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Report No. 57

A Simple Way to Deal With Missing Observations From Designed Experiments *and* Finding Bad Values in Factorial Designs

George Box

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A Simple Way to Deal With Missing Observations From Designed Experiments

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ABSTRACT

A common difficulty in using designed experiments is that for one reason or another certain observations may be missing. This article discusses a simple way due to Draper and Stoneman to deal with this problem for two level factorials and fractional factorials. Some broader philosophical issues concerning missing observations are also discussed.

KEYWORDS: Designed experiments, two level factorials, fractional factorials

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A Simple Way to Deal With Missing Observations From Designed Experiments

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A common difficulty in using designed experiments is that for one reason or another certain observations may be missing. This article discusses a simple way due to Draper and Stoneman to deal with this problem for two level factorials and fractional factorials. Some broader philosophical issues concerning missing observations are also discussed.

I'm often asked by engineers and other experimenters what to do if one or more of the observations from a factorial, fractional factorial, or other orthogonal array is missing. For example, Table 1 shows the plus and minus signs for the analysis of a 2^4 design from the BH^2 book (Box, Hunter & Hunter, 1978, p. 325).

factors			−	+
A	catalyst charge	(lb)	10	15
B	temperature	(°C)	220	240
C	pressure	(psi)	50	80
D	concentration	(%)	10	12

The runs were actually made in random order but, for convenience, they are listed here in regular standard order. The columns *A*, *B*, *C*, *D*, display the various factor combinations run in the experiment and the last column shows the *response* "% conversion". The other columns can be used to calculate the fifteen *effects* of the various factors and their interactions *AB*, *AC*, ... , *ABCD*. These are shown in Table 2 in the column marked "full design". They are obtained from the columns of signs in the usual way by adding the results corresponding to the plus signs and subtracting the results corresponding to the minus signs and then dividing by eight.

But if an observation is missing from such an experiment what are we to do? One technique is to fit in an appropriate model to the data by least squares (regression). A simpler procedure due to Yates (1933) which gives the same estimated effects is to

Table 1
The plus and minus signs used to calculate the effects from a 2^4 factorial design.

run	*A*	*B*	*C*	*D*	*AB*	*AC*	*AD*	*BC*	*BD*	*CD*	*ABC*	*ABD*	*ACD*	*BCD*	*ABCD*	conversion (%)
1	−	−	−	−	+	+	+	+	+	+	−	−	−	−	+	71
2	+	−	−	−	−	−	−	+	+	+	+	+	+	−	−	61
3	−	+	−	−	−	+	+	−	−	+	+	+	−	+	−	90
4	+	+	−	−	+	−	−	−	−	+	−	−	+	+	+	82
5	−	−	+	−	+	−	+	−	+	−	+	−	+	+	−	68
6	+	−	+	−	−	+	−	−	+	−	−	+	−	+	+	61
7	−	+	+	−	−	−	+	+	−	−	−	+	+	−	+	87 (w)
8	+	+	+	−	+	+	−	+	−	−	+	−	−	−	−	80
9	−	−	−	+	+	+	−	+	−	−	−	+	+	+	−	61
10	+	−	−	+	−	−	+	+	−	−	+	−	−	+	+	50
11	−	+	−	+	−	+	−	−	+	−	+	−	+	−	+	89
12	+	+	−	+	+	−	+	−	+	−	−	+	−	−	−	83
13	−	−	+	+	+	−	−	−	−	+	+	+	−	−	+	59 (x)
14	+	−	+	+	−	+	+	−	−	+	−	−	+	−	−	51
15	−	+	+	+	−	−	−	+	+	+	−	−	−	+	−	85
16	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	78
	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	← divisor

plug in a "fitted value" for the missing observation and carry out the analysis as before. An easy way to do this for factorial-type designs is described in detail by Draper and Stoneman (1964). It goes like this.

