

Comments on the Neyman–Fisher Controversy and Its Consequences

Arman Sabbaghi and Donald B. Rubin

Abstract. The Neyman–Fisher controversy considered here originated with the 1935 presentation of Jerzy Neyman’s *Statistical Problems in Agricultural Experimentation* to the Royal Statistical Society. Neyman asserted that the standard ANOVA F-test for randomized complete block designs is valid, whereas the analogous test for Latin squares is invalid in the sense of detecting differentiation among the treatments, when none existed on average, more often than desired (i.e., having a higher Type I error than advertised). However, Neyman’s expressions for the expected mean residual sum of squares, for both designs, are generally incorrect. Furthermore, Neyman’s belief that the Type I error (when testing the null hypothesis of zero average treatment effects) is higher than desired, whenever the expected mean treatment sum of squares is greater than the expected mean residual sum of squares, is generally incorrect. Simple examples show that, without further assumptions on the potential outcomes, one cannot determine the Type I error of the F-test from expected sums of squares. Ultimately, we believe that the Neyman–Fisher controversy had a deleterious impact on the development of statistics, with a major consequence being that potential outcomes were ignored in favor of linear models and classical statistical procedures that are imprecise without applied contexts.

Key words and phrases: Analysis of variance, Latin squares, nonadditivity, randomization tests, randomized complete blocks.

1. CONFLICT AND CONTROVERSY

Prior to the presentation of *Statistical Problems in Agricultural Experimentation* to the Royal Statistical Society in 1935 (Neyman, 1935), Jerzy Neyman and Ronald Aylmer Fisher were on fairly good terms, both professionally and personally. Joan Fisher Box’s biography of her father (Box, 1978, pages 262–263, 451) and Neyman’s oral autobiography (Reid, 1982, pages 102, 114–117) describe two scientists who respected each other during this time. However, Ney-

man’s study of randomized complete block (RCB) and Latin square (LS) designs sparked Fisher’s legendary temper (Reid, 1982, pages 121–124; Box, 1978, pages 262–266; Lehmann, 2011, pages 58–59), with the resulting heated debate recorded in the discussion. The relationship between Fisher and Neyman became acrimonious, with no reconciliation ever being reached (Reid, 1982, pages 124–128, 143, 183–184, 225–226, 257; Lehmann, 2011, Chapter 4).

The source of this conflict was Neyman’s suggestion that RCBs were a more valid experimental design than LSs, for both hypothesis testing and precision of estimates. He reached this conclusion using potential outcomes, which he introduced in 1923 as part of his doctoral dissertation (Splawa-Neyman, 1990), the first place formalizing, explicitly, the notation of potential outcomes for completely randomized (CR) experiments. Neyman (1935) extended this framework in a natural way from CR designs to RCBs and LSs, and

Arman Sabbaghi is Assistant Professor of Statistics, Department of Statistics, Purdue University, 250 N. University Street, West Lafayette, Indiana 47907, USA (e-mail: armansabbaghi.stat@gmail.com). Donald B. Rubin is John L. Loeb Professor of Statistics, Department of Statistics, Harvard University, 1 Oxford Street Fl. 7, Cambridge, Massachusetts 02138, USA (e-mail: rubin@stat.harvard.edu).

calculated the expected mean residual sum of squares and expected mean treatment sum of squares for both.

Neyman (1935) stated that, under the null hypothesis of zero average treatment effects (*Neyman's null hypothesis*), the expected mean residual sum of squares equals the expected mean treatment sum of squares for RCBs, whereas the expected mean residual sum of squares is less than or equal to the expected mean treatment sum of squares for LSs, with equality holding under special cases, such as *Fisher's sharp null hypothesis* of no individual treatment effects. From this comparison of the expected mean residual and treatment sums of squares, Neyman concluded that the standard ANOVA F-test for RCBs was "unbiased," whereas the corresponding test for LSs was "biased," potentially detecting differentiation among the treatments, when none existed on average, more often than desired (i.e., having a higher Type I error than advertised under Neyman's null):

In the case of the Randomized Blocks the position is somewhat more favourable to the z test [i.e., the F-test], while in the case of the Latin Square this test seems to be biased, showing the tendency to discover differentiation when it does not exist. It is probable that the disturbances mentioned are not important from the point of view of practical applications. (Neyman, 1935, page 114)

Fisher's fury at Neyman's assertions is evident in his transcribed response:

Professor R. A. Fisher, in opening the discussion, said he had hoped that Dr. Neyman's paper would be on a subject with which the author was fully acquainted, and on which he could speak with authority Since seeing the paper, he had come to the conclusion that Dr. Neyman had been somewhat unwise in his choice of topics. . . . Apart from its theoretical defects, Dr. Neyman appears also to have discovered that it [the LS] was, contrary to general belief, a less precise method of experimentation than was supplied by Randomized Blocks, even in those cases in which it had hitherto been regarded as the more precise design. It appeared, too, that they had to thank him, not only for bringing these discoveries to their notice, but also for concealing them from public knowledge until

such time as the method should be widely adopted in practice! . . . I think it is clear to everyone present that Dr. Neyman has misunderstood the intention . . . of the z test and of the Latin Square and other techniques designed to be used with that test. Dr. Neyman thinks that another test would be more important. I am not going to argue that point. It may be that the question which Dr. Neyman thinks should be answered is more important than the one I have proposed and attempted to answer. I suggest that before criticizing previous work it is always wise to give enough study to the subject to understand its purpose. Failing that it is surely quite unusual to claim to understand the purpose of previous work better than its author. (Fisher, 1935, pages 154, 155, 173)

Although Fisher reacted in an intemperate manner, his discussion nevertheless hints at errors in Neyman's calculations. In fact, Fisher was the sole discussant who identified an incorrect equation (27), in Neyman's appendix:

Then how had Dr. Neyman been led by his symbolism to deceive himself on so simple a question? . . . Equations (13) and (27) of his appendix showed that the quantity which Dr. Neyman had chosen to call σ^2 did not contain the same components of error as those which affected the actual treatment means, or as those which contributed to the estimate of error. (Fisher, 1935, page 156)

Neyman in fact made a crucial algebraic mistake in his appendix, and his expressions for the expected mean residual sum of squares for both designs are generally incorrect. We present the correct expressions in Sections 2.1 and 2.3, and provide an interpretation of these formulae in Section 2.5. As we shall see, if one subscribes to Neyman's suggestion that a comparison of expected mean sums of squares determines Type I errors when testing Neyman's null, then the F-test for RCBs is predictably wrong, whereas the F-test for LSs is unpredictably wrong.

However, Neyman's suggestion is generally incorrect. We present in Section 3.2 simple examples of LSs for which Neyman's null holds and the expected mean residual sum of squares equals the expected mean treatment sum of squares, yet the Type I error of the F-test is smaller than nominal. Such examples lead to the general result that, for any size RCB or LS, Type I errors

are not dictated by a simple comparison of expected sums of squares without further conditions.

A cacophony of commentary on this controversy exists in the literature, and we compiled the most relevant articles in Sections 2.2, 2.4 and 3.1. Our results agree with similar calculations made by Wilk (1955) and Wilk and Kempthorne (1957). A major difference is that we work in a more general setting of Neyman’s framework, whereas others [especially Wilk (1955)] tend to make further assumptions on the potential outcomes, albeit assumptions possibly justified by applied considerations. Furthermore, although Wilk and Kempthorne (1957) extend Neyman’s framework to consider random sampling of rows, columns and treatment levels from some larger population for LSs, their ultimate suggestion that the expected mean residual sum of squares is larger than the expected mean treatment sum of squares is not generally true. A different parametrization of similar quantities, used in Section 2.5, reveals how the inequality could go in either direction.

This controversy had substantial consequences for the subsequent development of statistics for experimental design. As we discuss in Section 4.1, deep issues arising from this disagreement led to a shift from *potential outcomes* to additive models for *observed outcomes* in experiments, seriously limiting the scope of inferential tools and reasoning. Our ultimate goal in this historical study is not simply to correct Neyman’s algebra. Instead, we wish to highlight the genesis of the current approach to experimental design resulting from this controversy, which is based on linear models and other simple regularity conditions on the potential outcomes that are imprecise without applied contexts.

2. CONTROVERSIAL CALCULATIONS

2.1 Randomized Complete Block Designs: Theory

We first consider RCBs with N blocks, indexed by i , and T treatments, indexed by t , with each block having T experimental units, indexed by $j = 1, \dots, T$. Treatments are assigned randomly to units in a block, and are applied independently across blocks (Hinkelmann and Kempthorne, 2008, Chapter 9). Although our results hold for general RCB designs, we adopt the same context as Neyman: blocks represent physical blocks of land on a certain field, and we compare agricultural treatments that may affect crop yield, for example, fertilizers.

We explicitly define treatment indicators $\mathbf{W} = \{W_{ij}(t)\}$ as

$$W_{ij}(t) = \begin{cases} 1, & \text{if unit } j \text{ in block } i \text{ is assigned treatment } t, \\ 0, & \text{otherwise.} \end{cases}$$

Neyman (1935) specified the potential outcomes as

$$x_{ij}(t) = X_{ij}(t) + \varepsilon_{ij}(t),$$

where $X_{ij}(t) \in \mathbb{R}$ are unknown constants representing the “mean yield” of unit j in block i under treatment t , and $\varepsilon_{ij}(t) \sim [0, \sigma_\varepsilon^2]$ are mutually independent and identically distributed (i.i.d.) “technical errors,” independent of the random variables \mathbf{W} . This framework for the potential outcomes, excluding the $\varepsilon_{ij}(t)$, is similar to that presented in Neyman’s 1923 dissertation (Splawa-Neyman, 1990).

Neyman [(1935), pages 110, 114, 145] stated that technical errors represent inaccuracies in the experimental technique, for example, inaccuracies in measuring crop yield, and assumed that technical errors are Normal random variables. We find these technical errors rather obscure, but their inclusion does not alter our conclusions. To summarize, in Neyman’s specification there are two sources of randomness: the unconfounded assignment mechanism (Rubin, 1990), that is, the random assignment of treatments to plots specified by the distribution on \mathbf{W} , and the technical errors $\varepsilon_{ij}(t)$.

