Practical Data Analysis for Designed Experiments Errata for First Printing

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Many thanks for typos and other suggestions from
Hyungjun Cho, John M. Grego, Lixin Han, Yufen Huang, Edward C. Malthouse,
Nicholas Montpetit, Jon Seltzer, Jun Shao and Chen Wang.

typos and Changes

Preface

xv:

Numerous typos have been corrected in this printing, and several sections have been clarified. Sections in Chapter 5 have been renumbered. Writing a book is a continual process, with many opportunities for rethinking the way to present material. There is more that I would like to do with this text, but time is too short for this time around.

Note in particular that I would like to add another part on categorical data and generalized linear models. Many students complete degrees without much exposure to this subject, given the short amount of time available to take courses. It seems natural to show the connections with linear models, giving readers an intuitive insight into practical data analysis for counts.

At this point, there are some parts that I would like to rethink, expand or reorganize. Other minor changes are in the planning stages. These will have to wait for a clear block of time in the not too distant future. Thank you for your ideas and support in the use and development of this book.

Many thanks to the students and colleagues who have given me feedback on this text. These include Hyungjun Cho, John Grego (1998 review), Lixin Han, Yufen Huang, Edward C Malthouse, Nicholas Montpetit, Jon Seltzer, Jun Shao and Chen Wang. Chen Wang served as my Teaching Assistant when I first taught from this book in print.

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ppp.ll:
page ppp, line ll (from bottom if negative)
ppp.rr.ll:
page ppp, paragraph rr, line ll (from bottom if negative)

Part A: Placing Data in Context

4.–3:
effects of one factor on response at different levels of other factors.
7.5:
dered in the language of the original questions in a manner accessible to other scientists.
15.2.3:
ment and execution of strategies for data analysis. The recent package `JMP`
16.4.2:
organize all this information and provide ready access to those aspects which
21.4.3:
project and provide the first hints of the experiment and key questions.
36.6.–4:
non-smokers, but instead have been self-selected based on a confusing variety of
39.2.7:
that EU but do not increase the number of independent assignments of factor levels. Time cannot be randomized, which can be problematic, particularly when there are repeated measurements on the same subject.

Part B: Working with Groups of Data

58 Example 4.7:
However, the `cultivars` and the anomalous `odd`
59.4.1:
Suppose interest focuses on estimating a particular group mean μ_i . The
65.2.4:

$$\text{Prob}\{p < \alpha : H_0,\ \text{data}\ \} = \alpha\ .$$

65.3.3:
among group variances are deferred to Part E.
71.2.:

This chapter examines the problem of comparing several means simultaneously. Section 5.1 sets up compound hypotheses, while Section 5.2 develops linear contrasts of means. An overal test of differences among means is developed in Section 5.3. Section 5.4 explores partitioning the total sums of squared deviation around the sample grand mean. Expected values of sums of squares comprise Section 5.5, with power and sample size addressed briefly in Section 5.6.

5.1 From two to many

86 Problem 5.1(b):
orthogonal contrasts which would be uncorrelated for equal sample sizes.
98.4:
error rate is conservative in such situations (Hayter 1984).
98.8:

$$\sqrt{n} \max_i |\bar{y}_{i\cdot} - \bar{y}_{0\cdot}| / \hat{\sigma}$$

Part C: Sorting out Effects with Data

108.5.3:
factor combination. Their estimators, $\bar{y}_{i\cdot\cdot}$, $\bar{y}_{ij\cdot}$ and $\bar{y}_{ijk\cdot}$, respectively, are unique and unbiased, and are linear combinations of the responses in the appropriate model.
112.–1:

$$SS = \sum_{i=1}^{a} \sum_{j=1}^{b} \sum_{k=1}^{n_{ij}} (y_{ijk} - \mu - \alpha_i - \beta_j - \gamma_{ij})^2$$

113.4:

$$n_{ij}\gamma_{ij} = \sum_k y_{ijk} - n_{ij}(\mu + \alpha_i + \beta_j) \ .$$

119.3.1:
Thus, given a set of constraints, it is possible to simplify the general form of estimable functions. However, the estimable functions are the same regardless
123.3.4:
of estimable functions. Knowing how those are calculated is vital!
127.–1:

$$E(MS_A) = \sigma^2 + bn \sum_i (\alpha_i - \bar{\alpha}.)^2/(a-1) \ .$$

132.10:

$$\delta^2_{AB} = n \sum_{ij} (\gamma_{ij} - \bar{\gamma}_{\cdot j} - \bar{\gamma}_{\cdot j} + \bar{\gamma}_{\cdot\cdot})^2/\sigma^2 \ ,$$

135 Figure 8.2:
move caption – (b) margin plot – down one line
152–3:
not linear in the model parameters. Tukey's one-degree-of-freedom test of $H_o : G = 0$ can be performed in a few steps with most statistical packages, as shown in the next section.

