Published online May 15, 2018

On-Farm Replicated Strip Trials

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Chapter Purpose

On-farm research has gained popularity because it allows farmers to test different agronomic questions using their equipment and management practices on their own fields. Farmers working with scientists and agronomists can conduct on-farm replicated strip trials to evaluate different products, management practices, and technologies. This chapter provides a brief overview of how to plan, design, and conduct on-farm replicated strip trials. Practical considerations are listed when using different types of equipment. Examples are presented on how to summarize data from individual locations, as well as how to interpret experiment conducted. While some precision agriculture technologies will change and evolve in the future, the basic concepts of on-farm research will remain the same. The goal of this chapter is to provide future farmers, agronomists, agriculture industry professionals, and environmentalists or policymakers with the basic knowledge and tools required to conduct on-farm trials.

Precision Agriculture Technology and On-Farm Research

In the past, small plot field, greenhouse, and laboratory experiments were the primary methods for conducting agronomic research. These studies provided excellent information and in many situations the equipment the scientists used was very similar to equipment used on the farm. However, with time, field equipment and farms have expanded in size and capacity. Currently, many farmers own and operate combines that are equipped with yield monitors and a global positioning system (GPS). This equipment, allows farmers to implement precision farming practices and conduct on-farm research. On-farm research includes any experiments that farmers conduct to test new products, technologies, and management practices prior to wide-scale adoption on their farm (Fig. 13.1). In many fields, these treatments are applied in strips across the entire field. As with all experiments, they are most successful when they are replicated and based on carefully constructed questions or hypotheses.

Farmers generally conduct on-farm research in collaboration with researchers, local agronomists or crop consultants. While precision agriculture technologies enable farmers to conduct on-farm studies, not all are comfortable with the on-farm research process. On-farm trials often require additional planning and resources, as well as external help to analyze data. A current trend is to organize farmers into local groups or networks, which serve as platforms for on-farm participatory research and learning. On-farm research networks offer new ways to bring together science,

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doi:10.2134/precisionagbasics.2016.0096

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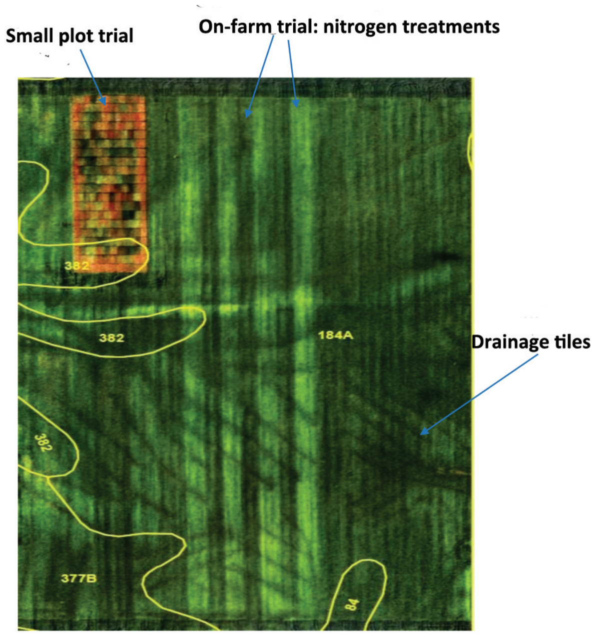


Fig. 13.1. Small-plot experiment and a replicated strip trial in the same field. A field with a small-plot experiment in the upper left corner and an on-farm replicated strip trial with two nitrogen fertilizer rate treatments in the center. The dark angled lines indicate the location of drainage tiles and the yellow lines indicate soil map units. The dark vertical strips are side-dressed applications. Notice that because of plot size, the drainage tiles may impact yield variability more in the small-plot than in the on-farm replicated strip trial with the field-length strips.

technology, and a farmers' own personal knowledge, ideas, and experiences. These networks help to enhance the understanding of how and where farm management improvements are possible.

Planning an Experiment


Courtesy of Precision Hawk

Video 13.1. How do farmers benefit from on-farm trials? http://bit.ly/on-farm-trial-benefits

Ask the Right Research Question

Successful experiments start by defining the problem and with a question. Asking the right question, however, usually requires doing some homework. In other words, start by learning what kind of research has already been completed. Past information can be obtained by interviewing experts or reading scientific papers and reports. By doing their "homework", farmers may be more likely to identify more valuable questions.

Keeping the research question simple is another key to success. Large, complex studies are not well suited for on-farm experiments. It is important to consider who is asking the question, who will benefit from the resulting knowledge, and how much work is required to conduct the research. Additional questions may include:

1. How many treatments and replications are required?
2. What is the land area available for the treatments?
3. What equipment is available and is the equipment size compatible?
4. What is the researcher and farmer time commitment?
5. Is there the need for additional resources to analyze data and summarize results?
6. What is the risk of yield loss in the experimental area?
7. Is there a willingness to accept inconveniences such as slower planting, spraying or harvesting?

Formulate a Research Hypothesis

A research *hypothesis* is a simple statement that captures what researchers and farmers plan to discover from their research. Two complementary hypotheses exist for research questions. The first is the *null hypothesis*, which usually states that no differences exist among treatments. The second is the *alternative hypothesis*, which contradicts the null hypothesis, stating that if the null hypothesis is rejected then the differences could be due to the treatment effect (Table 13.1).

Table 13.1. Examples of research questions and corresponding null and alternate hypotheses for two on-farm trials: i) with two seed treatments and ii) with two planting rates.

	Seed treatment example	Planting rate example
Research question	Is there yield difference between two seed treatments?	Does the increased seeding rate lead to higher yields compared with the farmer current seeding rate?
Null hypothesis	Both seed treatments have the same mean yields.	The mean yields from the high seeding rates and the farmer current seeding rate are the same.
Alternate hypothesis	One of the seed treatments has a statistically different yield.	The average yield associated with the higher seeding rates is statistically discernable from the farmer current seeding rate.

Error Control

The data collected in an on-farm trial is only a small sample, which is required to draw inferences for a larger population such as the entire farm, larger local area, or region. Yet, there is a chance that the sample statistics will not accurately capture the true conclusion. One of the main errors researchers deal with is a *false positive*. A false positive occurs when statistically significant differences are found among the treatments when, in fact, none exists (the null hypothesis is rejected when it should be accepted). The vigor of hypothesis testing is controlled by the probability of false positive, or α level, which is denoted by the Greek letter, α. The α level is often 5% in scientific papers (1 in 20 chance of rejecting the null hypothesis when it is true) and 10% in agronomic studies (1 in 10 chance of rejecting the null hypothesis when it is true). From the onset of the experiment, the researchers should determine the error level they are willing to accept during the hypothesis testing (Clay et al., 2017).

Selecting Treatments for a Field Experiment

A *treatment* is a variable of interest which is manipulated by the experimenter. Treatment selection usually follows logically from your hypothesis, previous knowledge or personal beliefs. For example, if you want to test whether an increase in the planting rate results in higher yields, the experiment may include two planting rates: a control and the adjusted planting rate. The control provides a reference to your standard practice. For example, if the farmer's typical or current corn seeding rate is 31,500 seeds per acre, the 31,500 seeds per acre might be the control compared to a higher seeding rate of 34,500 seeds per acre.

In addition to including a control treatment, there are several other practical considerations when selecting treatments. We will discuss those below, but in general, experiments need to be set up with the available equipment in mind. Other factors to consider are the land area designated for on-farm trials, as it may limit the number of treatments to be tested, and the cost and time available for on-farm research.

