USING FRACTIONAL FACTORIAL DESIGNS IN FOREST PRODUCTS RESEARCH[1]

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ABSTRACT

Fractional replication is an experimental design that enables a researcher to evaluate several factors, in an experiment of manageable size, by using a portion of the treatment combinations that normally would be used in a complete factorial experiment. Such a technique is useful for 1) screening a large number of factors, as in an exploratory study, and 2) identifying which of many factors merit more detailed evaluation. This article describes fractional replication and presents an example of its application to a study of the effect of eight factors on white oak plywood bond quality.

Keywords: Experimental design, fractional replication, white oak, plywood.

INTRODUCTION

Forest products researchers often conduct experiments involving many factors, i.e., types of treatment, in preliminary phases of product development. For example, in the process of developing plywood from a new species, a researcher might be interested in the effect of veneer moisture content, the type and quantity of glue, and press pressure, temperature, and time. Similarly, flake geometry, resin content, and press time and temperature may be factors of interest for flakeboard production.

An experiment involving all combinations of only two or three treatments, called levels, of each factor listed above could involve more than 200 treatment combinations, which is often unacceptably large or too time-consuming. The usual solution to this problem is to conduct a series of complete factorial experiments, i.e., all combinations of all levels of the factors being studied. Initially, two or three factors are studied, while the other factors are set at a value assumed to be close to optimum. Results of the initial study are used in a subsequent

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TABLE 1. *Description of factors used in the study of white oak plywood production.*

Factor	Description	Levels	
AT	assembly time (min)	0.5	10
PP	press pressure (psi)	200	300
PT	press temperature (°F)	285	350
TM	press time (min)	6	9
PR	prepressing	no	yes
MC	veneer moisture content (%)	4	8
NA	NaOH content of glue (%)	3	6
EX	extender in glue (%)	4	8

experiment designed to examine a few remaining factors. This process is continued until all factors have been studied at least once.

There is another, usually more efficient, way to approach an experiment involving many factors: a fractional replication of the factors of interest. Fractional replication is an experimental design that can be applied to experiments that involve at least four factors. Only a specifically selected set of treatment combinations are studied, often ½, ¼, ⅛, or even 1/16 of the number of combinations in a complete factorial. The result is a smaller experiment than a complete factorial of all the factors of interest.

A consequence of using a fraction of the treatments in a complete factorial is that high-order interactions, which can be estimated in a complete factorial, cannot be estimated. High-order interactions occur when the effect of one factor depends upon the level of other factors. For an experiment with the objective of examining all interactions among the factors, fractional replication would be unacceptable. But fractional replication can be useful for an experiment in which the researcher is reasonably confident that high-order interactions are negligible, and for an exploratory experiment in which the objective is to identify factors that merit further study.

Although fractional replication has been used by statisticians for many years, few forest products researchers have applied the technique. We feel that it has considerable potential application in forest products research. This paper describes fractional replication and presents an example of its application in plywood manufacturing.

A COMPLETE FACTORIAL

Certain properties of a complete factorial design and a few statistical concepts need to be presented to develop a basis for describing fractional replication. As the basis for an example of a complete factorial, we will use a study done on the effect of eight factors—assembly time (AT), press pressure (PP), press temperature (PT), press time (TM), prepressing (PR), veneer moisture content (MC), glue mix NaOH content (NA), and glue mix extender content (EX)—on the percentage wood failure of white oak plywood.[2] The original study was a fractional replication involving 32 of the possible 256 treatment combinations that would have resulted from studying two levels of each of the eight factors. For the complete factorial example, we will study the effect of the first four factors listed in Table 1, using half of the data from the study.

For each factor listed in Table 1, a minus (−) will be used to represent one level and a plus (+) will be used to represent the other. For the example, the two levels of assembly time are referenced as − for 0.5 minutes and as + for 10 minutes. By using the system, the 16 treatment combinations of the four factors are listed in the second through fifth columns of the rows numbered 1 to 16 in Table 2. For reasons that will become apparent later, the treatments are not listed in serial order. The first four columns of +'s and −'s in each row represent one treatment combination, e.g., + + + + represents a board made with a 10-min assembly time, then pressed at 300 psi at a temperature of 350 F for 9 min.. The remaining columns of +'s and −'s in Table 2, labelled AT × PP through AT × PP × PT × TM, represent 11 interactions of the four factors. The +'s and −'s in the interaction col-

[2] For a more detailed description of the study and its results, see the unpublished M.S. thesis by Daniel DiCarlo. 1984. *The effect of some processing variables on white oak plywood.* On file at Iowa State University.

