GENERAL COMPLETE BLOCK DESIGNS

v treatments (They might be factorial treatment combinations)
b blocks
k = vs experimental units in each block
The units in each block are randomly assigned to the v treatments in such a way that each treatment is assigned s units per block
Total number of experimental units n =

Compare and contrast with RCBD.

Models:
1. *Block-treatment model*:

$Y_{hit} = \mu + \theta_h + \tau_i + \varepsilon_{hit}$
$\varepsilon_{hit} \sim N(o,\sigma^2)$
ε_{hit}'s independent

where
Y_{hit} is the random variable representing the response for observation t of treatment i observed in block h,
μ is a constant (which may be thought of as the overall mean – see below)
θ_h is the (additive) effect of the h^{th} block (h = 1, 2, … , b)
τ_i is the (additive) effect of the i^{th} treatment (i = 1, 2, … , v)
ε_{hit} is the random error for the observation t of the i^{th} treatment in the h^{th} block.

Compare and contrast with RCBD model, main effects model.
Does not model interaction between block and factor.

2. *Block-treatment interaction model*

$Y_{hit} = \mu + \theta_h + \tau_i + (\theta\tau)_{hi} + \varepsilon_{hit}$
$\varepsilon_{hit} \sim N(o,\sigma^2)$
ε_{hit}'s independent

where
Y_{hit} , μ ,θ_h , τ_i ,ε_{hit} are as above, and $(\theta\tau)_{hi}$ is a *block-treatment* interaction term.

Compare and contrast with block-treatment model, 2-way complete model.
Does model interaction between block and factor.

Fitting the model: Least squares, etc. gives fits

Block-treatment model: $\hat{y}_{hit} = \bar{y}_{h\bullet\bullet} + \bar{y}_{\bullet i\cdot} - \bar{y}_{\bullet\bullet\bullet}$

Block-treatment interaction model: $\hat{y}_{hit} = \bar{y}_{hi\bullet}$

Residuals, ssE, etc. are defined as usual.

Model checking: As before, but also check for possible interaction by using an interaction plot or plotting y_{hit} against treatment factor levels i for each block h separately. Parallel lines in each plot suggest no interaction and small error variability. Lines not parallel suggest either interaction or large error variability.

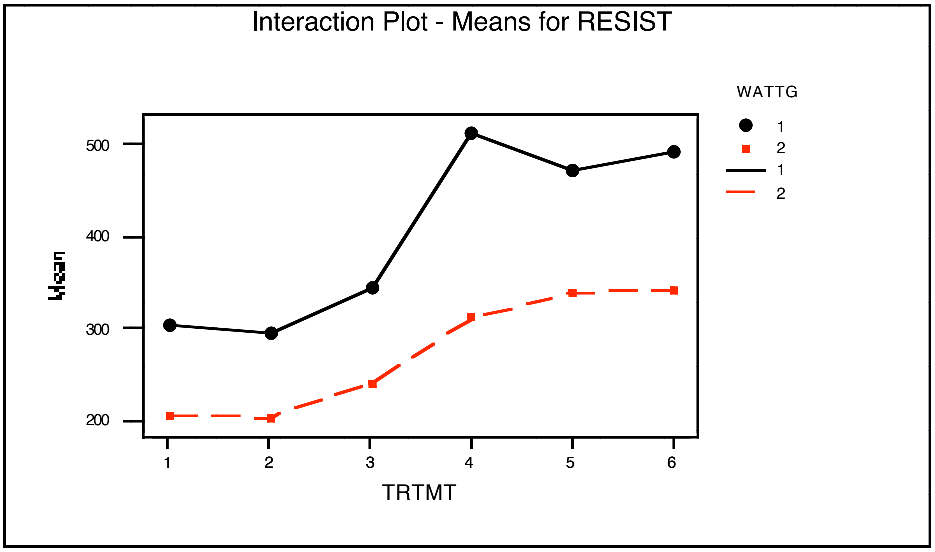
Analysis: The analysis for the two models looks just like the analysis for the two-way main effects model and the two-way complete model, respectively, and can be run on software using the same routines. (But remember, there is no test for block, although the msB to msE ratio is an informal assessment of the utility of blocking in reducing error variance.)

