

A More Flexible Approach to Valuing Flexibility

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Abstract. The DOD routinely demonstrates its capability to develop phenomenal systems; however, these accomplishments are often tarnished by substantial cost and schedule overruns. While defense policies are continually being revised to address these problems, many believe that a more fundamental source of these overruns is the lack of flexibility in the systems being developed. But providing justification to invest in flexibility is a tough sell, as stakeholders struggle to quantitatively demonstrate the potential return on investment. Therefore, this paper introduces an alternative methodology for valuing the inherent ability of a system or design to accommodate change. The proposed methodology is essentially a modification of the current life cycle model and is premised on the notion that the need for capability changes in a program arise in a stochastic manner that can be incorporated into a continually updated, expected value model presented in terms of total program cost.

Introduction and Motivation

While the U.S. Department of Defense (DOD) routinely fields world-class weapons systems, there is tremendous opportunity for improving the acquisition of these systems, at least with respect to cost and schedule performance. In 2009, the Government Accountability Office found that of the DOD's major ongoing acquisition programs that provided relevant cost data, 69 percent reported an increase in total acquisition costs, with over 40 percent of those programs reporting an increase in acquisition unit costs of at least 25 percent. Moreover, on average, total research and development costs were 42 percent higher than originally estimated and systems were 22 months behind schedule. Moreover, the older the program, the more pronounced the cost overruns and schedule delays. Major defense programs that have been in development more than 15 years have seen an average 138 percent increase in acquisition costs, and over three years of schedule delays [1].

These systemic failings are widely known to those familiar with defense acquisitions, and there is nothing particularly surprising in the latest numbers. Nor is there anything surprising in how the DOD is likely to respond to the problem. If the past is any indication of the future, then we will soon see another acquisition reform effort spawned and promulgated with the expressed intent of reducing monetary waste and/or improving overall mission responsiveness. This observation is not meant to disparage the various well-intentioned reform efforts and the dedicated professionals that create and implement them; the point is, rather, that the desired improvements are seldom, if ever, realized [2,3,4].

One possible explanation for the lack of effectiveness of these acquisition policies is that they are aimed at the cause rather than the symptoms. For most engineering problems, this would be exactly the right approach. One time it is not is when the root cause is ineluctable. When this is the case, resources may actually be squandered by focusing on the cause, and instead should be aimed at how best to mitigate the effects. As an analogy, it is more sensible to construct a waterproof shelter than to try to develop a technique for stopping rainfall entirely.

With respect to acquisition programs, the metaphorical role of the inevitable raincloud is filled by *uncertainty*. Every major program must contend with myriad sources of uncertainty, to include the emergence of new threats, technological setbacks/breakthroughs, requirement creep, test failures, budget fluctuations, market volatility, workforce turnover, and, of course, new acquisition policies. Regarding the last item, the steady barrage of acquisition reform efforts that attempt to overcome uncertainty are arguably futile since *uncertainty cannot be overcome*. Worse, it may be that some of these strategies (e.g., requirement-driven acquisition) contribute to the development of point-solution designs that are ironically less capable of responding to these various sources of uncertainty when they do arise, thereby inevitably wreaking havoc with program budgets and schedules. So instead of tilting (or, at least, instead of *only* tilting) at the windmill of uncertainty, a better approach may be to accept uncertainty as a fact of life, and explore how we can design systems to better respond to it.

While the definitional landscape related to a system's ability to respond to uncertainty is large and ambiguous, the term most often associated with this concept is *flexibility* [5]. If systems can be designed in such a way that they are able to more readily respond to various sources of change, then it stands to reason that when uncertainties become realities, the impact to the program will be lessened. Designing flexibility into a system, which paradoxically focuses on the predictable effect, rather than the unavoidable cause, may be vital to achieving the persistently elusive goal of improved cost and schedule performance.

Background

The belief that a system can be designed such that it is inherently capable of being modified more quickly or more inexpensively when confronted with change is of growing interest to the DOD. The Office of the Secretary of Defense is currently sponsoring a number of research topics related to this notion of flexibility, aimed at answering questions such as—

- What is the relationship between flexibility and cost, schedule, and performance?
- How much does flexibility cost, both in terms of money and other system tradeoffs?
- How do we measure the value of flexibility?
- When, and to what extent, should we invest in flexibility?