TABLE 2. *Average percentage wood failure for all combinations of assembly time (AT), press pressure (PP), press temperature (PT) and press time (TM).*

Treatment #	AT	PP	PT	TM	AT × PP	AT × PT	AT × TM	PP × PT	PP × TM	PT × TM	AT × PP × PT	AT × PP × TM	AT × PT × TM	PP × PT × TM	AT × PP × PT × TM	Ave.
1	+*	+	+	+	+	+	+	+	+	+	+	+	+	+	+	17.6
2	+	−	−	+	−	−	+	+	−	−	+	−	−	+	+	26.9
3	−	+	+	−	−	−	+	+	−	−	−	+	+	−	+	10.3
4	−	−	−	−	+	+	+	+	+	+	−	−	−	−	+	8.2
5	+	+	−	−	+	−	−	−	−	+	−	−	+	+	+	10.0
6	+	−	+	−	−	+	−	−	+	−	−	+	−	+	+	9.9
7	−	+	−	+	−	+	−	−	+	−	+	−	+	−	+	10.8
8	−	−	+	+	+	−	−	−	−	+	+	+	−	−	+	18.5
9	+	+	−	+	+	+	+	−	+	−	−	+	−	−	−	19.2
10	+	−	+	+	−	+	+	−	−	+	−	−	+	−	−	22.5
11	−	−	+	−	+	−	+	−	+	−	+	−	−	+	−	9.2
12	−	+	−	−	−	+	+	−	−	+	+	+	−	+	−	12.1
13	+	+	+	−	+	+	−	+	−	−	+	−	−	−	−	9.8
14	+	−	−	−	−	−	−	+	+	+	+	+	+	−	−	9.9
15	−	+	+	+	−	−	−	+	+	+	−	−	−	+	−	11.9
16	−	−	−	+	+	+	−	+	−	−	−	+	+	+	−	14.4

* See Table 1 for description of levels of factors.

umns are determined by multiplying the +'s and −'s in the columns for the main effects in the interaction.

The average percentage wood failure of each treatment combination is listed in the last column of Table 2. An analysis of variance of the data for the experiment, as shown in Table 3, indicates a strong assembly time by press time interaction (AT × PT), a press pressure by press time interaction (PP × PT). The differences between the two levels of assembly time (AT) and press time (PT) appear to be significant, but the interactions must be evaluated before these effects can be properly evaluated.

TABLE 3. *Analysis of variance of data treated as a complete factorial for the effect of assembly time (AT), press pressure (PP), press temperature (PT), and press time (TM) on percentage wood failure.*

Source	df	SS	*F*	Pr > *F*
Blocks	1	3.6	0.24	0.63
Treatments	15	928.4		
AT	1	95.1	6.3	0.02
PP	1	52.4	3.5	0.08
AT × PP	1	9.8	0.7	0.43
PT	1	0.1	0.0	0.93
AT × PT	1	8.6	0.57	0.46
PP × PT	1	0.1	0.0	0.94
AT × PP × PT	1	10.7	0.7	0.41
TM	1	505	34.	<0.01
AT × TM	1	136	9.0	0.01
PP × TM	1	82.2	5.5	0.03
PT × TM	1	0.2	0.01	0.91
AT × PP × TM	1	1.4	0.09	0.77
AT × PT × TM	1	26.2	1.7	0.21
PP × PT × TM	1	0.0	0.00	0.98
AT × PP × PT × TM	1	0.43	0.03	0.87
Error	15	225.9		

TABLE 4. *Average percentage wood failure for combinations of assembly time and press time.*

Assembly time (min)	Press time (min)	
	6	9
0.5	10.1	9.4
10	13.9	21.5

A major advantage of a complete factorial involving many factors, compared with a series of experiments which study only a few factors at a time, is that a researcher can examine all interactions of the factors being studied. Detecting interactions, when present, can be of vital importance in understanding the effects of a number of factors on a particular response; but the size of interactions generally decreases as the number of factors involved increases, i.e., in a multifactor experiment, two-way interactions are, on the average, larger than three-way interactions, three-way interactions are, on the average, larger than four-way interactions, and so on. Two-way interactions also tend to be smaller than main effects (Box et al. 1978).

