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V-C Peanut News

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I'm going to venture down a slightly different path for this column. We post a lot of information on the Peanut Extension Portal (https://peanut.ces.ncsu.edu/). Rather than go through some of the things that will be important in the months to come, I'm going to walk through what I think it takes to put in a good experiment. We try to do this on the research stations and with on-farm tests. The goal is to make sure that when we see a difference among treatments in a trial (or we see no differences,) we have a sense of knowing how repeatable the results will be.

I've heard a number of people say, "A hundred pounds is a hundred pounds." The context is that when something is compared in a field experiment, if there is a difference of 100 pounds, it is real. I wouldn't argue with that person, but only if they are referring to the comparison of the treatments for that year and in that field and no more. In research, we are comparing various treatments with the goal of extrapolating beyond our trial to the broader farming community. We may conduct a trial on a few acres only to find that our results are used on tens of thousands of acres if not hundreds of thousands of acres. We need some checks and balances to make sure that what we find in one experiment or in a group of experiments holds up where it really counts. In my line of work that is farmer adoption.

The first step in an experiment is to make sure you have the right controls. The positive control in a trial is the standard or what is known to work. For example, if the trial is designed to see what can be used in place of Lorsban, the most logical positive control is Lorsban. This can be really nice to have in the trial but it is not absolutely critical. The non-treated control is absolutely critical. Without it you don't know whether or not the pest was present. In another example, if the native fertility in the soil is really high and in good balance, there may be no space for an increase in yield to occur due to fertilizer treatments. I might conclude that either product will work fine. But if I don't have a non-treated control, I will never know if the pesticide or fertilizer really helped. Saying that response to either treatment is the same could lead to an erroneous recommendation. If I know whether the pest was present or fertility was low by having the non-treated control, I would be able to draw a reasonable conclusion.

The second step is to make sure the treatments you are comparing, let's say a group of varieties, are replicated and randomized across the field (Figures 1 ad 2). The standard is to have varieties or other treatments randomized across the field at least 3 times. Think about sighting in a rifle. The first two shots are important, but the third shot helps you establish a grouping. A fourth shot really helps you establish the grouping more accurately. The same is the case for a variety comparison. The third and fourth replications are essential. Of course, for folks that have sighted in a rifle, after a few shots your shoulder begins to hurt and you may begin to flinch. Maybe that scope is going to catch my eye on the next shot. It can become taxing to get more shots in. The same is true for a variety trial. Once you get past four reps you gain little for the extra effort.

Randomizing the varieties helps you overcome any unexplained difference in spots in the field. If I had all of my Bailey II plots on one side of the field, it might be the highest yielder. But it may

simply be that one side or section of the field has the best soil for peanuts in general. Maybe it had a lower number of nematodes or pathogens. Bailey II might have been given an unfair advantage simply because of placement in the field. Emery, on the other side of the field, might have found itself in a poorly drained area with more disease. In another example, we might be comparing several fungicide treatments. If one fungicide is on one side of the field, it's possible that it will look really good or really bad solely based on the presence of the pathogen. We certainly don't want to say one treatment controls disease better than another one if it was really the side of the field we were comparing and not the fungicides. This explains why we often insist that we have at least four replications and that we need to alternate varieties or sprays all the way across the field.

So, let's say we have the appropriate controls in the trial and we have replicated and randomized all of the treatments adequately. What do we do with the data we have collected? Here's where an appropriate statistical analysis is needed. The goal of the analysis is to compare the variation in yield of varieties, for example, with the variation in yield that is not accounted for by varieties. I'll call this unknown variation. Being a sloppy researcher or having a non-uniform field relative to soil characteristics or pest pressure can contribute to unknown variation. Ultimately, if I have enough variation in yield when I compare varieties relative to the unknown variation that I can't account for, I can say there are statistical differences among at least some of my treatments. In this process, we also want to know how likely it is to make a conclusion due to chance. What's the probability or likelihood that this could happen. There's not enough time and space to describe probability values, but we want to make sure, as much as possible, that we don't tell you there is no difference in yield between Bailey II and Emery when in fact there really is a difference in yield between these two varieties. You would be very disappointed in us if we make that mistake very often. Statistics help us keep from making this mistake. "Even a blind hog gets an acorn every now and then." I'm not sure who coined that phrase but I think it applies to statistics. Just by chance one variety might out yield another variety. We don't want to make recommendations based on random chance.

Let's say we ran the trial with the correct controls in place, replicated and randomized treatments adequately, collected meaningful data then analyzed the data appropriately, and considered how frequently we might say there was no difference yield in when there was a difference. You still need to know, for example, whether yield of Bailey II, Emery, Sullivan and Walton differed from one another in the trial. We often calculate a number called the least significant difference. That simply means that the yield of Bailey II has to be at least some number greater than yield of Emery to be considered statistically different. I know many of you hate it when we show a table with averages for various treatments and say there is no difference in yield between a variety that weighs 5,500 pounds per acre and one that weighs 4,900 pounds per acre. We do that because we are thinking about how to extrapolate our trial results beyond the few trials we have. That's our goal. To use information from our trials to recommend practices to farmers. Statistics serve as a check and balance to make sure we don't get too far out there and find that our recommendations don't hold up in the real world.

Finally, it's not enough to have one trial in one year at one location. We need to repeat our trials over space (research stations, farmer fields, etc.) and time (years.) It takes a while to develop a meaningful data set that points us in the right direction in terms of recommendations. The variation we have in weather from year to year and region to region within a year require us to look at a set of treatments for several years at several locations.

Not sure this column helps you gain an appreciation for what constitutes a trial that has the potential to give meaningful results that is part of the puzzle in making recommendations that hold up over time. We need the right control(s) in the trial, we need treatments replicated and randomized, we need to analyze the data appropriately to know where the variation is coming from, and we need to repeat the trial over space and time.

In one of the courses I help teach at NC State (*Soil-Crop Management Systems*), we provide information on what you should ask when someone puts a set of data in front of you. At the farm level, folks are trying to sell you something or get you to change a practice. It's ultimately up to you to ask the questions that help you know how valid the claims are about a new product or changing a practice.

Figure 1. Example of field layout for a variety trial with the varieties planted all the way through the field with appropriate randomization and replication.

Bailey II
Emery
Sullivan
Emery
Sullivan
Bailey II
Sullivan
Bailey II
Emery
Bailey II
Sullivan
Emery

Figure 2. Example of a small-plot disease management trial with appropriate randomization and replication.

101 Non-treated control	201 Fungicide program 2	301 Fungicide program 1	401 Fungicide program 2
102 Fungicide program 1	202 Non-treated control	302 Fungicide program 3	402 Fungicide program 1
103 Fungicide program 2	203 Fungicide program 3	303 Non-treated control	403 Fungicide program 3
104 Fungicide program 3	204 Fungicide program 1	304 Fungicide program 2	404 Non-treated control