This pattern of interaction can easily be detected with a margin plot. Consider what happens to the model when averaging over the levels of B,

$$\bar{y}_{i\cdot} = \mu + \alpha_i + \bar{\beta}. + G\alpha_i\bar{\beta}. + \bar{e}_{i\cdot}$$

Solving for α_i yields

$$\alpha_i = E[(\bar{y}_{i\cdot} - \mu - \bar{\beta}.)/(1 + G\bar{\beta}.)]$$

Ignoring the expectation, plug this into the Tukey interaction model as

$$y_{ij} \approx \mu^*_j + \beta^*_j \bar{y}_{i\cdot}$$

with $\mu^*_j = (1 - G\mu)(\beta_j - \bar{\beta}.)/(1 + G\bar{\beta}.)$ and $\beta^*_j = 1 + G(\beta_j - \bar{\beta}.)/(1 + G\bar{\beta}.)$. This suggests plotting cell means y_{ij} against marginal means $\bar{y}_{i\cdot}$ to examine the fit to the Tukey interaction model.

A **margin plot** is an interaction plot of cell means y_{ij} (or $\bar{y}_{ij\cdot}$ in general) on one set of marginal means, say $\bar{y}_{i\cdot}$ (or $\bar{y}_{i\cdot\cdot}$) for factor A. One purpose of such a plot is to provide a natural order for factor levels, positioning levels at their marginal means. Thus levels with similar marginal effect are placed beside each other. In addition, margin plots provide a way to interpret factors as regressors. That is, imagine regressing the cell means on the marginal means for A

$$\bar{y}_{ij\cdot} = \mu^*_j + \beta^*_j \bar{y}_{i\cdot\cdot} + e^*_{ij}$$

The margin plot is a scatter plot of $\bar{y}_{ij\cdot}$ against $x_{ij} = \bar{y}_{i\cdot\cdot}$ using plot symbols or connected lines for levels of factor B. This is conceptually reasonable, but has certain theoretical problems, since the response is on both sides of the equation. It should only be used as a graphical guide, with formal inference deferred to the next section.
154 Table 9.1:

geno	2	1494.1	747.1	5.32	0.075
lat	2	76.1	38.0	0.27	0.78

157 Table 9.2:

geno	2	1494.1	747.1	143.4	0.0011
lat	2	76.1	38.0	7.3	0.070
inter	1	546.4	546.4	104.9	0.0029

157.2.–1:
much as was done above. Tests can be developed in a similar fashion.
158.9–10:
(a) Write down a model for the quality score. Define everything carefully.
158.15–16:
standard errors, `LSD` bars and the like. Label plots and tables clearly!
158.23–25:
lems with assumptions. Do not attempt any formal analysis. Show the data using plot symbols to identify factors or factor combinations.

Part D: Dealing with Imbalance

161.7:
In many other experiments the scientist cannot control the balance.
163.2.2:
population means do not correspond to the **sample marginal means**
163.2.5:

$$\bar{y}_{\cdots} \quad = \quad \sum_i \sum_j \sum_k y_{ijk}/n_{\cdot\cdot} \ ,$$

169.3:

$$SS_{B*A|A,B} = R(\gamma|\mu,\alpha,\beta) = \sum_{ij} n_{ij}(\bar{y}_{ij\cdot} - \bar{y}_{i\cdot\cdot} - \bar{y}_{\cdot j\cdot} + \bar{y}_{\cdots})^2 \ .$$

170.3.7:

$$H_{0B|A} : \sum_i \frac{n_{ij}\mu_{ij}}{n_{\cdot j}} = \sum_{ik} \frac{n_{ij}n_{ik}\mu_{ik}}{n_{i\cdot}n_{\cdot j}} \ .$$

179 Table 11.2:

	with zeros			
BA/TDZ	0	0.2	2.0	20
0	0.02c	0.51a	0.18bc	0.20bc

184.5.6:

$$H_0 : \sum_{j \in S_{ia}} \mu_{aj} = \sum_{j \in S_{ia}} \mu_{ij} \ , \ i = 1, \cdots, a-1 \ ,$$

