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Tailoring the Statistical Experimental Design Process for LVC Experiments

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THESIS

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Abstract

The use of Live, Virtual and Constructive (LVC) Simulation environments are increasingly being examined for potential analytical use particularly in test and evaluation. The LVC simulation environments provide a mechanism for conducting joint mission testing and system of systems testing when fiscal and resource limitations prevent the accumulation of the necessary density and diversity of assets required for these complex and comprehensive tests. The statistical experimental design process is re-examined for potential application to LVC experiments and several additional considerations are identified to augment the experimental design process for use with LVC. This augmented statistical experimental design process is demonstrated by a case study involving a series of tests on an experimental data link for strike aircraft using LVC simulation for the test environment. The goal of these tests is to assess the usefulness of information being presented to aircrew members via different data link capabilities. The statistical experimental design process is used to structure the experiment leading to the discovery of faulty assumptions and planning mistakes that could potentially wreck the results of the experiment. Lastly, an aggressive sequential experimentation strategy is presented for LVC experiments when test resources are limited. This strategy depends on a foldover algorithm that we developed for nearly orthogonal arrays to rescue LVC experiments when important factor effects are confounded.

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Table of Contents

Page

Page

List of Figures

List of Tables

Table | Page

List of Abbreviations

TAILORING THE STATISTICAL EXPERIMENTAL DESIGN PROCESS FOR LVC EXPERIMENTS

1. Introduction

The use of Live, Virtual and Constructive (LVC) Simulation environments are increasingly being examined for potential analytical use particularly in test and evaluation. LVC simulation environments provide a potential mechanism for conducting joint mission testing and system of systems testing when fiscal and resource limitations prevent the accumulation of the necessary density and diversity of assets required for these complex and comprehensive tests. In 2004 the Department of Defense (DoD) issued the Testing in a Joint Environment Roadmap [**?**] which outlined a way to transform the test and evaluation (T&E) process from service-centric system tests to testing system of systems in a joint environment. This guidance proposes changes to the T&E process to allow the Department of Defense (DoD) to "test like we fight". One of the key recommendations made in the Testing in a Joint Environment Roadmap is to institutionalize the use of LVC simulations to create a realistic joint test range to test systems in a joint system of systems environment over the entire acquisition life cycle.

The majority of research in LVC has thus far been aimed at developing the distributed simulation infrastructure necessary to host joint test events. Another research stream is currently working to create methods and procedures to harness available DoD infrastructure to create effective test campaigns in the LVC environment [**?**]. In addition, a significant amount of research is being conducted to create best practices for verification, validation, and accreditation VV&A of LVC models [**?**]. VV&A is well understood for individual models but the current best practices for individual models are too cumbersome to be used with distributed LVC experiments. Thus, new best practices are needed to conduct VV&A on LVC systems to ensure models are credible [?]. Lastly, a research area introduced by **?** proposes the use of experimental design techniques for testing the joint mission effectiveness of a weapons system in a complex joint environment provided via LVC simulation. This stream has not received much attention but will be essential in the eventual use of LVC in test or other analytical purposes. We extend Gray's research by studying the unique nature of testing with LVC simulations in order to create designed experiments that allow testers to make accurate, statistically significant assessments in a system of systems context.

1.1 A Brief History of Testing in a Joint Environment

Prior to Operation Desert Storm multiple service military operations were conducted by coordinating separate air, land, and sea operations. These separate operations preserved traditional system roles but did not take advantage of any synergies in cooperating service capabilities. This mode of operation changed with Operation Desert Storm; joint service operations continue to this day in Iraq and Afghanistan. During the early stages of joint service operations combatant commanders discovered that systems across services were incompatible. In response to this shortfall, the Secretary of Defense (SECDEF) mandated a new capabilities-based approach to identify gaps in Services' ability to carry out joint missions. By his direction, each service must develop new systems to fill those gaps and, most importantly, must test those systems to ensure they can operate in a joint mission environment [?]. This joint mission test requirement created a need for new capabilities to produce realistic joint mission environments so that testers can fully exercise a system in its intended end-use environment [?].

The Testing in a Joint Environment Roadmap [?] rightly concluded that no single test facility could consistently provide a sufficiently robust joint environment and that networking capabilities could allow testers to assemble distributed tests conducted at separate facilities, connected by a persistent network to make them appear as one large test [?].

Historically, service acquisition requirements were primarily concerned with meeting their obligation to train and equip combat forces with little consideration for the joint mission environment in which the system would eventually be employed. This led to system-centric testing assessing only the effectiveness and suitability to meet those requirements or specifications. The current Service T&E capabilities are world class, but tests are limited in scope to a systems operational environment that does not fully reflect the complexity of joint operations. The SECDEF's guidance requires the DoD T&E community innovate and implement core test capabilities to enable testers to conduct T&E of systems against the joint-centric capability requirements in a realistic joint mission environment. To develop and field joint capabilities the DoD needs to place testing in a joint environment at the core of T&E activity instead of placing it as an extension of system-centric testing. One of those core test capabilities proposed by the SECDEF is to use LVC to test systems in a joint environment. [**?**]

The Joint Test Evaluation Methodology (JTEM) project was established by the Director of Operational Test and Evaluation (DOT&E) in response to the SECDEF's mandate. JTEM was chartered to investigate, evaluate, and make recommendations to improve test capability across the acquisition life cycle in realistic joint environments. One result of JTEM's efforts was the Capability Test Methodology (CTM) **?**. CTM are "best practices" that provide a consistent approach to describing, building, and using an appropriate representation of a joint mission environment across the acquisition life cycle. The CTM enables testers to effectively evaluate system contributions to system-of-systems performance, joint task performance, and joint mission effectiveness [**?**].

CTM focuses on the materiel aspects of the system as well as all aspects of doctrine, organization, training, materiel, leadership and education, personnel, and facilities (DOTMLPF). Considering all these joint capability requirements significantly impact the complexity of the T&E process. To meet the challenge of this increase in complexity, the CTM Analyst Handbook notes that future tests will require innova-

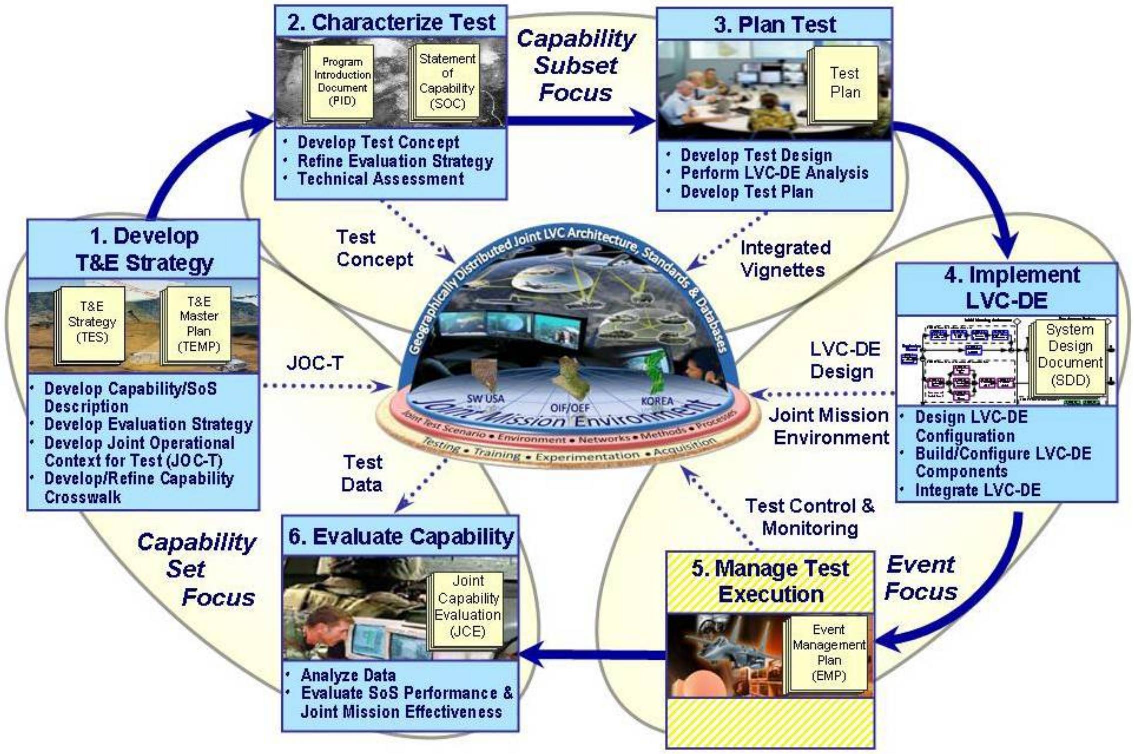

Figure 1 Capability Test Methodology [?]

tive experimental design practices as well as a distributed LVC test environment to focus limited test resources [?].

LVC is key to CTM [?]. LVC can connect geographically dispersed test facilities over a persistent computer network. LVC can also create the necessary variety (number of different systems) and density (number of each system) of assets representative of a joint environment; creating such a joint environment in actual practice would present logistical and cost nightmares. Figure ??, the CTM Handbook [?], illustrates the central role LVC plays in CTM. LVC simulations are well suited to experimentation throughout the acquisition life cycle. Early in system development, relatively simple joint mission environments may involve mostly constructive entities. Live and virtual entities may be added as the subsequent maturity of the system warrants. Cost is yet another reason that LVC is being pursued as a core test capability. LVC is cost effective. While not inexpensive, LVC cost will likely remain a far cheaper alternative to live joint mission experiments. Furthermore, LVC simulation also facilitates examining joint mission scenarios of greater complexity than likely attainable at any single DoD test facility.

1.2 LVC In Training

The LVC concept was first introduced to the DoD by the Joint National Training Center (JNTC) which was established in January 2003 to provide war fighters across all services opportunities to train in a realistic joint mission environment. LVC simulation architecture is the pillar of the JNTC because it allows training exercises to span the full range of current joint tasks while also allowing for improvements in joint warfighting capabilities. The JNTC uses a permanently installed global communications network that significantly reduces the amount to time required to configure a LVC environment. The enhanced training capability broadens and deepens existing joint training by allowing exploration of both strategic and tactical training venues [?].

One of the goals of the Testing in a Joint Environment Roadmap is to leverage the existing LVC architecture currently used for training to meet JCIDS requirements to test in a joint mission environment. Training and T&E each have independent objectives but often share common resources needs, and sometimes, analytical methodologies. Dr. Paul Mayberry, Deputy Under Secretary of Defense for Readiness stated:

> JNTC is a tremendous resource with value and benefit well beyond training. The 'T' can also stand for 'testing.' The underlying pillars for JNTC are the same as those required for a realistic operational test event. We must partner with the testing community to maximize our commonality in the areas of instrumentation, data collection, cross-functional use of ranges, as well as long-term range sustainment [?].

While what Dr. Mayberry says is true, utilizing LVC for test requires a fundamental shift away from the way that LVC is viewed by the training community. More is said about this in Chapter ??, Section ??.

1.3 Components of LVC in Testing

While testing with LVC has yet to be fully realized, components of LVC have been used independently throughout the test enterprise. Constructive simulations have been used extensively in the DoD to experiment with the joint battlespace environment. Specifically constructive simulations have been used to screen factors to determine which factors are significant; compare experimental design methods; compare tactics, techniques, and procedures (TTPs); and compare alternative material solutions to fill joint capability gaps. Virtual simulations have also been used to support tests in human factors studies. However, those studies are focused on the human as the subject under test and not the system with the human as a component. Designed experiments for LVC-based tests can still benefit from those human factor studies since design considerations take into account the variability of the human operator which will have direct application to testing in the LVC environment.

1.4 Issues Associated With Experiments in the LVC Environment

There are many issues that become important when conducting tests in the LVC environment. The complexity of the joint mission environment introduces additional complexity and potentially rich sources of variability that in simpler, systems-oriented experiments, would not be studied or considered. Furthermore, humans-in-the-loop are common in LVC experiments and can be one of the biggest sources of experimental variability. Methods must be developed and employed to correctly account for and estimate the various components of variance so that the error estimate does not become inflated and potentially mask important factor effects.

The new focus on testing in a joint mission environment has made test and evaluation substantially more complex; it now includes testing system of systems performance as well as mission effectiveness. The focus of future tests will not only be on the material components of the joint capability but may include all aspects of doctrine, organization, training, materials, leadership, personnel, and facilities (DOTMLPF) [?]. This means the use of design of experiments (DOE) for testing with LVC must be investigated to ensure that experimental designs are robust enough to capture the complexity of the joint mission environment and allow analysts to make statistically valid factor comparisons based on statistical principles.

A potential challenge with LVC experiments is that in many cases the initial number of factors of interest in a joint mission environment is significantly larger than that of simpler, system-level experiments. **?** provides an illustrative example of testing seven qualitative factors at two levels each in an LVC environment. By using a fractional factorial, split-plot design (FFSPD) the number of runs required was reduced from 128 to 32. While seven factors and 32 runs is not an incredibly large test space, it is important to point out that Gray is presenting a simple case to demonstrate the application of an experimental design to testing with LVC. **?** indicate that there can be up to 30 factors in a realistic joint capability test each with more than two factor levels; this is clearly beyond any test organizations available resources to fully examine, so parsimonious test matrices are required.

Additionally, in many cases testing in a joint environment will involve multiple qualitative factors considered at more than two levels. Qualitative factors often contain more than two levels and cannot be ordered in any numerically meaningful way. Consequently there is no way to exclude factor levels without losing the information provided by the excluded level [**?**]. When this is the case a full factorial design can be intractable and fractioning a design with mixed factor levels becomes very difficult. This large factor space issue is further compounded in LVC because tests conducted in the LVC environment often force a small sample size due to resource limitations. LVC experiments are expensive, manpower intensive, and time consuming. Additionally, tests in an LVC environment are run in near real-time making each run relatively lengthy. This means that fewer, if any, replications can be obtained in an LVC experiment when compared to those obtained in a purely constructive simulation.

In defense experimentation, restrictions on randomization occur with regularity and can prevent the use of a completely randomized design. **?** shows that there are often factors that are difficult to change from one run to the next necessitating the experiment be run in blocks. In such cases care must be taken to design and analyze the experiment with these restrictions in mind [**?**]. Many industrial experiments are fielded as split-plot experiments which accommodate restrictions on randomization yet are erroneously analyzed as completely randomized designs [**?**]. These limitations must be understood and taken into account when planning LVC experiments to maximize the amount of information gained from each test and prevent factor effects from being confounded.

Two analysis techniques, regression and response surface methodologies, are not particularly useful with qualitative factors in the experiment. Other analysis techniques, such as analysis of variance, multiple comparison and non-parametric analysis, are better suited to analyzing experiments with qualitative variables. Collectively, these design issues make designing and analyzing experiments for LVC a challenging endeavor.

1.5 Purpose of Study and Scope

The focus of this research effort is to develop experimental design methods applicable to experiments conducted using LVC simulation. In chapter 2 a general approach to designing industrial experiments is presented followed by a discussion of four classes of experimental designs; split-plot designs, orthogonal arrays (OA), nearly orthogonal arrays (NOA), and D-optimal designs. Each of these four design classes are analyzed for suitability to LVC experiments with particular attention paid to the best array construction methods. OAs and NOAs can significantly reduce the number of runs required for an experiment but have limited estimation capacity because of the small number of runs. Uncrossed split-plot designs can reduce the number of required runs and accommodate randomization restrictions. D-optimal designs are a subset of NOAs and are easily constructed using common statistical software packages.

Chapters 3, 4, and 5 are each presented in journal article format. Chapter 3 presents a well-known experimental design process for industrial experiments and highlights additional considerations when using this process to plan and execute LVC experiments. Additionally, the aforementioned classes of experimental designs are discussed and analyzed for suitability to LVC experiments. In Chapter 4 the statistical experimental design process is applied to a data link experiment using LVC to create the test environment. The case study illustrates how the LVC test experience is improved by using a statistical experimental design methodology. Chapter 5 presents a sequential experimentation strategy for LVC experiments when test resources are limited. This strategy depends on a foldover algorithm that we developed to break the aliasing between factors in certain nearly orthogonal arrays. This algorithm allows testers to rescue LVC experiments when post-test analysis reveals that important factor effects are confounded.

2. Survey of Relevant Literature

Most of the studies in the literature regarding testing with LVC have discussed the processes, procedures, and methods that DoD organizations have used to coordinate and plan tests in a joint environment.

2.1 LVC in Literature

? write that the joint testing and methodology (JTEM) project was chartered by the Director of Operational Test and Evaluation (DOT&E) to investigate improvements to the acquisition life cycle in realistic joint environments. Specifically, JTEM was focused on testing in a joint environment (TIJE). A key aspect of the JTEM's study was investigating the use of LVC joint test environments to evaluate system performance and mission effectiveness.

Over three years JTEM used various T&E activities to test and evaluate methods and processes. These activities included the Air Force's INTEGRAL FIRE and the Army's Joint Battlespace Dynamic Deconfliction events. INTEGRAL FIRE was intended to represent typical testing in a joint environment during early system development using the Capability Test Methodology (CTM) [**?**]. The INTEGRAL FIRE test objective was to evaluate the contributions of two developmental weapons systems to joint mission effectiveness when those weapon systems were employed together in a system of systems context [**?**]. These test cases provided JTEM with an opportunity to implement CTM processes and consider applying experimental design methods [**?**] as well as using data collected from these distributed LVC events to evaluate system performance and mission effectiveness [**?**].

A crucial insight stemming from JTEM's activities was the use of LVC to evaluate design alternatives early in the system life cycle when it is relatively easy (and cost effective) to change any constructive or virtual prototypes of the system of interest. Furthermore, they recommend that tactics, techniques, and procedures (TTPs) be included as factors of interest in the experiment since system effectiveness inherently depends on how it is used [**?**]. These insights represent a profound change in the way T&E is utilized in future test activities and presents new challenges to the test community. Including design alternatives and TTPs in test activities can potentially introduce qualitative factors with mixed factor levels thereby increasing the complexity of the ensuing experimental design. In such cases traditional two-level factorial designs, as are typically presented in any text on experimental design are no longer a feasible option.

Test practitioners have also been interested in defining a set of use cases to help test teams determine if LVC is appropriate for their particular test application. In 2009 a focus group was conducted at the AIAA Air Force T&E Days Conference to discuss potential use cases for LVC in T&E and proposed exploratory testing, test rehearsal, specification compliance, confirmatory analysis, and TTP development as such potential use cases. Additionally participants concluded that LVC is best utilized for the following types of tests [**?**]:

1. Tests that involve human interactions and/or actual hardware and/or software,
2. System of systems tests to evaluate interoperability or develop TTP, and
3. Mission and task-level evaluations that require highly dense threat environments, scarce or one-of-a-kind resources, and interoperability assessments and TTP development.

The participants also concluded that LVC is not normally suitable for:

1. Traditional performance, structural and handling qualities envelope expansion
2. Reliability, availability, and maintainability testing
3. Any test where transport latency issues cannot be tolerated, such as electronic attack at pulse level, and
4. Physical environment testing. [**?**].

The proposed use cases provide a good start to defining a set of appropriate applications of LVC. These use cases need to be continually refined and expanded should some of the proposed applications fail to meet expectations and future applications are discovered.

The use of design of experiments (DOE) for LVC is important for DoD use of LVC in testing. However, past employment of DOE in LVC appears quite limited. **?**'s use of a fractional factorial split plot design for a robust parameter experiment using LVC appears to be the only paper that applies statistical experimental design processes to LVC experiments.

2.2 Designs for Small Sample Size and Mixed Level Factors

As mentioned earlier, testing in a LVC simulation environment often results in experiments requiring small sample size and a large number of mixed level factors. These design constraints make standard designs like fractional factorial designs a sometimes inappropriate design choice. There are however alternative designs that can be used to accommodate these constraints depending on the objectives of the experiment. Each design is best suited to certain test scenarios.

2.2.1 Split-Plot Designs. Split-plot experiments began in the agricultural industry and the split-plot's agricultural terms, whole-plot and sub-plot have persisted. For example, one factor in an agricultural experiment is usually a fertilizer or irrigation method, it can only be applied to large sections of land called whole plots. The factor associated with this is therefore called a whole plot factor. Within the whole plot, another factor, such as seed variety, is applied to smaller sections of the land, which are obtained by splitting the larger section of the land into subplots. This factor is therefore referred to as the subplot factor.

These split-plot designs are used when there are restrictions in randomization that prevent the use of a completely randomized design. These restrictions can be caused by the presence of hard-to-change (HTC) factors, human factors limitations, or in the case of Robust Product Design (RPD), even the objectives of the experiment. These restrictions make a completely randomized design inappropriate and can lead the experimenter to erroneous conclusions if analyzed in a manner inconsistent with the design and execution of the experiment [?]. In split-plot designs, HTC factors are assigned to a larger experimental unit called the whole plot while all other factors are assigned to the subplot. ? state that in the presence of HTC factors, a split-plot design can significantly increase the ease of experimentation and save precious time and resources. A side benefit of some split-plot designs is that they may require fewer runs than a completely randomized design.

In experiments where humans are part of the system under study it can be advantageous to change some factors less often than others to prevent human operator confusion (or learning) that can artificially inflate the error estimate. For example, consider a machine shop interested in testing the effect of certain lathe operation procedures under a variety of operational settings. Depending on the complexity of the procedures, the potential for operator error can increase if procedures change between each run. A better estimate of the procedure effects might be obtained if the operator were to operate the lathe with one set of procedures before moving to the next set. All other factors potentially effecting lathe operations are assigned to the subplot with the schedule of runs completely randomized in that subplot.

RPD is an experimental design concept pioneered by Taguchi [?]. RPD experiments seek process settings that minimize the process's sensitivity to random noise found in operational settings. In spite of Taguchi's revolutionary concept, his RPD designs require large run sizes. Smaller run sizes for robust parameter experiments can be obtained by using split-plot designs and combined array designs making them a popular choice for this class of experiments [?]. In RPD the factors of interest in the experiment are divided into two categories, design factors and environmental noise factors. Noise factors are not of primary interest and consequently are assigned to the whole plot. Design factors are placed in the subplot since better estimates of their effects can be obtained from the subplot. The error structure of split-plot designs is readdressed later.

Split-plot designs can be analyzed using a standard, mixed-model, ANOVA-based approach when the experiment is balanced and orthogonal [**?**, 558]. The ANOVA model for a balanced two-factor split-plot design, where there are a levels of the whole plot factor A (applied to c whole plots) and b levels of the subplot treatment B is given by

$$Y_{ijk} = \mu + \alpha_i + \beta_j + (\alpha\beta)_{ij} + \gamma_{k(i)} + \varepsilon_{ijk} \tag{1}$$

Where μ is the intercept; α_i are the a whole plot treatment effects; β_j, the b subplot treatment effects; $(\alpha\beta)_{ij}$, the ab interaction effects; $\gamma_{k(i)}$, the ac whole plot errors assumed independent and distributed as $N(0, \sigma_w^2)$; and ϵ_{ijk} are independent $N(0, \sigma^2)$ subplot error terms [**?**].

A split-plot experiment is a blocked experiment where the blocks serve as an experimental unit for a subset of factors. In a split-plot design there are two different sets of experimental units. The HTC factors are assigned to the larger experimental unit, called the whole plot, and the easy to change factors are assigned to the smaller experimental unit, called the subplot. The split-plot experiment is run by randomly selecting a whole plot and randomly running each design point within that whole plot, repeating until each whole plot is run. This design results in two independent error terms, one for the whole plot and one for the sub-plot. The whole plot error has fewer degrees of freedom than the subplot since it contains fewer randomized runs. Consequently, less precise estimates can be made of factor effects for factors assigned to the whole plot [**?**].

In some circumstances a more precise estimate of the whole plot factors is needed. **?** propose a hybrid method that falls between a completely randomized design and split-plot design in terms of factor level changes. This design changes the HTC factors more frequently creating more whole plots thereby increasing the degrees of freedom available to estimate the whole plot effects. They state six benefits to using this hybrid approach.

1. The statistical efficiency of the experiment is increased.

2. Increasing the number of level changes protects against systematic errors if something goes wrong at a HTC factor level.

3. An increased number of whole plots ensures an improved control of variability and provides better protection against trend effects.

4. More degrees of freedom are available for the estimation of the whole plot error.

5. An increased number of HTC factor level changes allows a more precise estimation of the coefficients corresponding to these factors.

6. The number of factor level changes is generally smaller than a completely randomized design.

They present an algorithm for constructing D-optimal, split plot designs to generate these designs. For more details regarding the construction of D-optimal split plot designs, consult **?** .

In some instances a full factorial split-plot design is unachievable due to resource constraints so the design must be fractioned. **?** give an excellent survey of fractional factorial split-plot (FFSP) designs in which they discuss two approaches to constructing FFSPs; Cartesian product design and split-plot confounding. The Cartesian product design generators separate the whole plot factors and the subplot factors into separate defining words. For example, in a 2^{7-4} FFSP experiment with whole plot factors A, B, C, and D and subplot factors p, q, and r, the Cartesian product design uses

$$D = ABC,\ q = p \text{ and } r = p \tag{2}$$

as the defining words. This design is obtained by crossing a resolution IV design, 2^{4-1}, in the whole plots with a resolution II design, 2^{3-2}, in the subplot making the overall design resolution II, meaning that some of the main effects are confounded. A resolution II design is unacceptable for most applications. A resolution IV design can be created using split-plot confounding by including whole plot factors in the split plot factorial generators. The split-plot confounding technique uses

$$D = ABC,\ q = BCp \text{ and } r = ACp \tag{3}$$

as the FFSP design generators giving a superior design with none of the main effects are confounded.

At times test conditions may not remain homogeneous over a fractional factorial split-plot experiment making it necessary to run the experiment in blocks. McLeod and Brewster give a ranking scheme to find the best minimum aberration design out of many possible combinations of defining words. They present designs that cover blocking in powers of two but recognize that practical considerations might prevent such a design from being used [?].

