— 2003 NTC Technology Transfer, October. Nursery Technology Cooperative, Dept of Forest Science, Oregon State Univ., Corvallis, OR.

Read this! study tips

Every year, nursery and reforestation managers invest an enormous amount of time and resources to monitor seedlings or to study new products or technology. Many acres are set aside, fences are built, seedlings are carefully planted, treatments are applied, and data are collected. Too often, neglecting a few simple guidelines leads to data that are considered "anecdotal," are confounded with an outside influence, or are insufficient to adequately assess the question at hand.

Example -Setting up a Study - (see figure on page 7)

A forester sets up a study to compare four stocktypes and establishes four plots (one for each treatment) with 200 seedlings in each. Each seedling is planted well and a fence is built to eliminate variation due to browsing.

What could go wrong? --

A wildfire comes through and a plot on one side of the study is destroyed. This effectively eliminates one of the stocktypes from the study.

OR, unbeknownst to the forester, there is a gradient in soil quality (e.g. fertility, drainage, texture, water holding capacity) across the site such that one of the stocktypes is planted in a better soil environment than the others. The forester analyzes the data and erroneously concludes that that particular stocktype is superior to the others.

OR, due to tall trees in an adjacent stand and the aspect of the site, one side of the study area gets shaded more than the other side. As a result, the most shaded plot is under much less daily transpirational stress than the other. The differences in growth and survival found in this situation may have very little to do with stocktype although the forester may incorrectly conclude that the stocktype planted in the shadiest plot is the best.

Example - Collecting Data -

A nursery manager would like to determine if there is a micronutrient deficiency during the growing season. Foliar samples are carefully collected from several nursery beds and sent for analyses.

What could go wrong? --

To save money, the foliar samples are combined and a single sample is sent to the lab. Although one area of the nursery does have a deficiency, this is unlikely to show up in the data. The nursery manager is pleased to have saved money at the lab but in fact the money was wasted on useless data. A few more bucks spent on more intensive nutrient sampling may have saved thousands of dollars in losses due to culls.

OR, foliar samples are collected only once during the season. Depending on how fast the seedlings are growing at that time and how long it's been since the last fertilization, the foliar nutrient concentration could be relatively high or low and not necessarily reflective of the overall crop nutrition.

OR, foliar samples are collected on the best looking seedlings only. If the objective is to assess the best of the best, this is fine. But, if the objective is to characterize the crop as a whole, than the data will not be at all representative.

Obviously, management decisions based on conclusions made from such data can be risky and costly. By considering a few basic principals of study design and data collection, you can yield highly credible information which can be used to answer questions or make decisions. And, despite beliefs to the contrary, this usually requires little or no additional input of time and resources.

The three R's of generating quality data are:

Randomization: By randomly assigning treatments to seedlings/plots or randomly selecting seedlings to be measured, you can avoid bias. Bias occurs when results systematically differ from the truth. Some examples of bias: "This plot looks weedy, let's put the vegetation control treatment here.", This area is close to the road, let's install the fertilizer treatments here so we don't have to carry it up the hill.", "Looks good to me!"

Representation: The study site, seedlings, treatments, etc. need to be representative of the population to which you would like to apply the results.

Replication: By replicating the treatments, you can be more confident that the response you measure is not a one-time event. Also, blocking can account for site variability and can make it easier to determine whether or not there is a treatment difference. The resulting design allows for the analysis to isolate the variability due to site, thus providing the best estimate of differences among treatments. For NTC studies, we usually have five or more replications (blocks) in a study and never less than four.

