Screening (Sieve) Design of Experiments in Metal Cutting

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This chapter discuses particularities of the use of DOE in experimental studies of metal cutting. It argues that although the cost of testing in metal cutting is high, there is no drive to improve or generalize the experimental results. It explains that full factorial design of experiments and the most advanced group method of data handling (known as GMDH) method allow accurate estimation of all factors involved and their interactions. The cost and time needed for such tests increase with the number of factors considered. To reduce these cost and time, two-stage DOE procedure to be used in metal cutting experimental studies is suggested: screening DOE in the first stage and full factorial DOE in the second stage. The Plackett and Burman DOE is found to be very useful in screening tests in metal cutting studies.

1 Introduction

Although machining is one of the oldest manufacturing processes, most essential characteristics and outcomes of this process such as tool life, cutting forces, integrity of the machined surface, and energy consumption can only be determined experimentally. As a result, new improvements in the tool, machine and process design/optimization, and implementation of improved cutting tool materials are justified through a series of experimental studies. Unfortunately, experimental studies in metal cutting are very costly and time-consuming requiring sophisticated equipment and experienced personnel. Therefore, the proper test strategy, methodology, data acquisition, statistical model construction, and verification are of prime concern in such studies.

Metal cutting tests have been carried out in systematic fashion over at least 150 years, in tremendously increasing volume. However, most of the tests carried out so far have been conducted using a vast variety of cutting conditions and test

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methods having little in common with each other. It is understood that test results are meaningless if the test conditions have not been specified in such a way that the different factors, which affect the test results, will be under a reasonable and practical degree of control. Though this sounds simple and logical, the main problem is to define and/or determine these essential factors.

Unfortunately, there is lack of information dealing with test methodology and data evaluation in metal cutting tests. Some information about setup and test conditions can be found in most of the reported experimental studies. On the contrary, it is rather difficult to find corresponding information about test methodology and answers to the questions of why the reported test conditions or design parameters of the setup were selected at the reported levels, what method(s) was (were) used for experimental data evaluation, etc.

Although the cost of testing in metal cutting is high, there is no drive to improve or generalize the experimental results in the published experimental works and even up to the level of national and international standards. For example, the standard ANSI/ASME Tool Life Testing with Single-Point Turning Tools (B94.55M-1985) suggests conducting the one-variable-at-a-time test. When it comes to acquisition of test results, the only calculation of the confidence interval limits is required to carry out and, thus report. As a result, only the influence of cutting speed on the tool life can be distinguished for a given machine (static and dynamic stiffness, spindle runout, accuracy of motions, etc.), workpiece parameters (metallurgical state, dimensions, holding method, etc.), cutting tool material and cutting tool design, the accuracy of the cutting tool setting in the tool holder, and in the machine spindle (for round tools).

The design of experiments (DOEs) technique allows a significant improvement in the methodology of machining tests. DOE is the process of planning of an experiment so that appropriate data will be collected, which are suitable for further statistical analyses resulting in valid and objective conclusions. Because there are a number of different methodologies of DOE, one is always challenged to select the appropriate methodology depending on the objective of the test and the resources available.

Reading this, a logical question, "what seems to be the problem?" is frequently asked. In other words, why does another chapter or even a book on DOE in machining needed? Indeed, the theory of DOE is fully covered in many fundamental books, e.g. [1–3]; its application to machining studies is discussed by many researches including the author [4–7]. Moreover, there are many commercial DOE software packages as, for example, Minitab by Minitab, Inc., SAS by SAS Institute, Inc., S-Plus by Mathsoft, Inc., Design-Expert by Stat-Ease, Inc., STATISTICA/StatSoft by Dell Software, with detailed online manuals (e.g. http://www.statsoft.com/textbook/experimental-design#general). A great body of the available literature and online sources combined are readily available as commercial software packages that apparently make DOE almost effortless. In the author's opinion, however, the simplicity of DOE is really pseudo-simplicity or masked complexity. That is, when it comes to machining, the available DOE sources represent only the tip of the iceberg, i.e., a much greater, in terms of size and complicity, part is often hidden underwater in dark.

This chapter aims to discuss an important but rarely discussed issue of accounting for factors' numbers and their interactions in DOE machining tests. It argues that including such interactions into the consideration in DOE in metal cutting makes experimental studies not only effective but also efficient. In this context, the term "effectiveness" is understood as doing the right things, i.e. the use of DOE, whereas the term "efficiency" is understood as doing things right, i.e., accounting for the relevant number of factors and their interactions. The latter allows optimization of not only parameters of the machining regime but even intricate parameters of the cutting tool including tool geometry, material, setting, etc.

2 Basic Terminology

DOE is one of the most powerful, and thus widely used statistical methods in machining tests. The outcomes of machining are affected by many factors as shown in Fig. 1a. In order to design a new process or to improve the existing machining process, the relationship between the inputs and outputs should be established. DOE is a statistical formal methodology allowing an experimentalist to establish statistical correlation between a set of inputs (input variables) with chosen outcomes of the system/process under certain uncertainties, called environmental influence. An input factor in a process is determined as a source of variability in the output of the process. Once the process input variables for a process are determined they are often termed as the key process input variables (known as KIPV in the literature). Thus, a statistically-based experiment can be designed so that optimal values for each factor to achieve the desired output quality can be revealed. In this respect, DOE is the process of determine the correlations of KPIVs with the output of the process. A key point of the DOE process is that it changes several variables at once. This allows the statistics behind the process to identify interactions between the KPIVs in terms of their influence on the output.

The visualization of this definition as it used in DOE is shown in Fig. 1b, where (x_1, x_2, … x_n) are n KPIVs selected for the analysis; (y_1, y_2, … y_m) are m possible system/process outputs from which one should be selected for the analysis; and (z_1, z_2, … z_p) are p uncontrollable (the experimentalist has no influence) inputs (often referred to as noise). The system/process is designated in Fig. 1b as a black box,[1] i.e., it is a device, system, or object that can be viewed solely in terms of its input,

[1]The modern term "black box" seems to have entered the English language around 1945. The process of network synthesis from the transfer functions of black boxes can be traced to Wilhelm Cauer who published his ideas in their most developed form in 1941. Although Cauer did not himself use the term, others who followed him certainly did describe the method as black-box analysis.

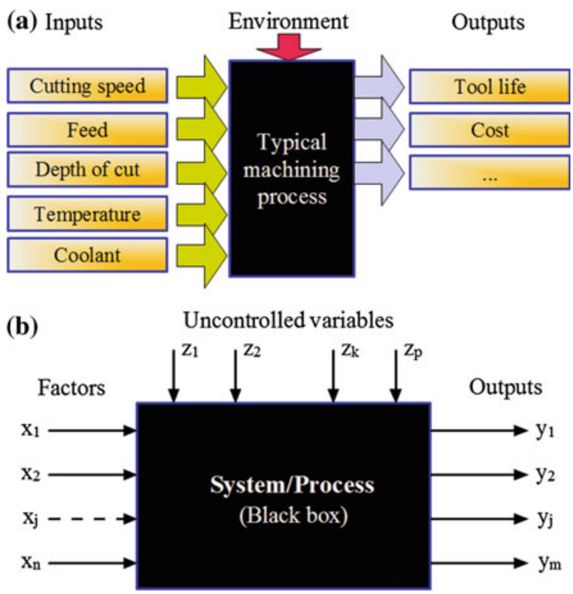

Fig. 1 Visualization of: **a** DOE intent, and **b** formal definition of DOE

output, and transfer (correlation) characteristics without any knowledge of its internal workings, that is, its implementation is "opaque" (black).

The first stage of DOE requires the formulation of clear objective(s) of the study. The statistical model selection in DOE requires the quantitative formulation of the objective(s). Such an objective is called the response, which is the result of the process under study or its output as presented in Fig. 1. The process under study may be characterized by several important output parameters but only one of them should be selected as the response.

The response must satisfy certain requirements. First, the response should be the effective output in terms of reaching the final aim of the study. Second, the response should be easily measurable, preferably quantitatively. Third, the response should be a single-valued function of the chosen parameters.

The proper selection of KPIVs cannot be overstated. In DOE, it is necessary to take all the essential factors into consideration. Unconsidered factors change arbitrarily, and thus increase the error of the tests. Even when a factor does not change arbitrarily but is fixed at a certain level, a false idea about the optimum can be obtained because there is no guarantee that the fixed level is the optimum one.

The factors can be quantitative or qualitative but both should be controllable. Practically, it means that the chosen level of any factor can be set up and maintained during the tests with certain accuracy. The factors selected should affect the response directly and should not be a function of other factors. For example, the cutting temperature cannot be selected as a factor because it is not a controllable parameter. Rather, it depends on other process parameters as the cutting speed, feed, depth of cut, etc.

The factor combinations should be compatible, i.e., all the required combinations of the factors should be physically realizable on the setup used in the study. For example, if a combination of cutting speed and feed results in drill breakage, then this combination cannot be included in the test. Often, chatter occurs at high cutting regimes that limits the combinations of the regime parameters.

3 Factor Interactions

The famous UCLA Coach John Wooden used to say: "A player who makes a team great is much more valuable than a great player." When the Brazilian soccer team led by Pele, reportedly the best player in the soccer history, came to the 1966 World Cup final in England, almost no one had a doubt that this competition was only a formality for this team. Brazil assembled a "dream team" from the ranks of the top FIFA superstars. The expectation was that this high-powered assembly of top talent would walk all over their competition. However, Brazil lost in the group matches to Hungary and then to Portugal. Brazil returned home early, without getting past the first stage of the cup. For the disorganization and for the bad results, this is considered the worst performance of Brazil in a World Cup.

How could this have happened, particular the devastating lost to Hungary? Clearly the individual Brazilian players were superior to their Hungarian counterparts. But the Hungarian squad had trained together and was used to playing by the slightly different rules of World Cup soccer. By contrast, the Brazilian team was assembled shortly before the games and had not practiced very much. They had not "jelled" as a team. Similarly, some of the parameters of metal cutting regime or tool that one may be testing may be superstars individually, i.e., the tool cutting edge angle and cutting feed. But one should be looking for the combination of variables that performs best when presented together.

What is a variable interaction? Simply put, it is when the setting for one variable in your test positively or negatively influences the setting of another variable. If they have no effect on each other, they are said to be independent. In a positive interaction, two (or more) variables create a synergistic effect (yielding results that are greater than the sum of the parts). In a negative interaction, two (or more) variables undercut each other and cancel out some of the individual effects.