Potential outcomes are decomposed by Neyman [(1935), page 111] into

$$(2.1) \quad x_{ij}(t) = \bar{X}_{..}(t) + B_i(t) + \eta_{ij}(t) + \varepsilon_{ij}(t),$$

where

$$\bar{X}_{..}(t) = \frac{1}{NT} \sum_{i=1}^N \sum_{j=1}^T X_{ij}(t),$$

$$B_i(t) = \bar{X}_{i.}(t) - \bar{X}_{..}(t),$$

$$\eta_{ij}(t) = X_{ij}(t) - \bar{X}_{i.}(t),$$

with

$$\bar{X}_{i.}(t) = \frac{1}{T} \sum_{j=1}^T X_{ij}(t).$$

Neyman describes $B_i(t)$ as a correction for the specific fertility of the i th block, and $\eta_{ij}(t)$ as a correction for fertility variation within the block or, alternatively, the soil error. Hinkelmann and Kempthorne [(2008), page 300] refer to terms such as $\eta_{ij}(t)$ as unit-treatment interactions, but they distinguish between

strict unit-treatment interactions and block-treatment interactions. For strict unit-treatment interaction, treatment effects depend on the experimental unit, in the sense that for two treatments t, t' and experimental units j, j' in a block i ,

$$X_{ij}(t) - X_{ij}(t') \neq X_{ij'}(t) - X_{ij'}(t').$$

Block-treatment interactions are characterized by treatment effects depending on the block, in the sense that for two treatments t, t' , experimental units j, j', j'', j''' , and blocks i, i' ,

$$X_{ij}(t) - X_{ij'}(t') \neq X_{i'j''}(t) - X_{i'j'''}(t').$$

As pointed out by a referee, allowing fertility variation to depend on treatment t was a unique contribution by Neyman and was never recognized in the discussion by Fisher, who focused on his sharp null hypothesis (described next), under which the corrections do not depend on t .

The purpose of the local field experiment, as described by Neyman [(1935), page 111] is to compare the $\bar{X}_{..}(t)$ for $t = 1, \dots, T$, each of which represents the average mean yield when one treatment t is applied to all plots in the field, a conceptual experiment. As stated in the discussion, and later by Welch [(1937), page 23] Neyman does not test *Fisher's sharp null hypothesis* of zero individual treatment effects, that is (when excluding technical errors),

$$\begin{aligned} H_0^\# : X_{ij}(t) &= X_{ij}(t') \\ \forall i &= 1, \dots, N; j = 1, \dots, T; t \neq t'. \end{aligned}$$

Instead, Neyman sought to test the more general null hypothesis

$$H_0 : \bar{X}_{..}(1) = \dots = \bar{X}_{..}(T),$$

referred to throughout as *Neyman's null hypothesis*:

I am considering problems which are important from the point of view of agriculture. And from this viewpoint it is immaterial whether any two varieties react a little differently to the local differences in the soil. What is important is whether on a larger field they are able to give equal or different yields. (Neyman, 1935, page 173)

If the treatment effects are additive across all units, that is,

$$\begin{aligned} X_{ij}(t) &= U_{ij} + \tau(t) \\ \forall i &= 1, \dots, N; j = 1, \dots, T; t = 1, \dots, T, \end{aligned}$$

then testing Neyman's null is equivalent to testing Fisher's sharp null.

The observed yield of the plot assigned treatment t in block i is

$$y_i(t) = \sum_{j=1}^T W_{ij}(t)x_{ij}(t),$$

and the observed average yield for all plots assigned treatment t is

$$\bar{y}_{.}(t) = \frac{1}{N} \sum_{i=1}^N y_i(t).$$

Neyman [(1935), page 112] noted that an unbiased estimator for the difference between average treatment means, $\bar{X}_{..}(t) - \bar{X}_{..}(t')$, is $\bar{y}_{.}(t) - \bar{y}_{.}(t')$, and correctly calculated its sampling variance over its randomization distribution as

$$\begin{aligned} \text{Var}\{\bar{y}_{.}(t) - \bar{y}_{.}(t')\} &= \frac{2\sigma_\varepsilon^2}{N} + \frac{\sigma_\eta^2(t) + \sigma_\eta^2(t')}{N} \\ &\quad + \frac{2r(t, t')\sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')}}{N(T-1)}, \end{aligned}$$

where

$$\begin{aligned} \sigma_\eta^2(t) &= \frac{1}{NT} \sum_{i=1}^N \sum_{j=1}^T \eta_{ij}(t)^2, \\ r(t, t') &= \frac{\sum_{i=1}^N \sum_{j=1}^T \eta_{ij}(t)\eta_{ij}(t')}{NT\sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')}}. \end{aligned}$$

Neyman [(1935), page 145] assumed that $\sigma_\eta^2(t)$ and $r(t, t')$ are constant functions of t, t' only to save space and simplify later expressions; this particular set of assumptions appears to have been made purely for mathematical simplicity, and is not driven by any applied considerations, unlike assumptions made by Wilk (1955) and Wilk and Kempthorne (1957) (described in Sections 2.2 and 2.4).

Neyman then calculated expectations of mean residual sum of squares and mean treatment sum of squares, expressed in our notation as (resp.)

$$\begin{aligned} S_0^2 &= \frac{1}{(N-1)(T-1)} \\ &\quad \times \sum_{i=1}^N \sum_{t=1}^T \{y_i(t) - \bar{y}_{.}(t) - \bar{y}_i(\cdot) + \bar{y}_{.}(\cdot)\}^2 \end{aligned}$$

and

$$S_1^2 = \frac{N}{T-1} \sum_{t=1}^T \{\bar{y}_{.}(t) - \bar{y}_{.}(\cdot)\}^2.$$

As proven in our appendix (Sabbaghi and Rubin, 2014), the expectations are

$$\begin{aligned} \mathbb{E}(S_0^2) &= \sigma_\varepsilon^2 + \frac{1}{T} \sum_{t=1}^T \sigma_\eta^2(t) \\ &\quad + \frac{1}{T(T-1)^2} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')} \\ &\quad + \frac{1}{(N-1)(T-1)} \sum_{i=1}^N \sum_{t=1}^T \{B_i(t) - \bar{B}_i(\cdot)\}^2 \end{aligned}$$

and

$$\begin{aligned} \mathbb{E}(S_1^2) &= \sigma_\varepsilon^2 + \frac{1}{T} \sum_{t=1}^T \sigma_\eta^2(t) \\ &\quad + \frac{1}{T(T-1)^2} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')} \\ &\quad + \frac{N}{T-1} \sum_{t=1}^T \{\bar{X}_{..}(t) - \bar{X}_{..}(\cdot)\}^2. \end{aligned}$$

Neyman [(1935), pages 147–150] correctly calculated the expected mean treatment sum of squares, but made a mistake when calculating the expected mean residual sum of squares. His incorrect expression is equation (27) on page 148. Sukhatme [(1935), page 166] his Ph.D. student at the University of London, incorrectly calculated the expectation for the general case when $\sigma_\eta^2(t)$ and $r(t, t')$ are not constant in t, t' , and the corresponding incorrect expression is his equation (3):

$$\begin{aligned} \sigma_\varepsilon^2 + \frac{1}{T} \sum_{t=1}^T \sigma_\eta^2(t) \\ + \frac{1}{T(T-1)^2} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')}. \end{aligned}$$

To see why the last term in $\mathbb{E}(S_0^2)$ is missing in these equations, note that the expression within the brackets of S_0^2 can be written as the sum of the three terms

$$\begin{aligned} &B_i(t) - \bar{B}_i(\cdot), \\ &\sum_{j=1}^T W_{ij}(t)\eta_{ij}(t) - \frac{1}{N} \sum_{i=1}^N \sum_{j=1}^T W_{ij}(t)\eta_{ij}(t) \\ &- \frac{1}{T} \sum_{t=1}^T \sum_{j=1}^T W_{ij}(t)\eta_{ij}(t) \\ &+ \frac{1}{NT} \sum_{i=1}^N \sum_{t=1}^T \sum_{j=1}^T W_{ij}(t)\eta_{ij}(t) \end{aligned}$$

TABLE 1

Table of potential outcomes for a RCB with $\mathbb{E}(S_0^2) > \mathbb{E}(S_1^2)$

	Treatment 1	Treatment 2
Block 1, Plot 1	10	15
Block 1, Plot 2	10	2
Block 2, Plot 1	20	3
Block 2, Plot 2	30	50

and

$$\begin{aligned} &\sum_{j=1}^T W_{ij}(t)\varepsilon_{ij}(t) - \frac{1}{N} \sum_{i=1}^N \sum_{j=1}^T W_{ij}(t)\varepsilon_{ij}(t) \\ &- \frac{1}{T} \sum_{t=1}^T \sum_{j=1}^T W_{ij}(t)\varepsilon_{ij}(t) \\ &+ \frac{1}{NT} \sum_{i=1}^N \sum_{t=1}^T \sum_{j=1}^T W_{ij}(t)\varepsilon_{ij}(t). \end{aligned}$$

Neyman’s equation (17) is missing the first term $B_i(t) - \bar{B}_i(\cdot)$, which is not necessarily equal to zero, and was never explicitly declared to be zero by Neyman.

Consequently, under Neyman’s null, the expected mean residual sum of squares is greater than or equal to the expected mean treatment sum of squares, with equality holding if and only if for each block i , $B_i(t)$ is constant across treatments t . Alternatively, equality holds under Fisher’s sharp null. If one accepts Neyman’s logic regarding “unbiased tests” (discussed in Section 3.1), then the correct expressions for the expectations of mean squares suggest that the standard ANOVA F-test for RCBs has a Type I error bounded above by its nominal level.