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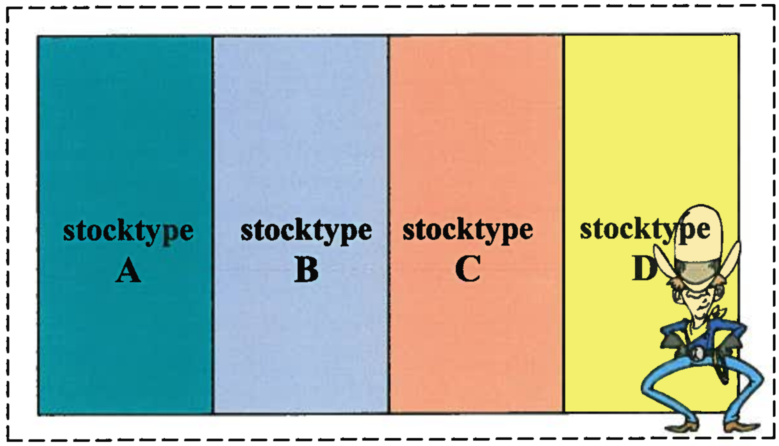
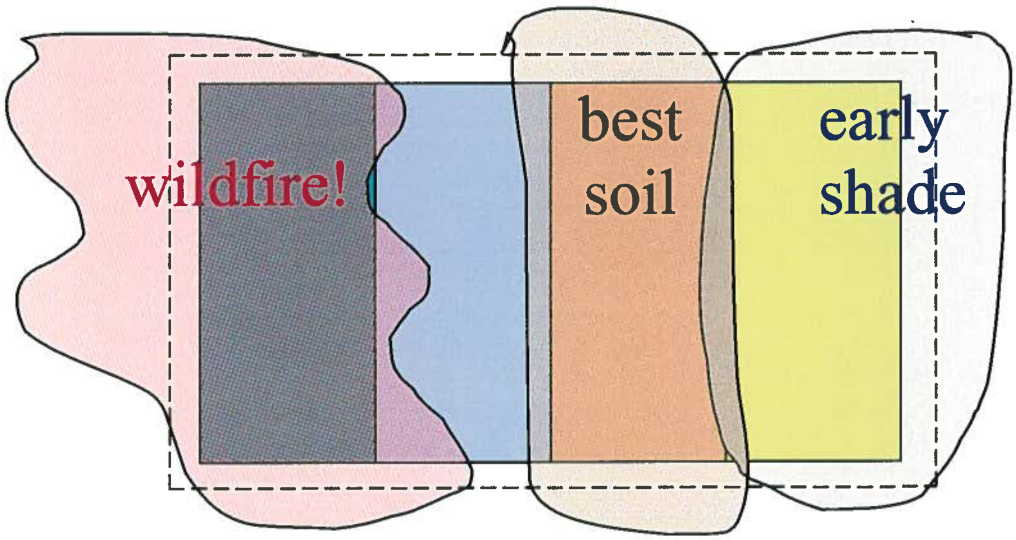
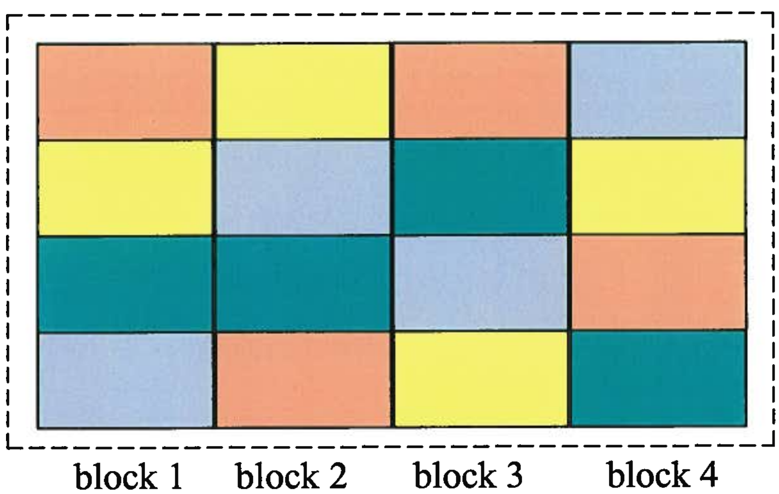
Original study idea with no randomization or replication—typical of observational studies throughout the PNW.



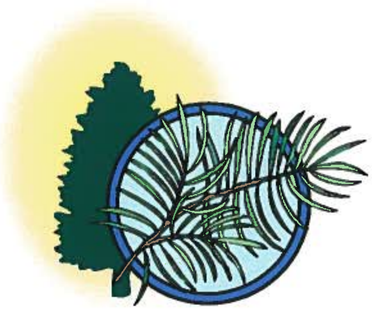
Potential problems which could confound the study as originally designed.



Redesigned study with the same acreage and same number of seedlings but with incorporation of randomization and blocking.



A study which incorporates the three R's is the strongest kind of study and offers a data set which can be analyzed with confidence. Back to the example on setting up a study, the study could be redesigned to incorporate replication by dividing the area into four blocks. Each block should be selected such that conditions within the block are as uniform as possible. Then, treatment plots of 50 seedlings each can be randomly assigned within each block (see figure on page 7). The site, seedlings, and stocktypes used should be typical of the area so that the results can be applied with confidence. The redesigned study still consists of the same number of seedlings and acreage planted. However, it can still yield useful information despite shading, wildfire, and site variability. As is common with many projects conducted in the field, this example illustrates how a good study design does not necessarily require additional resources, only extra forethought.