Identifying Variables to be Measured

Once the research question has been identified and the treatments selected, the most appropriate response variables must be chosen. The response variable is the available soil, crop or other variables that respond to the treatments. A response variable should be measured or collected if it is important for interpreting the results of the on-farm trial or

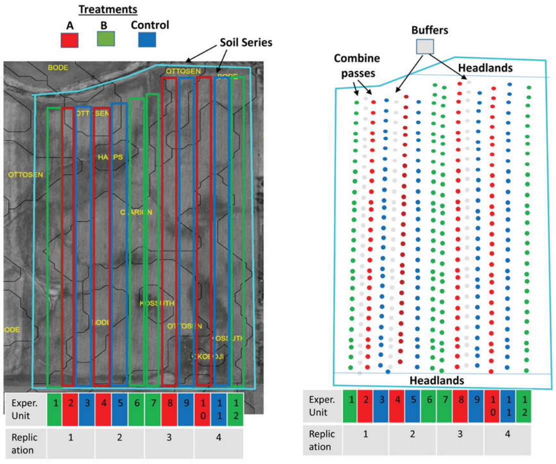


Fig. 13.2. A) Randomized block design with four replications. Three treatments (a, b, and c) arranged in a randomized complete (block) design with four replications in a typical rainfed field of Central Iowa. Soil series map is overlaid with the aerial imagery of the soil surface. Combine passes with individual yield monitor observations (b) corresponding to each treatment and buffer (light gray) between some of the treatments.

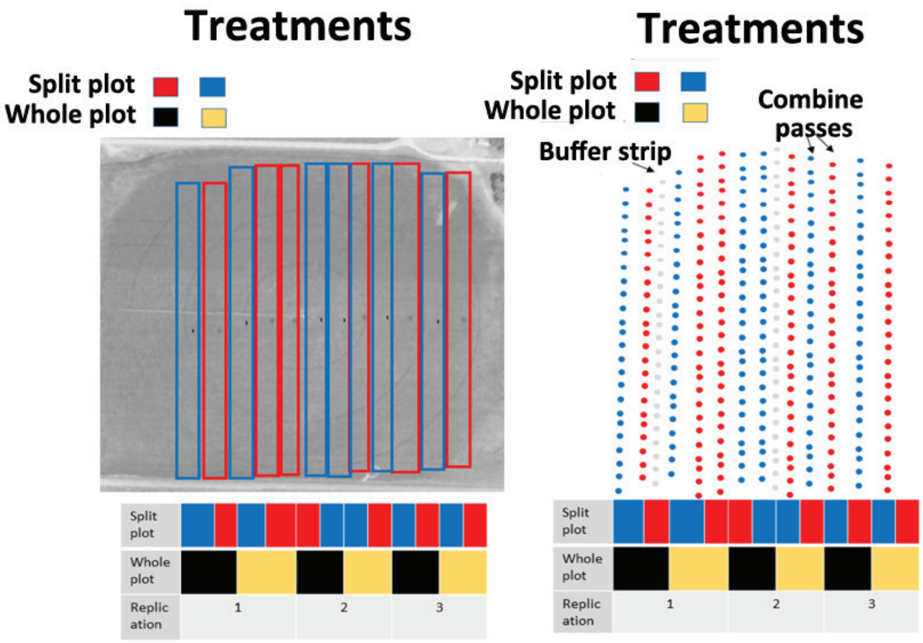


Fig. 13.3. Split plot design with 4 replications. Two factors with two treatment levels arranged in a split-plot design with four replications in a field irrigated by a center pivot (a). Example of the treatments for the whole plot can be tillage type (e.g., no till vs. conventional tillage) and for the split or subplot plot can be seed treatments or different fertilizer rates. The subplot treatments are nested within the whole plots. Treatments at the whole and subplot levels can be arranged in a randomized complete block design. Combine passes with individual yield monitor observations corresponding to each treatment (b) and buffers between some of the treatments.

if it improves the sensitivity of the analysis. Common response variables to measure for on-farm research are yield (bushel per acre), soil erosion (ton per acre per year), soil nutrient levels (ppm), disease level (e.g., % leaf coverage). A common error is to fail to collect data needed to test the hypothesis. As an example, it would be important to collect disease severity observations from a fungicide trial, as it may help explain the observed yield response to fungicide, or to collect soil nutrient information from a nitrogen fertilizer experiment. Some practical considerations for determining which variables to measure include: (i) the ease of measurement (time and cost considerations), (ii) the accuracy and precision needed for the measurement, and (iii) if the measurements should be repeated before, during, or after the growing season.

Develop a Robust Protocol

A protocol is a set of instructions, typically a one- to two-page document that is used to execute the experiment. Protocols should clearly state the objectives, treatments, data collection protocols, provide field maps, and state the expectations for farmers, researchers and technical providers. While developing the protocol, equipment restrictions or other considerations should be highlighted.

Setting Up On-farm Experiments

On-farm replicated strip trials are designed experiments that, when well executed, can be used to draw statistically valid cause and effect relationships between the treatments. *The treatments, which are often called factors,* may include different rates of fertilizers, fungicides, insecticides, herbicides, cover crop or tillage types. Categories of each factor are often called levels. For example, nutrients and fungicides can have several rates or doses (levels) while cover crop trials may have multiple crop species or mixtures. *The treatments can be applied at multiple scales, ranging from small plots to field-length strips. Experimental units* are the smallest individual plots or field-length strips that receive treatment applications independently of other plots or strips (Fig. 13.2A). Small plot and field-length strips are fundamentally different. In small plots, variability in soil properties within the plots is minimized, where in field length strips, it is not. In both methods, the plot dimensions are influenced by available equipment.

Treatment Design

Treatment design demonstrates how the treatments are assigned to various experimental units within a field. There are many ways to design on-farm research. In most methods, replications and randomization are critical. For statistical reasons, field plots should be as similar as possible and where possible, paired treatments should all have the same size and dimensions.

The most common experimental design for on-farm trials is the *randomized complete block design (RCBD)* (Fig. 13.2A). Blocks, which are often called replications, group all experimental units within a given area. It is assumed that within a block, variability is minimized. Blocking is a process of grouping experimental units within a field, often following a gradient or spatial trend in soil properties, previous history, or other characteristics. Blocking does not necessarily mean square treatment dimension, but instead how the treatments are strategically grouped within the trial area. All treatment and factor combinations should be present in each block. The purpose of blocking is to make valid comparisons between the treatments.

Split-plot is another experimental design used for on-farm trials. Unlike a randomized complete block design, the split-plot design has two types of treatments and experimental units that differ in size (Fig. 13.3A). For example, a trial may consist of large strips (whole plot) that have certain tillage treatments such as no till vs. strip till. These tillage treatments are split into smaller experimental units (subplots) where different crop genetics, herbicide rates, or nutrient rates can be applied. Split-plot design allows for smaller and fewer experimental units. At the whole-plot level, the treatment arrangement can be either with or without defined blocks. Randomization is recommended at the whole-plot and subplot levels.

Some on-farm strip trials are focused on evaluating different site-specific recommendations. Usually these trials have treatments that consider changing the application rate within experimental units. The common comparisons are variable rate versus a farmer's normal practice. The key is that the variable rate treatments vary based on soil, crop canopy, topography or previous management history.

Often, a *buffer* is between the plots. Buffers are areas where treatments are not applied to avoid cross-contamination between treatments (Fig. 13.2B and 13.3B). Cross-contamination can occur, for example, when treatments are assigned the highest nitrogen rates immediately next to the treatment with one of the lowest rates. This results in soil or plants from one plot impacting plants or soil in the next plot. Another example of cross-contamination could occur when pesticide applied to one treatment drifts onto an adjacent treatment. The problems with drift effects can be minimized by including buffer areas of untreated plants between the treatments.

There are other possible experimental designs that can be used for on-farm trials. It is important to use experimental and treatment designs that best fit the research objectives or hypotheses, and are practical for farmers to execute. There are three principles of a designed experiment: **replication**, **randomization**, and **local control.**

Video 13.2. How are on-farm trials conducted?
http://bit.ly/on-farm-trials

Replication

A replication is a physical repetition of experimental treatments within the same field. (Fig. 13.3A). Replications are needed to capture variation and conduct statistical analyses. Variability in field experiments is mostly due to systematic error, random error or random noise. Both are common because of spatial variability throughout the field, measurement errors, different environmental conditions, equipment issues, human error or inability to replicate the same treatments.