A simple example makes this concrete. Suppose $N = T = 2$ and $\sigma_\varepsilon^2 = 0$, with the potential outcomes in Table 1. Note that $\bar{X}_{..}(1) = \bar{X}_{..}(2)$, so Neyman’s null is satisfied. We calculate $\mathbb{E}(S_0^2) = 215.875$, $\mathbb{E}(S_1^2) = 213.625$, and

$$\mathbb{E}(S_0^2) - \mathbb{E}(S_1^2) = 2.25 = \sum_{i=1}^2 \sum_{t=1}^2 \{B_i(t) - \bar{B}_i(\cdot)\}^2.$$

2.2 Randomized Complete Block Designs: After the Controversy

Neyman’s potential outcomes framework is similar to the “conceptual yield” framework developed by Kempthorne (1952, 1955). Certain features of these two are only cosmetically different: for example, Kempthorne [(1952), page 137] and later Hinkelmann

and Kempthorne [(2008), page 280] represent treatment indicators by δ_{ij}^k (with k denoting treatment level) and potential outcomes as y_{ijk} . As emphasized by a referee, using treatment indicators as random variables provides a mathematical foundation for the randomization theory of Fisher (1971), connecting potential outcomes with observed responses.

An important difference between Neyman and Kempthorne concerns the notion of technical errors. Hinkelmann and Kempthorne [(2008), page 161] make a distinction between experimental and observational errors, and include separate terms for each, allowing them to depend on treatment. Neyman effectively only considers their sum when defining technical errors, which may be a source of confusion. Of course, Neyman's results were for local field experiments, in which case he might not have considered it necessary to introduce observational errors arising from random sampling of experimental units from some larger population.

Kempthorne (1952) made an interesting comment relating to Fisher's sharp null, Neyman's null and Neyman's notation for technical errors:

If the experimenter is interested in the more fundamental research work, Fisher's null hypothesis is more satisfactory, for one should be interested in discovering the fact that treatments have different effects on different plots and in trying to explain why such differences exist. It is only in technological experiments designed to answer specific questions about a particular batch of materials which is later to be used for production of some sort that Neyman's null hypothesis appears satisfactory ... Neyman's hypothesis appears artificial in this respect, that a series of repetitions is envisaged, the experimental conditions remaining the same but the technical errors being different. (Kempthorne, 1952, page 133)

Furthermore, Kempthorne [(1952), pages 145–151] correctly noted (in agreement with our results in Section 2.5) that block-treatment interactions must be zero in order for $\mathbb{E}(S_0^2) = \mathbb{E}(S_1^2)$ under Neyman's null, also known as unbiasedness of a design in the Yates (1939) sense. As Kempthorne stated in a later article:

For the case of randomized blocks it is found that block treatment interactions must be zero in order that the design be unbiased in Yates's sense. ... It does not ap-

pear to be at all desirable to section the experimental material into ordinary randomized blocks, of ... highly different fertilities (or basal yields) because this procedure is likely to lead to block treatment interactions. (Kempthorne, 1955, page 964)

Additivity of treatment effects was not invoked by Neyman, and nonadditivity for RCBs was investigated later (Tukey, 1949; Kempthorne, 1955; Wilk, 1955; Mandel, 1961). Perhaps the most substantial work, in the same direction as Neyman, was done by Wilk (1955), who extended the results of Kempthorne [(1952), pages 145–151] for RCBs to the case of generalized randomized blocks. Wilk studied randomization moments of mean sums of squares, estimation of various finite-population estimands and Normal theory approximations for testing Fisher's sharp null and Neyman's null. He also distinguished between experimental error, that is, the failure of different experimental units treated alike to respond identically, and technical error, or limitations on experimental technique that prevent the exact reproduction of an applied treatment. To us, this use of notation confuses mathematical derivations and practical interpretations of symbols.

More importantly, although Wilk made assumptions on the potential outcomes (consequently not working in our more general setting), he attempted to justify them as physically relevant, as opposed to Neyman, who only made assumptions to facilitate calculations. For example, when translating Wilk's notation into Neyman's, we see that Wilk [(1955), page 72] explicitly considered the physical situation that, if the blocking of experimental units is successful, then the $\eta_{ij}(t) - \bar{\eta}_{ij}(\cdot)$ will be negligible for all i, j, t , whereas block-treatment interactions $B_i(t) - \bar{B}_i(\cdot)$ would be important, in the sense of varying with t . When units in a block are as homogeneous as possible with respect to background covariates, the assumption of no strict unit-treatment interactions becomes more plausible, similar to the plausibility of zero partial correlation among potential outcomes given all measured covariates. Accordingly, block-treatment interactions become more important. A referee made a similar comment, remarking that for agronomic experiments, it is reasonable to assume that the $\eta_{ij}(t)$ are negligible, whereas in situations such as medical experiments involving human subjects, this may no longer be true.

Wilk's explicit physical consideration is used to justify his assumption (stated without further explanation by Hinkelmann and Kempthorne [(2008), page 301] in their description of the general model for RCBs) that treatments react additively within a block but can react

nonadditively from block-to-block, that is,

$$\{X_{ij}(t) - \bar{X}_{ij}(\cdot)\} - \{\bar{X}_i(t) - \bar{X}_i(\cdot)\} = \eta_{ij}(t) - \bar{\eta}_{ij}(\cdot) = 0$$

for all i, j, t , even though

$$B_i(t) - \bar{B}_i(\cdot) \neq 0$$

for at least one pair (i, t) . Wilk [(1955), page 73] then stated that, if

$$\eta_{ij}(t) - \bar{\eta}_{ij}(\cdot) \neq 0$$

for at least one triple (i, j, t) , then the expected mean treatment sum of squares is not equal to the expected mean residual sum of squares under Neyman’s null. Hinkelmann and Kempthorne [(2008), page 301] when summarizing Wilk’s work, noted that the expected mean residual sum of squares for RCB designs contains the interaction between blocking and treatment factors, similar to our result.

2.3 Latin Square Designs: Theory

It was in his treatment of LSs that Neyman’s error substantially changes conclusions. We consider $T \times T$ LSs with rows and columns denoting levels of two blocking factors, for example, north-south and east-west. Our treatment indicators are

$$W_{ij}(t) = \begin{cases} 1, & \text{if the unit in row } i, \text{ column } j, \\ & \text{is assigned treatment } t, \\ 0, & \text{otherwise.} \end{cases}$$

Neyman specified the potential outcomes as

$$x_{ij}(t) = X_{ij}(t) + \varepsilon_{ij}(t),$$

with $X_{ij}(t) \in \mathbb{R}$ unknown constants representing the “mean yield” of the unit in cell (i, j) under treatment t , and $\varepsilon_{ij}(t) \sim [0, \sigma_\varepsilon^2]$ technical errors that are i.i.d. and independent of \mathbf{W} . Potential outcomes were then decomposed into

$$(2.2) \quad \begin{aligned} x_{ij}(t) &= \bar{X}_{..}(t) + R_i(t) + C_j(t) \\ &\quad + \eta_{ij}(t) + \varepsilon_{ij}(t), \end{aligned}$$

where

$$\begin{aligned} R_i(t) &= \bar{X}_i(t) - \bar{X}_{..}(t), \\ C_j(t) &= \bar{X}_{.j}(t) - \bar{X}_{..}(t), \\ \eta_{ij}(t) &= X_{ij}(t) - \bar{X}_i(t) - \bar{X}_{.j}(t) + \bar{X}_{..}(t). \end{aligned}$$

Similar to RCBs, Neyman described $R_i(t)$ and $C_j(t)$ as corrections for specific soil fertility of the i th row and j th column, respectively, and $\eta_{ij}(t)$ as the soil error for plot (i, j) under treatment t .

We define $\bar{x}_{..}^o(t)$ as the observed average yield for plots assigned treatment t ,

$$\bar{x}_{..}^o(t) = \frac{1}{T} \sum_{i=1}^T \sum_{j=1}^T W_{ij}(t)x_{ij}(t).$$

Neyman (1935) correctly noted that $\mathbb{E}\{\bar{x}_{..}^o(t) - \bar{x}_{..}^o(t')\} = \bar{X}_{..}(t) - \bar{X}_{..}(t')$ and that

$$\begin{aligned} \text{Var}\{\bar{x}_{..}^o(t) - \bar{x}_{..}^o(t')\} &= \frac{2\sigma_\varepsilon^2}{T} + \frac{\sigma_\eta^2(t) + \sigma_\eta^2(t')}{T - 1} \\ &\quad + \frac{2r(t, t')\sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')}}{(T - 1)^2}. \end{aligned}$$

Neyman then calculated the expected mean sums of squares. The mean residual and treatment sums of squares are defined as (resp.)

$$\begin{aligned} S_0^2 &= \frac{1}{(T - 1)(T - 2)} \\ &\quad \times \sum_{i=1}^T \sum_{j=1}^T \left\{ y_{ij} - \bar{y}_{i.} - \bar{y}_{.j} \right. \\ &\quad \left. - \sum_{t=1}^T W_{ij}(t)\bar{x}_{..}^o(t) + 2\bar{y}_{..} \right\}^2 \end{aligned}$$

and

$$S_1^2 = \frac{T}{T - 1} \sum_{t=1}^T \{\bar{x}_{..}^o(t) - \bar{y}_{..}\}^2,$$

with $y_{ij} = \sum_{t=1}^T W_{ij}(t)x_{ij}(t)$ the observed response of cell (i, j) , and

$$\begin{aligned} \bar{y}_{i.} &= \frac{1}{T} \sum_{j=1}^T y_{ij}, \\ \bar{y}_{.j} &= \frac{1}{T} \sum_{i=1}^T y_{ij}, \\ \bar{y}_{..} &= \frac{1}{T} \sum_{j=1}^T \bar{y}_{.j} = \frac{1}{T} \sum_{i=1}^T \bar{y}_{i.}. \end{aligned}$$

