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The Comparison of the 3^k Full Factorial and 3^{k-p} Confounded Design for the assessment of Wheat Production

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ABSTRACT

The purpose of an irrigation experiment is to increase food security during the dry season; thus, it is crucial to select an experimental design that can effectively and efficiently assess the effects of various irrigation techniques on crop productivity. A complete factorial design and a confounded design are two possible designs that may be employed. Hence, this study compared the performance of the 3^3 full factorial and 3^{3-p} confounded design for the dry season irrigation experiment. The objectives of the study include: to group the treatment in a 3^3 full factorial experiment into various blocks using confounding; to test the significance of main effects and the interaction effects not confounded and to calculate the gain in precision when the 3^3 full factorial design is confounded. The data for this study were secondary data obtained from the Anambra State River Basin Authority (ARBA) in Anambra State. The findings of the study showed that factor B (row spacing) has a significant impact on the design using the full factorial design while the no effect and interactions were found to

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insignificantly impact on the confounded design. It was found that the full factorial 3^3 experiment minimizes the average variance of the parameter estimates and minimizes the maximum variance of all possible normalized linear combinations of parameter estimates than the confounded experiment which was found to relatively maximize the information matrix than the confounded experiment but the confounded experiment which relatively maximizes the information matrix than the full factorial experiment. The full factorial design provides an increase in precision of about 13% when employed instead of the confounded design, and it was found that the full factorial design comparatively optimizes the information matrix compared to the confounded design with a value of 0.94, which is close to 1. As a result, it was shown that confounding a full factorial design does not always result in a more efficient design since some information is lost when specific effects are confounded with blocks.

Keywords: Confounded, Factorial Experiment, Irrigation experiment, Wheat

INTRODUCTION

The experimental design is a method for meticulously organizing studies to ensure that your findings are both impartial and reliable. A strategy for applying various experimental circumstances to various units is known as an experimental design. This plan aims to find out how the conditions impact certain measurements, sometimes referred to as the criteria or the dependent variable (Crespi, 2016). The person may be exposed to various settings under various conditions. These circumstances emerge from elements that are typically referred to as independent variables that have a range of values termed levels (Eae, 2003). Variables with two or more fixed values, or levels, are considered to be the elements of an experimental design. To investigate how the amounts of the factors affect the dependent variable, experiments are conducted (Gujral *et al.,* 2018).

The experimental design used should ideally specify how treatments are assigned to experimental groups. A typical approach is a totally random design, in which treatments are distributed to groups at random. An alternative approach is known as randomized block design, in which treatments are split into homogeneous blocks and then randomly assigned to groups. Moreover, confounding factors should be reduced or eliminated as they might provide additional justifications for the findings of the experiment. It also reduces variability, making it simpler for you to identify variations in treatment results by drawing conclusions about the link between independent and dependent variables.

A factorial experiment is a crossing factor design that often includes numerous factors, allowing for the examination of every conceivable factor combination

(Fukuda et al., 2018). Because there are more variables, there are more treatment options available, and it becomes impossible to fit all of these treatment combinations into a single homogeneous block. In industrial research and development, the two-level series of the factorial and fractional factorial design is frequently used (Montgomery, 2001).

Certain issues come up in the statistical design of experiments these issues are illustrated as follows. Firstly, the research problem should be translated into statistical terms. Thus, applying a statistical design always involves consideration of the field of research as well as statistical considerations. A second issue is the construction of a set of treatments. This entails the decision of using factorial designs if such designs are to be used, then, one has to decide on the level of the factors and the combinations of factor level to be used in the experiment. Usually, the levels-or at least the range of these-are chosen based on earlier experience. The choice of combinations of levels is typically a statistical issue, although there may be practical constraints. In most research, there is to be a two-level experiment, possibly because of cost considerations, waste of experimental materials and the efficiency of the design of the experiment (Schoen, 2000). The factorial permits the sensitive detection of active interactions. All combinations of the factor's levels are tried out, permitting the unambiguous estimation of all main effects and all interactions. All the treatments are tried more than once. This feature permits the estimation of random error. Thirdly, there is the issue of allocating treatments to the experimental units. This issue is intimately connected to the experimental conduct. Also, the statistical analysis of the results can also be viewed as a design issue. This is because the separation of random and symmetric differences highly depends on the exact handling of experimental material and the available treatments (Schoen, 2000).