Split-plot designs have promising application to LVC experiments since randomization restrictions often arise. **?** discusses an LVC experiment conducted to compare the effect of several factors on the joint mission effectiveness of air launched weapon designs. The primary goal is to evaluate each weapon's design based on joint mission effectiveness and robustness to uncontrollable sources of variation. He found that there are seven two-level factors of interest with four factors considered operational noise factors and three factors considered design factors. A common RPD uses a split plot design and assigns the noise factors to the whole plot and the design factors to the subplot. The four noise factors in Gray's experiment placed in the whole plot and the three design points are placed in the subplot. The design factors are placed in the subplot to obtain good estimates of the effects, find design settings insensitive to noise factors and optimize the weapon's effectiveness.

Gray defines $k_1 = 4$ factors in the whole plot and $k_2 = 3$ factors in the sub plot with f_1 and f_2 as the number of factors aliased with interaction terms in the whole plot and sub plot respectively. Gray uses the notation $2^{k_1-f_1} \times 2^{k_2-f_2}$ [?] to represent the fractional factorial split-plot design. Gray points out that there are many possibilities for aliasing the effects and great care must be taken to ensure that the test objectives are achieved. For example, he shows that the most obvious design generator

$$D = ABC \text{ and } r = pq \tag{4}$$

which yields the complete defining relation

$$I = pqr = ABCD = ABCDpqr \tag{5}$$

is not necessarily the design with the best resolution. This design has only partial resolution III in the subplot factors which means that the main effects are confounded with two factor interactions. Since the factor effects in the subplot are often of most interest, this design is unacceptable in many applications. Gray uses a minimum aberration FFSP design, with split-plot resolution V, from table 4 in **?** to show that higher resolution designs can be obtained by using split plot confounding. The design generators for this design are

$$D = ABC \text{ and } r = ABpq \tag{6}$$

and yields the complete defining relation

$$I = ABCD = ABpqr = CDpqr \tag{7}$$

which is superior to the previous design. This is an important result since it allows the experimenter to efficiently estimate the main effects and two factor interactions in the subplot as well as the whole plot by subplot interaction. This is crucial since the subplot factors and interactions that are most interesting in a RPD [**?**].

Tests conducted using LVC may have restrictions that prevent the test from being executed in completely random order. This makes split-plot designs a critical design for LVC experiments with randomization restrictions.

2.2.2 Orthogonal Arrays. Orthogonal arrays (OA), introduced by **?**, are a powerful class of designs that can significantly reduce the experiment run size and accommodate many mixed-level factors when there are no restrictions on randomization. OAs are becoming an increasingly popular class of experimental design. There are two general types of OAs, symmetric and asymmetric. Symmetric OAs, which are more widely used, have the same number of factor levels in every column of the design matrix. These arrays are used mostly in screening experiments for larger two-level factorial designs. The most prominent example of a two-level symmetric OA is the Plackett-Burman design. Some controversy surrounds the use of such designs since the aliasing of effects can make interactions difficult to disentangle [**?**].

Asymmetric OAs differ from symmetric OAs in that they have at least one factor that contains a different number of levels than the other factors in the design [**?**]. The asymmetric OAs have significant potential for LVC experiments as they can accommodate mixed level factors while maintaining an economical run size. For example, consider an experiment with a three-level factor and four two-level factors where resources provide for only 12 runs. A full-factorial design would require 48 runs and fractioning the design would be very complicated. An orthogonal array can be constructed with 12 runs and will allow each of the main effects to be estimated independently along with select interactions. When all available degrees of freedom are used to estimate main effects the design is said to be saturated.

A variety of methods have been used to construct OAs including combinatorial, geometrical, algebraic, coding theoretic, and algorithmic approaches. We will focus primarily on **?**'s approach using difference matrices and **?**'s algorithmic approach. **?** is an excellent resource to learn more about OAs.

There are many exchange algorithms that have been proposed for constructing exact D-optimal designs [**?**]. These algorithms can be used to construct OAs but they are inefficient and unable to produce very large designs. In fact, the largest design published so far using this technique is $\mathrm{OA}(12, 2^{11})$ [**?**]. Nguyen modified an exchange algorithm and proposed an interchange algorithm that can be used to construct supersaturated designs [?]. Nguyen's algorithm is capable of constructing two-level OAs with the largest OA constructed being OA$(20, 2^{19})$.

Global optimization search algorithms such as simulated annealing, thresholding accepting, and genetic algorithms can be used to construct OAs. These algorithms are powerful but they often require a large number of iterations and are slow to converge to a solution which makes them a relatively ineffective way to construct OAs [?]. **?** proposed an algorithm for constructing mixed-level OAs via searching some existing two-level OAs. Their objective was to construct mixed-level OAs with as many two-level columns as possible. Their algorithm succeeded in constructing several new large mixed-level OAs.

? give an approach for constructing several general classes of asymmetrical orthogonal arrays using difference matrices. (Note: WW's approach is later modified and the difference matrix approach is used to construct nearly orthogonal arrays. More will be said about this in (**??**)) They begin by constructing the difference matrices, using Kronecker sums, that are of the form of a generalized Hadamard matrix. A difference matrix, denoted by $D_{\lambda g, r; g}$, is a square matrix such that the difference between the elements of any two columns, modulus p, occurs λ times. If the transpose of a difference matrix is also a difference matrix then it is called a *generalized Hadamard matrix*. **?** let G be an additive group of g elements denoted by $\{0, 1, \cdots, g-1\}$. A $\lambda g \times r$ matrix with elements from G is a difference matrix $D_{\lambda g, r; g}$ if among the differences of the corresponding elements of any two columns each element of G occur λ times. For example in the matrix

$$D_{2(3),6;3} = \begin{bmatrix} 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & 2 & 0 & 1 & 2 \\ 0 & 2 & 1 & 1 & 0 & 2 \\ 0 & 0 & 2 & 1 & 2 & 1 \\ 0 & 2 & 0 & 2 & 1 & 1 \\ 0 & 1 & 1 & 2 & 2 & 0 \end{bmatrix}$$

$g = 3$ and the difference between the corresponding six elements of any two columns each take the values 0, 1, and 2 (mod 3) twice. For a $n \times r$ matrix $A = [a_{ij}]$ and a $m \times s$ matrix B, they define the Kronecker sum to be the $mn \times rs$ matrix

$$A \otimes B = [B^{a_{ij}}]_{1 \leq i \leq n, 1 \leq j \leq r}$$

where

$$B^{a_{ij}} = (B \oplus a_{ij} J) \bmod p$$

is obtained from adding a_{ij} mod p to the elements of B where J is the $m \times s$ matrix of ones. To illustrate this method consider

$$L_3(3) \otimes D_{6,6;3} = \begin{bmatrix} D_{6,6;3} + \mathbf{0} \\ D_{6,6;3} + \mathbf{1} \\ D_{6,6;3} + \mathbf{2} \end{bmatrix}$$

where $L_3(3)$ is the 3×1 matrix $(0, 1, 2)^T$ and the addition is done modulo 3. The resulting matrix is now an 18×6 orthogonal array $L_{18}\left(3^6\right)$. More generally, let $L_1 = L_{\mu g}(g^s)$ be an orthogonal array with μ copies of g elements in the array and let $D = D_{\lambda g, r; g}$ be a difference matrix. Then $L_1 \otimes D$ is an orthogonal array $L_{\lambda \mu g^2}(g^{rs})$. The construction procedure is completed by adding another orthogonal array to $L_1 \otimes D$

to use up the remaining degrees of freedom. Consider again the matrix

$$\begin{bmatrix} D_{6,6;3} + \mathbf{0} \\ D_{6,6;3} + \mathbf{1} \\ D_{6,6;3} + \mathbf{2} \end{bmatrix}.$$

Out of the 17 df available, only 12 are used in the array $L_{18}\left(3^6\right)$. To use the remaining 5 df, three copies of $L_6(6)$ are added to the matrix, which results in the orthogonal array:

$$L_{18}(6 \cdot 3^6) = \begin{bmatrix} D + \mathbf{0} & L_6(6) \\ D + \mathbf{1} & L_6(6) \\ D + \mathbf{2} & L_6(6) \end{bmatrix}$$

which can be re-written in short form as $\left[L_3(3) \otimes D_{6,6;3}, 0_3 \otimes L_6(6)\right]$. More generally, let L_1 and D be defined as before, let $0_{\mu g}$ be the $\mu g \times 1$ vector of zeros, and let $L_2 = L_{\lambda g}(q_1^{r_1} \cdots q_m^{r_m})$ be an orthogonal array. Then the matrix

$$[L_1 \otimes D, 0_{\mu g} \otimes L_2] \tag{8}$$

is an orthogonal array $L_{\lambda \mu g}(g^{rs} \cdot q_1^{r_1} \cdots q_m^{r_m})$.

Using this method they create several asymmetrical orthogonal arrays of size 18, 24, 36, 40, 48, 50, 54, 72, 80, 90, 96 and 98 runs. The reader is referred to **?** for the specific $L_1, D, 0_{\mu g}$, and L_2 used to construct each array for a particular run size.

? uses an columnwise *interchange* and *exchange* algorithm to construct orthogonal and nearly orthogonal arrays (NOA) using the J_2 optimality criterion to evaluate candidate columns. The J_2 optimality criterion measures the amount of correlation between columns of the design matrix. A weighted sum

$$\delta_{i,j}\left(\mathbf{d}\right) = \sum_{k=1}^{n} w_k \delta(x_{ik}, x_{jk}) \tag{9}$$

is used to measure the similarity of the i^{th} and j^{th} rows of $\mathbf{d}$ where $\delta(x_{ik}, x_{jk}) = 1$ if $x_{ik} = x_{jk}$ and 0 otherwise. Then $J_2(\mathbf{d})$ is calculated by taking the sum of squares of all $\delta_{i,j}(\mathbf{d})$ for $1 \leq i < j \leq N$.

$$J_2(\mathbf{d}) = \sum_{1 \leq i < j \leq N} [\delta_{i,j}(\mathbf{d})]^2 \tag{10}$$

A design is J_2 optimal if it minimizes the J_2 criterion (**??**). Xu also provides efficient methods to calculate a lower bound for J_2.

The **?** algorithm adds randomly generated, balanced columns sequentially and then interchanges (swaps) pairs of column elements until the design reaches a lower bound or no further improvement is possible. The algorithm avoids an exhaustive search for improvement in columns, which can be computationally inefficient. This means that the algorithm performs a local search often resulting in a design that is only locally optimal. To overcome this, Xu adds a global exchange procedure to the algorithm allowing the search to move around the entire design space thereby increasing the likelihood of finding the global optimal solution. The exchange procedure does not guarantee that a global optimal solution will be found.

2.2.2.1 Projection Properties of Orthogonal Arrays. In the early stages of experimental planning it is often necessary to assume that not all factors being initially examined significantly affect the system under study [**?**]. This assumption is based on the well-known and accepted *sparsity of effects* principle which states that, a system is usually dominated by main effects and low-order interactions. Thus it is most likely that main (single factor) effects and two-factor interactions are the most significant responses with interactions involving three or more factors being very rare. An important consequence of this principle is that factors can be dropped from the model when analysis reveals those factors are inactive thereby projecting the original design into a stronger design. This stronger design allows experimenters to estimate higher order interactions for a subset of active factors. Projection increases the available degrees of freedom needed to estimate interactions between the significant factors and, depending on the size of the original design, can provide more degrees of freedom to estimate the error. Thus, projection is an important property that can be exploited in factor screening experiments.

All OAs estimate the main effects equally well but not all OAs can be projected into stronger designs. This makes projection an important property used to classify and discriminate between OAs. The projectivity of a design can be summarized by its strength. Rao said that an OA of strength m is an array in which, for every m-tuple of columns, every level combination occurs equally often [**?**]. This means that every m-tuple of columns contains at least one replicate of a full factorial design. An OA of strength m has some desirable properties:

1. Any full projection model involving m factors is estimable. This means that all main effects and interactions can be estimated.
2. The analysis of main effects can be conducted with the highest efficiency.
3. The analysis of the full projection model involving m factors can be conducted with the highest efficiency [**?**].

Saturated designs, or main effect plans (MEP), are OAs where all degrees of freedom are used up estimating the main effects. Saturated designs can be difficult to analyze if any interactions are present because of the complex aliasing between factors and interactions. This has led many to question the usefulness of such designs. **?** counter that it is the projection properties of these designs that make them useful.

Plackett-Burman designs are well known two-level MEP. Lin and Draper studied projections of PB designs and found all of the $12, 16, 20, 24, 28, 32,$ and 36 run PB designs project onto three factors [**?**]. **?** and **?** considered the projections of 12 run PB designs onto four and five factors and found that projecting the PB design onto four factors always allowed the main effects and two factor interactions to be estimated for the four factors. Wang and Wu also found this result when considering 20 run PB designs and proposed the term *hidden projection*.

? observed that the results found by Lin and Draper and Wang and Wu were mostly computer works and attempted to derive a more general approach to the projection of two-level orthogonal arrays. He considered projection properties of $OA(N, 2^k, t)$ to $t + 1$ and $t + 2$ factors, where N is the $N \times N$ PB design matrix and t is the strength of the array. He found that if N is not a multiple of 8, then any OA with N runs and two-levels has the following two level hidden projection property: Any four-factor projection can entertain all four main effects and all two factor interactions among them. **?** also give three general results that provide a theoretical basis for the empirical discoveries and provide a means for categorizing the projective properties of PB designs .

One drawback with PB designs is that they cannot accommodate factors with more than two levels. **?** extends the concept of hidden projection to other widely used nonregular designs such as three-level and mixed-level designs. He introduces *moment aberration projection* (MAP) as a new criterion to rank and classify nonregular designs, including multi-level orthogonal arrays. A nonregular design can be identified by its complex alias structure as opposed to the simpler alias structure of regular designs where all main effects are either orthogonal or completely confounded. A nonregular design is characterized by at least one pair of effects that are neither orthogonal nor fully aliased. Nonregular designs are not often considered because of the difficulty that accompanies their complex alias structure. However, interest in nonregular designs was renewed after **?** devised a method that uses stepwise regression to resolve the the complex alias structure. **?** expanded analysis options for nonregular designs by developing a Bayesian variable selection technique for regression models .

Hamada and Wu's approach can glean much information from the aliased terms given there are only a few interactions that are significant and the interactions are smaller than the main effects. If interaction effects are larger than the main effects some significant main effects may be masked by the interaction effect. The stepwise regression analysis technique was designed primarily for the 12 run PB design; however, it can be used for other PB designs and general mixed-level orthogonal designs [?].

Experiments using LVC often require nonregular designs. While analysis techniques are available for nonregular designs, these techniques utilize regression which is not ideal for LVC experiments since many factors are not quantitative. Such mixed-level experiments may be better analyzed using multiple comparison techniques to determine the best factor level settings once the active factors have been discovered.

LVC is intended for testing throughout the entire life cycle of systems that operate in a joint environment. OAs are well suited for factor screening experiments early on in the system life cycle where little is known about the system. The projection property of OAs make them an efficient approach to gain information about the active effects and interactions.

2.2.3 Nearly Orthogonal Arrays (NOA). Orthogonal arrays are sometimes unable to reduce the run size sufficiently while accommodating the necessary number of $k \geq 2$ level factors. One option is to increase the run size, which may not be feasible due to resource restrictions. **?** show that an orthogonal array $L_{12}(3^1, 2^k)$ exists for $k \leq 4$ but for $k = 6$ no such orthogonal array exists. Orthogonality can only be restored by adding an additional 12 runs. This is a costly, often unachievable alternative. The other option is to relax the orthogonality requirement.

? use a combinatorial method for constructing NOAs; most research on NOAs use algorithmic approaches. Several authors have proposed algorithmic methods for constructing NOAs with most using some form of column-wise exchange procedure to search for the best design. Nguyen uses an exchange algorithm to construct mixed-level NOAs and evaluates the columns with an approximation of D- and A- optimal criteria [?]. This algorithm is fast, easy to understand and implement. **?** uses an interchange and exchange algorithm and evaluates the candidate columns using a J_2-optimality criteria . This algorithm is computationally inexpensive and more flexible than the other methods previously mentioned. **?** use two algorithms to build NOAs with useful projective properties . Each approach is summarized below.

Wang and Wu pioneered the use of nearly orthogonal arrays and introduced criteria for comparing designs [**?**]. **?** constructed orthogonal arrays by taking the Kronecker sum of an orthogonal array, $L_N(k)$, and a difference matrix, $D_{\lambda p;r,p}$ with the result being another orthogonal array (**??**) . By slightly modifying that method they can construct NOAs. A $n \times r$ *nearly difference matrix*, $D'_{n,r;g}$, is used rather than a *difference matrix* $D_{n,r;g}$ with entries from the group G such that, among the differences of the entries of any two columns, the elements of G occur as evenly as possible; where G is an additive group of g elements denoted by {0,1,...,g-1}. The result is a matrix

$$[L_1 \otimes D', 0_{\mu g} \otimes L_2] \tag{11}$$

that is a NOA $L'_{\lambda \mu g}(g^{rs}, \cdot q_1^{r_1} \cdots q_m^{r_m})$. Although effective, constructing NOAs with this method is cumbersome since it requires that the experimenter have a set of OAs and nearly difference matrices to construct NOAs. Furthermore, the number of NOAs that can be created is limited by the number and variety of nearly difference matrices that are available to the experimenter. Otherwise the experimenter must have an algorithm for constructing nearly difference matrices in addition to Wang and Wu's NOA construction method.

Wang and Wu propose two criterion for evaluating the suitability of a NOA. The first is to compute the overall estimation efficiency of the array using the D-optimal criterion

$$|X^t X|^{1/k} \tag{12}$$

for estimating the main effects, where $X = [x_1/||x_1||, ..., x_k/||x_k||]$. They show that since the columns of X are standardized, D achieves it's maximum value of 1 if and only if the columns of X are orthogonal to each other. Another useful criterion given by WW is the D_s criterion

$$\{x_i^t x_i - x_i^t (X_{(i)}^t X_{(i)})^{-1} X_{(i)}^t x_i\} / x_i^t x_i \tag{13}$$

which measures the orthogonality of column x_i to the rest of the matrix $X_{(i)}$ where $X_{(i)}$ is the matrix with column i deleted. D_s achieves its upper bound value of 1 if and only if x_i is orthogonal to $X_{(i)}$. Wang and Wu give a systematic construction of NOAs of strength two with small run sizes. The reader is referred to **?** for the designs.

? uses a sequential columnwise algorithm for constructing mixed-level NOAs with few runs. His procedure is limited to constructing NOAs where the number of runs is divisible by the number of levels of each factor. The algorithm starts with a base OA, or NOA with mixed levels $L_n^{(1)}(s_1^{k_1}, ..., s_r^{k_r})$, builds up the $n \times m_0$ ($m_0 = \sum k_i(s_i - 1)$) design matrix X_0 from this array using two-level orthogonal polynomials, and evaluates the design using $f = \sum_{i<j} s_{ij}^2$ from the newly formed $X'X$ matrix.

? states that an obvious advantage of using the $\sum_{i<j} s_{ij}^2$ criterion over the more familiar D- and A- optimality criterion is that it is computationally cheaper because it works with $X'X$ instead of $(X'X)^{-1}$. He notes that $\sum_{i<j} s_{ij}^2$ is only an approximation of the D- and A- optimality criteria, hence among designs with the same $\sum_{i<j} s_{ij}^2$ the one with the highest $|X'X|$ is selected.

Using this procedure Nguyen creates more efficient NOAs than similar designs produced by Wang and Wu's combinatorial method for the same factors and run size in all but four designs. The reader is referred to Nguyen for more detailed comparisons between NOAs constructed by Nguyen and Wang and Wu [**?**].

? constructed OAs by using an interchange and exchange algorithm and taking the first n_0 orthogonal columns for an $N \times n$ design. The same algorithm is used to construct NOAs by using another global exchange parameter T_2. For a given candidate column Xu computes the lower bound $L(2)$ for the optimality criterion J_2 and chooses a global search parameter $T = T_1$ or $T = T_2$ depending on whether the columns of the design matrix $\mathbf{d}$ are orthogonal: $T = T_1$ if $\mathbf{d}$ is orthogonal and $T = T_2$ if $\mathbf{d}$ is not. The value of T determines the number of times a column is exchanged and searched again. Xu recommends that the user choose a moderate value for T_2, say 100, when constructing NOAs [**?**].

Projection properties of OAs are well documented and provide an elegant method for estimating higher order effects when there are few active effects in a model. **?** extends this useful property to NOAs and demonstrates his method by introducing several new NOAs of strength 2 and strength 3. **?** defines a NOA of strength m if for every m-tuple of columns, all possible level combinations occur at least once in n runs and the design has the minimal $B(m)$ value. The $B(m)$ criteria is a measure of m-balance. A design is said to have m-balance if the numbers of all level combinations of any m factors occur equally often. NOAs do not possess the m-balance property and the $B(m)$ criteria is a way to measure how far the design has departed from this property.

Consider a design $D(n; q_1 \cdots q_m)$ written as an $n \times k$ matrix $\mathbf{X} = (x_1, x_2, ..., x_k)$. The $B(m)$ criteria can be computed for every m-tuple of columns of $\mathbf{X}$, $(x_{l_1}, x_{l_2}, ..., x_{l_m})$

$$B_{l_1 \dots l_m}(m) = \sum_{\alpha_1, \dots, \alpha_m} \left(n^{(l_1 \dots l_m)}_{\alpha_1, \dots, \alpha_m} - \frac{n}{q_{l_1} \cdots q_{l_m}} \right)^2 . \qquad (14)$$

$B_{l_1 \dots l_m}(m)$ measures a given m column subdesign's departure from m-balance where $n^{(l_1 \dots l_m)}_{\alpha_1, \dots, \alpha_m}$ is the number of runs that $(x_{l_1}, x_{l_2}, \cdots, x_{l_m})$ takes the level combination $(\alpha_1, \cdots, \alpha_m)$. The summation is taken over all $q_{l_1} \cdots q_{l_m}$ level combinations. When all m column subdesigns have been calculated, the average of the $B_{l_1 \dots l_m}(m)$ values is the $B(m)$ criteria,

$$B(m) = \sum_{1 \le l_1 < \cdots < l_m \le k} \frac{B_{l_1 \ldots l_m}(m)}{\binom{k}{m}}, \tag{15}$$

which is a global measure of how close the design is to m-balance.

? use two algorithmic approaches to construct NOAs. A columnwise-pairwise (CP) algorithm is used to construct strength-2 NOAs and a sequential algorithm for constructing strength-3 NOAs. They use the m-projection property and the $B(m)$ criterion to evaluate candidate NOAs where the design with the minimal $B(m)$ value is chosen. Several new designs were discovered and are found in **?**.

? provide an important development with tremendous potential for LVC experiments, particularly when screening for factors in the early stages of experimentation. This method is particularly useful when higher order interactions are suspected and only a few factors are believed to be active. One drawback is that significant correlation can be introduced into the array to achieve the desired projection properties which in turn makes the analysis more complex. An example of this is shown in Figure **??**. Notice that columns 6 and 8 contain significant correlation which would make analyzing any pair containing those columns more difficult.

2.2.4 D-Optimal Designs. Optimal designs are so named because their nearly orthogonal design is constructed to optimize some evaluation criteria of the design. Optimal designs are an excellent way to construct mixed level designs with D-optimal being the most widely used design. **?** demonstrated the potential use of optimal designs in wind-tunnel experimentation. The D-optimal criterion maximizes the overall degree of orthogonality of the design matrix. Two popular alternatives are the A and G-optimal design criteria. The A-optimal design criterion minimizes the degree of correlation between the columns of the design matrix. The G-optimal criterion minimizes the maximum prediction variance and is useful if a regression model built from the experimental data is to be used to make predictions about the system response.

Table 1 NOA design for LVC Experiment.

	Factors									
Run	DL	V	NP	AC	EP	ES	FP	FS	R	TL
1	0	0	0	0	0	0	0	0	0	0
2	0	1	1	1	1	0	0	0	0	0
3	0	0	1	0	1	1	1	1	1	1
4	0	1	0	1	0	1	1	1	1	1
5	1	0	1	1	0	0	1	1	0	0
6	1	1	0	0	1	0	1	0	0	1
7	1	0	1	1	0	1	0	0	1	1
8	1	1	0	0	1	1	0	1	1	0
9	2	0	0	1	1	0	1	0	1	1
10	2	1	1	0	0	0	0	1	1	1
11	2	0	0	1	1	1	0	1	0	0
12	2	1	1	0	0	1	1	0	0	0
D_s	1.00	0.89	0.89	0.89	0.89	0.76	0.76	0.76	0.33	0.36

DL defined as Data Link
V defined as Vignette
NP defined as Node Position
AC defined as Aircrew
EP defined as Enemy Air Forces Position
ES defined as Enemy Air Forces Size
FP defined as Friendly Air Forces Position
FS defined as Friendly Air Forces Size
R defined as Route
TL defined as Target Location

Table 2 A 15-Run D-optimal Mixed-level Design for Five Factors

	Factors				
Run	Factor A	Factor B	Factor C	Factor D	Factor E
1	L4	L2	L1	1	1
2	L1	L1	L3	1	1
3	L5	L4	L2	1	1
4	L3	L3	L2	1	0
5	L4	L1	L2	0	0
6	L2	L4	L3	1	0
7	L1	L4	L1	0	0
8	L5	L2	L3	0	0
9	L3	L2	L3	1	0
10	L3	L1	L1	0	1
11	L2	L2	L2	0	1
12	L4	L3	L3	0	1
13	L5	L3	L1	1	0
14	L1	L2	L2	1	0
15	L2	L1	L1	1	0

L_i defined as level i of the associated factor

?, 382 shows the power of optimal designs with the following example. Consider an experiment with five factors: A is categorical with five levels, B is categorical with four levels, C is categorical with three levels, and D and E are continuous with two levels each. Estimates of all of the main effects are desired. A full factorial has 240 runs and is an orthogonal design; however, it is terribly inefficient at estimating the main effects since only 11 degrees of freedom are required to do so. The one-half, one-quarter, and one-eighth fraction designs would require 120, 60, and 30 runs respectively, are not orthogonal, and require too many runs to be considered efficient designs. A 15-run D-optimal design, shown in Table **??** constructed using the optimal design tool in JMP has near balance and nearly uniform relative variance (variance divided by σ^2). The relative variances for the individual model effects for the 15-Run D-optimal design are shown in Table **??**.