The 3 R's should also be followed when collecting data. Back to our second example – the first question is to determine what the samples should represent. If the objective is to determine overall foliar nutrients in the nursery, then replications can be established throughout the nursery. However, if the objective is to determine the nutrition of specific seedlots or nursery beds, then replication must occur within those lots or beds. Once that is determined, samples can be collected in a non-biased, random way. There should be at least one sample per replication (even more is better). Within a replication, foliage from several randomly selected seedlings can be composited.

When collecting data, it's important to establish and follow consistent measuring or sampling criteria. For example, seedling height is always measured groundline to the base of the terminal bud and is measured to the side of the seedling if there is a significant slope. The resulting data will avoid introduction of variability due to slope or measuring differences and can therefore provide a better estimate of the seedling response due to the treatments being evaluated.

It is important to account for and avoid bias that can be inadvertently introduced during data collection. Any measuring equipment that requires calibration should be checked and monitored. Foresters tend to gravitate to the best-looking seedling on a site. Often, researchers gather around the healthiest tree in a study as if it is a warm campfire. It's difficult to approach a study without expectations regarding treatment effects. However, it is important not to introduce this personal bias to data collection. For example, if one is always rounding up the height measurements for seedlings which received a fertilizer treatment and rounding down the numbers for those which did not, then the resulting data set will have an inflated estimate of height response to fertilization.

<u>Other things to consider in order to avoid bias, confounding, and excess variation</u>:

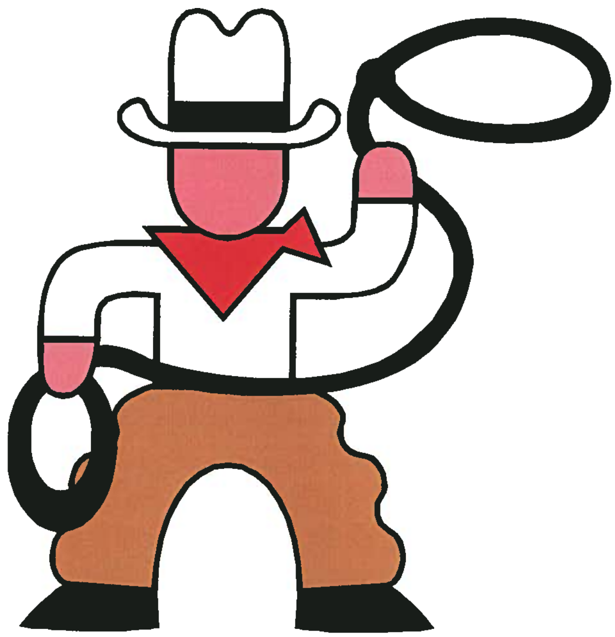
- Unless your objective is to compare seedlots, or nurseries use the same lot and same nursery for your entire study. If you are including both bareroot and container stock in your study, "stocktype" will be one of your comparison factors. However, they should each be from the same seedlot, otherwise you may conclude that one stocktype is performing better than the other when in fact it is a genetic effect.
- Protect from animal damage.
- Do not try to answer too many questions in a single study
- Control effects of adjacent treatments on each other. Install buffer strips if needed.
- Do not assign one planter to each treatment. If one planter plants differently than the others, then planting technique will be confounded with treatment. For example, if a planter plants deep, all seedlings in that treatment will have shorter heights than those in the other treatments even though this has nothing to do with treatment.

<u>Miscellaneous tips:</u>

- Take a lot of pictures.
- Clearly identify the study with flags, tags, signs, posts.
- Make a map of the study.
- Contact us for suggestions and assistance. Send the data to Diane for statistical analyses (take full advantage of your NTC membership!–a service that only a few of you have been using.)
- Write a study plan. It should read like a recipe so that anyone can follow it. Record any deviations from the plan in detail. Things to include in the plan:

⇒ *objective*
⇒ *description of seedlings and treatments*
⇒ *dates/procedures for establishment & treatment application*
⇒ *protocol for data collection (frequency, method, sample size)*
⇒ *labor and equipment needed*
⇒ *site description and location*
⇒ *study design and statistical analyses*
⇒ *time line*
⇒ *maintenance needs*
⇒ *expected outcomes*

COWBOY SCIENCE



Guest Editorial by Robin Rose published in Western Forester March/April 2000 issue

Many years ago I was standing before a group of foresters who represented the whole of forestry in the region to discuss a research project I was proposing. The proposal had support, but there were many questions to be answered about outcomes. As I recall, one supporter suggested rather strongly that "the seedlings need to be planted operationally." He explained that the seedlings needed to be planted operationally "because that will best simulate the working environment and better reflect operations to field foresters." I could not have disagreed more!