A simple experimental design is the full-factorial $r3^k$, which consists of all possible combinations of the levels of the factors. In a full-factorial design, all main effects, two-way interactions, the higher-order interactions are estimable and uncorrelated. Several factors or variables are varied concurrently in a full factorial design, allowing for the analysis of both the main effects and the interaction effects of each variable on the outcome of interest. A full factorial design might entail adjusting variables including irrigation schedule and frequency, type and quantity of fertilizer used, and crop variety in the context of a dry season irrigation experiment. Alternatively, the full-factorial design can be said to involve testing all possible combinations of the elements under investigation is part of a full factorial design. This would entail experimenting with every conceivable combination of various irrigation methods and timings in the case of an irrigation experiment. The ability to identify particular interactions between the components being researched makes this sort of design extremely effective, but it may also be time- and resource-intensive. This approach can give more detailed and accurate information about how each element affects agricultural output and quality, and it can assist determine the best combinations of factors to increase food security.

The problem with a full-factorial design is that, for most practical situations, it is costly and tedious to have subject's rate at all possible combinations. For this reason, researchers often use confounded designs, which have fewer treatments than a full-factorial design. Confounding is when a factorial experiment is performed in more than one incomplete block (Collins, 2009). This implies that when the estimates of two experimental effects are indistinguishable, then they are said to be confounded. A confounded design, on the other hand, combines specific factors or variables into one factor or treatment level on purpose, which might save cost and improve the experiment. A confused design in the context of a dry season irrigation experiment may divide various irrigation timings and frequencies into one treatment level and compare this group to another treatment group with various fertilizer and crop variety combinations. Compared to a complete factorial design, this form of design may be more effective and use fewer resources, but it may also make it harder to interpret the data since the effects of the several components may be mixed together. The price of having fewer treatments is that some effects become confounded with blocks. Effects are confounded when information on such effects are sacrificed. The term "orthogonal array," as it is sometimes used in practice, is imprecise. It correctly refers to designs that are orthogonal and balanced, hence efficient. It is also imprecisely used to refer to designs that are orthogonal but not balanced, and hence potentially inefficient. A design is balanced when each level occurs equally often within each factor, which means the intercept is orthogonal to each effect. The imbalance is a generalized form of nonorthogonality, which increases the variances of the parameter estimates. Despite the loss of information of some treatment combinations when a full factorial design is confounded, researchers still prefer the later design as it handles the cost and time problem associated with the earlier design. As regards to these issues, this study seeks to compare the full factorial to confounded design concerning the efficiency of the designs.

Three-level designs are useful for investigating quadratic effects. The three-level designs are written as a 3^k factorial design. The three-level design may require the prohibitive number of treatments, unfortunately, the three-level design is prohibitive in terms of the number of treatments, and thus in terms of cost and effort. A two-level design with center points is much less expensive while it is still a very good (and simple) way to establish the presence or absence of curvature.

However, the justification for the present study was based on the quest for diversification of the Nigerian economy into active agriculture and other non-oil sector across the nation. Hence, the need for experiment on growing wheat in the South Eastern Nigeria became necessary. Wheat in Nigeria is largely supplied from the Northern Part of the Country and the state of insecurity in the North has made it necessary for the authorities to encourage formers in other parts of the country to close the gap on food insecurity in the Country. Aside from the nutritious benefits of wheat, wheat production in an irrigated area helps improve soil quality, circulate nutrients and add nitrogen, break the cycle of annual and perennial weeds, protect the soil from soil erosion, distribute farms and get a good return on investments.

Also, findings from available literature on the subject matter observed that the majority of the literature focused on a 2^k factorial design while very few literatures focused on the 3^n factorial design. It was observed that most of the researchers in this area employ the 2^k designs over the 3^k design because of its limitations in terms of the number of treatment combinations, time, financial constraints and efficiency. Hence, the need for the present study to consider a full factorial design and also reduce the treatment combinations to be applied in the experiment by confounding to save experimental materials, cost, time and reduce experimental error.

Hence, the aimed at comparing the 3^k full factorial and 3^{k-p} confounded design for wheat production in Anambra State with the following specific objectives: to group the treatment in a 3^3 full factorial experiment into various blocks using confounding, to test the significance of main effects and the interaction effects not confounded, to calculate the gain in precision when the 3^3 full factorial design is confounded, and to determine the best design between the full factorial design and the confounded design.