Table 3 Relative Variances for the Individual Model Effects for the 15-Run D-optimal Design Shown in Table **??** [?].

Effect	Relative Variance
Intercept	0.077
A1	0.075
A2	0.069
A3	0.078
A4	0.084
B1	0.087
B2	0.063
B3	0.100
C1	0.070
C2	0.068
D	0.077
E	0.077

2.3 Summary

LVC simulation is a powerful experimental tool that has many benefits when testing systems in a joint mission environment. First, LVC experiments can significantly reduce the size of the experiment footprint while creating a sufficiently robust experiment environment. The number and diversity of assets that can be assembled in a distributed LVC simulation is far beyond the available resources at any single DoD test facility; at a fraction of the cost. Secondly, LVC simulation offers unparalleled flexibility and repeatability to execute test missions. Many of test entities are constructive (digital) and can be near-perfectly controlled thereby improving the repeatability of each run and increasing the precision of the effect estimates. Constructive and virtual (human-in-the-loop) entities can be created, moved, started, and stopped easily which allows insignificant events, such as takeoff and landing to be skipped saving time and potentially allowing more test runs.

Finally, the fidelity of LVC experiments can be scaled to match the requirements of the system's test. Scalability allows LVC use in tests at any level for the system under study in its operational environment. Some caution needs to be exercised when considering the desired level of fidelity for a LVC experiment. There lure of complexity is powerful and unwary experimenters may unknowingly confound effects because they fail to properly scope the experiment. By trying to answer all questions with a single high-fidelity LVC experiment the experimenter may find that they are unable to answer any questions at all!

In many ways LVC experiments are no different from purely live experiments; however, some aspects of the design must be considered more carefully to ensure test objectives can be met.

- **Changing the LVC Paradigm** LVC was initially conceived as a means of training joint combat forces in a realistic joint environment prior to employment in the operational theater. Little, if any, analytical planning is required to set up and execute these joint training exercises. Now that LVC is being considered for T&E the stakes have been raised and post-operation analytical planning must be a central component of designing LVC simulations for test.

- **Scoping the Experiment** This is perhaps the most difficult task in any experiment but the difficulty is amplified when conducting experiments with LVC. The number of objectives, environments, scenarios, entities, and data structures are seemingly endless. The size of the test space can quickly become overwhelming and paralyze experimental planning. Consequently, the experiment is either delayed and/or the LVC environment is over-built because the simulation developers try to consider everything in the absence of requirements certainty.

- **Mixed-Level Factors** LVC experiments are often comprised of mixed-level factors and small run sizes. This class of experiments is not taught in basic DOE courses and constructing experimental designs for them requires statistical rigor to ensure that test objectives can be met.

- **Qualitative Measures** Many of the objectives of the experiment are qualitative in nature and lack a straightforward response variable. Experimenters must ensure that proxy response variables are closely related to specific test objectives or risk wasting valuable resources and effort.

- **Increased Variability** The joint mission is extremely complex and contains copious sources of noise that must be carefully considered. Identifying and isolating the sources of noise should take a significant portion of experimental planning. This is especially true when human operators are part of the system under study.

In the next section each of the aforementioned characteristics of LVC experiments are considered and specific experimental designs are demonstrated showing that test objectives can be achieved in a complex joint mission environment.

3. Using Statistical Experimental Design to Realize LVC Potential in T&E[1]

3.1 Introduction

Live, virtual, and constructive (LVC) simulation is a test capability the Department of Defense (DoD) views as useful to test systems and system of systems in realistic joint mission environments. This DoD need for joint interoperability arose to prominence during the first joint operations conducted in Operation Desert Storm. Operation Desert Storm highlighted many interoperability issues clearly showing an incompatibility of systems across services [?]. The Secretary of Defense (SECDEF) directed use of a new capabilities-based approach to identify and fill gaps in the services' ability to carry out joint missions [?]. The SECDEF also mandated testing all joint systems in a joint mission environment thus exercising systems in their intended end-use environment. Collectively, this meant that future testing of systems be capability focused [?].

The Joint Test Evaluation Methodology (JTEM) project was established by the Director of Operational Test and Evaluation (DOT&E) in response to the SECDEF's mandate. JTEM was chartered to investigate, evaluate, and make recommendations to improve test capability across the acquisition life cycle using realistic joint environments. One result of JTEM's efforts was the Capability Test Methodology (CTM). The CTM provides "best practices" yielding a consistent approach to describing, building, and using an appropriate representation of a joint mission environment across the acquisition life cycle. The CTM enables testers to effectively evaluate system contributions to system-of-systems performance, joint task performance, and joint mission effectiveness [?].

The CTM focuses on the materiel aspects of the system as well as all aspects of doctrine, organization, training, materiel, leadership and education, personnel, and facilities (DOTMLPF). Considering all these joint capability requirements significantly

[1] This chapter has been submitted as a regular journal paper to the *ITEA Journal*.

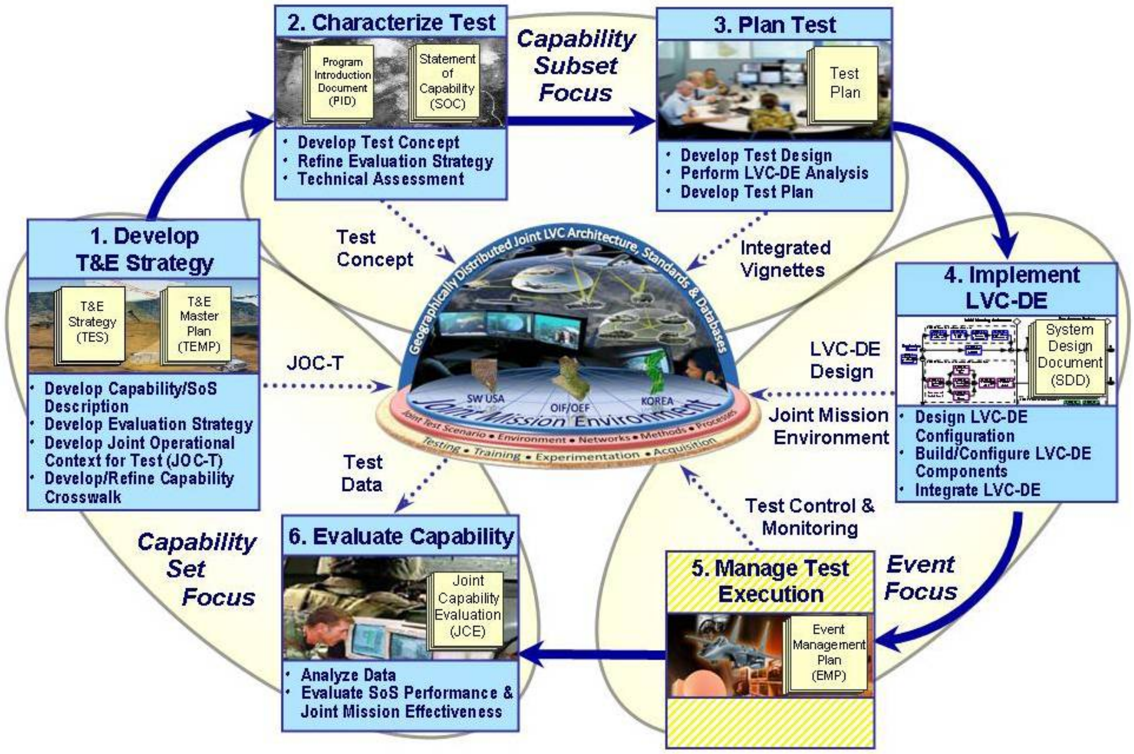

Figure 2 Capability Test Methodology [?]

impact the complexity of the T&E process. To meet the challenge of this increase in complexity, the CTM Analyst Handbook notes that future tests will require innovative experimental design practices as well as a distributed LVC test environment to focus limited test resources [?].

LVC is key to realizing the CTM [?]. LVC can connect geographically dispersed test facilities over a persistent computer network. LVC can also create the necessary variety or diversity (number of different systems) and density (number of each system) of assets representative of a joint environment; creating such a joint environment in actual practice would present logistical and cost nightmares. Figure ??, from the CTM Handbook [?], illustrates the central role LVC plays in the CTM. LVC simulations are well suited to experimentation throughout the acquisition life cycle. Early in system development, relatively simple joint mission environments may involve mostly constructive entities. Live and virtual entities may be added as the subsequent maturity of the system warrants. Cost is yet another reason that LVC is being pursued as a core test capability. LVC is cost effective. While not inexpensive, LVC cost will likely remain a far cheaper alternative to live joint mission experiments. Furthermore, LVC simulation also facilitates examining joint mission scenarios of greater complexity than likely attainable at any single DoD test facility.

3.2 Live-Virtual-Constructive Simulation

LVC simulations are software systems that create an environment where multiple, geographically dispersed users interact with each other in real-time via a persistent network architecture [?]. For DoD, LVC involves entities from three classes of military simulations: live, virtual, and constructive. In a live simulation, real people operate real systems. A pilot operating a real aircraft for the purpose of training or testing is a live simulation. Real people operate simulated systems or simulated people operate real systems in a virtual simulation. A pilot in a mock-up cockpit operating a flight simulator represents a virtual simulation. In constructive simulations, simulated people operate simulated systems. LVC is really a hybrid simulation assembling various (or select) simulation applications distributed over some network and allowing them to that interact by sharing current state information through that network.

The LVC environment allows incorporation of available live system assets. Necessary system assets unavailable are representable via some virtual or constructive model. This provides the diversity of assets needed for a test. If available live system assets are too few in number, the LVC can augment those with either virtual or constructive representations. This provides the density of assets needed for a test.

LVC simulation, properly utilized, offers significant T&E capability. However, experimenters need to understand the limitations of LVC when designing LVC experiments. Using statistical-based experimental design techniques can increase the likelihood of efficiently collecting useful data. Statistical experimental design is a systematic design process that allows experimenters to plan, structure, conduct, and analyze experiments to produce valid, objective conclusions in complex test environments. Statistical experimental design gives experimenters a firm foundation for conducting LVC experiments. Section **??** discusses the benefits and challenges of conducting experiments with LVC. Section **??** provides an overview of the experimental design process followed by a summary of designs that are useful for LVC in **??**.

3.3 Experimental Benefits and Limitations of LVC Simulation

LVC simulations provide experimenters with capabilities not found in purely live system test environments. First, systems can be tested in robust joint environments at a fraction of the cost of live tests. Capabilities, systems, and processes can be examined while still conceptual or prior to purchase. This can thus reduce the time and cost of a test program. Consequently, the reduced cost of LVC experiments can sometimes allow for more experiments considering more design factors. More experiments over more factors means greater precision and broader insights from the test results.

The virtual and constructive elements provide flexibility in experimental design. Live experiments may introduce restrictions that do not exist in LVC. More specifically, a live experiment may require a split-plot experimental design to accommodate randomization restrictions while the LVC can employ the easier to analyze randomized designs. LVC also allows for greater control over the test environment. Increased experimental control improves the repeatability of the experiment potentially increasing the precision of the estimate of experimental noise. With the exception of live assets, all entities in the simulation experiment can be controlled with near-perfect precision which allows the analyst to scale the fidelity of the model as needed to suit the experimental objective.

Two important experimental limitations or considerations are the humans involved in the LVC and the sophistication of the LVC environment. Human operators may be a focus of the experiment or merely a component in the experiment. In either case, the human element can bias results so its role must be considered in the

LVC test design. The sophistication of the simulation needs to be justified by verifying that the LVC environment is only complex enough to adequately investigate the problem being studied. All other complexities should be abstracted out of the LVC environment so that clear insights and defendable conclusions can be drawn from the experiment. Additionally, the LVC environment is a simulation so verification and validation of that simulation to the real world is a must.

Designing experiments for LVC is not a simple process; creating the LVC environment and defining the LVC test event can be quite dynamic. Several experimental design issues must be addressed to fully realize the benefits of LVC. Some of these challenges are described below.

1. **Mixed Factor Levels and Limited Resources.** LVC experiment plans often contain many mixed-level, qualitative factors but the experiment is given only enough resources to collect data from a small sample size experiment. Mixed-level designs and small sample size traditionally do not mix well; mixed-level designs often require large designs and are more difficult to reduce to meet the small sample size constraint.

2. **Qualitative Objectives.** Test objectives in LVC experiments are often qualitative in nature, such as how effective is the system in a joint mission environment. One may argue this is a result of the common use of LVC for training or demonstration purposes. For the analytical purposes envisioned for T&E, responses need to be more quantitative such as measuring the percentage or absolute improvement of performing joint mission tasks for a new system or capability. Consequently choosing the quantitative response variables may not be a straightforward task. Surrogate measures may sometimes be needed to augment qualitative measures to ensure that the stated problem is adequately investigated by the experiment.

3. **Noisy Test Environments.** The joint mission environment contains copious sources of noise that must be carefully considered in the experimental design.

LVC instrumentation strategies can provide a means to measure noise levels and appropriate blocking strategies can be used to isolate subsequent error effects and avoid erroneous conclusions. One of the biggest contributors of noise in the LVC experiment is the human operator in live and virtual assets. Fortunately, the human factors and human-computer interaction research areas have long considered the human element so LVC test planners need to leverage those experiences.

4. **Data Collection.** LVC experiments produce an abundance of data that must be reduced and analyzed. Experimenters need to plan data collection methods carefully so that time and effort are not wasted collecting irrelevant system information. A complication in LVC is correlating quantitative data (e.g. system measurements) to qualitative assessments (e.g.questionnaires) to support or refute study hypothesis.

5. **Lure of complexity.** LVC is flush with capability, often enticing testers to create environments more complex than required to investigate the particular problem under study. Simulations can accommodate very large factor spaces. If the experimenter is not careful, factor effects can be confounded due to too many factors being included without thought as to how they are being included. LVC for T&E will require increased discipline in making the LVC environment ready for the test.

This list is by no means exhaustive but serves as a starting point to realizing experimental design for LVC analytical experiments. As such, we next adapt a well-known experimental planning approach to LVC.

3.4 Overview of Experimental Design

Experimental design provides a strategy to plan, collect and analyze appropriate experimental data using statistical methods to produce valid conclusions. Statistical designs are often necessary if meaningful conclusions are to be drawn from the experiment. If the system response is subject to experimental errors, then statistical methods are the most objective approach to the analysis. Often in test, the system response is reported as a point estimate (such as the mean response) when the individual responses are subject to a random component. This oversimplification of the system response can often lead to erroneous conclusions because the random component of the response is unaccounted for.

According to **?**, the three basic principles of statistical experimental design are randomization, replication, and blocking. Randomization is the cornerstone of statistical methods for experimentation. Statistical methods require that the experimental observations be independent. Randomization typically ensures that this assumption is valid. You can think of randomization as spreading the experimental error as evenly as possible over the entire set of runs. A replication is an independent repeat of some factor combination and provides an important benefit to experimenters; providing an independent estimate the pure error of the experiment. This error estimate is the basic unit of measurement for determining whether observed differences in the data are statistically different. In general, the more times an experiment is replicated the more precise the estimates of effects will be.

Blocking is a design technique that helps to improve the precision of estimates when comparisons among the factors of interest are made. Blocking measures the variability of nuisance factors in the experiment; factors that influence the outcome of the experiment but are not of direct interest in the experiment. To illustrate blocking, consider a flight test where two different operators are used in the experiment. The operators themselves are not of interest to the experiment but experimenters are concerned that differences between the performance of the operators may confound the results and lead to erroneous conclusions. To overcome this, the operators are assigned to two separate blocks of test runs. By assigning the operators to blocks, any variability between operators introduced can be estimated and and removed from the estimate of experimental error thereby yielding better insights into factors of statistical significance.

Table 4 Common Objectives for Experiments

Objective	Type of System	Rationale for usage
System Characterization	New system	Little understanding how control variables affect system response
Optimization	Mature System	Seek control settings for best system response performance
Robustness	Mature System	Seek control settings to reduce system response variation from noise

To apply statistical methods to the design and analysis of experiments, the entire test team must have a clear understanding of the objectives of the experiment, how the data is to be collected, and a preliminary data analysis plan prior to conducting the experiment. **?** propose guidelines to aide in planning, conducting, and analyzing experiments. An overview of their guidelines is given as follows:

1. **Recognition and statement of the problem.** Every good experimental design begins with a clear statement of what is to be accomplished by the experiment. While it may seem obvious, in practice this is one of the most difficult aspects of designing experiments. It is no simple task to develop a clear, concise statement of the problem that everyone agrees on. It is usually necessary to solicit input from all interested parties: engineers, program managers, manufacturer, and operators in this phase. At a minimum a list of potential questions and problems to be answered by the experiment should be prepared and discussed among the team to ensure their alignment with the objective of the experiment. Some common experiment objectives are given in Table **??**.

   At this early stage in experimental planning it is important to remember that one big experiment that seeks to answer all questions often results in adequately answering none. A single comprehensive experiment requires the experimenter to know the answers to many of the questions about the system in advance. This kind of system knowledge is unlikely in the early stages of system development. The single large experiment also means greater complexity of the LVC, greater stress on instrumentation to collect response data, and more assumptions of how this LVC instance relates to the actual system of interest. If the experimenters make assumptions about the system that are wrong can lead to inconclusive results. A sequential approach using a series of smaller experiments, each focusing on a specific objective, is a better test strategy towards achieving meaningful results.

2. **Selection of the response variable.** When selecting any response variable, the experimenter should be sure that it provides useful information about the system under study. It is critical to identify issues associated with collecting a response variable and how it is to be measured before conducting the experiment. Choosing a response variable that directly measures the problem being studied is naturally the best response option. When a direct response is unobtainable, a surrogate measure may be used. Experimenters must ensure that the surrogate adequately measures how the system performs relative to the problem being studied and clearly define that measures use in achieving experimental design objectives.

3. **Choice of factors, levels, and range.** When considering which factors influence the experiment two categories of factors frequently emerge: design and nuisance factors. Design factors can be controlled by the design of the system or the operator during system use. Nuisance factors affect the response of the system but are not of particular interest to experimenters. The simulation environment is often a source of nuisance factors in LVC experiments. Nuisance factors can be controllable, uncontrollable, or noise factors. Blocking and measurement are design techniques used to accommodate the effect of nuisance factors when designing the experiment. Techniques for accommodating nuisance factors are found in **?**, or any other quality text on experimental design.

   After choosing the factors for the experiment it is important to identify the number of settings or levels of each factor to consider. Quantitative factors with a continuous range are usually well represented by two levels and center points if system response is suspected to involve curvature, or nonlinearity. When factors are qualitative the number of levels are generally fixed to the number of categories employed since there is no effective way to reduce the number of factor levels without losing the ability to make inferences about that category's effect on the system response. The range of factors level settings must be carefully considered in the design process because the range directly affects the variability of the predictions. Factor levels that are too narrowly spaced can miss important response changes while factor levels that are too wide risk having insignificant effects appear to be active. A subject matter expert is invaluable when determining the range of factors levels to use.

4. **Choice of experimental design.** Choosing the particular experiment design builds upon the efforts to date. Choosing a design involves considering the sample size, selecting a random run order, and deciding whether blocking is necessary or not. Give the number of factors, levels, and ranges, various software packages can easily help to generate and refine alternative designs to consider. Design team members should keep the experiment objectives in mind when choosing the design to actually implement.

5. **Performing the experiment.** Experimenters are most familiar with this step. In this step it is vital to ensure that the experiment is conducted as planned. Conducting trial runs prior to the actual experiment helps to test methods and equipment, assess planning adequacy, and even assess expected results from the experiment.

6. **Statistical analysis of the data.** If the experiment was designed and executed correctly the statistical analysis should follow planned approaches. Often software packages that are used to generate the design can be used to seamlessly analyze the experiment. Hypothesis testing and confidence interval estimation procedures are useful in analyzing data from a designed experiment. Common analysis techniques include analysis of variance (ANOVA), regression, and mul-

tiple comparison techniques and provide a means for the design team to present results more meaningful than simply a point estimator.

7. **Conclusions and recommendations.** A well designed experiment is meant to answer a specific question or set of questions. Hence, the experimenter should draw practical, defendable conclusions from the results of the experiment. The beauty of a well designed and executed experiment is that once the data have been analyzed the interpretation of the data is based on sound and fully defendable statistical principles.

? give more details on the steps of experimental design for the interested reader. Additionally, **?**'s *Design and Analysis of Experiments* builds on those guidelines as part of its complete coverage of statistical experimental design. Note, the above guidelines ignore the myriad of details that go into preparing the LVC, and its components, scheduling the resources, and garnering experimental support. The guidelines focus just on preparing the design of the LVC experiment.

3.5 Using Experimental Designs for LVC

Thus far, we have discussed LVC for test, identified some unique challenges to using LVC for analysis and summarized a systematic approach to planning for the LVC experiment. Now we turn our attention to assessing four alternative classes of experimental designs for their suitability for use in LVC experiments. These design alternatives are given along with some rationale for their use. Three are randomized designs while the fourth accommodates restrictions on randomization.

3.5.1 Completely Randomized Designs. The flexibility of LVC experiments can sometimes allow the use of simpler completely randomized designs in situations where a comparable live system test in a real environment would have restrictions. Orthogonal arrays (OA), Nearly Orthogonal Arrays (NOA), and optimal designs are excellent design choices for LVC experiments when randomization is unrestricted.

3.5.1.1 Orthogonal Arrays. OAs have significant potential for LVC experiments as they can accommodate mixed-level factors while maintaining the economical run size necessary in most LVC experiments. An array is defined as fully orthogonal if each column of the array is orthogonal to every other column in the array. This orthogonality yields independence between the columns and their resulting effect estimates. For example, consider an experiment with a three-level factor and four two-level factors where testing resources only allow for 12 runs. A full factorial design would require 48 (3×2^4) runs and reducing the design in a fractional factorial mantter would be quite complicated. An orthogonal array can be constructed with 12 runs and will allow independent estimates of each of the 5 main effects. Table **??** is one such OA. In Column A, 0,1,2 represent the low, middle, and high values of the factor while in the other columns, 0 represents a low factor level setting and 1 a high factor level setting. These are standard level coding approaches.

In the early stages of experimental planning it is often necessary to assume that not all factors being examined will significantly affect the system under study [**?**]. This assumption is based on the well-known *sparsity of effects* principle which presumes that only a few factors will be active in an experiment where many factors are considered and of those, the lower order effects will drive system response. An important consequence of this principle is that factors can be dropped from the model (and subsequent analysis) when initial analysis reveals those factors are inactive. In experimental design, as factors are dropped from the experiment, we can reuse the data already collected to provide a clearer picture of the remaining factors. This is a projection of the smaller design in many factors into a stronger design in fewer factors. When factors are dropped from an OA having good projection properties, the stronger design can estimate factor interactions along with the main effects. All OAs estimate the main effects equally well but not all OAs have equal projection properties. Consequently, when considering OAs for an experiment, the experimental team not only ensures the OA has good projection properties, but those projection

Table 5 OA($12, 3 \times 2^4$). OA with largest number of two-level factors and one three level factor with 12 runs.

Factors					
Run	A	B	C	D	E
1	0	0	0	0	0
2	0	0	1	0	1
3	0	1	0	1	1
4	0	1	1	1	0
5	1	0	0	1	1
6	1	0	1	1	0
7	1	1	0	0	1
8	1	1	1	0	0
9	2	0	0	1	0
10	2	0	1	0	1
11	2	1	0	0	0
12	2	1	1	1	1

[?]

Table 6 NOA($12, 3 \times 2^6$). Orthogonality was lost by adding two more two-level factors, F and G, to the orthogonal array OA($12, 3 \times 2^4$) in Table 2.

Factors							
Run	A	B	C	D	E	F	G
1	0	0	0	0	0	0	0
2	0	0	1	0	1	0	1
3	0	1	0	1	1	1	1
4	0	1	1	1	0	1	0
5	1	0	0	1	1	1	0
6	1	0	1	1	0	1	1
7	1	1	0	0	1	0	0
8	1	1	1	0	0	0	1
9	2	0	0	1	0	0	1
10	2	0	1	0	1	1	0
11	2	1	0	0	0	1	1
12	2	1	1	1	1	0	0

properties are strong in the most likely projection directions, which are those factors deemed most likely active during the experimental planning process.

LVC accommodates testing throughout the entire life cycle of systems that operate in a joint environment. Orthogonal arrays are well suited for factor screening experiments early on in the system life cycle where little is known about the system and we want to drop inactive factors. The projection property of OAs make them an efficient approach to gain information about the active effects and interactions and to build upon that information in the sequential nature of weapon system life cycle testing.

3.5.1.2 Nearly Orthogonal Arrays. Sometimes orthogonal arrays cannot sufficiently reduce the run size while accommodating the necessary number of $k \geq 2$ level factors. One option is to increase the run size, which may not be feasible due to resource restrictions. **?** show that a 12-run orthogonal array $OA_{12}(3^1, 2^k)$

exists for $k \leq 4$ but for $k = 6$ no such orthogonal array exists. In this case, an option is to relax the orthogonality requirement and use a NOA [?]. Researchers such as **?**, **?**, and **?** have constructed NOAs using various algorithmic approaches to the design construction.

A consequence of relaxing the orthogonality requirement in the design matrix is a less precise estimate of the error in the experiment. The error estimate is actually biased high due to the correlation between the columns of the design matrix resulting from the non-orthogonality. This bias means some caution should be exercised when using NOAs. A less precise estimate of the error can cause some active factor effects to be declared inactive if their effect is relatively small (the inflated error hides the active factor causing a Type 1 error). Another consequence of using NOAs is that the data analysis and interpretation becomes more difficult when compared to the OA design. Table **??** is an NOA for 12 runs to examine 7 factors; the coding scheme is the same as used in Table **??**.