186.3.1:
The test for **interaction** in a two-factor model is the same regardless of
187.1.7:
is wise to examine some balanced contrasts involving main effects.
188.1.6:

$$y_{ijk} = \mu + \alpha_i + \beta_j + \gamma_k + e_{ijk}$$

194.2.3:
`taste` and `odor`. They created 16 combinations of six factors in a frac-
197.2–3:
$\mathbf{b}^{\mathrm{T}}\mathbf{y}$, with $\mathbf{a} = \mathbf{X}^{\mathrm{T}}\mathbf{b}$. The estimators $\mathbf{a}^{\mathrm{T}}\hat{\boldsymbol{\beta}} = \mathbf{b}^{\mathrm{T}}\mathbf{X}\hat{\boldsymbol{\beta}} = \mathbf{b}^{\mathrm{T}}\mathbf{X}(\mathbf{X}^{\mathrm{T}}\mathbf{X})^{-}\mathbf{X}^{\mathrm{T}}\mathbf{y}$ are **best linear unbiased (BLUE)**,
200.2.10:
with $\mathbf{J} = \mathbf{1}\mathbf{1}^{\mathrm{T}}$ an $n \times n$ matrix of all 1s. That is, the model becomes
204.2.9:

$$E(\mathbf{y}) = [\mathbf{1} : \mathbf{X}_2 : \mathbf{X}_3 : \mathbf{X}_4] \begin{bmatrix} \mu \\ \boldsymbol{\alpha} \\ \boldsymbol{\beta} \\ \boldsymbol{\gamma} \end{bmatrix} = \mathbf{1}\mu + \mathbf{X}_2\boldsymbol{\alpha} + \mathbf{X}_3\boldsymbol{\beta} + \mathbf{X}_4\boldsymbol{\gamma} \ .$$

204.3.6:
propriate. That is, the distribution of $R(\boldsymbol{\alpha}|\mu, \boldsymbol{\beta}, \boldsymbol{\gamma})$ depends on

Part E: Questioning Assumptions

213.3.1:
The first and most important assumption is that the model has been cor-
213.–1:
Chapter 8, Part C) of mean response for factor combinations can reveal deficiencies
218.3.2–3:
Skewness, as measured by the third moment,

$$\texttt{SKEW} = E[(y_{ij} - \mu_i)^3]/\sigma^3 \ (= 0 \text{ for normal}) \ ,$$

219.2.4:

$$\texttt{KURT} = E[(y_{ij} - \mu_i)^4]/\sigma^4 - 3 \; (= 0 \text{ for normal}) \; .$$

219.2.8:

$$V(\hat{\sigma}^2) = \sigma^4[2/(n-1) + \texttt{KURT}/n] \approx \frac{2\sigma^4}{n-1}\left(1 + \frac{\texttt{KURT}}{2}\right) \; .$$

219.2.–2:
with approximate degrees of freedom $\hat{d} = 1/(1 + \texttt{KURT}/2)$, where the kurtosis
221.3:
question the sense of comparing means when variances are unequal. A weighted
221.9:
such as log for relative data or square root for counts, may change the
224.2.2:
square of an Welch's approximate t test,
224.4.3:

$$S = \hat{\sigma}_1^2/n_1 + \hat{\sigma}_2^2/n_2$$

225.3:
If $n_1 = n_2$ and $\sigma_1 = \sigma_2$, then this reduces to the usual test with $r = n_1 + n_2 - 2$ and $a = 1$. The degrees of freedom can still be $r = n_1 + n_2 - 2$ provided that $\sigma_1^2/\sigma_2^2 = n_1(n_1-1)/[n_1(n_1-1)]$, but in this case

$$a = \frac{\sigma_1^2}{n_1(n_1-1)} = \frac{\sigma_2^2}{n_2(n_2-1)} = \left(\frac{\sigma_1^2}{n_1} + \frac{\sigma_2^2}{n_2}\right)/(n.-2)$$

The approximate
225.–1:

$$Z = Z(\bar{Y}_{1\cdot}, \bar{Y}_{2\cdot}) = \frac{(\bar{Y}_{1\cdot} - \bar{Y}_{2\cdot}) - (\mu_1 - \mu_2)}{\sqrt{\sigma_1^2/n_1 + \sigma_2^2/n_2)}} \; ,$$

226.3:

$$X_i^2 = X_i^2(S_i^2) = \frac{(n_i - 1)S_i^2}{\sigma_i^2} \sim \chi^2_{n_i - 1} \; , \; i = 1, 2.$$

