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RANDOMIZATION PROCEDURES APPLIED TO ANALYSIS OF BALLISTIC DATA

MALCOLM S. TAYLOR
BARRY A. BODT

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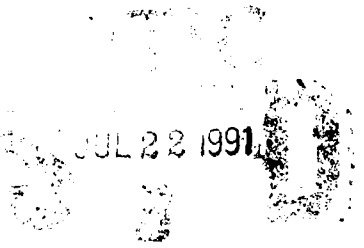

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Malcolm S. Taylor and Barry A. Bodt

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ATTN: SLCBR-SE-P
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Randomization procedures are shown to offer a viable approach to the analysis of ballistic data in a variety of settings. Distribution assumptions are unnecessary and, of even greater importance, random samples of data are not required. Small sample sizes, while never welcome, can also be accommodated. The price paid for randomization is increased computation, since every problem requires a tailored solution. Application of normal theory may only be valid to the extent that it approximates the results obtained from randomization. Reconciliation of some theoretical questions raised by the application of randomization procedures to ballistic data establishes this as an important and practical area of research.

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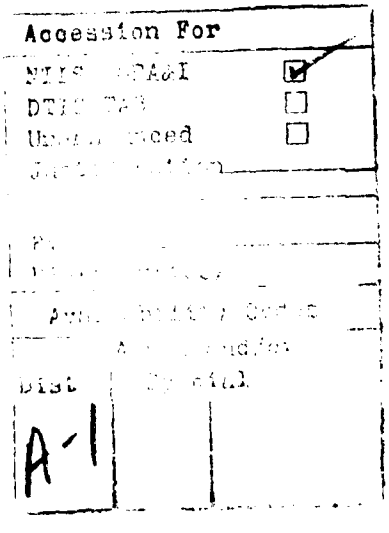

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1. Introduction

Investigators engaged in Army research and development are constantly confronted with the need to make comparisons; e.g., comparison of penetration measurements taken on different types of armor with penetration measurements for rolled homogeneous armor; comparison of data collected on penetrators with differing metalurgical properties with data for an existing penetrator; comparison of wear data for several styles of military boot with that for the current issue. All these situations share a common data structure: s sets of observations $Y_{i1}, \ldots, Y_{in_i}$, $i=1, \ldots, s$, taken on processes of like form, need to be compared against a reference set $Y_1, \ldots, Y_{n_0}$. Because this data structure is so prevalent in the social and health sciences the reference set $Y_1, \ldots, Y_{n_0}$ has come to be called a baseline, or control, and the experimental data sets $Y_{i1}, \ldots, Y_{in_i}$, $i=1, \ldots, s$, are called treatments. We will adopt this well-established terminology.

Suppose then that s treatments are to be compared to a control. The purpose of the comparison is to determine which treatment(s), if any, offer an improvement over the control. Often, improvement is reflected by the tendency of a treatment to take on larger values than the control. (The sense of the inequality can always be reversed, since min f(x) = max -f(x).) Standard techniques for analysis of these data, broadly described as multiple comparison procedures or simultaneous statistical inference, are developed under several basic premises, some of which may be difficult, if not impossible, to meet. Scrutiny of the underlying assumptions will be maintained in the sections to follow.

In Section 2 we consider a normal theory approach to the problem; in Section 3 nonparametric, or distribution-free, approaches are described; in Section 4 applications of randomization procedures to ballistic data are given; and in Section 5 we state our conclusions.

2. Normal theory approach

A normal theory approach to analysis of the data described in Section 1 requires three basic assumptions. The first is that the s treatment data sets and the control are all samples from normal populations. The second is that the samples are *random* samples, loosely meaning that they are highly representative of the populations from which they came; and third, a homogeneity of variance assumption is made – the population variances, although unknown, are identical.

In their aggregate, these three assumptions remove much of the difficulty from the problem. What remains is s+1 identical normal populations and a question of their location on the real line; i.e., what are their means? The random sample assumption is then invoked to facilitate estimation of the means.

When these assumptions can be justified, the normal theory approach is unsurpassed. When one or more of the assumptions cannot be justified the normal theory approach may be unwarranted, although arguments of *robustness* may be made by the statistician who has no alternative to offer.

