

The 2-Stage Nested Designs

So far emphasis has been placed on factorial experiments where all factors are crossed (i.e., it is possible to study the same FLCs of all factors in a completely randomized order). In such experiments, all interactions of any order amongst factors can be studied. However, there are true-life experiments where the levels of a factor, say B, cannot be crossed with levels of another factor, say A, but the levels of B are nested under those of factor A. For the sake of illustration, consider the Example 14.1 on pp. 607-612 of Montgomery(8e), where a company buys raw material in batches (factor B) from 3 different suppliers (factor A). To understand this concept better, I have reproduced Montgomery's Table 14.3 at the bottom of page 607, as shown below, where B nested under A is a random factor, i.e., each of the 4 batches were selected at random from the 3 suppliers' lots.

Table (14.3) on p. 607 of Montgomery's 8th Ed. (y_{ijk} = Purity – 93 %)

Factor A	Supplier 1				Supplier 2				Supplier 3			
Batches B(A)	1	2	3	4	5	6	7	8	9	10	11	12
	1	-2	-2	1	1	0	-1	0	2	-2	1	3
	-1	-3	0	4	-2	4	0	3	4	0	-1	2
	0	-4	1	0	-3	2	-2	2	0	2	2	1
$y_{ij.}$	0	-9	-1	5	-4	6	-3	5	6	0	2	6
$y_{i..}$	-5				4				14			

Montgomery's Table (14.3) clearly shows that Batches 1, 5, and 9 do not represent the same levels of factor B because Batch 1 belongs to the Supplier 1 (A_1), batch 5 belongs to A_2 and batch 9 belongs to the supplier 3, i.e., there is absolutely no way to cross the levels of B with those of A. In short, we say the levels of factor B are nested within (or under) the levels of A; the nesting of B under A is statistically exhibited as B(A). The statistical model for the above 2-stage nested design, with $n = 3$ responses per cell, is given by

$$y_{ijk} = \mu + A_i + B_{j(i)} + \epsilon_{(ij)k} \quad (18)$$

where $A_i = \mu_i - \mu$, $B_{j(i)} = \mu_{j(i)} - \mu_i$, and $\epsilon_{(ij)k} = y_{ijk} - \mu_{j(i)}$. Note that there is no interaction term in

the above model (18) because A and B cannot be crossed. The notation $B_{2(1)} = \mu_{2(1)} - \mu_1$ simply means the effect of Batch 2 from supplier 1, which should not be estimated because batches were selected at random from suppliers' lots. We will estimate the variance component $\sigma_{B(A)}^2$ after the ANOVA is presented. If factor A in Eq. (18) is fixed, then it is constrained to

$$\sum_{i=1}^a A_i \equiv 0.$$

The USS is computed in the usual manner as $USS = 153$ (with 36 *df*) and the $CF = 13^2/36 = 4.694444$. Thus, $SS_T = 148.3055\bar{5}$ (with 35 *df*). The $SS(A)$ is also computed in the usual manner as $SS(A) = SS(\text{Suppliers}) = [(-5)^2 + 4^2 + 14^2]/12 - CF = 15.05555$. However, $SS(\text{Batches})$ cannot be computed in the usual manner because of nesting; thus, we proceed as follows:

$$SS(B(A_1)) = [0^2 + (-9)^2 + (-1)^2 + 5^2]/3 - (-5)^2/12 = 33.5833\bar{3},$$

$$SS(B(A_2)) = [(-4)^2 + (6)^2 + (-3)^2 + 5^2]/3 - (4)^2/12 = 27.3333\bar{3},$$

and similarly $SS(B(A_3)) = 9.0$, each with 3 *df*. Hence, $SS_{B(A)} = 33.5833\bar{3} + 27.3333\bar{3} + 9.00 =$

$69.9166\bar{6}$ (with 9 *df*). In order to determine the denominators of the F statistics in the ANOVA table, we must develop the EMS column for this 2-stage nested design, which is shown in Table 24.