Calculating an interaction involves observing how the mean difference between two levels of a factor changes as the levels of the other factors change. As an example of a two-factor interaction, the average percentage wood failure for the four combinations of assembly time and press time, averaged over all combinations of press pressure and press temperature, can be analyzed (Table 4). From the four means, two estimates of the effect of assembly time can be obtained. One estimate is the difference between the 6-min and 9-min levels for boards with an assembly time of 0.5 min, which is $9.4 - 10.1 = -0.7$. The other estimate is the difference between boards with an assembly time of 10 min, which is $21.5 - 13.9 = 7.6$. If the two factors were additive, the two values would be, on average, equal, although for a particular experiment they probably would be slightly different because of random variation. A statistical test can be used to decide whether the difference between the two estimates is significantly different from zero, which would indicate that the factors interact. Additivity occurs when the effect of one factor is unaffected by (independent of) the particular level of other factors. If factors are additive, they do not interact and vice versa. If the two factors interact, the two estimates generally would be different. The difference between the two estimates, $7.6 - (-0.7) = 8.3$, is an estimate of the size of the interaction of the two factors. The F-value from the analysis of variance (Table 3) is large, indicating that the difference is much larger than would be expected at random ($P = 0.01$).

The difference in the effect of assembly time for the two press times can also be calculated by using individual treatment means. For an assembly time of 0.5 min, i.e., the $-$'s in column AT in Table 2, treatment 7 (referred to as T7) is $+$ in column TM and treatment 3 (referred to as T3) is $-$ in column TM; so one estimate of the effect of assembly time on percentage wood failure is T7 $-$ T3. Table 2 shows three other similar estimates. Consequently, the mean difference in percentage wood failure for boards with an assembly time of 0.5 min is

$$(T7 - T3 + T8 - T4 + T15 - T11 + + T16 - T12)/4 = -0.7 \quad (1)$$

where Ti represents the mean for treatment i in Table 2. Similarly, for an assembly time of 10 min, the difference is

$$(T1 - T5 + T2 - T6 + T9 - T13 + + T10 - T14)/4 = 7.6 \quad (2)$$

Subtracting (1) from (2) and rearranging, yields

$$(T1 + T2 + T3 + T4 - T5 - T6 - - T7 - T8 + T9 + T10 + T11 + + T12 - T13 - T14 - T15 - - T16)/4 = 8.3 \quad (3)$$

Note that the arrangement of $+$'s and $-$'s in (3) is the same as in the AT $\times$ TM column of Table 2. Thus, another way to calculate the interaction is to multiply the column of $+$'s and $-$'s in column AT $\times$ TM, as $+1$'s and -1's, by the column of means, add up the 16 values, and divide by 4, i.e., $[(+1)\cdot(17.6) +$

$(+1)\cdot(26.9) + (+1)\cdot(10.3) + \ldots + (-1)\cdot(14.4)]/4$. Three-way and higher interactions are calculated similarly.

Note that each column of +'s and −'s in Table 2 is unique. Because there are 16 unique treatment combinations, 15 independent treatment effects, i.e., the influence of a particular combination of factors relative to the overall mean, can be estimated, which results in the 15 degrees of freedom for treatments in Table 3. The rule is that if there are N unique treatment combinations, N − 1 independent treatment effects can be estimated.

It is obvious that a treatment effect can be estimated because there are two levels. In the plywood study, the main effect, i.e., the difference between the mean of a level of a factor and the overall mean, of assembly time can be estimated because there are two levels. If only one level had been used, we could not estimate a difference. The same is true for estimating interactions. Each interaction is calculated by the unique combination of +'s and −'s. If only + or − treatments for an interaction were used, it could not be estimated.

A complete factorial allows an investigator to estimate the main effect of each factor and all of the interactions efficiently. Chapter 5 in Cochran and Cox (1957) and chapter 12 and 13 in Box et al. (1978) present excellent discussions on the construction and analysis of complete factorial experiments.

FRACTIONAL REPLICATION

Consider a multifactor experiment for which a researcher decided to use only those treatment combinations that would be + for the highest-order interaction of a complete factorial of all factors, e.g., the treatments with a + in the AT × PP × PT × TM column in Table 2. Such an experiment would require only half of the treatment combinations of a complete factorial. As just noted, by using only the combinations that are +, the AT × PP × PT × TM interaction cannot be estimated. But not being able to estimate one interaction, particularly the highest-order one, seems like a small price to pay for conducting a smaller experiment. Unfortunately, the price may be higher, but, depending upon the situation, maybe not too much higher.