The normality assumption still admits some flexibility, supported in part by the powerful Central Limit Theorem [2], and may sometimes serve as an adequate model for measurements on either discrete or continuous random variables. The random sample assumption is usually more unrealistic, since a random sample is difficult to obtain in the best circumstances – and ballistic data is rarely collected in the best circumstances. Homogeneity of variance is a recalcitrant assumption, difficult to supplant in this data framework if relaxed, and so it will be retained in what follows.

In addition to questionable compliance with the normal theory assumptions, ballistic data often presents further problems for the data analyst, a principal one being small sample size. Samples of size 3, 4, 5, ... , cannot be modeled with confidence by any distribution, unless the experimenter possesses concomitant information beyond the data itself.[1] For these, and other less compelling reasons, we will not pursue the well documented normal theory approach any further here.

3. Nonparametric approach

In a nonparametric approach to the problem, the normality assumption (and, indeed, any specific distribution assumption) is removed; the requirement that both treatment and control groups be random samples will be retained temporarily.

3.1 Rank tests

A direct nonparametric approach to the situation described in Section 1 is to repeatedly apply Wilcoxon rank-sum tests [8] for pairwise comparison of the s treatments with the control.[2] For each treatment i, the n_i observations on the i^{th} treatment and the n_0 control observations are combined and ranked by arranging their values from smallest to largest and assigning the integer k to the k^{th} smallest element, $k = 1, \ldots, n_0 + n_i$. If tied ranks occur, they are averaged. The i^{th} treatment is declared superior to the control if its rank-sum

$$R_i = \sum_{j=1}^{n_i} R(Y_{ij})$$

is sufficiently large; that is

$$R_i \geq c_i \qquad (3.1.1)$$

where the notation $R(Y_{ij})$ represents the rank assigned to the treatment observation Y_{ij}.

The procedure for comparison of treatment and control remains incomplete until the parameters c_i in (3.1.1) are specified. Under a null hypothesis H_i of no treatment effect for

1. For example, it has been historically established that small arms fire on a vertical target generates a pattern of impact locations which is modeled well with a bivariate normal distribution.
2. The Wilcoxon rank-sum test is also known as the Mann-Whitney test, since an equivalent procedure appears in the literature under both names.

the i^{th} treatment, c_i may be determined to satisfy the relation

$$P_{H_i}(R_i \geq c_i) = \alpha_i \qquad (3.1.2)$$

where α_i is an acceptable error of rejecting a valid null hypothesis. Since R_i can assume only $(n_0+n_i)!/n_0!n_i!$ distinct values (disregarding tied ranks), only a limited choice of values for α_i is available in (3.1.2). With this caveat, when $n_1 = \cdots = n_s$, at the discretion of the investigator, α_i, and hence c_i, may be chosen independently of i and (3.1.2) becomes

$$P_{H_i}(R_i \geq c) = \alpha \qquad \nabla\, i$$

The probabilities $\alpha_i(\alpha)$ may be thought of as measuring the frequency of false significance statements in a large number of comparisons between the i^{th} treatment and control when H_i of no treatment effect is true. This is sometimes referred to as the *error rate per comparison*. This suggests that for a fixed value of α, a large number of treatments for comparison (a large value for s) will likely lead to an erroneous rejection of at least one hypothesis H_i. It can be shown (Lehmann [8],p. 228) that this likelihood is maximum when all the hypotheses H_i are true. To accomodate this problem of multiplicity it is sometimes preferable to consider the s comparisons as a single entity and establish an *experimentwise error rate*, independent of s.

If the s comparisons are considered a single entity, and an accompanying global null hypothesis H of no treatment effect for any treatment is invoked, then evidence of at least one treatment effect suffices to invalidate H. This will occur when $\max R_i \geq c$ with probability α'; i.e.,

$$P_H(\max R_i \geq c) = \alpha' . \qquad (3.1.3)$$

The determination of c in expression (3.1.3) requires an involved computation, parameterized by the treatment sample sizes $n_1, \ldots, n_s$, the control group size n_0, the number of treatments considered, s, and the choice of α'(experimentwise error rate). The parameters may still be manageable; what is unmanageable is the probability structure relating the rank-sums R_i, i = 1, ... , s, which must be incorporated into the computation. The rank-sums R_i are determined using a common control, and are thus stochastically dependent in some fashion which usually cannot be determined. Limited computational results on (3.1.3), after several simplifying assumptions have been made, are provided by Steel [13] and Miller [10].