In metal cutting, we want to know interactions. We want to use factors interaction to achieve the maximum effect, i.e., to optimize the process using the selected criteria (criteria) of optimization. We want to detect any parts of the tool geometry that are working at cross-purposes and undercutting the performance of the machining regime. Our goal should be to find the best-performing group of machining process elements.

Some DOEs (such as A-B split testing and many forms of fractional factorial parametric testing widely used in metal cutting testing [2, 8]) assume that there are absolutely no interactions among process variables and that these variables are completely independent of each other.

In the author's opinion, this is an absurd assumption in metal cutting testing. Very strong interaction effects (often involving more than two variables) definitely exist although admitted rarely. This should not be a surprise to anyone, because the optimization of metal cutting is intentionally trying to create a combination of the process parameters that are greater than the sum of their parts. In doing such an optimization, one should be looking for synergies among all of machining elements and trying to eliminate combinations of variable values that undermine the desired outcome.

Although one may be able to get some positive results by ignoring interactions, he or she will not get the best results. So where can you look for interactions? In general, there is no way to guarantee that any subset of your testing elements does not interact. However, you should consider elements that are in physical proximity, or that are otherwise confounded with each other. For example, if a new tool material prone to chipping (e.g., CVD diamond) is used in face milling, extremely sharp cutting edges should be honed to prevent their chipping due to shock loading. To balance the negative effect of the edge radius, r_{ed}, the uncut chip thickness (h_D) should be increased to keep the ratio $h_D/r_{\text{ed}} > 6$ to maximize the tool life [4]. In turn, the uncut chip thickness depends on the cutting feed per tooth, f_z and tool cutting edge angle, κ_r. As follows, a strong correlation of three parameters of face milling, namely the edge radius, r_{ed}, the cutting feed per tooth, f_z, and tool cutting edge angle, κ_r are clearly established.

Therefore, possible variable interactions should not be ignored in metal cutting tests because interactions exist and can be very strong.

4 Examples of Variable Interaction in Metal Cutting Testing

The common statistical representation of the results of metal cutting test for tool life and the cutting force are

$$T = C_T v_c^{x_T} f^{y_T} a_p^{z_T} \tag{1}$$

$$F = C_F v_c^{x_T} f^{y_F} a_p^{z_F}, \tag{2}$$

where C_T, C_F, x_T, y_T, z_T, x_F, y_F, z_F are initially assumed as constants.

Therefore, even the initial structure of these statistical equations assumes that there are no interactions among the included factors. One can argues, however, that the possible interactions as hidden (distributed) in the corresponding constants and the intervals of factors variation are chosen to avoid, or at least, to reduce the possible interactions. In the author's opinion, these arguments are not valid because if the former is the case then the experimentalist deliberately decreases the range of factors variation, and thus significantly reduces the value of the obtained results.

Moreover, as the factors interactions are not known before testing, there is no way to take some purposeful measures to reduce these integrating at the stage of test planning.

This section provides some examples of factors interaction detected in the proper-planned and statistically-evaluated metal cutting tests.

Example 1 This example relates to the experiments on longitudinal turning [9]. Test samples were carbon steel bars DIN Ck45 (steel ANSI 1045) 100 mm in diameter and 380 mm in length. The cutting tool included a holder DDJNL 3225P15 with coated inserts DNMG 150608-PM4025. The tool geometry was with rake angle 17°, clearance angle 5°, tool cutting edge angle 93°, and nose radius 0.8 mm. The experiments were carried out using the rotatable central composite design with five levels (coded by: −1.6817; −1; 0; +1 and +1.6817) of three cutting parameters (Table 1). The cutting force was chosen to be the response.

The required number of experimental points is $N = 2^3 + 6 + 6 = 20$ [2]. There are eight factorial experiments (3 factors on two levels, 23) with added 6 star points and center point (average level) repeated 6 times reduce test error. The test result is represented as

$$F_c = 187.937 - 1970.77f + 10.418a_p + 1598fa_p + 40.6765f^2 + 40.953a_p^2 \quad (3)$$

As can be seen, the factors affecting F_c, are cutting feed f, depth of cut a_p, square of feed f^2 (which can be conditionally termed as ‘self-interaction’), square of depth of cut, a_p^2 (self-interaction), and the interaction of feed and depth of cut $f \cdot a_p$. Therefore, two main and three interaction effects were revealed.

Example 2 As discussed by Astakhov [4], the cutting temperature θ_{ct}, understood as the mean integral temperature at the tool–chip and tool–workpiece interfaces as measured by a tool-work thermocouple, is the most important parameter to correlate the tribological conditions at the discussed interfaces with tool wear. Moreover, for a given combination of the tool and work materials, there is the cutting temperature, referred to as the optimal cutting temperature, at which the combination of minimum tool wear rate, minimum stabilized cutting force, and highest quality of the machined surface is achieved. This temperature is invariant to the way it has been achieved (whether the workpiece was cooled, pre-heated, etc.).

Table 1 Physical and coded values of factors in Test 1

Factors/levels	Lowest	Low	Center	High	Highest
Coding	−1.6817	−1	0	+1	+1.6817
Cutting speed (m/min) $X_1 = v_c$	266	300	350	400	434
Cutting feed (mm/rev) $X_2 = f$	0.23	0.30	0.40	0.50	0.57
Depth of cut (mm) $X_3 = a_p$	1.0	1.5	2.25	3.0	3.5

The objective of the considered test is to establish the correlation of this temperature with parameters of the cutting system. The following correlation equation was used:

$$\theta_{\rm ct} = C_\theta v_c^{x_\theta} f^{y_\theta} a_p^{z_\theta}, \tag{4}$$

where C_θ is constant which depends on the properties of the work material, x_θ, y_θ, and z_θ are powers to be determined in DOE.

The longitudinal turning tests were carried out. Test samples were carbon steel bars made of steel ANSI 1020 of 50 mm in diameter and 260 mm in length. The cutting tool was made of T15 high speed steel. The tool geometry was: rake angle 10°, clearance angle 10°, tool cutting edge angle 45°, and nose radius 0.2 mm. The experiments were carried out using the rotatable central composite design with five levels (coded by: −1.6817; −1; 0; +1 and +1.6817) of three cutting parameters (Table 2). The cutting temperature measured in millimeters of millivolt meter tape was chosen to be the response.

Analogous to the previous example, the required number of experimental points is $N = 2^3 + 6 + 6 = 20$. There are eight factorial experiments (3 factors on two levels, 23) with added 6 star points and center point (average level) repeated 6 times reduce test error. The result DOE is represented as

$$\theta_{\rm ct} = 26.8 v_c^{(1.58-0.34\ln v_c)} f^{(0.46-023\ln f)} a_p^{(0.44-0.15\ln a_p)} \tag{5}$$

As follows from Eq. (5), the powers x_θ, y_θ and z_θ are not constants as routinely assumed in metal cutting experimental studies; rather, a complicated self-interaction of each factor is revealed by DOE.

Example 3 The third example is the use of DOE in the experimental study of the influence of three parameters: cutting speed, v_c, feed f, and the cutting fluid flow rate Q on the roughness $\Delta_{\rm sf}$ and roundness Δ_R of the machined hole in gundrilling [4]. A 2^3 DOE, complete block is used.

The test conditions were as follows:

– Work material: hot rolled medium carbon steel AISI 1040 was used. The test bars, after being cut to length (40 mm diameter, 700 mm length), were normalized to a hardness of HB 200.

Table 2 Physical and coded values of factors in Test 2

Factors/levels	Lowest	Low	Center	High	Highest
Coding	−1.6817	−1	0	+1	+1.6817
Cutting speed (m/s) $X_1 = v_c$	0.072	0.115	0.229	0.454	0.725
Cutting feed (mm/rev) $X_2 = f$	0.082	0.110	0.170	0.260	0.463
Depth of cut (mm) $X_3 = a_p$	0.25	0.36	0.61	1.04	1.49

Table 3 The levels of factors and their intervals of variation

Level of factors	Code	v_c (m/min)	f (mm/rev)	Q (l/min)
Basic	0	100	0.07	60
Interval of variation	Δx_i	15	0.02	20
Upper	+1	115	0.09	80
Lower	−1	85	0.05	40

– Cutting tool: gundrills of 12.1 mm diameter were used. The material of their tips was carbide M 30. The parameters of drill geometry were as discussed in [4].

The levels of the factors and intervals of factor variations are shown in Table 3. At each point of the design matrix, the tests were replicated thrice. The sequence of the tests was arranged using a generator of random numbers.

The results DOE are represented as

Surface roughness

$$\Delta_{\mathrm{sf}} = 2.7446 - 0.0198v - 33.9583f + 0.2833vf \tag{6}$$

Roundness of drilled holes

$$\begin{aligned}\Delta R(\mu\mathrm{m}) = {} & -20.044 + 0.238v + 396f + 0.462Q \\ & - 3.960vf - 0.005vQ - 6.600fQ + 0.066vfQ\end{aligned} \tag{7}$$

Equation (6) reveals that for the selected upper and lower limits of the factors, the surface roughness in gundrilling depends not only on the cutting speed and feed singly, but also on their interaction as follows from Eq. (7). Therefore, although the cutting speed, feed, and cutting fluid flow rate have significant influence of roundness, they cannot be judged individually due to their strong interactions.

Reading these examples, one can wonder what seems to be a problem with factors' interactions? The factors and their interaction are accounted for in the models obtained by full factorial DOEs in the discussed examples. The problem, however, is in the number of factors included in these DOE. For example, in Example 3, the surface roughness and roundness in gundrilling strongly depend not only on the considered factors but also on the many parameters of the gundrill geometry (e.g., the point angles of the outer and inner cutting edges, backtaper, margin width [10]) not included as factors in the test.

Full factorial DOE allows accurate estimation of all factors involved and their interactions. However, the cost and time need for such a test increase with the number of factors considered. Normally, any manufacturing test includes a great number of independent variables. In the testing of drills, for example, there are a number of tool geometry variables (the number of cutting edges, rake angles, flank angles, cutting edge angles, inclination angles, side cutting edge back taper angle, etc.) and design variables (web diameter, cutting fluid holes shape, their cross-sectional area and location, profile angle of the chip removal flute, length of the cutting tip, the shank length and diameter, etc.) that affect drill performance.