227.2.5:
formance of means tests. Thus, it is often better to examine patterns in
227.5.2–5:
is possible with many experimental units appropriately arranged in subgroups. He suggests another way to study to experiments in which there is

sub-sampling (see Chapter ??, Part ??), or multiple measurements on experimental units, by examining their log variance as a measure of quality.
228.1.2–23:
(1995). There are four groups and a total of 31 observations. The range of SDs is modest (1.5 to 5.7), but it is unwise to assume equal variance.
(a) Write down the usual assumptions and report usual anova results. (b) By hand, make a 'dot-plot' of the data, using letter symbols for group. Comment. Now drop the assumption that variances are equal.
(c) Plot mean vs. variance by group, noting any (lack of) pattern.
(d) Use the inverse of group variance as weight, justifying this choice. Briefly critique it. How does the use of estimates of group variances affect the p-value? [Hint: examine the new SDs.]
(e) The exact test differs from all of these. It is based on the randomization principle: if there are no group differences, all group assignments are equally likely. That is, one could (in theory) examine every permutation of the 31 responses (with nine `A`s, seven `B`s, eight `C`s and seven `D`s) and compute the F statistic for each one. The p-value is the proportion of F values that are as extreme as or larger than the one observed. The 'right' p-value, based on exact generalized inference, for the raw data is 0.030, or for the ranks (exact Kruskal–Wallis) is 0.06.

228.2.1:
Consider inference on the variances themselves.

228.2.8–10:
(b) Conduct Levene's test for unequal variance. This test does not depend on normality and can be used for small samples. Interpret results.

Part F: Regressing with Factors

251.2.6–252.1.3:
with u_i some independent zero-mean noise. If the y_i are regressed on the observable w_i , the slope is attenuated. That is, the model becomes

$$y_i = \beta_0^* + \beta_1^* w_i + e_i$$

with $\beta_1^* = \lambda\beta_1$ attenuated by the reliability ratio, $\lambda = \sigma_x^2/\sigma_w^2$, which is at most one. The relationship is illustrated in Figure 16.6, which differs in important ways from the regression calibration model shown in Figure 16.5. The intercept changes as well, to $\beta_0^* = \beta_0 + (1-\lambda)\beta_1\bar{x}.$.
252.2.3:

$$V(y_i|w_i) = \sigma^2 + \lambda^2\beta_1^2\sigma_u^2 \ .$$

254 Problem 16.4:
Consider a simple linear regression of `dmi` on the `diet` rank.
256.4:
or curvilinear relationships, as shown in Section 17.6.
259.2.7:
These are in general biased estimators, since $E(\breve{\mu}_i) = \mu_i + \beta(\bar{x}_{i\cdot} - \bar{x}_{\cdot\cdot})$ and $E(\breve{\beta}) = \beta + T_{x\mu}/T_{xx}$. As it turns out, these can be unbiased and do coincide with adjusted estimates developed below if
263.3.10:

$$SS_{X|A} = \sum_{ij}(\hat{y}_{ij} - \bar{y}_{i\cdot})^2 = \sum_{ij}[\hat{\beta}(\bar{x}_{i\cdot} - \bar{x}_{\cdot\cdot})]^2 = \hat{\beta}^2 W_{xx} = \hat{\beta} W_{xy} \ .$$

263.3.16–17:

$X\|A$	1	$SS_{X\|A} = \hat{\beta}^2 W_{xx}$
error	$n. - a - 1$	$SS_E = W_{yy} - \hat{\beta}^2 W_{xx}$

263.–1:

$$SS_{A,X} = B_{yy} + \hat{\beta}^2 W_{xx} \ .$$

265.–1:

$$E(\hat{\beta}^2 W_{xx}) = \sigma^2 + \beta^2 W_{xx} \ .$$

266.4.3:
factor is applied, if appropriate, are surely unaffected by the factor. Thus it may be
266.5.3–4:

X=Covariate	1	$SS_X = \breve{\beta}^2 T_{xx}$
error	$n. - 2$	$SS_E = T_{yy} - \breve{\beta}^2 T_{xx}$

267.7–9:

X	1	$SS_X = \breve{\beta}^2 T_{xx}$
$A\|X$	$a - 1$	$SS_{A\|X} = B_{yy} + \hat{\beta} W_{xy} - \breve{\beta}^2 T_{xx}$
error	$n. - a - 1$	$SS_E = W_{yy} - \hat{\beta}^2 W_{xx}$

267.2.3:

$$E(\check{\beta}^2 T_{xx}) = \sigma^2 + T_{xx}[\beta + T_{x\mu}/T_{xx}]^2 \ ,$$

268.2–4:

X	1	$\check{\beta}^2 T_{xx}$	$\sigma^2 + \beta^2 T_{xx}$
$A\|X$	$a-1$	$B_{yy} + \hat{\beta}^2 W_{xx} - \check{\beta}^2 T_{xx}$	$\sigma^2 + \sum_i n_i(\mu_i - \tilde{\mu}.)^2/(a-1)$
error	$n. - a - 1$	$W_{yy} - \hat{\beta}^2 W_{xx}$	σ^2

268.–2:
thesis, $T_{x\mu} = 0$ and $\mu_i = \tilde{\mu}. = \mu$, which implies $\delta_A^2 = 0$. Thus the pivot
269.2–4 Table 17.3:

$X\|A$=covariate	1	$\hat{\beta}^2 W_{xx}$	$\sigma^2(1 + \delta_X^2)$
$A\|X$=treatment	$a-1$	$B_{yy} + \hat{\beta}^2 W_{xx} - \check{\beta}^2 T_{xx}$	$\sigma^2(1 + \delta_A^2)$
error	$n. - a - 1$	$W_{yy} - \hat{\beta}^2 W_{xx}$	σ^2

Part G: Deciding on Fixed or Random Effects

306.2.–4:

$$df_A \approx \frac{2[E(SS_A)]^2}{V(SS_A)} = \frac{a-1}{1 + v/[(a-1)(n_0 + \sigma^2/\sigma_A^2)^2]} \ ,$$

315.2.4:
levels would only be done on a post-hoc basis to select levels for subsequent
315.–1:

$$F = MS_{AB}/MSE \sim \frac{\sigma^2 + n\sigma_{AB}^2}{\sigma^2} F_{(a-1)(b-1),ab(n-1)} \ ,$$

332 Problem 21.1:
with the restriction that $\sum_{i=1}^{a} C_{ij}^* = 0$.

Part H: Nesting Experimental Units

339.4.2:
that measurement variance is $\sigma^2 = 1$ while EU variance is $\sigma_B^2 = 2$, and

339.4.–2:
increases. Both the non-centrality parameter $\delta^2 = bn/E(2MS_{B(A)})$ and the
358.3 Table 23.1:

$$\text{plot error} \quad 3 \quad \sigma^2 + 4\sigma_P^2$$

359.5 Table 23.3:

$$\text{subplot error} \quad 9 \quad \sigma^2$$

373.4–6:
combine nested factors as a single factor to simplify analysis and/or employ multiple comparisons methods. Thus it may not be possible simply to use one approach for all factors in an experiment which is unbalanced.

Part I: Repeating Measures on Subjects

387.4.2–3:
$(\mathbf{U}_i = \sigma_P^2 \mathbf{I})$. In practice, there are a variety of ways to examine treatment differences over time by reducing data to a **single measurement**
389.–1:

$$\text{cov}(r_{i1}, r_{i2}) = (\Lambda_i)_{12} \quad = \quad \sigma_P^2(\omega_{i1} + \omega_{i2})$$

390.3:

$$\mathbf{V} = \sigma^2[\mathbf{I} + \boldsymbol{\lambda}\mathbf{1}^{\mathrm{T}} + \mathbf{1}\boldsymbol{\lambda}^{\mathrm{T}}] = \sigma^2 \begin{bmatrix} 1+2\lambda_1 & \lambda_1+\lambda_2 & \cdots & \lambda_1+\lambda_t \\ \lambda_1+\lambda_2 & 1+2\lambda_2 & \cdots & \lambda_2+\lambda_t \\ \vdots & \vdots & \vdots & \vdots \\ \lambda_1+\lambda_t & \lambda_2+\lambda_t & \cdots & 1+2\lambda_t \end{bmatrix} .$$

391.2.4–5:

$$\begin{aligned} SS_{SPE} &= \textstyle\sum_{ikm}(y_{ikm} - \bar{y}_{ik\cdot} - \bar{y}_{i\cdot m} + \bar{y}_{i\cdot\cdot})^2 \\ &= \textstyle\sum_{ikm} e_{ikm}^2 - n\sum_{ik} \bar{e}_{ik\cdot}^2 - t\sum_{im} \bar{e}_{i\cdot m}^2 + nt\sum_i \bar{e}_{i\cdot\cdot}^2 \end{aligned}$$