Table 24. (F = Fixed factor, R = Random Factor)

Model Terms	F 3 i	R 4 j	R 3 k	EMS
A_i (Supplier _i)	0	4	3	$12\Phi_A + 3\sigma_{B(A)}^2 + \sigma_{\epsilon}^2$
$B_{j(i)}$ (Batch _j)	1	1	3	$3\sigma_{B(A)}^2 + \sigma_{\epsilon}^2$
$\epsilon_{(ij)k}$	1	1	1	σ_{ϵ}^2

The above EMS column in Table 24 clearly indicates that factor A must be tested against $MS_{B(A)}$, i.e., the so-called error term for factor A is B(A), but the denominator of the F statistic for factor B(A) must be MS(Error), which has $2 \times 12 = 24$ *df*. The proper ANOVA table is shown

atop page 609 of Montgomery(8e) in his Table 14.6.

In order to point-estimate the variance component $\sigma_{B(A)}^2$, we make use of the EMS column of the above Table 24, replacing parameters with their unbiased moment estimators, i.e., $MS_{B(A)} = 3 \hat{\sigma}_{B(A)}^2 + \hat{\sigma}_{\epsilon}^2 \rightarrow \hat{\sigma}_{B(A)}^2 = (7.76852 - 2.63889)/3 = 1.710$. Because factor A is fixed, then its effect point estimates are $\hat{A}_1 = \bar{y}_{1..} - \bar{y}_{...} = (-5/12) - 13/36 = -0.7777\bar{7}$, $\hat{A}_2 = \bar{y}_{2..} - \bar{y}_{...} = (4/12) - 13/36 = -0.02777\bar{7}$, $\hat{A}_3 = \bar{y}_{3..} - \bar{y}_{...} = 0.8055\bar{5}$ and $\sum_{i=1}^3 \hat{A}_i \equiv 0$, as expected. Note that $SS(A) = \sum_{i=1}^3 \sum_{j=1}^4 \sum_{k=1}^3 \hat{A}_i^2 = 12 \sum_{i=1}^3 \hat{A}_i^2 = 15.05555\bar{5}$, which is consistent to 2

decimals with Montgomery's ANOVA Table atop page 608.

Montgomery(8e) discusses residuals from a 2-factor nested experiment of the nested model (18) on his pages 609-611. By definition a residual is the difference between the actual observed value of y obtained from the experiment and the corresponding fitted value from the pertinent model, which in this case is given by $\hat{y}_{ijk} = \hat{\mu} + \hat{A}_i + \hat{B}_{j(i)} + \hat{\epsilon}_{(ij)k}$. Because the best predictor of $\epsilon_{(ij)k}$ is zero, the model predictor for y_{ijk} reduces to $\hat{y}_{ijk} = \hat{\mu} + \hat{A}_i + \hat{B}_{j(i)} = \bar{y}_{...} + (\bar{y}_{i.} - \bar{y}_{...}) + (\bar{y}_{j(i).} - \bar{y}_{i.}) = \bar{y}_{j(i)}$. $\rightarrow e_{ijk} = y_{ijk} - \hat{y}_{ijk} = y_{ijk} - \bar{y}_{j(i)}$. For the Example 14.1 of Montgomery(8e) on p. 607, $e_{312} = y_{312} - \bar{y}_{1(3)} = 4 - (6/3) = 2$, which matches the value in the 26th row of Montgomery's Table at the bottom of his page 610.

For a 3-stage nested design, study pp. 612-615 of Montgomery's 8th edition, for which I am providing the EMS column of problem 14.5 on pages 637-8 of Montgomery(8e), in the

Table 25 atop the next page, where $\Phi_{B(A)} = \sum_{i=1}^2 \sum_{j=1}^3 (\mu_{j(i)} - \mu_{i.})^2 / 4$.

Exercise 40. Work problems 14.5 and 14.7 on pages 673-8 of Montgomery(8e).