Consider a sixteen run experiment like that in Table 1, but suppose there is a missing observation. Since we now do not have a complete set of data we can no longer estimate *all* fifteen effects. So if we were using the least squares method we might take some effect which we believed would be negligible and omit it from the model. This turns out to be equivalent to substituting a fitted value obtained by *setting* the supposedly negligible effect equal to zero. It sounds more complicated than it is, so let's try it.

Suppose in Table 1 that observation number thirteen is missing. Now in fact the response actually observed for this run was fifty-nine, but let's assume we don't know this. Call the response from this run "x"—the unknown quantity. In the absence of any evidence to the contrary, we might expect the highest order interaction *ABCD* is the effect most likely to be negligible. On this assumption we can estimate the missing value x by *setting* this *ABCD* effect equal to zero. Using the *ABCD* column of signs in Table 1 and writing x for the unknown value of the thirteenth observation we get

$$71 - 61 - 90 + 82 - 68 + 61 + 87 - 80 - 61 + 50 + 89 - 83 + x - 51 - 895 + 78 = 0.$$

So that $\quad -61 + x = 0$ and $x = 61$.

An analysis with this "fitted" value substituted for observation thirteen is shown in Table 2. For this set of data, where the *ABCD* interaction estimated for the complete data was small to begin with, this has little effect on the estimates.

The same idea can be applied with two observations missing. For illustration suppose that in addition to the observation missing from run thirteen, which we called x, the observation from run seven is also missing. Let's call this one w. It will be possible to obtain fitted values for both these missing observations if we can assume that two *suitably chosen* effects are negligible.

What I mean by "suitably chosen" is best explained by an example. Suppose, as before, we select *ABCD* as our first "null" column. If you look at the plus and minus signs for this column you will notice that in rows seven and thirteen, where there are missing data, the *same sign* (+) occurs. Therefore we must arrange that, for our second null column, the signs in rows seven and thirteen are *different*. Thus we could choose *ABC* or *ACD* but not *ABD* or *BCD*

Table 2
Estimates of effects for the 2^4 factorial designs: (a) with observation thirteen omitted and ABCD assumed to be zero and (b) with observations thirteen and seven omitted and both ABCD and ABC assumed to be zero.

	full design	*(a)*	*(b)*
average	72.25	72.375	72.375
A	−8.00	−8.25	−8.25
B	24.00	23.75	23.25
C	−2.25	−2.00	−2.00
D	−5.50	−5.25	−4.75
AB	1.00	1.25	1.75
AC	0.75	0.50	0.50
AD	0.00	−0.25	−0.75
BC	−1.25	−1.50	−2.00
BD	4.50	4.25	4.25
CD	−0.25	0.00	0.50
ABC	−0.75	−0.50	assumed zero
ABD	0.50	0.75	0.75
ACD	−0.25	−0.50	−1.00
BCD	−0.75	−1.00	−1.00
ABCD	−0.25	assumed zero	assumed zero

as the second null column. For illustration lets choose *ABC*. Then

$$ABCD = 0 \text{ gives } w + x = 148$$

$$ABC = 0 \text{ gives } w - x = 22 \text{ .}$$

You can see now why we need the signs to switch for the missing rows in the two "null" columns. This is to ensure that the two equations we end up with will have a solution. In this example solving the equations by first adding them together and then subtracting one from the other we get

$$w = 85, \; x = 63 \text{ .}$$

The result of plugging in these fitted values in Table 1 is shown in the last column of Table 2.

The various effects do not differ materially from those calculated originally. Again, this is because, from the full set of data, both the *ABCD* and the *ABC* interactions have small effects.

This simple computational device can also be used for the analysis of *fractional* factorials and other orthogonal arrays when observations are missing. The method will give the same estimates as a least squares analysis in which the effects we have put equal to zero are *omitted* from the model. It has the advantage that it is usually a lot easier to do. Notice that the result of this procedure depends somewhat on which columns we choose to set equal to zero. This is because the least squares solution depends on which effects we choose to drop out of the model.

