Experimental design and regression analysis

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Case Study

Experimental design and regression analysis in simulation: An FMS case study

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Abstract: The machine mix for a particular FMS, the number of machines performing each of three operations and the number of machines performing any of the three operations (flexible machines), is input to an FMS simulation. An intuitively selected combination of these four inputs are compared to a 2^{4-1} fractional factorial design. The throughput predicted by the simulation is analyzed through two different regression models. These models are validated. A regression model in two inputs including their interaction, gives valid predictions and stable explanations.

Keywords: Statistics, fractional factorial design, regression model, analysis of variance model, metamodel, validation, cross-validation, flexible manufacturing system

Introduction

Simulation is a technique applied in many areas because of its flexibility, simplicity and realism. However, because simulation involves experimenting (with the model of a real system) it requires statistical design and analysis. The present paper concentrates on strategic issues, namely, which variants of the simulation model are actually run (i.e., which combinations of parameter values are input), and how can the resulting output be analyzed? Strategic issues arise in both random and deterministic simulation, whereas tactical issues (like runlength and confidence intervals) arise only in random simulation. The case study of the present paper concerns a deterministic simulation model of a Flexible Manufacturing System (FMS).

FMS

Figure 1 shows one possible layout of the flexible manufacturing system being studied [2]. A cart path surrounds the machines. One cart moves parts of one type between the wash station and the machine to perform the next operation. After the operation is completed, the cart moves the part back to the wash station. Parts are washed before each operation is performed and after the last operation. Parts enter from the lathes and exit to the inspection station. There are three operations, OP.10, OP.20 and OP.30. In Figure 1, five machines perform OP.10, two OP.20, two OP.30 and one is flexible to perform any of the three operations.

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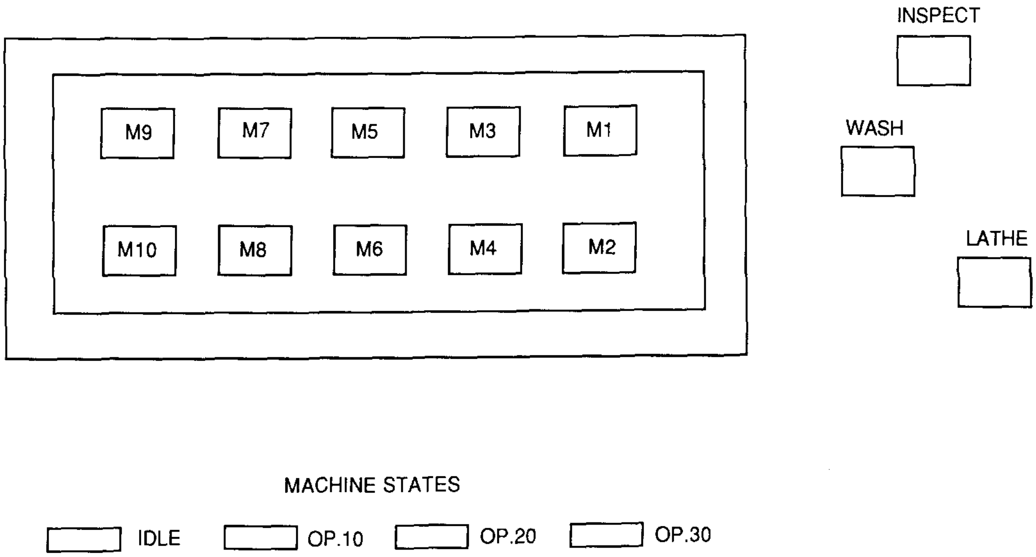

Figure 1. Evaluation of FMS design

Problem scope

The case-study determines the machine mix, the number of machines performing each operation and the number of flexible machines performing any operation:

x_1: number of machines for operation 10,
x_2: number of machines for operation 20,
x_3: number of machines for operation 30,
x_4: number of flexible machines for operation 10, 20 or 30.

The machine mix (combination of x_1, x_2, x_3, x_4) should produce a throughput of 3140 parts per week. The scope of the case study is restricted to a given control logic of the FMS. The system operates on only one part type.