Farmers will often compare treatments by splitting a field into two parts, known as the "split-field", half-field design, or side-by-side method, wherein part of a field is one treatment and another part is a different treatment. Because these comparisons are conducted without replications, classical statistical techniques cannot be used to determine treatment differences. For example, due to spatial soil variability the side of the field receiving "Fungicide Treatment A" may be different than the side of the field receiving "Fungicide Treatment B." The resulting differences in yield may not be due to the fungicide treatments but rather due to different soil types. Replications minimize the effect of external factors that are not of interest in the study. It is highly recommended to replicate all treatments four or more times in each trial. Some types of trials require more than four replications to capture the entire field for spatial analysis of yield responses.

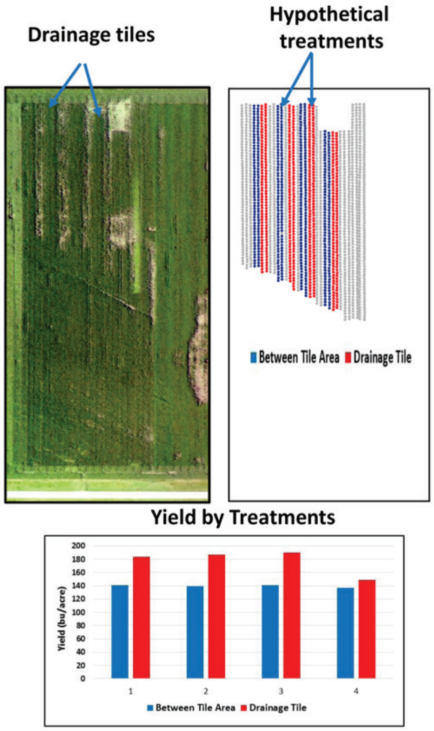


Fig. 13.4. Potential bias in estimated yield differences produced by the direction of drainage tiles that coincide with the treatment direction.

Fig. 13.5. A field-scale two-treatment on-farm replicated trial established in a corn field in eastern Iowa with large spatial variability in soil organic matter and sand content was used to test the effect of gypsum ($CaSO_4 \times H_2O$) on corn yield. The early July aerial imagery of the corn canopy showed a potential sulfur effect in the upper portion of the field within sandy soils.

Randomization

Randomization of treatments within a replication considers chance in area selection and helps to avoid bias when assigning treatments. An older way to randomize treatments was to flip a coin or draw treatment labels from a hat; today, randomization software can be used to assign treatments randomly.

Randomization is used to:

1. Minimize bias from unknown factors.
2. Help draw statistical inferences and utilize different statistical techniques.
3. Neutralize, balance, or disperse spatial variability.

Randomization helps to minimize bias from unknown factors that could affect yield or the response variables being measured. Also, randomization helps avoid bias from management practices other than treatments such as previous manure applications, previous field boundaries, extremely large within-field variability, non-uniform irrigation, residue distribution, pest pressure or tile drainage patterns (Fig. 13.4). A farmer's personal knowledge of within-field variability is often just as important as random treatment assignment.

Randomization also helps researchers draw statistical inferences from the data using a wide range of statistical methods. Although, more complex statistical methods such as spatial analysis, among others, do not require treatment randomization.

Randomization essentially seeks to neutralize, balance, or disperse the effect of spatial variability across the trial. However, a common objective of some trials is to quantify the effect of spatial variability on yield or to evaluate or develop site-specific recommendations.

It is critical to control all factors that might affect the experiment except the treatments that are being studied. Like replication and randomization, local control also minimizes the experimental error.

Methods of Selecting Fields and Locations within Fields

Field selection for on-farm trials depends on the product or practice being tested. Some trials are targeted to specific geographical locations or field areas that have certain characteristics. For example, some experiments require areas of low soil pH and/or low soil organic matter (Fig. 13.5 A.), whereas other experiments require a specific disease history. There are many different resources besides field history such as soil survey and satellite imagery, that can help farmers decide which fields or portion of fields are best suited for on-farm research. These include county soil surveys as well as past and current aerial or satellite images (Table 13.2). Experimental errors can be reduced by selecting areas with similar characteristics. A soil map unit is the basis for the soil map. Each map unit has a unique symbol or letters. These letters have different meaning. For example BaA may means that that the dominant soil is a Beltsville silt loam (Ba) with a slope between 0 and 2% (A), whereas a BaB may mean that the dominant soil is a Beltsville silt loam (Ba) with a slope between 2 and 5% (B)(Brewer, 2011).

Self-generated or purchased information can also help with site selection. This can include historical yield monitor data, in-season aerial or satellite images, crop canopy reflectance maps, soil testing and/or soil fertility maps, soil electrical conductivity (EC) information, and scouting reports for weeds, insects or diseases. Farmers can also provide information about the location of manure piles, manure storage, or previous animal confinement areas. These areas should be avoided, for example, in future on-farm trials with phosphorous.

In addition, parts of fields in which on-farm trials were conducted in the past should be avoided for future on-farm trials.

Tools to Collect Data and Interpret Results

Once on-farm trials have been established, data need to be collected (Table 13.3). The most common information collected are crop yields, crop quality measures, plant stand counts, disease ratings, or crop canopy reflectance. In many experiments, qualitative and quantitative data such as seeding rate, emergence rate, or soil nutrient levels are

Table 13.2. Publicly available resources to aid in site selection for on-farm research.

Measurement	Webpage	Web address
County soil surveys	Geospatial Data Gateway- NRCS	https://gdg.sc.egov.usda.gov
	Web soil survey	http://websoilsurvey.sc.egov.usda.gov/App/WebSoilSurvey.aspx
Aerial or satellite imagery	Google Earth	https://earth.google.com
	National Agriculture Imagery Program	gis.apfo.usda.gov/ArcGIS/rest/services/NAIP
Topography	Digital elevation model (DEM)	https://lta.cr.usgs.gov/LIDAR

Table 13.3. Common variables and tools or techniques required to collect data for on-farm trials.

Variable	Tools required
Yield	Yield monitor or weigh wagon, grain moisture analyzer
Crop characteristics (e.g., stand counts, growth stages, biomass, plant height)	Field guide, tape measure, shovel for digging plant roots, visual observations, photos
Crop canopy spectral properties	Canopy sensors, chlorophyll meters, aerial imagery
Grain quality	Visual assessment, grain moisture analyzer, lab analysis
Soil test values	Hand and hydraulic soil probes and lab analysis
Disease levels	Field guide, visual assessment, lab analysis for nematodes and difficult to identify diseases
Insect levels	Field guide, visual assessment, sweep net, sticky cards, pheromone or pit traps, sheet
Weed types and counts	Field guide, visual assessment

Table 13.4. Tools to collect information on external factors that may affect an experiment.

Factor	Tool
Climate (temperature, growing degree days, relative humidity, rainfall, soil moisture, wind speed and direction, leaf wetness, hail, etc.) Weather extremes (flood, drought, frost, excessive heat, hail)	Field-specific weather stations, rain gauges Local weather reports State-wide reports (e.g. Mesonet or Climate Corp) Weather companies (e.g., SkyBit)
Soil and topography (pH, ponding, nitrate and phosphorous concentration, erosion)	Hand probe (soil samples for nutrient analysis and compaction) Sensors (pH, EC, specific ion, temperature, moisture) Lidar data, topographical maps Visual assessment
Current and historical field management (planting and harvest dates, variety/hybrid, fertilization rates, manure history, disease and pest history, crop rotation.	Paper forms On-line data collection tools and forms Historical records Personal communication
Edge of field water (tile drain flow, sediment)	Water sample from tile drainage outlets Visual assessment of sediments

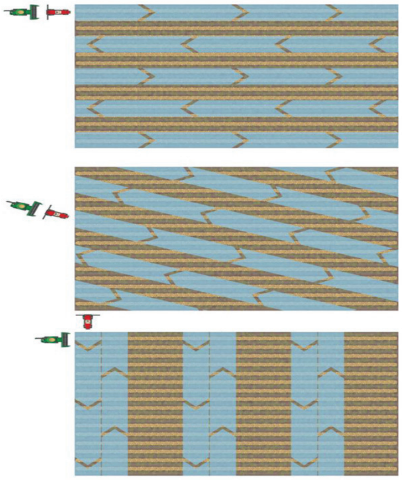


Fig. 13.6. Patterns of treatment applications using ground sprayers and harvesting the treatment strips using a grain combine. The upper figure shows that applications and harvest are done with crop rows; the middle figure, applications and harvest are done at an angle; the lower figure, applications are done across the rows and harvest is done with the rows.

collected to better explain the results. For example, corn may respond differently at 30,000 plants per acre than 40,000 plants per acre (Table 13.4).