Table 4 shows the number of runs needed for the full factorial DOE where the two levels of factors variation are considered (as that used in Example 3). If one runs more accurate DOE discussed in Examples 1 and 2, i.e., using the rotatable central composite design with five levels, the number of tests will be even greater.

The time and cost constraints on such DOE are definitely important in the DOE planning stage. In metal cutting tests, however, other constraints should be considered as they can be decisive. The major problem is with a great number of cutting tools and a significant amount of the work material needed to carry out the tests. It is difficult to keep the parameters of these tools and properties of the work material within limits needed to obtain statistically reliable results when the test program is too large. If the variations of these parameters and properties are also included in DOE, a virtually infinite number of runs would be needed to complete the study.

One may further argue that omitting the influence of the interactions is the price to pay to keep DOE within the reasonable number of runs, and thus within reasonable cost. To understand what can be missed in the test results, let us consider a comparison of the result of tool life DOEs.

The first test is a 2^3 DOE, complete block with the test conditions as in Example 3. The following result, presented as a correlation equation was achieved:

$$T = \frac{e^{9.55} d_w^{0.19}}{v_c^{1.37} f^{0.14}} \tag{8}$$

The second test used to carry out the tool life test in gundrilling is the Group Method of Data Handling (hereafter, GMDH) [11]. This kind of DOE is more complicated than the above-discussed DOEs, but it has a number of advantages in terms of the number of variables included in the test and objectivity of statistical evaluation of the test results [12].

Eleven input factors were selected for the test. They are: x_1 is the approach angle of the outer cutting edge (φ_1); x_2 is the approach angle of the inner cutting edge (φ_2); x_3 is the normal clearance angle of the outer cutting edge (α_1); x_4 is the normal clearance angle of the outer cutting edge (α_2); x_5 is the location distance of the outer cutting edge (c_1); x_6 is the location distance of the outer cutting edge (c_2); x_7 is the location distance of the drill point with respect to the drill axis (m_d); x_8 is the location distance of the two parts of the tool rake face with respect to drill axis (m_k); x_9 is the clearance angle of the auxiliary flank surface (α_3); x_{10} is the cutting speed (v_c); x_{11} is the cutting feed (f). The design matrix is shown in Table 5.

The following result was obtained with GDMT DOE

Table 4 Two-level designs: minimum number of runs as a function of number of factors for full factorial DOE

Number of factors	Number of runs	Number of runs	Number of repetitions	
			3	5
1	2	2	6	10
2	$4 = 2^2$	$4 = 2^2$	12	20
3	$8 = 2^3$	$8 = 2^3$	24	40
4	$16 = 2^4$	$16 = 2^4$	48	80
5	$32 = 2^5$	$32 = 2^5$	96	160
6	$64 = 2^6$	$64 = 2^6$	192	320
7	$128 = 2^7$	$128 = 2^7$	384	640
8	$256 = 2^8$	$256 = 2^8$	768	1280
9	$512 = 2^9$	$512 = 2^9$	1536	2560
10	$1024 = 2^{10}$	$1024 = 2^{10}$	3072	5120

Table 5 The levels of factors and their intervals of variation used in GMDH DOE

Levels	x_1 (°)	x_2 (°)	x_3 (°)	x_4 (°)	x_5 (mm)	x_6 (mm)	x_7 (mm)	x_8 (mm)	x_9 (°)	x_{10} (mm)	x_{11} (mm)
+2	34	24	20	16	1.50	1.50	16.0	17.5	20	53.8	0.21
+1	30	22	17	14	0.75	0.75	14.0	11.5	15	49.4	0.17
0	25	18	14	12	0.00	0.00	11.0	8.75	10	34.6	0.15
−1	22	15	11	10	−0.75	−0.75	8.75	6.0	5	24.6	0.13
−2	18	12	8	8	−1.50	1.50	6.0	3.5	0	19.8	0.11

$$T = 6.7020 - 0.6518\frac{\alpha_1}{\varphi_2 c_2} - 0.0354\frac{\alpha_2 \ln c_2}{m_d} - 0.0005\frac{\varphi_1^2}{c_2} + 0.0168\frac{\ln c_2}{\varphi_1} - 2.8350\frac{vf}{\varphi_1} - 0.5743\frac{c_2 m_d}{\ln \alpha_1 \varphi_1} \quad (9)$$

The statistical model of tool life (Eq. 9) indicates that tool life in gundrilling is a complex function of not only design and process variables but also of their interactions. The inclusion of these interactions in the model brings a new level of understanding about their influence on tool life. For example, it is known that the approach angle of the outer cutting edge (φ_1) is considered as the most important parameter of the tool geometry in gundrilling because it has controlling influence on tool life and on other important output parameters [13]. Traditionally, this angle along with approach angle of the inner cutting edge (φ_2) is selected depending on the properties of the work material. Although the contradictive influence of these angles has been observed in practice, none of the studies reveals their correlations with the cutting regime as suggested by Eq. (9). Moreover, three- and four-factor interaction terms are found to be significant.

5 Need for a Screening Test

It is discussed above that although a full factorial DOE and GMDH methods allow accurate estimation of all factors involved and their interactions, the cost and time needed for such tests increase with the number of factors considered. Therefore, the pre-process stage in a full factoring DOE is considered to be of high importance in metal cutting studies [4] because pre-process decisions to be made at this stage are crucial to the test, whereas they are not nearly formalized. Among them, the proper selection of KPIVs is probably the most important.

The major problem in pre-process decision is the selection of KPIVs justifying two important rules:

1. The number of factors should be kept to a possible minimum defined by adequate time, resources, or budget to carry out the study. This is an obvious rule.
2. The second rule pointed out explicitly in the classical paper by Box and Hunter [14] includes the assumptions that the observations are uncorrelated and have equal variance. Note that this is next to impossible to verify the latter in practice.

Often, pre-process decisions rely on experience, available information, and expert opinions and thus they are highly subjective. Even a small inaccuracy in the preprocess decisions may affect the output results dramatically. Therefore, the pre-process stage of full factorial DOE should be more formalized.

As discussed above, any machining test includes a great number of independent variables. However, when many factors are used in DOE, the experiment becomes expensive and time-consuming. Therefore, there is always a dilemma. On one hand, it is desirable to take into consideration only a limited number of KPIVs carefully selected by the experts. On the other hand, even if one essential factor is missed, the final statistical model may not be adequate to the process under study.

Unfortunately, there is no simple and feasible way to justify the decisions made at the pre-process stage about the number of KPIVs prior to the tests. If a mistake is made at this stage, it may show up only at the final stage of DOE when the corresponding statistical criteria are examined. Obviously, it is too late then to correct the test results by adding the missed factor or interaction. However, being of great importance, this problem is not the principal one.

The principal 'silent' problem of all experimental studies in metal cutting including DOE studies is that the results obtained in the test are valid only for the set of conditions used in the test, i.e., DOE studies in metal cutting are not efficient (see Sect. 1). To explain this statement consider the common statistical representation of the results of metal cutting test for tool life and for the cutting force defined by Eqs. (1) and (2). The use of these equations in a simple DOE-assisted turning test implies that the cutting force and tool life depend only on the parameters of the machining regime under the fixed work material (kind, grade, and mechanical properties), dimensions of the workpiece (diameter and length) [15], tool geometry parameters (the tool cutting edge angles of the major and minor cutting edges, the rake, clearance, and inclination angles, nose and cutting edge radii, etc.), tool material, particularities of the tool holder, system runout, test setup rigidity, etc. If one or more of the listed parameters is changed, the test results may not be valid for the new machining conditions.

Another dimension of the discussed problem is that a certain (out of the listed) parameter can be not important for one test condition, while for others it is of chief importance. For example, the radius of the cutting edge does not have a significant influence on rough turning of carbon steel where great cutting feeds are used. It, however, becomes important in finish turning with shallow feeds. Moreover, it is of crucial importance in turning of titanium alloys. Another example is backtaper. Backtaper applied to a drill might not be a significant factor in drilling soft materials or cast irons, but it is highly significant in machining titanium and aluminum alloys having low elasticity modulus [10].

The theory of DOE offers a few ways to deal with such a problem [2]. The first relies on the collective experience of the experimentalist(s) and the research team in the determination of KPIVs. However, the more experience such a team has, the more factors they recommend to include in DOE.

A second way is to use screening DOE [16]. This method appears to be more promising in terms of its objectivity. Various screening DOEs are used when a great number of factors are to be investigated using a relatively small number of tests [17]. This kind of test is conducted to identify the significant factors and factors' interactions for further analysis. In other words, the whole project is divided into two stages. In the first stage, the important factors and interaction are determined. These are used in a full factorial DOE in the second stage. In the author's opinion, this is the only feasible way to deal with the above-discussed principal problem.

For the further discussion on the selection of the adequate screening DOE, the notion of the DOE resolution should be briefly introduced.

6 Resolution Level

In general, a particular kind of the statistical model which correlates the factors and factor interactions with the chosen output is initially unknown due to insufficient knowledge of the considered phenomenon. Thus, a certain approximation for this model is needed. Experience shows [18] that a power series or polynomial (Taylor series approximations to the unknown true functional form of the response variable) can be selected as an approximation

Table 6 Resolution levels and their meaning

Resolution level	Meaning
II	Main effects are confounded with others. In other words, main effects are linearly combined with each other ($\beta_i + \beta_j$)
III	Can estimate main effects, but they may be confounded by two variable interactions. In other words, main effects are linearly combined with two-way interactions ($\beta_i + \beta_{jk}$)
IV	Can estimate main effects unconfounded by two variable interactions. Can estimate two variable interactions, but they may be confounded by other two variable interactions. It means that main effects are linearly combined with three-way interactions ($\beta_i + \beta_{jkl}$) and two-way interactions with each other ($\beta_{ij} + \beta_{kl}$)
V	Can estimate main effects unconfounded by three (or lower) variable interactions. Main effects and two-way interactions are not linearly combined except with higher-order interactions ($\beta_i + \beta_{jklm}$) and ($\beta_{ij} + \beta_{klm}$)

$$y = \beta_0 + \sum_{i=1}^{p} \sum_{\substack{j=1 \\ i \neq j}}^{p} \beta_{ij} x_i x_j + \sum_{i=1}^{p} \sum_{\substack{j=1 \\ i \neq j \neq k}}^{p} \sum_{k=1}^{p} \beta_{ijk}\, x_i x_j x_k + \cdots, \tag{10}$$

where β_0 is the overall mean response, β_i is the main effect of the factor (i = 1,2, …, p), β_{ij} is the two-way interaction effect between the ith and jth factors, and β_{ijk} is the three-way interaction effect between the ith, jth, and kth factors.