We prove in our appendix (Sabbaghi and Rubin, 2014) that the correct expectations are

$$\begin{aligned} \mathbb{E}(S_0^2) &= \sigma_\varepsilon^2 + \frac{T - 2}{(T - 1)^2} \sum_{t=1}^T \sigma_\eta^2(t) \\ &\quad + \frac{2}{(T - 1)^3} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')} \end{aligned}$$

$$+ \frac{1}{T(T-1)^2} \sum_{i=1}^T \sum_{j=1}^T \sum_{t=1}^T [\{R_i(t) - \bar{R}_i(\cdot)\}^2 + \{C_j(t) - \bar{C}_j(\cdot)\}^2]$$

and

$$\begin{aligned} \mathbb{E}(S_1^2) &= \sigma_\varepsilon^2 + \frac{1}{T-1} \sum_{t=1}^T \sigma_\eta^2(t) \\ &+ \frac{1}{(T-1)^3} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')} \\ &+ \frac{T}{T-1} \sum_{t=1}^T \{\bar{X}_{..}(t) - \bar{X}_{..}(\cdot)\}^2. \end{aligned}$$

Neyman [(1935), page 152] made a similar mistake as he did for RCBs, excluding

$$R_i(t) + C_j(t) - \bar{R}_i(\cdot) - \bar{C}_j(\cdot)$$

in a simplified expression for the term inside the brackets of S_0^2 in his equation (50). In effect, Neyman once again excluded corrections for soil fertility, as it is not necessarily true (nor stated explicitly) that $R_i(t)$ is constant in t for all rows i and that $C_j(t)$ is constant in t for all columns j . Sukhatme [(1935), page 167] made a similar mistake for the case when $\sigma_\eta^2(t)$ and $r(t, t')$ are not constant in t, t' .

After incorrectly calculating the expected mean residual sum of squares, Neyman stated that the expected mean residual sum of squares was less than or equal to the expected mean treatment sum of squares under Neyman’s null (Neyman, 1935, page 154), with equality only under special cases, such as Fisher’s sharp null. Based on this observation, Neyman conjectured that the standard ANOVA F-test for LSs is potentially invalid in the sense of having a higher Type I error than nominal, that is, rejecting more often than desired under Neyman’s null.

However, the expected mean residual sum of squares is not necessarily less than the expected mean treatment sum of squares under Neyman’s null. In fact, the inequality could go in either direction. We describe in Section 2.5 how the inequality depends on interactions between row/column blocking factors and the treatment.

Two examples of LSs with $T = 3$, $\sigma_\varepsilon^2 = 0$, and $\bar{X}_{..}(1) = \bar{X}_{..}(2) = \bar{X}_{..}(3)$ (i.e., Neyman’s null) demonstrate this fact. In Tables 2 and 3, each unit’s potential outcomes are represented by an ordered triple, with

TABLE 2
Table of potential outcomes for a LS with $\mathbb{E}(S_0^2) > \mathbb{E}(S_1^2)$

	Column 1	Column 2	Column 3
Row 1	(3, 10, 15)	(50, 30, 13)	(20, 20, 40)
Row 2	(10, 13, 50)	(20, 40, 3)	(30, 15, 20)
Row 3	(13, 3, 20)	(15, 20, 10)	(40, 50, 30)

the t th coordinate denoting the potential outcome under treatment t . For Table 2, $\mathbb{E}(S_0^2) = 252.07$, $\mathbb{E}(S_1^2) = 172.38$. From our formulae,

$$\begin{aligned} \mathbb{E}(S_0^2) - \mathbb{E}(S_1^2) &= -\frac{1}{(T-1)^2} \sum_{t=1}^T \sigma_\eta^2(t) \\ &+ \frac{1}{(T-1)^3} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')} \\ &+ \frac{1}{T(T-1)^2} \sum_{i=1}^T \sum_{j=1}^T \sum_{t=1}^T [\{R_i(t) - \bar{R}_i(\cdot)\}^2 \\ &\quad + \{C_j(t) - \bar{C}_j(\cdot)\}^2]. \end{aligned}$$

We verify by explicit randomization that the discrepancy $\mathbb{E}(S_0^2) - \mathbb{E}(S_1^2) = 79.69$ equals this expression, so that this is one LS for which the expected mean residual sum of squares is greater than the expected mean treatment sum of squares. The inequality is in the other direction for Table 3, with $\mathbb{E}(S_0^2) = 4.96$, $\mathbb{E}(S_1^2) = 6.77$.

2.4 Latin Square Designs: After the Controversy

As with RCBs, no additivity assumption is made on the potential outcomes for LSs. Nonadditivity for LSs has been further studied in the literature (Gourlay, 1955b; Tukey, 1955; Rojas, 1973). Kempthorne recognized the issue of interactions between row/column blocking factors and the treatment factor in a LS (discussed in the next section):

TABLE 3
Table of potential outcomes for a LS with $\mathbb{E}(S_0^2) < \mathbb{E}(S_1^2)$

	Column 1	Column 2	Column 3
Row 1	(7, 4, 8)	(5, 9, 4)	(6, 6, 5)
Row 2	(8, 5, 6)	(3, 3, 3)	(2, 2, 7)
Row 3	(1, 8, 2)	(4, 7, 9)	(9, 1, 1)

It is clear that, if there are row-treatment or column-treatment interactions, these will enter into the error mean square but not into the treatment mean square. The situation is entirely analogous to that of randomized blocks in that block-treatment interactions enter the error mean square but not the treatment mean square. (Kempthorne, 1952, page 195)

Kempthorne [(1952), page 204] continued by noting a defect of large LSs, namely, that there are more opportunities for row/column interactions with treatments.

A substantial investigation in the spirit of Neyman was performed by Wilk and Kempthorne (1957), and is briefly summarized by Hinkelmann and Kempthorne [(2008), page 387]. Wilk and Kempthorne [(1957), page 224] adopt the same specification of potential outcomes as Neyman (1935), allowing technical errors to differ based on treatment level k :

$$y_{ijk} = Y_{ijk} + \varepsilon_{ijk}.$$

One difference that makes the conceptual yield framework of Wilk and Kempthorne more general is that they consider randomly sampling rows, columns and treatments from some larger population. In any case, Wilk and Kempthorne [(1957), page 227] reach the reverse conclusion as Neyman, stating that, usually, the expected mean residual sum of squares is larger than the expected mean treatment sum of squares. Wilk and Kempthorne [(1957), page 227] explain this difference and the fact that Neyman did not recognize interactions between row/column blocking factors and the treatments, by noting that Neyman [(1935), page 145] made additional homogeneity assumptions. However, Neyman's assumptions were invoked solely to facilitate calculations and had no physical justifications.

Our results are in agreement with a summary of their work in Table 3 from Wilk and Kempthorne [(1957), page 226]. Thus, it appears that Wilk and Kempthorne do not seriously consider the possibility that the inequality could go in the direction Neyman claimed. In fact, Hinkelmann and Kempthorne [(2008), page 387] when summarizing this paper, explicitly state that the expected mean residual sum of squares is larger than the expected mean treatment sum of squares under Neyman's null. A possible explanation can be found in the sixth remark on page 227, where Wilk and Kempthorne discuss how the standard approach to designing LSs may likely result in interactions of row/column blocking factors with treatments.

As explained in our next section, the magnitudes of these interactions ultimately drive the direction of the inequality.

Cox (1958) built on the work of Wilk and Kempthorne, and provided a rather unique viewpoint on this entire problem. After first summarizing Wilk and Kempthorne's results by stating that it is usually the case that the expected mean residual sum of squares is larger than the expected mean treatment sum of squares, Cox then considered the practical importance of this difference of expectations, which he correctly recognized as being related to interactions between the treatment and blocking factors. Cox [(1958), page 73] raised the thought-provoking question of whether, for a LS, the practical scientific interest of the null

$$H_0: \mathbb{E}(S_0^2) = \mathbb{E}(S_1^2)$$

is comparable to, or greater than, Neyman's null, especially when the difference between these expected mean sums of squares is considered important. He concluded that testing Neyman's null when there is no unit-treatment additivity does not seem to be helpful:

... if substantial variations in treatment effect from unit to unit do occur, one's understanding of the experimental situation will be very incomplete until the basis of this variation is discovered and any extension of the conclusions to a general set of experimental units will be hazardous. The mean treatment effect, averaged over all units in the experiment, or over the finite population of units from which they are randomly drawn, may in such cases not be too helpful. Particularly if appreciable systematic treatment-unit interactions are suspected, the experiment should be set out so these may be detected and explained. (Cox, 1958, page 73)

Cox [(2012), page 3] later argued that when this more realistic null is formulated, the biases described earlier disappear, and so do issues surrounding the LS. A related point for the LS design noted by Cox is the marginalization principle, in which models having nonzero interactions and zero main effects are not considered sensible [similar to the effect heredity principle (Wu and Hamada, 2009, page 173)]. Box (1984), when commenting on Cox (1984), provided an opposing view that makes such a principle context-dependent.

2.5 Block-Treatment Interactions and Expected Sums of Squares

Neyman excluded the following (respective) terms in $\mathbb{E}(S_0^2)$ for RCBs and LSs:

$$\begin{aligned} & \frac{1}{(N-1)(T-1)} \sum_{i=1}^N \sum_{t=1}^T \{B_i(t) - \bar{B}_i(\cdot)\}^2, \\ & \frac{1}{(T-1)^2} \sum_{i=1}^T \sum_{t=1}^T \{R_i(t) - \bar{R}_i(\cdot)\}^2 \\ & + \frac{1}{(T-1)^2} \sum_{j=1}^T \sum_{t=1}^T \{C_j(t) - \bar{C}_j(\cdot)\}^2. \end{aligned}$$

In each, we are adding squared differences between the fertility correction for a specific combination of block and treatment levels, and the average (over treatments) fertility correction for the same block level. For the LS, this is decomposed as a sum over the row and a sum over the column blocking factors.