This approach has been followed to the point of intractable computation. As mentioned at the onset, distribution assumptions were removed, but the random sample assumption was retained. We now turn our attention to a second nonparametric technique which holds both theoretical and practical appeal, and which we advocate for a large class of ballistic data analyses.

3.2 Randomization tests

In a second nonparametric approach and the main topic of this paper, both distribution assumptions and the random sample requirement are removed. The penalty paid is that inferences are limited to the data actually considered; generalization to a conceptual population cannot be made, since the samples, which are no longer random, are not neccessarily representative of some larger population.

The penalty, however, is not a particularly heavy one. According to Edgington [3], "Few experiments in biology, education, medicine, psychology, or any other field use randomly selected subjects, and those that do usually concern populations so specific as to be of little interest. ... The population of interest to the experimenter is likely to be one that cannot be sampled randomly."

Randomization was apparently first suggested by Fisher [4] and extended to nonrandom samples by Pitman [11]. Although a conceptually simple and fundamentally sound procedure, it has not been fully utilized by applied statisticians. The reason for this is likely due to the fact that the procedure is computationally intensive compared to its parametric counterparts; however, those counterparts (t-test, F-test, etc.) may be valid only to the extent that they approximate the results given by the randomization procedure. The ideas behind a randomization test are best conveyed by example.

Suppose that we have the following measurements:

Table 1. Measurements on a Single Treatment vs. Control

control	treatment
7.01	7.72
7.37	7.62
6.81	7.29

A cursory examination of the data reveals that the treatment group contains most of the larger values. As a matter of fact, if the control and treatment data were combined and ranked as in the Wilcoxon test procedure detailed in Section 3.1, ranks 3, 5, and 6 would be assigned to treatment. Whether this is sufficient to assert the superiority of treatment over control remains to be established quantitatively.

We will choose as a null hypothesis that the treatment is ineffective and has no impact on the measurements recorded; in other words, the experimental units would have provided the same measurements regardless of whether they were assigned to the treatment or the control group.

Under the null hypothesis of no treatment effect or, equivalently, that wherever an experimental unit is assigned, its measurement goes with it, the labels "control" and "treatment" become completely arbitrary. Hence, any three measurements might be called control and the remaining three measurements treatment; i.e., assignment to treatment and

control may be *randomized*. The number of ways this assignment can be made is the number of ways three objects can be chosen from six distinguishable objects – ${}_6C_3=20$. In Table 2 is listed the sum of the treatment values corresponding to the twenty assignments. We have summed the actual measurements, rather than their ranks as was done for the Wilcoxon test, for technical reasons beyond the scope of this paper, although the interested reader is referred to Lehmann and Stein [9], and Hoeffding [5], for information on scoring.[3]

The important point is that, under the null hypothesis, the treatment sum actually observed, 22.63, could be exceeded by only one other assignment. This means that the probability of observing values as large, or larger than 22.63 when the treatment is ineffective as assumed, is 2/20 =.10 . This probability, .10, is alternately called the *p-value* or the *observed significance level* in statistical literature. Whether this probability of occurrence is sufficiently small to suggest rejection of the null hypothesis must be decided by the experimenter, and his tolerance for error.

Table 2. Sum of Treatment Values Corresponding to All Data Permutations

treatment sum	
21.11	21.92
21.19	22.00
21.44	22.02
21.47	22.10
21.54	22.15
21.67	22.28
21.72	22.35
21.80	22.38
21.82	**22.63**
21.90	22.71

The randomization test compares treatment data with control; specifically, it asks how unusual are the observed treatment values if there is no difference between treatment and control. The parametric analog of the randomization test in this instance is the t-test. The t-test assumes both treatment and control measurements are random samples from normal populations that differ at most by their location on the real line, and looks for difference in location (means). For these data, the t-test provides an observed significance level of .042,

3. As a technical fact and an historical aside, nonparametric rank tests, including the Wilcoxon test, were developed as a compromise between the then-computationally-burdensome randomization test and the alternative – a fully parameterized model.

suggesting that the treatment may indeed be effective. Compare this value to the randomization test's .10 . The t-test is inappropriate for these nonrandom data, but is commonly (mis)used.