Usually the best way to *analyze* designs of this kind, whether you have missing observations or not, is to make a Daniel plot using normal probability paper. In such a plot, points falling off the straight line are likely to represent real effects not easily explained as merely due to noise (experimental error). But remember that, for example, if you have two missing observations from a sixteen-run design you have only really *estimated* thirteen effects (you *set* the other two equal to zero). These zero values should not be plotted therefore. The remainder may be arranged in order of size and plotted in the usual way at % probability values given by $P_i = 100(i - ½)/m$ with $m = 13$ (BH^2 p. 330). This will provide an adequate approximate analysis so long as only a few observations are missing. In particular if you do this for the data in Table 1 you will find that the same effects A, B, D and BD, indicated in Table 2 by arrows, show up as distinguishable from noise with or without missing observations.

Remember, as for any other technique, that the results are only as good as the assumptions. Thus, the "null columns" ought to be chosen, not only so as to make the equations solvable but, so that the effects (or with fractional designs the strings of aliased effects) correspond to quantities that you really think are likely to be small. Remember also that what we have here is simply a convenient computational device. It does not of course recover the information that has been lost. For example, in my earlier column in CQPI Report No. 46 I referred to an eight run experiment on ball bearings by Hellstrand. The discovery that the life of the bearings could be increased five-fold rested entirely on the experiment results in which two factors—heat treatment, and outer ring osculation—were increased *together*. If these particular results had been missing, no statistical analysis of any kind could have recovered this vital information. So don't rely on your results if you have too many missing observations. Usually, I would start to feel uncomfortable with the analysis when there was more than one missing observation in an eight run experiment, or more than two observations missing from a sixteen run experiment.

So much for the technique. Now let's talk about some, perhaps more important, philosophical issues concerning missing observations. When we have missing observations it is always best if possible *to get them repeated* (usually with some other runs repeated for comparison, in case something has slipped). But often the most important question about a missing observation is "Why is it missing?" Because questions such as this about the conduct of the experiment almost always come up, it is very important to keep a notebook with a detailed record of what happened in each run. Perhaps the missing value has occurred *simply* because of a failure to record, or, of much more concern, because the machine or process could *not be run* at these particular set of conditions. In either case it is important to follow up on such possibilities immediately. The fact that the process cannot be run at particular conditions is, in itself, important information. Suppose, for example, that the other runs suggest that the conditions that "cannot be run" might be especially favorable. In that case we ought to ask whether the problem of making these runs is an insuperable one, or whether by some not too difficult modification these conditions could in fact be tried.

Sometimes an observation is not actually missing, but is, for some reason or another, *suspect*. In that case you may want to use this technique to discover what the effect of dropping that particular observation might be. But if a large discrepancy is found between the observed and the fitted value you should *not* just automatically substitute the fitted value in the analysis. The difference you find may contain important information. The first thing to do is to look at the notes made at the time of performing the suspect run to see if there were unusual circumstances that might explain the result. If so it may be worthwhile to run a repeat which replicates the recorded peculiarities of the original run to see if the result can be duplicated. For, suppose the problem was to define conditions that gave a *high* value for the response. If the discrepancy indicated an exceptionally high value, this could be telling us that something we accidentally did differently was especially desirable. On the other hand if the discrepancy indicated an unusually low value, this could be telling us these are conditions were to be especially avoided. Although most of the time it is the main effects and two-factor interactions that are important in a factorial design, there is no law that says that every phenomenon fits into that pattern. Occasionally a particular combination of factor levels may give an unusually good or bad result which is not explicable in terms of main effects and low order interactions. For example the exceptional hardness of certain steel alloys and the necessary conditions for an atomic explosion depend on the unique coming together of certain specific levels of a large number of factors. We must therefore always be ready to learn from repeatable occurrences however odd they may look at first sight.

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Finding Bad Values in Factorial Designs

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ABSTRACT

Sometimes the results from a designed experiment contain "bad or suspect" values. This article discusses a simple way due to Cuthbert Daniel of detecting a bad value. It also describes how you might re-estimate its value. More general issues are considered surrounding observations that appear discrepant.