Formally, these terms gauge whether, for each level of a blocking factor, the fertility corrections are constant over the treatments, and represent interactions between blocking factors and treatments. For RCBs, we have

$$B_i(t) - \bar{B}_i(\cdot) = \{\bar{X}_{i\cdot}(t) - \bar{X}_{i\cdot}(\cdot)\} - \{\bar{X}_{\cdot\cdot}(t) - \bar{X}_{\cdot\cdot}(\cdot)\},$$

which is the interaction between the i th block and the t th treatment in terms of potential outcomes. Similarly, we have for LSs that

$$R_i(t) - \bar{R}_i(\cdot) = \{\bar{X}_{i\cdot}(t) - \bar{X}_{i\cdot}(\cdot)\} - \{\bar{X}_{\cdot\cdot}(t) - \bar{X}_{\cdot\cdot}(\cdot)\},$$

$C_j(t) - \bar{C}_j(\cdot) = \{\bar{X}_{\cdot j}(t) - \bar{X}_{\cdot j}(\cdot)\} - \{\bar{X}_{\cdot\cdot}(t) - \bar{X}_{\cdot\cdot}(\cdot)\},$ which are the interactions between the i th row and t th treatment, and the j th column and the t th treatment, respectively, in terms of potential outcomes.

Intuitively, these interactions, which are functions of potential outcomes, should reside within the expectation of the mean residual sum of squares. Without invoking additivity on the potential outcomes, these interactions are not necessarily zero and, because we lack replications within blocks for either RCB or LS designs, we cannot form an interaction sum of squares from the observed data, so that the potential outcome interactions will instead be included in the expectation of the mean residual sum of squares (Fisher, 1971, Chapters IV, V). In contrast, for randomized block designs that include replications within each block, this interaction term is no longer present in the expected mean residual sum of squares.

To better understand the expected mean sums of squares for LSs, consider their difference under Neyman's simplifying assumption that $\sigma_\eta^2(t)$ and $r(t, t')$ are constant, so that $\sigma_\eta^2(t) = \sigma_\eta^2$ and $r(t, t') = r$ for all treatments t, t' . Then the difference between $\mathbb{E}(S_0^2)$ and $\mathbb{E}(S_1^2)$ under Neyman's null is

$$\begin{aligned} & \sum_{i=1}^T \sum_{t=1}^T \{R_i(t) - \bar{R}_i(\cdot)\}^2 \\ & + \sum_{j=1}^T \sum_{t=1}^T \{C_j(t) - \bar{C}_j(\cdot)\}^2 - T\sigma_\eta^2(1-r), \end{aligned}$$

and this expression, in some sense, measures the difference between row/column interactions with treatment and the variance of the potential outcome residual terms (scaled by the number of treatments, T , times one minus the correlation between potential outcome residual terms for different pairs of treatments). Note that $0 \leq 1-r \leq 2$, so $0 \leq T\sigma_\eta^2(1-r) \leq 2T\sigma_\eta^2$.

To interpret the difference in expectations for the general case, first note that

$$\begin{aligned} & \sum_{i=1}^T \sum_{j=1}^T \bar{\eta}_{ij}(\cdot)^2 \geq 0 \Rightarrow \\ & \sum_{t=1}^T \sigma_\eta^2(t) \geq - \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t)\sigma_\eta^2(t')}. \end{aligned}$$

As such, $\mathbb{E}(S_0^2) - \mathbb{E}(S_1^2)$ under Neyman's null is bounded from below by

$$\begin{aligned} & \frac{1}{(T-1)^2} \sum_{i=1}^T \sum_{t=1}^T \{R_i(t) - \bar{R}_i(\cdot)\}^2 \\ & + \frac{1}{(T-1)^2} \sum_{j=1}^T \sum_{t=1}^T \{C_j(t) - \bar{C}_j(\cdot)\}^2 \\ & - \frac{T}{(T-1)^3} \sum_{t=1}^T \sigma_\eta^2(t), \end{aligned}$$

so that, if

$$\begin{aligned} & \sum_{i=1}^T \sum_{t=1}^T \{R_i(t) - \bar{R}_i(\cdot)\}^2 + \sum_{j=1}^T \sum_{t=1}^T \{C_j(t) - \bar{C}_j(\cdot)\}^2 \\ & - \frac{T}{T-1} \sum_{t=1}^T \sigma_\eta^2(t) \geq 0, \end{aligned}$$

then $\mathbb{E}(S_0^2) \geq \mathbb{E}(S_1^2)$. Even in the most general case for LSs, $\mathbb{E}(S_0^2) - \mathbb{E}(S_1^2)$ can still be interpreted as a com-

parison between row/column interactions with treatment and the (scaled) sum of variances of residual potential outcomes $\eta_{ij}(t)$.

In the context of an agricultural experiment, we obtain a more meaningful interpretation for this difference. Latin squares are implemented to block on fertility gradients in two direction (Neyman, 1935; Fisher, 1971, Chapter V; Hinkelmann and Kempthorne, 2008, Chapter 10). If the variability of specific soil fertility corrections across rows and columns (i.e., interactions between rows/columns and treatments) are substantially larger than the residual variability of the potential outcomes [i.e., the variability of the $\eta_{ij}(t)$], then $\mathbb{E}(S_0^2) - \mathbb{E}(S_1^2)$ is larger than zero. An example was given in Table 2, where

$$\begin{aligned} & \sum_{i=1}^T \sum_{t=1}^T \{R_i(t) - \bar{R}_i(\cdot)\}^2 + \sum_{j=1}^T \sum_{t=1}^T \{C_j(t) - \bar{C}_j(\cdot)\}^2 \\ &= 569.93, \\ & - \sum_{t=1}^T \sigma_\eta^2(t) = -313.56, \\ & \frac{1}{T-1} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t) \sigma_\eta^2(t')} = 62.41. \end{aligned}$$

The interaction is nearly twice the variability of the residual potential outcomes, and so the difference $\mathbb{E}(S_0^2) - \mathbb{E}(S_1^2)$ is greater than zero. For Table 3,

$$\begin{aligned} & \sum_{i=1}^T \sum_{t=1}^T \{R_i(t) - \bar{R}_i(\cdot)\}^2 + \sum_{j=1}^T \sum_{t=1}^T \{C_j(t) - \bar{C}_j(\cdot)\}^2 \\ &= 9.48, \\ & - \sum_{t=1}^T \sigma_\eta^2(t) = -14.59, \\ & \frac{1}{T-1} \sum_{t \neq t'} r(t, t') \sqrt{\sigma_\eta^2(t) \sigma_\eta^2(t')} = -2.11, \end{aligned}$$

and the variance of the residuals completely dominates the interaction.

Hence, $\mathbb{E}(S_0^2) > \mathbb{E}(S_1^2)$ in the presence of a strong fertility gradient, with the interaction between row/column blocking factors and treatment greater than the variance of the residual potential outcomes or, alternatively, when the unit-treatment interactions are negligible. Similarly, $\mathbb{E}(S_0^2) < \mathbb{E}(S_1^2)$ in cases where no strong interaction exists between row/column blocking factors and the treatment when compared to the variability of the residual potential outcomes or, alternatively,

when the unit-treatment interactions are substantial. It is important to recognize that such important interactions can never be assessed without replication, which is not available in the original LS design.

3. CONTROVERSIAL CONNECTIONS

3.1 Connecting Expected Mean Sums of Squares with Type I Error

Neyman (1935) calculated expectations of mean sums of squares to argue that the standard ANOVA F-test for RCB designs is valid and the test for LS designs is invalid when testing Neyman’s null: a test was said to be “unbiased” if $\mathbb{E}(S_0^2) = \mathbb{E}(S_1^2)$ under Neyman’s null (Neyman, 1935, page 144). The reasoning behind this definition is not discussed at all and, given our current understanding of hypothesis testing, seems somewhat crude. After all, to determine whether a particular testing procedure is “biased,” one typically calculates the probability of rejecting a true null hypothesis, which generally depends on the test statistic’s distribution, not just its expectation.

To better understand the logic potentially driving Neyman’s reasoning, it is useful to review the testing of Fisher’s sharp null. A randomization test that uses any a priori defined test statistic automatically yields the correct Type I error under Fisher’s sharp null and regularity conditions on the potential outcomes and number of randomizations. Furthermore, when using the statistic $F = S_1^2/S_0^2$, this randomization distribution is well approximated by the F-distribution, for both RCB and LS designs. Welch (1937) calculated the first two moments of

$$(3.1) \quad \frac{df_1 S_1^2}{df_1 S_1^2 + df_0 S_0^2} = \frac{df_1 F}{df_1 F + df_0},$$

where df_1 denotes the degrees of freedom for treatment sum of squares, and df_0 the degrees of freedom for residual sum of squares. Pitman (1938) calculated the first four moments of this statistic. For both RCB and LS designs, $df_1 S_1^2 + df_0 S_0^2$ remains constant over the randomizations under Fisher’s sharp null, making calculation of the moments of (3.1) much easier than of F itself. Furthermore, under regularity conditions on the potential outcomes, it was shown that these moments are approximately equal to the corresponding moments of a Beta distribution. In this respect, the standard ANOVA F-test that uses rejection cutoffs based on the F-distribution has approximately the correct Type I error, and the F-distribution can be viewed

as a simple approximation to the randomization distribution of the F-test statistic when testing Fisher's sharp null (Kempthorne, 1952, pages 172, 193). Indeed, as stated by Wilk [(1955), page 77] the amount of computation to perform a randomization test could be prohibitive, and statisticians had little recourse except to use such approximations. Kempthorne made a similar remark:

It should be realized that the analysis of variance test with the F distribution has a fair basis apart from normal law theory and is probably in most cases a good approximation to the randomization analysis of variance test, which is a nonparametric test. (Kempthorne, 1955, page 966)

Kempthorne earlier stated that for LSs:

The randomization test for the Latin Square or for any randomized design is entirely valid in the sense of controlling Type I errors, but the approximation to this test by the F-distribution when there is nonadditivity is apparently completely unknown. (Kempthorne, 1955, page 965)

As Neyman did not invoke additivity or any other regularity conditions on the potential outcomes, the reasoning outlined in the previous paragraph that establishes the F-distribution as an approximation to the true distribution of the F-test statistic is no longer valid when testing Neyman's null: for example, $df_1 S_1^2 + df_0 S_0^2$ is generally no longer constant over the randomizations, and calculating moments of equation (3.1) generally becomes very difficult. Wilk [(1955), page 79] realized this, remarking that the standard ANOVA F-test for testing Neyman's null in RCBs depends on the assumption that block-treatment interactions are zero. Wilk and Kempthorne [(1957), page 228] also stated that the effect of nonadditivity on the Type I error of the standard ANOVA F-test for a LS is unknown.