Nesting can also occur in a factorial experiment, where two factors A and B are crossed but factor C is nested under either factor A or B. Montgomery(8e) provides a good example of such an experiment in Example 14.2 on pp. 616-620, where factor F =

Table 25. (The EMS for Problem 14.5 of Montgomery); C = C(B(A))

Model Terms	F 2 i	F 3 j	R 2 k	R 2 r	EMS
A _i (Alloy)	0	3	2	2	$12\Phi_A + 2\sigma_C^2 + \sigma_\epsilon^2$
B _{j(i)} (Heats)	1	0	2	2	$4\Phi_{B(A)} + 2\sigma_C^2 + \sigma_\epsilon^2$
C _{k(ij)} (Ingots)	1	1	1	2	$2\sigma_C^2 + \sigma_\epsilon^2$
$\epsilon_{(ijk)r}$	1	1	1	1	σ_ϵ^2

Fixtures at 3 fixed levels, L = Layouts at 2 fixed levels, but factor O = "Operators" at 4 random levels nested under the levels of factor L. This implies that there were actually 8 operators in the experiment who were selected at random; operators 1, 2, 3, and 4 with 3 *df* are nested within Layout 1, and operators 5, 6, 7, and 8 with 3 *df* are nested under L₂. Therefore, operators carry 3 + 3 = 6 *df* in the entire experiment, and n = 2 determinations of assembly time, y_{ijk_r} , were made at each FLC. The EMS column is provided in Table 14.13 on page 618 of Montgomery(8e), and is reproduced below in my notation.

Tables 14.11 & 14.13 on pages 617-8 of Montgomery(8e) (Restricted Model)

Model Terms	F 3 i	F 2 j	R 4 k	R 2 r	EMS	<i>df</i>
F _i	0	2	4	2	$16\Phi_F + 2\sigma_{F \times O(L)}^2 + \sigma_\epsilon^2$	2
L _j	3	0	4	2	$24\Phi_L + 6\sigma_{O(L)}^2 + \sigma_\epsilon^2$	1
(F×L) _{ij}	0	0	4	2	$8\Phi_{F \times L} + 2\sigma_{F \times O(L)}^2 + \sigma_\epsilon^2$	2
O _{k(j)}	3	1	1	2	$6\sigma_{O(L)}^2 + \sigma_\epsilon^2$	6
(F×O) _{ik(j)}	0	1	1	2	$2\sigma_{F \times O(L)}^2 + \sigma_\epsilon^2$	12
$\epsilon_{(ijk)r}$	1	1	1	1	σ_ϵ^2	24

Exercise 41. Work problems 14.13 and 14.14 on pages 638-9 of Montgomery(8e).

THE SPLIT-PLOT DESIGN

We have extensively discussed the randomized complete block designs in Chapter 4, where complete randomization is carried out within a block. In some experiments, complete randomization of all FLCs within a block is either too time-consuming, too impractical, too expensive, or a combination thereof. As a result, the experimenter has to carry out randomization in 2 stages within a block (or replicate), where the levels of one factor are randomized first (this factor is called the whole plot), and then the levels of a second factor (called the split-plot) are next randomized within the randomly selected level of the whole plot. To illustrate, consider the split-plot experiment of problem 14.19 of Montgomery(8e) on his page 639, where oven temperature (A) is at 2 fixed levels (1500, 1600 °F) and heat treatment time (factor B) is at 3 fixed levels 10, 20, and 30 minutes. On shift 1 (or block 1, or replicate 1), a temperature is selected at random, say $A_2 = 1600$ °F, by a coin flip. This is the 1st stage of randomization, where one of A_1 or A_2 is selected at random. For the sake of argument, let's assume that the level A_2 is selected at random 1st, followed by A_1 on shift 1. The factor of (most) interest is heat treatment time (B) at 3 fixed levels 10, 20, and 30 minutes. At the 2nd stage, a time is selected at random from 10, 20, and 30 minutes. Note that there is only one restriction on randomization, namely that the levels of A (the factor whose levels are hardest to change) are randomized 1st followed by those of the split-plot B within the randomly selected level of A. A randomized complete block design requires that the 6 FLCs of A and B, [(1500, 10), (1500, 20), (1500, 30), (1600, 10), (1600, 20), (1600, 30)], be completely randomized within a shift (or block, or replicate), but this is too impractical and too time-consuming! Because once the oven (only one oven) is heated to the 1st randomly-selected temperature 1600, it will be more practical to insert 3 specimen of steel (Instead of one; the alternative of inserting 12 specimen leads to Duplicate-Measurements) into the oven and randomly treat one of the 3 specimen for 10 minutes, another randomly selected component at 20 minutes and the last specimen for 30 minutes. However, the drawback of such a design is that one of our main factors, namely oven temperature, is actually confounded with plots. That is, if experimental conditions change from one oven temperature to another, such uncontrollable changes will also show up as oven temperature effect. Further, there is only one restriction on randomization because at the 1st stage, randomization is restricted only to

the levels of oven temperature. This 2-stage randomization (with one restriction, or one split) is carried out each time for all 4 shifts (or replicates R) until all $N = 4 \times 2 \times 3 = 24$ observations are collected. The statistical model for this experiment is provided below:

[The whole-plot] [The split-plot]

$$y_{ijk} = \mu + [R_i + A_j + (R \times A)_{ij}] + [B_k + (R \times B)_{ik} + (A \times B)_{jk} + (R \times A \times B)_{ijk}].$$

Note that the 1st bracketed term on the RHS of the above model represents the whole plot, while the last bracketed term on the RHS presents the split plot.

Table 26 below presents the EMS column for the above split-plot experiment. The EMS column in Table 26 shows that the denominator of the F statistic for testing $H_0: A_i = 0$ has only

Table 26. (r denotes repeat observation in each replicate)

Model Terms	R	F	F	R	EMS	df
	4	2	3	1		
	i	j	k	r		
R_i	1	2	3	1	$6\sigma^2_R + \sigma^2_\epsilon$	3
A_j	4	0	3	1	$12\Phi_A + 3\sigma^2_{R \times A} + \sigma^2_\epsilon$	1
$(R \times A)_{ij}$	1	0	3	1	$3\sigma^2_{R \times A} + \sigma^2_\epsilon$	3
B_k	4	2	0	1	$8\Phi_B + 2\sigma^2_{R \times B} + \sigma^2_\epsilon$	2
$(R \times B)_{ik}$	1	2	0	1	$2\sigma^2_{R \times B} + \sigma^2_\epsilon$	6
$(A \times B)_{jk}$	4	0	0	1	$4\Phi_{A \times B} + \sigma^2_{R \times A \times B} + \sigma^2_\epsilon$	2
$(R \times A \times B)_{ijk}$	1	0	0	1	$\sigma^2_{R \times A \times B} + \sigma^2_\epsilon$	6
$\epsilon_{(ijk)r}$	1	1	1	1	σ^2_ϵ (not retrievable)	0

The Whole-Plot

The Split-Plot

3 df, but the test for B (the factor of most interest in the split) does have 6 df in the denominator. For this reason and the fact that one factor is always confounded with plots, it is generally best to place the factor of most interest and easy-to-change levels in the split if experimentally feasible. I coded the data of Problem 14.19 (p. 639 of Montgomery8e) by subtracting 50 and then used SAS to obtain the ANOVA Table. For the coded data, the $USS = 11859$, $CF = 423^2/24 = 7455.375$, and the $SS_T = 4403.625$. In response to the question during a

class (December 4, 2002), I must categorically state that in the above split-plot experiment of problem 14.19 each specimen of steel was used only once (i.e., the measurement on each specimen was not repeated), and therefore, a total of $N = 24$ specimens were used in the entire 4 shifts. Since no repeat observations could be gathered from any of the 24 specimens, the above split-plot experiment could not provide any degrees of freedom for the Pure Error (or replication error) SS in the ANOVA Table. This is in quite contrast to the R&R Study of the Example 13.1 on pages 575-578 of Montgomery's 8th edition, where each operator gathered 2 observations at random order (i.e., not necessarily successively) from each of the same part with the same gauge and hence each of the $60 = 3 \times 20$ cells provided exactly one *df* for the SS(PE), and the Minitab ANOVA of Table 13.2 on page 577 of Montgomery(8e) confirms 60 degrees of freedom for the pure experimental error sum of squares. Of course, the experimenter may decide to insert 12 specimen of steel at the same time into the oven once a temperature level is selected at random and randomly heat 4 of them for 10 minutes, 4 of them for 20 minutes, and the last 4 for 30 minutes. However, this does not create true replication error because the 4 specimen are being heated at the same time (resulting in much less variability in the response) and hence not a CRD (see the problem with Duplicate Measurements in Example 6.5 on pp. 274-278 of Montgomery8e).