KEYWORDS: Designed experiments, factorial designs, Daniel plot

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Finding Bad Values in Factorial Designs

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Sometimes the results from a designed experiment contain "bad or suspect" values. This article discusses a simple way due to Cuthbert Daniel of detecting a bad value. It also describes how you might re-estimate its value. More general issues are considered surrounding observations that appear discrepant.

Look at the data in Table 1. It shows a 2^4 factorial experiment from Box and Meyer (1987). Shown in Figure 1 are the effects plotted on normal probability paper.

As you may know, this plot (which I prefer to call a Daniel plot in honor of its originator) is a simple, but most valuable, tool for discriminating between effects likely to be due to noise and those effects which are almost certainly real. The former will plot as points on a straight line, the latter will fall off the line. At first sight it looks as if you might draw a rough straight line through all the points, indicating that there were no effects detectable different from noise. But you can see that a better fit might be obtained by drawing *two* straight lines. Cuthbert Daniel (1976) pointed out that such a plot provides a strong clue that you have a discrepant data value.

You can see why this would be by imagining what would happen with a *good* set of data if you miswrote one of the data values. For illustration let's say for observation number three you had written down 53.13 when it should have been 43.13, thus making that value ten units too high. Now remember you calculate these effects column by column by adding together all the data values opposite plus signs and subtracting these opposite minus signs. The resulting contrasts are divided by eight to give the effects A, B, C, D, AB, AC ... etc. Now if observation

Table 1

Results from a 2^4 factorial design with calculated effects before and after adjustment.

Effects	–0.80	–4.22	3.71	1.01	0.91	–2.49	–0.58	–0.80	–1.18	1.49	1.20	0.72	0.40	–1.58	1.52	
Adjusted Effects	0.00	–3.42	2.91	0.21	0.11	–1.69	0.22	0.00	–0.38	0.69	0.40	–0.08	1.20	–0.78	0.72	
Run	*A*	*B*	*C*	*D*	*AB*	*AC*	*AD*	*BC*	*BD*	*CD*	*ABC*	*ABD*	*ACD*	*BCD*	*ABCD*	
1	–	–	–	–	+	+	+	+	+	+	–	–	–	–	+	47.46
2	+	–	–	–	–	–	–	+	+	+	+	+	+	–	–	49.62
3	–	+	–	–	–	+	+	–	–	+	+	+	–	+	–	43.13
4	+	+	–	–	+	–	–	–	–	+	–	–	+	+	+	46.31
5	–	–	+	–	+	–	+	–	+	–	+	–	+	+	–	51.47
6	+	–	+	–	–	+	–	–	+	–	–	+	–	+	+	48.49
7	–	+	+	–	–	–	+	+	–	–	–	+	+	–	+	49.34
8	+	+	+	–	+	+	–	+	–	–	+	–	–	–	–	46.10
9	–	–	–	+	+	+	–	+	–	–	–	+	+	+	–	46.76
10	+	–	–	+	–	–	+	+	–	–	+	–	–	+	+	48.56
11	–	+	–	+	–	+	–	–	+	–	+	–	+	–	+	44.83
12	+	+	–	+	+	–	+	–	+	–	–	+	–	–	–	44.45
13	–	–	+	+	+	–	–	–	–	+	+	+	–	–	+	59.15 (57.75)
14	+	–	+	+	–	+	+	–	–	+	–	–	+	–	–	51.33
15	–	+	+	+	–	–	–	+	+	+	–	–	–	+	–	47.02
16	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	47.90
Signs of likely "error" effects	–			+	+	–	–	–	–	+	+	+	+	–	+	