Bearing these facts in mind, a comparison of expected mean residual and treatment sums of squares could be viewed as a crude way of assessing whether the Type I error is correct when testing Neyman's null using the standard ANOVA F-test. Neyman (1935) himself may have realized this:

...in the case of the Randomized Blocks the z test may be considered as unbiased in the sense that the expectations of S_0^2 and S_1^2 have a common value ... On the other hand,

by the arrangement in Latin Square the expectation of S_1^2 is equal to $\frac{1}{2}n'\sigma_d^2$, while that of S_0^2 is generally smaller. This suggests, although it does not prove, that by the Latin Square arrangement the z test may have the tendency to detect differentiation when it does not exist. (Neyman, 1935, page 144)

After calculating expected mean sums of squares for RCBs, Neyman states that

If there is no differentiation among the $X_{..}(k)$, then $\mathbb{E}(S_1^2) = \mathbb{E}(S_0^2)$, and we see that the test of significance usually applied is unbiased in the sense that if there is no differentiation, then the values of S_1^2 and S_0^2 must be approximately equal. This, of course, does not prove the validity of Fisher's z test. (Neyman, 1935, page 150)

Furthermore, Neyman states that for LSs:

We conclude, therefore, that at present there is no theoretical justification for the belief that the z test is valid in the case of the arrangement by the Latin Square: not only is there the difficulty connected with the non-normality of the distribution of the η 's, but also the functions which are usually considered as unbiased estimates of the same variance have generally different expectations. This may (though not necessarily so) cause a tendency to state significant differentiation when this, in fact, does not exist. ... These, of course, are purely theoretical conclusions, and I am personally inclined to think that from the practical point of view the existing bias will prove to be negligible. (Neyman, 1935, page 154)

This same consideration of expected mean sums of squares for hypothesis testing continues in the present literature on experimental design:

It is the form of the expected mean squares, $\mathbb{E}[\text{MS}(i)]$, which determines, for example, how tests of hypotheses are performed and how error variances are estimated. (Hinkelmann and Kempthorne, 2008, page 37)

Also:

In this case, MS(E) is on average larger than MS(T) under the hypothesis of no

treatment effects and hence the usual F-test will lead to fewer significant results. In this case the LSD is not an unbiased design. (Hinkelmann and Kempthorne, 2008, page 387)

It is interesting to note that the specific justification for this last statement was never made, nor was any attempt made to calculate explicitly the Type I error. Even more interesting is how these statements contradict Kempthorne’s earlier position on the connection between expected mean sums of squares and hypothesis testing (e.g., as given by Kempthorne [(1952), page 149]), for example:

To establish the property of unbiasedness for this design it is . . . necessary to show that the expectation over randomizations of the error mean square resulting from this model is equal to the mean square among all observations in the absence of treatment effects. . . it should perhaps be noted that this property has no intrinsic relation to the concept of unbiasedness of a test. (Kempthorne, 1955, page 956)

Wilk and Kempthorne (1957) hold this same position, stating that:

We accept the view that tests of significance are evaluatory procedures leading to assessments of strength of evidence against particular hypotheses, while tests of hypotheses are decision devices. We are here concerned with the former, and in this connection it should be noted that (a) the expectations of mean squares are in some degree irrelevant to the exact (permutation) test of significance of the null hypothesis that the treatments are identical. (Wilk and Kempthorne, 1957, page 228)

3.2 Concrete Calculations

From Section 2.1, the F-test for RCBs is generally biased in one direction under Neyman’s conception of an unbiased test, potentially leading to fewer rejections under Neyman’s null. Furthermore, because we do not make any assumptions about the difference between the interactions of rows/columns with treatment and the residual variances in Section 2.3, we actually cannot claim that the F-test for LSs is biased in any one direction. A more rigorous justification for the “unbiasedness” of the F-test for either design would compare the actual distribution of the F-test statistic to the

TABLE 4
Table of potential outcomes for a 4 × 4 LS, with $\mathbb{E}(S_0^2) = \mathbb{E}(S_1^2)$

	Column 1	Column 2	Column 3	Column 4
Row 1	(1, 1, 1, 1)	(0, 0, 0, 0)	(0, 0, 0, 0)	(0, 0, 0, 0)
Row 2	(0, 0, 0, 0)	(1, 1, 1, 1)	(0, 0, 0, 0)	(0, 0, 0, 0)
Row 3	(0, 0, 0, 0)	(0, 0, 0, 0)	(0, 0, 0, 0)	(0, 0, 0, 0)
Row 4	(0, 0, 0, 0)	(0, 0, 0, 0)	(0, 0, 0, 0)	(0, 0, 0, 0)

associated F-distribution. By determining whether the distribution of $F = S_1^2/S_0^2$ is adequately approximated by the F-distribution under Neyman’s null, one would be able to conclude whether the Type I error is approximately as advertised.

We performed this comparison for various RCBs and LSs, and observed that Neyman’s definition of unbiased tests fails. In particular, we can generate infinitely many RCBs and LSs such that (1) Neyman’s null holds, (2) there is no interaction between blocking factor(s) and treatment, (3) the expected mean residual sum of squares equals the expected mean treatment sum of squares, and yet there is zero probability of rejecting Neyman’s null when the rejection rule is based on a comparison of the observed value of S_1^2/S_0^2 with $\alpha = 0.05$ cutoffs used in the standard ANOVA F-test.

For simplicity, consider the case with no technical errors. One simple example of a 4 × 4 LS, with $\sigma_\eta^2(t), r(t, t')$ constant, $\mathbb{E}(S_0^2) = \mathbb{E}(S_1^2)$, and no interactions between row/column blocking factors and the treatment, is presented in Table 4. Now $F_{3,6,0.95} = 4.76$ and, as we have all potential outcomes, we can calculate the probability that $S_1^2 > kS_0^2$ for any positive number k over the distribution of S_1^2 and S_0^2 . These probabilities are given in the left of Figure 1, which also displays probabilities that $F_{3,6} > k$; probabilities from the randomization distribution of S_1^2/S_0^2 are plotted as dots, and probabilities for the $F_{3,6}$ distribution as dashes. A horizontal line at 0.05 and a vertical line at 4.76 were drawn to illustrate conclusions obtained at the 0.05 significance level. The probability of rejecting Neyman’s null when using the standard ANOVA F-test is zero.

The crucial factor here is the structure of the potential outcomes. Fisher’s sharp null holds, so the total sum of squares, and the sum of squares for row and column blocking factors, remain constant over the randomization. Furthermore, the treatment sum of squares takes only two values, corresponding to whether cells (1, 1) and (2, 2) receive the same treatment or not, and similarly the residual sum of squares takes only two

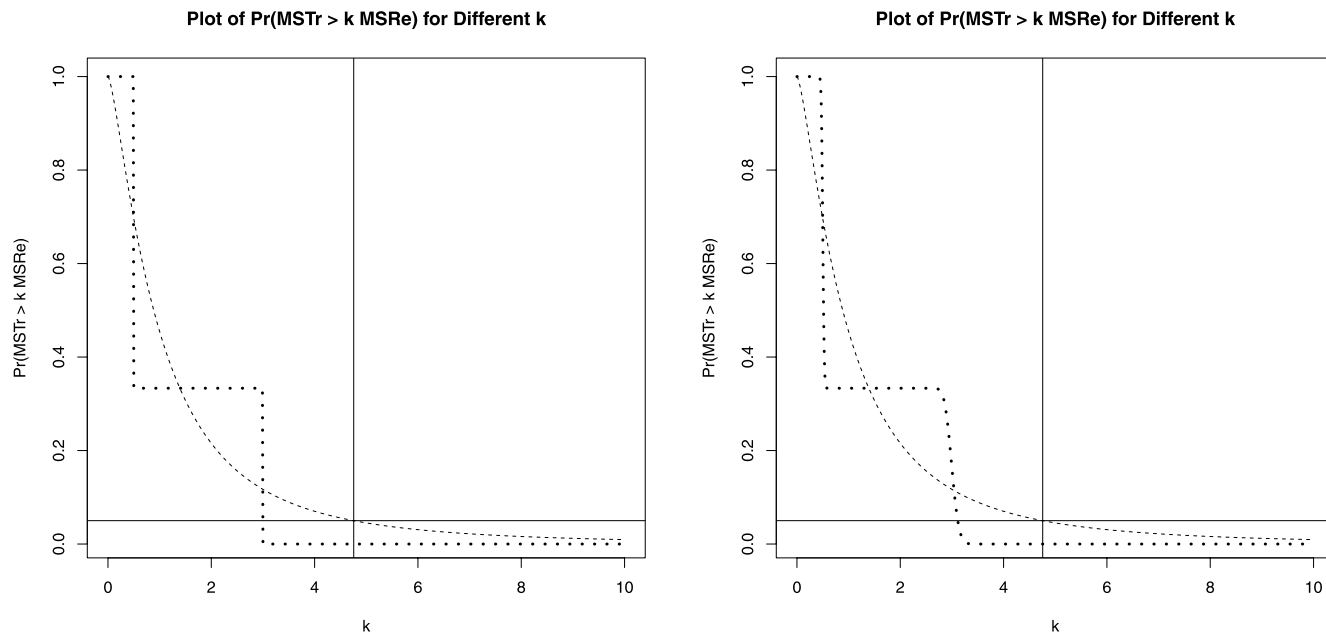


FIG. 1. Comparison of the distributions of S_1^2/S_0^2 and $F_{3,6}$ for Table 4; the distribution of S_1^2/S_0^2 is represented by dots and that of $F_{3,6}$ by dashes. The figure on the left is for the case with no technical errors, and the figure on the right is for technical errors with $\sigma_\varepsilon = 0.01$.

values. Hence, the F-test statistic takes only two possible values, so that cutoffs given by consideration of the F-distribution will not yield approximately correct Type I errors for testing Neyman's null.