Some factors can be controlled and some cannot be controlled. For those that can be controlled, create strategies to ensure they do not limit yield or quality. For example, if the study is focused on planting populations of soybean and an outbreak of aphids infests the field, the entire field should be sprayed with insecticides to minimize the impacts of the aphids on yield.

Computers and Telematics

Farmers should have access to a computer with an Internet connection, software to view spatial data and generate simple data summaries, and a monitor with a GPS receiver on field machinery. To speed up data collection, on-line data collection tools, wireless data transfer or telematics technologies can be adopted.

Equipment for Conducting Experiments

Planters and Grain Drills

Planters equipped with variable-rate drives, hydraulic downforce, insecticide delivery system, in-furrow liquid applicators, or other technology are well suited for experiments that include multiple seeding rates, seed treatments, row spacings, variety or hybrid comparisons, and in-furrow treatments.

Planters with individual row seed boxes, two bulk tanks, or section shutoffs for in-furrow applications are used to establish *split-planter trials.* Split-planter trials are when a farmer sets up treatment comparisons with different sections of the planter. The most common split-planter trials consist of only two treatments, but three treatments can be applied in one planter pass as well. A split-planter trial is considered one of the easiest trials to implement because once the planter is loaded, treatments can be placed across the entire field.

Prescription planting trials can be implemented if a planter has electric- or hydraulic-drive capabilities. A treatment prescription can be loaded into the tractor monitor to control planting rates or activate application of products for various treatments. The prescription is communicated directly to the planter from the monitor, eliminating the need for farmers to manually adjust seeding rates or other treatments.

Farmers can also establish trials by planting alternate passes with one treatment and returning to plant the skipped passes with a second treatment, manually turning the in-furrow applicator on or off every other pass, or manually adjusting seeding rates.

Each crop has different challenges associated with setting up trials using planters. The most common challenge is the harvest equipment not matching the width of the treatments.

Ground Sprayers

Sprayers are used to apply a variety of products to crops including foliar fungicides, insecticides, herbicides, biologicals, micro- and macronutrients. With some modifications, sprayers can also be used to seed cover crops. Similar to planter trials, sprayer trials are generally easy to execute. A common application method is to spray with or along the rows. For example, a recommendation for farmers is to apply an insecticide treatment to a 12-row strip alongside an untreated 12-row strip. Another recommendation for farmers who prefer to harvest narrowly-planted crops at a slight angle is to apply the treatment in wider swaths to ensure that enough data can be collected from the center of the wider swath (Fig. 13.6 A).

There are several issues concerning applications of chemicals or products using sprayers. Obtaining and following the specified protocol and labeled rate is very important. For example, many products have a suggested or required growth stage for application. Incorrect application timing may result in lack of treatment effect, a potential yield loss, or it may be even illegal. Combining multiple products can save cost and time; however, it is important to ensure the products being mixed are compatible and do not have a negative effect on the crops. Be sure to include the proper control comparison when evaluating combined products. Sometimes it may be appropriate to compare Products A + B to Product A alone, instead of or in addition to having a true untreated control.

Aerial Applicators

Airplanes and helicopters are used to apply many of the same products as ground sprayers. Aerial applications allow the testing of products when ground applicators are not appropriate. Treatments applied with fixed-wing airplanes should be wider than one pass across the field to ensure uniform treatment coverage. This is because the airplane spray delivery system is specifically calibrated to overlap one pass with another. Also, multiple airplane passes will ensure that the treatment is wider than a full combine pass. While providing similar benefits as fixed-wing airplane, helicopters can apply treatments more accurately within fields, because they can maneuver better around trees, power lines and other obstructions. Helicopters have a relatively limited payload capacity but can reload and refuel at the edge of the field.

Trial treatments that are applied with an aerial applicator instead of with a ground sprayer or high-clearance applicator may be more difficult to apply accurately. In addition, variable winds and applicator speeds can create challenges when applying treatments. For example, if a product is applied on a windy day, the treatments may not be accurately placed (most pesticide labels specify spraying in wind conditions of 10 or 15 mph or less). To account for potential drift, wider strip swaths should be considered. To avoid problems, it is important to communicate clearly with the applicator about the plot plan.

Depending on field layout and surrounding obstacles (e.g., wind turbines, trees, cell phone towers), aerial applicators may not fly at a constant altitude and apply a uniform rate across the entire field. If using a fixed wing aerial applicator, select fields with fewer obstructions and long, straight rows to minimize variability. Many aerial applicators use light bar technology, but do not necessarily record their application data. To ensure that the application map data are collected and available, check with the applicator prior to spraying.

Nutrient Applicators

Different types of nutrient applicators are used in trials designed to compare fertilizer rates, forms, timings, and dates. These trials are implemented using fertilizer carts, floaters, manure applicators, toolbars, or in-season high clearance sprayers. In these experiments, it is preferable to use GPS and flow-meter capable equipment.

All equipment require calibration. For trials with dry and liquid fertilizers, wind speed can create challenges when applying replicated strips. Try to apply lime or dry fertilizers on a less windy day, or create wider strip swaths to account for potential drift.

Spinner spreaders are often designed to have overlapping swaths. If a single pass is used for a treatment, the edges of the swath may receive lower fertilizer rates than the center of the swath. Proper calibration should reduce inconsistent product distribution with spinner application systems.

Tillage Implements

Tillage studies can span multiple years. Tillage treatments are usually conducted in the fall or spring; therefore, the time window to establish tillage trials is usually wider than that for other types of trials. Tillage passes should go with the rows, if possible, to ensure one or multiple combine swathes. If the field is tilled at an angle, experimental units for tillage treatments should be wide enough to collect yield data.

Comparing two different tillage systems, such as vertical tillage and deep tillage, requires different equipment. Implement width, tillage depth, machine compaction, and other factors need to be considered to reduce or eliminate potential errors. In these experiments, it is important to ensure that the implement widths are wide enough to allow full planting and harvest passes.

In addition, different tillage methods may require adjustments to the planter to properly manage residue, soil penetration, seed-to-soil contact, and closing the trench. Appropriate coulters or row cleaners should be used for different soil conditions within different tillage treatments. New tillage equipment should be tested and coulters and/or cleaners adjusted prior to use. Sufficient weight and ground contact must remain on the gauge wheels to ensure good seed-to soil contact for an even plant stand.

Crop Canopy Sensors

Testing crop canopy sensors usually requires establishing a reference, calibration or nitrogen rich strips with slightly above-optimal nitrogen status within a field. Reference strips are generally applied before planting to allow the crop to develop the canopy reflectance patterns specific for each variety or hybrid. The calibration strips are sensed prior to variable-rate applications.

When conducting trials with crop sensors or testing other variable-rate prescriptions, it is important to accurately record the rate of nutrients being applied.

Irrigation

Common irrigation systems are center pivot (sprinkler systems), furrow and subsurface irrigation. Water regime treatments in on-farm trials with furrow and subsurface irrigation are easier to establish than with a center pivot irrigation. If on-farm trials include irrigation treatments, the irrigation schedule should be jointly developed with the farmer.