394.–2:

$$\bar{y}_{\cdot 3\cdot} - \bar{y}_{\cdot 4\cdot} = \bar{\mu}_{\cdot 3} - \bar{\mu}_{\cdot 4} + \bar{e}_{\cdot 3\cdot} - \bar{e}_{\cdot 4\cdot} \ .$$

395.–2:

$$\bar{y}_{13\cdot} - \bar{y}_{14\cdot} = \mu_{13} - \mu_{14} + \bar{e}_{13\cdot} - \bar{e}_{14\cdot}$$

396.2:

$$\bar{y}_{13\cdot} - \bar{y}_{23\cdot} = \mu_{13} - \mu_{23} + \bar{r}_{1\cdot} - \bar{r}_{2\cdot} + \bar{e}_{13\cdot} - \bar{e}_{23\cdot}$$

402 Table 26.1:

source	F	naive	G-G	H-F	Box
tree soils					
ϵ-adjustment			0.38	0.48	

402.–1:

$$V(\mathbf{c}^{\mathrm{T}}\mathbf{y}_{im}) = \mathbf{c}^{\mathrm{T}}\mathbf{V}\mathbf{c}\ .$$

403.2:
$\mathbf{c}^{\mathrm{T}}\mathbf{V}\mathbf{c} = \sigma^2$. However, in general, the variance of a contrast over time
403.3.13:

$$S = (n(a-1)-r)\, log \left(\frac{\det(\mathbf{C}^{\mathrm{T}}\hat{\mathbf{V}}\mathbf{C}))}{|\mathrm{tr}(\mathbf{C}^{\mathrm{T}}\hat{\mathbf{V}}\mathbf{C})/(t-1)|^{t-1}} \right) \sim \chi^2_r$$

406.2:

$$\mathbf{X}\hat{\boldsymbol{\beta}}_k = \mathbf{X}(\mathbf{X}^{\mathrm{T}}\mathbf{X})^{-}\mathbf{X}^{\mathrm{T}}\mathbf{y}_k = (\mathbf{I}-\mathbf{P})\mathbf{y}_k \sim N(\mathbf{X}\boldsymbol{\beta}_k, \sigma_{kk}(\mathbf{I}-\mathbf{P}))$$

407.10:

$$\mathbf{m}^{\mathrm{T}}\mathbf{Y}^{\mathrm{T}}\mathbf{P}\mathbf{Y}\mathbf{m} = (n-r)\mathbf{m}^{\mathrm{T}}\hat{\mathbf{V}}\mathbf{m} \sim \mathbf{m}^{\mathrm{T}}\mathbf{V}\mathbf{m}\chi^2_{n-r}$$

409.17:
(e) How do the multivariate tests compare with polynomial (or other)

412–413:

Consider a variation on the split plot model in which the whole plot factor is the sequence of treatment levels applied to the subplots. Here think of the subplots as time periods, although that is not necessary. Such a model might include

$$y_{ijkm} = \mu + (\alpha_i + r_{im}) + (\beta_j + (\alpha\beta)_{ij} + e_{ijkm})$$

with $i = 1, \cdots, a$ sequences of treatment levels, $m = 1, \cdots, n_i$ subjects per sequence, $j = 1, \cdots, b$ treatment levels, and c repeats of each treatment level. Note that there are $t = bc$ subplots (times) per subject. It is often nice for recording purposes to have $k = 1, \cdots, t$ identify the time periods even though this leads to redundancy in the triplet ijk much as there is in the Latin square design. In fact, the sequence of treatments over time periods is prescribed by the sequence i. If there are the same number of periods as treatments ($b = t$), there are $t!$ $(=t \times (t-1) \times \cdots \times 2 \times 1)$ possible sequences, although only a subset might be used in some experiments.

With μ as the usual reference, α_i is the sequence effect, β_j is the treatment effect, r_{im} the subject error and e_{ijkm} the time period error. This form indicates which treatment is assigned to each time period, but assumes there is no trend over time. Recall the repeated measure model,

$$y_{ijkm} = \mu + (\alpha_i + r_{im}) + (\tau_k + (\alpha\tau)_{ik} + e_{ijkm})$$

with τ_k the time effect, and the treatment subscript just included for convenience. Combine these concepts, and consider a more complicated model which allows for treatment and time

$$y_{ijkm} = \mu + (\alpha_i + r_{im}) + (\beta_j + \tau_k + \gamma_{ijk} + e_{ijkm})$$

with the three-factor interaction γ_{ijk} representing the different response to treatment in different periods not explained by main effects. This is often attributed to the **carry-over** of treatments from earlier periods.