To help explain what would happen if only the treatments that are + for an interaction were studied, i.e., what the whole price is, we will examine the consequences of doing the plywood study with only the treatments that are + for AT × PP × PT × TM. Note that the first eight rows in Table 2 are all + for AT × PP × PT × TM.

With the 16 treatment combinations of the original complete factorial, 15 independent treatment effects can be estimated (see Table 3). With the eight treatment combinations of the fractional replication, only seven independent treatment effects can be estimated. Of the 15 treatment effects that could be estimated by the complete factorial, the AT × PP × PT × TM interaction has been sacrificed, which leaves 14 effects to be estimated. Obviously 14 effects cannot be estimated from seven. What has happened?

Recall that for the complete factorial, each main effect and interaction could be calculated by using its column of +'s and −'s. The same is true for the one-half replicate. For the complete factorial there are 15 columns, and each one is unique. For the one-half replicate there are also 15 columns, but the columns are not unique. Aside from the AT × PP × PT × TM column, each of the other 14 columns is identical to one of the other columns, resulting in seven pairs of columns that are different from the other pairs. By examining the first eight rows in Table 2, you will see that columns labelled AT and PP × PT × TM are identical. This means that both the main effect of assembly time (AT) and the press pressure by press temperature by press time interaction (PP × PT × TM) are estimated with the same combination of treatment means. This does not mean that the two are equal. They very likely are different, but because of the treatment combinations that were used, the main effect of assembly time and the PP × PT × TM interaction effects are combined and cannot be separated. Only the sum of their effects can be

estimated. This is called confounding and is a consequence of using a fractional replication. The first eight rows of Table 2 illustrate that each main effect is confounded with the three-way interaction of the other three factors, and that each two-way interaction is confounded with the two-way interaction of the other two factors.

As each of the 14 effects is confounded with one of the other effects, there are only seven effects that can be estimated. But the seven effects represent the combination of two effects that could be estimated with the complete factorial. Thus, although we know that there are 14 treatment effects present, only seven effects, each one representing the sum of two treatment effects, can be estimated.

What good is an estimate of the sum of the main effect of assembly time and the PP × PT × TM interaction, or the sum of any other two effects? Such an estimate has almost no value unless we have an estimate of one of the two effects. Recall that high-order interactions tend to be smaller than main effects and low-order interactions. Thus, the three-way interaction, PP × PT × TM, is probably smaller than the main effect of assembly time. In fact, if we could assume that PP × PT × TM were negligible, the estimate of AT + PP × PT × TM would be AT, i.e., if AT + PP × PT × TM = 10 and PP × PT × TM = 0, then AT = 10. If we make similar assumptions for the other three three-way interactions, we then have estimates of PP (=PP + AT × PT × TM), PT (= PT + AT × PP × TM) and TM (= TM + AT × PP × PT).

One might have serious qualms or objections about assuming that some interactions are negligible. Interactions can be large, but sometimes a researcher has worked with the factors being studied for enough time to be confident that they are unlikely to interact, or, better yet, has done complete factorial experiments and has not found interactions among the factors. In these situations, assuming interactions to be negligible is reasonable and using fractional replication can be very efficient. Even if a researcher lacks knowledge about interactions, experience has shown that high-order interactions generally are negligible, especially very high-order ones.

Treatment effects that are confounded are said to be aliases. The PP × PT × TM interaction is said to be the alias of assembly time, AT, and AT is said to be the alias of PP × PT × TM. It is easy to determine which effects are aliases for a one-half replicate. First, you need to identify the defining contrast, i.e., the interaction used to choose a portion of the treatments in the complete factorial by using only those treatments that are either + or − for the interaction. For the example using the first eight rows of Table 2, only the + levels of AT × PP × PT × TM were used. Thus, +AT × PP × PT × TM is the defining contrast. Next, multiply the letter or letters representing each main effect and interaction by the defining contrast, e.g., AT and +AT × PP × PT × TM = +AT × AT × PP × PT × TM and AT × PP and +AT × PP × PT × TM = +AT × AT × PP × PP × PT × TM. If any letter or letters representing an effect appear twice, cancel them out, e.g., +AT × AT × PP × PT × TM → +PP × PT × TM and +AT × AT × PP × PP × PT × TM → +PT × TM. What remains is the alias. When effects are aliases, they are indicated by an equals (=) symbol, e.g., AT = +PP × PT × TM and AT × PP = +PT × TM.