MATERIALS AND METHODS

Source of Data

The data for this study were secondary data obtained from the Agriculture Department of Anambra State River Basin Authority (ARBA) in Anambra State. Dry season irrigation experiment was conducted by Anambra State River Basin Authority (ARBA) in which the effects of 3 factors (planting materials, seed rates in kg/ha, and row spacing in cm) were investigated on plant size. Planting was by drilling to a depth of 3 inches, irrigation was carried out immediately and thereafter subsequent irrigation was carried on a weekly interval until the maturity of the plants. Recommended fertilizer rate is 120kg/ha of urea (source of nitrogen (N) in fertilizer), 60kg/ha of P_2O_5

and applied at split dose, half at planting and the other half in 2 weeks after germination.

Planting Materials:

V_1 – Local Variety

V_2 – R_{32}-BB-PCBWH-98

V_3- Top's' NARO-CM3-PCBWH-1729

Seed Rates:

1. 50kg/ha
2. 100kg/ha
3. 150kg/ha.

Row Spacing (in between rows)

1. 15cm
2. 25cm
3. 35cm

Data were collected by quadrant, seeds threshed and measured in weighed. All measurements are in kg/plot.

Model Specification

It can be seen that one of the factors is qualitative and the remaining are quantitative. Therefore, we could fit a quadratic model such as:

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + \beta_{11} x_1^2 + \beta_{22} x_2^2 + \varepsilon$$

(1)

Where,

y is the response

β's are the model coefficients

Suppose we let

$\beta_{12} = \beta_3, \beta_{11} = \beta_4, \beta_{22} = \beta_5$. $x_1 x_2 = x_3, x_1^2 = x_4, x_2^2 = x_5$.

Then we can have,

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_3 + \beta_4 x_4 + \beta_5 x_5 + \varepsilon$$

(2)

Which is a linear regression model, but the qualitative will have an impact, thus the statistical model in regression form is given as:

$$y(x_1 x_2 x_3) = \beta_{000} + \beta_{100} x_1 + \beta_{010} x_2 + \cdots + \beta_{222} x_1^2 x_2^2 x_3^2 + \varepsilon(x_1 x_2 x_3)$$

$$E(y) = \hat{\beta}_{000} + \hat{\beta}_{100} x_1 + \hat{\beta}_{010} x_2 + \cdots + \hat{\beta}_{222} x_1^2 x_2^2 x_3^2$$

Since $E(\varepsilon) = 0$.

The standard order for a 3^3 design is

$(1), A_L, A_Q, B_L, A_LB_L, A_QB_L, B_Q, A_L B_Q, A_QB_Q, C_L, A_L C_L, A_QC_L, B_L C_L, A_LB_LC_L, A_QB_LC_L, B_QC_L, A_LB_QC_L,$
$A_QB_QC_L, C_Q, A_LC_Q, A_QC_Q, B_L C_Q, A_LB_LC_Q, A_Q B_L C_Q, B_QC_Q, A_LB_QC_Q, A_QB_QC_Q$

(0, 1, 2) was used to code each factor at the various levels, hence, the treatment combination can be presented as follows: 000,100, 200, 010, 110, 210, 020, 120, 220, 001,101, 201, 011, 111, 211, 021, 121, 221, 002, 102, 202, 012, 112, 212, 022, 122, 222

Estimation of Model Parameters

The method of least squares is typically used to estimate the parameters of the model equation (Equation 2). We may write the model equation as:

$$y_i = \beta_0 + \beta_1 x_{i1} + \beta_2 x_{i2} + \ldots + \beta_k x_{ik} + \varepsilon_i$$
$$= \beta_0 + \sum_{j=1}^{k} \beta_j x_{ij} + \varepsilon_i, i = 1, 2, \ldots, n$$

(3)

This can be written in matrix form as:

$$y = X\beta + \varepsilon$$

(4)

Where:

$$y = \begin{pmatrix} y_1 \\ y_2 \\ \vdots \\ y_n \end{pmatrix}, X = \begin{pmatrix} 1 & x_{11} & x_{12} \cdots x_{1k} \\ 1 & x_{21} & x_{22} \cdots x_{2k} \\ \vdots & \vdots & \vdots \\ 1 & x_{n1} & x_{n2} \cdots x_{nk} \end{pmatrix}, \beta = \begin{pmatrix} \beta_1 \\ \beta_2 \\ \vdots \\ \beta_k \end{pmatrix}, \text{ and } \quad \varepsilon = \begin{pmatrix} \varepsilon_1 \\ \varepsilon_2 \\ \vdots \\ \varepsilon_n \end{pmatrix}$$

In general, y is an $(n\times1)$ vector of the observations, X is an $(n\times p)$ design matrix of the levels of the independent variables, β is a $(p\times1)$ vector of the regression coefficients, and ε is an $(n\times1)$ vector of random errors.