An algebraic, naive solution to the machine-mix problem is typically computed as follows. The throughput constraint (3140 parts per week) means that the number of parts produced per minute is 0.73. It is known that operations 10, 20 and 30 require per part: 7.5, 2.5 and 3.5 minutes respectively. Hence the computed machine mix is $x_1 = 5.4$, $x_2 = 1.8$ and $x_3 = 2.5$.

Rounding these results up produces a machine mix of $x_1 = 6$, $x_2 = 2$, and $x_3 = 3$. This machine mix provides excess capacity for each machine type. Thus no flexible machines are necessary, $x_4 = 0$. Rounding these results down produces a machine mix of $x_1 = 5$, $x_2 = 1$ and $x_3 = 2$. The capacity needed from flexible machines is $(0.4 + 0.8 + 0.5 = 1.7)$. Thus $x_4 = 2$. Flexible machines are much more expensive than machines performing one operation (fixed machines). Thus, substituting flexible machines for fixed machines need not be considered.

The naive solution suggests the following experimental area over which the inputs to the simulation may vary:

$$\begin{aligned} &5 \leqslant x_1 \leqslant 6, \\ &1 \leqslant x_2 \leqslant 2, \\ &2 \leqslant x_3 \leqslant 3, \\ &0 \leqslant x_4 \leqslant 2. \end{aligned} \tag{1}$$

Experimental design

Given the problem of the preceding section, 24 combinations might be simulated. In many simulation studies the number of combinations is much greater! How a *fraction* of all possible combinations still yields adequate results is illustrated.

Table 1 specifies an *intuitively* selected fraction of input combinations, specified without a knowledge of the statistical theory of experimental design.

Formal experimental design theory proceeds as

Table 1
Intuitive fractional design [a]

Run i	OP. 10 x_1	OP. 20 x_2	OP. 30 x_3	Flexible x_4	(Total Σx_j)
1	−	−	−	−	(8)
2	+	−	+	M	(11)
3	−	+	+	M	(11)
4	+	+	−	−	(10)
5	−	−	+	+	(11)
6	+	−	−	M	(10)
7	−	+	−	M	(10)
8	+	+	+	+	(13)

[a] Legend: − minimum value of x_j ($j=1,\ldots,4$); + maximum value, M middle value of x_4 ($x_4=1$).

follows. Each 'factor' (independent variable x_j with $j=1,\ldots,4$) is studied at only two 'levels' (values). (In the case study the factors have a very narrow range and the levels must be integer numbers; hence the two levels coincide with the possible levels for the first three factors; obviously a possible value for x_4 is one, which does not coincide with its levels, zero and two; see eq. (1).) This restriction still yields 2^4 combinations of input values. Statistical theory suggest that only a fraction of these 2^4 combinations be simulated. How big that fraction should be depends on the (regression or analysis of variance) *metamodel*. If it could be assumed that the simulation model yields a throughput y equal to the *additive* effects of the four inputs x_j ($j=1,\ldots,4$), then only five input combinations would suffice. In other words, if the relationship between the simulation model's output y and its inputs x_j could be approximated by the metamodel

$$\hat{y}=\hat{\beta}_0+\hat{\beta}_1 x_1+\hat{\beta}_2 x_2+\hat{\beta}_3 x_3+\hat{\beta}_4 x_4 \tag{2}$$

Table 2
Formal 2^{4-1} fractional factorial design

Run i	OP. 1 x_1	OP. 2 x_2	OP. 3 x_3 [a]	Flexible x_4	(Total Σx_j)
1	−	−	−	−	(8)
2	+	−	+	−	(10)
3	−	+	+	−	(10)
4	+	+	−	−	(10)
5	−	−	+	+	(11)
6	+	−	−	+	(11)
7	−	+	−	+	(11)
8	+	+	+	+	(13)

[a] $x_{i3}=x_{i1}x_{i2}x_{i4}$ ($i=1,\ldots,8$); see also [1].

then then five estimated effects $\hat{\beta}_{j'}$ could be computed from only five simulation runs ($j'=0,1,\ldots,4$). Actually it seems dangerous to assume apriori that the simulation inputs do not interact. Therefore more than five combinations are simulated. Statistical theory (see Kleijnen, 1987) results in the fractional design of Table 2.

