Typographical and other errors found in Judd & McClelland (1989).

Page: line	*Description*
89: -2	For "chances" read "chance".
91: 5	A better definition for power: the probability of rejecting the null hypothesis.
100: (5.4)	For "n=1" read "n–1".
102: -5	For "Exhibits 2.4 and 2.5" read "Exhibits 2.10 and 2.11".
103: 7	SSE(A) = 36.04: see also Exhibit 2.9, p 27.
119: 4	"hundreds of people per square mile": The data in Exhibit B.1 give the numbers of persons per square mile. To obtain the regression coefficient reported as .24, first divide Density by 100.
119: 18	*cf.* the one-parameter model, p 103.
122: 1, -3	For "is necessarily positive" read "cannot be negative" (two places).
122: 4	Square expression in parentheses.
122: 5	Substitute "Since $n > 0$ it follows that"
136: 16	After "chapter" insert "(p 119)".
137: 12	In actual computations, better to express X_i' as $X_i - \text{mean}(X)$. "1.45" may not be sufficiently precise.
138: 9	The mean of Y is actually 3.572, so SSR should be $50(.228)^2 = 2.599$, not 2.65. At line 22, PRE = .102, and $F^* = 5.45$.
139: 16, 19	PRE = 2.599/38.64 = .067 and $F^* = 3.53$.
152: 21	For roman b_0, b_1 read *italic* b_0, b_1.
163: (8.5)	In MODEL A, for β_{ip} read β_p.
168: 8	Notice that $F^* = t^{*2}$: $20.1 = 4.484^2$. (*Cf.* Exhibit 8.3, p 160.)
180:	Exhibit 8.8, Model 1, SSE: for 183.373 read 183.573.
186: -2	"distribution" is misspelled
196: -11	For -4.772 read $(-4.772)^2$.
198: 1-2	These two lines are redundant, *cf* p 197.
209: 20, 24	For "Chapter 4" and "Exhibit 4.2" read "Chapter 5" and "Exhibit 5.2".
212: 9	For "exhibits" read "tables". (Graphs are a kind of exhibit.)
214: -3	For "Y_{ij}'s" read "Y_j's".
217: -15	Add: For a definition of PA see p 83.
232: 17	Omit "exactly". The results are identical to 3 digits, but not to infinite precision.
234: -3	For "the Anscombe's" read "Anscombe's"
235: fn	For $1-h_{ii}$ read $1/(1-h_{ii})$ or $(1-h_{ii})^{-1}$.
237: 2	For "Exhibit 8.4" read "Exhibit 8.21, p 194".
240 *ff*	The data in Exhibit 10.1 do not agree with the analyses presented whenever Age is one of the predictors. I suspect one or more typographical errors in the Age values, but have been unable to verify that conjecture.
241	For the data displayed, the mean of Age is 39.66 (not 39.70), and the standard deviation of Age is 11.56 (not 11.60). Add at bottom of exhibit "N = 80".
289: 5	Equation denominator for "SSE(A)" read "SSE(C)".
296: 9	For "is only met ... in Set I" read "is *not* met ... in Set II". (It is also met in other sets of contrasts not shown here.)
296: 14, 22, 23, ...	For "contrast codes" read "orthogonal contrast codes" (throughout).
296: 23, 24	For "no longer" read "not" (2 places). ("No longer" implies that a condition that once was true is not now true: that is, that the set of codes had changed somehow. This is incorrect.)
307: -13	After "nonredundant" insert "— that is, orthogonal —".
320: 3	After "four" insert "independent" or "orthogonal".
333: -7	For "at fault" read "false". (There is no fault to be found.)
348: 10	For "the appropriate codes are" read "appropriate codes may be"
354: -4	The value of F^* is actually 12.96, because MSError = 50/12.

355: 12.5.2 "Numbers" (plural) in heading.

403: 11 Insert "approximately" before "normally".

422: -5 For "total sum of squares of these W_{0i} values" read "total sum of squared deviations of these W_{0i} values".

442: 1 For "Exhibit 14.30" read "Exhibit 14.29".

475 Exh. 15.23 title: "Hypothetical" is misspelled.

A-92 *ff* There appear to be minor errors in the SAS codes for exhibits in Appendix C:

A-92, code for Exhibit C.1, the footnote is omitted.

A-93, code for C.2, format 6.2 should read 6.3.

A-94, footnote: for BETAINV read FINV.

A-95, footnote: for BETAINV read FINV.

A-97, label df1, for 'PA - PA' read 'PA - PC'.

A-105: -6 Insert: orthogonal polynomials, 300

Comments stimulated by Judd & McClelland (1989).

128: 21 *et passim* Throughout the text, the authors seem to use "reliable" as a synonym or substitute for "significant". This encourages confusion with the idea of "reliability" in the technical measurement sense, which is not at all the same thing. While acknowledging that "significant" is often poorly understood (or grossly *mis*understood), usually because of inadequate attention to its careful definition, I do not consider "reliable" to be a suitable replacement. In a number of contexts in the text, "credible" may do, although it suffers from the same problem as "reliable" in that it is not well-defined as a technical term.

137: 12 Use $X_i' = X_i - \text{mean}(X_i)$ instead of $X_i - 1.45$. More precise, and doesn't require knowing the sample mean beforehand.

137: 14, -1 In MODEL A the intercept is the mean of Y; in MODEL C it's 3.80. To obtain SSR (138: 8-9) in Minitab,

```
let c12 = 'Y' - mean('Y')   # Model A
let c13 = 'Y' - 3.80        # Model C
let k4 = SSQ(c13) - SSQ(c12)    # SSR
print k4
```

260: 10 Insert after the italicized clause, or at the end of the paragraph, a discussion of interaction constructed to be orthogonal to the components of the interaction.