Statistical Design

The 3^k factorial design is a factorial arrangement with k factors each at three levels. One possible way to differentiate the factor levels in a 3^k design is to represent the factor levels as 0(Low), 1(intermediate), and 2(high).

A single replicate of the 3^kdesign if considered requires so many treatments that it is unlikely that all treatments can be made or applied to the experimental units under uniform conditions. Thus, confounding in blocks is often necessary. Confounding in design of experiment is the act of influencing both the dependent variable and independent variable thereby causing a spurious association. Confounding is necessary in design of experiment when controls do not allow the experimenter to reasonably plausible alternative explanations for an observed association between the explanatory and response variables.

A confounding design equally involves a situation where by some treatment effects (main or interactions) are estimated by the same linear combination of the experimental observations as some blocking effects. In this situation, the treatment effect and the blocking effect are said to be confounded. Confounding is also used as a general term to indicate that the value of a main effect estimate comes from both the main effect itself and also contamination or bias from higher order interactions.

The 3^k design may be confounded in 3^p incomplete blocks, where $p< k$. Thus, these designs may be confounded in three blocks, nine blocks, and so on (Montgomery, 2012).

The 3^3 Factorial Design

Suppose there are three factors (A, B, and C) under study and that each factor is at three levels arranged in a factorial experiment. This is a 3^3 factorial design, and the experimental layout and treatment combination notation are shown below. The 27 treatment combinations have 26 degrees of freedom. Each main effect has two degrees of freedom, each two-factor interaction has four degrees of freedom, and three factor interaction has eight degrees of freedom.

The sums of squares may be calculated using the standard methods for factorial designs. In addition, if the factors are quantitative, the main effects may be partitioned into linear and quadratic components, each with a single degree of freedom. The two-factor interactions may be decomposed into linear × linear, linear × quadratic, quadratic × linear and quadratic × quadratic effects. Finally, the three-factor interaction ABC can be partitioned into eight single-degrees-of-freedom components corresponding to linear × linear × linear, linear × linear × quadratic, and so on. Such a breakdown for the three-factor interaction is generally not very useful.

It is also possible to partition the two-factor interactions into there I and J components. These would be designated AB, AB^2, AC, AC^2, BC, and BC^2, and each component would have two degrees of freedom. As in the 3^2 designs, these components have no physical significance.

The three-factor interaction ABC may be partitioned into four orthogonal two degrees-of-freedom components, which are usually called the W, X, Y, and Z components of the interaction. They are also referred to as the AB^2C^2, AB^2C, ABC^2, ABC components of the ABC interaction, respectively. The two notations are used interchangeably; that is,

$$W(ABC) = AB^2C^2$$

(5)

$$X(ABC) = AB^2C$$

(6)

$$Y(ABC) = ABC^2$$

(7)

$$Z(ABC) = ABC$$

(8)

Note that no first letter can have an exponent other than 1. Like the I and J components, the W, X, Y, and Z components have no practical interpretation. They are, however, useful in constructing more complex designs.

The General 3^kFactorial Design

The concept utilized in the 3^3factorial design can be readily extended to the case of k factors, each at three levels, that is, to a 3^k factorial design. The usual digital notation is employed for the treatment combinations, so 0120 represents a treatment combination in a 3^4 design with A and D at low levels, B at the intermediate level, and C at high level. There are 3^k treatment combinations, with $3^k – 1$, degrees of freedom between them. These treatment combinations allow sums of squares to be determined for k main effects, each with two degrees of freedom; $\binom{k}{2}$ two-factor interactions, each with four degrees of freedom; and one k-factor interaction with 2^k degrees of freedom. In general, high-factor interaction has 2^h degrees of freedom. If there are n replicates, there are $n3^k – 1$ total degrees of freedom and $3^k(n – 1)$ degrees of freedom for error.