Defining Flexibility. Even a cursory examination of the literary landscape makes it clear that the terminology related to flexibility is largely a quagmire. Despite its wide usage and high regard, flexibility remains a distinctly ambiguous concept. In many cases, the problem extends to—and is exacerbated by—the casual usage of many of the other non-traditional system design parameters (so-called “-ilities”). For instance, the terms “flexibility” and “adaptability” are often used interchangeably, or conflated with descriptors like *agility* and *versatility*.

This ontological problem is tackled by the authors in a precursor paper [5], where an extensive literature review is used to construct a novel analytical framework for clarifying salient aspects of flexibility-related terminology. While it was not possible to descry consensus definitions from the literature, we did identify certain dominant characteristics that enabled us to formulate a clearer and more consistent view of certain flexibility-related terms. A summary of these definitions is provided below to orient the reader to the terminology used in this paper (the detailed rationale behind these definitions may be found in the referenced paper). The reader is cautioned not to place too much stock in these definitions, or become too agitated should they be out of accord with the reader's own views. Not only are these definitions not intended to be authoritative, but we believe the methodology proposed in this paper for valuing flexibility serves to effectively obviate much of the distinctions in terminology.

- **Versatility:** Denotes *a broad range of capabilities*; a versatile system has many capabilities, but may or may not be flexible. In other words, versatility ensures the system possesses many capabilities, but does not guarantee that existing capabilities may be easily enhanced or new capabilities may be easily added. Logically, a versatile system is likely to be better poised to accommodate unforeseeable sources of change.
- **Overcapacity:** Refers to *designing more capability into a system than what is required*. Sometimes pejoratively referred to as “gold-plating,” overcapacitization includes adding entirely new non-validated capabilities (e.g., ability to release munitions from a reconnaissance vehicle) or exceeding required performance attributes (e.g., designing for Mach 2.5 when the requirement is Mach 2.0). It only pertains to foreseeable sources of change.
- **Process Flexibility:** This is *the collection of structured management techniques for ensuring a program is best poised to respond to change*. It includes parallel development, management reserve, incremental deliveries, and delayed differentiation. Fundamentally, process flexibility is about employing sound systems engineering principles that allow management to delay decisions and keep options open in order to remain more responsive to the various uncertainties that arise in a program.
- **Design Flexibility:** Defined as *the degree to which a system can be modified to increase its capability in response to foreseeable external change*. Fundamentally, design flexibility is about designing a system in such a way that its capabilities can be more easily modified, essentially creating *capability options*. Design flexibility can be achieved through pre-provisioning (i.e., scarring) the system with nascent capabilities that can be matured to full implementation at a fraction of the cost than they otherwise would have required. Design flexibility can also be achieved by broadening the design-space to allow for competing design implementations both now and in the future. Note that unlike overcapacity, design flexibility does not commit one to a capability; instead, it poises the system to implement the capability more easily at a later time.

While this paper seeks to develop a methodology capable of incorporating all these elements, the proximate discussion is largely focused on *design* flexibility. This is both for simplicity of argument, and because it best captures the most commonly perceived notion of flexibility.

Measuring Flexibility. With respect to the effort to quantify the value of flexibility, there is extensive confusion in the literature regarding the difference between *measuring a parameter* and *measuring the value of a parameter* [6]. In fact, there have been a surprising number of attempts to quantify flexibility, per se, but very few attempts to quantify the *value* of flexibility. Unfortunately, even if we were to assume that measuring flexibility is a necessary stepping stone to measuring the value of flexibility, the extant methodologies appear to be of questionable theoretical validity, and/or are too restricted in their applicability [6,7]. For instance, some approaches to measuring flexibility only provide an arbitrary flexibility “score” [8,9,10], which is generally not possible to translate into a value framework. Similarly, there appears not to exist a methodology for measuring *process* flexibility, and those methods that might otherwise be useful for measuring *design* flexibility are simply not applicable to the defense sector. Military acquisition is fundamentally different from private sector acquisition in that system value is not ascertained based on profit forecasts; rather, value is determined by a system’s capabilities, and how well they meet warfighter needs. Further, value is also driven by the urgency of the need, which is not always predictable a priori. These types of factors are not considered in any of the extant proposed methods to measure flexibility.