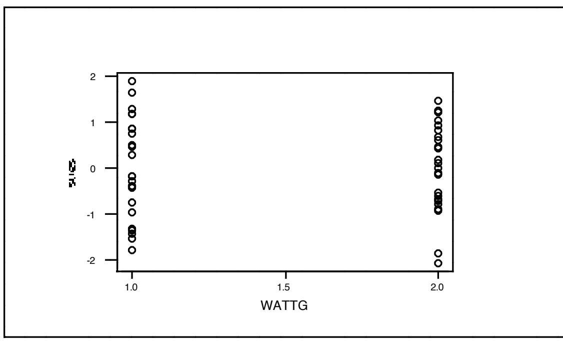
In particular, in the block-treatment interaction model, the null hypothesis for the test for interaction between block and treatment is

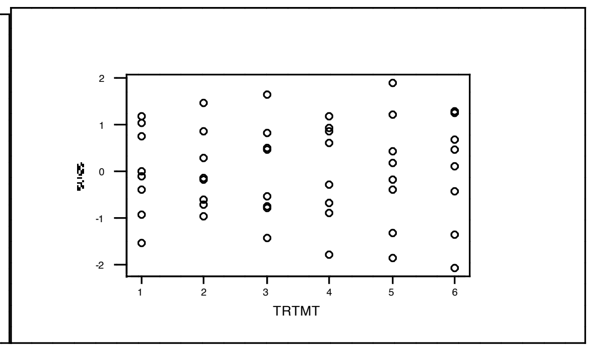
$$H_0^{\theta T} : [(\theta\tau)_{hi} - [(\theta\tau)_{hs}] - [[(\theta\tau)_{gi} - [(\theta\tau)_{gs}] \text{ for all } h \neq g,\ i \neq s$$

Contrasts: Methods for multiple comparison used for factorial designs are valid with suitable modifications; see pp. 312 – 313 for details.

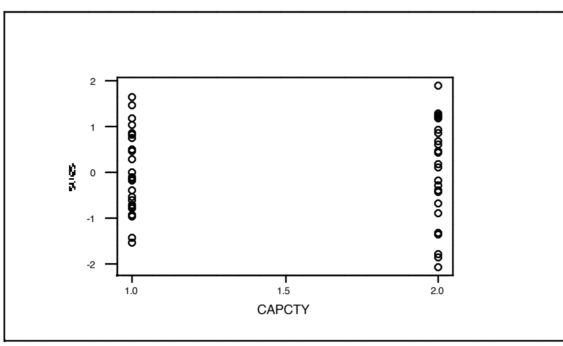
Example: Light bulb experiment: The purpose of the experiment was to compare the light intensities of three different brands (coded 1, 2, 3) of light bulbs to determine the best brand. (Brand 3 was cheaper than brands 1 and 2.) A second treatment factor was percent of capacity, which was set at two levels, 50% and 100%, by adjusting the current passed through the bulbs. The experimenters wished to compare brands averaged across capacities, and also for each capacity. Both 60 watt and 100 watt bulbs were to be compared across brands, but comparisons between wattages were not of interest. Also, since the experiment needed to be run at two different times, it was convenient to run the 60 watt bulbs one day and the 100 watt bulbs the other day. Thus the watt-day combinations were used as blocks. In each block, 4 observations were taken on each of the 6 treatments. Resistance was the response, since low resistance implies high illumination. It was reasonable to believe that there might be interaction between treatment combination and wattage (hence block), so the block-treatment interaction model was used. Indeed, an interaction plot for treatment and block suggested interaction. (Compare Figure 10.4, p. 312)