Sums of squares for effects and interactions are computed by the usual methods for factorial designs. Typically, three-factor and higher interactions are not broken down any further. However, any h-factor interaction has 2^{h-1} orthogonal two-degrees-of-freedom components. For example, the four-factor interaction ABCD has $2^{4-1} = 8$ orthogonal two-degrees-of-freedom components, denoted by $ABCD^2$, ABC^2D, AB^2CD, $ABCD$, ABC^2D^2, AB^2C^2D, AB^2CD^2, and $AB^2C^2D^2$. In writing these components, note that the only exponent allowed on the first letter is 1. If the exponent on the first letter is not 1, then the entire expression must be squared and the exponents will be reduced to modulus 3. To demonstrate this, consider

$$A^2BCD = (A^2BCD^2)^2 = A^4B^2C^2D^2 = AB^2C^2D^2$$

(9)

These interaction components have no physical interpretation, but they are useful in constructing more complex designs.

Confounding the 3^kFactorial Design

When a single replicate of the 3^kfactorial design is considered, the design requires so many treatments that it is unlikely that all 3^k treatments can be made under uniform conditions. Thus, confounding in blocks is often necessary. The 3^kfactorial design may be confounded in 3^p incomplete blocks, where $p < k$. Thus, these designs may be confounded in three blocks, nine blocks, and so on. We shall illustrate the principles of confounding in 3^k in 3^p plots per block with the help of 3^3 experiments laid out in blocks of size 3^2=9. Let the three factors be A, B and C and the confounded interaction will be ABC^2. The three levels of each of the factor be denoted by 0,1 and 2 and a particular treatment combination be $x_i x_j x_k, i, j, k = 0,1,2$.

Number of blocks per replication = $3^{k-p} = 3$

Block size = $3^p = 9$

Degrees of freedom confounded = 2

Number of interactions confounded per replicate = $\frac{3^{k-p}-1}{3-1} = 1$

The number of treatments in 3 blocks are determined by solving the following equations models

$x_1 + x_2 + 2x_3 = 0$

(10)

$x_1 + x_2 + 2x_3 = 1$

(11)

$x_1 + x_2 + 2x_3 = 2$

(12)

The combinations in the blocks can be represented as presented in Table 1:

Table 1: Block Layout

Block 1			**Block 2**			**Block 3**		
A	B	C	A	B	C	A	B	C
1	0	1	1	0	0	1	0	2
0	1	1	0	1	0	0	1	2
1	1	2	1	1	1	1	1	0
2	0	2	2	0	1	2	0	0
0	2	2	0	2	1	0	2	0
2	1	0	2	1	2	2	1	1
1	2	0	2	2	0	1	2	1
2	2	1	1	2	2	2	2	2
0	0	0	0	0	2	0	0	1

The general procedure is to construct a defining contrast

$L = \alpha_1 x + \alpha_2 x_2 + ... + \alpha_k x_k$

(13)

Where α_i represents the exponent on the ith factor in the effect to be confounded and x_i is the level of the ith factor in a particular treatment combination. For the 3^k series, we have $\alpha_i = 0, 1, or 2$ with the first nonzero α_i being unity, and x_i = 0 (low level), 1 (intermediate level), or 2 (high level). The treatment combinations in the 3^kfactorial design are assigned to blocks based on the value of L (mod 3). Because L (mod 3) can take on only the values 0, 1, or 2, three blocks are uniquely defined. The treatment combinations satisfying L = 0 (mod 3) constitute the principal block. This block will always contain the treatment combination 00…0 (Montgomery, 2012; Zimmer et al., 2017).

Thus, the 3^k factorial design may be confounded in 3^p blocks of 3^{k-p} observations each, where p< k. The procedure is to select p independent effects to be confounded with blocks. As a result, exactly (3^p – 2p – 1)/2 other effects are automatically confounded. These effects are the generalized interactions of those effects originally chosen (Montgomery, 2012).

The goodness or efficiency of an experimental design can be quantified. Common measures of the efficiency of an ($N_D \times$ k) design matrix X are based on the information matrix X'X. The variance-covariance matrix of the vector of parameter estimates $\hat{\beta}$ in a least-squares analysis is proportional to$(X'X)^{-1}$. More precisely, it equals $\sigma^2 (X'X)^{-1}$. The variance parameters σ^2, is an unknown constant. Since σ^2 is constant, it can be ignored (or assumed to equal one) in the discussion that follows. The diagonal elements of $(X'X)^{-1}$ are the parameter estimate variances, and the standard errors are the square roots of the variances. Since they depend only on X (and σ^2), they can be reported by design software before any data are collected. An efficient design has a “small” variance matrix, and the eigenvalues of $(X'X)^{-1}$ provide measures of its “size”. The process of minimizing the eigen values or variances only depends on the selection of the entries in X not on the unknown σ^2 parameter.