Value Driven Design. If we are to assign value to flexibility, we must be able to assign value to different system design options. Therefore, of key interest to us is the principle of value-driven design (VDD), which attempts to incorporate value metrics into engineering designs.

VDD is a movement to refocus systems engineering processes on the optimization of the overall system design, vice the optimization of specific system performance parameters [11].

To implement VDD, we must first develop a value model, which is the objective function for comparing the worth of one design to another [12]. Value models are suitable for capturing the upside of uncertainty, departing from the standard reckoning of only the downside uncertainty via traditional risk management techniques. Thus, a VDD model could feasibly reflect the value added by investing in a more flexible design because of the potential payoff later. Furthermore, if the value of flexibility can be quantified in units that are commensurable with cost, then meaningful cost-value tradeoffs can be made. In other words, VDD may help us with the critical task of directly comparing costs and benefits by assigning values to each parameter that have the same units of measurement (presumably dollars). Then the best (i.e., most cost-effective) design is simply the one with the highest expected utility [13].

Value-driven design is not yet used routinely in the defense industry, but the underlying principle represents a potential sea-state change in how the DOD would manage its programs. To date, the DOD has been unable to accurately and reliably predict the costs of weapon systems that meet certain requirements over a given period of time. By valuing non-traditional system characteristics like flexibility, VDD essentially provides a more efficient and strategic approach to systems acquisition, potentially providing a more accurate and integrated assessment of true system cost than other extant putative measures of life-cycle cost. Brown outlines a method to leverage VDD in this manner, which he refers to as “lifecycle cost under uncertainty” [14]. The notion of a life cycle cost metric that attempts to account for non-deterministic events in a program’s life cycle is at the heart of the “more flexible approach to valuing flexibility,” which will be outlined in the final section of this paper.

Decision Making Under Uncertainty. Based on the literature, we know that the value of flexibility is positively correlated to uncertainty, such that the greater the uncertainty in the system, the greater the value a flexible design option is likely to have [7,15,16,17]. But if we are to make any headway on quantifying the value of flexibility, we need the ability to make the best decision under conditions of uncertainty. This type of problem has been studied extensively in economics. The economic approach to this problem is net present value (NPV) analysis. NPV is a standard method for determining the time value of money. It takes into account the net cash flow at a particular time t , as well as the *required rate of return* (i.e., *discount rate*). Thus, the expected cash flows are discounted at an interest rate that accounts for the time value of money as well as the project risk. Several studies use NPV as part of their effort to quantify flexibility, including [13,16,18,19,20].

Another approach is *real options analysis*, which exists at the intersection of value and uncertainty. Economic theory defines a real option as the “right, but not the obligation to take an action at a predetermined cost and at a predetermined time” [17]. Copeland [21] claims that only real options can “provide a theoretically sound tool for valuing” decision flexibility. In a manufacturing application, Ajah [22] touts real options, stating “that the adoption of the real options approach early in the conceptual design process can offer to the designer, extra degrees of freedom of systematically considering and designing system elements.”

Research Gaps

NPV. In general, researchers tend not to be in favor of using NPV for decisions involving flexibility [6]. While NPV is sufficient in cases of “low uncertainty, or [when] you have no scope to change course” [21,23], it is not appropriate for situations involving great uncertainty, as it assumes a predetermined path through an established set of alternatives. This is antithetical to the core aim of flexible processes and designs, so a different method is needed—

one that can take more (and fewer predetermined) decision options into account [12,24,25].