Typically there are not enough periods to investigate full interactions involving treatment and period within sequence. Instead, simplifying assumptions about the form of the carry-over γ_{ijk} are made, such as

$$\gamma_{ijk} = \sum_{l=0}^{k-1} \lambda_{j(i,l)}$$

the sum of carry-overs from all previous periods, including a possible initial effect $\lambda_{j(i,0)}$ which is usually set to zero. Most applications simplify further

$$\gamma_{ijk} = \lambda_{j(i,k-1)}$$

to an effect associated with the treatment in the previous period. This leads to the **cross-over model**

$$\begin{aligned} y_{ij1m} &= \mu + (\alpha_i + r_{im}) + (\beta_j + \tau_1 + e_{i1km}) \\ y_{ijkm} &= \mu + (\alpha_i + r_{im}) + (\beta_j + \tau_k + \lambda_{j(i,k-1)} + e_{ijkm}) \ , \ k > 1 \ . \end{aligned}$$

The relationship of treatment j to sequence i and period k for subject m is made explicit here to avoid ambiguity. The notation is identical to that used for nested effects.

413–414:

sequence	period	mean
1	1	$\mu_{1A1} = \mu + \alpha_1 + \beta_A + \tau_1$
1	2	$\mu_{1B2} = \mu + \alpha_1 + \beta_B + \tau_2 + \lambda_A$
2	1	$\mu_{2B1} = \mu + \alpha_2 + \beta_B + \tau_1$
2	2	$\mu_{2A2} = \mu + \alpha_2 + \beta_A + \tau_2 + \lambda_B$

The difference in means between sequences is

$$\bar{\mu}_{1\cdot\cdot} - \bar{\mu}_{2\cdot\cdot} = (\alpha_1 - \alpha_2) + (\lambda_A - \lambda_B)/2 \ ,$$

which hopelessly confounds sequences effect with carry-over. In fact, the sequence effect is precisely the difference due to which treatment is given first, or the carry-over. Thus it is reasonable to think of carry-over as measuring the sequence effect, or rather to assume $\alpha_i = 0$. The difference of period means

$$\bar{\mu}_{\cdot\cdot 1} - \bar{\mu}_{\cdot\cdot 2} = (\tau_1 - \tau_2) - \bar{\lambda}.$$

is confounded with the average carry-over effect. For this reason it is usual to assume $\bar{\lambda}. = 0$, or with two treatments $\lambda_B = -\lambda_A$.

The difference in mean response between treatments is

$$\bar{\mu}_{\cdot A\cdot} - \bar{\mu}_{\cdot B\cdot} = (\beta_A - \beta_B) + (\lambda_B - \lambda_A)/2 \ ,$$

which confounds treatment and carry-over effects. Combining with the sequence mean differences, one can isolate the treatment effects as

$$\begin{aligned} \beta_A - \beta_B &= (\bar{\mu}_{\cdot A\cdot} - \bar{\mu}_{\cdot B\cdot}) + (\bar{\mu}_{1\cdot\cdot} - \bar{\mu}_{2\cdot\cdot}) \\ &= \mu_{1A1} - \mu_{2B1} \ , \end{aligned}$$

which is the difference in mean responses for the first period only.

414 Example 27.1b Carry:
Consider a cross-over experiment with three periods per subject and three treatments (1,2,3). There are six possible sequences

	period		
sequence	1	2	3
1	1	2	3
2	1	3	2
3	2	1	3
4	2	3	1
5	3	1	2
6	3	2	1

Each subject is assigned to a sequence, with $n = 2$ subjects per sequence. The expected response for subjects in sequences $i = 3, 4$ are

sequence	period	mean
3	1	$\mu_{321} = \mu + \alpha_3 + \beta_2 + \tau_1$
3	2	$\mu_{312} = \mu + \alpha_3 + \beta_1 + \tau_2 + \lambda_2$
3	3	$\mu_{333} = \mu + \alpha_3 + \beta_3 + \tau_3 + \lambda_1$
4	1	$\mu_{421} = \mu + \alpha_4 + \beta_2 + \tau_1$
4	2	$\mu_{432} = \mu + \alpha_4 + \beta_3 + \tau_2 + \lambda_2$
4	3	$\mu_{413} = \mu + \alpha_4 + \beta_1 + \tau_3 + \lambda_3$

The difference in means between sequences is

$$\bar{\mu}_{3\cdot\cdot} - \bar{\mu}_{4\cdot\cdot} = (\alpha_3 - \alpha_4) + (\lambda_1 - \lambda_3)/3 \ ,$$

which again confounds sequence effects with carry-over. However, notice that comparing sequences 3 and 1 does not involve the carry-over, since it cancels out due to the balance in the first two periods. In fact, the sequence effect is precisely the difference due to which treatment is given first, or the carry-over. Thus it is reasonable to think of carry-over as measuring the sequence effect, or rather to assume $\alpha_i = 0$.