If a trial does not include irrigation treatments, the optimal amount and uniform distribution of water is paramount because extremes below or above the optimal water amount will impact the treatment results. Excessive irrigation may lead to nitrogen loss or deficiency of other nutrients, while applying too little water may reduce the yield potential and increase water stress. Uncertain water supply or problems with the water supply from the irrigation system during periods of extreme droughts can impact yields as well.

The quality of irrigation water (e.g., nitrate content, salinity, etc.) should be measured in on-farm trials testing nutrients or animal manure sources. In addition, accurate records of rainfall and soil moisture are important in all experiments.

Harvest Equipment

Yield data should be collected with a properly calibrated yield monitor. Weigh wagons can also be used to collect yield data but then spatial data will not be collected.

Farmers should have a harvest plan for each trial before harvesting the field. It is important that the entire trial is harvested on the same day, with the same combine to avoid calibration differences. The combine header width should line up with each treatment to have full, or complete, harvest passes.

Harvesting some crops with a combine platform at an angle can minimize wear on the head. Harvesting at too much of an angle, or harvesting through narrow trial replications can result in the loss of yield data. It is important to harvest treatments with the rows. If this is not possible, planning and establishing wider treatments, along with minimizing the harvest angle, can help negate problems.

Quality Control

When conducting on-farm research, three goals are to collect accurate information, archive the data for future use, and to convert the information into better decisions.

Aerial imagery can be used to identify anomalies such as where water is ponding, hybrid or variety changes, applications that do not match the protocol, and nitrogen skips, as well as identify other management or equipment issues that may have affected some treatment areas but not others. If aerial photos suggest that yield data from one treatment was affected by an external that should be removed from the data set.

Visual observations while scouting during the season can also be used for quality control purposes. Many on-farm protocols require additional trips through the field to measure plant stand counts, disease or insect levels, weed pressure or plant root development. Take advantage of scouting trips to identify any possible problems.

As-applied or as-planted data for each trial recorded with a GPS-enabled monitor can be overlaid with yield data and aerial imagery using GIS software. A clean yield harvest pass is a pass within one treatment, with the same hybrid or variety, harvested on the same day and with all other factors, except the treatment being studied, kept constant. Yield observations for the headlands and approximately the first 50 feet of each pass are removed to adjust for the combine grain flow delay.

Yield data collected with a combine equipped with a GPS-enabled yield monitor contain several other attributes that are crucial in the quality control process. For example, combine speed, grain moisture and GPS time can be used in data cleaning.

Remove Outliers

Outliers, or extreme yield points that fall below or above specific thresholds should be removed to reduce bias and errors. When grain reaches a flow sensor, the initial impact often causes the yield monitor to register very high yield values. Also, when the combine header is left running while not actively harvesting, the yield monitor will report zero yield. Speeding up or slowing down also impacts yield monitor measurements. These extremely high or low yield values can be removed during the yield cleaning process. Additional information on yield monitors is available in Chapter 5 (Fulton and Port, 2018).

Harvest date: Yield monitors collect harvest dates as well. The entire trial should be harvested on the same day to maintain minimal differences in grain moisture and yield monitor calibration settings. If it is not possible to harvest all replications on the same day, care should be taken to harvest complete replications on one day and the remaining replications the next day.

Grain moisture: Detecting drastic changes in grain moisture (e.g., 2% or more) is an important part of the quality control process. If grain moisture varies substantially from the calibration level, the reported yield values may be higher or lower than actual levels.

Combine speed: During harvest, consistent combine speed is essential because drastic speed changes or deviation from the yield monitor calibration speed affects yield, and therefore treatment yield differences. Treatments must be harvested at the same or at similar speed.

Data Analyses and Result Interpretations

While there are many different tools and methods to conduct statistical analyses and summarize information, data analysis can be a daunting task. Farmers, consultants, and even scientists alike are often frustrated by this process. Additional information on data management is available in Chapter 12 (Fulton and Port, 2018).

The goal of data analysis is to separate the signal from the noise. The *signal* is what we try to identify based on the research hypothesis and the *noise* is mostly random variation or other unidentified error sources. Another objective of data analysis is to draw inference from the observed data. The *target population* may include all possible fields or conditions for a specific geographic area or specific management practices. Key descriptive statistics for summarizing observation from data samples are mean (averages), standard deviation (spread of the data), median (midpoint of data distribution), minimum and maximum values, and range.

After outliers and errors are cleaned from the data, the first step is to verify whether the response variable is continuous or categorical. Continuous variables are numeric with an infinite number of

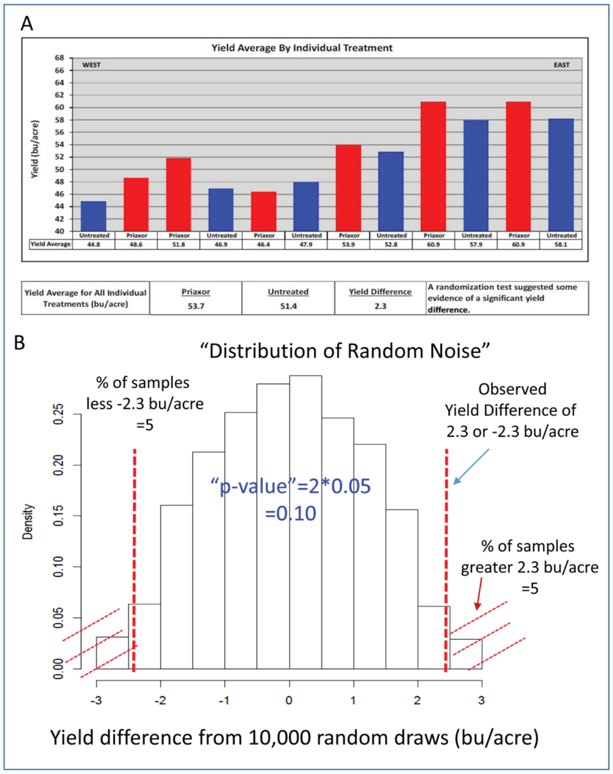


Fig. 13.7. Example of detecting nonsignificant yield difference ($\alpha = 0.10$) between two treatments using a randomization test. The p-value is the probability of a yield difference as extreme as the observed yield difference (i.e., plus or minus 3.1 bushels per acre) given that the null hypothesis is true. The randomization or distribution of random noise shows potential yield differences when the null hypothesis is true; yield differences are likely due to random chance.

values while categorical variables contain a finite number of distinct groups, for example, treated vs. untreated. The type of response variable will help to choose the appropriate data analysis technique.

The second step is to check the data distribution. This can be done by calculating the skewness or symmetry of distribution and kurtosis values or measure whether data have heavy tales or light tails relative to normal distribution. Plotting histograms (using a spreadsheet) or box plots (using statistical software packages) of the variables of interest will indicate whether data are normally distributed with a symmetrical bell shaped curve. Most of the yield data does not fall perfectly within a normally distributed bell shaped curve. Data transformation or selecting other distributions should be considered if data are not normally distributed, which is common for data from fields with large spatial variability. Other distributions should be used for count data (nonnegative integers), categorical or binary data, maximum or minimum values, and for bounded data such as ratios and percentages.

When analyzing data, it is best to use an appropriate statistical package such as SAS, R, or JMP. Please consult professionals for writing the appropriate codes and extracting relevant output statistics.