Experimental designs can be categorized by their resolution level. A design with a higher resolution level can fit higher-order terms in Eq. (10) than a design with a lower resolution level. If a high enough resolution level design is not used, only the linear combination of several terms can be estimated, not the terms separately. The word "resolution" was borrowed from the term used in optics. Resolution levels are usually denoted by Roman numerals, with III, IV, and V being the most commonly used. To resolve all of the two-way interactions, the resolution level must be at least V [19]. Four resolution levels and their meanings are given in Table 6.

7 Using Fractional Factorial DOEs for Factors Screening

A type of orthogonal array design which allows experimenters to study the main effects and some desired interaction effects in a minimum number of trials or experimental runs is called a fractional factorial design [2]. These fractional factorial designs are the most widely and commonly used types of design in industry.

In the introduction of this type of DOE, the following rationale is provided. In theory, it is possible that every variable that is tested has interactions with every specific value of every other variable. In practice, this is usually not the case.

During tests, one may discover that many or even most of the elements that have been decided to include do not impact performance at all. They simply do not affect the output variable. It is also common that strong interactions between two variables exist but that higher-order interactions (among three or more variables) are insignificant. In such cases, the behavior of the output variable can be described by looking at the main effects and a few low-order interactions (involving two variables). Unfortunately, not much attention is paid to the described limitation. In other words, no effort is made to verify that these limitations are applicable in a particular test.

The mentioned basic idea of the fractional factorial design arises as a consequence of three empirical principles commonly accepted in the testing community:

1. *Hierarchical Ordering Principle*. Lower-order effects are more likely to be important than higher-order effects. Effects of the same order are equally likely to be important. This principle suggests that when resources are scarce (i.e., the data collection rate low), priority should be given to estimating main effects and lower-order interactions.
2. *Effect Sparsity Principle*. The numbers of relatively important effects in a factorial experiment are small. This is another formulation of the 80/20 rule. Only a few variables combine to produce the biggest effects, and all of the rest will not matter nearly as much.
3. *Effect Heredity Principle*. In order for an interaction to be significant, at least one of its parent factors should be significant. This is another application of common sense. If a variable does not produce any big effects of its own (i.e., it is benign or negligible), it is unlikely to do so when combined with something else. It may be that a big interaction effect is produced by variables that do not show the largest main effects, but at least one of the variables involved in an interaction will usually show some main effect.

The underlying rationale behind fractional factorial design is that one can collect data on a fraction of the recipes needed for an equivalent full factorial design and still maximize the model's predictive value.

Fractional factorial designs are expressed using the notation l^{k-p} (l is the common branching factor for all variables (the number of levels of factors) in the test; k is the number of variables (factors) investigated, and p describes the size of the fraction of the full factorial search space used. $1/2^p$ represents the fraction of the full factorial 2 k [20]. For example, $2^{(5-2)}$ is a 1/4th fraction of a 2^5 full factorial DOE. This means that one may be able to study 5 factors at 2 levels in just 8 experimental trials instead of 32 trials. In mathematical terms, p is the number of generators (elements in your model that are confounded and cannot be estimated independently of each other). In other words, when p is increased, some of the input variables are not independent and can be explained by some combination of the other input variables or their interactions.

Creating a proper fractional factorial design is beyond the scope of this chapter. The basic steps are as follows:

- Based on the generators (see above) of the chosen design, one can determine the defining relation.
- The defining relation specifies the alias structure.
- A fractional factorial experiment is created from a full factorial experiment by using the chosen alias structure.

One common constraint on fractional factorial tests is that the branching factor is two for all variables (i.e. $l = 2$). The methods for creating custom test designs outside of this constraint are complex. Many testers simply copy "standard" designs from statistical texts or use standard DOE software packages, and thus restrict themselves to a choice of variables and branching factors that fit the model.

7.1 Short Overview of Common Fractional Factorial Methods

Although there is some difference in common fractional factorial methods, their basic predictive power, required data sample size, and underlying assumptions are pretty similar. The main difference lies in the shape of the search spaces that each can be used for. So if one is going to use any of the methods below, the final decision should be based on one's familiarity with each and the number and branching factor of the variables included in the test.

In the following sections, the sparsest fractional factorial approaches are described in detail:

- Plackett–Burman,
- Latin squares,
- Taguchi method.

There is no reason to prefer the Taguchi method over Plackett–Burman or Latin squares. All three fractional factorial methods suffer from the same fundamental issues. These problems are a direct consequence of their origins in manufacturing. Let us take a look at some of the characteristics of this original environment:

1. *Expensive prototypes*. The underlying assumption is that creating alternative recipes is difficult, time-consuming, or expensive. When applications involve physical processes or manufacturing technology, this is indeed the case. So the goal is to minimize the required number of recipes (also called "prototypes" or "experimental treatments") in the experiment.
2. *Small test sizes*. A direct consequence of the expensive prototypes is that one needs to keep the number of elements that he or she tests to an absolute minimum, and focus only on the most critical variables.
3. *No interactions*. As another consequence of the expensive prototypes, one can only measure the main effects created by the included variables. The small test

size and expensive data collection force one to assume very sparse fractional factorial models that cannot accurately estimate even two variable interactions.
4. *High yields*. In most cases, the process or outcome that one is measuring had a high probability of success.
5. *Continuous variables*. Many of the input variables involved in the tests were continuous (e.g., temperature, cutting force). Although one had to pick specific levels of the variable for the test, he or she could often interpolate between them to estimate what would happen at non-sampled settings of the variable.

These approaches were transplanted to the manufacturing tests (and machining tests in particular) because of their relative simplicity and familiarity. Unfortunately, the assumptions that accompanied them came along for the ride, even though they were not applicable to the new environment where factors' interactions may play a significant role.

All three listed fractional factorial methods are resolution III designs. It implies that they can only estimate the main effects in the model. In other words, they cannot capture all possible two-variable interactions (or any higher-order interactions). Some of them explicitly assume that there are no interactions. They use this radical assumption to dramatically lower the number of sampled recipes and the amount of data required to estimate the main effects. An important additional requirement for all of these approaches is that the data collection is balanced across all possible values of a variable.

Let us assume that one wants to collect data for each of the variable main effects in the examples that follow. He or she can construct a series of increasingly larger tests and see how to achieve the desired results with only a few recipes. The simplest case is an A-B split test containing two recipes, *a* and *b* [2, 8]. He or she needs to split the traffic 50/50 between *a* and *b*. Therefore, two recipes are needed to measure the two values of variable *V*1. These two recipes represent the entire search space.

Now imagine that one has two variables, each with a branching factor of two. This results in four possible recipes: *aa*, *ab*, *ba*, and *bb*. Assume further that one chooses to sample only from recipes *aa*, and *bb* (still only two recipes as in the previous example). Note that half the data collected involves *V*1*a* (from recipe *aa*), while the other half involves *V*1*b* (from recipe *bb*). Similarly, half the data covers *V*2*a* (from recipe *aa*), while the other half involves *V*2*b* (from recipe *bb*). As you can see, equal amounts of data on each main effect are collected, which was done by sampling only half of the total search space (two out of four recipes).

Let us extend the considered example to three variables, each with a branching factor of two. This results in eight possible recipes: *aaa*, *aab*, *aba*, *abb*, *baa*, *bab*, *bba*, and *bbb*. Assume that one chooses to sample only from recipes *aaa* and *bbb* (still only two recipes). Note that half the data that one collects involves *V*1*a* (from recipe *aaa*), while the other half involves *V*1*b* (from recipe *bbb*). Similarly, half the data collected covers *V*2*a* (from recipe *aaa*), while the other half involves *V*2*b* (from recipe *bbb*). A half of the collected data will also cover *V*3*a* (from recipe *aaa*), while the other half will cover *V*3*b* (from recipe *bbb*). As can be seen, equal amounts of data have been collected again on each main effect, and have done it by sampling only a quarter of the total search space (two out of eight recipes).

Of course one cannot continue to sample just two recipes and still cover all main effects at larger test sizes. But by clever test construction, one can keep the number of unique recipes surprisingly small (especially when considered as a proportion of the total search space).

Underlying the use of fractional factorial methods is the assumption that creating a test run is difficult or time-consuming. Therefore, one needs to keep the number of recipes that he or she samples as low as possible. This may have been true in the manufacturing setting. For practical data collection purposes, it does not matter how many unique recipes one has in the test. When recipe construction is expensive and time-consuming, a heavy price is paid during data gathering. By sampling very limited recipes, one can significantly reduce the cost of testing. In doing so, however, he or she destroys the ability to do a comprehensive analysis and find variable interactions later.

7.1.1 Plackett–Burman DOE

The idea and principles of the DOE was published by R.L. Plackett and J. P. Burman in their paper "The Design of Optimal Multifactorial Experiments" in 1946 [21]. In it, they describe a very efficient and economical method for constructing test designs. The requirements for a Plackett–Burman (PB) design are a branching factor of two on all variables, and the number of recipes sampled must be a multiple of four. PB designs exist for 12, 20, 24, 28, and larger sizes. Each PB design can estimate the main effects of one fewer variable than the size of the design (e.g., the PB design with 24 recipes may be used for an experiment containing up to 23 two-value variables).

PB designs are all resolution III and are known as saturated main effect designs because all degrees of freedom in the model are used to estimate the main effects. PB designs are also known as nongeometric designs. Because of their construction, they do not have a defining relationship (since interactions are not identically equal to main effects). They are efficient at detecting large main effects (assuming that all interactions are relatively small). It was discovered in the 1990s that PB designs have an additional interesting property of being "3-projectible." This means that one can find important interactions involving any subset of three variables in the design. The use of this remarkable property will be discussed further.

7.1.2 Latin Squares

Latin squares were first described by Euler in 1782 [22]. They are used for a number of applications (including the popular Sudoku puzzles) and have extensive mathematical literature describing them.

Latin squares are square arrangements of numbers and can be of different sizes (2×2, 3×3, etc.). Each position in the Latin square contains one of the numbers (from 1 to n) arranged in such a way that no orthogonal (row or column) contains the same number twice.