3.5.1.3 Optimal Designs. Optimal designs are so named because their nearly orthogonal design is constructed to optimize some evaluation criteria of the design. Optimal designs are an excellent way to construct mixed level designs with D-optimal being the most widely used design. **?** demonstrated the potential use of optimal designs in wind-tunnel experimentation. The D-optimal criterion maximizes the overall degree of orthogonality of the design matrix. Two popular alternatives are the A and G-optimal design criteria. The A-optimal design criterion minimizes the degree of correlation between the columns of the design matrix. The G-optimal criterion minimizes the maximum prediction variance and is useful if a regression model built from the experimental data is to be used to make predictions about the system response.

? gives the following example involving a D-optimal design. Consider an experiment with five factors: A is categorical with five levels, B is categorical with four levels, C is categorical with three levels, and D and E are continuous with two levels

Table 7 A 15-Run D-optimal Mixed-level Design for Five Factors

	Factors				
Run	Factor A	Factor B	Factor C	Factor D	Factor E
1	L4	L2	L1	1	1
2	L1	L1	L3	1	1
3	L5	L4	L2	1	1
4	L3	L3	L2	1	0
5	L4	L1	L2	0	0
6	L2	L4	L3	1	0
7	L1	L4	L1	0	0
8	L5	L2	L3	0	0
9	L3	L2	L3	1	0
10	L3	L1	L1	0	1
11	L2	L2	L2	0	1
12	L4	L3	L3	0	1
13	L5	L3	L1	1	0
14	L1	L2	L2	1	0
15	L2	L1	L1	1	0

L_i defined as level i of the associated factor

each. Estimates of all of the main effects are desired. An orthogonal, full-factorial design requires 240 runs; however, this approach is terribly inefficient at estimating the main effects. The one-half, one-quarter, and one-eighth fraction designs would require 120, 60, and 30 runs, respectively, are not orthogonal, and still require too many runs to be considered efficient designs. A 15-run, D-optimal design (such as shown in Table **??**) is nearly balanced[2] and has nearly uniform relative variance (variance divided by σ^2). The relative variances for the individual model effects for the 15-Run D-optimal design are nearly uniform; a desired property in optimal and nearly orthogonal designs. The relative variances are shown in Table **??**. One drawback to D-optimal designs is that the user must specify the model (i.e. which factor effects and interactions to estimate) prior to experimentation. This misspecification can transmit bias to the effect estimates and lead to incorrect conclusions. **?** discuss

[2]A design is balanced if each level combination occurs equally often

Table 8 Relative Variances for the Individual Model Effects for the 15-Run D-optimal Design Shown in Table ?? [?].

Effect	Relative Variance
Intercept	0.077
A1	0.075
A2	0.069
A3	0.078
A4	0.084
B1	0.087
B2	0.063
B3	0.100
C1	0.070
C2	0.068
D	0.077
E	0.077

model misspecification as well as other design criteria when considering D-optimal designs for factor screening.

3.5.2 Design for Randomization Restrictions. Split-plot designs are used when there are restrictions on complete randomization. These restrictions can be caused by a variety of factors such as the presence of hard-to-change (HTC) factors, human factors limitations, or in the case of Robust Product Design (RPD), even the objectives of the experiment. These restrictions make a completely randomized design inappropriate and can lead the experimenter to erroneous conclusions if the data is analyzed in a manner inconsistent with the design and execution of the experiment [?]. In split-plot designs, HTC factors are assigned to a larger experimental unit called the whole plot while all other factors are assigned to the subplot. Sub-plots are fully randomized within the whole-plot where they are placed. **?** state that in the presence of HTC factors, a split-plot design can significantly increase the ease of experimentation and can save precious time and resources. A side benefit of some split-plot designs is that they typically require fewer runs than a completely

Table 9 A 2^5 split-plot design matrix with whole-plot factors (A, B) and sub-plot factors (c, d, e)

	Factors				
	Whole Plot		Sub-Plot		
Run	A	B	c	d	e
1	0	0	0	0	0
2			1	0	0
3			0	1	0
4			0	0	1
5	1	0	1	0	1
6			0	1	1
7			0	1	1
8			1	1	1
9	0	1	0	0	0
10			1	0	0
11			0	1	0
12			0	0	1
13	1	1	1	0	1
14			0	1	1
15			0	1	1
16			1	1	1

randomized design. The complication with the split-plot design is the more complex error structure.

In experiments where humans are part of the system under study, such as will almost always be the case in LVC, it can be advantageous to change some factors less often than others to prevent human operator confusion resulting in biasing the estimated error. For example, consider a flight test experiment focused on studying the effect of certain radar operation procedures under a variety of operational settings. Depending on the complexity of the procedures, the potential for operator error can increase if procedures change between each run. A better estimate of the procedure effects could be obtained if the operator were to operate the radar with one set of procedures before moving to the next. All other factors potentially effecting radar operations are assigned to the subplot with the schedule of runs completely randomized within that subplot.

Consider the split-plot design with five factors at two levels each (Low, High) in Figure ??. The HTC factors A, and B are assigned to the larger experimental unit, called the whole plot, and the easy to change factors c, d, and e are assigned to the smaller experimental unit, called the subplot. The split-plot experiment is run by randomly selecting a whole plot and then randomly running each design point within that whole plot. This design results in two independent error terms, one for the whole plot and one for the sub-plot [?]. The whole plot error has fewer degrees of freedom than the subplot since it contains fewer randomized runs. This means that less precise estimates can be made of factor effects for factors assigned to the whole plot. Consequently, the most important factors should be assigned to the sub-plot whenever possible [?].

In some circumstances the most important factors must be assigned to the whole-plot and a more precise estimate of the whole plot factors is needed. ? propose a hybrid method that falls between a completely randomized design and split-plot design in terms of factor level changes. This design changes the HTC factors more frequently creating more whole plots thus increasing the degrees of freedom available to estimate the whole plot effects and the whole plot error. They list six benefits to this hybrid approach.

1. The statistical efficiency of the experiment is increased.
2. Increasing the number of level changes protects against systematic errors if something goes wrong at a HTC factor level.
3. An increased number of whole plots ensures an improved control of variability and provides better protection against trend effects.
4. More degrees of freedom are available for the estimation of the whole plot error.
5. An increased number of HTC factor level changes allows a more precise estimation of the coefficients corresponding to these factors.
6. The number of factor level changes is generally smaller than a completely randomized design.

? present an algorithm for constructing these D-optimal, split plot designs.

3.5.3 General LVC Designs. It may occur that an LVC experiment can employ more traditional experimental designs such as factorial or fractional factorial designs. Team planning will help decide upon the best choice of design. Our intent in this paper was to discuss non-standard designs that may be best suited to particular LVC experiments.

3.6 Summary

LVC environments offer an increasingly attractive option for testing systems in a joint mission environment. Using LVC technologies means testers can build large scale operationally representative joint environments that are otherwise unobtainable and potentially supplant some operational tests that are well represented in LVC. While LVC has great potential for T&E purposes there are unique challenges that arise when using LVC for analytical purposes. These challenges must be addressed to make effective use of LVC capabilities for T&E. The breadth and depth of capability offered by LVC can potentially make it difficult to scope experiments down to manageable sizes. There is also a strong lure towards building unnecessarily complex test environments whose unrealistic goal is to answer all system questions concurrently. The preferred approach is to answer system questions in smaller sets with a series of smaller experiments. However, since LVC-based experiments are more complex than traditional system-centric tests, they may require the use of innovative experimental designs to capture relevant system information to support the analysis required from the test; we discussed four such designs in this paper.

Statistical experimental design is a structured approach to designing experiments conducted in complex environments. The three principles of experimental design, randomization, replication, and blocking allow experimenters to improve the precision of effect estimates and isolate the experimental error from variation due to changing factor levels. Statistical designs ensure that the necessary assumptions are satisfied to allow experimenters to make valid inferences about system data. The complexity of joint mission environments introduces copious sources of random error into the experiment requiring that experiments be designed using statistical methods. These methods can greatly improve the quality of system information collected from LVC experiments and increase the experimental efficiency. There are numerous statistical designs that are available to experimenters. The specific choice of design is dictated by test objectives, available resources, and constraints. By using statistical design methods LVC users can improve their ability to make inferences on the test data and draw objective conclusions about the systems performance and mission effectiveness in a joint environment.

Disclaimer: The views expressed in this article are those of the author and do not reflect the official policy or position of the United States Air Force, Department of Defense or the U.S. Government.

4. Planning for Experiments Using LVC[1]

4.1 Introduction

Live, virtual, and constructive (LVC) simulation is a test capability being pursued by the Department of Defense (DoD) to test systems and system of systems in realistic joint mission environments. The DoD was made acutely aware of the need for designing and testing systems in a joint environment during the first joint operations conducted in Operation Desert Storm. Operation Desert Storm highlighted a host of interoperability issues, namely that systems across services were incompatible with one another [?]. The Secretary of Defense (SECDEF) responded by mandating a new capabilities based approach to identify gaps in services' ability to carry out joint missions and fill those gaps with systems designed with joint missions in mind [?]. Additionally, the SECDEF mandated that all joint systems be tested in a joint mission environment so that systems can be exercised in their intended end-use environment. This implies that future testing of systems be capability focused [?].

In response to the SECDEF's mandate, the Director of Operational Test and Evaluation (DOT&E) set up the Joint Test Evaluation Methodology (JTEM) project. The purpose of JTEM was to investigate, evaluate, and make recommendations to improve test capability across the acquisition life cycle in realistic joint environments. One result of JTEM's efforts was the development of the capability test methodology (CTM). CTM is a set of "best practices" that provide a consistent approach to describing, building, and using an appropriate representation of a joint mission environment across the acquisition life cycle. The CTM enables testers to effectively evaluate system contributions to system-of-systems performance, joint task performance, and joint mission effectiveness [?].

CTM is unique in that it focuses not only on the materiel aspects of the system but also on aspects of doctrine, organization, training, materiel, leadership and education, personnel, and facilities (DOTMLPF). The inclusion of these joint capability

[1] This chapter has been submitted as a journal article to *Systems Engineering*.

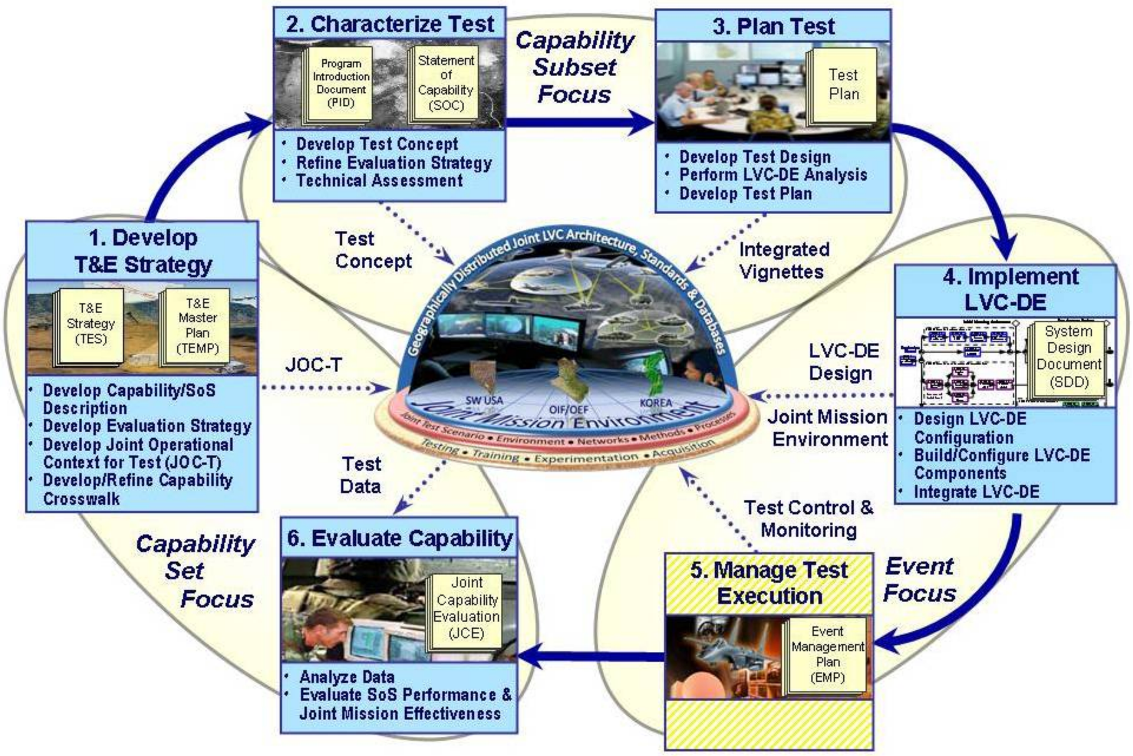

Figure 3 Capability Test Methodology [?]

test requirements add significant complexity to the T&E process. Because of this increase in complexity, the CTM Analyst Handbook states that future tests will require innovative experimental design practices as well as the use of a distributed LVC test environment to focus limited test resources [?].

LVC is a central component of CTM due to its ability to connect geographically dispersed test facilities over a persistent network and potentially reduce test costs. LVC is able to create the necessary variety and density of assets representative of a joint environment. Figure ??, from the CTM Handbook [?], illustrates the centrality of LVC to CTM. LVC simulations can scale to different levels of fidelity thus making LVC well suited to experiments across the acquisition life cycle. Simple joint mission environments can be developed using mostly constructive entities in the early stages of system development with live and virtual entities added as the system matures.

Cost is yet another reason that LVC is being pursued as a core test capability. While the cost of LVC experiments can be significant, it often remains a cheaper alternative to joint mission experiments using only live assets. Furthermore, LVC simulation can build joint mission scenarios of greater complexity than can be assembled at any single DoD test facility.

4.1.1 Live-Virtual-Constructive Simulation.

? defines LVC simulations as software systems that create an environment where multiple, geographically dispersed users interact with each other in real-time via a persistent network architecture. LVC is a collection of entities from three classes of simulations: live, virtual, and constructive. In a live simulation, real people operate real systems. A pilot operating a real aircraft for the purpose of training under simulated operating conditions is a live simulation. In a virtual simulation, real people operate simulated systems or simulated people operate real systems. A pilot in a mock-up cockpit operating a flight simulator is a well-known example of virtual simulation. In constructive simulations, simulated people operate simulated systems. LVC is a hybrid simulation environment assembled from a collection of autonomous distributed simulation applications that interact by sharing current simulation state information over a network.

LVC simulations have the potential to provide experimenters with several benefits not found in purely live system tests. First, systems can be tested in robust joint environments at a fraction of the cost of using only live assets. Test ranges, threats, emitters, and conceptual next-generation capabilities can be included in the simulation without purchasing the live asset. These assets are expensive and their specific inclusion could significantly increase the cost of a test program using just a live system test. The reduced cost of LVC experiments can sometimes allow for more runs and consideration of more design factors when cost is the limiting resource. More runs using LVC can result in more information than could be obtained in a similar test only utilizing live assets.

The virtual and constructive elements of LVC give experimenters increased flexibility in designing the experiment. Statistical experiments are founded on completely randomizing the order of the experiments. Split-plot designs provide approaches when complete randomization is restricted. In some situations completely randomized designs can be used in the LVC instead of the more complex split-plot designs often found in live test because the virtual and constructive elements can be easily reconfigured before each run. An important caveat is to use caution when changing virtual and constructive elements if humans are active in the experiment; changing test conditions too often can lead to operator confusion and introduce bias in the results.

Another benefit of LVC is that it allows the user to exercise greater control over the test environment. Increased control improves the repeatability of the experiment potentially increasing the precision of the estimate of the experimental error used when making statistical statements regarding the results. Reduced experimental error also means more precise effect estimates for the active factors in the experiment. With the exception of live assets, all entities in the simulation experiment can be controlled with greater precision which allows the analyst to scale the fidelity of the model as needed to suit the experimental objective.

The LVC environment is also fairly easy to instrument. This provides an improved capability to gather data to support decisions pertaining to the test objectives. The design team does, however, need to spend time evaluating potential measures and implementing only those needed.

4.1.2 Change the LVC Paradigm. The LVC concept was introduced to the DoD by the Joint National Training Center, which was established in January 2003 to provide war fighters across all services training opportunities in a realistic joint mission environment [?]. In a training environment large, complex, noisy environments are preferred because it appropriately prepares soldiers for the "fog of war". Further, training outcomes do not always require quantitative-based, objective results. For analytical purposes such as test, "fog" is a detriment because it obscures the underlying factors that are driving system performance and effectiveness. In test, we want to abstract out certain parts of the representative environment so that we can identify the factors that affect the system in its end-use environment. If LVC is going to be successfully implemented as a core test capability LVC practice will require a fundamental shift from the way LVC users currently employ the technology and towards a paradigm in which the LVC generates quantitative-based, analytically defendable results.

If LVC simulation is properly utilized it offers significant test capability to T&E practitioners. Care must be taken to ensure that users understand the limitations of LVC or risk collecting useless data. Statistical experimental design techniques greatly increase the likelihood of collecting useful data and doing so in an efficient manner. Statistical experimental design is a methodical design process that plans, structures, conducts, and analyzes experiments to support objective conclusions in complex test environments. Statistical experimental design gives experimenters a firm foundation for conducting LVC experiments but its use represents a fundamental shift in how LVC is used currently. In Section **??** we give an overview of the experimental design process and a summary of designs useful for LVC. In Section **??** we discuss additional considerations for conducting experiments with LVC. Lastly, a case study is presented to illustrate the benefits of experimental design for LVC experiments in Section **??**.

4.2 The Statistical Experiment Design Process

Experimental design is a strategy of experimentation to collect and analyze appropriate data using statistical methods resulting in statistically valid conclusions. Statistical designs are quite often necessary if meaningful conclusions are to be drawn from the experiment. If the system response is subject to experimental errors then statistical methods provide an objective and rigorous approach to analysis. Often in test, the system response is measured as a point estimate (such as the mean response) when the individual responses are actually subject to a random component. Oversimplifying the system response can often lead to erroneous conclusions because the random component of the response is unaccounted for.

The three basic principles of statistical experimental design are randomization, replication, and blocking [**?**]. Randomization is the cornerstone of statistical methods. Statistical methods require that the run-to-run experimental observations be independent. Randomization typically ensures that this assumption is valid. Randomization also spreads the experimental error as evenly as possible over the entire set of runs so that none of the effect estimates are biased by experimental error. A replication is an independent repeat of each factor combination and provides two important benefits to experimenters. Replication provides an unbiased estimate the pure error in an experiment. This error estimate is the basic unit of measurement for determining whether observed differences in the data are statistically different. More precise effect estimates is another benefit of replicatoin. In general, the more times an experiment is replicated the more precise the estimates of error will be and any inferences pertaining to factor effects will be more informed.

Blocking is a design technique that improves the precision of estimates when comparing factors. Blocking controls the variability of nuisance factors; factors that influence the outcome of the experiment but are not of interest in the experiment. To illustrate blocking, consider a machining experiment where two different operators are used in the experiment. The operators themselves are not of interest to the experiment but experimenters are concerned that any differences between the operators may confound the results and lead to erroneous conclusions. To overcome this, the operators are assigned to two separate blocks of test runs. By assigning the operators to blocks any variability between operators can be estimated and those effects removed from the experimental error estimates, thus increasing overall experiment precision.

A statistical experiment design process for LVC must not only consider the three basic principles of statistical experiment design, but also include considerations such as:

- models, simulations and assets used in the experiment;
- scenarios considered during the experiment;
- factors that change each run and how to control those that do not change;
- the fidelity of models and simulations used; and
- how human operators might influence results.

The above complications truly call for an LVC experimental design process.

4.2.1 An Experimental Design Process. To apply statistical methods to the design and analysis of experiments, an entire test team must have a clear understanding of the objectives of the experiment, how the data is to be collected, and a preliminary data analysis plan prior to conducting the experiment. **?** propose guidelines to aide in planning, conducting, and analyzing experiments. An overview of their guidelines follow, keep in mind these guidelines pertain only to the development of the experimental plan, not the myriad of other factors that arise when planning and coordinating the resources for actual experiments. These guidelines are useful for defining an LVC-experiment design process.

1. **Recognition and statement of the problem.** Every good experimental design begins with a clear statement of what is to be accomplished by the experiment. While it may seem obvious, in practice this is one of the most difficult aspects of designing experiments. It is no simple task to develop a clear, concise statement of the problem that everyone agrees on. It is usually necessary to solicit input from all interested parties: engineers, program managers, manufacturer, and operators. At a minimum a list of potential questions and problems to be answered by the experiment should be prepared and discussed among the

Table 10 Common Objectives for Experiments

Objective	Type of System	Rationale for usage
System Characterization	New system	Little understanding how control variables affect system response
Optimization	Mature System	Seek control settings for best system response performance
Robustness	Mature System	Seek control settings to reduce system response variation from noise

team. It is helpful if not necessary to keep the objective of the experiment in mind. Some common experiment objectives are given in Table **??**.

At this stage it is important to formulate large problems into a series of smaller experiments each answering a different question about the system. A single comprehensive experiment often requires the experimenter to know the answers to many of the questions about the system in advance. This kind of system knowledge is sometimes unlikely and the experiment often results in disappointment. If the experimenters make incorrect assumptions about the system, the results could be inconclusive and the experiment wasted. A sequential approach using a series of smaller experiments, each with a specific objective, is a superior test strategy.

2. **Selection of the response variable.** The response variable measures system response as a function of changes in input variable settings. A good response variable provides useful information about the system under study as it relates to the objectives of the experiment. Test planners need to determine how to be measure response variables before conducting the experiment. The best response variables directly measures the problem being studied. Sometimes a direct response is unobtainable and a surrogate measure must be used instead. When surrogate measures are used test planners must ensure that the surrogate adequately measures how well the system performs related to the objectives and

the system is properly instrumented to capture the surrogate measure information.

3. **Choice of factors, levels, and range.** Factors are identified by the design team as potential influences on the system response variable. Two categories of factors frequently emerge: design and nuisance factors. Design factors can be controlled by either the design of the system or the operator during use. Nuisance factors affect the response of the system but are not of particular interest to experimenters. Often nuisance factors are environmental factors. Blocking is a design technique that can be used to control the effect of nuisance factors on an experiment. For more details on techniques that deal with nuisance factors see **?**.

   After choosing the factors it is necessary to choose the number of levels set for each factor in the experiment. Quantitative factors with a continuous range are usually well represented by two levels but more levels often arise in the more complex, comprehensive designs. When factors are qualitative the number of levels are generally fixed to the number of qualitative categories. Unlike continuous factors, there is no way to reduce the number of factor levels for categorical factors without losing the ability to make inferences on that level's effect on system response. The range of factors levels must also be carefully considered in the design process. Factor levels that are too narrowly spaced can miss important active effects while factor levels that are too wide can allow insignificant effects to drive the system response. A subject matter expert working in conjunction with the statistical experimental design expert is invaluable when choosing the range of factors levels.

4. **Choice of experimental design.** Choosing an experimental design can be relative easy if the previous three steps have been done correctly. Choosing a design involves considering the sample size, randomizing the run order, and deciding whether blocking is necessary. Software packages are available to help generate alternative designs given the number of factors, levels, and number of

runs available for the experiment. More unique designs like orthogonal arrays and nearly orthogonal arrays can be created with available computer algorithms. Some good resources for creating unique designs are given in section **??**.

5. **Performing the experiment.** In this step it is vital to ensure that the experiment is being conducted according to plan. Conducting a few trial runs prior to the experiment can be helpful in identifying mistakes in planning thus preventing a full experiment from being wasted. While tempting, changing system layouts or changing factors during the course of an experiment, without considering the impact of those changes, can doom and experiment.

6. **Statistical analysis of the data.** If the experiment was designed and executed correctly the statistical analysis need not be elaborate. Often the software packages used to generate the design help to seamlessly analyze the experiment. Hypothesis testing and confidence interval estimation procedures are very useful in analyzing data from designed experiments. Common analysis techniques include analysis of variance (ANOVA), regression, and multiple comparison techniques. A common statistical philosophy is that the best statistical analysis cannot overcome poor experimental planning. The important aspect of statistical analysis is to involve the professional statistician for the analysis.

7. **Conclusions and recommendations.** A well designed experiment is meant to answer a specific question or set of questions. Hence, the experimenter should draw practical conclusions about the results of the experiment and recommend an appropriate course of action. The beauty of a well designed and executed experiment is that once the data have been analyzed the interpretation of the data should be fairly straightforward, objective and defendable.

? give details on the steps of experimental design. Additionally, most texts on experimental design, including **?**, provide some experimental design methodology.

4.2.2 Additional design considerations for LVC. The **?** guidelines offer comprehensive general guidelines for industrial experiments. However, LVC experiments are non-industrial representing a more dynamic process. There are several experimental design issues that need to be addressed before the benefits of LVC can be fully realized.

1. **Scoping the Experiment.** Scoping LVC experiments require more careful treatment than most traditional experiments. LVC is flush with capability; users and experimenters can build very large, complex, joint mission environments. Experimenters are often enticed to create environments that are more complex than required to actually satisfy the experiment's objective When these LVC environments are used for analytical purposes, such as the case in T&E, more discipline must be exercised to ensure the test environment is not overbuilt but remains constructed to align with the analytical objectives. LVC has enormous data generation capability making the number of possible problems that can be researched significantly larger than that of live asset tests. An LVC builder can instrument just about any process included in the environment. Experimenters are faced with vast alternatives to choose from when designing the experiment. This means planners have to say no to investigating some interesting problems and investigate only those that are most important.