The SAS codes and the associated ANOVA Table for the Problem 14.19 on p. 639 of Montgomery(8e) are provided below.

SAS Codes:

```
data Prb14_19;
input R @;
do j =1 to 2; input A @@; do k =1 to 3; input B @@;
input y @@; output; end; end;
datalines;
1 1500 10 13 20 4 30 11 1600 10 39 20 41 30 12
2 1500 10 0 20 2 30 9 1600 10 30 20 22 30 19
3 1500 10 -2 20 24 30 21 1600 10 23 20 31 30 19
4 1500 10 4 20 -2 30 9 1600 10 38 20 42 30 14
;

proc anova;
title ' '; title2' The split-plot Experiment of';
title3' Problem 14-19 on p. 556 of Montgomery's 6th edition, data coded by 50';
classes R A B ;
```

Model $y = R|A|B$;
 test H = A E = R*A; test H=B E = R*B;
 test H = A*B E =R*A*B; run;

Anova					
Source	DF	SS	Mean Square	F Value	Pr > F
R	3	145.45833333	48.48611111	.	.
A	1	2340.37500000	2340.37500000	.	.
R*A	3	240.45833333	80.15277778	.	.
B	2	159.25000000	79.62500000	.	.
R*B	6	478.41666667	79.73611111	.	.
A*B	2	795.25000000	397.62500000	.	.
R*A*B	6	244.41666667	40.73611111	.	.

Tests of Hypotheses using the Anova MS for R*A as an error term

Source	DF	SS	Mean Square	F Value	Pr > F
A	1	2340.3750	2340.37500	29.20	0.0124

Tests of Hypotheses using the Anova MS for R*B as an error term

Source	DF	SS	Mean Square	F Value	Pr > F
B	2	159.2500	79.62500000	1.00	0.4223

Tests of Hypotheses using the Anova MS for R*A*B as an error term

Source	DF	SS	Mean Square	F Value	Pr > F
A*B	2	795.2500	397.62500	9.76	0.0130

Since the above SAS output shows that $A \times B$ is highly significant, then these two factors have to be crossed in order to determine the optimum X_0 bearing in mind that $y = \text{“Strength”}$ is an LTB type QCH.

To better understand the differences between randomization order of a CR factorial and split-plot experiment, I have provided, in Tables 27, one possible run order (of both design types) for the replicate I of problem 14.19 of Montgomery(8e).

Exercise 42. Provide another possible experimental run orders for the other 3 replicates of problem 14.19 of Montgomery(8e). (b) Study the OA on the next page , which is

Tables 27

CRD Factorial

Time \ Temp	1500	1600
10 Min	2	3
20	5	1
30	4	6

Split-Plot Design

Time \ Temp	1500	1600
10	6	3
20	4	1
30 Min	5	2

unrelated to Problem 14.19, and determine which factor is the whole-plot and which is the split-plot.

OA

Run Order : 1 2 3 4 5 6 7 8 9 10 11 12
 Replicate : | | | | | | | | | | | |
 Levels of B : 2 4 1 3 2 3 4 1 1 3 2 4
 Levels of A : 2 2 2 2 1 1 1 1 3 3 3 3

As Montgomery points out on page 625 of his 8th edition, the split-plot design has its origin in agriculture, where as a specific example, 3 different types of crop (or seeds, namely factor A) are randomly assigned to 3 large fields (or plots) at the 1st stage of randomization (or 1st restriction). Note that the levels of factor A (= Seed Type) are actually confounded with plots (or fields) because differences in fields will also be part of differences in seed types (each field containing only one seed type assigned at random). Thus, the three seed types are called whole-plots because they are confounded with plots. However, the main objective of the experiment is to ascertain if 4 types of fertilizers have a significant impact on crop yield. At the 2nd stage of randomization, the three fields (or large plots) are equally subdivided (or split) into 4 subplots, where the 4 types of fertilizers (called split plots) are randomly assigned to the 4 subplots. It is essential to place the factor of the most interest (fertilizer) in the split-plot. Each replicate of such an experiment will involve exactly 3 fields, and thus for 5 full replicates, we must make use of 15 fields (or large plots of land).

