

# NO REPLICATION

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Before 1930, many forestry experiments did not involve replication of treatments. Treatments were applied, observations were made, and papers were published from nonreplicated trials (e.g. Retan 1915). In some nursery studies, treatments produced a small difference in germination percentage, but it was left up to the author to guess if the observed difference was real or was just a random effect. At that time, researchers did not calculate a probability of making a Type 1 error (i.e. claiming a difference was real when it was not). Often, recommendations were based only on the absolute difference between treatment means.

During the 1920s, Sir Ronald Fisher published several books on statistics (Fisher 1925a, b) and, at that time, there might have been 12 forester researchers who knew about statistical analysis (according to Bruce 1925). After reading these books, some began to replicate treatments since this would increase the power of the test and reduce the chance of making a Type 1 error. Even though forestry schools were not teaching statistics, Gevorkiantz (1935) said "...that a working knowledge of the statistical method should be made a part of the working equipment of every scientific forester." Soon afterward, replicated studies began to appear in the Journal of Forestry (e.g. Meginnis 1940; Muntz 1944).

Some foresters were quick to adopt the practice of replication. For example, Philip Wakeley (the second recipient of the Barrington Moore Award) installed replicated trials before 1930. Later, Wakeley (1965) discussed the matter of unreplicated trials at a meeting at Syracuse University. The paragraphs below are from his paper.

*With one notable exception, experimental treatments in our early nursery studies were unreplicated. Fisher's analysis of variance, first published as I recall, in 1924, had not yet seeped down to even the research workers in the forestry profession. Snedecor, I believe, had not yet begun to publish. Simple and obvious though the principle is, we had not yet, ourselves, thought of replication. In the literature you will note the consistent lack of replication (as well as many other deficiencies) in American thinning studies established prior to 1930. Jonathan W. Wright has commented scathingly on the lack of replications in the older provenance tests both here and abroad.*

*But we did have one replication in a nursery test at Bogalusa. It came about in this way. In our 1925 experimental nursery, the first one that I had a hand in sowing, we devoted three beds-- that is, 12 quarter-bed units-- to chemical weed control treatments applied to one species of pine.*

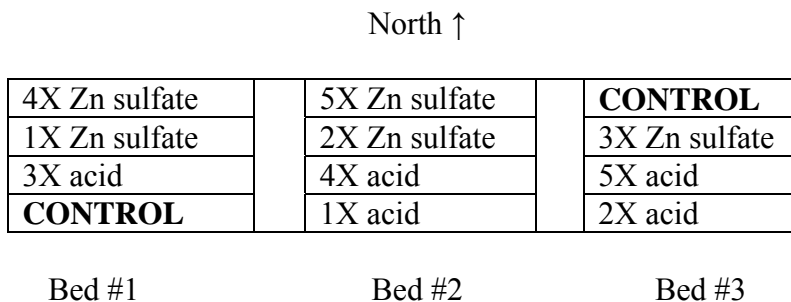
*The first or southernmost quarter-bed was sown as a check without chemical treatment. Then we applied, as I recall, sulfuric acid at five successively higher levels, followed by zinc sulfate at five successively higher levels. The check plus the ten chemical treatments left us with one unused quarter-bed unit, at the north end of the third bed. To get on to the installation of the next study we made a quick decision and sowed this last quarter-bed as a second untreated check.*

*Well, there happened to be a distinct gradient in soil texture and fertility running from south to north along those three beds. When the results were summarized the following fall they were devastating. Compared to the first check, all chemical treatments were beneficial. Compared to the second check, at the north end, all chemical treatments were deleterious. The man under whom I was working... was furious. "Damn it, Phil", he said, "if only you hadn't put in that second check, we could have published a bulletin!"*

This shows that some believe publication is more important than making a Type 1 error. In my opinion, this belief still persists. Some authors continue to submit unreplicated trials for publication in journals and proceedings. No doubt, some papers are rejected by editors while others pass the review process. This might occur because some manuscript reviewers are not aware of the difference between true replications and pseudoreplication (Hurlbert 1984, Bergerud 1988). In addition, after reading the methods section, some reviewers might not be able to determine the true experimental unit (Bergerud 1988). As a result, papers that involve no replication may include claims about “significant” treatment differences (due to an incorrect analysis of variance table). It seems this trend might be due to a growing decline in the knowledge of proper statistical procedures.

For all of the above reasons, foresters should realize that researchers differ in their opinions regarding what constitutes “scientific proof.” Some require replication of treatments while others produce conclusions that are based solely on pseudoreplication.

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 PLOT MAP



An example of a nonreplicated weed control trial in 1925 (after Wakeley 1965). Seedlings in the north control plot were larger than seedlings in other plots. Seedlings in the south control plot were smaller than all other plots. Conclusions regarding the nonreplicated weed control treatments would differ greatly depending upon the location of the control plot. In this and other non-replicated trials, environmental differences among plots are confounded with treatment location.

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