   Over-scoping the experiment not only affects the quality of data garnered from the experiment but also leads to delays in experiment execution. LVC simulation developers work off of the requirements supplied by the test team; if too many requirements are demanded then developers can become task saturated and unable to deliver the LVC environment in time for the test event. Breaking the experiment up into a series of smaller experiments that build on each other can improve the experiment data quality and increase the likelihood of meeting test deadlines. When used for training or assessments, increased complexity in the LVC environment has become accepted. When used for analytical insight this same increased complexity can ruin any meaningful results.

2. **Qualitative Objectives.** Objectives in LVC experiments are often qualitative in nature. LVC is used primarily for joint mission tests to evaluate system-of-systems performance, joint task performance, and joint mission effectiveness. Nebulous qualities such as task performance and mission effectiveness are often difficult to define and measure. More often than not there are no direct metrics to quantify system performance and mission effectiveness. Questionnaires and opinions are often used. Consequently choosing an appropriate response variable is not straightforward. Surrogate measures need to be circumspectly examined to make certain that the experiment objectives are actually measured. This may actually require some innovative thinking on the part of the design team to build instrumentation into the LVC environment to gather the data necessary to support otherwise qualitative assessments of system performance in a system-of-systems context.

3. **Mixed Factor Levels and Limited Resources.** Joint mission environments are complex often containing many mixed-level, qualitative factors with scant resources available. Mixed-level factors refers to multiple factors where at least one factor contains a differing number of levels than the other factors. Often mixed-level designs require a large sample size making them inappropriate for tests that demand a small sample size due to resource constraints. Mixed-level designs can be fractioned into smaller designs but doing so can be tedious and independent estimates are not guaranteed for all fractioned designs. For the LVC experiment planners early consideration of these mixed factor problems can lead to changes in experiment focus, objectives, or even design to accommodate the problem.

4. **Interaction Effects** Unlike most traditional experiments, large simulation experiments can have a significant number of higher order interaction effects (i.e., 3-way or higher factor interactions). When using small designs these higher order effects may be aliased with the main effects meaning that the source of the effect is difficult, if not impossible to isolate and estimate (the main effect

and interaction effect are intermingled). Active higher order interactions can wreck the outcome of the experiment unless they are considered and appropriately accounted for in the choice of experimental design. The multi-disciplinary experiment design team can anticipate these interactions and choose designs for the LVC experiment that avoid the aliasing problem.

5. **Noisy Test Environments.** The joint mission environment contains copious sources of noise that must be prudently considered. Noise in the test environment can be harmful to an experiment if appropriate measures are not taken to control it or measure it. Effects that are thought to be important may not appear to be so because of over-estimated experimental error. To overcome this problem appropriate statistically-based noise control techniques are used in the LVC experiment planning process. Often human operators are the largest contributors of noise in the experiment and thus should only be used as necessary in LVC experiments. The benefits or necessity of including human subjects in the experiment must outweigh the risk that is assumed by including them. This judicious use of the human component in the LVC experiment is likely one of the larger paradigm shifts when moving LVC from a training environment to an analytical environment. Increasing system complexity by integrating additional (possibly unnecessary) assets can also increase noise in test.

6. **Human System Integration.** HSI principles should be applied to LVC experiments since LVC is a software system that requires extensive human interaction. **?** states that HSI practices propose that human factors be considered an important priority in system design and acquisition to reduce life-cycle costs. Furthermore, he states that each of the seven HSI considerations are necessary to satisfy operational stakeholders needs. We would add that HSI principles should be applied across all T&E activities where humans interact with software systems and offer some HSI considerations for T&E when human-software system interaction is central to the experiment, as is often the case with LVC. HSI considerations for LVC-based T&E activities ensure that:

(a) The right tradeoffs have been made between the number of humans included in the experiment and the quality of data required.

(b) Including joint human-machine systems in the experiment supports the objectives with human-machine systems only included when the experiment's analytical requirements can still be satisfied.

(c) The design of the experiment circumvents the likelihood of excessive experimental error caused by human-machine systems by using appropriate experimental noise control techniques.

(d) Data planning and analysis takes into account the additional variability introduced when humans adapt to new conditions or respond to contingencies (e.g., consider and avoid human learning invalidating the experimental results).

Human System Integration is native to the systems engineering process from a design point of view but foreign to T&E activities. For LVC experimentation to be effective, HSI considerations must be included across all test planning activities; such HSI considerations for LVC experimental planning is left for future research.

7. **Improved Test Discipline.** An LVC environment is extremely flexible. Assets can be added, deleted or modified, in some cases, quite easily. Given its strong history in training and demonstration events, LVC experimenters often "tweak" the LVC based on early results. Changing the LVC system mid-way through a randomized experimental design changes the fundamental assumptions of subsequent experiments from those already completed. In other words, the experimental design is compromised and no amount of statistical analysis can save poor designs.

8. **Experimental Design Size.** Unfortunately, there may be the belief that large, complex LVC experiments can answer any questions pertaining to the system (or systems) of interest. While the LVC may seem to address such questions,

answering quantitatively those questions would require far too many experimental runs; LVC experiments have run budgets like any other experimental event. Fortunately there are a range of reduced sample size experimental designs quite applicable to LVC experimentation. Some are fundamental, usually covered in basic training guides. Others are more advanced but powerful in their ability to obtain meaningful results.

4.3 Some Useful Experimental Designs for LVC Applications

The LVC environment offers many unique capabilities to T&E. However, to use LVC results in the analytically rigorous manner required by T&E necessitates that experimental designs be scrutinized to ensure they satisfy the objectives of the LVC-based joint mission tests. Several advanced designs seem well suited to the LVC test environment: orthogonal arrays, nearly orthogonal arrays, optimal designs, and split-plot designs. The first three designs can be used in experiments that allow full randomization while the split-plot designs are useful when there are restrictions on randomization.

An array is considered orthogonal if every pair of columns in the array is independent. This is accomplished by making each level combination in each column occur equally often [**?**]. Orthogonality improves our ability to estimate factor effects. To illustrate the usefulness of OAs, consider an experiment with a three-level factor and four two-level factors where testing resources only allow for 12 runs. A full factorial (all combinations of all factor levels) design requires 48 runs (3×2^4) and fractioning the design into a smaller, useful design would be very complicated. An orthogonal array can be constructed with 12 runs and will generate independent estimates of each of the 5 main effects. Table 12.7 in **?** contains many mixed-level orthogonal arrays for the interested reader.

At times orthogonal arrays cannot sufficiently reduce the run size while accommodating the necessary number of factors. A design team can relax the orthogonality requirement and reduce the experiment run size through the use of a nearly orthogonal array. A drawback to nearly orthogonal arrays is that the estimates of the effects are somewhat correlated (i.e., loss of independence when orthogonality was relaxed) making the data analysis somewhat more difficult. [**?**]. Several researchers such as **?**, **?**, and **?** have constructed nearly orthogonal arrays using algorithmic approaches with nice results.

Optimal designs are another excellent way to construct mixed-level designs. Optimal designs are nearly orthogonal designs optimized to some design criterion. Statistical software packages help create optimal designs making them a convenient choice for experimenters faced with mixed-level factors and limited resources. The D-optimal criterion (arguably the most widely used) measures the overall degree of orthogonality of the design matrix. The G-optimal criterion measures the extent that the maximum prediction variance for regression parameters is minimized. The G-optimal criterion is useful if a regression model is built from the experimental data to be used to make predictions about the system response. There are other optimal designs but not as pertinent to LVC experimentation in our view (see **?** for a cursory introduction to these other designs).

Split-plot designs are used when there are restrictions on experiment run randomization that prevent the use of a completely randomized design. Randomization restrictions make a completely randomized design inappropriate and can lead the experimenter to erroneous conclusions if the responses are analyzed in a manner inconsistent with the design and execution of the experiment [**?**]. In split-plot designs, hard-to-change factors are assigned to a larger experimental unit called the whole plot while all other factors are assigned to the subplot. Each of the whole plot and subplot carry an error component that must be estimated. Split-Plot designs are thus more difficult to analyze than completely randomized designs because of this more complicated error structure. See **?** for more details on split-plot designs.

There are of course many other classes of designs that may be applicable to LVC experimentation for T&E. The three classes discussed above provide, in our opinion, a broad range of options the LVC experimental design team should consider. Final design choices must be appropriate to the specifics of the LVC experiment planned. Use of orthogonal and nearly-orthogonal array designs are discussed in the subsequent case study.

4.4 Conducting a Data Link Experiment with LVC [2]

Currently there are aircraft that can only receive Link-16 communications from Command and Control (C2) assets in denied access environments. The Multifunctional Advanced Data Link (MADL) is a technology that would allow aircraft to transmit to other friendly forces in a denied access environment. The Air Force Simulation and Analysis Facility (SIMAF) was tasked with assessing the suitability of the MADL data link for aerospace operations in a denied access environment using a distributed LVC environment. The experiment will connect two geographically separated virtual aircraft simulators and augment them with constructive entities to make up the complete joint mission environment. Two separate test events are funded with enough resources to conduct two weeks of testing for each event. The experiment is characterized as a factor screening experiment aimed at gaining insight into the usefulness of the MADL network. Additionally, we want to ascertain which factors affect MADL usability in a denied access environment. Aircrew are in short supply with only two aircrew available per week per test phase. This case study focuses on the planning process for this LVC experiment. The experiment execution, data analysis, and conclusions will be discussed in a subsequent paper.

4.4.1 MADL Data Link. MADL allows aircrews to use voice communication in denied access environments and introduces two other capabilities: text chat, and machine-to-machine communication as shown in Table **??**. To effectively transmit communications in a denied access environment the data link must not greatly increase the vulnerability of the aircraft to enemy air defenses. To prevent detection during

[2] This case study is an actual event with specific weapons systems unnamed

Table 11 MADL Capabilities

Level	Available Communication Capability
1	Voice Only
2	Voice and Text
3	Voice, Text, and Machine-to-Machine

communication, MADL transmits a narrow beam of data between aircraft. With MADL, each aircraft in the network is assigned a node in the communication chain. To communicate with specific aircraft the subsequent traffic may go direct to that aircraft or be delivered to the aircraft through other aircraft nodes. This network structure can create latency, even failure, in message delivery. Suppose aircraft A, B, and C are linked via MADL and aircraft A wants to communicate with Aircraft C. If aircraft B transmits at the same time as aircraft A then aircraft B ”steps on” A's transmission and the message never reaches aircraft C. In other instances, if an aircraft in the network is in an unfavorable geometry at the time of transmission, the MADL chain is broken and the message could be lost. These two issues are of particular interest in the study and can be studied in a controlled manner using the LVC environment.

A simple scenario with an aircraft operating in a denied access environment includes: command and control aircraft operating, friendly fighter forces performing combat air patrol, and targets inside the denied airspace. Figure **??** depicts a notional MADL operation sufficient to support our discussion. The potential exists for the aircraft or other fighter aircraft to encounter enemy aggressors at any point in the denied airspace. Current operation procedure have the aircraft following pre-planned routes that minimize the probability of detection by enemy integrated air defense (IADS). An experiment objective includes determining if communicating in the denied access environment is useful enough to justify acquiring such capability. This represents an ideal example of using computing power to ascertain the operational effectiveness of proposed upgrades without investing in changes to the weapon systems.

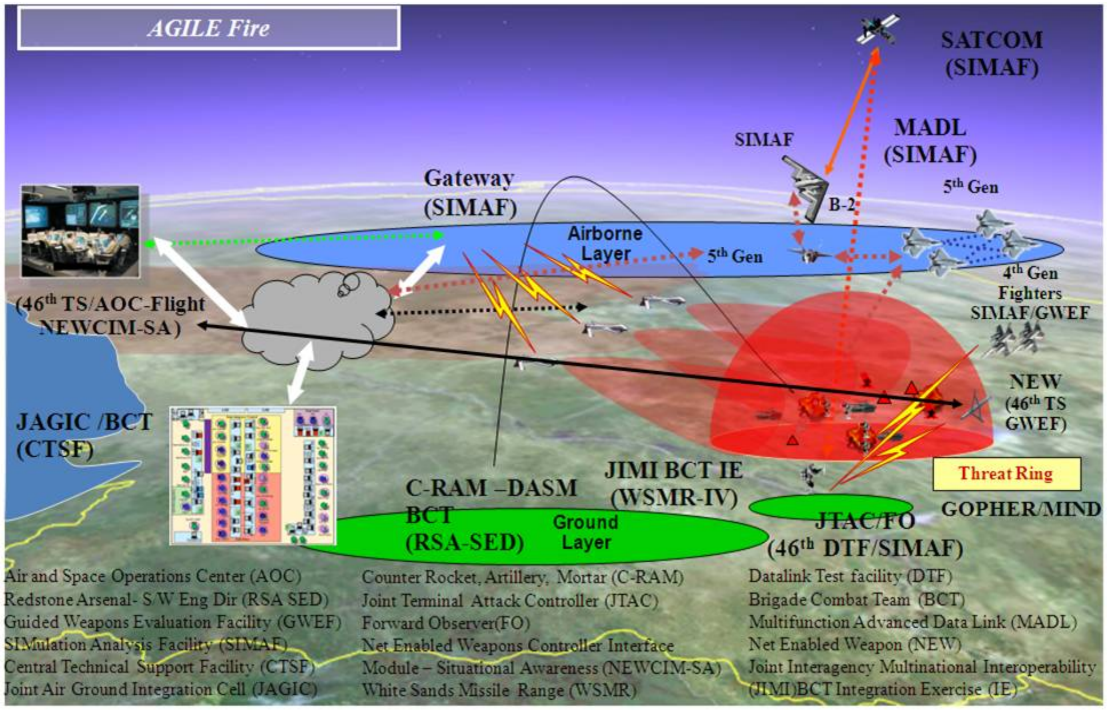

Figure 4 Notional LVC Representation of a Joint Operation Network in a Denied Access Environment [?]

4.4.2 Defining Experiment Objectives. The first task in an experimental design process is to clearly define the problem to be studied. Defining a clear, agreed upon problem statement for the LVC experiment was the most difficult task in the design process. Four to five months were spent defining the problem statement because influential members of the planning team were focused on defining the requirements for the LVC test environment instead of the data link problem being investigated; the test should drive what LVC provides. This distraction slowed the progress of the planning phase appreciably, but is really attributable to the paradigm shift associated with using LVC for new purposes. After much deliberation, two related objectives were chosen, one for each phase of the test program.

1. **Phase I:** Assess the usefulness of data messages passed on the MADL network assuming a perfect network configuration and performance.

2. **Phase II:** Assess the usefulness of the MADL network given a realistic level of degraded network performance.

Phase I assesses whether the message content and message delivery capabilities of MADL are useful to aircrews in prosecuting targets in denied access airspace. Subobjectives include determining which factors affect the usability of MADL for aircrews and find out which message delivery capabilities are preferred. Following phase I, the set of MADL messages and capabilities will be evaluated with useful messages and capabilities carried forward to phase II. The messages and capabilities deemed not useful will be dropped from the test set. The objective of phase II is to evaluate the usability of MADL messages and capabilities in a realistic environment when network degradation is present (as will likely occur in actual operations).

Breaking the test into two phases is important because it ensures that factor effects are easily identifiable in the data analysis. Consider what would happen if only phase II of the experiment were conducted and the degraded network makes the system so cumbersome that aircrew give it an unfavorable rating. This test method makes it more difficult to tell whether the MADL messages and delivery capabilities are problematic or whether poor network service is the problem. Experimental design helps to focus and clarify the objectives and the data required to achieve the objective.

4.4.3 Choosing Factors of Interest and Factor Levels. The factors of interest came primarily out of the requirements for the LVC test environment. Initially MADL and the vignettes (operational environment scenarios for the test) were the only two factors proposed for the study. This created an overly simplistic model for study especially when you consider that several other test conditions were to be varied across runs. Such a simplistic yet changing model of the experiment would have yielded results with factor effects confounded with hidden effects. Analytically, no defendable insights could come from such an experiment. Accidental factor confounding is not uncommon if statistical experimental design issues are ignored. Unfortunately, subsequent analyses may proceed without knowledge of the confounding.

Table 12 Proposed Factors of Interest

Factors	Levels
MADL	4
MADL Node Position	2
Quality of Service	2
Vignettes	4
Route	2
Target Location	2
Aircrew	2
Size of Enemy Air	2
Position of Enemy Air	2
Size of Friendly Air	2
Position of Friendly Air	2

Statistical experimental design was re-emphasized at this point in the planning process. Brainstorming resulted in an initial set of 10 (Table **??**) factors with further consideration reducing the set to 4 factors for phase I and 6 factors for phase II, given in Table **??** and Table **??**, respectively. Additionally, one of the MADL factor levels was dropped from the test requirements. Besides MADL as the factor of interest, the operational context (vignettes), ingress route, target location, and aircrew were included as factors in phase I of the experiment. The three latter factors were not of primary interest but were chosen to prevent learning effects in the aircrew during the experiment and its biasing of the outcome. The routes and target locations vary systematically the aircrew factor will be a blocking effect. These statistical techniques help guard the experiment against excessive noise introduced by human operators influencing the final results.

In phase II, two additional factors, node position and quality of network service, are added to the phase I design. The additional factors allow a measure of the variation caused by the degraded network. The rule of thumb for choosing factors of interest is to consider adding any setting or test condition changed from run to run as a factor of interest in the experiment.

Table 13 Final Set of Factors of Interest for Phase I

Factor	Level
MADL	3
Vignettes	4
Route	2
Target Location	2
Aircrew	2

Table 14 Final Set of Factors for Phase II

Factor	Level
MADL	3
Vignettes	4
Route	2
Target Location	2
Aircrew	2
Node Position	2
Quality of Service	2

4.4.4 Selecting the Response Variable. Selecting an appropriate response variable is never easy and can be particularly troublesome in an LVC experiment where many test problem statements are qualitative in nature. Quite often LVC tests employ user surveys to assess qualitative aspects and thus aircrew surveys were proposed for the current test. However, an LVC can collect system state data quite easily. Such state data, if properly defined provides potential insight into the potential benefits of improved system capabilities. In other words, state data can be correlated to qualitative measures, such as aircrew surveys, to develop quantitive measures on qualitative aspects. The approach agreed upon was to use the aircrew survey as a primary response variable with the system state data collected to cross-check and verify aircrew responses and perceptions of the system capabilities.

4.4.5 Choice of Experimental Design. LVC test requirements can be dynamic; the current case was no exception. Since an LVC offers a tremendous flexibility to expand the test event, unlike comparable live test events, the temptation is to continue to expand the LVC. Due to the ever-changing nature of the test requirements, several experimental designs were considered at various stages in the design process. As requirements were refined, more information about the size and scope of the experiment, the number of virtual and constructive simulation entities, environmental constraints, and aircrew availability came to light. A few of the designs that were contemplated are discussed below along with the rationale for considering that design.

A 16-run 4×4 factorial design was initially considered. The design was discounted as overly simplistic because it ignored potentially important environmental factors. A split-plot design was then considered since the experiment involved a restricted run order. The experimental design team was concerned that completely randomizing MADL capabilities would confuse operators due to large changes in available capability from one level to another. To avoid potential operator confusion the team considered a restricted run order where the run order is chosen by fixing MADL at a particular level then randomizing the run order for the remaining factors. Once all runs have been completed for a given level of MADL, a new MADL level is chosen and the process is repeated until all test runs have been completed for all MADL levels. Such randomization restriction makes the use of split-plot analysis an imperative. **?** shows that analyzing restricted run order experiments as completely randomized designs can lead to incorrect conclusions, a conclusion echoed in **?**.

Future use of LVC for test is quite likely to examine impacts of new methods or technology and such examinations affect the design. In the current setting, the MADL-voice-only option was removed as a factor, run separately, and used as a baseline for performance measurement. The rest of the design, now smaller given the removal of a factor, was completely randomized. A replicated, 12-run orthogonal array, shown in Table **??**, was chosen for phase I. Four additional, replicated runs are completed using voice only to provide a baseline capability for comparison. The orthogonal array is a good option for factor screening experiments since it provides estimates of each of the main effects and select interactions of interest.

Phase II will add two more factors to the experiment making an orthogonal array unusable for a sample size of 12. This led to choosing a nearly orthogonal array (NOA) with replicates. The NOA used for phase II is shown in Table **??**. If phase I reveals that some factors are inactive then those factors may be dropped from phase

Table 15 Run matrix for Phase I test in standard order

Run	MADL	Vignette	Route	Target Location
1	1	1	2	2
2	1	2	1	2
3	1	3	1	1
4	1	1	2	1
5	2	1	1	1
6	2	2	2	2
7	2	3	1	2
8	2	2	2	1
9	3	1	1	2
10	3	2	1	1
11	3	3	2	1
12	3	3	2	2

II and orthogonality in the design could potentially be restored since Phase II will involve fewer factors.

4.5 Conclusions

LVC offers the T&E community a viable means for testing systems and system-of-systems in a joint environment. However, the added capability is not without cost and a shift in the paradigm of LVC use. Planning joint mission tests using LVC is a challenging endeavor and requires careful upfront planning. The nature of LVC experiments requires experimenters to decide what should be studied in the experiment when defining the objectives. There is a strong lure toward unnecessary complexity in LVC that entices experimenters to tackle excessively large tests with a misplaced hope that many questions about the system can be addressed simultaneously in that one large experiment. Experimenters need to be aware of this lure and exercise good test discipline by structuring LVC experiments to gain system knowledge incrementally thereby ensuring sound test results. This experimental design method is easily

Table 16 Run matrix for Phase II test in standard order

Run	MADL	Vignette	Route	Target Location	Node Position	Quality of Service
1	1	1	2	2	2	2
2	1	2	1	2	1	1
3	1	3	1	1	1	2
4	1	1	2	1	2	1
5	2	1	1	1	1	2
6	2	2	2	2	1	1
7	2	3	1	2	2	1
8	2	2	2	1	2	2
9	3	1	1	2	1	1
10	3	2	1	1	2	2
11	3	3	2	1	1	2
12	3	3	2	2	2	1

manageable for planning, executing, and analyzing data and builds system knowledge piece by piece.

LVC test environments have many sources of random error. Considering and exploiting Statistical experimental design techniques allow for objective conclusions when the system response is affected by random error. The system response variable should be chosen based on how well that measure relates to the experiment objectives. The response variable should measure this relation as directly as possible. Direct measurements are unobtainable in many LVC experiments so surrogate measures should be devised and examined for suitability. The factors of interest should be chosen from the set of environmental and design parameters that are thought to have an effect on the system response. A good rule of thumb when choosing factors is to consider including any test parameter that will be varied across the runs. Additional design considerations for LVC experiments were proposed to deal with the nuances of LVC. The additional design considerations are by no means exhaustive and should be updated as new challenges are encountered in LVC.

The reported data link experiment demonstrates how experimental design techniques can be used to ultimately better characterize the performance and effectiveness of a new system in a joint environment generated by LVC. The application of experimental design principles uncovered substantial mistakes in test planning and improved the overall test strategy by using an incremental test approach. Important factors that were initially missed were added to the system as a result of using statistical experimental design. Noise control techniques were used to improve the quality of the data collected. These techniques added necessary complexity to the experiment but improve data quality. The experiments also showed how innovative experimental designs, such as orthogonal and nearly orthogonal arrays, effectively accommodate the large, irregular factor space with limited test resources that are typical of most LVC experiments.

Following the experimental design process saved time, resources and more importantly reduced wasted effort by systematically structuring the problem in a way to collect high quality data. Future LVC experiments can benefit greatly from using such statistical experimental design techniques. This paper did not address the myriad technical issues involved in realizing an LVC environment. Much of the work (and finding) in LVC focuses on solving these technical issues. Our focus in this paper is the design of the experiment that uses the LVC to generate results used in analytical settings. We understand technical issues can affect system responses and we understand that experimental design choices can affect LVC system technical aspects. We leave this discuss to future work for now.

5. An Algorithmic Foldover Procedure for Nearly Orthogonal Arrays with Projection[1]

5.1 Introduction

Nearly orthogonal arrays (NOAs) are a class of designs that are useful in experiments that have multiple, mixed-level factors with limited runs available such as is the case with many Live-Virtual-Constructive (LVC) simulations. LVC is a test capability being investigated by the Department of Defense (DoD) to economically test systems in a joint mission environment. LVC environments combine live equipment and personnel, with pure simulation (constructive) and interactive simulation (virtual) into a single simulation environment. Such environments are complex with many mixed-level, often qualitative factors. As a result an LVC-based experiment may require use of a NOA design. A handful of techniques for constructing NOAs currently exist with recent papers focusing almost exclusively on algorithmic construction techniques with **?** being the only exception.

? introduced a combinatoric construction approach using *near-difference* matrices thus pioneering the effort to create useful NOAs for factorial experiments. Both **?** and **?** use a variation of columnwise-pairwise construction techniques and in many cases were able to obtain NOAs with better properties than **?**. **?** created a class of NOAs characterized by their projection properties, strength m, extending a familiar class of orthogonal array (OAs) designs to NOAs. These properties are discussed in Section **??**. **?**'s development provides tremendous potential for LVC experiments, particularly when screening factors in the early stages of experimentation. This screening method is particularly useful when higher order interactions are suspected and only a few factors are believed to be active. One drawback to their method is that significant correlation can be introduced into the array to achieve the desired projection properties dramatically lowering the estimation efficiency for some factors.

[1] This chapter has been submitted as a regular paper to the *International Journal of Experimental Design and Product Optimisation.*

Table 17 Factors of Interest

Rank	Factors	Levels
1	Data Link	3
2	Vignettes	2
3	Node Position	2
4	Aircrew	2
5	Enemy Air Size	2
6	Enemy Air Position	2
7	Friendly Air Size	2
8	Friendly Air Position	2
9	Route	2
10	Target Location	2

Table 18 Active Factors Found in Week 1 of Testing

Factors	Levels
Data Link	3
Vignettes	2
Node Position	2
Aircrew	2
Route	2
Target Location	2

To illustrate this, consider an Air Force experiment to assess the utility of an experimental data link for joint mission environments using a LVC simulation. The test is to be conducted over two weeks with 12 runs available each week for a total of 24 runs. Subject matter experts (SME) have identified 10 potential factors of interest in the experiment but we expect (under the *sparsity-of-effects* principle) that only a subset of factors will be active. This uncertainty as to which factors should be included in the experiment is due to the novelty of both the system under study, and the LVC simulation environment. The proposed factors of interest are listed and ranked by the *a priori* expected factor effects on the system response in Table **??**.