What would have happened if the − treatments for AT × PP × PT × TM had been used instead of the + treatments, i.e., −AT × PP × PT × TM had been used as the defining contrast? Everything would be the same except that the aliases would be slightly different. An examination of the sequences of +'s and −'s for AT and its alias, PP × PT × TM, associated with the − treatments of AT × PP × PT × TM (Table 2) shows that the treatments involved are the same, but the signs are exactly reversed.

$$\begin{aligned} AT &= T9 + T10 - T11 - T12 \\ &\quad + T13 + T14 - T15 - T16 \\ PP \times PT \times TM &\\ &= -T9 - T10 + T11 + T12 \\ &\quad - T13 - T14 + T15 + T16 \end{aligned}$$

Instead of saying AT = PP × PT × TM, we

would say that AT = − PP × PT × TM or −AT = PP × PT × TM. Using the − treatments will only affect the sign (+ or −) of the aliases; e.g., if −PP × PT were the defining contrast, the aliases of AT and PT would be −AT × PP × PT and −PP, respectively.

Choosing − treatments is neither better nor worse than choosing + treatments for the defining contrast; it is only different. Depending upon the true values of the aliased effects, however, the results of using the two halves can be very different. For example, to estimate the main effect of assembly time, we assume that PP × PT × TM is negligible. Thus, when we calculate AT, which we know is really AT + PP × PT × TM, we will say the entire effect is AT. But what if the effects of AT and PP × PT × TM were both +5? If the + treatments are used, AT = PP × PT × TM and the estimated effect for AT + PP × PT × TM would be +10, an overestimate of AT. If the − treatments were used, AT = −PP × PT × TM and the estimated effect of AT + PP × PT × TM would be 0, an underestimate of AT. Therefore, if the alias of a main effect is not negligible, the estimate of the main effect can be very wrong. If PP × PT × TM were small, either the + or the − treatments would give similar results. Because we never know what the truth is, we can use either half with equal confidence.

Analysis of data for a factorial replication is not difficult. It can be done by hand, as discussed in chapter 6A of Cochran and Cox (1957), or by some statistical analysis programs.

The analysis of variance for the two replicates of the treatments that are + for AT × PP × PT × TM (a one-half replicate) in Table 2 is presented in Table 5. The analysis yields an interpretation similar to the one obtained from Table 3.

A comparison of Tables 3 and 5 shows some interesting similarities and dissimilarities in the results of the analyses of the complete factorial and the one-half replicate. Table 3 has one effect for each of the 15 degrees of freedom for treatments, and Table 5 has one effect for each of the 7 degrees of freedom for treatments. Table 3 has four three-way interactions and one four-way interaction. The four-way interaction does not appear in Table 5 because it was the defining contrast. And although the four three-way interactions do not appear, they are present in the four main effects, AT, PP, PT, and TM. Table 5 also lists three degrees of freedom for two-way interactions. Because AT × PP = PT × TM, AT × PT = PP × TM, and AT × TM = PP × PT, the two-way interactions cannot be estimated and are lumped together. A general rule for estimating effects in a fractional replication is that a main effect can be estimated only if all of its aliases are two-way interactions or higher, a two-way interaction can be estimated only if all of its aliases are three-way interactions or higher, and so on. Therefore, because AT × PP = PT × TM, neither AT × PP nor PT × TM can be estimated.

TABLE 5. *Analysis of variance of the effect of assembly time (AT), press pressure (PP), press temperature (PT), and press time (TM) on the percentage wood failure for a one-half replicate of data.*

Source	df	SS	F	Pr < F
Block	1	21.6	2.0	0.20
Treatments	7	5,682		
AT	1	48.4	4.4	0.07
PP	1	76.3	7.0	0.03
PT	1	0.3	0.0	0.87
TM	1	331	30.3	<0.01
Two-way interactions	3	143	4.4	0.05
Error	7	76.4		

The estimates of main effects of the four factors from the complete factorial and the one-half replicate are listed in Table 6. There is little difference in the estimates between the two studies. The three-way interactions in Table 3 seem to be small, i.e., they have large P values. Therefore, we would expect the mean differences for the main effects to be fairly close for the two studies.