The two possible Latin squares of size 2×2 shown here are

$$\begin{bmatrix} 1 & 2 \\ 2 & 1 \end{bmatrix} \begin{bmatrix} 2 & 1 \\ 1 & 2 \end{bmatrix} \tag{11}$$

The 12 possible Latin squares of size 3×3 shown here are

$$\begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 3 & 1 & 2 \\ 2 & 3 & 1 \end{bmatrix} \begin{bmatrix} 1 & 3 & 2 \\ 3 & 1 & 2 \\ 2 & 3 & 1 \end{bmatrix} \begin{bmatrix} 1 & 3 & 3 \\ 2 & 1 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix}$$
$$\begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \begin{bmatrix} 1 & 2 & 3 \\ 2 & 3 & 1 \\ 3 & 1 & 2 \end{bmatrix} \tag{12}$$

The number of possible Latin squares grows very quickly with the size (576 at size 4×4, 161,280 at size 5×5, etc.)

Latin squares are used in experimental designs when input variables of interest have a branching factor of greater than two, and there are assumed to be no interactions among the input variables. The combination of the row and columns labels with the cell contents in the Latin square defines a recipe in the experimental design. For example, let us assume that one wants to understand the effect of four different Co contents (%) in the carbide tool material on the radial accuracy (wear) of a single-point cutter. If he or she has four tool holders and four operators available, then a full factorial design (for a total of 64 recipes) can be employed.

However, if he or she is not really interested in which tool holder is better or which operator is more productive, nor any minor interaction effects between tool holders and operators and tool holders and machine tools, then other type of DOE can be used. In other words, the major concern is to estimate the main effects. As such, it is desirable to make sure that the main effects for tool holders and machine tools do not bias the obtained estimates for the cobalt content. Hence, one can randomize across all tool holders and operators by using the following 4×4 Latin square design, illustrated in Table 7, where each letter represents one of the cobalt contents being tested.

As can be seen, each operator will try cutting inserts of every cobalt content and each machine tool will be run using inserts of every cobalt content. The assumption that all variables are independent allows one to complete the study by sampling only 16 recipes (instead of the full 64).

Table 7 Design matrix

	Machine tool				
Operator		1	2	3	4
	1	A	B	C	D
	2	B	A	D	C
	3	C	D	A	B
	4	D	C	B	A

7.1.3 Taguchi Method

Genichi Taguchi was a Japanese mathematician and a proponent of manufacturing quality engineering. He focused on methods to improve the quality of manufactured goods through both statistical process control and specific business management techniques. Taguchi developed many of his key concepts outside of the traditional Design of Experiments (DOE) framework and only learned of it later. His main focus was on robustness—how to develop a system that performed reliably even in the presence of significant noise or variation [23]. In traditional DOE, the goal is to model the best-performing recipe. In other words, the higher the value of the output variable (e.g., tool life), the better. So the goal is to find the highest mean. When taking repeated samples, any variation is considered a problem or a nuisance.

Taguchi had a different perspective. He felt that manufacturing quality should be measured by the amount of deviation from the desired value. In other words, he was concerned not only with the mean, but also with the amount of variation or "noise" produced by changing the input variables. Hence optimization from the Taguchi perspective means finding the best settings for the input variables, defined as the ones producing the highest signal-to-noise ratio (the highest mean with the least amount of variation). An important consideration is how to keep the noise in the output low even in the face of noisy inputs.

The numbers of variables (factors) and alternative values for each variable (levels) is arbitrary in metal cutting optimization tests. One can easily find additional variables to test, or come up with alternative values for each variable. Unfortunately, basic Taguchi arrays exist only for the following experimental designs: L4: Three two-level factors; L8: Seven two-level factors; L9: Four three-level factors; L12: Eleven two-level factors; L16: Fifteen two-level factors; L16b: Five four-level factors; L18: One two-level and seven three-level factors; L25: Six five-level factors; L27: Thirteen three-level factors; L32—Thirty-two two-level factors; L32b: One two-level factor and nine four-level factors; L36: Eleven two-level factors and twelve three-level factors; L36b: Three two-level and twelve three-level factors; L50: One two-level factor and eleven five-level factors; L54: One two-level factor and twenty-five three-level factors; L64: Twenty-one four-level factors; L81: Forty three-level factors.

Orthogonal arrays are particularly popular in applications Taguchi methods in technological experiments and manufacturing. An extensive discussion is given in [24–26]. For simplest experiments, the Taguchi experiments are the same as the fractional factorial experiments in the classical DOE. Even for common experiments used in the industry for problem solving and design improvements, the main attractions of the Taguchi approach are standardized methods for experiment designs and analyses of results. To use the Taguchi approach for modest experimental studies, one does not need to be an expert in statistical science. This allows working engineers on the design and production floor to confidently apply the technique.

While there is not much difference between different types of fractional factorial methods for simpler experiment designs, for mixed level factor designs and building robustness in products and processes, the Taguchi approach offers some revolutionary concepts that were not known even to the expert experimenters. These include standard *method for array modifications*, experiment designs to include *noise factors in the outer array*, *signal-to-noise ratios* for analysis of results, *loss function* to quantify design improvements in terms of dollars, treatment of systems with *dynamic characteristics*, etc. [23].

Although the Taguchi method was developed as a powerful statistical method for shop floor quality improvement, a way too many researchers have been using this method as a research and even optimization method in manufacturing, and thus in metal cutting studies (for example, [27–31]).

Unfortunately, it became popular to consider the use of only a fraction of the number of test combinations needed for a full factorial design. That interest spread because many practitioners do not take the time to find out the "price" paid when one uses fractional factorial DOEs including the Taguchi method: (1) Certain interaction effects lose their contrast so knowledge of their existence is gone; (2) Significant main effects and important interactions have aliases—other 'confounding' interaction names. Thus wrong answers can, and often do come from the time, money, and effort of the experiment.

Books on DOE written by "statistical" specialists add confusion to the matter claiming that interactions (three-factor or higher order) would be too difficult to explain, nor could they be important. The author wishes to remind to many statisticians that the ideal gas law (1834 by Emil Clapeyron), known from high-school physics as

$$PV = nRT \tag{13}$$

(where P is the pressure of the confined gas, V is the volume of the confined gas, n is the number of moles of gas, R is gas constant, T is the temperature) plots as a simple graph. It depicts a three-factor interaction affecting y (response) as pressure, or as volume. The authors of these statistical books/papers may have forgotten their course in high-school physics.

The problem is that the ability of the Taguchi method is greatly overstated by its promoters, who described Taguchi orthogonal tables as Japan's "secret super weapon," which is the real reason for developing an international reputation for quality. The major claim is that a large number of variables could now be handled with practical efficiency in a single DOE. As later details became available, many professionals realized that these arrays were fractional factorials, and that Taguchi went to greater extremes that other statisticians in the degree of fractionating. According to the Taguchi method, the design is often filled with as many single factors for which it has room. The design becomes “saturated” so no degrees of freedom are left for its proper statistical analysis. The growing interest in the Taguchi method in the research and optimization studies in manufacturing attests to the fact that manufacturing researches either are not aware of the above-mentioned “price” paid for apparent simplicity or know of no other way to handle more and more variables at one time.

7.2 Two-Stage DOE in Metal Cutting Tests

By now, readers have probably determined the author’s preference for full factorial over fractional factorial data collection in metal cutting studies. There is no efficiency disadvantage to full factorial designs during the data collection stage and significant advantages during the analysis stage. As discussed above, the major limitation is in the number of factors included as the cost, time, and test accuracy depend on this number. This problem can, and in the author’s opinion, should be resolved by using two-stage approach to testing. Ideally, the first, simple, and relatively inexpensive stage of DOEs in metal cutting should provide a help in determining the significant factors and interactions to be included in any kind of full factorial DOE to be used in the second stage of the study. Therefore, a closer look at various fractional factorial DOEs should be taken to find which one can justify the above-mentioned requirements.

Most Taguchi method test arrays are resolution III design, and thus can only estimate the main effects in the model. In other words, they cannot capture all possible two-variable interactions (or any higher-order interactions). Some of them explicitly assume that there are no interactions. They use this radical assumption to dramatically lower the number of sampled recipes and amount of data required to estimate the main effects. An important additional requirement for all of these approaches is that data collection is balanced across all possible values of a variable (i.e., you cannot use uneven data sampling, or it may complicate or throw off your use of standard data analysis).

8 The Use of Plackett and Burman DOE as a Sieve DOE in Metal Cutting

As discussed above, Plackett and Burman [21] developed a special class of fractional factorial experiments that includes interactions. When PB DOE is conducted properly using a completely randomized sequence, its distinctive feature is high resolution.

Despite a number of disadvantages (for example, mixed estimation of regression coefficients), this method utilizes high-contrast diagrams for the factors included in the test as well as for their interactions of any order. This advantage of PB DOE is useful in screening tests in metal cutting studies [32].

The method has its foundation in the Plackett–Burman design ideas, an over-saturated design matrix and the method of random balance. PB DOE allows the experimentalist to include as many factors (impute variables) as needed at the first phase of the experimental study and then to sieve out the nonessential factors and interactions by conducting a relatively small number of tests. It is understood that no statistical model can be produced in this stage. Instead, this method allows the experimentalist to determine the most essential factors and their interactions to be used at the second stage of DOE (full factorial or RSM DOE).

PB DOE includes the method of random balance. This method utilizes over-saturated design plans where the number of tests is fewer than the number of factors and thus has a negative number of degrees of freedom [33]. It is postulated that if the effects (factors and their interactions) taken into consideration are arranged as a decaying sequence (in the order of their impact on the variance of the response), this will approximate a ranged exponential-decay series. Using a limited number of tests, the experimentalist determines the coefficients of this series and then, using the regression analysis, estimates the significant effects and any of their interactions that have a high contrast in the noise field formed by the insignificant effects.

The initial linear mathematical model, which includes k number of factors (effects), has the following form:

$$y = a_0 + a_1x_1 + \cdots + a_kx_k + a_{12}x_1x_2 + \cdots + a_{k-1,k}x_{k-1}x_k + \delta, \tag{14}$$

where a_0 is the absolute term often called the main effect, a_i (i = 1, k) are the coefficients of linear terms, a_{ij} (i = 1, …, k − 1; j = i + 1, …, k, $i \neq j$) are the coefficients of interaction terms, and δ is the residual error of the model.