Inclusion of technical errors does not change our general conclusion. Suppose technical errors are Normally distributed with $\sigma_\varepsilon = 0.01$. The corresponding figure for the LS in Table 4 is displayed in the right of Figure 1. We generated this figure by simulation: we first drew $\varepsilon_{ij}(t)$, then performed the randomizations to generate the distribution of S_1^2 and S_0^2 for that specific draw of technical errors, and finally repeated this process 2000 times to estimate the probabilities.

4. CONTROVERSIAL CONSEQUENCES AND CONCLUSIONS

4.1 Consequences

The most immediate consequence of this entire controversy was the resulting hostile relationship between Neyman and Fisher for essentially the remainder of their careers, with each seeking to undermine the other. For example, Neyman was slightly critical in a discussion of a paper presented by Yates (1935) on factorial designs. Box [(1978), page 265] claimed that Neyman wanted to demonstrate his superiority by finding flaws in Fisher's work at this meeting. Reid [(1982), page 126] described an interesting encounter between

Neyman and Fisher, taking place in Neyman's room at University College London one week after this discussion. Fisher demanded that Neyman only use Fisher's books when lecturing on statistics at the university. When Neyman refused to do so, Fisher openly declared that he would oppose Neyman in all his capacities, and banged the door when he left the room.

These skirmishes continued for some time (Reid, 1982, pages 143, 169, 183–184, 223–226, 256–257). Neyman appears to have attempted some type of reconciliation, inviting Fisher to lecture at Berkeley (Reid, 1982, page 222), and generally became more conciliatory toward Fisher and his contributions to statistics (Neyman, 1976; Reid, 1982, page 45). In any case, these passages suggest an indirect consequence of this controversy: Neyman's decision to depart for America, where he created a world-class center for statistics at the University of California Berkeley (Reid, 1982, page 239), established a prominent series of symposia (Reid, 1982, pages 197–198), and helped to nurture, through his leadership, the American Statistical Association and Institute of Mathematical Statistics (Reid, 1982, page 218).

Fienberg and Tanur (1996) suggest that this break in the professional relationship between Neyman and Fisher may have led to a sharper division between the fields of sample surveys and experimental design:

Because of the bitterness that grew out of this dispute ... Fisher and Neyman were never able to bring their ideas together and benefit from the fruitful interaction that would likely have occurred had they done so. And in the aftermath, Neyman staked out intellectual responsibility for sampling while Fisher did the same for experimentation. It was in part because of this rift between Fisher and Neyman that the fields of sample surveys and experimentation drifted apart. (Fienberg and Tanur, 1996, page 238)

Cox (2012) makes the interesting remark that more effort was devoted to issues in randomization following this controversy:

The general issues of the role of randomization were further discussed in the next few years, mostly in *Biometrika*, with contributions from Student, Yates, Neyman and Pearson, and Jeffreys. With the exception of Student's contribution, which emphasized the role of randomization in escaping biases arising from personal judgement, the discussion focused largely on error estimation. (Cox, 2012, page 3)

Another consequence was undue emphasis on linear models for analysis of experimental data. As stated by Gourlay [(1955a), page 228] Neyman's work in 1935 led to increased attention on models (for observed data) that formed the basis of statistical analyses such as ANOVA. Eisenhart (1947), for example, explicitly laid out the four standard assumptions used to justify ANOVA, and noted the importance of additivity. Immediately following this article, Cochran (1947) explored the consequences for an analysis when additivity (and the other assumptions) were not satisfied, and Bartlett (1947) discussed various transformations of the data that make additivity more plausible for ANOVA.

Accordingly, past and present books on experimental design tend to invoke additive models when testing Neyman's null using the standard ANOVA F-test, an assumption that automatically yields a test of Fisher's sharp null (Kempthorne, 1952, Chapters 8, 9, 10; Hinkelmann and Kempthorne, 2008, Chapters 9, 10). When additivity is believed not to hold, one is generally advised to search for a transformation that yields an additive structure on the potential outcomes. For example, Wilk and Kempthorne [(1957), page 229]

make the strong recommendation to transform to a scale where additivity more nearly obtains for purposes of estimation. This also reflects the motivation behind the famous Box and Cox (1964) family of transformations.

Of course, greater emphasis on linear models with Normal errors for observed potential outcomes can generate doubts as to whether randomization is necessary in experimental design. What is then lost is the fact that explicit randomization, as extolled by Fisher, provides the scientist with internally consistent statistical inferences that require no standard modeling assumptions, such as those required for linear regression. It is ironic that many textbooks on experimental design focus solely on Normal theory linear models, without realizing that such models were originally motivated as approximations for randomization inference.

Additivity has even been considered an essential assumption for interpreting estimands. For example, Cox [(1958), pages 16–17] states that the average difference in observed outcomes for two treatments estimates the difference in average potential outcomes for the two treatments in the finite population, but that this estimand of interest is "... *rather an artificial quantity*" if additivity does not hold on the potential outcomes. Perhaps Kempthorne [(1952), page 136] can best justify this statement with the specific example where, for each experimental unit, the square root of the potential outcome under treatment is 5 more than the square root of the potential outcome under control. If one experimenter has three experimental units with control potential outcomes equal to 25, 64 and 100, then the effect of the treatment on the raw measurement scale would range from 75 to 125. However, another experimenter working with units having control potential outcomes ranging from 9 to 16 would have treatment effects ranging from 55 to 65 on the raw scale. As Kempthorne states:

Under these circumstances both experimenters will agree only if they state their results in terms of effects on the square root of the observation. It is desirable then to express effects on a scale of measurement such that they are exactly additive. (Kempthorne, 1952, page 136)

Thus, Kempthorne's justification for additivity is that it enables externally consistent conclusions to be drawn from a particular analysis, that is, two experimenters

working with different samples from the same population will reach the same conclusion on the treatment effect. One could also interpret this as suggesting that experimenters should model the potential outcomes, with additive treatment effects being one simple model for an analysis.

Kempthorne continues to state that:

Such a procedure has its defects, for experimenters prefer to state effects on a scale of measurement that is used as a matter of custom or for convenience reasons. It is probably difficult, for instance, to communicate to a farmer the meaning of the statement that a certain dose of an insecticide reduces the square root of the number of corn borers. A statement on the effect of number of corn borers can be made but is more complex. These difficulties are not, however, in the realm of the experimenter. He should examine his data on a scale of measurement which is such that treatment effects are additive. The real difficulty, in general, is to determine the scale of measurement that has the desired property. (Kempthorne, 1952, page 136)

We again read in this quote the perceived importance of additivity that helped motivate the Box and Cox (1964) family of transformations. We do not believe it is necessary to study treatment effects on an additive scale: it is arguably more important to have an internally consistent definition and statistical procedure for studying treatment effects before deciding on externally consistent considerations. In our opinion, an ultimate consequence of this controversy is that, by focusing almost solely on linear models, advances in experimental design have been seriously inhibited from their original, useful and liberating formulation involving potential outcomes.

4.2 Conclusions

The Neyman–Fisher controversy arose in part because Neyman sought to determine whether Fisher’s ANOVA F-test for RCBs and LSs would still be valid when testing Neyman’s more general null hypothesis. Unfortunately, Neyman’s calculations were incorrect. In fact, under Neyman’s conception of unbiased tests, the F-test for RCB designs potentially rejects at most at the nominal level, yet we could never know for any particular situation whether the F-test for LS designs would reject more often than nominal or not. Furthermore, Neyman’s definition of unbiased tests is too

crude, because expected mean sums of squares do not determine the Type I error of the F-test when testing Neyman’s null. Two of the greatest statisticians argued over incorrect calculations and inexact measures of unbiasedness for hypothesis tests, adding an ironic aspect to this controversy.

What is also ironic is that apparently no statistician deigned to check Neyman’s algebra or reasoning; the only discussant who suggested there was a mistake in Neyman’s algebra was Fisher, but he did not explicitly state that Neyman was missing interactions in both expected mean residual sums of squares. Sukhatme [(1935), pages 166, 167] recalculated the expected mean sums of squares in the general case where $\sigma_{\eta}^2(t)$ and $r(t, t')$ are not constant, and did not catch Neyman’s mistake. Sukhatme also performed sampling experiments for two examples of LSs to support Neyman’s claims. In both of Sukhatme’s examples, there is no interaction between row/column blocking factors and treatment, so that $\mathbb{E}(S_0^2) < \mathbb{E}(S_1^2)$. Neyman [(1935), page 175] then considered his algebra correct, because “... none of my critics have attempted to challenge it.”

Fisher never referenced Neyman (1935) in his book on experimental design and apparently ignored potential outcomes for many years (Rubin, 2005; Lehmann, 2011, page 59). Fisher’s avoidance of potential outcomes led him to make certain oversights in causal inference. In particular, as described by Rubin (2005), Fisher never bridged his work on experimental design and parametric modeling, and gave generally flawed advice on the analysis of covariance to adjust for post-treatment concomitants in randomized trials.

There is only one reference to Neyman (1935) by Hinkelmann and Kempthorne [(2008), page 387] and it was referred to as “... an interesting somewhat different discussion ...”. The standard accounts of Fisher and Neyman’s professional careers (Box, 1978; Reid, 1982) do not mention any further work being done on questions raised by Neyman (1935), although Kempthorne is quoted as saying:

The allusion to agriculture is quite unnecessary and the discussion is relevant to experimentation in any field of human enquiry. The discussion section ... is interesting because of the remarks of R. A. Fisher which are informative in some respects but in other respects exhibit Fisher at his very worst ... The judgement of the future will be, I believe, that Neyman’s views were in the correct direction. (Reid, 1982, page 123)

Even the recent account by Lehmann [(2011), Chapters 4, 5] does not mention any statistician addressing Neyman's claims or checking his algebra. In fact, Lehmann ends his discussion of this controversy by recounting the destruction of the physical models Neyman used to illustrate his thoughts on RCB and LS designs during his 1935 presentation, thought to have been perpetrated by Fisher in a fit of anger (Reid, 1982, page 124; Lehmann, 2011, Chapter 4).