The Split-Split-Plot Design

These are experiments where there are 3 stages of randomization within the same replicate but 2 restrictions on randomizations. As an example, consider the experiment (described by C. R. Hicks and K. V. Turner, *Fundamental Concepts in the Design of Experiments*, p. 300, Oxford Press, ISBN:0-19-512273-9) where the effects of factors L = "Laboratory" at 3 fixed levels 1, 2, 3, T = "Temperature" at 3 fixed levels 145, 155, 165 (°C), and M = "Mix types" at 3 fixed levels 1, 2, 3 on the cure rate, y , of rubber samples are being studied. Material for the 3 Mixes are sent to one of the randomly selected labs (1st restriction), where an experiment is performed by selecting one of the 3 temperatures at random (2nd restriction) and heating the oven to that temperature, and then placing the rubber of all 3 mixes into the oven and measuring the resulting cure rate of the 3 samples in the oven at the randomly selected temperature. Clearly, the levels of L are randomized 1st so that Lab is the whole plot; then levels of T are randomized within Lab_{*i*} (*i* = 1, 2, 3) so that Temperature (T_j) forms a split plot; finally, the 3 mixes (i.e., the 3 samples of each Mix) are randomly selected within T_j , forming what is called the split-split plot. A possible randomization order for the 1st nine experiments are provided below.

Run No. :	1	2	3	4	5	6	7	8	9
Replicate :	1	1	1	1	1	1	1	1	1
L _{<i>i</i>} :	3	3	3	3	3	3	3	3	3
T _{<i>j</i>} :	2	2	2	3	3	3	1	1	1
M _{<i>k</i>} :	3	2	1	1	3	2	2	3	1

The resulting coded data (by subtracting 5) for all 3 replicates are provided in Table 28. Table 29 provides the EMS column for the above split-split plot design. The $R \times L$ interaction is called the whole-plot error, the $R \times L \times T$ and $R \times T$ interactions are referred to as the split-plot errors (sometimes only $R \times L \times T$ is called the split-plot error). It should be obvious to the reader as to what interactions should qualify as the split-split plot error. Most statisticians believe that if there are not sufficient df for testing main factors (L, T, M) and their interactions, then some of the interactions that include R (= replicates) may be pooled to obtain more df for the tests. However, the pooling must be done within plots, split plots, and split-split plots. This means

Table 28

R	L	T								
		145			155			165		
		M ₁	M ₂	M ₃	M ₁	M ₂	M ₃	M ₁	M ₂	M ₃
I	1	13.6	9.5	16.1	4.5	2.8	6.2	0.4	0.2	1.3
	2	15.0	13.4	17.5	6.4	5.8	8.3	1.8	1.0	2.7
	3	14.7	11.3	17.7	4.3	4.1	6.3	1.7	0.7	1.6
II	1	12.0	10.8	15.8	4.4	3.3	5.0	0.3	0.0	1.4
	2	15.1	13.1	17.7	6.5	6.1	9.0	1.9	1.2	3.0
	3	13.3	11.7	16.9	5.2	4.2	6.0	1.0	0.5	1.5
III	1	13.7	11.5	16.8	4.5	3.9	6.5	0.7	-.7	0.8
	2	14.4	11.5	16.5	6.4	4.5	7.0	1.0	0.0	1.6
	3	11.8	9.4	14.3	4.8	3.0	5.9	0.0	-.4	0.9