The study involved a simple hypothesis. Does an increase in root volume lead to better growth and bigger seedlings? Previous research indicated that larger root volumes would indeed lead to bigger seedlings. I explained this, but the forester still came back with, "but if you don't plant them operationally it won't simulate the operational environment." He wanted a better answer than what I was providing.

I looked at the group and said, "I am not interested in the operational environment where this experiment is concerned," as I reminded them that I, too, had worked in industrial forestry. Everyone looked concerned that they were stuck with a bloody academic who barely know trees needed to be planted green side up. I just smiled. "You see, if I wanted to test operational planting I'd set up a study with multiple planters, say seven planters. After selecting my seven planters I would make sure none of them actually knew they were being tested. Then each would be given specific areas to plant and everyone would get very similar seedlings that I had pre-measured. I do not want confounding." I got some blank looks but pressed on toward my conclusion.

"After having them plant the trees, I would dig each of the trees up using a methodology designed to assess differences in operational planting," I continued. "Now, why am I not interested in operational planting for this root volume study?"

First, I am not interested in confounding my results with differences in planter technique. Second, each tree must be planted perfectly. Third, with perfect planting we will have a superbly executed research project designed to test one thing–do large root volume seedlings grow better than small root volume seedlings? Since I know you all will agree that operational planting can be highly variable, it seems best to leave th at source of variation out of the study." There was more discussion and we did the experiment. As it turns out, root volume plays a key role in getting seedlings to green-up faster. The work is published! No operational planting occurred.

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This story has special significance to me personally because I have seen so many mistakes in operational forestry research throughout my career. It always seems best to run "a trial," "a demo" or "a quick and dirty test." In most cases these are unreplicated large "see what we get" studies designed for an upper level manager that wants to see what happens operationally "if we do it this way." It can be spaying a hundred acres with one chemical or fertilizing one row of seedlings in the middle of a clearcut...and with the rest of the area as a "control."

Unfortunately, lots of these efforts yield little and many times lead to more questions than answers. Sometimes the non-result is a career embarrassment! A common statement I hear is that "we didn't have the time to do more and I know wish we had done the whole thing differently." Well, I wish in all too many cases that we would stop doing "operational science!" In fact, one of my good friends in industry gave it a name, Cowboy Science. It is about quick answers and quick operational outcomes.

The dangers of Cowboy Science are many. If the study outcome looks wonderful, how do you know it worked for the reasons originally conceived? If it did not work, do you really know why?

I learned years ago from my industrial experience that the idea of research is to arrive at answers to questions. Research is kind of like the law. It is specific. A question is sculpted down to its basic elements and a methodology is set up to answer the question. In research we remove as much "noise" or confounding elements from the experiment as possible. We want to be careful about out application technique and concentrations if it is an herbicide study. We want replication so that we know if the treatment we applied really occurred–not a one-time event. No replication is a one-time event. We want randomness to be sure our treatment woks without prejudice.

Good research advances forest science through a doggedly boring methodical process whereby experiments isolate specific answers to specific questions. Once we have an answer it then becomes a matter of adapting that answer to the operational working environment. We scale up. A decade and half ago no one in the Northwest would have guessed we would be planting Douglas-fir styro-15s or 1+1s with 9-13mm calipers. But the research both in the nursery and the field has brought us to this point. Large seedlings with big root systems grow really well. Now we are in the process of adding slow release fertilizers to these new improved operational seedlings.

Cowboy Science does not work because there are no quick answers! A day will come in some areas when we will have Douglas-fir at green-up in two years. This will only come after proceeding with a doggedly boring methodical process to attain two meters growth in two years.

Please consider your options the next time someone suggests a demo, a trial, or a quick and dirty test. With some forethought you may be surprised that a real research experiment with a hypothesis and workable methodology is no more expensive and time consuming than the four-acre unreplicated plots they had in mind because it will be easier. Research is not easy. It is a (you got it) doggedly boring methodical process!