The complete model represented by Eq. (14) can be rearranged as a split of a linear form considering that some x_i designate the iterations terms as

$$\begin{aligned} y &= a_0 + a_1x_1 + \cdots + a_{k-l,k}x_{k-l} + b_1z_1 + b_2z_2 + \cdots b_lz_l + \delta \\ &= a_0 + a_1x_1 + \cdots + a_{k-l,k}x_{k-l} + \Delta \end{aligned}, \tag{15}$$

where

$$\Delta = b_1z_1 + b_2z_2 + \cdots b_lz_l + \delta \tag{16}$$

and

$$\sigma^2\{\Delta\} = b_1^2\sigma^2\{z_1\} + b_2^2\sigma^2\{z_2\} \cdots + b_l^2\sigma^2\{z_l\} + \sigma^2\{\delta\} \tag{17}$$

In the construction of the split model represented by Eq. (15), $(k - l)$ significant effects were distinguished and l effects were assigned to the noise field. Naturally, the residual variance $\sigma^2\{\Delta\}$ is greater than the tests variance $\sigma^2\{\delta\}$ so that the regression coefficients in Eq. (15) will be estimated with greater errors and, moreover, the estimates of the coefficients of this model are mixed. Therefore, the sensitivity of the random balance method is low so that the resultant model has a little significance, and thus should not be used as a valid statistical model. However, this method is characterized by the great contrast of essential effects, which could be distinguished easily on the noisy fields formed by other effects. The latter makes this method the simplest yet highly reliable screening method that can be used at the first stage of testing to distinguish the significant factors and interaction to be used in the full factoring DOE including RSM.

The step-by-step methodology of the discussed test was presented by the author earlier using a tool life test of the gundrill as an example [31]. The test was carried out using eight parameters of the gundrill geometry as the input variables. The test result shows two linear effects and one interaction having the strongest effects on tool life. As was expected (well known from the practice of gundrilling [10]), the strongest influence on tool life has the drill point offset m_d. The second strongest effect was found to be the approach angle of the outer cutting edge, φ_1. This result is also expected. What was not expected is a strong influence of the interaction term "shoulder dub-off location—the approach angle of the outer cutting edge." This distinguished interaction has never been considered before in any known studies of gundrilling. Using this factor and results of the full factorial DOE, a new pioneering geometry of gundrills has been developed (for example US Patent 7147411).

Another implementation of this DOE is considered in this chapter. It deals with sieve test to reveal significant factors of the tool geometry of 5 mm drill that affect tool life. The parameters chosen for the test and their levels are shown in Table 8. The construction of this table is based on the manufacturing practice and preliminary observations of the performance of this drill type in machining of a medium-carbon steel of HB 149 hardness.

The design matrix was constructed as follows. All the selected factors were separated into two groups. The first group contained factors x_1, x_2, x_3, x_4, form a half-replica 2^{4-1} with the defining relation $I = x_1x_2x_3x_4$. In this half-replica, the factors' effects and the effects of their interactions are not mixed. The second half-replica was constructed using the same criteria. A design matrix was constructed using the first half-replica of the complete matrix and adding to each row of this replica a randomly selected row from the second half-replica. Three more rows were added to this matrix to assure proper mixing and these rows were randomly selected from the first and second half-replicas. Table 9 shows the constructed design matrix.

As soon as the design matrix is completed, its suitability should be examined using two simple rules. First, a design matrix is suitable if it does not contain two identical columns having the same or alternate signs. Second, a design matrix should not contain columns whose scalar products with any other column result in a column of the same ("+" or "−") signs. The design matrix shown in Table 9 was found suitable as it meets the requirements set by these rules.

Table 8 The levels of the factors selected for the sieve DOE

Factors	Length of working part, w (mm)	Clearance angle, α_n (°)	Point angle, φ_p (°)	Web diameter, d_w (mm)	Length of the chisel edge (mm)	Margin width, b_m (mm)	Surface roughness the rake and flank faces (μm)	Hardness HRC
Code designation	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8
Upper level (+)	58.7	20	136	0.91	1.5	0.7	0.81	65.4
Lower level (−)	56.6	10	118	0.81	0.2	0.5	0.57	64.2

Table 9 Design matrix

Run	Factors								Average tool life (min)	Corrections	
	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	$\bar{y}$	$\bar{y}_{c1}$	$\bar{y}_{c2}$
1	+1	+1	−1	−1	+1	−1	+1	−1	10.80	16.80	13.60
2	+1	+1	+1	+1	−1	+1	+1	−1	17.20	10.08	6.83
3	−1	+1	−1	−1	+1	+1	−1	+1	9.09	9.09	2.64
4	−1	−1	+1	+1	+1	−1	−1	+1	42.00	28.26	21.81
5	+1	−1	−1	+1	−1	−1	+1	−1	16.91	9.17	9.17
6	+1	−1	+1	−1	+1	+1	−1	−1	19.46	25.46	19.01
7	−1	−1	−1	−1	−1	+1	−1	+1	10.21	10.21	10.21
8	−1	+1	−1	+1	−1	−1	+1	+1	27.31	13.57	13.57
9	+1	−1	−1	−1	−1	+1	−1	+1	4.54	10.54	10.54
10	−1	+1	+1	+1	−1	+1	−1	−1	36.00	22.26	19.01
11	+1	−1	+1	−1	−1	−1	+1	−1	12.20	18.20	14.95

The results of the first round of the tests are shown in Table 9 as the responses $\bar{y}_i$ i … 11. They are the average tool life calculated over three independent tests replicas (3 replicas were used) obtained under the indicated test conditions. Analysis of these results begins with the construction of a correlation (scatter) diagram shown in Fig. 2. Its structure is self-evident. Each factor is represented by a vertical bar having on its left side values (as dots) of the response obtained when this factor was positive (the upper value), while the values of the response corresponding to lower lever of the considered factor (i.e., when this factor is negative) are represented by dots on the right side of the bar. As such, the scale makes sense only along the vertical axis.

Each factor included in the experiment is estimated independently. The simplest way to do this is to calculate the distance between means on the left and right side of each bar. These distances are shown on the correlation diagram in Fig. 2. Another way is to take into account the number of points in the upper and lower part of the scatter diagram. For example, there are three dots for factor x_4 (Fig. 2) at the (+) level that have resonances greater than the greatest response on the (−) level. Similarly, there are four dots at the (−) level that have responses smaller than the smallest response at the (+) level. The total number of distinguishing points for factor x_4 is seven. In Fig. 2, a large group of such dots is signified by braces. The greater the number of distinguishing points, the stranger effect the corresponding factor has.

As seen in Fig. 2, factors x_1 and x_4 can be easily distinguished after the first sieve. Thus these two factors are selected for analysis. The effects of factors are calculated using special correlation tables. A correlation table (Table 10) was constructed to analyze the considered two factors. Using the correlation table, the effect of each selected factor can be estimated as

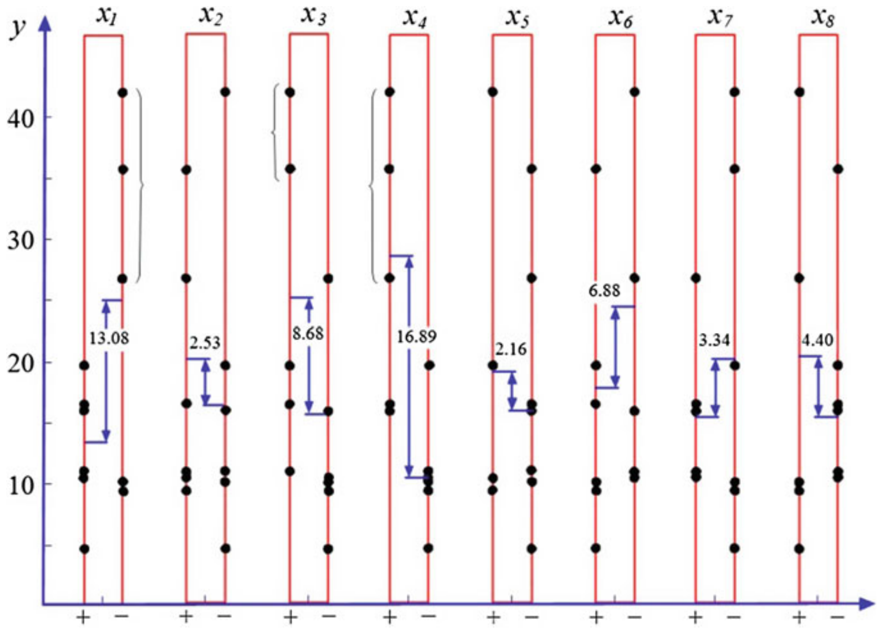

Fig. 2 Correlation diagram (original data)

$$X_i = \frac{\bar{y}_1 + \bar{y}_3 + \cdots + \bar{y}_n}{m} - \frac{\bar{y}_2 + \bar{y}_4 + \cdots + \bar{y}_{n-1}}{m}, \tag{18}$$

where m is the number of $\bar{y}$ in Table 10 for the considered factor assigned to the same sign ("+" or "−"). It follows from Table 10 that $m = 2$.

The effects of the selected factors were estimated using data in Table 10 and Eq. (18) as

$$X_1 = \frac{\bar{y}_{1-1} + \bar{y}_{1-3}}{2} - \frac{\bar{y}_{1-2} + \bar{y}_{1-4}}{2} = \frac{17.37 + 11.75}{2} - \frac{31.5 + 9.65}{2} = -6.00 \tag{19}$$

$$X_4 = \frac{\bar{y}_{1-1} + \bar{y}_{1-2}}{2} - \frac{\bar{y}_{1-3} + \bar{y}_{1-4}}{2} = \frac{17.37 + 31.50}{2} - \frac{11.75 + 9.65}{2} = 13.74 \tag{20}$$

The significance of the selected factors is examined using the Student's t-criterion, calculated as

$$t = \frac{\left(\bar{y}_{1-1} + \bar{y}_{1-3} + \cdots + \bar{y}_{1-n}\right) - \left(\bar{y}_{1-2} + \bar{y}_{1-4} + \cdots + \bar{y}_{1-(n-1)}\right)}{\sqrt{\sum_i \frac{s_i^2}{n_i}}}, \tag{21}$$

where s_i is the standard deviation of ith cell of the correlation table defined as

Table 10 Correlation table (original data)

Estimated factor	$+x_1$	$-x_1$	Estimated factor	$+x_1$	$-x_1$
$+x_4$	17.82 16.91 $\sum y_{1-1} = 34.73$ $\bar{y}_{1-1} = 17.37$	36.00 42.00 27.31 $\sum y_{1-2} = 105.31$ $\bar{y}_{1-2} = 31.50$	$-x_4$	10.80 19.46 4.54 12.20 $\sum y_{1-3} = 47.00$ $\bar{y}_{1-3} = 11.75$	9.09 10.21 $\sum y_{1-4} = 19.30$ $\bar{y}_{1-4} = 9.65$

$$s_i = \sqrt{\frac{\sum_i y_i^2}{n_i - 1} - \frac{\left(\sum_i y_i\right)^2}{n_i(n_i - 1)}}, \tag{22}$$

where n_i is the number of terms in the considered cell.