Table 29

Model Terms	R	F	F	F	EMS	DF
	3	3	3	3		
	r	i	j	k		
R _r	1	3	3	3	$27\sigma_R^2$	2
L _i	3	0	3	3	$27\Phi_L + 9(\sigma_{R \times L})^2$	2
(R×L) _{ri}	1	0	3	3	$9(\sigma_{R \times L})^2$	4
T _j	3	3	0	3	$27\Phi_T + 9(\sigma_{R \times T})^2$	2
(R×T) _{rj}	1	3	0	3	$9(\sigma_{R \times T})^2$	4
(L×T) _{ij}	3	0	0	3	$9(\Phi_{L \times T})^2 + 3(\sigma_{R \times L \times T})^2$	4
(R×L×T) _{rij}	1	0	0	3	$3(\sigma_{R \times L \times T})^2$	8
M _k	3	3	3	0	$27\Phi_M + 9(\sigma_{R \times M})^2$	2
(R×M) _{rk}	1	3	3	0	$9(\sigma_{R \times M})^2$	4
(L×M) _{ik}	3	0	3	0	$9\Phi_{L \times M} + 3(\sigma_{R \times L \times M})^2$	4
(R×L×M) _{rik}	1	0	3	0	$3(\sigma_{R \times L \times M})^2$	8
(T×M) _{jk}	3	3	0	0	$9\Phi_{T \times M} + 3(\sigma_{R \times T \times M})^2$	4
(R×T×M) _{rjk}	1	3	0	0	$3(\sigma_{R \times T \times M})^2$	8
(L×T×M) _{ijk}	3	0	0	0	$3\Phi_{L \times T \times M} + (\sigma_{R \times L \times T \times M})^2$	8
(R×L×T×M) _{rijk}	1	0	0	0	$(\sigma_{R \times L \times T \times M})^2$	16

that for the above design the best we can do for L is the 4 *df* of R×L, but we can attain 12 *df* for testing $H_0 : T_j = 0$, by pooling $SS(R \times T)$ and $SS(R \times L \times T)$. Similarly, we may obtain up to 36 *df* for testing the effect of M. I wrote a set of SAS codes to analyze the data in Table 28, whose output is given below.

The Example on pages 108-110 of INSY 7300-6 notes,
a Spit-Split Plot design 08:00 Wednesday, July 12, 2000

Analysis of Variance Procedure Dependent Variable: Y

Source	DF	Sum of Squares	Mean Square	F Value	Pr > F
Model	80	2570.4972840	32.1312160	.	.
Error	0				
CSS	80	2569.4972840			
R-Square		C.V.	Root MSE	Y Mean	
1.000000		0	0	6.74197531	

Source	DF	Anova SS	Mean Square	F Value	Pr > F
R	2	7.72246914	3.86123457	.	.
L	2	38.5558025	19.77790123	.	.
R*L	4	11.19234568	2.79808642	.	.
T	2	2332.17876543	1166.08938272	.	.
R*T	4	1.44493827	0.36123457	.	.
L*T	4	3.33827160	0.83456790	.	.
R*L*T	8	5.39135802	0.67391975	.	.
M	2	124.60024691	62.30012346	.	.
R*M	4	1.19012346	0.29753086	.	.
L*M	4	0.15234568	0.03808642	.	.
R*L*M	8	1.39061728	0.17382716	.	.
T*M	4	35.80049383	8.95012346	.	.
R*T*M	8	1.43358025	0.17919753	.	.
L*T*M	8	1.37802469	0.17225309	.	.
R*L*T*M	16	3.72790123	0.23299383	.	.

Tests of Hypotheses using the ANOVA MS for R*L as an error term

Source	DF	Anova SS	Mean Square	F Value	Pr > F
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L	2	38.55580247	19.77790123	7.07	0.0468
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Tests of Hypotheses using the Anova MS for R*T as an error term

Source	DF	Anova SS	Mean Square	F Value	Pr > F
T	2	2332.17876543	1166.08938272	3228.07	0.0001

Tests of Hypotheses using the Anova MS for R*L*T as an error term

L*T	4	3.33827160	0.83456790	1.24	0.3680
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Tests of Hypotheses using the Anova MS for R*M as an error term

M	2	124.60024691	62.30012346	209.39	0.0001
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Tests of Hypotheses using the Anova MS for R*L*M as an error term

L*M	4	0.15234568	0.03808642	0.22	0.9204
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Tests of Hypotheses using the Anova MS for R*T*M as an error term

T*M	4	35.80049383	8.95012346	49.95	0.0001
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Exercise 43. For replicate I of the above experiment, write a possible randomization order for the 18 experiments at L_1 and L_2 . This will complete the randomization order for Replicate I.

Exercise 44. Work problems 14.20 and 14.22 pages 639-640 of Montgomery(8e).

Erratum for Chapter 14 of Montgomery's 8th Edition

Page 615, in Figure 14.6 delete the two normal curves atop this page for τ and β because both Alloy formulation and Heats are fixed. Fixed factors cannot have a variance or a statistical distribution.