TABLE 3
The impact of point 3 on some regression results for the data in Stewart's Table 3[a]

Var.	ι_j		IMP_j		$\tilde{\beta}$		Var ($\tilde{\beta}$)	
	F	R	F	R	F	R	F	R
X_1	.213	.181	.04	.07	2.19	1.89	.041	.129
X_2	.602	.470	.06	.26	1.15	.90	.004	.063
X_3	.104	.090	.05	.06	.76	.63	.029	.043
X_4	.171	.083	.04	.19	.49	.23	.003	.066
X_5	.001	.247	.06	.48	.02	10.02	1.065	95.988

[a] F denotes full data and R denotes reduced data (point 3 deleted). Values of ι_j and IMP_j are from Stewart's equations (5.1) and (5.2), respectively.

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ADDITIONAL REFERENCES

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VELLEMAN, P. F. and VELLEMAN, A. Y. (1986). *The Data Desk Handbook.* Data Description, Ithaca, N. Y.

Rejoinder

G. W. Stewart

I would like to begin by thanking the commentators for giving my paper a fair and careful reading. Since the following remarks must necessarily focus on our differences, let me stress at the outset that I find much to agree with in their comments.

I am happy to acknowledge that Donald Marquardt knew of the connection between variance inflation factors and collinearity. My only quibble is that one must read a rather small section of his 1970 paper very carefully in order to see it. Marquardt never uses the word collinearity and only asserts that the variance inflation factors depend on the partial correlations, without explicitly stating the nature of the dependency. From his comment one can deduce that he takes a partial correlation near one as a synonym for collinearity and means for the reader to infer that the dependency is the same as the one he writes down for two variables. However, the passage can also be read as a vague afterthought, which is how I interpreted it on first reading.

On nomenclature, the difficulty with the term variance inflation factor is that it draws attention to one effect of near collinearity to the exclusion of other, equally important effects. It seems more natural to me to give a simple characterization of near collinearity and then show how it affects statistical procedures. Taking the square root of the variance inflation factors not only simplifies the formulas but stresses a useful connection with the condition number.

David Belsley's comments are practically a paper in themselves, and a complete response would amount to another. Here I will only make a few observations and trust the reader to sort out the issues.

Belsley would make a distinction between data and models, and in a sense I heartily agree. Numerical and statistical tricks are no substitute for a knowledge of the science underlying a problem. However, on close inspection his distinction appears elusive. Is a constant term model or data? How do we classify the design matrix for an unbalanced analysis of variance? Moreover, the term model has come to mean many things. Belsley's "rather exhaustive" survey evidently did not include Draper and Smith (1981, page 86) or Seber (1977, pages 42 and 43), who use the term model in much the same sense as I do. Attempting to preempt the word model is like trying to tell the tide where to come in.

I will save my comments on importance for the end of this rejoinder. Regarding centering, I will simply restate that centering is a change of variables, and the new ones are not equivalent to the old. There is nothing vague or "psychological" about this observation, and it is ironic that Belsley quotes at length from a passage that describes the psychological biases in the opposing view.

Belsley points out that the collinearity indices do not tell the dimension of the approximate null space and provide little help in selecting an independent