Fractional replication should not be used with less than four factors, and with four factors only a one-half replicate can be done. But with more factors, even a greater degree of fractionation can be done. When two levels of

TABLE 6. *Estimate of main effect of assembly time, press pressure, press temperature, and press time from analysis of data as a complete factorial and a one-half replicate.*

Main effect	Complete	One-half
Assembly time	3.5	3.5
Press pressure	2.5	4.3
Press temperature	0.0	0.3
Press time	9.9	9.3

each factor are used, replicates can be of the form $(\frac{1}{2})^n$ where n is an integer.

Although we would not do a one-quarter replicate on a four-factor experiment, we will do it with the plywood experiment as an example of how it would be done. A one-quarter replicate, $(\frac{1}{2})^2$, requires two defining contrasts. +AT × PP × PT × TM and +PP × PT × TM will be used as the two defining contrasts. The top four rows of Table 2 list the four treatments that are + for AT × PP × PT × TM and PP × PT.

Because AT × PP × PT × TM and PP × PT were used as defining contrasts, they appear only as +'s for the four treatments and thus cannot be estimated. An examination of the four treatments in Table 2 shows that all of the other treatment effects have 2 +'s and 2 −'s except AT × TM, which is all +'s. AT × TM, like AT × PP × PT × TM and PP × PT, cannot be estimated because it is all +'s. +AT × TM is called the generalized interaction of the two defining contrasts, and like the defining contrasts, its effect cannot be estimated because only the + (or −) levels of it are studied. The generalized interaction is a main effect or interaction that cannot be estimated because only its + or − treatments remain after the defining contrasts are chosen.

Inasmuch as one-quarter of the 16 treatments is 4, there are only 3 treatment effects that can be estimated for the one-quarter replicate. Aliases for a one-quarter replicate are calculated just as they are for a one-half replicate, except that each effect is multiplied by the two defining contrasts and the generalized interaction, which results in three aliases. Aliases for AT are AT × (AT × PP × PT × TM) = PP × PT × TM, AT × (PP × TM) = AT × PP × TM, and AT × (AT × TM) = TM. The aliases form the three groups listed below.

Group 1 AT = TM = AT × PP × PL =
= PP × PT × TM
Group 2 PP = PT = AT × PP × TM =
= AT × PT × TM
Group 3
AT × PP = AT × PT =
= PT × TM = PP × PT

This is a terrible design because AT = PT and PP = PT. If the effect for Group 1 were significant, a researcher would not be sure if it were due to AT or PT or both AT and PT, assuming that AT × PP × PT and PP × PT × TM were negligible. Main effects should almost never be confounded with each other.

What if two other effects, say +AT × PP × PT and −PP × PT × TM, were chosen as defining contrasts? Their generalized interaction would be −AT × TM, and the three groups of aliases are listed below.

Group 1 AT = −TM = PP × PT =
= −AT × PP × PT × TM
Group 2 PP = AT × PT =
= −PT × TM =
= −AT × PP × TM
Group 3 PT = AT × PP =
= −PP × TM =
= −AT × PT × TM

This set, though not good, is somewhat better than the previous set, in that PP and PT are not aliased with other main effects, but AT = −TM. Inasmuch as there are only three groups and four main effects, at least two main effects will always be aliases. Therefore, a one-quarter replicate of a four-factor experiment should almost never be done.

Note that when the defining contrasts are changed, so are the aliases. A careful choice of defining contrasts can leave main effects al-

iased with high-order interactions, which is desirable, or with other main effects, which can be disastrous. Fortunately, sets of defining contrasts that allow estimation of all main effects and as many low-order interactions as possible have been developed. See pages 276–292 in Cochran and Cox (1957) for a number of sizes of experiments and various degrees of fractionation.

A four-factor experiment is a small experiment, so let's consider the problem of confounding on an eight-factor experiment with each factor at two levels. For a one-half replicate of the 256 treatments, using the eight-way interaction as the defining contrast, each of the eight main effects would have a seven-way interaction as its alias, each of the 28 two-way interactions would have a six-way interaction as an alias, and each three-way interaction would have a four-way interaction for an alias. If we assumed that all four-way and higher interactions were negligible, the one-half replicate would allow estimation of all main effects and two-way and three-way interactions. The main effects and two-way interaction would be very clean.[3] If a one-quarter replicate was done by using two carefully chosen five-way interactions as the defining contrasts, all main effects would have three-way and higher interactions as aliases. Most two-way interactions would be aliased with three-way and higher interactions. The remaining two-way interactions would have aliases that are four-way interactions or higher. For a one-eighth replicate developed with three carefully selected four-way interactions as defining contrasts, each main effect would be aliased with three-way and higher interactions. All individual two-way interactions of two of the factors could be estimated, but none of the others. Even using only 32 of the 256 treatments, a one-eighth replicate, the eight main effects are still fairly cleanly estimated.