Two Treatment Comparisons

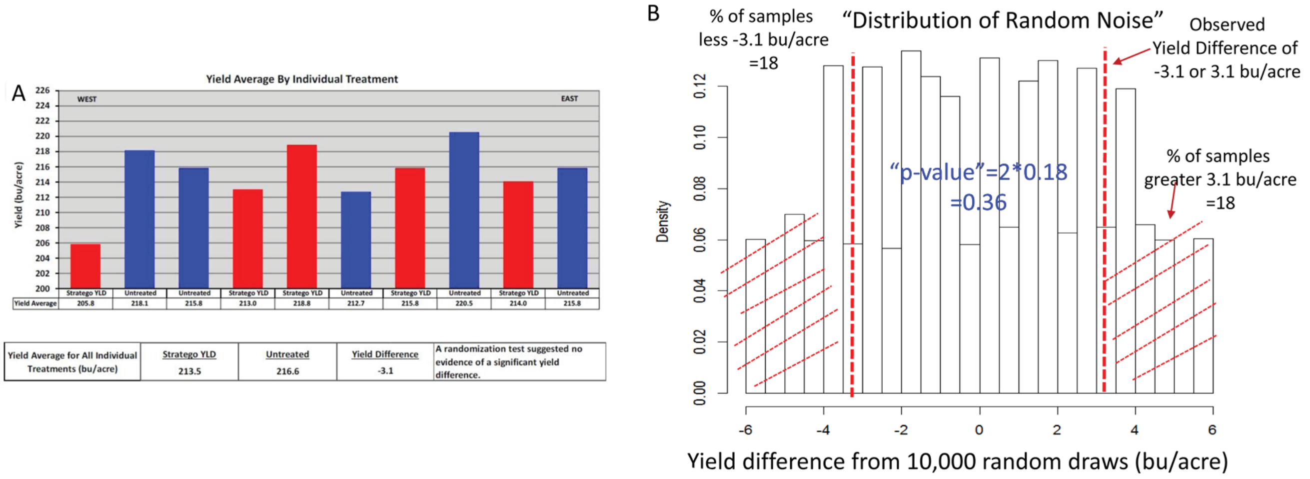
When analyzing experiments with two treatments, the observed differences between two treatments should be compared with differences likely produced by random chance. This distribution is called the random noise distribution and shows what would happen if the null hypothesis was true. The *p*-values are used to provide the evidence needed to reject or accept the null hypothesis.

The *p*-value, a number between 0 and 1, indicates the probability of a statistical difference between the treatments. If *p*-value is extremely small, then the difference was likely caused by the treatments. The probability of a significant difference decreases with increasing *p*-values.

The meaning of the *p*-value must be interpreted with reference to the sample size. A general classification of *p*-values in terms of the evidence of statistically discernible yield difference includes: > 0.10, no evidence of significant yield difference; 0.05 to 0.10, some evidence of significant yield difference;< 0.05, strong evidence of significant yield difference.

With a given amount within-field variability, increasing the number of replication improves the ability to detect differences. A nonsignificant effect has two potential meanings: either there was no treatment effect or the effect was undetectable due to the relatively large variability or too few replications.

No Significance Difference Example (Fig. 13.8): A foliar fungicide trial with six replications compared a fungicide (red) and with an untreated control (blue). The null hypothesis stated that the yields for both treatments were statistically similar, or that the yield difference between the two treatments equals zero. The average yield response from the fungicide was 3.1 bushels per acre. The percentage of samples with less than and more than 3.1 bushels per acre yield difference in the distribution of random noise indicates that the calculated p-value was 0.36. This means that it is



	Stratego YLD	Untreated	Untreated	Stratego YLD	Stratego YLD	Untreated	Stratego YLD	Untreated	Stratego YLD	Untreated
Yield Average	205.8	218.1	215.8	213.0	218.8	212.7	215.8	220.5	214.0	215.8

Yield Average for All Individual Treatments (bu/acre)	Stratego YLD	Untreated	Yield Difference	A randomization test suggested no evidence of a significant yield difference.
	213.5	216.6	-3.1	

Fig. 13.8. Example of detecting significant yield difference ($\alpha = 0.10$) between two treatments using a randomization test. The *p*-value is the probability of a yield difference as extreme as the observed yield difference (i.e., plus or minus of 2.3 bushels per acre) given that the Null hypothesis is true. The randomization or distribution of random noise shows potential yield differences when the null hypothesis is true: yield differences are likely due to random chance.

unlikely that the two treatments were different. A p-value greater than 0.10 means that yield response was not statistically significant, so the null hypothesis would not be rejected.

Significant Difference Example (Fig. 13.7): For this on-farm trial, a foliar fungicide was compared with the untreated control. The null hypothesis was that yields for both treatments were similar. In this example, the observed yield response was 2.3 bushels per acre.

The percentage of samples with less than and more than 2.3 bushels per acre in the distribution of random noise is 10%, so the calculated *p*-value is 0.10. Although the yield difference was less than the 3.1 bushels per acre in Example 1. In this case, the null hypothesis was rejected because the difference between the two treatments was statistically significant.

Paired *t* test for Two-Treatment Trials

An alternative for the randomization test is a paired *t* test, which can be calculated in Microsoft Excel. Paired *t* tests are based on the same statistical logic as the randomization test above.

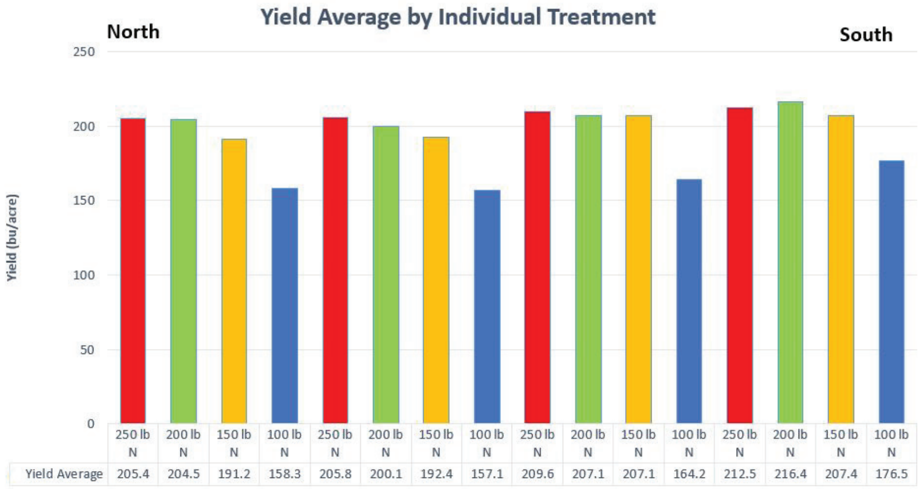
Multiple Treatment Comparisons

The randomization test or paired *t* test are more difficult to use when on-farm trials have more than two treatments. In this case, a multiple treatment comparison such as the least significant difference (LSD) test can be used. The LSD test is one of the most commonly used test statistics in agronomic studies. The basic idea is to generate a number that will indicate whether the treatment difference meets a threshold of significant difference or not. When displaying these differences, a value called a LSD can be provided, or two means will have different letters next to the value such as the letters "a", "b", "c" next to a data value. If the difference between the two means is greater than the LSD value, then the two means are statistically different.

A word of caution with the LSD test is that it should only be used when at least one pair of treatments is significantly different. Otherwise, the test may claim significant yield differences when none are present. There are many tests like LSD; each can potentially produce different statistical inferences.

Multiple Treatment Comparison Example (Fig. 13.9): Four rates of nitrogen- 100, 150, 200, and 250 lb nitrogen per acre are compared in this on-farm trial. By calculating the LSD values at 10% significance level ($\alpha = 0.10$), yield differences among the three lowest nitrogen rates (all three of the 100, 150, and 200 lb nitrogen per acre have different letters such as "a", "b", and "c") are statistically discernable while the yield difference between the two highest nitrogen rates (200 and 250 lb nitrogen per acre) are not statistically discernable.

If statistical analyses are conducted for individual trials, it is important to show not only *p*-values for the statistical tests but also the effect size (average yield differences, treatment means), confidence intervals for the means and within- and across-treatment variability values.



