Letters to the Editor

COMMENTS ON LIN (1993)

To reduce experimental cost, the number of factors considered in an experiment might be greater than the number of experimental runs. If only a few of these factors are believed to be active, supersaturated designs are surely useful in setting up such experiments. Lin (1993) offered many such designs. A careful reading of his article, however, reveals some difficulties.

Lin's method for constructing supersaturated designs is basically to use one column in a Hadamard matrix as the branching column to separate experimental runs. This results in two groups of the same size, and Lin's suggestion is to use either group of runs to set up an experiment with more factors than runs. For illustration, he used a real example. Half of the data given by Williams (1968) were taken and analyzed to see if the idea has promise. To further investigate the usefulness of Lin's idea, we also analyzed the *other* half of the Williams data. In the process of doing this, we noticed what appears to be an error in Lin's Table 3. Columns 13 and 16 in that table are identical. We wanted to correct this apparent problem before doing our analysis but found that both Box and Draper (1987) and Williams (1968) gave the data exactly as it is in Lin's article. Thus, if there is a problem with the data as reported by Lin, it cannot be corrected, so we simply used the data as reported. Although this apparent problem makes us unable to distinguish between the effects of factors 13 and 16, it does not affect the validity of our subsequent comments.

After using SAS to reproduce the results in Table 4 of Lin (1993), we also used the same analysis on the other half of the data. The results are summarized in Table 1. Some factors appear important, but factors 10 and 15 are not significant at all. In fact, the model does not include these two factors even when R^2 as large as .99 is used. On the other hand, factors 10 and 15 were identified as important in Lin's analysis of the first half of the data set. This means that one of these two analyses is misleading. To find out which would require subject-matter expertise. But those two different results are enough to support the following advice: "Do not use supersaturated designs for experiments unless you are forced to." When one has to use less runs than factors, Lin's results are surely helpful. But one needs to realize that there is a risk of being misled by the results of the experiment.

The kind of problem noted here is, of course, also present in more familiar contexts. Suppose, for example, that the effect structure in a complete two-level factorial situation is complicated. Then even in the complete absence of experimental variation, the two standard half fractions can yield different pictures of reality, *neither* of which offers a complete view of the effect structure.

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REFERENCES

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Table 1. Stepwise (forward) Selection for the Other Half Fraction of Williams's (1968) Data

	Entering variables						
Step	*4*	*22*	*23*	*18*	*24*	σ	R^2
1	45.14 (2.61)					64.72	36%
2	49.85 (3.26)	−32.98 (−2.15)				56.69	55%
3	45.70 (3.71)	−37.13 (−3.01)	−33.20 (−2.70)			45.25	74%
4	50.25 (4.54)	−48.95 (−3.97)	−37.75 (−3.41)	24.55 (1.99)		39.75	82%
5	51.72 (5.39)	−40.43 (−3.52)	−39.22 (−4.09)	24.26 (2.27)	−20.56 (−2.00)	34.40	88%

NOTE: The constant term is 116.14 at all steps.

RESPONSE

We thank Professor P. C. Wang for pointing out some important issues that, perhaps, should have been addressed in the first place. In a sense, we all live in a supersaturated world—there are *always* more variables than we can handle. Conventional approaches heavily rely on so-called "expert knowledge" to do a first round of variable screening. The contribution of a supersaturated design is to alleviate that knowledge. If the expert's knowledge is indeed correct, the whole supersaturated design will project into a conventional design. On the other hand, if the expert's knowledge is false, the resulting data will still produce useful information.

A fractional factorial design of resolution III confounds main effects with two-factor interactions. This results in increased probabilities of both Type I and Type II errors. Such risks increase when the designs are saturated—that is, when one has no degree of freedom left for the residuals. With supersaturated designs, the main effects are partially confounded with other main effects, and consequently the risk of a "false negative" increases. The difference between resolution III designs and the supersaturated designs, however, is not one of nature but only of degree. Users should be aware of the increased risk of a "false negative" associated with these designs, and the results of data analysis should be evaluated with such confounding in mind. We thus disagree with Wang's comment. "Do not use supersaturated designs for experiments unless you are forced to." Rather, we believe that there is no harm to beginning with an experiment of small size, thereby gaining some knowledge to approve/disapprove prior assumptions and help design the follow-up experiments.

Specifically, in the Williams (1968) example, stepwise selection procedures identify factors **15**, **20**, and **17** (and perhaps **4**); factors **15**, **12**, **20**, **4**, and **10**; factors **4**, **22**, **23**, **18**, and **24** as "active" factors for the full 28-run design, first half-fraction design, and the second half-fraction design, respectively. Examining the correlation matrices in detail for both half fractions, one can see that factors **2**, **4**, **17**, and **15** have correlation $\pm.43$ with each other. Therefore, the practical conclusion is similar for the two half-fraction designs—one should complement the original design with a small design involving factors **2**, **4**, **15**, and **17**. A follow-up experiment mentioned by Lin (1993) is needed to resolve the ambiguities. It is a great benefit to know that all confounding patterns in such supersaturated designs provide useful information for follow-up experimentations.

In conclusion, we would like to offer the following remarks:

1. Supersaturated designs are certainly more risky than designs with more runs. They are, however, far superior to other experimentation approaches, such as subjective selection of factors or changing factors one-at-a-time. The latter can be shown to have unresolvable confounding patterns which cause real problems for data analysis and planning follow-up experiments.
2. Supersaturated designs provide good plans for very early stages of the experimental investigation of complicated systems and processes involving many factors. They should not be used for a terminal experiment. Knowledge of the confounding patterns makes possible the interpretation of the results and provides understanding of how to plan the follow-up experiments.
3. The success of a supersaturated design depends heavily on the "effect sparsity" assumption. Consequently, the projection properties play an important role in designing a supersaturated experiment.
4. Combining several data-analysis methods to analyze the data resulting from a supersaturated design is always recommended. Besides the stepwise selection procedure [and other methods mentioned by Lin (1993)], PLS (partial least squares) and adjusted p value are two promising procedures used to identify active factors.
5. Another particularly suitable use for these designs is to test "robustness" where the objective is not to identify important factors but to vary all possible factors and verify that the response will remain within the specifications.

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