For the considered case, the Student's criteria, calculated using Eqs. (21) and (22), are $t_{X_1} = -2.37$ and $t_{X_4} = 5.18$. A factor is considered to be significant if $t_{X_i} > t_{\mathrm{cr}}$ where the critical value, t_{cr} for the Student's criterion in found in a statistical table for the following number of degrees of freedom

$$f_r = \sum_i n_i - k = 11 - 4 = 7, \tag{23}$$

where k is the number of cells in the correlation table.

For the considered case, $t_{0.05} = 2.365$ and $t_{0.10} = 1.895$ (Table 5.7 in [34]) so that the considered factors are significant with a 95 % confidence level.

The discussed procedure is the first stage in the proposed sieve DOE. This first stage allows the detection of the strongest factors, i.e., those factors that have the strongest influence on the response. After these strong linear effects are detected, the size of "the screen" to be used in the consecutive sieves is reduced to distinguish less strong effects and their interactions. Such a correction is carried out by adding the effects (with the reverse signs) of the selected factors (Eqs. 19 and 20) to column $\bar{y}$ of Table 9, namely by adding +6.00 to all results at level "$+x_1$" and −13.74 to all results at level "$+x_4$." The corrected results are shown in column $\bar{y}_{c1}$ of Table 9.

Following the procedure presented by the author earlier [7] and using the results shown in column $\bar{y}_{c1}$ of Table 9, one can construct a new correlation diagram shown in Fig. 3, where for simplicity, only a few interactions are shown although all possible interactions have been analyzed through constructing a new correlation table. Evaluation of the effects obtained and the subsequent corrections of the results are carried out till the remaining effects are found insignificant as having less than 10 % significance level. In the considered case, the sieve procedure was stopped after the second sieve (the corrected results are shown in column $\bar{y}_{c2}$ of Table 9). Aftereffects X_7 and X_8 of the corresponding factors were found

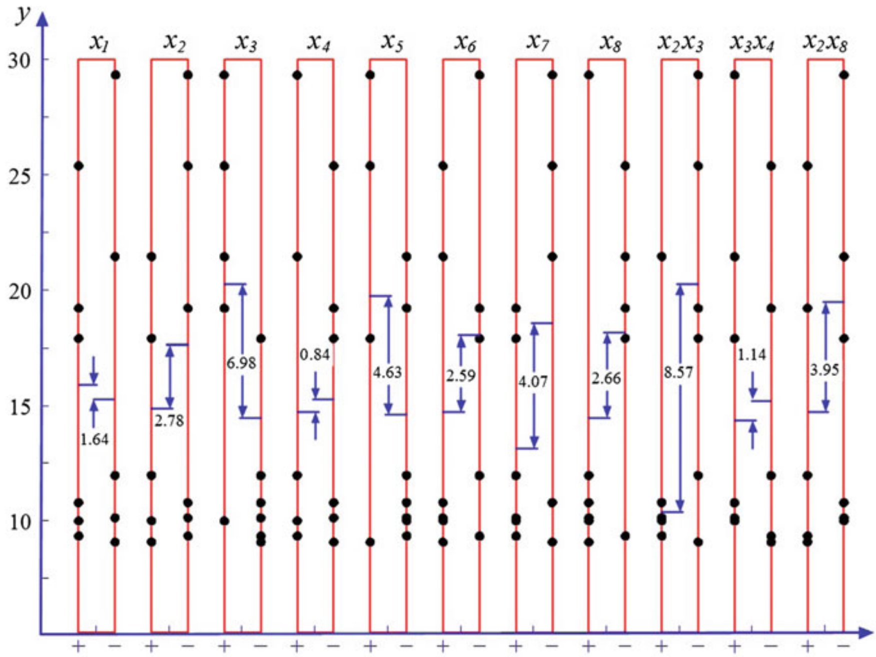

Fig. 3 Correlation diagram (first sieve)

insignificant because the calculated student's coefficients for these factors are $t_7 = 0.79$ and $t_8 = 0.87$, whereas the critical value, $t_{cr(0.10)} = 1.865$.

A scatter diagram was constructed (Fig. 4) to visualize the screening effects. As can be seen, the scatter of the factors reduces after each screening. The results of factors screening are shown in Table 11. Figure 5 shows the significance of the distinguished effects in terms of their influence on tool life. As seen, four linear effects and one interaction were distinguished.

The analysis of the obtained results shows that the web diameter has the strongest effect on tool life. This result was expected by the known experience [10]. What was not expected is the second strongest effect of the interaction of the clearance angle with the point angle, particularly the negative sign of this interaction. The meaning of the negative sign of x_2x_3 and x_1 is that tool life decreases when these parameters increase. The obtained effect of factors x_3, x_1, and x_5 are known from drilling practice.

Consider another example of the use of PB DOE in face milling of a gray cast iron pump cover with a face milling tool equipped with PCBN cartridges. Twelve input factors, including machining regime and tool geometry parameters, were included in DOE, namely x_1 is the cutting speed (m/min); x_2 is the cutting feed (mm/rev); x_3 is the depth of cut (mm); x_4 is the length of the chamfer cutting edge (mm); x_5 is the normal rake angle (°); x_6 is the normal clearance angle of the major cutting edge (°); x_7 is the normal clearance angle of the minor cutting edge (°); x_8 is

Fig. 4 Scatter diagram

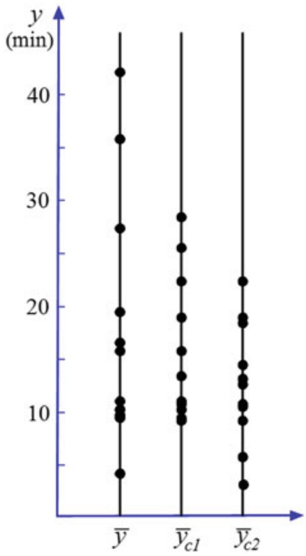

Table 11 Summary of the screening test

Stage of analysis	Effects	Value of effects	Calculated *t*-criteria
Initial data	X_1	−6.00	2.37
First sieve	X_4	13.74	5.18
	X_3	6.36	2.38
	X_5	3.20	1.91
	X_2^{a}	–	–
Second sieve	X_7	2.71	0.87
	X_8	2.03	0.79

[a]Factor is distinguished due to its interaction with factor x_3 with effect −7.65 (t_{23} = 1.97)

Fig. 5 Significance of the effects distinguished by the sieve DOE (Pareto analysis)

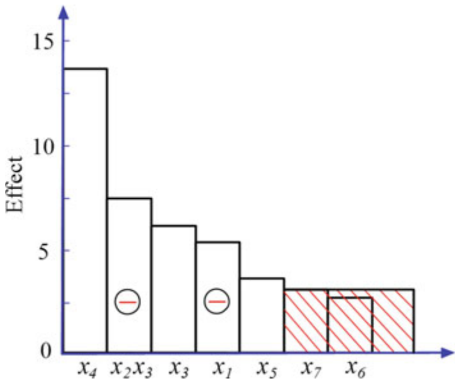

Table 12 The levels of factors selected for the sieve DOE

Factors	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}	x_{11}	x_{12}
+1	1580	0.25	0.50	2.0	+5	20	20	20	0.2	60	30	10
−1	700	0.05	0.05	0.5	−15	5	5	5	0	30	10	0
0	1180	0.75	0.275	1.25	−5	12.5	12.5	12.5	0.1	45	20	5

the normal clearance angle of the chamfered part of the cutting edge (°); x_9 is the edge preparation parameter (mm); x_{10} is the tool cutting edge angle of the major cutting edge (°); x_{11} is the tool cutting edge angle of the minor cutting edge (°); x_{12} is the inclination (in the tool-in-holder system [35]) angle of the major cutting edge (°). The levels of factors are shown in Table 12. Experience shows that these include parameters may affect tool life; some of them significantly whereas other might have much less significant effect depending on the machining conditions, setting level of factors, and other test particularities.

The tool life, T measured by the operating time over which the roughness of the milled surface remains less than or equal to Ra = 1.25 microns was chosen as the response (the test output). Pre-DOE experience shows that the flank wear is the predominant wear mode under the test conditions. As such, the average flank wear VB_B = 0.3–0.5 mm was observed at the end of tool life. Moreover, it particular value in this range depends on the test conditions (Table 12).

The design matrix was constructed by random mixing of four one-fourth replicas the matrix of the full factorial DOE. All the selected factors were separated into two groups. Factors x_1, x_2, x_3, x_4, x_5, x_6 are gathered in the first group used to contract a one-fourth replica of the full factorial DOE 2^{6-2} with defining contrast $1 = x_1\ x_2\ x_3\ x_5$. In this replica, the effects of the factors are not mixed. The same way is used to construct replica for the other half of the factors. A design matrix was constructed using the first half-replica of the complete matrix and adding to each row of this replica a randomly selected row from the second replica. Table 13 shows the constructed design matrix. As before, the suitability of the constructed design matrix was analyzed and the constructed matrix is proven to be suitable.

In Table 13, the responses $\overline{T}_i\ i\ \ldots\ 11$ are the average tool life calculated over three independent tests replicas obtained under the indicated test conditions.

Analysis of the result of sieve DOE begins with the construction of a correlation (scatter) diagram shown in Fig. 6. Its structure is self-evident [4]. Each factor is represented by a vertical bar having on its left side values (as dots) of the response obtained when this factor was positive (the upper value), while the values of the response corresponding to lower lever of the considered factor (i.e., when this factor is negative) are represented by dots on the right side of the bar. As such, the scale makes sense only along the vertical axis.