Real Options. While real options analysis is widely seen as preferable to NPV, it comes with its own set of criticisms. For example, the underlying financial model (known as Black-Scholes) is generally only valid under certain constraining assumptions, many of which are not likely to be applicable to defense acquisition. One of the model assumptions is that the valued asset must be traded on an “efficient” market, where there is no possibility of arbitrage. While arguably true in the broader capital market, this assumption is not warranted within the DOD, where markets are often artificial, and far from efficient. Another stipulation of the Black-Scholes model is that the asset must have a price that follows geometric Brownian motion, thus creating a return on the asset that is consistent with a random lognormal distribution. However, random fluctuation of price is a debatable premise in a standard open market, let alone in the cloistered defense industry. Finally, real options in military acquisition programs are likely to be path-dependent and highly interdependent. In both cases, traditional financial options methodologies tend to fail because the underlying stochastic differential equations are not available or simply do not apply [12,26]

A Basic Formulation. Regardless of the specific framework employed for decision-making, there are a couple of mandatory elements as part of any conceivable valuation approach. One is the cost of the investment, and the other is the return on that investment. With respect to decisions related to design flexibility, this same basic approach applies, but must be tweaked somewhat. To begin with, an initial investment is required to implement a more flexible design, which we refer to as the investment cost. Complicating our formulation, however, is the fact that the return on that investment is not directly linked to the value of the flexibility. *A flexible design does not have intrinsic value*; instead, it is the concomitant capability associated with that flexibility that has value (to digress the argument further, it is really the military outcome that can be achieved via a given capability that has value). Therefore, the value component of the decision formula is the probabilistic benefit that a particular capability may be realized with fewer resources (e.g., time or money) than had we not chosen to make that initial flexibility investment. In addition, though, our notion of flexibility may require some additional cost later to actually implement the capability, which is also dependent on the probability that the capability will be effected. Finally, the very act of investing in flexibility (e.g., adding brackets to a tank chassis) may adversely affect other performance attributes (e.g., speed, maintainability, etc) such that we need to include another value term to potentially decrement lost value associated with the flexibility investment.

Momentarily setting aside the aforementioned concerns related to NPV, we could conceptually (and neglecting time-value of money considerations) formulate the decision to invest in flexibility as follows—

(1)

Note that the _____ term—much like the _____ term—needs to include any value decrements associated with adverse consequences to other performance attributes that are incurred by implementing the capability (e.g., adding armor may make a combat vehicle more survivable, but will likely reduce its speed and maintainability).

Assuming that all terms are commensurable measures (i.e., monetized), this formulation indi-

cates that if the NPV is greater than zero, the investment in the flexible design option is worth pursuing. While constructing equation (1) is relatively straightforward, assigning values to each of these terms is where the challenge arises. The two cost terms, while seldom trivial, are likely the most easily obtained, as we have ample experience in estimating the cost of engineering solutions. Establishing a valid probability term is more difficult as it is inherently linked to uncertainty; however, it at least can be rationally estimated. The real dilemma is associated with the value terms, which are extremely difficult—if not impossible—to meaningfully quantify in the context of defense acquisition. This is the crux of the problem.

Value of Capability. In order to make meaningful value judgments, we must establish a utility function that will quantify capability in some ratio-level units of comparable value. While this is relatively routine for profit-driven commercial systems, it will necessarily be more challenging for military systems, as the utility function will almost certainly not involve a monetizable metric like earnings. Instead, for example, we would need to somehow devise a function (or more likely, a series of functions) for determining the utility of an extremely wide range of military capabilities, such as being able to resist jamming or increase an airplane's top speed from Mach 2.0 to Mach 2.5.

In principle, there is a solution. Under the neoclassic economic definition of value, an item's value can be established by determining a customer's *willingness to pay*. Thus, we can surmise that the value of a particular military capability can be determined by ascertaining the maximum amount the government is willing to give up (of some measureable resource) to obtain the capability (i.e., *the value of a given capability to the government = the maximum cost the government is willing to pay for the capability*). The devil is in the details, however. Assigning a numerical value to the right side of this equation is a daunting endeavor. The most obvious approach would be to use the dollar amount budgeted by the government. But this is problematic for a multitude of reasons. Consider that the actual system cost may include a number of other scarce resources (e.g., time, critical skills, facilities) that are not captured in the government budget. Technically, economic cost includes the loss of opportunities as well, so we would also need to account for the cost of losing or vitiating other capabilities by virtue of the fact that we are committing resources to this capability. Once again, though, we would face the dilemma of assigning a value to a capability, with only budgets to guide us, so our original problem is further complicated because it is now recursive.