A 12-run nearly orthogonal array of strength 2, taken from **?**, was chosen as the experimental design (see Table **??**). Two replicates of the design were planned. Such a design strategy has the following benefits:

1. The design makes efficient use of scarce test resources.
2. The design can be fully projected in any two columns.
3. Replicating the design gives an estimate of the pure error independent of the number of factors included.
4. Replication guards against outliers biasing the system response function.

Table 19 NOA design for LVC Experiment.

	Factors									
Run	DL	V	NP	AC	EP	ES	FP	FS	R	TL
1	0	0	0	0	0	0	0	0	0	0
2	0	1	1	1	1	0	0	0	0	0
3	0	0	1	0	1	1	1	1	1	1
4	0	1	0	1	0	1	1	1	1	1
5	1	0	1	1	0	0	1	1	0	0
6	1	1	0	0	1	0	1	0	0	1
7	1	0	1	1	0	1	0	0	1	1
8	1	1	0	0	1	1	0	1	1	0
9	2	0	0	1	1	0	1	0	1	1
10	2	1	1	0	0	0	0	1	1	1
11	2	0	0	1	1	1	0	1	0	0
12	2	1	1	0	0	1	1	0	0	0
D_s	1.00	0.89	0.89	0.89	0.89	0.76	0.76	0.76	0.33	0.36

DL defined as Data Link
V defined as Vignette
NP defined as Node Position
AC defined as Aircrew
EP defined as Enemy Air Forces Position
ES defined as Enemy Air Forces Size
FP defined as Friendly Air Forces Position
FS defined as Friendly Air Forces Size
R defined as Route
TL defined as Target Location

The drawback of the design in Table **??** is that it has low estimation efficiency (i.e. D_s) in both columns 6 and 8. The D_s-efficiency is a measure of the precision of each of the effect estimates that can be obtained by a given experimental design. The experimental design team assigned the factors of interest by rank to columns in descending order of estimation efficiency to give the factors believed to be most important the most precise estimates. Route and Target Location were thought least likely to affect the system response and were assigned to columns 6 and 8, respectively.

The experiment was run and the following factors were deemed active: MADL, Vignettes, Node Position, Aircrew, Route, and Target Location with the latter two factors having a much larger than expected effect on the system response. The experiment revealed that SME's were incorrect in their assessment of likely active factor effects resulting in unacceptably imprecise estimates of the large factor effects. Previously, such results would mean that the test team would have to accept undesirable test results or redesign and rerun the experiment; in this case wasting half of the available test resources. A preferred method is to create a second design, a foldover of the initial design, to "rescue" the experiment.

This paper proposes an algorithmic foldover approach to break aliasing between factors of interest while maintaining the desired projective properties of certain NOAs. In Section **??** we define NOA projection as given by **?** followed by Section **??** where we propose an algorithmic foldover procedure to increase estimation efficiency for factors of interest. The foldover technique is applied to the data link experiment and the resulting design is given in Section **??**.

5.2 Defining Projection for NOAs

? introduced the concept of strength m designs for orthogonal arrays. An OA is said to be strength m if for every m-tuple of columns, every level combination occurs equally often, thereby achieving m-balance. Designs that are strength m have two useful properties.

1. Any full projection model involving m factors can be estimated with highest efficiency. A full projection model contains the m main effects and all higher order interactions among the m factors.

2. All main effects in the design can be estimated with highest efficiency.

? define a NOA of strength m if it possesses the m-projection property and is as close to m-balance as can be achieved. The m-projection property is achieved if for every m-tuple of columns there is at least one replicate of a full factorial in n runs. As stated previously, a design achieves m-balance is if every level combination occurs exactly the same number of times. To measure how near a design is to m-balance **?** use the $B(m)$ criterion defined as follows. A design $D(n; q_1, ..., q_k)$ can be written as an $n \times k$ matrix X= $(x_1, x_2, ...x_k)$. For every m-tuple of columns of $\mathbf{X}$, $(x_{l_1}, x_{l_2}, ..., x_{l_m})$, **?** define

$$B_{l_1 \ldots l_m}(m) = \sum_{\alpha_1, \ldots, \alpha_m} \left(n^{(l_1 \ldots l_m)}_{\alpha_1, \ldots, \alpha_m} - \frac{n}{q_{l_1} \cdots q_{l_m}} \right)^2 . \tag{16}$$

Here, $n^{(l_1 \ldots l_m)}_{\alpha_1, \ldots, \alpha_m}$, is the number of runs that $(x_{l_1}, x_{l_2}, ..., x_{l_m})$ takes the level combination $\alpha_1, ..., \alpha_m$, and the summation is taken over all the $q_{l_1} \cdots q_{l_m}$ level combinations. This $B_{l_1 \ldots l_m}(m)$ criterion measures the closeness of m-balance of the subdesign consisting of m columns. The $B_{l_1 \ldots l_m}(m)$ equals zero if and only if the subdesign is an OA of strength m. When all m-column submatrices are considered, the average of $B_{l_1 \ldots l_m}(m)$ values, defined as

$$B(m) = \sum_{1 \le l_1 < \cdots < l_m \le k} \frac{B_{l_1 \ldots l_m}(m)}{\binom{k}{m}}, \tag{17}$$

is used as a global measure of closeness to m-balance of the design [**?**].

Nearly orthogonal designs are unable to meet the strength m requirement since every level combination does not occur equally often. **?** modify the definition of strength m design to accommodate NOAs. A NOA is said to be strength m if it meets the following conditions:

1. It possesses the property of m-projection.
2. It has the minimal $B(m)$ value.

The first condition is easy to verify but the second condition may not be easily verified in some cases. The minimal $B(m)$ value is met if, for every subdesign involving m factors the subdesign either forms an OA of strength m (in this case $B(m) = 0$) or the number of different level combinations differ from each other by no more than one. When the number of level combinations differ by one the subdesign is *nearly balanced*. **?** give a formula for computing the lower bound of $B(m)$ for the interested reader. We do not consider the lower bound since the lower bound for our 24-run example in Section **??** is zero.

5.3 An Algorithmic Foldover for NOAs of Strength 2 [2]

In this section we present an algorithmic approach to foldover nearly orthogonal arrays with good projection properties. The algorithm involves a search process that employs columnwise-pairwise exchange procedures to search the design space. A columnwise-pairwise exchange algorithm selects a column of the design and randomly chooses a pair of differing column elements to swap; proceeding until all of the columns in the design have been searched and/or the evaluation criteria have been met. For this particular algorithm, candidate designs are evaluated using **?**'s minimal $B(m)$ criteria as the first design objective and the well-known D_s estimation efficiency as the second objective.

[2] This algorithm was developed for a real data link experiment conducted by the Air Force Simulation and Analysis Facility (SIMAF) but was not used due to changes in the original experimental design. The data link experiment presented in this paper is a notional example.

For a given column i, the D_s criterion measures the degree of orthogonality between column i and every other column in the design. The D_s criterion is computed as follows:

$$D_s = \{x_i^t x_i - x_i^t (X_{(i)}^t X_{(i)})^{-1} X_{(i)}^t x_i\} / x_i^t x_i \qquad (18)$$

where x_i is the column for which the D_s criterion is being computed and $X_{(i)}$ is the design matrix without column i.

Algorithms from **?** and **?** are adapted for this procedure. The steps to add r runs to the original $n \times k$ design are given as follows:

1. Start with the original $n \times k$ NOA design.
2. Delete inactive factors if applicable.
3. Augment the original design with r additional runs such that each of the columns of r are random and balanced.
4. Set T_1 (the number of pairwise exchanges considered for each column search) and T_2 (the number of algorithm re-starts).
5. Start with column $i = 1$. If the column is orthogonal to every other column (i.e. $D_s = 1.00$) then go to step **??**. Otherwise perform random-pairwise exchanges of elements in $(n + 1)$ to $(n + r)$, T_1 times. If the pair exchange results in improvement in the $B(2)$ criteria then the candidate column replaces the original column, otherwise the original column is kept. If $B(2)$ for the candidate column is equal to the original column then the column with the largest D_s value is kept.
6. Let $i = i + 1$, and repeat step **??** for all k columns.
7. Repeat steps **??** and **??**, T_2 times using the best design found in the previous iteration as the starting design.

8. The strength 2 design with the minimum $B(2)$ value and maximum D_s values is recorded and returned as the $(n+r) \times k$ design.

To illustrate the algorithm consider a 12-run, 6-factor design augmented with 6 additional runs given by $\left|\frac{X}{F_i}\right|$, where X is the original 12×6 design matrix and F_i is the random 6×6 matrix used to initialize the foldover search algorithm and i is the current iteration. The algorithm calculates initial $B(2)$ value and the column estimation efficiencies, D_s, for the full design matrix $\left|\frac{X}{F_i}\right|$. Next set $i = 1$ and check column orthogonality. In this case no further improvement can be made to column 1 since $D_s = 1.00$. Increment i to $i = 2$. Column 2 has a $D_s < 1$ so two elements of F_1 are randomly chosen and swapped. The $B(2)$ and D_s values are recomputed and compared to the values found in the previous iteration. F_1 and F_2 are shown in Figure **??** along with the design evaluation criteria computed for each iteration. Notice that both evaluation criteria improved after swapping the elements; hence the candidate column replaces the original column. This column pairwise procedure is repeated T_1 times before moving to the next design column. Once all k columns have been searched the algorithm returns to column $i = 1$ and repeats the entire procedure T_2 times; returning the best foldover design.

This algorithm can be used to conduct a full or partial foldover depending on the number of runs available. The algorithm performs a random search that does not converge to an optimal solution. For foldover designs consisting of 6 to 12 runs $T_1 = 40$ and $T_2 = 5$ are usually sufficient to find a good solution. However, the algorithm may need to be re-run multiple times if a good solution is not found. These search parameters resulted in search times ranging between 2.5 to 3.5 seconds for a 6 factor design matrix with 12 runs using a MacBook with a 2.13 GHz Intel Core 2 Duo processor and 4 GB of 800 MHz DDR2 SDRAM. In the next section multiple foldover designs found using the search algorithm are presented and the design properties and variance structure are discussed for each type of design.

$$\left|\frac{X}{F_1}\right| = \left|\begin{array}{cccccc} 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & 1 & 0 & 0 & 1 \\ 0 & 0 & 1 & 1 & 1 & 0 \\ 0 & 1 & 0 & 1 & 1 & 1 \\ 1 & 0 & 1 & 0 & 0 & 1 \\ 1 & 1 & 0 & 0 & 1 & 0 \\ 1 & 0 & 1 & 1 & 1 & 1 \\ 1 & 1 & 0 & 1 & 0 & 0 \\ 2 & 0 & 0 & 1 & 1 & 1 \\ 2 & 1 & 1 & 1 & 1 & 0 \\ 2 & 0 & 0 & 0 & 0 & 1 \\ 2 & 1 & 1 & 0 & 0 & 0 \\ \hline 0 & 0 & 0 & 0 & 0 & 0 \\ 1 & \mathbf{1} & 1 & 1 & 1 & 1 \\ 2 & \mathbf{0} & 0 & 0 & 0 & 0 \\ 0 & 1 & 1 & 1 & 1 & 1 \\ 1 & 0 & 0 & 0 & 0 & 0 \\ 2 & 1 & 1 & 1 & 1 & 1 \end{array}\right| \Longrightarrow \left|\begin{array}{cccccc} 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & 1 & 0 & 0 & 1 \\ 0 & 0 & 1 & 1 & 1 & 0 \\ 0 & 1 & 0 & 1 & 1 & 1 \\ 1 & 0 & 1 & 0 & 0 & 1 \\ 1 & 1 & 0 & 0 & 1 & 0 \\ 1 & 0 & 1 & 1 & 1 & 1 \\ 1 & 1 & 0 & 1 & 0 & 0 \\ 2 & 0 & 0 & 1 & 1 & 1 \\ 2 & 1 & 1 & 1 & 1 & 0 \\ 2 & 0 & 0 & 0 & 0 & 1 \\ 2 & 1 & 1 & 0 & 0 & 0 \\ \hline 0 & 0 & 0 & 0 & 0 & 0 \\ 1 & \mathbf{0} & 1 & 1 & 1 & 1 \\ 2 & \mathbf{1} & 0 & 0 & 0 & 0 \\ 0 & 1 & 1 & 1 & 1 & 1 \\ 1 & 0 & 0 & 0 & 0 & 0 \\ 2 & 1 & 1 & 1 & 1 & 1 \end{array}\right| = \left|\frac{X}{F_2}\right|$$

$$\begin{aligned} B(2) = 8.1 &\Longrightarrow B(2) = 6.8 \\ D_s = 0.82 &\Longrightarrow D_s = 0.93 \end{aligned}$$

Figure 5 The first iteration of the foldover search algorithm with the bolded elements of column 2 randomly swapped. Both design evaluation criteria improved in this iteration so the candidate column from F_2 replaces the original column.

5.4 Data Link Experiment

We now revisit the Air Force Data Link experiment where the above algorithm was used to improve the estimation efficiency of columns with low estimation efficiency. Four inactive factors were deleted from the original design and the foldover procedure proposed in Section **??** was performed on the remaining six factors: Data Link, Vignettes, Node Position, Aircrew, Route, and Target Location. Two potential, 12-run, foldover designs were created and are shown in Table **??** and Table **??**, respectively. The design in Table **??** has better estimation efficiencies but has a higher $B(2)$ value than the design in Table **??**.

One drawback to using a full foldover design is that we are unable to obtain an independent estimate of the pure error; this was one of the reasons we chose a replicated NOA to begin with. Another option is to create a 6-run foldover using the same algorithm (Table **??**), replicate that foldover (Table **??**) and thereby obtain an independent estimate of the error. Each of these foldover options need to be explored to see if a design with suitable $B(2)$, D_s, and nearly-uniform variance can be found.

Near-orthogonality has implications for the variance structure of a design and therefore needs to be considered when evaluating nearly orthogonal designs; including foldover options. An orthogonal array is a balanced design with minimum, uniform variance in all factors. When evaluating nearly orthogonal designs it is desirable to choose the design with properties that are closest to similar orthogonal designs. Uniform variance is a highly desirable property in an experimental design; it guarantees that the variance is the same everywhere in the design space of equal distance from the design center. Minimum variance is useful because it produces factor effect estimates that are as precise as possible.

The relative variance structure of each foldover are shown in Table **??**. This structure is calculated by taking $(X'X)^{-1}$ of the respective, 24-run, design matrices. The variance structure for each of the foldover designs are compared with a similar 24-run, 6-factor, orthogonal array (OA) adapted from **?**. An OA with similar design parameters makes a natural standard for comparison since it has minimum, uniform variance for all factors with the same number of levels. Each of the foldover NOAs have acceptable near-uniform variance; however, the partial foldover, partially replicated design (Table **??**) has the most uniform variance but the foldover design in Table **??** is a close second with more precise estimates.

We chose the 6-run, replicated, foldover becuase it is nearly uniform and it provides an independent estimate of the pure experimental error. The partially replicated NOA is 11%-16% less precise than a comparable OA. This is a tradeoff that must be made in order to independently estimate the experimental error. This experimenta-

Table 20 12-run foldover with various design criteria.

	Factors					
Run	MADL	Vignette	Node Position	Route	Target Location	Aircrew
13	1	1	0	0	1	0
14	0	1	0	0	1	0
15	0	0	1	1	0	0
16	2	0	0	1	0	0
17	2	0	1	1	0	0
18	1	1	1	0	1	0
19	0	1	1	0	0	1
20	0	0	0	0	1	1
21	1	0	0	1	1	1
22	2	1	0	1	0	1
23	2	1	1	0	1	1
24	1	0	1	1	0	1
D_s	0.94	0.82	0.97	0.84	0.93	0.97
ΔD_s	-0.06	-0.07	0.08	0.49	0.57	0.08
D	0.90					
$B(2)$	1.07					

tion strategy poses several benefits, chiefly that it gives experimenters the tools to more aggressively screen factors, estimate interaction effects, independently estimate experimental error, and salvage the experiment when *a priori* test assumptions are found to be in error. Our foldover algorithm gives experimenters the confidence to design and execute experiments that would be otherwise deemed too risky.

5.5 Conclusions

Nearly orthogonal arrays are a useful class of experimental designs screening many factors with limited test resources. **?**'s designs allow experimenters to gain more insight from these experiments by allowing stronger designs to be projected into subsets of the original design. The foldover algorithm we presented reduces the risk of using NOAs with projective properties and allows experimenters to gain system information in a more efficient and parsimonious manner. This technique is

Table 21 Alternate 12-run foldover. This design has higher estimation efficiencies in most design columns than Table ?? but a higher $B(2)$ value.

	Factors					
Run	MADL	Vignette	Node Position	Route	Target Location	Aircrew
13	0	0	0	0	1	0
14	2	1	0	0	1	1
15	1	0	0	1	0	0
16	0	1	0	1	0	1
17	1	1	0	1	0	1
18	2	0	0	0	1	0
19	1	1	1	0	0	0
20	0	1	1	1	1	1
21	1	0	1	1	0	1
22	0	0	1	0	1	0
23	2	1	1	0	1	1
24	2	0	1	1	0	0
D_s	0.99	0.97	1.00	0.96	1.00	0.94
ΔD_s	-0.01	0.08	0.11	0.63	0.64	0.05
D	0.90					
$B(2)$	1.33					

Table 22 6-run partial foldover. This design has the best $B(2)$ design criterion with excellent D_s estimation efficiencies.

	Factors					
Run	MADL	Vignette	Node Position	Route	Target Location	Aircrew
13	0	0	1	1	0	0
14	2	0	0	0	1	0
15	1	1	0	1	0	0
16	1	0	1	0	1	1
17	2	1	1	1	0	1
18	0	1	0	0	1	1
D_s	1.00	0.95	0.94	0.94	0.94	0.95
ΔD_s	0.00	0.06	0.05	0.61	0.58	0.06
D	0.90					
$B(2)$	0.67					

Table 23 12-run partial foldover created by replicating Table ??. This design gives us an estimate of the experimental pure error and more precise variance estimates than the design in Table ??.

	Factors					
Run	MADL	Vignette	Node Position	Route	Target Location	Aircrew
13	0	0	1	1	0	0
14	2	0	0	0	1	0
15	1	1	0	1	0	0
16	1	0	1	0	1	1
17	2	1	1	1	0	1
18	0	1	0	0	1	1
19	0	0	1	1	0	0
20	2	0	0	0	1	0
21	1	1	0	1	0	0
22	1	0	1	0	1	1
23	2	1	1	1	0	1
24	0	1	0	0	1	1
D_s	1.00	0.89	0.85	0.89	0.89	0.89
ΔD_s	0.00	0.00	-0.04	0.56	0.53	0.00
D	0.96					
$B(2)$	2.4					

Table 24 Comparing the variance structure of three foldover designs against a 24-run, 10 factor, orthogonal array

Factors	Unreplicated 24-run NOA [a]	Unreplicated 24-run NOA [b]	Partially Replicated 24-run NOA [c]	Unreplicated 24-run OA
MADL	0.067	0.063	0.063	0.063
Vignette	0.051	0.043	0.047	0.042
Node Position	0.043	0.042	0.049	0.042
Route	0.050	0.043	0.047	0.042
Target Location	0.045	0.042	0.047	0.042
Aircrew	0.043	0.044	0.047	0.042

[a] See Table ??
[b] See Table ??
[c] See Table ??

particularly useful when there is uncertainty as to which factors affect the system response. A data link experiment was presented to demonstrate potential usage of the foldover algorithm. Using this foldover algorithm, three alternate foldover designs were presented to demonstrate the procedure; each design having distinct advantages. The replicated, 6-run, foldover was chosen because it is able to estimate the pure error of the system and has near-uniform, near-minimum variance, resulting in precisely estimated factor effects. A limitation of the algorithm is that it employs a random search for the foldover design meaning that it may not reach an optimal solution. A better search heuristic could be used to make the algorithm to converge to an optimal solution, but is left to follow on work.

Disclaimer: The views expressed in this article are those of the author and do not reflect the official policy or position of the United States Air Force, Department of Defense or the U.S. Government.

6. Conclusions

Live, virtual, and constructive (LVC) simulation is a test capability the Department of Defense (DoD) views as useful to test systems and system of systems in realistic joint mission environments. Joint mission environments created via LVC have several advantages over similar live joint mission environments. LVC can connect geographically dispersed test facilities over a persistent computer network and create the necessary variety and density of assets representative of a joint environment in a cost effective manner. Creating such a joint environment representation is often unachievable with a live test environment. LVC environments also afford the test team more flexibility in designing the experiment because the simulated entities can be controlled with greater precision than live assets. Collectively, these benefits make LVC technology an attractive option for DoD experiments involving joint mission environments. However, some limitations to LVC capability mean that caution is warranted when using LVC to construct joint mission environments for experiments.

In Chapter **??** we define LVC and discuss the benefits and limitations of its use. To take advantage of the benefits and overcome the limitations of LVC, a well-known experimental design process is presented. This experimental design process guides the test team in structuring the problem to maximize the amount of information extracted from the experiment. Additionally, we present four classes of experimental designs that have potential application to LVC experiments.

In Chapter **??** we apply the experimental design process to a data link experiment that uses LVC to create the test environment. The case study illustrates how the LVC test experience is improved by using a statistical experimental design methodology. Additional experimental design considerations for LVC experiments uncovered during the case study are presented and discussed. In particular we advocate shifting the LVC paradigm to ensure that LVC experiments are conducted with analytical rigor. These special considerations increase awareness of the uniqueness of LVC experiments and can aid future attempts to apply the experimental design process to such experiments.

Finally, we propose an aggressive sequential experimentation strategy for LVC experiments in Chapter **??** using replicated NOAs with projection to gain as much information as possible when faced with limited test resources. This strategy depends on a foldover algorithm that we developed to break the aliasing between factors in certain NOAs. This algorithm allows testers to rescue LVC experiments when post-test analysis reveals that important factor effects are confounded. We demonstrate the algorithms usefulness with a 12-run, 10-factor experiment NOA with low estimation efficiency in some factors. The foldover algorithm is able to significantly increase the estimation efficiency for the factors of interest. The complete design has desirable estimation efficiencies and nearly uniform variance.

Chapters **??**, **??**, **??** have been submitted for publication to ITEA Journal, Systems Engineering, and International Journal of Experimental Design and Process Optimization, respectively. Material from Chapters **??** and **??** was published at the International Test and Evaluation Association's Live-Virtual-Constructive Simulation Conference in El Paso, TX. Finally, a conference paper has been submitted to the Industrial Engineering Research Conference. This paper advocates the use of statistical design methods as a means to increase the analytical rigor of LVC experiments and move away from the demonstration and training paradigm currently held by many LVC users. These conference presentations have been included in Appendix **??** and Appendix **??**, respectively.

Several technical issues that confront LVC users are not addressed in this work. Issues such as latency in the shared system state data or missing data caused by dropped data packets will affect the analysis and mitigating procedures should be considered in the experimental design. Choosing adequate response variables for joint mission experiments with qualitative problem statements was only given cursory attention in this work. More work needs to be done to develop a standardized framework for choosing system response variables that quantify how well a system performs in a joint mission environment. Such a framework could allow testers to increase the rigor of system assessments in joint mission environments more efficiently.

Finally, the search used for our foldover algorithm is inefficient and does not converge to an optimal solution. A better search heuristic could improve the speed, efficiency, and convergence properties of our algorithm. Such improvements are left to future work.

