 0.5 N*, 0.75N*, and 1.25 N*.

Figure 3.6: A Plot of Mean Quit NPV against Year of Experimentation for 10 and 4 Plot Designs When Corn and N Prices Double.
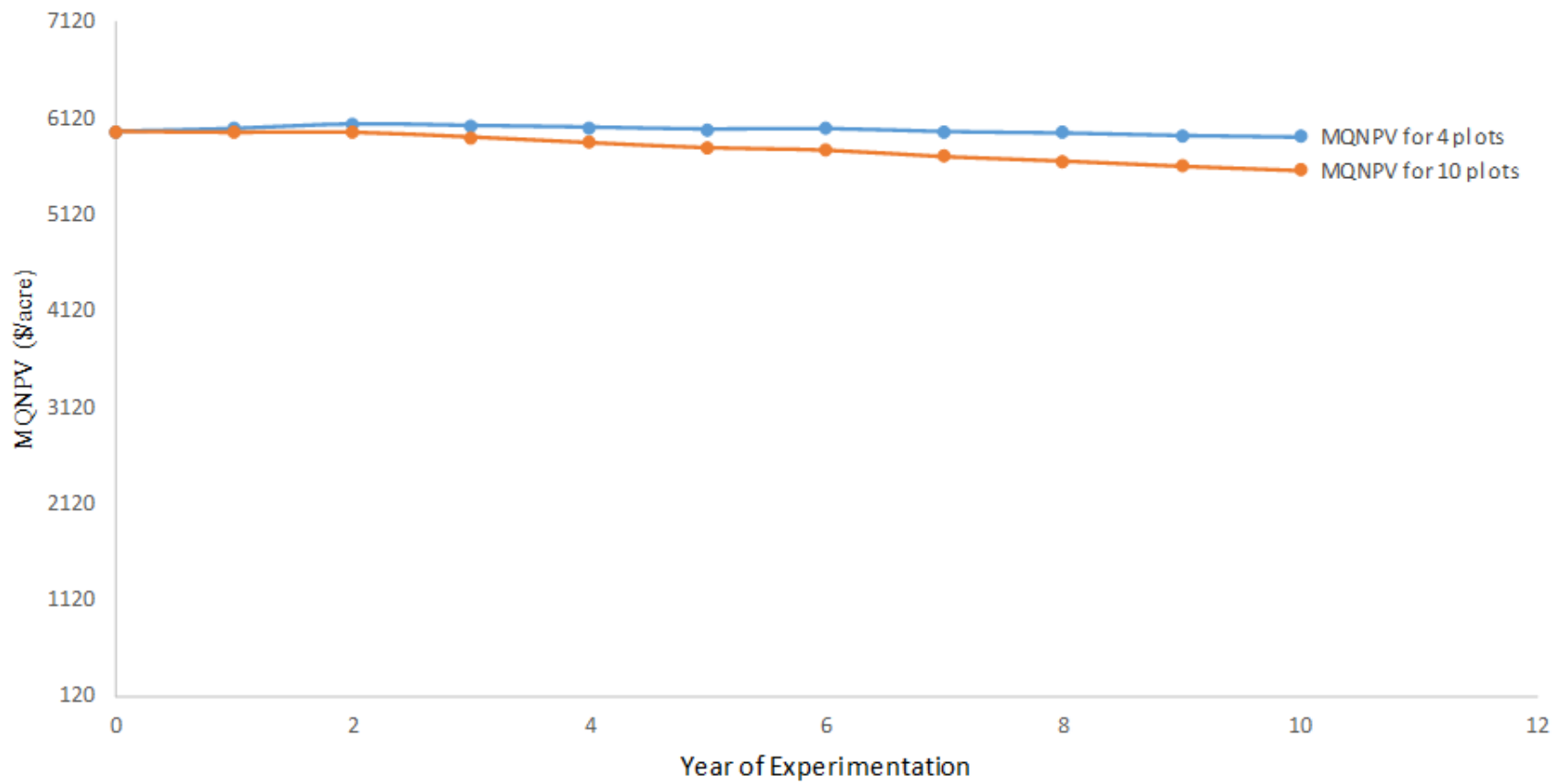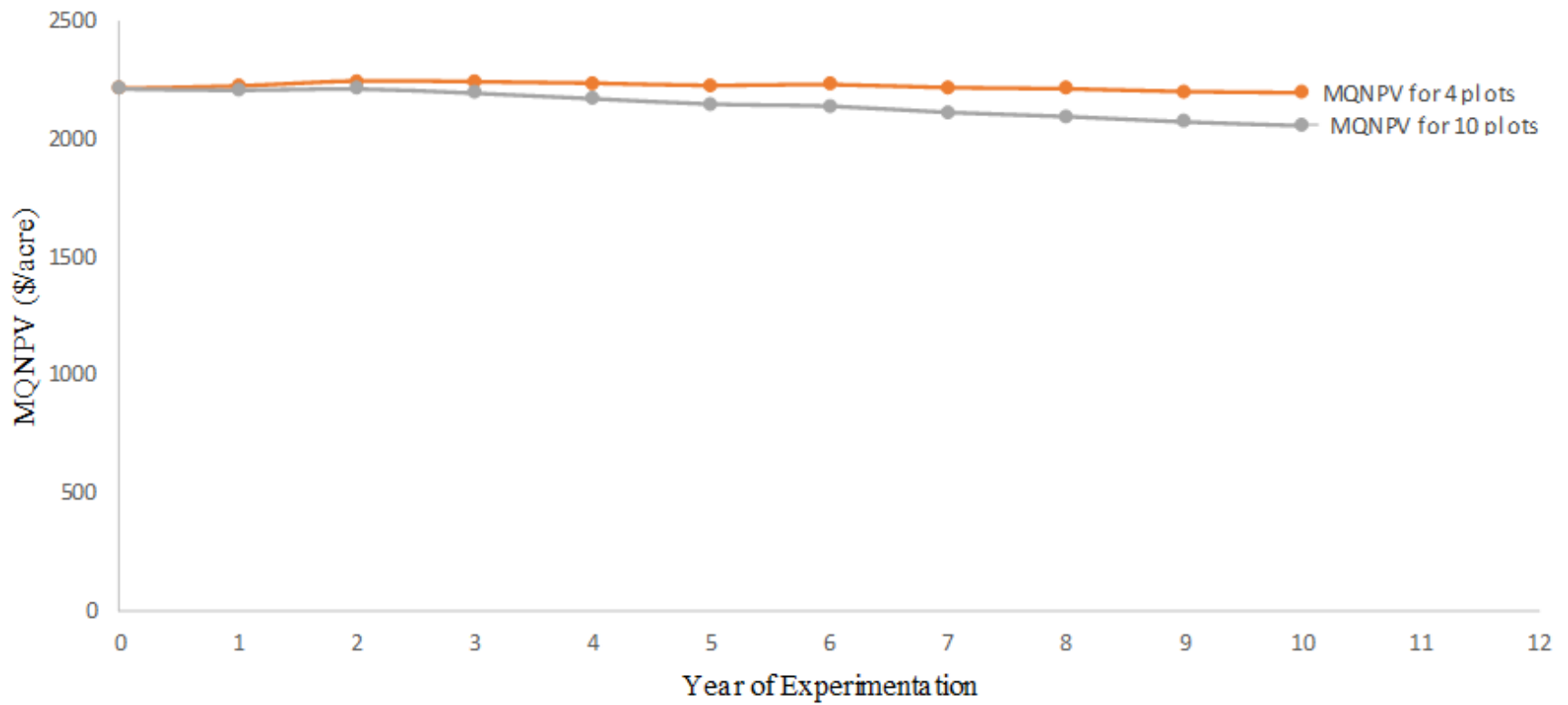
Figure 3.7: A Plot of Mean Quit NPV against Year of Experimentation for 12-Plot and 30-Plot Designs When Only Corn Price Doubles.

Figure 3.8: A Plot of Mean Quit NPV against Year of Experimentation for 12-Plot and 30-Plot Designs When Only N Price Doubles.

104

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