In addition, even if we were to accept that budgeted costs will be adequate, there is no reason to believe this represents the *maximum* cost the government is willing to pay. Firstly, the government may, in principle, be willing to budget more for a particular capability, but has reason to believe that a lower amount will suffice. The problem is that the government generally establishes its program budgets based on expected actual costs, not the perceived value of the program or resulting capability set. Secondly, budget allocation processes are notoriously volatile, subject to any number of political and bureaucratic vagaries that have nothing to do with the merits of a particular program or capability. Thus, one year's total budget allocation for a given program may be substantially different from the next year's allocation for the same program, though there was no change in the perception of its value.

For the sake of argument, let's assume that we can tolerate a lower fidelity estimate, and we can convince ourselves that the budgeted costs represent all costs with sufficient accuracy, and that these costs also represent the maximum cost the government is willing to pay. Unfortunately, there is still another practical obstacle to establishing a specific dollar amount corresponding to the value of a capability. The fact is that defense budgets can rarely be traced so cleanly to desired system capabilities, and certainly not at the levels of precision that would be required to make this a viable approach for detailed design decisions. Imagine a

\$10.0 billion program to develop an aircraft with various capabilities related to range, reliability, speed, maneuverability, lethality, etc. The notion that we could indicate exactly how much of that \$10.0 billion investment the government is willing to spend to achieve a speed of Mach 2.5 may not be feasible, and is certainly not the basis on which government program budgets are allocated or managed today.

Clearly, using budgeting information to infer the value of capabilities is full of pitfalls. An alternative approach would be to query system end users directly. The most obvious drawback to this approach is the inherent subjectivity; even within a single user community, different users will perceive the value of a given capability differently. This would drive a comprehensive solicitation of all potential users, in combination with some (to be specified) means of aggregating and reconciling those inputs. In addition, each user's value input would need to be provided within the context of a resource-constrained environment; else value assignments would lose relative meaning. Another potential problem stems from the fact that the end-user of the capability who is most able to appreciate its value is, ironically, the least likely to have any experience with budgeting and finance, and thus may not even be able to translate the mission value into monetary terms. Similarly, the user group may simply have no direct insight into the costs associated with the capabilities it has access to due to the nature of service/capability relationships among defense organizations.

Even more fundamentally, flexible design options may have no practical meaning to the user. Since we are inherently interested in the value of *potential* capabilities—*vice validated* capabilities—the user may be unwilling and/or unable to assign any value to the capability at all. For if the potential capability were valued to any level of significance, then it likely would have already been translated into a valid system requirement! Finally, many potentially flexible design decisions over the course of a program's life (particularly those that pertain to *process* flexibility) have little to no impact on end capabilities.

It can be argued that by attempting to employ both the willingness to pay and the user query methods, it may be possible to obtain a dollar range that could serve to at least bracket the value terms. However, it's not clear we could assign valid confidence values to this range or that the calculated range would be narrow enough to have practical utility. Therefore, given the difficulty of establishing the value of military capabilities, we need a more *flexible* approach to determine the value of flexibility.

A More Flexible Approach

What if we could establish the merits of a capability without having to explicitly determine its value? This may be feasible through a modification to the familiar life cycle cost (LCC) model. The idea is to refine current life cycle cost calculations to better account for the value of capability opportunities that are likely to arise throughout the life of a program. Furthermore, the methodology we propose would be capable of inherently evaluating design options in aggregate, thereby rendering distinctions in capability like *flexibility*, *versatility*, and *over-capacity* as entirely arbitrary. Before proceeding to a more comprehensive explanation, however, it may be beneficial to review the salient aspects of DOD's current LCC methodology.