Appendix A. Matlab Code for Foldover Algorithm

```
function [X, telapsed]=foldover(NOA, answer, del_fac, order, add_run,
num_search, num_restart)
%foldover is a columnwise, pairwise exchange algorithm that takes in a
%NOA and performs a foldover breaking the aliasing between select columns
%   This algorithm takes in the following variables:
%   NOA - nearly orthogonal array from phase I of testing; type - Matrix
%   answer - 'Y' or 'N' ; type - 'char'
%   del_fac - vector of indices of inactive factors to be deleted; vector
%   order - vector containing the order of factors to be folded over
%   add_run - scalar; number of runs to add to the original
%   num_search - scalar; number of times a column is to be searched
%   num_restart - scalar; number of times the algorithm is to be restarted
%
%   This algorithm returns the following:
%   X - object containing:
%       value - Matrix; design matrix in 'uncoded' elements
%       code - Matrix; design matrix coded
%       D - Scalar; D-optimal criterion of design
%       Ds - vector; contains D_s criterion for each column
%       Bm - scalar; measures m-balance of design matrix
%   telapsed - scalar; the time it takes to run the exchange algorithm
%
%This algorithm takes in a NOA, deletes user-specified inactive factors,
%adds new runs, codes the full design matrix, then folds the design to
%break the aliasing between columns of interest specified by the order of
%foldover. This algorithm uses subfunctions: code, deletefactors,
%efficiency, and mbalance.
%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%%

%Declare fields for design matrix structure
X.value = NOA;
X.size = size(X.value);
X.code = zeros(X.size);
X.index = zeros(X.size(2));
X.D = 0;
X.bm = 100;
X.Ds = zeros(1,X.size(2));

m = X.size(1); % number of rows in design matrix

% Code Matrix
% function that codes the original design matrix into coded variables

[X] = code(X);
```

```
% Delete factors and recompute efficiency
%check if factors need to be deleted and pass those columns indices
if answer == 'Y';
   del_fac;
%sub-function that deletes unwanted factors from design matrix
[X, original_index] = deletefactors(X,del_fac);
else
end

%sub-function to calculate the D and Ds efficiency to see if deleted
%effects improve efficiency of design (both D and Ds)
[X] = efficiency(X);

%% Create Foldover
%find columns that have Ds == 1 and use them to start foldover, if no
%columns are orthogonal then take the column with Ds > 0.9
% index_of_indices = find(X.Ds == 1);
% new_index = original_index(index_of_indices);
% if isempty(new_index)
%     new_index = find(X.Ds > 0.9);
% end
%
% [order,~,I] = setxor(new_index,order);
% order_index = size(new_index,2)+ I;

% [order,Ind] = sortrows(temp',1);
% order = order(:,2)';

%create random column of rows to add to each column, provides initialized
%vector to perform pairwise column swap to search for best overall design
%based on Lu's projection criteria and Ds efficiency

two_level = zeros(add_run,1);
three_level = zeros(add_run,1);

for i=1:add_run
    two_level(i) = mod(i,2);

    three_level(i) = mod(i,3);

end

[two_level] = code(two_level);
[three_level] = code(three_level);
```

```
 two_level = sort(two_level,1,'ascend');
 three_level = sort(three_level,1,'ascend');

%% Starting Matrix
%starting matrix for foldover; start with columns that are already
%orthogonal in the design matrix and have not been dropped.  Make the
%columns as orthogonal to each other as possible as each column is added.
tstart = tic;

tempMat.value = X.code;
% cat(2,X.code(:,new_index),X.code(:,order_index));
tempMat.code = tempMat.value;

%coltempMat = size(tempMat,2);
tempMat.level = X.level;
% cat(2,X.level(new_index),X.level(order));
% templevel(1,size(index)+1:n) = level(order);

    for i = 1:size(tempMat.level,2)
         switch tempMat.level(i)

            case 1
            tempMat.code(m+1:m+add_run,i) = two_level;
            X.code(m+1:m+add_run,i) = two_level;

             case 2
            tempMat.code(m+1:m+add_run,i) = three_level;
            X.code(m+1:m+add_run,i) = two_level;
         end

    end

%Perform columnwise-pairwise changes on the additional runs

%Column pairwise routine; this routine swaps elements of the column and
%computes the estimation efficiency of the column (i.e. orthogonality) to
%the other columns in the design matrix.  Once an orthogonal column has
%been found the routine will break and go on to the next column.

% Define Variables
% q - outerloop counter that moves through the columns of the design matrix
        % one by one

for restart = 1:num_restart
```

```
    for q = 1:size(tempMat.level,2)

        %the temporary array is one row larger than the column being searched,
        %that is for the Ds value to be stored
        %Temp = zeros(m+add_run,1);

        for i = 1:num_search
            a = 1;
            while a == 1

%create two random column elements to swap from the added rows
            j= m+ randi(add_run,1);
            k= m+ randi(add_run,1);

            % check if j = k, and if col elements same since we don't want to
            % swap the same values and we don't want to "swap" the same element
            % in the array.
            if j ~= k && tempMat.code(j) ~= tempMat.code(k)
                a = 0;
            end

            %swap the column elements using temporary storage
                temp = tempMat.code(j,q);
                tempMat.code(j,q) = tempMat.code(k,q);
                tempMat.code(k,q) = temp;
            end

            %now that column elements have been swapped, evaluate the Ds
            %efficiency and B(m) (m-balance) criteria
               [tempMat]=efficiency(tempMat);

               [bm] = mbalance(tempMat);

                if  bm < X.bm
                    % update design matrix with best column to date
                    X.code(m+1:m+add_run,q) = tempMat.code(m+1:m+add_run,q);
                    X.Ds(q) = tempMat.Ds(q);
                    X.D = tempMat.D;
                    X.bm = bm;

                 elseif bm == X.bm && tempMat.Ds(q) > X.Ds(q)
                    X.code(m+1:m+add_run,q) = tempMat.code(m+1:m+add_run,q);
                    X.Ds(q) = tempMat.Ds(q);
                    X.D = tempMat.D;
```

```
                else
                    % return to original permutation
                    temp = tempMat.code(j,q);
                    tempMat.code(j,q) = tempMat.code(k,q);
                    tempMat.code(k,q) = temp;

                end

                if X.Ds(q) == 1 || bm == 0

                %once an orthogonal array has been found stop searching
                % and move to the next column;
                break
                end
        end
    end
end
[X]=efficiency(X);
telapsed = toc(tstart);
end

function [XD, level] = code(XD)
%this function transforms the design matrix into coded variables
% Define Variables

% level - the number of factor levels in a given column
% ind - index of the factor column, used to find the index of each factor
% level so that the reassignment for coding is easier
% index_0 - index of all vector elements with the value 0
% index_1 - index of all vector elements with the value 1
% index_2 - index of all vector elements with the value 2

    if isstruct(XD); %1st branch used if variable to code is a structure

    fieldnames(XD);

    %initialize level variable
    level= zeros(1,XD.size(2));

    for ind=1:XD.size(2)
        level(ind) = max(XD.value(:,ind));

        switch level(ind)
            case 1
                index_0 = find(XD.value(:,ind)==0);
```

```
                index_1 = find(XD.value(:,ind)==1);
                XD.code(index_0,ind)=-1;
                XD.code(index_1,ind)= 1;

            case 2
                index_0 = find(XD.value(:,ind)==0);
                index_1 = find(XD.value(:,ind)==1);
                index_2 = find(XD.value(:,ind)==2);
                XD.code(index_0,ind)=-1;
                XD.code(index_1,ind)= 0;
                XD.code(index_2,ind)= 1;
        end

    end
    XD;
    XD.level = level;

    %code plain variable
    else
%         XD = zeros(XD);
        %initialize level variable
        colXD = size(XD,2);
    level= zeros(1,colXD);

    for ind=1:colXD
        level(ind) = max(XD(:,ind));

        switch level(ind)
            case 1
                index_0 = find(XD(:,ind)==0);
                index_1 = find(XD(:,ind)==1);
                XD(index_0,ind)=-1;
                XD(index_1,ind)= 1;

            case 2
                index_0 = find(XD(:,ind)==0);
                index_1 = find(XD(:,ind)==1);
                index_2 = find(XD(:,ind)==2);
                XD(index_0,ind)=-1;
                XD(index_1,ind)= 0;
                XD(index_2,ind)= 1;
        end

    end
    XD;
```

```
    end
end

function [XD, original_index] = deletefactors(XD, del_fac)
    %This function takes an array (index) of factors to be deleted from the
    %design matrix

    %Define Variables

    % i - counter to index the array
    % del_fac = array of indices of factors to be deleted
    % j - index to move through the del_fac array element by element
    % k - index; keeps track of original indices of design matrix

    fieldnames(XD);
    index = zeros(1,XD.size(2));
    %index matrix columns to keep track of columns to be deleted in loop below
    for i = 1:XD.size(2)
        index(i)=i;
    end

    %delete the inactive effects
    for i=1:size(del_fac,2)

        j = del_fac(i);
        if i ==1
            index(j) = [];
            XD.value(:,j) = [];
            XD.code(:,j) = [];
            XD.Ds(:,j) = [];
            XD.level(:,j) = [];
        else
            k = find(index == j);
            XD.value(:,k) = [];
            XD.code(:,k) = [];
            XD.Ds(:,k) = [];
            XD.level(:,k) = [];
            index(k)=[];
        end

    end
    original_index = index;
end

function[X] = efficiency(X)
```

```
        %EFFICEINCY - calculates the Ds and D efficiencies of the design matrix

        %Declare variables
        %m - number of rows in X
        %n - number of columns in X
        %Xprime - design matrix with standardized columns
        %D - D efficiency of design matrix (scalar)
        %Ds - Ds efficiency for a given column
        %Dsvec - Ds efficiency for each column (vector)

            fieldnames(X);

            X.size = size(X.value);
            m = X.size(1);
            n = X.size(2);
            %Calculate the D-efficiency of the NOA
            Xprime = zeros(m,n);

            %standardize each of the columns of X to use in Deff calculations
            for j = 1:n
                Xprime(1:m,j) = X.code(1:m,j)/norm(X.code(1:m,j));
            end

            %Defficiency calculation
            D = det(Xprime'*Xprime)^(1/(X.size(2)+1));

            %Store Defficiency as part of the X structure
            X.D = D;

            %Calculate the Ds efficiencies for each column of X
            Dsvec = zeros(1,n);

                for i = 1:n
                    Xi = X.code;
                    Xi(:,i)=[];
 Ds = (X.code(:,i)'*X.code(:,i)-X.code(:,i)'*Xi*(Xi'*Xi)^(-1)*Xi'*X.code(:,i))
                 /(X.code(:,i)'*X.code(:,i));
                    Dsvec(1,i) = Ds;

                end

                X.Ds = Dsvec;

end
```

```
    function [bm] = mbalance(X)
%This function computes the mbalance of a nearly orthogonal array

%move through design matrix so that xj < xk, each pair of columns gets
%computed

%Define variables
numcol = size(X.code,2);
numrow = size(X.code,1);
blm = zeros(1,nchoosek(numcol,2)); %
k = 1; %initialize index for blm vector

fieldnames(X);

  % define all possible level combinations
            lc_1 = [-1,-1];
            lc_2 = [-1,1];
            lc_3 = [0,-1];
            lc_4 = [0,1];
            lc_5 = [1,-1];
            lc_6 = [1,1];

%index for xj
for col = 1:numcol

    %index for xk
    for col2 = col+1:numcol

        %determine number of level combinations
        if X.level(col) == 2 && X.level(col2) ==1
            n = 6;
            nlc = zeros(1,n);

            %count each level combination
            for row = 1:numrow

                 if isequal(X.code(row,[col,col2]), lc_1)
                        nlc(:,1) = nlc(:,1)+1;

                 elseif isequal(X.code(row,[col,col2]),lc_2)
                        nlc(:,2) = nlc(:,2)+1;

                 elseif isequal(X.code(row,[col,col2]),lc_3)
                        nlc(:,3) = nlc(:,3)+1;
```

```
        elseif isequal (X.code(row,[col,col2]),lc_4)
               nlc(:,4) = nlc(:,4)+1;

        elseif isequal(X.code(row,[col,col2]), lc_5)
               nlc(:,5) = nlc(:,5) + 1;

        elseif isequal(X.code(row,[col,col2]), lc_6)
               nlc(:,6) = nlc(:,6) + 1;
       end

    end

    for p = 1:size(nlc,2)
    blm(1,k) = blm(1,k) + (nlc(p) - numrow/n)^2;
    end

    k = k+1;

elseif X.level(col) == 1 && X.level(col2) ==1
    n = 4;

    nlc = zeros(1,n);

    %count each level combination
    for row = 1:numrow

        if isequal(X.code(row,[col,col2]), lc_1)
               nlc(:,1) = nlc(:,1)+1;

        elseif isequal(X.code(row,[col,col2]),lc_2)
               nlc(:,2) = nlc(:,2)+1;

        elseif isequal(X.code(row,[col,col2]),lc_5)
               nlc(:,3) = nlc(:,3)+1;

        elseif isequal (X.code(row,[col,col2]),lc_6)
               nlc(:,4) = nlc(:,4)+1;

       end

    end

    for p = 1:size(nlc,2)
    blm(1,k) = blm(1,k) + (nlc(p) - numrow/n)^2;
    end
```

```
            k = k+1;
        end
    end
end

bm = sum(blm/nchoosek(numcol,2));

end
```

Appendix B. ITEA Live-Virtual-Constructive Simulation Conference Presentation

Statistical Analysis for a Data Link Experiment Using Live, Virtual, Constructive Simulation

Casey Haase, Capt, USAF

Raymond Hill, Ph.D

Overview

- Experiments Using LVC
- Case Study Background
- Planning a Statistically Valid Experiment
- Case Study
  - Planning the Experiment
  - Compare Alternative Experimental Designs
  - Design Chosen for Experiment
- Additional Planning Considerations For LVC
- Summary

LVC for Experiments

- LVC is being considered for analytical purposes
  - Testing Systems in a Joint Environment
  - System of Systems context
- LVC can build large, complex test environments
  - Introduces new experimental design and analysis issues
  - Collecting quality data requires changes in the way users view LVC
- Experimental design techniques are necessary to collect quality data from tests using LVC

Guidelines for Planning Statistically Valid Experiments

Planning a Statistically Valid Experiment

1. Define and State Test Objectives
2. Choose Factors and Levels
3. Select Response Variable(s)
4. Choose an Experimental Design
5. Conduct Experiment
6. Analyze Data
7. Draw Conclusions and/or Make Recommendations

D. E. Coleman and D. C. Montgomery
A Systematic Approach to Planning for a Designed Industrial Experiment
Technometrics, Volume 35, No 1, 1993

Case Study

Case Study Background

- Aircraft are unable to transmit in denied access area
  - Transmitting makes them vulnerable to air defenses
  - Follow pre-planned routes to strike targets
    - Minimizes probability of detection
    - Limits ability to strike targets of opportunity
- Multifunctional Advanced Data Link (MADL)
  - Potentially allow friendly aircraft to talk in denied access area
  - Low probability of detection
- AF Simulation and Analysis Facility (SIMAF) tasked to assess MADL usefulness and suitability using LVC

Test Setup / Constraints

- Use LVC distributed simulation
  - SIMAF (virtual cockpit and constructive simulation host)
  - Second location (virtual cockpit)
- Test will be conducted in two phases (incremental tests)
  - Phase I – Assess MADL assuming perfect net capability
  - Phase II – Assess MADL with realistic capability degradation
  - Two weeks per phase
- Small incremental tests with one objective per test are preferred to one big test with multiple objectives

Test Setup / Constraints

- Two different aircrews will be used each week
  - 4 aircrew total
  - One pilot per virtual cockpit per week
- Human factors experts suggest aircrew can conduct 4-6 runs per day before fatigue affects data quality
- Potential for 12 – 16 runs per week

Test Setup / Constraints

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- Test Environment
  - The test will be conducted using a typical operational strike environment
    - 4 different operational scenarios (vignettes)
    - Friendly strike aircraft
    - Friendly fighters
    - Enemy fighter aircraft
    - Various targets
    - Various bombing routes

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Experiment Objectives

Experiment Planning

1. **Define and State Test Objectives**
2. Choose Factors and Levels
3. Select Response Variable(s)
4. Choose an Experimental Design

<u>Stated Objective:</u>

Assess the usefulness of information passed over the MADL link

- Determining test objective - 5 months
- Difficulties:
  - Hard to focus on defining objectives
    - Many people get involved – not in agreement
    - Easier to focus on building LVC, not what to study
  - Building consensus for conducting series of small experiments vice single big experiment

Experiment Factor Levels

Experiment Planning

1. Define and State Test Objectives
2. **Choose Factors and Levels**
3. Select Response Variable(s)
4. Choose an Experimental Design

- Potential Factor List with levels

Factors	Type	Levels
MADL	Categorical	3
Vignettes	Categorical	4
Route	Categorical	3
Target Position	Categorical	2
# of Red Air	Quantitative	2
# of Blue Air	Quantitative	2
Node Position	Quantitative	2
Quality of Service	Categorical	2

Response Variable

Experiment Planning

1. Define and State Test Objectives
2. Choose Factors and Levels
3. **Select Response Variable(s)**
4. Choose an Experimental Design

- No direct measurement response variable
  - Cannot directly measure "usefulness"
- Must use a surrogate measure
  - Aircrew Surveys
  - 5 point scale
    - Crew briefed about meaning of each level
    - Comments from aircrew cross check survey measurement
- Caution required
  - Must ensure response variable actually reflects stated objective

Compare Experimental Designs

Experiment Planning

1. Define and State Test Objectives
2. Choose Factors and Levels
3. Select Response Variable(s)
4. **Choose an Experimental Design**

Four different experimental designs were considered in the design process

1. 2-factor factorial design (12 runs) replicated
2. Split-Plot design (24 runs) un-replicated
3. Orthogonal Array (12 runs) replicated
4. Nearly Orthogonal Array (12 runs) replicated

Each has strengths and weakness when actually employed

Compare Experimental Designs

Experiment Planning

1. Define and State Test Objectives
2. Choose Factors and Levels
3. Select Response Variable(s)
4. **Choose an Experimental Design**

1. 2-factor factorial design (12 runs) replicated
   - MADL and Vignettes are only factors considered
   - **Strengths:**
     - Simple
     - Easy to analyze
   - **Weakness:**
     - Too Simplistic
     - Ignores environmental factors
     - Doesn't account for human variability
     - Could introduce learning bias

Compare Experimental Designs

Experiment Planning

1. Define and State Test Objectives
2. Choose Factors and Levels
3. Select Response Variable(s)
4. **Choose an Experimental Design**

2. Split-Plot Design (24 runs) un-replicated
   - 3 Factors considered
     - MADL, Vignette, Crew
   - MADL factor changed less frequently than other factors to prevent crew confusion
   - **Strengths:**
     - Efficient experiment
     - Only way to design for restricted run order
   - **Weakness:**
     - More difficult to analyze
     - 2 error terms
     - Whole plot factors get less precise estimates
     - Still ignores some environment effects

Compare Experimental Designs

Experiment Planning

1. Define and State Test Objectives
2. Choose Factors and Levels
3. Select Response Variable(s)
4. **Choose an Experimental Design**

3. Orthogonal Array (12 runs) replicated
   - 4 Factors considered
     - MADL, Vignette, Target Location, Route
   - **Strengths:**
     - Environmental factors included in the design
     - Can accommodate up to 12 factors and still estimate the main effects
     - Replication allows for more precise estimate of error
     - Some interaction effects can still be estimated
     - Straightforward to analyze main effects
   - **Weakness:**
     - Analysis becomes complicated if interactions present
     - Lose ability to estimate all high order interactions

Compare Experimental Designs

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Experiment Planning

1. Define and State Test Objectives
2. Choose Factors and Levels
3. Select Response Variable(s)
4. **Choose an Experimental Design**

4. Nearly Orthogonal Array (12 runs)
   - 6 Factors considered
     - MADL, Vignette, Target Location, Route, Node Position, Quality of Service
   - **Strengths:**
     - Environmental factors included in the design
     - Accommodates more factors than Orthogonal Array
     - Replication allows for more precise estimate of error
     - Some interaction effects can still be estimated
     - Straightforward to analyze main effects
   - **Weakness:**
     - Analysis more complicated – correlated estimates
     - Correlation creates less precise estimates

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Chosen Experimental Design

- Phase I
  - Orthogonal Array (3 x 4 x 2^2)
    - 4 factors
    - Allows for replication
    - Accommodates environmental factors
    - Can obtain more precise estimates of effects and error
- Phase II
  - Nearly Orthogonal Array
    - 6 factors (3 x 4 x 2^4)
      - Quality of Service and Node Position added to design
    - 2 additional factors make orthogonal design impossible
    - Allows for replication
    - Caution when analyzing, error and effects may contain bias

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Lessons Learned from LVC Case Study

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Additional Planning Considerations for LVC

The AFIT of Today is the Air Force of Tomorrow.

- LVC was originally intended for training purposes
  - Little analytical rigor necessary for training
  - Little up-front planning required for **post-ops analysis**
  - High-fidelity, complex, noisy environments preferred
- T&E shares many resource requirements with training
  - Hence, LVC becoming central to DoD test strategy
- Requires new paradigm to use LVC effectively
  - Extensive up-front planning required
  - Excessive fidelity & complexity can & will ruin experiment

Additional Planning Considerations for LVC

The AFIT of Today is the Air Force of Tomorrow.

- Defining clearly stated objectives more difficult with LVC
  - As ability to create test environment gets better the number of potential test objectives get larger
  - There is a lure toward complexity
    - Requires discipline to scope test with realistic goals
    - Requires discipline to keep experiment within scope of study
    - Just because you can do something doesn't mean you should
- Response variables are not always obvious with LVC
  - Objectives are often qualitative
  - Response Variables are not obvious
  - Surrogate measures need to accurately reflect objectives

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Additional Planning Considerations for LVC

- Testing in Joint Environments using LVC
  - Joint mission environments contains copious noise
  - Experimenters must:
    - Be aware of noise
    - Account for noise by using statistical noise control methods
  - Human Operators are one of the largest sources of noise
- LVC experiments produce abundance of data
  - Extra effort required to plan
  - Else effort wasted collecting, sifting, and analyzing data

Summary

The AFIT of Today is the Air Force of Tomorrow.

- DoD seeks use of LVC for analytical purposes (T&E)
- Experimental design creates solid framework for conducting experiments that result in valid conclusions for LVC experiments
- LVC introduces additional considerations into the experimental design process
- Case study illustrates benefits of using statistical experimental design methods for LVC

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Applying Experimental Design to Live, Virtual, and Constructive (LVC) Evironments

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Abstract

Live, virtual, and constructive (LVC) simulation is a test capability being considered by the Department of Defense (DoD) to test systems and system of systems in realistic joint operation mission environments. As joint operations have increased, the need to test systems intended for joint operations in a robust joint environment has become more apparent. Unfortunately, the number of assets needed for the testing of joint operations (density of assets) as well as the variety of assets required (diversity of assets) prohibit full testing of new systems in joint operations. DoD's expanding LVC capabilities, a growing capability in training realms, are being seriously examined for analytical purposes. This work explores the analytical opportunities of LVC and presents the use of statistical experimental design principles as a necessary component of the LVC analytical tool kit. The work is presented in the context of an actual case study involving the Air Force Simulation Facility (SIMAF) and their use of LVC to examine an analytical question associated with a major weapons system.

Keywords
Live-Virtual-Constructive (LVC), Statistical Experimental Design, Experimental Design Process

1. Introduction
LVC is a central component of the DoD's joint mission test strategy due to its ability to connect geographically dispersed test facilities over a persistent network and potentially reduce test costs. LVC is able to create the necessary variety and density of assets representative of a joint environment and scale those assets to the appropriate level of fidelity based on system maturity. In the early stages of system development simple joint mission environments can be developed using mostly constructive entities with live and virtual entities added as the system matures. While the cost of LVC experiments can be significant, it often maintains a cost advantage to joint mission experiments using only live assets. Furthermore, LVC simulation can build joint mission scenarios of greater complexity than can be assembled at any single DoD test facility.

1.1 Live-Virtual-Constructive Simulation
LVC is a hybrid simulation environment assembled from a collection of autonomous distributed simulation applications (live, virtual, or constructive applications) that interact by sharing current state information over a persistent network. LVC simulations can provide experimenters with several benefits not found in purely live system tests. Expensive test assets can be simulated at a fraction of the cost of using live assets thereby reducing the overall cost of a test program. The reduced cost of LVC experiments can sometimes allow for more runs and consideration of more design factors resulting in more information than could be obtained in a similar test only utilizing live assets.

The virtual and constructive elements of LVC give experimenters increased flexibility in designing the experiment. In some situations completely randomized designs can be used instead of more complex split-plot designs often found in live test because the virtual and constructive elements can be easily reconfigured before each run. LVC also gives the user greater control over the test environment thus improving the precision of effect and error estimates' and providing greater capabilities to instrument the experiment to collect meaningful response data.

1.2 Change the LVC Paradigm

LVC has traditionally been used as a training vehicle in the DoD. Consequently, an analysis paradigm has emerged where post-operation analysis is an afterthought in LVC operations. Furthermore, the training community prefers complex, noisy environments because it appropriately prepares combatants for the “fog of war". For analytical purposes, such as test, where results are used in objective decision making“fog" is usually a detriment because it obscures the underlying factors that are driving system performance and effectiveness. For test to be effective we need to abstract out certain parts of the representative environment so that we can obtain clean estimates of the factor effects of interest on the system response. If LVC is going to be successfully implemented as a core test capability LVC practice will require a fundamental shift in the way LVC users currently employ the technology. The next section proposes statistical experimental design as a firm analytical foundation for conducting experiments using LVC. Section 3 illustrates the application of statistical experimental design to LVC experiments and highlights special considerations that arise when using LVC for experimentation and analytical purposes.

2. Statistical Experimental Design

Experimental design is a strategy of experimentation to collect and analyze appropriate data using statistical methods resulting in statistically valid conclusions. Statistical designs are quite often necessary if meaningful conclusions are to be drawn from the experiment. If the system response is subject to experimental errors then statistical methods provide an objective and rigorous approach to analysis.

The three basic principles of statistical experimental design are randomization, replication, and blocking [5]. Randomization usually ensures that experimental observations are independent of one another from run to run; a necessary assumption for statistical methods. Replication is an independent repeat of each factor combination and provides an unbiased estimate of the pure error in an experiment. This error estimate is the basic unit of measurement for determining whether observed differences in the data are statistically different. Blocking is a design technique that improves the precision of estimates when comparing factors. Blocking accounts for the variability of nuisance factors; factors that influence the outcome of the experiment but are not of interest in the experiment.

2.1 An Experimental Design Process

To apply statistical methods to the design and analysis of experiments, an entire test team must have a clear understanding of the objectives of the experiment, how the data is to be collected, and a preliminary data analysis plan prior to conducting the experiment. Coleman and Montgomery [3] propose guidelines to aide in planning, conducting, and analyzing experiments. An overview of their guidelines follow.

1. **Recognition and statement of the problem.** Every good experimental design begins with a clear statement of what is to be accomplished by the experiment. While it may seem obvious, in practice this is one of the most difficult aspects of designing experiments. It is no simple task to develop a clear, concise statement of the problem that everyone agrees on. It is usually necessary to solicit input from all interested parties: engineers, program managers, manufacturer, and operators. An LVC experiment may involve a very large team with very differing ideas of how to use the LVC.

2. **Selection of the response variable.** The response variable is a measurement of the system response as a function of changes in input variable settings. When selecting the response variable, the experimenter should ensure that it provides useful information about the system under study as it relates to the objectives of the experiment. The best response variables directly measures the problem being studied. Sometimes a direct response is unobtainable and a surrogate measure must be used instead.In LVC, additional consideration is given to instrumentation requirements to obtain response measures.