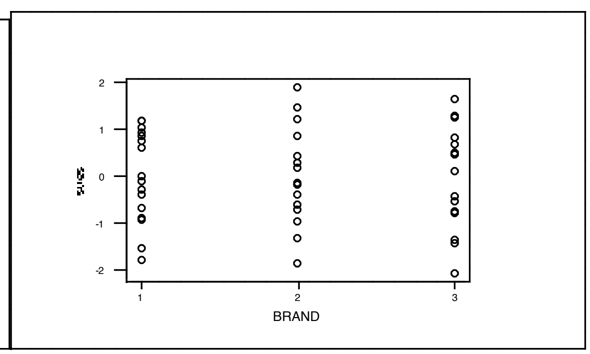






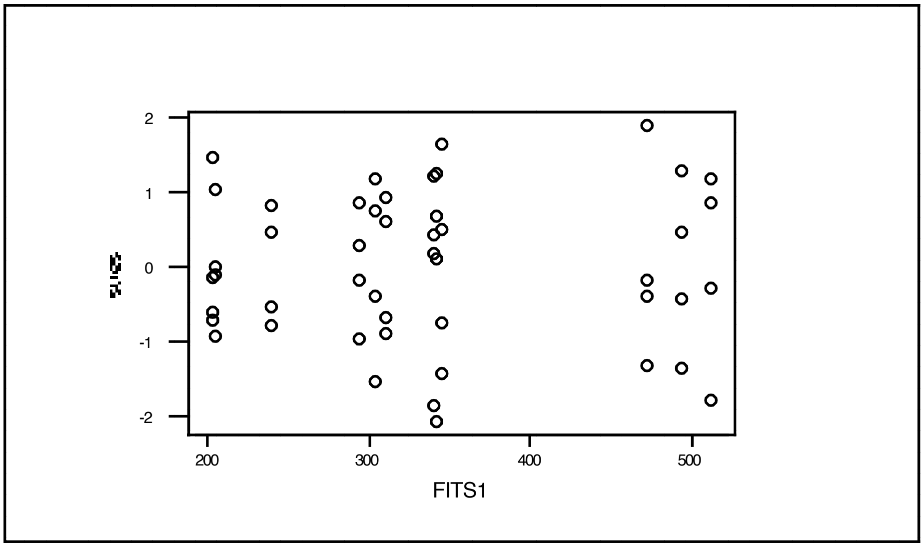
Plots of standardized residuals against block and treatment show no problems. Since we have the two treatment factors capacity and brand, we'll plot against them, too:



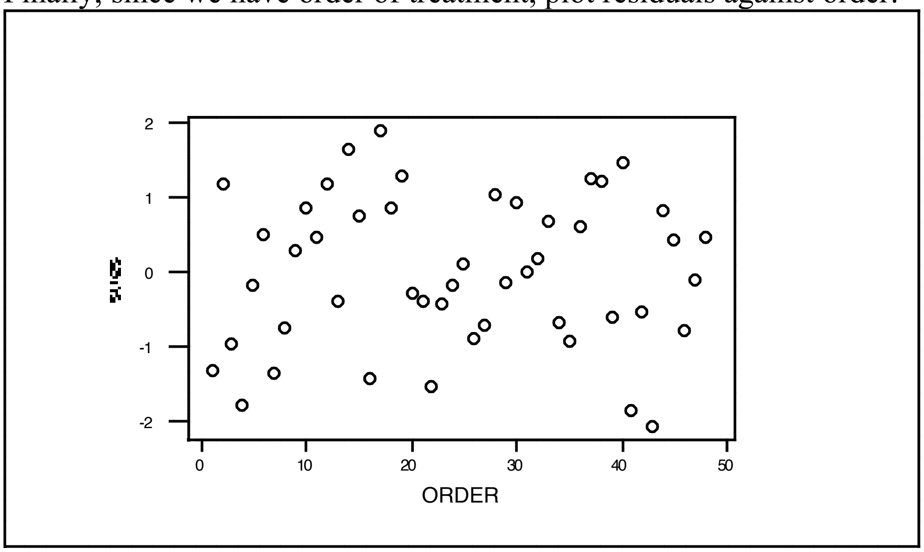


The only hint of any possible problem here is a slight suggestion of possible non-constant variance, but probably nothing to worry about – indeed, a check of standard deviation by treatment shows largest 1.229, smallest 0.825, giving variance ratio 2.219, which suggests no problem.

With no indications of variance problems, we still check the residuals vs fits to see if there might be any nonlinearity:



None is apparent.
Finally, since we have order of treatment, plot residuals against order:



No problems are apparent.

So turning to the Analysis of Variance table:

```
Analysis of Variance for RESIST

Source        DF        SS        MS        F       P
WATTG          1    203972    203972  2025.79   0.000
TRTMT          5    267645     53529   531.64   0.000
WATTG*TRTMT    5     17503      3501    34.77   0.000
Error         36      3625       101
Total         47    492745
```

The data do provide evidence for interaction between block and treatment. The high ratio of msB/msE also suggests that blocking has been useful in reducing variance.

The original questions of interest (comparing brands averaged across capacity and comparing brands for each capacity) call for contrasts. However, the interaction between block and treatment suggests that these comparisons should be made for each block (each wattage) separately. See Example 10.6.2 (pp. 313 – 315) for details of the further analysis.

OTHER BLOCK DESIGNS

Blocking in factorial experiments: When treatments in a block design are combinations of two or more factors, we can use models which involve the individual factors and their interactions as well as the blocking "factor." See Section 10.8 for examples.

Incomplete block designs: In these, the number of treatments per block is not a multiple of the total number of treatments – most commonly, it is less than the total number of treatments. This may be necessary because of limits on the size of the blocks, or because of limitations on availability of equipment, etc. In *balanced incomplete block designs*, each block has the same number of treatments, but the number of treatments per block is less than the total number of treatments. For more on incomplete block designs, see Chapter 11.

Mention factorial, incomplete block designs, other variations?