4. Examples

Example 4.1

Table 3 contains measurements of spin rates of long rod penetrators taken by Rapacki [12]. The natural frequency of the penetrators is about 120 cycles per second (hz). Spin rates close to this value amplify the initial manufacturing imperfections and increase in-flight bending. To avoid this, different fin configurations were designed to reduce the spin rate below 120 hz.

Table 3. Comparison of Two Fin Redesigns with a Control

initial design	redesign1	redesign2
163.6	97.5	78.1
109.0	122.2	76.7
218.7	108.2	88.5
143.2		
169.5		

A plot of the data is usually a good beginning:

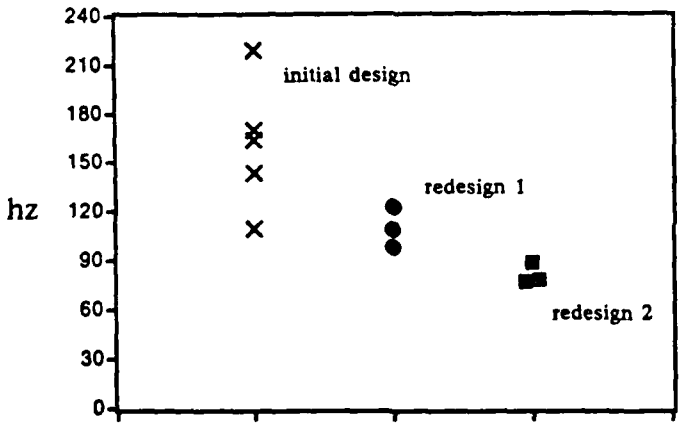

Figure 1. Plot of Spin Rate Data

It appears visually that the fin reconfiguration had the intended effect – reducing the spin rate. The role of statistics here is to see if this observation has quantitative support. Toward this end, we will randomize, choosing as a null hypothesis that the two redesigns provide no improvement over the initial design. If this is true, then the eleven observations may be arbitrarily partitioned into groups of 5, 3, and 3, and Table 3 is simply one of $11!/5!3!3! = 9240$ possible data configurations.

In this example we return more closely in format to the situation described in the Introduction. We also remain mindful of the two sources for error discussed in Section 3.1, error rate per comparison and experimentwise error rate. Experimentwise error rate will be controlled through the use of a multiple comparison procedure, a technique adjunct to the main topic of this report. An advanced reader may wish to consult Winer [15], Keppel [7], or Bancroft [1] for details and guidance in selecting an appropriate method. We will choose here a multiple comparison procedure known as *Fisher's modified least significant difference* (Winer [15], p.199) which has the desirable properties of being both nonparametric and applicable to unequal sample sizes.

Suppose we specify the largest tolerable experimentwise error rate to be $\alpha' = .05$ for multiple comparison of the two fin redesigns with the control. Adopting the obvious notation c, d1, d2 for control and redesign, we are interested in the comparisons c-d1 and c-d2. The observed significance level is determined for each of the pairwise comparisons following the randomization procedure just illustrated, where for each comparison only the data corresponding to the paired entities are permuted.[4] Each p-value is then multiplied by two (the number of comparisons) in accordance with Fisher's procedure to obtain an adjusted p-value. The p-values and adjusted p-values for comparison of c-d1 and c-d2 are given in Table 4.

Table 4. Multiple Comparison of Control and Two Treatments

comparison	c-d1	c-d2
p-value	.036	.018
adjusted p-value	.071	.036

The adjusted p-value, .036, corresponding to comparison of control and redesign2, falls well below the $\alpha' = .05$ value chosen for experimentwise error rate, and reflects a statistically significant difference between the items compared. Comparison of control and redesign1, with an adjusted p-value of .071, exceeds $\alpha' = .05$, and does not substantiate a claim of difference. These conclusions, now quantified, are consistent with the display in Figure 1.

4. We are testing here a *restricted null hypothesis*. It focuses attention on the comparisons of interest while easing the overall computational burden.

Another fundamental attribute of randomization tests that enhances their value is the ability to adapt to virtually any test statistic that seems appropriate. Thus far attention has been restricted to statistics sensitive to increased (decreased) treatment response when compared to a control; in the following example, we will consider instead relative dispersion.