We agree with Kempthorne's assessment that Neyman's views were in the correct direction in the following sense: by evaluating the frequency properties of statistics for both designs, one can see that the F-test is no longer precise without further assumptions on the potential outcomes. Such evaluations serve the important task of investigating the general properties of a design in a particular applied setting. The F-distribution is a useful approximation to the randomization distribution of the F-test statistic under Fisher's sharp null hypothesis and regularity conditions on the distribution of the potential outcomes or, alternatively, for testing Neyman's null under additivity (Welch, 1937; Pitman, 1938).

We also agree with Cox (1958) that, if block-treatment interactions are not negligible, then it is not particularly useful to test Neyman's null. More generally, we believe that one must think carefully about the type of null hypotheses one will test, and should be guided by an appropriate model on the potential outcomes. At one extreme, Fisher's sharp null hypothesis requires no model on the potential outcomes to test a reasonable, scientifically interesting null, with the reference distribution based solely on the randomization actually implemented during the experiment. To test Neyman's null, one either needs strong regularity conditions on the potential outcomes for standard procedures to work or one needs to think carefully to build and evaluate a model for the potential outcomes. In any case, one necessarily needs to make assumptions to assess more complicated null hypotheses, and it is important that assumptions on the potential outcomes are driven by actual science, routinely checked for their approximate validity, and not chosen based on necessary requirements for classical statistical procedures that have no real scientific merit.

Therefore, a better strategy than focusing on satisfying additivity to use the F-test for testing Neyman's null, we believe, is to introduce a Bayesian framework into the problem (Rubin, 1978). One can obtain a posterior predictive distribution for the estimand of interest (defined in terms of the potential outcomes) and

evaluate relevant Bayes' rules using the same criteria that Neyman and others have considered (e.g., consistency, coverage, Type I error) (Rubin, 1984). The Fisher randomization test can be viewed as a type of posterior predictive check (Rubin, 1984), and it can be more enlightening (as the example in Section 3.2 illustrates) to perform explicitly the Fisher randomization test for Fisher's sharp null, rather than using the F-distribution as an approximation when testing Neyman's null under additivity. When additivity may not hold, evaluating Bayes' rules motivated by the particular applied setting of a problem appears to be a more viable path to the solution of a specific problem than relying on classical statistical procedures that are imprecise without applied contexts.

ACKNOWLEDGMENTS

We are grateful to the Executive Editor, an Associate Editor and a referee for many valuable comments that improved this paper. The research of Arman Sabbaghi was supported by the United States National Science Foundation Graduate Research Fellowship under Grant No. DGE-1144152.

SUPPLEMENTARY MATERIAL

Supplementary materials for “Comments on the Neyman–Fisher Controversy and its Consequences” (DOI: [10.1214/13-STS454SUPP](https://doi.org/10.1214/13-STS454SUPP); .pdf). The supplementary material contains our reworking of Neyman's calculations, specifically expectations and variances of sample averages, and expectations of sums of squares for RCB and LS designs. These calculations form the basis of all results presented in this article. The supplementary material can be accessed via the following link: http://www.people.fas.harvard.edu/~sabbaghi/sabbaghi_rubin_supplement.pdf.

REFERENCES

- BARTLETT, M. S. (1947). The use of transformations. *Biometrics* **3** 39–52. [MR0020763](#)
- BOX, J. F. (1978). *R. A. Fisher: The Life of a Scientist*. Wiley, New York. [MR0500579](#)
- BOX, G. E. P. (1984). Discussion of paper by D.R. Cox. *International Statistical Review* **52** 26.
- BOX, G. E. P. and COX, D. R. (1964). An analysis of transformations (with discussion). *J. R. Stat. Soc. Ser. B Stat. Methodol.* **26** 211–252. [MR0192611](#)
- COCHRAN, W. G. (1947). Some consequences when the assumptions for the analysis of variance are not satisfied. *Biometrics* **3** 22–38. [MR0020762](#)

- COX, D. R. (1958). The interpretation of the effects of non-additivity in the Latin square. *Biometrika* **46** 69–73.
- COX, D. R. (1984). Interaction. *Internat. Statist. Rev.* **52** 1–31. [MR0967201](#)
- COX, D. R. (1958). *Planning of Experiments*, 1st ed. Wiley, New York.
- COX, D. R. (2012). Statistical causality: Some historical remarks. In *Causality: Statistical Perspectives and Applications* (C. Berzuini, P. Dawid and L. Bernardinelli, eds.) 1–5. Wiley, New York.
- EISENHART, C. (1947). The assumptions underlying the analysis of variance. *Biometrics* **3** 1–21. [MR0020761](#)
- FIENBERG, S. E. and TANUR, J. M. (1996). Reconsidering the fundamental contributions of Fisher and Neyman on experimentation and sampling. *International Statistical Review* **64** 237–253.
- FISHER, R. A. (1935). Comment on “Statistical problems in agricultural experimentation (with discussion).” *Suppl. J. Roy. Statist. Soc. Ser. B* **2** 154–157, 173.
- FISHER, R. A. (1971). *The Design of Experiments*, 9th ed. Hafner Publishing Company, New York.
- GOURLAY, N. (1955a). F-test bias for experimental designs in educational research. *Psychometrika* **20** 227–258.
- GOURLAY, N. (1955b). F-test bias for experimental designs of the latin square type. *Psychometrika* **20** 273–287.
- HINKELMANN, K. and KEMPTHORNE, O. (2008). *Design and Analysis of Experiments. Vol. 1: Introduction to Experimental Design*, 2nd ed. Wiley, Hoboken, NJ. [MR2363107](#)
- KEMPTHORNE, O. (1952). *The Design and Analysis of Experiments*. Wiley, New York. [MR0045368](#)
- KEMPTHORNE, O. (1955). The randomization theory of experimental inference. *J. Amer. Statist. Assoc.* **50** 946–967. [MR0071696](#)
- LEHMANN, E. L. (2011). *Fisher, Neyman, and the Creation of Classical Statistics*. Springer, New York. [MR2798202](#)
- MANDEL, J. (1961). Non-additivity in two-way analysis of variance. *J. Amer. Statist. Assoc.* **56** 878–888. [MR0131934](#)
- NEYMAN, J. (1935). Statistical problems in agricultural experimentation (with discussion). *Suppl. J. Roy. Statist. Soc. Ser. B* **2** 107–180.
- NEYMAN, J. (1976). Emergence of mathematical statistics. In *On the History of Statistics and Probability: Proceedings of a Symposium on the American Mathematical Heritage, to Celebrate the Bicentennial of the United States of America, Held at Southern Methodist University, May 27–29, 1974* (D. B. Owen, W. G. Cochran, H. O. Hartley and J. Neyman, eds.) 149–185. Dekker, New York.
- PITMAN, E. (1938). Significance tests which may be applied to samples from any populations: III. The Analysis of Variance Test. *Biometrika* **29** 322–335.
- REID, C. (1982). *Neyman—from Life*. Springer, New York. [MR0680939](#)
- ROJAS, B. (1973). On Tukey’s test of additivity. *Biometrics* **29** 45–52.
- RUBIN, D. B. (1978). Bayesian inference for causal effects: The role of randomization. *Ann. Statist.* **6** 34–58. [MR0472152](#)
- RUBIN, D. B. (1984). Bayesianly justifiable and relevant frequency calculations for the applied statistician. *Ann. Statist.* **12** 1151–1172. [MR0760681](#)
- RUBIN, D. B. (1990). Comment on J. Neyman and causal inference in experiments and observational studies: “On the application of probability theory to agricultural experiments. Essay on principles. Section 9” [*Ann. Agric. Sci.* **10** (1923) 1–51]. *Statist. Sci.* **5** 472–480. [MR1092987](#)
- RUBIN, D. B. (2005). Causal inference using potential outcomes: Design, modeling, decisions. *J. Amer. Statist. Assoc.* **100** 322–331. [MR2166071](#)
- SABBAGHI, A. and RUBIN, D. B. (2014) Supplement to “Comments on the Neyman–Fisher controversy and its consequences.” DOI:[10.1214/13-STS454SUPP](#).
- SPLAWA-NEYMAN, J. (1990). On the application of probability theory to agricultural experiments. Essay on principles. Section 9. *Statist. Sci.* **5** 465–472. [MR1092986](#)
- SUKHATME, P. (1935). Comment on “Statistical problems in agricultural experimentation (with discussion).” *Suppl. J. Roy. Statist. Soc. Ser. B* **2** 166–169.
- TUKEY, J. (1949). One degree of freedom for nonadditivity. *Biometrics* **5** 232–242.
- TUKEY, J. (1955). Query 113. *Biometrics* **11** 111–113.
- WELCH, B. (1937). On the z-test in randomized blocks and latin squares. *Biometrika* **29** 21–52.
- WILK, M. B. (1955). The randomization analysis of a generalized randomized block design. *Biometrika* **42** 70–79. [MR0068800](#)
- WILK, M. B. and KEMPTHORNE, O. (1957). Non-additivities in a Latin square design. *J. Amer. Statist. Assoc.* **52** 218–236. [MR0088137](#)
- WU, C. F. J. and HAMADA, M. S. (2009). *Experiments: Planning, Analysis, and Optimization*, 2nd ed. Wiley, Hoboken, NJ. [MR2583259](#)
- YATES, F. (1935). Complex experiments. *J. R. Stat. Soc. Ser. B Stat. Methodol.* **2** 181–247.
- YATES, F. (1939). The comparative advantages of systematic and randomized arrangements in the design of agricultural and biological experiments. *Biometrika* **30** 440–466.