Treatment	Yield
lb N/acre	bu/acre
100	164
150	200
200	207
250	208
lsd 0.05	5.2

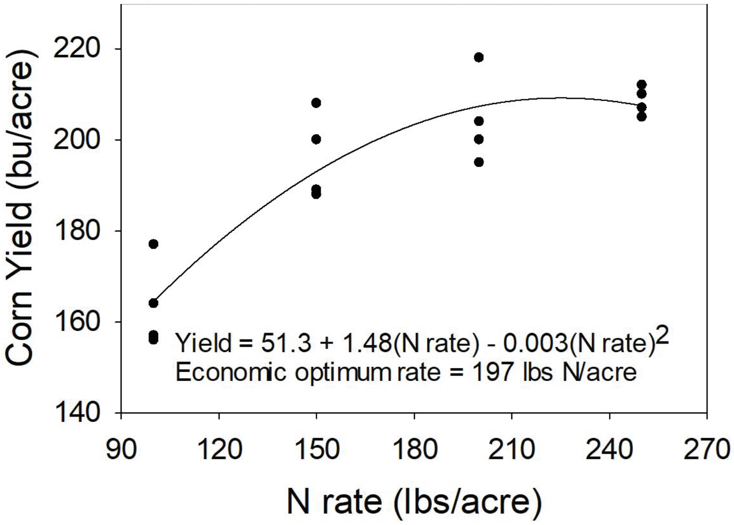


Fig. 13.9. Example using the least significant difference (LSD) test for identifying significant yield differences between treatments with four nitrogen rates and estimating economic optimal rate (EOR) of nitrogen fertilization and its confidence interval for the quadratic response function (data provided by Indiana InField Advantage in 2013).

Analysis of Multilocation Trials

While analyses are often focused on individual on-farm trials, a greater benefit is gained from analyses of data from multiple locations. Similar to individual trials, observations from multilocation trials can be analyzed using a randomized complete block design (RCBD) or split-plot design. More information is available earlier in this chapter.

During the analysis of multilocation trials, it is necessary to define the population or area of inference and identify whether the factors tested should be treated as *fixed* or *random* effects. The fixed effects are those where the treatments are fixed (for example, rates of lime application, rate of chemicals or type of tillage). Random effects are those where the treatments are not fixed. For example, random effects might include trial locations, years (especially in drier climates), multiple observations (called subsamples) from individual plants (plant leaves, stems, or roots), individual soil cores, or yield monitor observations.

Multilocation Analysis Example (Table 13.5): Multilocation on-farm trials where soybeans were planted in 30-incg rows were compared with soybeans planted in 15-inch rows. The last column of Table 13.5 shows the statistical inferences for testing the null hypothesis that yield difference between the two row spacings was zero.

A more useful analysis is to express field-level and across field-level mean yield responses as random effects using normal distributions, each with its own mean and standard deviation or variance. The fifth, 10th, 50th (median), 80th, and 90th percentiles of these yield response distributions

Table 13.5. Summary of on-farm replicated strip trials comparing 15-inch and 30-inch soybean row spacing in Iowa in 2014.

Trial designation	Number of replications	Mean yield response	Standard deviation	Randomization test	
		bu acre^{-1}		*p*-value†	Evidence of significant difference
A	26	9.3	4.0	0.0001	strong
B	3	1.9	1.2	0.24	no evidence
C	3	4.8	3.0	0.25	no evidence
D	3	-1.0	1.5	0.50	no evidence
E	4	1.6	1.3	0.25	no evidence
F	4	-1.1	2.1	1.0	no evidence
Pooled	–	2.6	2.0	–	–

† p-values from a randomization test; no-evidence of statistically significant yield difference if p-values > 0.10; some evidence, 0.01–0.10; strong evidence, *p*-value < 0.01.

will then be calculated. Confidence intervals that include zero or negative values indicates that there is little evidence for a significant yield difference. For the same confidence level, that is, 90%, the narrower the confidence interval, the more likely that the true value will fall inside the specific range. In general, a 90% confidence interval is narrower than 80% interval.

Pooling Across Fields Example (Table 13.6)

The same trials comparing 15-inch vs. 30-inch soybean row spacing are analyzed by partially pooling or sharing information across trials. For trial A, there is an 80% chance that the true field-level mean yield response of 15-inch row spacing vs. 30-inch row spacing will fall between 0.6 and 9.6 bushels per acre and a 90% chance that it will fall between 0 and 11.1 bushels per acre. When pooling information across trials, two of the six trials have 90% confidence intervals that do not include zero, suggesting some evidence of a discernable yield difference between treatments with 15-inch and 30-inch row spacing. However, the mean yield response of 1.6 bushels per acre for the "across-field level" is less meaningful since the 90% interval includes a negative value.

Summarizing Data

Use Metadata and Research Databases

To interpret field data, information about the data collected is needed. *Metadata* is information that describes how the data or experiments were conducted, details about calibration, extent and severity of the problem, details about what and when the treatments were applied, and who conducted the soil and plant analyses.

Metadata helps to interpret the data. For example, foliar disease levels, climatic conditions, plant growth stage, seeding and germination rates, estimated yields, and leaf area can help explain yield differences between fungicide treatments.

Table 13.6. Percentiles of distributions of yield responses from six on-farm replicated strip trials with soybean row spacing treatments of 15-inch vs. 30-inch conducted in Iowa in 2014.‡

Trial designation	Adjusted standard deviation†	Adjusted yield response for different quantiles				
		fifth	10th	50th	90th	95th
		Bushels per acre				
		Within-field level				
A^{y}	3.0	0.0	0.6	3.5	9.6	11.1
B	1.0	0.1	0.4	1.7	3.1	3.4
C	2.4	-0.1	0.2	3.0	5.7	7.6
D	1.4	-2.3	-1.9	0.1	1.6	2.2
E	1.1	-0.2	0.1	1.5	3.0	3.3
F	1.7	-2.7	-2.0	0.2	2.1	2.7
		Across-field level				
	2.9	-0.6	0	1.6	4.0	5.4

†These summaries were estimated using hierarchical analyses, where mean and standard deviations for the two different levels were modeled as common random distributions.

‡Trial A had hail damage during the summer that may have affected yield response.

Table 13.7. Estimated cost of inputs and their application for calculating the break-even yield response in on-farm research.

Input	Cost per acre†
Ground application	$10
Aerial application	$15
Insecticide	$10
Fungicide	$10–15
Micronutrient	$5–15
Seed treatment (insecticide plus fungicide)	$8–20
Herbicide	$8–20
Plant growth stimulators	$5–20
Variable-rate fertilizer or lime prescriptions	$5–20
Cover crop seeds	$15–30

†These are estimates. Actual values will vary from year to year and by location. Use values as accurate as possible when doing economic analyses.

Connecting the dots in this example could lead to developing a decision-making tool (e.g., excessive rainfall in July may lead to more disease which may lead to the increased yield difference between treatments) instead of simply providing unexplained yield observations.

Combining metadata with other layers of information and variables related to each on-farm trial can lead to the development of research databases. There are many potential benefits of utilizing research databases in statistical and economic analyses.

Economic Analysis

In addition to statistical analysis, it is important to consider the economic and practical significance of research findings. Economic considerations are important because statistically significant yield increases do not necessarily mean higher profits. Thus, if the yield, grain price, and costs of inputs are known, *break-even yield response*, *economic optimum rate*, *economic return*, *return on investment (ROI)* values can be calculated.

In general, many inputs in crop production including application, seed treatments, fungicides, insecticides, micronutrients among others range in cost between $10 to $20 per acre (Table 13.7). Keep in mind that all costs need to be factored into the analysis. For example, with a cover crop seeded by an airplane, the cost of a fixed-wing aircraft applications might be $10 per acre and the cover crop seeds might cost $30 per acre. The total cost of seeding the cover crop would be $40 per acre.

The break-even yield response is a yield value needed in bushels per acre to equal the costs of the treatment.

Break-even yield response = cost of the input ($/acre)/price of unit of yield ($/bu) [1]

- For example, the breakeven yield response to cover the additional cost of $20 per acre in corn production is 20($/acre)/4 ($/bu) = 5 bushels per acre
- The farmer needs to grow at least 5 bushels per acre higher yield to justify the cost of the treatments ($20/acre).