A 'run' means that a row of Table 1 or 2 is translated into the input values x_{ij}; this input is transformed by the simulation model into the simulation response y_i. Both the intuitive and the formal design require eight simulation runs: $n=8$. The only difference between the formal and intuitive design is the use of x_4 equal to 1 in runs 2, 3, 6 and 7. The next section will show that the formal design gives better conclusions.

Metamodel calibration and validation

Although the experimental designs of Tables 1 and 2 comprise more than five runs, the analysis of the experimental results starts assuming that the simple additive regression model of eq. (2) is valid; this assumption will be checked later. The effects $\hat{\beta}_{j'}$ of eq. (2) are computed (the metamodel is 'calibrated') using the Ordinary Least Squares (OLS) algorithm

$$\hat{\beta}=(X'X)^{-1}X'y \tag{3}$$

where $\hat{\beta}'=(\hat{\beta}_0, \hat{\beta}_1, \hat{\beta}_2, \hat{\beta}_3, \hat{\beta}_4)$, $X=(x_{ij})$ with $i=1,\ldots,8$ and $j'=0,1,\ldots,4$, $y'=(y_1,\ldots,y_8)$. The standard OLS algorithm also yields the covariance matrix

$$\Omega_{\hat{\beta}}=(X'X)^{-1}\sigma^2 \tag{4}$$

where σ^2 denotes the variance of the error terms $y-\hat{y}$, that is, if all possible combinations of inputs x_j were simulated resulting in the simulation responses y and if the inputs x_j were also used in the regression model of eq. (2), then the prediction errors $e=y-\hat{y}$ would form a statistical distribution with variance σ^2; see Kleijnen (1987). This σ^2 is estimated through the Mean Squared Residuals (MSR):

$$\hat{\sigma}^2=\frac{\Sigma_{i=1}^{n}(y_i-\hat{y}_i)^2}{n-q} \tag{5}$$

where q denotes the number of effects ($q=5$). Obviously $\hat{\mathrm{var}}(\hat{\beta}_j)$ is the j-th element on the main

Table 3
Estimated variance of estimated effects in additive model: $\hat{\text{var}}(\hat{\beta}_{j'})$

Effect	Intuitive design	Formal design
$\hat{\beta}_1$ (operation 1)	0.5	0.5
$\hat{\beta}_2$ (operation 2)	0.5	0.5
$\hat{\beta}_3$ (operation 3)	1.0	0.5
$\hat{\beta}_4$ (flexible)	0.5	0.13
$\hat{\beta}_0$ (constant)	20.6	19.6

Table 4
Stability of significant $\hat{\beta}$ upon run deletion

Run deleted	$\hat{\beta}_1$ (OP. 10)	$\hat{\beta}_2$ (OP. 20)	$\hat{\beta}_3$ (OP. 30)	$\hat{\beta}_4$ (flex.)	$\hat{\beta}_0$ (const.)
1		557		577	
2		712		500	2032
3		640		700	
4		629		694	
5				658	1962
6				736	
7				536	
8				541	3288
none				541	3288

diagonal of $\hat{\Omega}_{\hat{\beta}}$ obtained from eqs. (4) and (5). Eqs. (2) through (5) yield Table 3, which shows that the formal design gives more accurate estimates of the factor effects. Therefore the rest of the paper concentrates on the results of the formal design.

One way of validating the calibrated metamodel would be to ask: what happens to the values of the estimated effects $\hat{\beta}_{j'}$ if one run is *deleted* from Table 2? Obviously these values do change, certainly the value of the non-significant effects. This significance is tested through the t-statistic:

$$t_v^{(j')} = \frac{\hat{\beta}_{j'}}{\hat{\text{var}}(\hat{\beta}_{j'})}, \quad j' = 0, 1, \ldots, 4, \tag{6}$$

where $v = n - q$ so that $v = 3$ if no run is deleted and $v = 2$ is one run is deleted. The significant effects should remain stable upon run deletion.