Example 4.2

Table 5 lists measurements of horizontal displacement of centers of impact of three round shot groups from a reference aim point, reported by Webb, et.al. [15]. Each three round shot group was fired from a different 120 mm tank gun tube. The gun tubes were indexed (calibrated) by a standard method or by an alternative method based on the dynamic response of the tube during firing. The intent of indexing is to improve the precision of the delivered rounds. The question of whether or not dynamic indexing offers an improvement over standard indexing was pursued at the onset using normal theory analysis on the data shown in Table 5.

Table 5. Measurements of Center of Impact Displacement (mils)

standard	dynamic
0.14	0.96
0.22	0.28
0.40	0.20
0.15	0.74
0.11	0.43

The measure of precision chosen was the variance of the centers of impact. This measure combines both round-to-round and tube-to-tube variability. Webb, et.al. offer justification for claiming that round-to-round dispersion is approximately the same for both methods of indexing, leaving any observed differences in variation between the two methods attributable to variation between gun tubes.

The comparison of precision associated with the two methods of indexing can be made using any appropriate statistic; we chose

$$S^2_S / S^2_D \qquad \text{where } S^2 = \Sigma (x_i - \bar{x})^2/(n - 1), \tag{4.1}$$

the ratio of the sample variance for centers of impact among standard indexed tubes to that of dynamically indexed tubes. Large values of this ratio indicate smaller variability or increased precision of the dynamically indexed tubes relative to the standard; a ratio close to one represents roughly equivalent precision for the two methods. For the data displayed in Table 5, the ratio (4.1) was determined to be 0.13.

Any reasonable statistical procedure would fail to support the notion of improvement of dynamic over standard indexing based on this data set, but we will pursue a randomization approach in order to reinforce an important point made earlier. In compliance with the paradigm already outlined, and under a null hypothesis H_0 of no difference between the indexing procedures, the center of impact data are permuted between tube groups, providing 10!/5!5! = 252 arrangements for which the statistic (4.1) is evaluated. The observed significance level was determined to be

$$P_{H_0}(S^2_S/S^2_D \geq 0.13) = 0.85,$$

suggesting that an improvement can be claimed for dynamic indexing only if the experimenter is willing to accept an 85 per cent chance of misclassification when H_0 is correct.

Some readers will recognize the ratio (4.1) as Snedecor's F-statistic, whose quantiles under normal theory assumptions are extensively tabled and whose values may be computed in most statistical software packages. It is worth restating that without random sampling from normal populations, the F-statistic is useful only to the extent that it approximates the observed significance level produced by the randomization test.

Kempthorne [6] points out that using the F-statistic as an approximation produces a significance level that will sometimes be too small, and sometimes too large. In this example, reference to a table of the F-statistic provides a p-value of .96. A discrepancy in p-value of .11 with the randomization procedure is hardly cause for concern; the same decision will be made. But suppose the null hypothesis had been reversed; i.e., suppose the intent had been to decisively show that the standard indexing method resulted in greater precision. The p-values for the F-test and the randomization test now become .04 and .15, respectively. An experimenter interpreting these results in a decision theory framework would likely draw different conclusions – standard indexing is better in the first instance; and, insufficient evidence exists to claim that it is better in the second – due simply to choice of procedure. Nominal values of .05 and .10 are commonly chosen for allowable error in a decision-making context. The experimenter who relies on normal theory and the F-test as an approximation is once again misled.

5. Conclusion

Randomization procedures offer a viable approach to the analysis of ballistic data over a wide class of problems. Distribution assumptions are unnecessary and, of even greater importance, random samples of data are not required. Small sample sizes, while never welcome, may be accommodated as well.

In statistics, as elsewhere, there is no free lunch. The price paid for randomization is increased computation, since every problem requires a tailored solution, reflected through the enumerative process required to determine the p-values. However, use of the normal theory statistics – t-test, F-test, chi-square test, etc. – may only be valid to the extent that they approximate the p-values obtained from randomization.

In the examples detailed in this paper, the p-values were attainable through exhaustive enumeration and the tests described may be further delineated as *exact* randomization tests.

Computing power still limits the amount of enumeration possible however, in which case *approximate* randomization tests, not considered here, may then be appropriate.

Reconciliation of some theoretical questions raised by the application of randomization procedures to ballistic data analysis makes this an intriguing and highly practical area of research.

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IF MAILED
IN THE
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