The economic return (i.e., profit) can be estimated using formula 2.

Economic Return ($/acre) = [Yield (bu/acre) x Grain Price ($/bu)]– Input Cost ($/acre) [2]

Return-on-Investment (ROI) is percentage of monetary gain in yield relative to the input cost per acre.

ROI = 100 ×Economic Return ($/acre)/Input Cost Per Acre ($/acre) [3]

ROI Example (Table 13.6):

- two soybean row spacing (15-inch vs. 30-inch)
- the adjusted across-field median yield response of 1.6 bushels per acre
- the average ROI to the 15-inch row spacing was 44%
- Economic return = [(1.6 bu/acre×$9/bu)-$10/acre = $4.40/acre
- The ROI is 100 × 4.40/10, considering a $9 per bushel soybean price and $10 per acre additional cost to use a 30-inch planter to drive across a field twice to plant 15-inch rows. In this case, the ROI will be much lower if the farmer has to invest in a new 15-inch row planter.

It may be useful to estimate how to maximize the economic return per unit of area. To do so, the relationship between yield and input is often expressed as a production function or yield response curve. Calculating the maximum economic return per acre is then done by estimating the slope of the production function and making the slope equal to the ratio of unit of input cost to unit of crop price. The steeper the slope, the greater yield response per unit of input. The optimal rate indicates a point on the response curve where marginal return is equal to the marginal cost.

Economic Optimal Rate Example (Fig. 13.9): For the on-farm trial with four nitrogen (N) rates from Indiana, the economic optimal rate was estimated as 197 lb N per acre, considering that 1 lb of N costs \$0.50 and 1 bushel of corn has a value of \$4. A 90% confidence interval indicates that the true economic optimal rate (EOR) would fall between 188 and 216 lb N per acre at least 90% of the time.

To help farmers make better management decisions, data from multi-location on-farm trials can be used to extrapolate the observed yield responses for a broader area of interest. This type of analysis involves estimating risk, and it considers the whole distribution of potential yield responses under different soil conditions and weather scenarios.

Distribution of Yield Response Example 7 (Table 13.7 and Fig. 13.10): The predicted yield response for increasing the soybean seeding rates from 130,000 to 160,000 seeds/acre was based on 27 Iowa on-farm trials that were conducted in 2009. The findings showed that increasing the population could either decrease (loss 2 bushels per acre) or increase (gain 4 bushels per acre) for fields planted before and after May 20. The probability curves showed that there was a 60% chance of positive economic return if soybean planted after May 20 and a 30% chance of positive economic return if planted before May 20.

Join an On-Farm Research Network

While data collected in individual on-farm replicated trials can be valuable, organizing or joining an on-farm research network has many advantages. The most prominent advantage is increasing the ability to better summarize data. As part of a research network, data collected from individual trials will be combined with other similar trials and data results made available. This may expand the types of questions that can be asked to increase the statistical power for detecting differences between treatments. If data are stored properly, they may be used later for even more complicated analyses.

For farmers and their advisers, localized information is critical for making better decisions. As a research network, there will be more input on the appropriate products or practices to be tested to increase production.

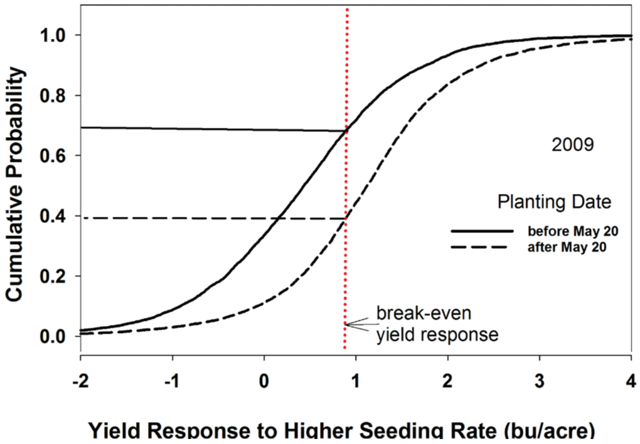


Fig. 13.10. Field-level predictions of yield responses for unobserved or new situations. The cumulative probability functions were derived using yield differences estimated at 100-feet grid patterns within each of 27 on-farm trial testing two soybean seeding rates, about 130,000 vs. 160,000 seeds per acre, with planting date before and after May 20 across Iowa in 2009. The break-even yield response (0.9 bushels per acre) shown as the red dashed line which can be moved to the right if seed costs increase or soybean prices decrease. The line can be moved left if the seed costs decrease or soybean prices increase. The two probability curves do not intersect, indicating that the curve for later planting dominates the one with early planting and suggesting a strong evidence of potential yield response with later, compared to earlier, soybean planting.

There are other less obvious benefits to joining or forming a research network. First, a research network can attract potential sponsors to cover treatment costs. Participating in research networks may increase access to existing research that has been already collected in small-plot or greenhouse trials. While these data are not collected using farmers' equipment, they may be more concise (less variable), complementing the on-farm data.

Finally, a research network provides a community for farmers and agronomists looking to improve the agronomic, environmental, and economic performance on their farms. Being part of a network allows farmers and agriculture professionals not only to learn from their own trials, but also learn from others' trial successes and failures.

Conclusions

This chapter discussed on-farm experiments conducted solely by farmers using modern precision agriculture equipment with the help of researchers and local agronomists. The success of these on-farm trials depends largely on communication and a good working relationship among farmers, researchers and technical providers collaborating as one team.

The keys for success of on-farm trials are: i) form a research hypothesis and make sure it is simple and practical by comparing only a limited number of treatments within a field; ii) follow the rules of designed experiments by replicating treatments, using randomization or personal knowledge of within-field variability or within-field management history; iii) keep all other management practices the same, except those used in treatments; and iv) develop a protocol that clearly outlines each step to improve the chances of having a successful experiment.

On-farm research can provide many benefits, but at times can be daunting, inconvenient or difficult. Organizing or joining a network of farmers and sharing on-farm research protocols can increase the chances of producing valuable data that can improve management decisions and lead to more sustainable farming. Finally, farmers participating in local research networks increase their ability to adapt to the economic and environmental challenges of modern crop production.

ACKNOWLEDGMENTS

Support for this document was provided by the Iowa Soybean Association, the Precision Farming Systems community of the American Society of Agronomy, International Society of Precision Agriculture, and the USDA-AFRI Higher Education Grant (2014-04572).

Study Questions

1. List two benefits of on-farm replicated strip trials as a research tool in agronomic studies, specifically for farmers, agronomists, and researchers.
2. List key differences between on-farm trials conducted by farmers and small-plot controlled field experiments done by university researchers and graduate students.
3. What aspects of on-farm experiments require the most attention?
4. On-farm strip trials fall into the category of "learning by doing". List the role of modern technologies, the internet and social media in on-farm research.
5. What new technologies may be helpful to conduct on-farm research in the near future? Why?
6. Why is a research hypothesis needed and what are the key elements of a research hypothesis?
7. Develop a short protocol for the following on-farm trials testing (i) effect of animal manure on wheat yield in rainfed conditions, and (ii) effect of in-furrow insecticide applications on corn yield in irrigated conditions.
8. List key climate and environmental variables needed to interpret results from on-farm trials studying (i) foliar fungicide applications on soybean and (ii) variable-rate planting on corn.
9. Describe the role of aerial imagery in cleaning yield data from on-farm trials.
10. What can go wrong right from onset of planning a good on-farm trial?
11. What are common considerations when using farmers' equipment such as planters or sprayers to conduct on-farm research?
12. Describe how publicly available tools can be used to select on-farm research locations. Explain three factors to consider when selecting a research site.
13. Explain the difference between "signal" and "noise" when analyzing data.
14. Give examples of metadata for on-farm research. What is the role of metadata?
15. List advantages of joining an on-farm research network.

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