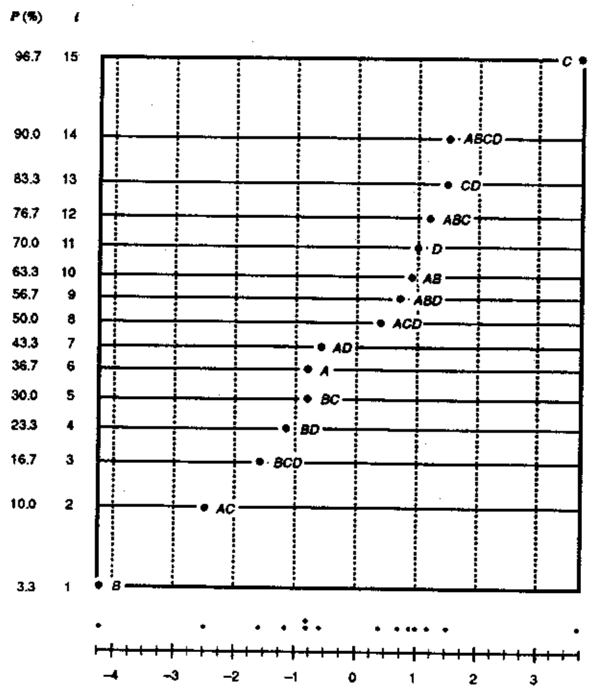

Figure 1 *Daniel plot of estimated effects from 2^4 design.*

number three is too high by ten units, you can see that the effect contrast for the main effect A will be ten units too *low* because the column for A has a minus sign in row three. Similarly the main effect contrast for B will be ten units too *high* because the column for B has a plus sign in row three, and so on. So after you've divided by eight the A effect will be discrepant by $^{-10}/_{8} = -1.25$ (1.25 units too low) and the B effect by $^{10}/_{8} = +1.25$ (1.25 units too high). If you look at the table of plus and minus signs opposite row three you can see that the effect of the discrepant value would be to make eight of the fifteen effects too low by -1.25 units and the remaining seven too high by $+1.25$ units.

Now think about the effects which are just due to noise (experimental error). If there were no bad values, these "error" effects ought to plot as a straight line cutting the 50% probability line close to zero on the horizontal axis. But because of the discrepant value, some of these error effects will be biased upwards and the others downwards so you could expect the data in the middle of the plot to appear not as one, but as *two*, straight lines.

Now think of the problem the other way around. You've made the plot and it looks like two straight lines rather than one, just as in Figure 1. What you want to find out is *which* data value is responsible for the discrepancy. I have indicated by plus signs below Table 1 all the positive effects that plot as the upper straight line in Figure 1 and by minus signs all the negative effects that plot as the lower straight line. Now cast your eyes over the rows of signs corresponding to the various observations and see if you can see a row of pluses and minuses that nearly matches this (or one that matches it if all the signs are reversed, because the discrepancy could be either way). You will see that in row thirteen all the signs except one match, so that the discrepant observation appears to be number thirteen. A more precise way to check the matching is to calculate the cross products of the signs in the rows of the table with the supposed error effects. The one that matches the best will give the largest cross product.

Let us now take this a stage further. Assuming that there is a discrepancy in observation number thirteen, what is its estimated magnitude? A rough way to make an estimate is as follows: Look at Figure 1. The plotted effects on the two lines which are closest to zero are most likely the result of the positive and negative biases ($\pm d$ say) plus error. So let us say that the true effects for $A, D, AB, BC, AD, BD, ABD$, and ACD are probably in reality small or nonexistent.

Then we have eight estimates for d obtained from

$$-A = 0.80, +D = 1.01, +AB = 0.91,$$
$$-BC = 0.80, -AD = 0.58, -BD = 1.18,$$
$$+ABD = 0.72, +ACD = 0.40\ .$$

On the assumption that the true values for these effects is zero the least squares estimate for d is obtained by averaging them to give $\bar{d} = 0.80$. Correspondingly observation thirteen is estimated to be $0.80 \times 8 = 6.40$ units too high.

We cheat a bit in getting these estimates, because we look at the data first to decide which effects to call error. A more precise method is given in the Box and Meyer paper but the results aren't very different. Using the adjusted value $59.15 - 6.40 = 52.75$ for observation thirteen we obtain the "adjusted effects" shown above Table 1 and a Daniel plot of these are shown in Figure 2. We see after making the adjustment that the main effects for factors B and C and the interaction AC are probably real.