272: 3 For "reliability" read "improbability".

281: -3 For "Such models" read "A restricted class of such models". *Traditional* analysis of covariance (ANCOV) has entailed the restriction that the continuous predictors be assumed not to interact with the categorical predictors. From the point of view of this text that is clearly unnecessary.

295: 12-14 "... by such a coding scheme we are assuming ... a linear one." The assumptions lie not in the coding *per se* but in the formal analyses applied to the data, and/or to the investigator's interpretation of the results of those analyses.

295: -14 "the second condition that must be met": This condition is *not* necessary, unless one wishes the several contrasts to be mutually orthogonal; which, while often desirable or convenient, is not logically necessary, and the authors do not present arguments for (or against) it.

305: -9 "... this value does not exceed the critical value ... at $\alpha = .05$, we cannot reject ...": The probability of observing this value or a larger one is .052 ; some readers may feel this statement to be unnecessarily rigid.

330 In Exhibit 12.5 it is instructive to consider also the interaction contrasts λ_6 with coefficients $2\ {-1}\ {-1}\ {-2}\ 1\ 1$ and $\lambda_7 = [\,0\ 1\ {-1}\ 0\ {-1}\ 1\,]$ as alternatives to λ_4 and λ_5. Notice that $\{\lambda_1\ \lambda_2\ \lambda_3\ \lambda_6\ \lambda_7\}$ are all mutually orthogonal, just as $\{\lambda_1\ \lambda_2\ \lambda_3\ \lambda_4\ \lambda_5\}$ are.

The authors define interaction contrasts invariably as the product of the contrasts used for the main effects. It is important to understand that since any interaction is orthogonal to the main effects that are its components, *any* set of products of main effect contrasts, whether those that were used for the present main effects or not, will do. Indeed, there can be interaction contrasts that are not expressible as products of main effect contrasts.

345: 9-10 ("... no SSR for b_0X_0 because the grand mean of 20 does not vary") This may be somewhat misleading. Note that SSR is defined (p 75) as the

reduction in the error SS for the compact model that is due to using the augmented model instead. The SSR due to the grand mean is zero either (1) because X_0 is not a contrast or (2) because the grand mean is part of both the compact and the augmented model; these are in a sense equivalent. There is, however, a SS associated with X_0 (in this case it is equal to 7200): it is the difference between the raw sum of squares and the sum of squared deviations about the grand mean, sometimes called the correction term due to the grand mean.

348: 10 This is one way of generating appropriate contrast codes. It is not the only way, as may be seen by considering λ_6 and λ_7 (see notes to p 330 above) as alternatives to λ_4 and λ_5 for the drug × enzyme interaction contrast.

The most useful contrast codes for the main effects of factors *A* and *B* may not be the most useful for describing the $A \times B$ interaction, except when both *A* and *B* are dichotomies (that is, have only 1 degree of freedom each). Also, and even when *A* and *B* are dichotomies, it may be useful to describe the observed — or expected, for that matter — pattern of effects by means of contrasts that do not correspond to formal main effects and pure interactions. For a simple example, consider a 2×2 design with cell means of 8, 8, 8, and 3. It is more parsimonious to represent the systematic variation among cells as a contrast comparing the cell with mean 3 to the average of the cells with mean 8, rather than to use two main effects and an interaction for the same purpose — particularly if the contrast just described has a convenient, evocative, or insightful interpretation. (See Judd & McClelland's comments on such a case at p 354.)

I am inclined to prefer approaching the analysis first via the Rules of Thumb, producing sources of variation that have multiple degrees of freedom. Then, for such main effects and interactions as have emerged as significant, pursue contrasts to sharpen the focus of the analysis.

Cf. the first paragraph of section 12.5.1, at 353: -1: "The choice of the one-way codes ... determines ... the interaction codes". This is an unnecessarily rigid approach to the analysis of higher-order designs.

441 Exhibit 14.30 displays one of the features of this approach that seems to me to be a disadvantage: the labels "*W*" and "*X*" are not very informative (except, I suppose, that "*W*" may seem to suggest "Within-Subjects"). I found it useful to add mnemonic labels to each variable: $Color_1$ for X_1, $Color_2$ for X_2, *Height* for X_3, $Ht*C_1$ for X_4, $Ht*C_2$ for X_5; $Letter_1$ for W_1, $Letter_2$ for W_2, *Number* for W_3, $No.*L_1$ for W_4, $No.*L_2$ for W_5, ..., $No.*L_2*Ht*C_2$ for W_5X_5.

451: -1*ff* "First, ... omnibus tests ... are difficult to interpret ..." In my view, before one attempts an interpretation one ought to be reasonably persuaded that there is indeed something to be interpreted. The omnibus tests are easier to perform to begin with; whenever an omnibus test is rejected, it is clearly in order to pursue the alternative hypothesis in question in finer detail. On the other hand, if the *SS* associated with an omnibus test is too small to permit rejecting a focussed null hypothesis *even if all the SS were concentrated in a single contrast*, the work involved in generating the appropriate *X*'s and/or *W*'s clearly exceeds their worth. Moreover, as I have remarked above (p 348), it is not always the case that the most appropriate contrast coding for an interaction is the set of products of the contrasts for the corresponding main effects. Whenever the shape of an interaction is not specified in advance by theory, it seems to me superior practice to inspect the cell means involved when the interaction emerges as significant, to find a parsimonious way of describing the interaction.