The two most prominent efficiency measures are based on quantifying the idea of matrix size by averaging (in some sense) the eigenvalues or variances. *A-efficiency* is a function of the arithmetic mean of the eigenvalues, which is also the arithmetic mean of the variances, and is given by trace $((X'X)^{-1})/k$. A-efficiency is perhaps the most obvious measure of efficiency. As the variances get smaller and the arithmetic mean of the variances of the parameter estimates goes down, A-efficiency goes up. However, there are other averages to consider. *D-efficiency* is a function of the geometric mean of the eigenvalues, which is given by$|(X'X)^{-1}|^{1/k}$. Both D-efficiency and A-efficiency are based on the idea of average variance, but in different senses of the word "average". In practice we usually use D-efficiency for two reasons. It is the easier and faster of the two for a computer program to optimize. Furthermore, relative D-efficiency, the ratio of two D-efficiencies for two competing designs, is invariant under different coding schemes. This is not true with A-efficiency. A third common efficiency measure, *G-efficiency*, is based onσ_M, the maximum standard error for prediction over the candidate set. All three of these criteria are convex functions of the eigenvalues of $(X'X)^{-1}$ and hence usually highly correlated.

For all three criteria, if a balanced and orthogonal design exists, then it has optimum efficiency; conversely, the more efficient a design is, the more it tends toward balance orthogonality. A design is balanced and orthogonal when $(X'X)^{-1}$ is diagonal for a suitably coded X. A design is orthogonal when submatrix of $(X'X)^{-1}$, excluding the row and column for the intercept, is diagonal; there might be off-diagonal non-zeros for the intercept. A design is balanced when all off-diagonal elements in the intercept row and column are zero.

These measures of efficiency can be scaled to range from 0 to 100 (for a suitably coded X):

$$A-efficiency = 100 \times \frac{1}{N_D trace((X'X)^{-1})/k}$$

(14)

$$D-efficiency = 100 \times \frac{1}{N_D \left|(X'X)^{-1}\right|^{1/k}}$$

(15)

These efficiencies measure the goodness of the design relative to hypothetical orthogonal designs that may be far from possible, so they are not useful as absolute measure of design efficiency. Instead, they should be used relatively, to compare one design with another for the same situation. Efficiencies that are not near 100 may be perfectly satisfactory.

RESULTS AND DISCUSSION

The result of the analysis obtained in Table 2 showed that only row spacing (i.e. factor B) is statistically significant at α=0.05 since p-value = 0.0468 is less than α=0.05 using the full factorial design.

The result obtained in Table 2 showed that no main effect or interaction effect was statistically significant after confounding. Further result showed that there is also a 13% gain in precision when we use the full factorial design over the confounded design. Thus, there is no gain in precision when we confound which implies that some information is lost due to confounding.

The result of the optimality presented in Table 4-7 showed that the full factorial design performs better than the confounded design and that the relative D-efficiency is 0.94 which is close to 1. This indicates that the confounded design is slightly D-efficient than the full factorial design and that does not give enough reason for us to say that the later design is better.

Table 2: ANOVA table for the full factorial experiment

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	P-value
A, Seed Rates	14620.91318	2	7310.45659	0.3275	0.7236
B, Row Spacing	15411.5905	2	77055.79525	3.4524	0.0468*
C, Variety	117758.1170	2	58879.0585	2.6380	0.0905
AB	180156.3856	4	45039.0964	2.0179	0.1213
AB^2	116001.8935	2	58000.94675	2.5987	0.0935
AB	64154.49631	2	32077.24816	1.4372	0.2558
AC	146436.4296	4	36609.1074	1.6402	0.1942
AC^2	51375.71631	2	25687.85816	1.1509	0.3319
AC	95033.17993	2	47516.58997	2.1289	0.1392
BC	48921.25285	4	12230.31321	0.5479	0.7021
BC^2	4206.1684	2	2103.0842	0.0942	0.9104
BC	44715.08868	2	22357.54434	1.0017	0.3809
ABC	76081.52932	8	9510.191165	0.4261	0.8946
AB^2C^2	5360.492978	2	2680.246489	0.1201	0.8873
AB^2C	8427.661433	2	4213.830717	0.1888	0.8290
ABC^2	20525.97368	2	10262.98684	0.4598	0.6364
ABC	41992.33743	2	20996.16872	0.9407	0.4032
Error	602622.0017	26	22319.3334		
Total	1340708.22	53			