The difference of period means

$$\bar{\mu}_{\cdot\cdot 1} - \bar{\mu}_{\cdot\cdot 2} = (\tau_1 - \tau_2) - \bar{\lambda}.$$

is again confounded with the average carry-over effect (which is again set to zero for convenience). The difference in mean response between treatments is

$$\bar{\mu}_{\cdot 1 \cdot} - \bar{\mu}_{\cdot 2 \cdot} = (\beta_1 - \beta_2) + 2(\lambda_2 - \lambda_1)/3 \ ,$$

which confounds treatment and carry-over effects. It may be useful to examine the combinations of treatments and carry-overs, as in the following table of triplets ijk. There are two triplets, from two distinct sequences, for all combinations except that no treatment has its own carry-over. The zero carryover is included for completeness.

	treatment		
carry	β_1	β_2	β_3
0	$111, 211$	$321, 421$	$531, 631$
λ_1	$--$	$122, 523$	$232, 333$
λ_2	$312, 613$	$--$	$133, 432$
λ_3	$413, 512$	$223, 622$	$--$

Thus this represents an incomplete factorial, but it is balanced. Each treatment (carry-over) level appears in 6 (4) time period per sequence combinations. Thus main effects of treatment and carry-over are estimable provided the sequence effect is assumed zero ($\alpha_i = 0$). However, inference for carry-over is tricky, since there is information in both the whole plot and the subplot. See Milliken and Johnson (1989) Example 32.3 for details, although the notation is somewhat different.

Suggestions for Further Work

7.3.1:
include newer context of data mining of massive data sets
8 Table 1.1:
pick labels that are not in alphabetical order
14:
update refs for SPSS, Systat and Minitab
15:
update JMP vs. SAS/Insight discussion
61 Example 4.9:
use real data set to illustrate
62 Figure 4.6(b):
move 3 up slightly
72.4:
need specifics on notched boxplots
75.2.:
how to describe orthogonal contrasts for unbalanced data: $\sum c_i d_i / n_i = 0$
75 Example 5.4:
plot the contrasts
89.4.:
SAS (1992) presents many
refer to Hsu (1996)
113–114:
move additive model parameterization to Section 7.2?
149.3.:
should 'rule of 2' discussion of pooling interactions go in Section 9.1?
215.2.:
tone down first sentence?
216.–2:
add reference to Cleveland (1975) and/or forward to next chapter on normal scores – where does s come from?
217 Figure 13.4:
make this more realistic
218.3.3:
eliminate gammas in favor of words for skew and kurt?
219.2.:
refer ahead to Satterthwaite in next chapter
224.2.2:
need reference for Welch's t-test
227.3.:
details of tests of variance
211 Figure 13.1:

change line type for SD lines
212 Figure 13.2:
Redraw with correct SD for fit without zeros
230.:
rewrite rationale for log / practical interpretation
232-233.:
develop comparions of $arcsin(\sqrt{y})$ vs. $\log(y)$ for proportions; i.e. normality vs. stabilizing variance
236.:
update references and material/focus on MCMC
245.:
include other path coef refs? Wright (1921) Li (1975) Sokal and Rolff (1981)
252.2.3:
elaborate how attenuation affects variance
253.:
awkward first two paragraphs
259.:
show details: $E(\check{\beta}) = \sigma^2 + T_{x\mu}/T_{xx}$
265.:
doube check the anovas in Table 17.1 and 17.2
266.:
$E(MS_A) = \sigma^2 + [B_{\mu\mu} + 2\beta B_{x\mu} + B_{xx}]/(a-1)$
267.:
$E(MS_{A|X}) = \sigma^2 + [B_{\mu\mu} + n.(T_{x\mu}/T_{xx})]/(a-1)$
271.:
consider replacing $\mu_i(x)$ with μ_{ix}
276.:
introduce word "manova"
302–306:
switch around sections 19.2 and 19.3 to make it easier to find n_0
330.:
make new section for higher order mixed models?
372.:
consider 96-well plates for strip plot example
340.:
redo Table 22.2 and Figure 22.1. Figure is almost correct, but table is way off.
430—:
Index has several changes