As the study on plywood production was conducted as a one-eighth replicate of an eight-factor experiment, we present the results of the analysis of the data as it should be done. The treatment combinations used in the study and the percentage wood failure of the two replicates are listed in Table 7. The analysis of variance of the data, analyzed as a randomized complete block design, is presented in Table 8. The two-factor interactions that are aliased with other two-factor interactions are grouped together, as are the three-way interactions.

The aliased two-factor and three-factor interactions are not very strong. If the analysis indicated strong interactions, another fractional replicate could be done to help identify the interacting factors.

Only a few two-way interactions can be examined with this design, and two of them, prepress by moisture content (PR × MC) and assembly time by press time (AT × TM), seem substantial. The prepress by moisture content interaction ($P = 0.05$) appears to be due to a large difference in percentage of wood failure at a moisture content of 4.7% (no prepress = 12.7% and prepressed = 17.2%), whereas there was little difference and it was in the opposite direction at a moisture content of 7.2% (no prepress = 14.6% and prepressed = 13.6%).

The assembly time by press time interaction ($P = 0.02$) seems to be due to a small difference in the percentage of wood failure for 0.5-min assembly time (6 min = 13.8% and 9 min = 11.9%); for 10-min assembly time, the difference was large and in the opposite direction (6 min = 14.1% and 9 min = 18.4%).

Increasing press pressure from 200 psi to 300 psi decreased the percentage of wood failure from 15.9% to 13.2% ($P = 0.03$), a 17% reduction. Decreasing the extender content by 50% decreased the percentage of wood failure

[3] Clean is not a statistical term but a concept that deals with aliases. An effect that has little influence from its aliases is cleanly estimated. The greater the difference between the effect of interest and its aliases in terms of number of interacting factors, the less likely the aliases will have an effect on the estimated value of the effect of interest. As a two-way interaction is closer to a main effect than a five-way interaction, a main effect aliased with a two-way interaction would be considered to be less clean than a main effect aliased with a five-way interaction.

TABLE 7. *Treatment combinations of assembly time (AT), press pressure (PP), press temperature (PT), press time (TM), prepressing (PR), veneer moisture content (MC), glue mix NaOH content (NA), and glue mix extender content (EX) evaluated in one-eighth replicate and percentage wood failure for two replicates.*

Treatment #	Factor									
	AT (min)	PP (psi)	PT (°F)	TM (min)	PR	MC (%)	NA (%)	EX (%)	Rep 1	Rep 2
1	0.5	200	285	6	No	4	3	4	23.1	22.2
2				6	Yes	8	3	8	11.5	6.2
3				9	No	8	6	8	8.1	10.8
4				9	Yes	4	6	8	13.3	15.6
5			350	6	No	8	6	4	26.4	7.8
6				6	Yes	4	6	8	8.3	10.0
7				9	No	4	3	8	16.7	15.6
8				9	Yes	8	3	4	22.2	14.7
9		300	285	6	No	4	6	8	11.9	12.2
10				6	Yes	8	6	4	6.6	18.6
11				9	No	8	3	4	13.1	8.6
12				9	Yes	4	3	8	6.8	9.4
13			350	6	No	8	3	8	13.6	6.9
14				6	Yes	4	3	4	15.6	19.4
15				9	No	4	6	4	12.8	11.1
16				9	Yes	8	6	8	4.5	6.9
17	10	200	285	6	No	8	3	8	7.2	12.5
18				6	Yes	4	3	4	10.0	18.6
19				9	No	4	6	4	25.3	28.6
20				9	Yes	8	6	8	18.6	22.8
21			350	6	No	4	6	8	8.1	11.7
22				6	Yes	8	6	4	31.9	10.8
23				9	No	8	3	4	21.1	23.9
24				9	Yes	4	3	8	16.6	13.9
25		300	285	6	No	8	6	4	17.5	18.3
26				6	Yes	4	6	8	6.9	9.4
27				9	No	4	3	8	12.3	20.0
28				9	Yes	8	3	4	12.2	26.1
29			350	6	No	4	3	4	20.8	22.2
30				6	Yes	8	3	8	7.3	12.2
31				9	No	8	6	8	8.7	13.8
32				9	Yes	4	6	4	19.4	15.3

from 17.9% to 11.2%, a 37.4% reduction ($P < 0.01$). The press temperature and NaOH content of the glue mix did not affect glue-bond quality ($P = 0.88$ and $P = 0.45$, respectively).