3. **Choice of factors, levels, and range.** Factors are identified by the design team as potential influences on the system response variable. When deciding which factors should be included in the experiment two categories of factors frequently emerge: design and nuisance factors. Design factors can be controlled by either the design of the system or the operator during use. Nuisance factors affect the response of the system but are not of particular interest to experimenters. A subject matter expert working in conjunction with the statistical experimental design

expert is invaluable when choosing the range of factors levels. In the LVC environment, the human element must be considered as the human operator may be a factor of interest, a nuisance factor, or even in some cases the response of interest.

4. **Choice of experimental design.** Choosing an experimental design can be relative easy if the previous three steps have been done correctly. Choosing a design involves considering the sample size, randomizing the run order, and deciding whether blocking is necessary. Software packages are available to help generate alternative designs given the number of factors, levels, and number of runs available for the experiment. More unique designs like orthogonal arrays and nearly orthogonal arrays can be created with available computer algorithms.

5. **Performing the experiment.** In this step it is vital to ensure that the experiment is being conducted according to plan. Conducting a few trial runs prior to the experiment can be helpful in identifying mistakes in planning thus preventing a full experiment from being wasted. For LVC, there is the additional discipline required to not change the LVC envrionmental setup between experimental runs.

6. **Statistical analysis of the data.** If the experiment was designed and executed correctly the statistical analysis is not elaborate. Often the software packages used to generate the design help to seamlessly analyze the experiment. Hypothesis testing and confidence interval estimation procedures are very useful in analyzing data from designed experiments. Common analysis techniques include analysis of variance (ANOVA), regression, and multiple comparison techniques. A common statistical philosophy is that the best statistical analysis cannot overcome poor experimental planning.

7. **Conclusions and recommendations.** A well designed experiment is meant to answer a specific question or set of questions. Hence, the experimenter should draw practical conclusions about the results of the experiment and recommend an appropriate course of action. The beauty of a well designed and executed experiment is that once the data have been analyzed the interpretation of the data should be fairly straightforward.

Coleman and Montgomery [3] give details on the steps of experimental design. Additionally, most texts on experimental design, including Montgomery [5], provide some experimental design methodology.

2.2 Additional Design Considerations for LVC

The Coleman and Montgomery [3] guidelines offer comprehensive general guidelines for industrial experiments. However, an LVC experiment seems quite non-industrial. Some additional challenges to designing to designing experiments for LVC are listed below.

1. **Properly Scoping the LVC Environment.** Scoping LVC experiments require more careful treatment than most traditional experiments. LVC is flush with capability; users and experimenters can build very large, complex, joint mission environments. Experimenters are often enticed to create environments that are more complex than required to actually satisfy the experiment's objective.

2. **Quantifying Qualitative Objectives.** Objectives in LVC experiments are often qualitative in nature. LVC is used primarily for joint mission tests to evaluate system-of-systems performance, joint task performance, and joint mission effectiveness. Nebulous qualities such as task performance and mission effectiveness are difficult to define and measure.

3. **Designing for Mixed Factor Levels with Limited Resources.** Joint mission environments are complex often containing many mixed-level, qualitative factors with scant resources available. Mixed-level factors refers to multiple factors where at least one factor contains a differing number of levels than the other factors. Often mixed-level designs require a large sample size making them inappropriate for tests that demand a small sample size due to resource constraints.

4. **Obtaining Clean Estimates in Noisy Test Environments.** The joint mission environment contains copious sources of noise that must be prudently considered. Noise in the test environment can be harmful to an experiment if appropriate measures are not taken to control it or measure it. Effects that are thought to be important may only appear to be so because of experimental error and not the factor of interest.

5. **Human System Integration (HSI) in Experimental Designs.** HSI principles should be applied to LVC experiments since LVC is a software system that requires extensive human interaction. Human operators are oftentimes the largest contributor of noise in the experiment and thus should only be used as necessary in LVC experiments. The right tradeoffs between including human subjects in the experiment and quality of data required must be made.

These design challenges are illustrated in the case study below and techniques are presented to successfully deal with them, thus ensuring experiment objectives are met.

3. Conducting a Data Link Experiment with LVC [1]

Currently there are aircraft that can only receive Link-16 communications from Command and Control (C2) assets in denied access environments. The Multifunctional Advanced Data Link (MADL) is a technology that would allow aircraft to transmit to other friendly forces in a denied access environment without significantly increasing the aircraft vulnerability to enemy air defense. The Air Force Simulation and Analysis Facility (SIMAF) was tasked with assessing the suitability of the MADL data link for aerospace operations in a denied access environment using a distributed LVC environment. A factor screening test strategy was chosen with two separate test events each conducted with two weeks of testing for each event. Aircrew are limited with only two aircrew available per week per test phase.

Current operation procedures have the aircraft following pre-planned routes that minimize the probability of detection by enemy integrated air defense (IADS). We are interested in determining if communicating in the denied access environment is useful enough to justify acquiring such capability. This represents an ideal example of using computing power to ascertain the operational effectiveness of proposed upgrades without investing in changes to the weapon systems.

3.1 Defining Experiment Objectives

The first task in the experimental design process was to clearly define the problem to be studied. Defining the objective of the experiment was the most difficult task in the design process. Four to five months were spent determining the objective of the experiment because influential members of the planning team were focused on defining the requirements for the LVC test environment instead of the test objective; the test should drive what LVC provides. This distraction slowed the progress of the planning phase appreciably, but is really attributable to the paradigm shift associated with using LVC for new purposes. Ultimately, two related objectives were chosen, one for each phase of the test program.

1. **Phase I:** Assess the usefulness of data messages passed on the MADL network assuming a perfect network configuration and performance.
2. **Phase II:** Assess the usefulness of the MADL network given a realistic level of degraded network performance.

Breaking the test into two phases is important because it ensures that factor effects are easily identifiable in the data analysis. Consider what would happen if only phase II of the experiment were conducted and the degraded network makes the system so cumbersome that aircrew give it an unfavorable rating. This test method makes it more difficult to tell whether the MADL messages and delivery capabilities are problematic or whether poor network service is the problem. Experimental design helps to focus and clarify the objectives and the data required to answer the objective.

3.2 Choosing Factors of Interest and Factor Levels

The factors of interest came primarily out of the requirements for the LVC test environment since several environmental factors were to be varied across runs. Brainstorming resulted in an initial set of 10 factors with further consideration reducing the set to 4 factors for phase I (see Table 1) and 6 factors for phase II (see Table 2). Additionally, one of the MADL factor levels was dropped from the test requirements leaving three levels as displayed in Table 3. Besides MADL as the factor of interest, the operational context (vignettes), ingress route, target location, and aircrew were included as factors in phase I of the experiment. The three latter factors were not of primary interest but were chosen to prevent learning aircrew during the experiment and its biasing of the outcome. The routes and target locations will be varied systematically and blocking will be used on the aircrew factor. These techniques help guard the experiment against excessive noise introduced by human operators.

[1] This case study is based on an actual event with the specific weapons systems unnamed

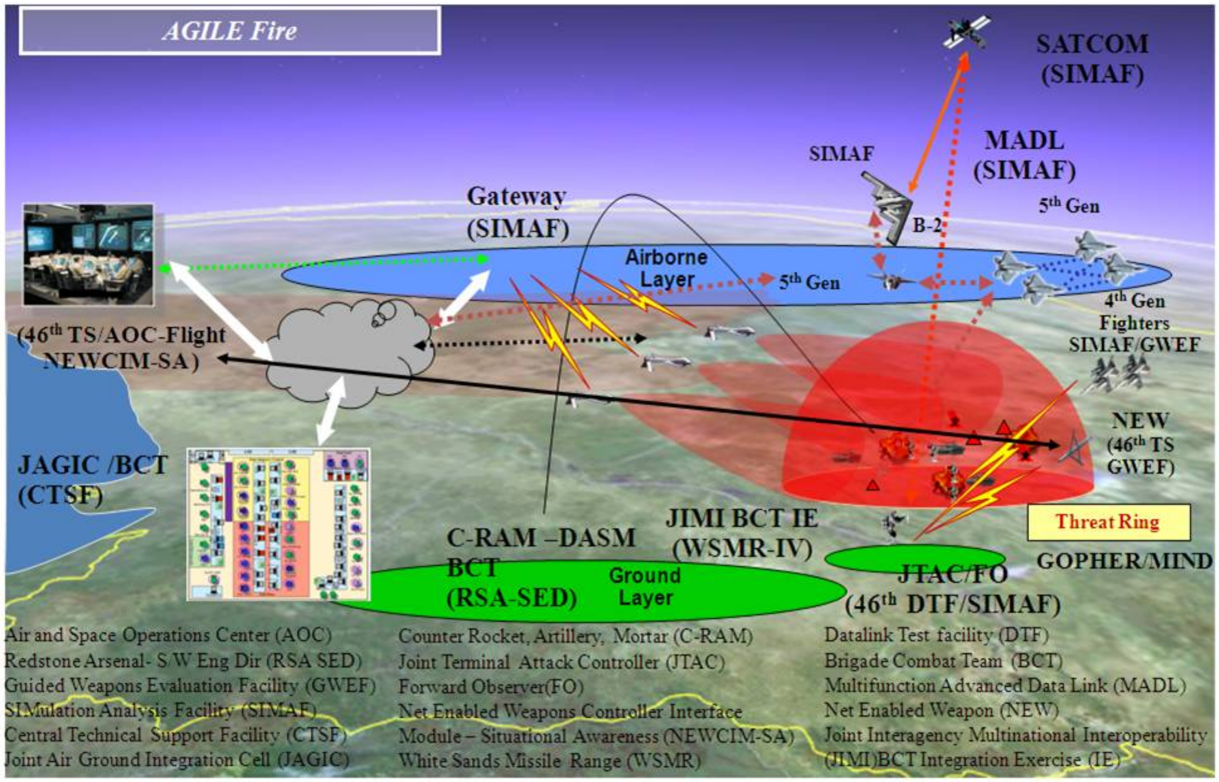

Figure 1: Notional LVC Representation of a Joint Operation Network in a Denied Access Environment [1]

In phase II, two additional factors, node position and quality of network service are to be added to the phase I design (see Table 2). The additional factors allow measure of the variation caused by the degraded network. The rule of thumb for choosing factors of interest is to consider adding any setting or test condition changed from run to run as a factor of interest in the experiment.

3.3 Selecting the Response Variable.

Selecting an appropriate response variable can be problematic and can be particularly troublesome in LVC where many test objectives are qualitative. Quite often LVC tests employ user surveys and thus aircrew surveys were proposed. However, an LVC can collect system state data quite easily. Such state data, if properly defined provides potential insight into the potential benefits of improved system capabilities. The approach agreed upon was to use the aircrew survey as a primary response variable with the system state data collected to cross-check and verify aircrew responses and perceptions of the system capabilities.

Table 1: Final Set of Factors of Interest for Phase I

Factor	Level
MADL	3
Vignettes	4
Route	2
Target Location	2
Aircrew	2

Table 2: Final Set of Factors for Phase II

Factor	Level
MADL	3
Vignettes	4
Route	2
Target Location	2
Aircrew	2
Node Position	2
Quality of Service	2

Table 3: MADL Capabilities

Level	Available Communication Capability
1	Voice Only
2	Voice and Text
3	Voice, Text, and Machine-to-Machine

3.4 Choice of Experimental Design

LVC test requirements can be dynamic; the current case was no exception. Due to the ever-changing nature of the test requirements, several experimental designs were considered at various stages in the design process. As requirements were refined, more information about the size and scope of the experiment, the number of virtual and constructive simulation entities, environmental constraints, and aircrew availability came to light. A few of the designs that were contemplated are discussed below along with the rationale for considering each design.

Early on a 16-run 4 4 factorial design was considered. The design was discounted as overly simplistic because it ignored potentially important environmental factors. A split-plot design was considered since the experiment involves a restricted run order. The experimental design team was concerned that completely randomizing MADL capabilities would confuse operators due to large changes in available capability among levels. To avoid potential operator confusion the team considered a restricted run order with the run order chosen by fixing MADL at a particular level then randomizing the run order for the remaining factors. Once all runs have been completed for a given level of MADL then a new MADL level is chosen and the process is repeated until all test runs have been completed for all MADL levels. Any randomization restriction makes the use of split-plot analysis an imperative. Jones and Nachtsheim [4] shows that analyzing restricted run order experiments as completely randomized designs can lead to incorrect conclusions, a conclusion echoed in Cohen [2].

Future use of LVC for test is quite likely to examine impacts of new methods or technology and such examinations affect the design. In the current setting, the MADL-voice-only option was removed as a factor, run separately, and used as a baseline for performance measurement. The rest of the design, now smaller given the removal of a factor, was completely randomized. A replicated, 12-run orthogonal array with four factors was chosen for phase I. Four additional, replicated runs are completed using voice only to provide a baseline capability for comparison. The orthogonal array is a good option for factor screening experiments since it can provide estimates of each of the main effects and a few select interactions of interest.

Phase II will add two more factors to the experiment making an orthogonal array unusable for a sample size of 12. This means use of a nearly orthogonal array with replicates. If phase I reveals that some factors are inactive then those factors may be dropped from phase II and orthogonality in the design could potentially be restored.

4. Conclusions

LVC offers the T&E community a viable means for testing systems and system-of-systems in a joint environment. However, the added capability is not without cost. Planning joint mission tests using LVC is a challenging endeavor and requires careful upfront planning. The nature of LVC experiments requires experimenters to decide what should be studied in the experiment when defining the objectives. There is a strong lure toward adding unnecessary complexity in LVC which then entices experimenters to tackle excessively large tests with a misplaced hope that many questions about the system can be addressed simultaneously in that one large experiment. Experimenters need to be aware of this lure and exercise good test discipline by structuring LVC experiments to gain system knowledge incrementally thereby ensuring sound test results. This experimental design method is easily manageable for planning, executing, and analyzing data and builds system knowledge piece by piece.

LVC test environments have many sources of random error. Statistical experimental design techniques allow for objective conclusions when the system response is affected by random error. The system response variable should be chosen based on how well the measure relates to the experiment objectives. The response variable should measure this relation as directly as possible. Direct measurements are unobtainable for most LVC experiments so surrogate measures should be devised and examined for suitability. The factors of interest should be chosen from the set of environmental and design parameters that are thought to have an effect on the system response. A good rule of thumb when choosing factors is to consider including any test parameter that will be varied across the runs. Additional design considerations for LVC experiments were proposed to deal with the nuances of LVC. The additional design considerations are by no means exhaustive and should be updated as new challenges are encountered in LVC.

The reported data link experiment demonstrates how experimental design techniques can be used to ultimately better characterize the performance and effectiveness of a new system in a joint environment generated by LVC. The application of experimental design principles uncovered substantial mistakes in test planning and improved the overall test strategy by using an incremental test approach. Important factors that were initially missed were added to the system as a result of using statistical experimental design. Noise control techniques were used to improve the quality of the data collected. These techniques added necessary complexity to the experiment but improve data quality. The experiments also showed how innovative experimental designs, such as orthogonal and nearly orthogonal arrays, effectively accommodate the large, irregular factor space with limited test resources that are typical of most LVC experiments.

Following the experimental design process saved time, resources and more importantly wasted effort by systematically structuring the problem in a way to collect high quality data. Future LVC experiments can benefit greatly from using such statistical experimental design techniques. Data latency and non-standardized simulation environments are two additional issues affecting LVC experiments that were not addressed in this paper. The effects of these issues on the quality of data collected from LVC experiments is relatively unknown and needs to be explored further as the use of LVC increases.

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Appendix D. Blue Dart

The use of Live, Virtual and Constructive (LVC) Simulation environments are increasingly being examined for potential analytical use particularly in test and evaluation. The LVC simulation environments provide a mechanism for conducting joint mission testing and system of systems testing when fiscal and resource limitations prevent the accumulation of the necessary density and diversity of assets required for these complex and comprehensive tests.

The statistical experimental design process is re-examined for potential application to LVC experiments and several additional considerations are identified to augment the experimental design process for use with LVC. This augmented statistical experimental design process is demonstrated by a case study involving a series of tests on an experimental data link for strike aircraft using LVC simulation for the test environment. The goal of these tests is to assess the usefulness of information being presented to aircrew members via different data link capabilities. The statistical experimental design process is used to structure the experiment leading to the discovery of faulty assumptions and planning mistakes that could potentially wreck the results of the experiment.

Lastly, an aggressive sequential experimentation strategy is presented for LVC experiments when test resources are limited. This strategy depends on a foldover algorithm that we developed for nearly orthogonal arrays to rescue LVC experiments when important factor effects are confounded. This strategy combined with the foldover algorithm gives testers the option to use more aggressive test strategies while mitigating the accompanying risk to data quality.

Tailoring the Experimental Design Process to Live-Virtual-Constructive Experiments

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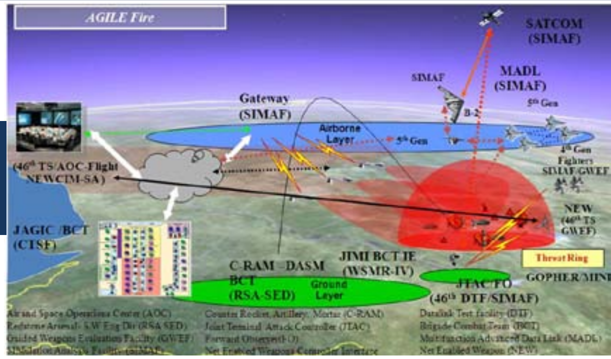


Research Objectives:

1. Apply the experimental design process to live-virtual-constructive experiments
2. Develop a set of experimental design "best practices" for LVC experiments

LVC Simulation

LVC is a hybrid simulation comprised of autonomous Live, Virtual, and Constructive simulations

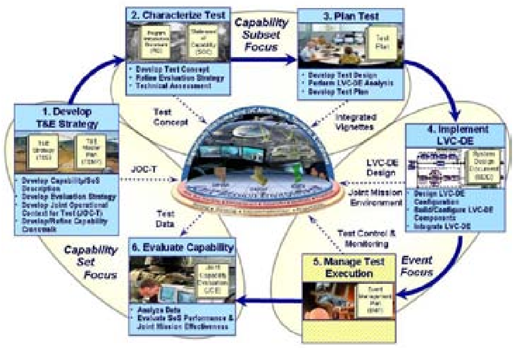
LVC connects geographically dispersed test assets to create a representative joint mission environment

LVC can be the only way to truly test systems in a joint mission environment

Capability Test Methodology

DoD test methodology for testing systems in a joint mission environment

Built on LVC simulation architecture



Experiment Design Issues Specific to LVC

1. Scoping vast experimental design options
2. Qualitative problem statements
3. Mixed factor levels & limited resources
4. Higher order interaction effects
5. Noisy test environments
6. Human System Integration Issues
7. Requires improved test discipline
8. Small experimental design restrictions

Useful Experimental Designs

Completely Randomized

Orthogonal Arrays

Nearly Orthogonal Arrays

D-Optimal Designs

Restricted Randomization

Split-Plot Designs

Nearly Orthogonal Arrays with Projection

Must be nearly balanced

The number of different factor level combinations differ from each other by no more than one.

Measured by the *B(m)* criterion

For every *m*-tuple of columns calculate

$$B_{l_1\cdots l_m}(m) = \sum_{\alpha_1,\cdots,\alpha_m} \left(n^{l_1\cdots l_m}_{\alpha_1\cdots\alpha_m} - \frac{n}{q_{l_1}\cdots q_{l_m}} \right)^2$$

then take the average of all $B_{l_1\cdots l_m}(m)$ values

$$B(m) = \sum_{1\le l_1<\cdots<l_m\le k} B_{l_1\cdots l_m}(m) \Big/ \binom{k}{m}$$

Desire highest D_s values

D_s measures the estimation efficiency of a given design column

$$D_s = \left\{ x_i^t x_i - x_i^t \left(X_{(i)}^t X_{(i)} \right)^{-1} X_{(i)}^t x_i \right\} \Big/ x_i^t x_i$$

$x_i =$ experimental design column of interest in X

$X_{(i)} =$ all other design columns in X

$0 \le D_s \le 1$

$D_s = 0$; factor estimate completely confounded

$D_s = 1$; most precise factor estimate possible

Methodology

New Experimental Design Strategy

1. Use NOA with projection to estimate higher order interactions
2. Replicate NOA
   a. Estimate pure experimental error
   b. Guard against outlier bias
   c. More precise estimates of factor effects
3. If the first replicate reveals that factors with low $D_s - efficiency$ significantly impact system response, then fold the design with remaining available runs.
4. Otherwise replicate the original design.

Foldover improves the $D_s - efficiency$ (precision) of factor effect estimates.

Foldover Algorithm

1. Start with original *n x k* design.
2. Delete inactive factors.
3. Augment original design with *r x k* matrix F_1.
4. Set T_1 (the number of pairwise exchanges).
5. Set T_2 (the number of algorithm restarts).
6. Start with column *i=1*. If the column is orthogonal to every other column go to step 7. Otherwise perform T_1 pairwise exchanges in (*n*+1) to (*n*+*r*) elements. If the exchange improves the *B(m)* criteria keep new column.
7. Let *i=i*+1 and repeat step 6 for all *k* columns.
8. Repeat steps 6 and 7, T_2 times.
9. Return the design with the minimum *B(m)* and maximum D_s design criteria.

$$\left|\frac{X}{F_1}\right| = \left|\begin{array}{cccccc}
0&0&0&0&0&0\\
0&1&1&0&0&1\\
0&0&1&1&1&0\\
0&1&0&1&1&1\\
1&0&1&0&0&1\\
1&1&0&0&1&0\\
1&0&1&1&1&1\\
1&1&0&1&0&0\\
2&0&0&1&1&1\\
2&1&1&1&1&0\\
2&0&0&0&0&1\\
2&1&1&0&0&0\\
\hline
0&0&0&0&0&0\\
1&1&1&1&1&1\\
2&0&0&0&0&0\\
0&1&1&1&1&1\\
1&0&0&0&0&0\\
2&1&1&1&1&1
\end{array}\right| \Longrightarrow \left|\begin{array}{cccccc}
0&0&0&0&0&0\\
0&1&1&0&0&1\\
0&0&1&1&1&0\\
0&1&0&1&1&1\\
1&0&1&0&0&1\\
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1&0&1&1&1&1\\
1&1&0&1&0&0\\
2&0&0&1&1&1\\
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2&0&0&0&0&1\\
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\hline
0&0&0&0&0&0\\
1&0&1&1&1&1\\
2&1&0&0&0&0\\
0&1&1&1&1&1\\
1&0&0&0&0&0\\
2&1&1&1&1&1
\end{array}\right| = \left|\frac{X}{F_2}\right|$$

$$B(2) = 8.1 \Longrightarrow B(2) = 6.8$$

$$D_s = 0.82 \Longrightarrow D_s = 0.93$$

Figure 1: Example of 1st iteration of foldover for 12-run NOA with 6 additional runs. Both design criterion improved so the new column is kept.

Results

	Factors									
Run	DL	V	NP	AC	EP	ES	FP	FS	R	TL
1	0	0	0	0	0	0	0	0	0	0
2	0	1	1	1	1	0	0	0	0	0
3	0	0	1	0	1	1	1	1	1	1
4	0	1	0	1	0	1	1	1	1	1
5	1	0	1	1	0	0	1	1	0	0
6	1	1	0	0	1	0	1	0	0	1
7	1	0	1	1	0	1	0	0	1	1
8	1	1	0	0	1	1	0	1	1	0
9	2	0	0	1	1	0	1	0	1	1
10	2	1	1	0	0	0	0	1	1	1
11	2	0	0	1	1	1	0	1	0	0
12	2	1	1	0	0	1	1	0	0	0
D_s	1.00	0.89	0.89	0.89	0.89	0.76	0.76	0.76	0.33	0.36

Table 1: NOA with projection. Route (R) and Target Location (TL) have low estimation efficiency

Performed Dropped Inactive Factors

Foldover Effects

	Factors					
Run	DL	V	NP	R	TL	AC
13	0	0	0	0	1	0
14	2	1	0	0	1	1
15	1	0	0	1	0	0
16	0	1	0	1	0	1
17	1	1	0	1	0	1
18	2	0	0	0	1	0
19	1	1	1	0	0	0
20	0	1	1	1	1	1
21	1	0	1	1	0	1
22	0	0	1	0	1	0
23	2	1	1	0	1	1
24	2	0	1	1	0	0
D_s	0.99	0.97	1.00	0.96	1.00	0.94
ΔD_s	-0.01	0.08	0.11	0.63	0.64	0.05
D	0.90					
$B(2)$	1.33					

Table 2: Foldover complement for Table 1. Significantly improved $D_s - efficiency$ for most factor effects. ΔD_s shows improvement in estimation efficiency

Variance Properties of Foldover Design

Factors	Unreplicated 24-run NOA [b]	Unreplicated 24-run OA
MADL	0.063	0.063
Vignette	0.043	0.042
Node Position	0.042	0.042
Route	0.043	0.042
Target Location	0.042	0.042
Aircrew	0.044	0.042

Foldover design has near-optimal variance properties

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14. ABSTRACT

The use of Live, Virtual and Constructive (LVC) Simulation environments are increasingly being examined for potential analytical use particularly in test and evaluation. The LVC simulation environments provide a mechanism for conducting joint mission testing and system of systems testing when scale and resource limitations prevent the accumulation of the necessary density and diversity of assets required for these complex and comprehensive tests. The statistical experimental design process is re-examined for potential application to LVC experiments and several additional considerations are identified to augment the experimental design process for use with LVC. This augmented statistical experimental design process is demonstrated by a case study involving a series of tests on an experimental data link for strike aircraft using LVC simulation for the test environment. The goal of these tests is to assess the usefulness of information being presented to aircrew members via different datalink capabilities. The statistical experimental design process is used to structure the experiment leading to the discovery of faulty assumptions and planning mistakes that could potentially wreck the results of the experiment. Lastly, an aggressive sequential experimentation strategy is presented for LVC experiments when test resources are limited. This strategy depends on a foldover algorithm that we developed for nearly orthogonal arrays to rescue LVC experiments when important factor effects are confounded.

15. SUBJECT TERMS

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