In all of this remember what I said in my last column. A discrepant value should not just be thrown away—it might be trying to tell us something! An awful warning is supplied by the "hole" in the ozone layer over the Antarctic discovered by British scientists. After the existence of the hole was confirmed the question was asked "how come the NASA satellite, that had been continually circling the

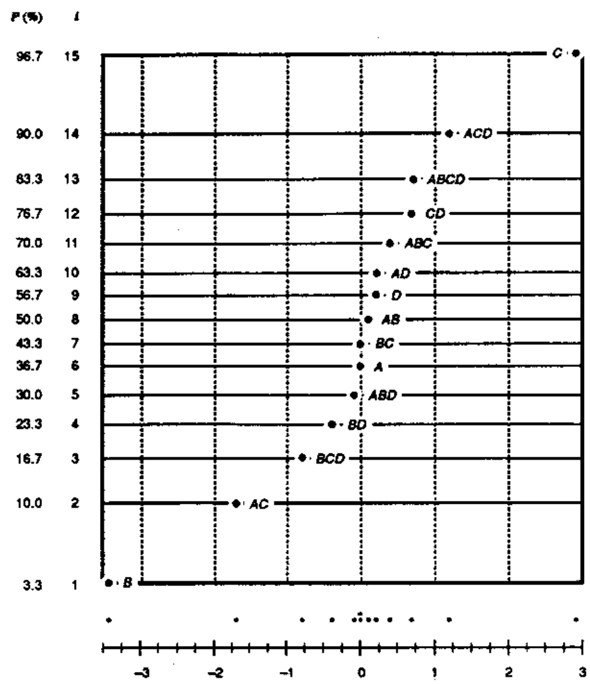

Figure 2. Daniel plot of adjusted effects from 2^4 design.

earth for the past several years, didn't find it?" The answer is that these data were automatically checked for outliers by a computer program and the hole in the ozone layer was screened out!

Let that be a lesson to us all.

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Letter codes indicate report type: *T* = technical; *M* = management; *G* = general; *S* = case study.

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43. *T*	**Process Control From An Economic Point of View–Chapter 2: Fixed Monitoring and Adjustment Costs**	Tim Kramer
44. *T*	**Process Control From An Economic Point of View–Chapter 3: Dynamic Adjustments and Quadratic Costs and Chapter 4: Summary and Future Research**	Tim Kramer
45. *TS*	**An Application of Taguchi's Methods Reconsidered**	Veronica Czitrom
46. *TG*	**Do Interactions Matter?**	George Box
47. *TG*	**Must We Randomize Our Experiment?**	George Box
48. *TG*	**Good Quality Costs Less? How Come?**	George Box
49. *TM*	**Design of Standards and Regulations**	Søren Bisgaard
50. *S*	**An Application of Box–Jenkins Methodology to the Control of Gluten Addition in a Flour Mill**	T. Fearn and P. I. Maris
51. *T*	**Existence and Uniqueness of the Solution of the Likelihood Equations for Binary Markov Chains**	Søren Bisgaard and Laurel E. Travis
52. *S*	**Quality Improvement Approaches for Chemical Processes**	William J. Hill and Lane Bishop
53. *T*	**Constrained Experimental Designs Part I: Construction of Projection Designs**	Ian Hau and George Box
54. *T*	**Constrained Experimental Designs Part II: Analysis of Projection Designs**	Ian Hau and George Box
55. *T*	**Constrained Experimental Designs Part III: Steepest Ascent and Properties of Projection Designs**	Ian Hau and George Box
56. *T*	**Designing Products That Are Robust to the Environment**	George Box and Stephen Jones
57. *TG*	**A Simple Way to Deal With Missing Obsevations From Designed Experiments *and* Finding Bad Values in Factorial Designs**	George Box

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