Each factor included in the experiment is estimated independently. The simplest way to do this is to calculate the distance between the means on the left and right side of each bar. These distances are shown on the correlation diagram in Fig. 6. As

Table 13 Design matrix

Number of test	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9	x_{10}	x_{11}	x_{12}	Average tool life, $\overline{T}$ (min)	Correction, T_c
1	+	−	−	+	−	−	+	+	−	−	+	−	1163.5	1162.5
2	+	−	+	−	−	+	−	−	+	−	−	+	106	26.0
3	−	+	−	−	−	+	+	−	−	+	+	+	10	423.75
4	+	+	+	+	+	+	+	+	+	+	+	+	0.05	423.80
5	+	+	−	−	+	−	−	−	−	+	−	−	50	582.18
6	−	+	+	+	−	−	−	+	+	+	−	−	0.0	542.18
7	+	−	+	+	+	−	−	−	−	+	−	+	400	430.00
8	−	−	−	−	+	+	+	+	+	+	−	−	645	510.67
9	−	−	+	−	+	−	+	−	+	−	+	−	770	770.00
10	−	−	−	+	+	+	−	+	−	−	−	+	1155	1020.57
11	−	+	+	−	−	−	+	+	−	−	−	+	10	552.10
12	−	−	+	−	−	+	+	−	−	+	−	−	5	134.40
13	−	−	−	−	−	−	−	+	+	+	+	+	270	270.00
14	+	+	+	−	+	−	−	+	−	−	+	−	25	433.75
15	+	+	−	+	−	+	−	−	+	−	+	−	20	487.75
16	−	+	−	+	+	−	+	−	+	−	−	+	88	924.18
17	0	0	0	0	0	0	0	0	0	0	0	0	245	924.18

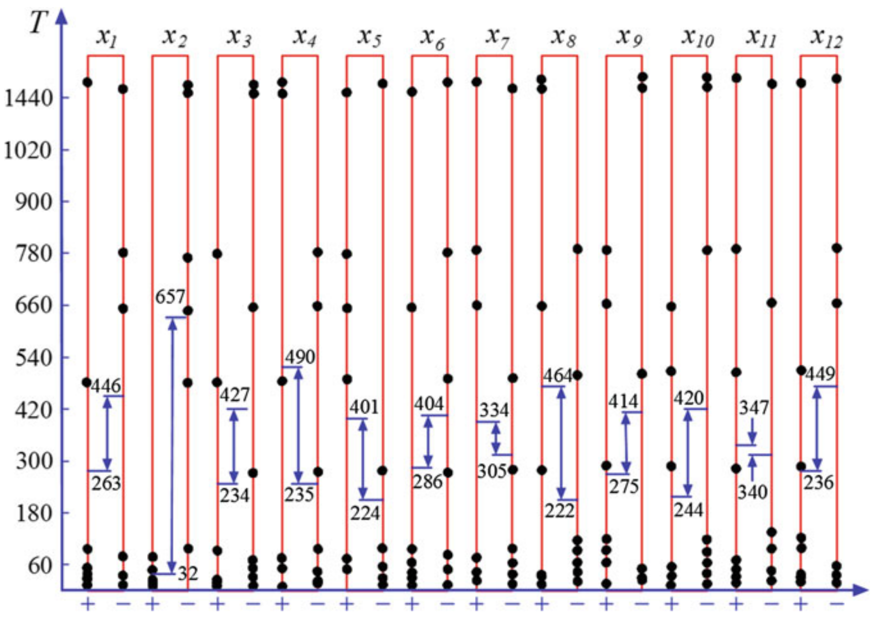

Fig. 6 Correlation diagram (original data)

Table 14 Correlation table (original data)

Estimated factor	$+x_2$	$-x_2$
$+x_4$	0.05 0.05 80 82 ——— $\overline{T}_1 = 40.1$	1162.5 498 1155 0 ——— $\overline{T}_2 = 701.87$
$-x_4$	16 50 10 25 ——— $\overline{T}_3 = 25.25$	108 645 770 270 ——— $\overline{T}_4 = 448.25$

can be seen, these are the greatest for factors x_2 (the cutting feed) and x_4 (the length of the chamfer cutting edge), and thus these two factors are selected for analysis.

The effects of factors are calculated using a correlation table shown in Table 14. As can be seen, the effects for the considered case are $X_2 = -524.18$ and $X_4 = 134.63$.

The Student's criteria for the selected factors were calculated to be $t_2 = 6.87$ and $t_4 = 5.24$. The critical value of the Student's criterion, t_{cr} is found in a statistical table for the following number of degrees of freedom:

$$f_r = \sum_i n_i - k = 12, \tag{24}$$

where k is the number of cells in the correlation table.

For the considered case, $t_{0.05} = 2.18$ (Table 5.7 in [34]). Therefore, the considered factors are significant with a 95 % confidence level as $t_2 > t_4 > t_{0.05}$, i.e., they have a significant influence on tool life.

The discussed procedure concludes the first stage of the sieve. The corrected results of the first sieve results are presented in column T_c of Table 13. Using the data of this table, a new correlation diagram is constructed as shown in Fig. 7. As can be seen in this figure, the scatter of the analyzed data reduces significantly after each sieve.

An analysis of Fig. 7 allowed to distinguish three factors, namely x_3 (the depth of cut), x_8 (the normal clearance angle of the chamfered part of the cutting edge), and x_{10} (the tool cutting edge angle of the major cutting edge) for further analysis.

A new correlation table (Table 15) was constructed to analyze the considered factors. The effects of factors were calculated using this table: $X_3 = -127.50$, $X_8 = 105.40$, and $X_{10} = 110.1$. The Student's criteria for the selected factors were calculated to be $t_3 = 5.01$, $t_8 = 4.42$, and $t_{10} = 4.91$. For the considered case, $t_{0.05} = 2.18$ (Table 5.7 in [34]). Therefore, the considered factors are significant with a 95 % confidence level as $t_2 > t_4 > t_{0.05}$, i.e., they have a significant influence on tool life. The critical value of the Student's criterion, t_{cr} is found in a statistical table

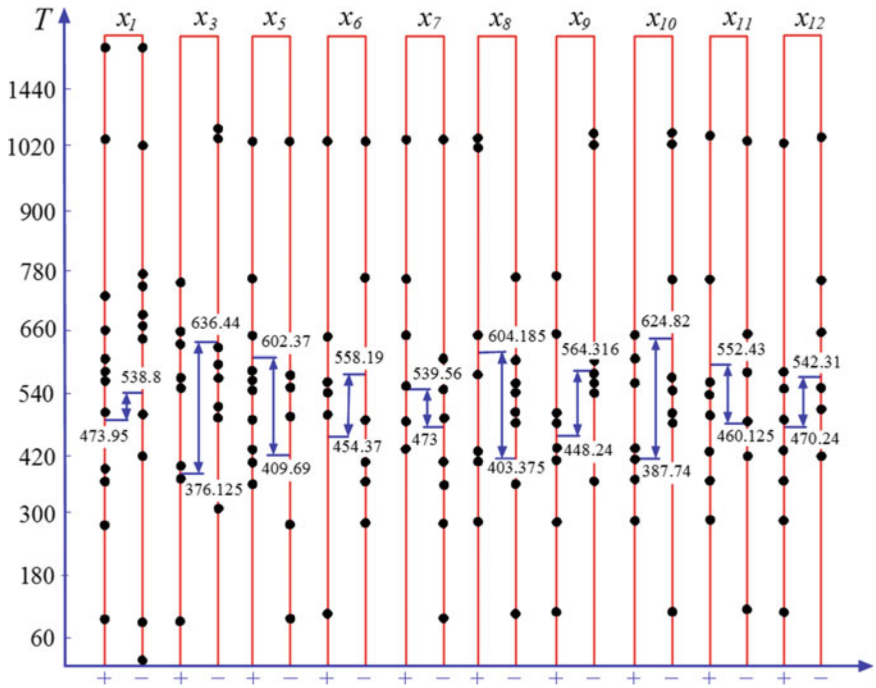

Fig. 7 Correlation diagram (first sieve)

Table 15 Correlation table (first sieve)

Estimated factor	$+x_3$		$-x_3$	
$+x_{10}$	$+x_8$	$-x_8$	$+x_8$	$-x_8$
	423.80 542.13	430.00 −134.43	510.57 270.00	423.75 542.18
$-x_{10}$	552.18 432.75	−26.00 770.00	1162.50 1020.60	487.75 624.18

for the number of degrees of freedom $f_r = \sum_i n_i - k = 16 - 2 = 8$ at 95 % significance level to be $t_{0.05}$ = 2.306 (Table 5.7 in [34]).

The results of factors screening are shown in Table 16. Figure 8 shows the significance of the distinguished effects in terms of their influence on tool life. The analysis of the obtained results shows that five factors out of twelve included in the test are found to be significant, and thus should be included in the full factorial test.

As expected, the cutting feed (factor x_2) has the strongest effect on tool life under the selected tool life criterion. The second strongest effect has the length of the chamfer of the cutting edge (factor x_4). This was not obvious before testing.

Table 16 Summary of the screening test

Stage of analysis	Effects	Value of effects	Calculated *t*-criteria
Initial data	X_2	−524.0	6.87
	X_4	134.4	5.24
First sieve	X_3	−127.5	5.01
	X_{10}	119.1	4.91
	X_8	10.4	4.42

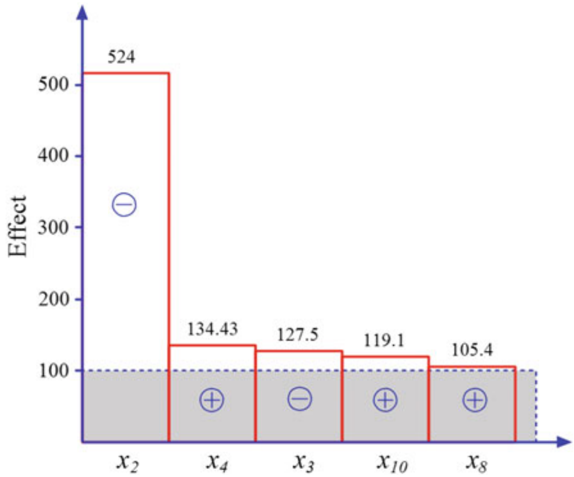

Fig. 8 Significance of the effects distinguished by the sieve DOE (Pareto analysis)

Influences of the depth of cut (factor x_3) and the tool cutting edge angle of the major cutting edge are common for metal machining in terms of their signs and effects. The most interesting finding is the effect of the normal clearance angle of the chamfered part of the cutting edge (factor x_8). Using the result of the subsequent full factorial DOE, and further optimization of this factor allowed an increase in tool life by factor 5 while solving a long-standing problem in the automotive industry.

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