Table 3: ANOVA table for confounded experiment (AB^2C^2 confounded)

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	P-value
A, Seed Rates	14620.91318	2	7310.45659	0.2886	0.7516
B, Row Spacing	15411.5905	2	77055.79525	3.0418	0.0650
C, Variety	117758.1170	2	58879.0585	2.3242	0.1178
AB	180156.3856	4	45039.0964	1.7779	0.1636
AB^2	116001.8935	2	58000.94675	2.2896	0.1213
AB	64154.49631	2	32077.24816	1.2662	0.2987
AC	146436.4296	4	36609.1074	1.4451	0.2474
AC^2	51375.71631	2	25687.85816	1.0140	0.3766
AC	95033.17993	2	47516.58997	1.8757	0.1734
BC	48921.25285	4	12230.31321	0.4828	0.7481
BC^2	4206.1684	2	2103.0842	0.0830	0.9205
BC	44715.08868	2	22357.54434	0.8826	0.4257
ABC	70721.03634	6	11786.83939	0.4653	0.8275
AB^2C	8427.661433	2	4213.830717	0.1663	0.8476
ABC^2	20525.97368	2	10262.98684	0.4051	0.6710
ABC	41992.33743	2	20996.16872	0.8288	0.4477
Error	607982.4949	24	25332.60395		
Total	1340708.22	53			

$$Precision = \frac{Information\ in\ Design\ 1}{Information\ in\ Design\ 2} \times 100\%$$

$$Information\ in\ Design = \frac{1}{MSE}$$

For full factorial design:

$$I = \frac{1}{22319.3334}$$

$$I = 4.48042 \times 10^{-5}$$

For Confounded design:

$$I = \frac{1}{25332.60395}$$

$$I = 3.94748 \times 10^{-5}$$

$$Precision = 113.5\%$$

Table 4: Optimality Table for 3^3 full factorial design

A-Optimality	D-Optimality	E-Optimality
729	1.8014×10^{16}	12934

Table 5: Optimality Table for the confounded design

A-Optimality	D-Optimality	E-Optimality
1324	9.1198×10^{16}	11584

Table 6: Efficiency Table for 3^3 full factorial design

A-Efficiency	D-Efficiency
0.1372	14.8

Table 7: Efficiency Table for 3^3 confounded designs

A-Efficiency	D-Efficiency
0.0755	15.7

$$Relative\ D - Efficiency = \left[\frac{D - Optimality\ of\ Design\ 1}{D - Optimality\ of\ Design\ 2}\right]^{\frac{1}{K}}$$

Where; K = 27

Relative D-efficiency = 0.94

CONCLUSIONS

This study compared the performance of the 3^3 full factorial and $3^{3\text{-}p}$ confounded design for the dry season irrigation experiment. The findings of the study showed that factor B (row spacing) has a significant impact on the design using the full factorial design while the no effect and interactions were found to insignificantly impact on the confounded design.

It was found that the full factorial 3^3 experiment minimizes the average variance of the parameter estimates and minimizes the maximum variance of all possible normalized linear combinations of parameter estimates than the confounded experiment which was found to relatively maximize the information matrix than the confounded experiment but the confounded experiment which relatively maximizes the information matrix than the full factorial experiment.

The confounded design can't be said to be better than the full factorial design since there is about a 13% gain in precision when the full factorial design is used over the confounded design and since it was found to relatively maximizes the information matrix of the full factorial design to the confounded design with a value of 0.94 which is close to 1. Hence, it is observed that confounding a full a factorial design does not necessarily make it a more efficient design, since some information is lost when we confound certain effects with blocks.

Based on the findings from the present study, it is recommended that when comparing two competing designs, one should always bear in mind that the designs are model dependent. Also, full factorial 3^3 design is recommended for estimating A and E-optimality for the dry irrigation experiment, while the confounding the design in blocks is recommended for estimating model for D-optimality.

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