Of all the variables tested in this experiment, glue-mix extender content had the most significant effect on bond quality. Altering assembly time, press pressure, and press time also resulted in improvements in bond quality. However, adhesive bond quality was still far below the minimum acceptable level outlines by Product Standard PS 1-74. Therefore, further improvements need to be made in bond quality if white oak is to be used as furnish for exterior plywood.

RECOMMENDATIONS

Fractional replication can be a very useful technique for experiments with a large number of factors. Its use does have a price, but with considerable forethought and some knowledge of the factors, the problem of aliases can be minimized.

If you are interested in trying fractional replication, there are two ways to approach it. The easiest and probably the safest is to start with

TABLE 8. *Analysis of variance of effect of assembly time (AT), press pressure (PP), press temperature (TM), press time (TM), prepressing (PR), veneer moisture content (MC), glue mix NaOH content (NA), and extender content (EX) on percentage wood failure.*

Source	df	SS	*F*	PR > *F*
Block	1	5.1	0.20	0.66
Treatment	30	1,730		
AT	1	202	8.0	0.01
PP	1	136	5.4	0.03
PT	1	0.6	0.0	0.88
TM	1	28.5	1.1	0.30
PR	1	40.6	1.6	0.21
MC	1	15.4	0.6	0.44
NA	1	14.4	0.6	0.45
EX	1	700	28	<0.01
PR × AT	1	2.0	0.0	0.78
PR × PP	1	0.2	0.0	0.93
PR × PT	1	11.4	0.5	0.51
PR × TM	1	11.3	0.5	0.51
PR × MC	1	102	4.1	0.05
PR × NA	1	8.2	0.3	0.57
PR × EX	1	2.9	0.1	0.73
AT × PP	1	3.8	0.1	0.70
AT × PT	1	8.4	0.3	0.57
AT × TM	1	166	6.6	0.02
AT × MC	1	29.6	1.2	0.29
AT × NA	1	40.6	1.6	0.21
AT × EX	1	12.9	0.5	0.48
Other two-way interactions	7	189.5	1.1	0.36
Three-way interactions	3	4.7	0.1	0.92
Error	32	805.6		

help from a statistician who works in the area of experimental design. After reading this article, you should be able to describe what you want to do and understand the problems associated with fractional replication.

If you do not have a statistician available and(or) are adventurous, you can do one by yourself. To do it yourself, however, you need to learn more about the technique. There are two excellent references that should be studied. Chapter 6A in Cochran and Cox (1957) covers fractional replication. It also presents a number of designs, complete with defining contrasts and a description of aliases of main effects and two-way interactions for experiments with four to eight factors at two levels and for some experiments with factors at various levels. Reading chapter 6 on confounding before chapter 6A would be helpful. Chapters 12 and 13 in Box et al. (1978) present a good description of fractional replication, along with a number of well-described applications of its use. The terminology used by Box et al. is a bit different from ours, which follows that of Cochran and Cox. Also, reading chapters 10 and 11 on factorial experiments before chapters 12 and 13 in Box et al. would help in understanding the content. Fractional replications generally involve only two levels of each factor, but information on the use of three levels or more can be found in Cochran and Cox and Box et al.

If you are going to do a fractional replication, here are some recommendations.

Before the experiment:

1. Start conservatively. Try a one-half or one-quarter replicate to start.

2. After deciding upon the factors to be studied and the degree of fractionation to be used, determine aliases for the effects of interest and study them. Decide if these effects are clean enough. If they are not, you need different defining contrasts and(or) less fractionation.
3. Randomly select the + or − halves of the defining contrasts. Also, decide upon randomization to be done in treatment assignment.
4. Decide on adequate replication.
5. Decide how data will be analyzed. Some well-known statistical packages can analyze the data, but you will probably need help from a person familiar with the package.

After the experiment:

1. Study the analysis very carefully. Consider the discussion of Box et al. (1978) on examples where aliased interactions can make some nonsignificant main effects seem to be significant.
2. If you are unsure of the cause of a certain effect, consider doing another replicate to clear up questions. Box et al. (1978) discussed the use of a series of fractional replications, each with a different defining contrast, to solve questions about aliases.

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