Table 6
Stability of significant $\hat{\gamma}$ upon run delection

Run deleted	$\hat{\gamma}_2$ (OP. 20)	$\hat{\gamma}_4$ (flex.)	$\hat{\gamma}_{2,4}$ (interact.)	$\hat{\gamma}_0$ (const.)
1	952	1364	−492	776
2	952	1300	−460	776
3	952	1324	−468	776
4	952	1340	−484	776
5	1152	1432	−576	576
6	752	1232	−376	976
7	952	1332	−476	776
8	952	1332	−476	776
none	952	1332	−476	776

Table 4 displays only the effects that are significant at $\alpha = 0.3$.

A more compact way of evaluating the effect of run deletion, is to concentrate on the predictor $\hat{y}$, in other words, the criterion becomes *prediction* instead of *explanation*. Table 5 displays the 'relative prediction error'

$$r_i = \frac{y_i - \hat{y}_i}{y_i}, \quad i = 1, \ldots, 8, \tag{7}$$

where $\hat{y}_i$ denotes the predicted response (throughput) using eq. (2) but now estimating the effects $\hat{\beta}_{j'}$ using only $n - 1$ runs, namely deleting run i from the simulation data: *cross-validation*.

The errors in Table 5 and the instabilities in Table 4 are so large that the additive regression model of eq. (2) is rejected, and a new metamodel is investigated (using the old data of the simulation experiment).

An alternative metamodel

Table 4 suggests that the factors x_1 and x_3 are not important. Therefore a regression model in the remaining factors (x_2 and x_4) is formulated, including possible interaction between these two factors:

$$\hat{y} = \hat{\gamma}_0 + \hat{\gamma}_2 x_2 + \hat{\gamma}_4 x_4 + \hat{\gamma}_{2,4} x_2 x_4. \tag{8}$$

Table 5
Cross-validation: $(y_i - \hat{y}_i)/y_i$

Run deleted:	1	2	3	4	5	6	7	8
Relative error: (in %)	10	27	−19	−18	13	33	−38	−35

Table 7
Cross-validation of eq. (8)

Run deleted:	1	2	3	4	5	6	7	8
Rel. error(%):	2	2	−1	1	−16	14	0	0

Table 8
Double-check of eq. (8)

x_2 (OP. 20)	x_4 (flex.)	x_2x_4 (interact.)	y (thruput)	$\hat{y}$ (predict.)	r (%) (error)
2	1	2	1368	3060	3
1	1	1	3456	2584	25
2	1	2	3408	3060	10
1	1	1	2896	2584	11

Table 4 is now replaced by Table 6: in the alternative model all estimated effects remain significant upon deletion of run i ($i = 1,\ldots,8$).

Table 5 is now replaced by Table 7. The relative prediction errors become much smaller. The model can also be *double-checked* in this case-study, because there are simulation data available for some extra input combinations (besides the eight runs of Table 2), namely the combinations listed in Table 1 but not in Table 2. The four combinations in Table 8 were not used to calibrate eq. (8) and are now used to double-check the predictive power of the alternative regression (meta)model.

Thus the metamodel is

$$\hat{y} = 776 + 952x_2 + 1332x_4 - 476x_2x_4.$$

This model suggests:

(1) Machines performing operation 20 and flexible machines are the bottlenecks in the system.

(2) There is a trade-off between using more machines performing operation 20 and more flexible machines as shown by the negative coefficient in the interaction term.

Conclusion

The present case-study illustrates how experimental design and regression analysis can be applied to evaluate an FMS. The statistical techniques are quite simple, i.e., the FMS simulation model was available at the beginning of the study and within a few days the results of the present paper (design and analysis) were obtained. Statistical techniques are not an aim in themselves. They can reduce the drawbacks of an empirical technique like simulation, i.e., at the end of this quick-and-dirty case-study the regression metamodel of eq. (8) helped the authors to better understand how an FMS works!

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