Life Cycle Cost. LCC is a systematic accounting approach for aggregating all direct and many indirect costs for a given system. It includes not just total acquisition costs, but also costs related to operations, maintenance, and disposal. Importantly, LCC also accounts for risks, generally either through sensitivity analyses or through formal quantitative risk analysis [27]. For large programs, calculating the LCC is generally a tedious undertaking involving substantial time and effort. But the outcome is nevertheless generally deemed to be worthwhile. As a formal measure, life cycle cost is entirely straightforward, and easily understood

by the typical spate of stakeholders, to include systems engineers, users, and contractor and government managers. Moreover, by providing senior decision-makers with their single best source of estimated cost to achieve a given capability, the LCC is often instrumental in determining the ultimate fate of a program.

Formal DOD guidance calls for the LCC to be first accomplished as part of the initial Analysis of Alternatives (AoA) and is only updated as part of major milestone decision reviews. Aside from these updates, however, the system LCC is generally a static measurement. When calculated, it provides a “snapshot” estimate of total life cycle cost on the assumption that there will be no deviations from key cost, schedule, and performance parameters, which are collectively referred to as the acquisition program baseline (APB) [27]. Of course, one thing we know with near certainty is that there will almost always be deviations from the APB.

While the assumption of a static APB may be unwarranted, programs proceed with it anyway, presumably because the alternative of trying to account for the non-deterministic uncertainty in precisely *how* the program will deviate from the APB is simply not possible, or at least just too daunting. It can be argued, however, that even though uncertainty is—by definition—not deterministic, it may be possible to employ stochastic probability methods that can yield cost estimates that are likely to be more accurate in the long run. Although establishing the initial models to accomplish this would require significant resource investment, the possibility of more accurate LCC estimates—and the improvement in decision-making that would accompany that—promises an enormous return on such an investment.

Life Cycle Cost Under Uncertainty. Thus, there is substantial motivation to provide improved LCC estimates, at least to the level required to support decisions considering alternative flexible design options. The notion that this can be done by accounting for random events that affect the system forms the basis of life cycle cost under uncertainty (also referred to as *stochastic life cycle cost*), which was mentioned earlier as part of the discussion on value-driven design. The idea of applying this strategy to acquiring military systems appears to have been first introduced by Brown in two papers related to the F6 satellite program [14,28]. As described by Brown, stochastic life cycle cost is premised on three assertions.

- *The cost to develop, procure, and operate a system with some assured minimum capability over its lifecycle is not a deterministic value.*
- *Instead, this cost can be modeled as a random variable with a probability distribution resulting from a set of uncertainties introduced throughout the system's life.*
- *This random variable metric is a relevant basis for comparison between alternative system architectures and design choices.*

Brown is to be commended for introducing this simple but deceptively powerful notion of stochastic life cycle cost. However, the initial treatment does not develop the principle fully, nor explore its broader applicability. The type of stochastic events he considers are only those specific events that critically influence the success of a satellite system, i.e., launch failure and on-orbit component failure. Brown explicitly does not consider other aspects of life cycle uncertainty that affect virtually all programs, such as “requirements creep, funding stream volatility, technology development risk, and volatility of demand” [14]. Yet he clearly does recognize that the model could be applied to these other sources of uncertainty, noting that these variables are “left for future analysis.” To date, it does not appear that such an analysis has been accomplished by him or others.

Consequently, we propose a research strategy to logically extend this promising technique in a manner that may provide a number of potential benefits over current practices. Specifically, we intend to expand the life cycle cost under uncertainty idea to a robust and comprehensive

methodology for effectively valuing system design alternatives. For the remainder of this paper, we explore how such an approach could be applied to uncertainty as related to *system performance*. We expect to address its applicability to other sources of uncertainty (i.e., cost and schedule) in subsequent efforts.

Another modification to enhance the utility of the LCC concept is that it should *not* be viewed as simply a static measure only to be crafted in support of key milestones. Just as LCC is an essential decision tool for those in the role of Milestone Decision Authority (MDA) and above to gauge the value of a program, it can fulfill the same principal function to those who serve at the program manager level and below. Moreover, estimates of life cycle cost are not useful just periodically, but have ongoing utility at all stages of the program, as design decisions are continually required at various levels of the program which (to varying degrees) are likely to impact the overall system cost. And whereas early LCC values would naturally be focused on high-level architectural decisions, as the program matures, and the requirements baseline migrates from *functional* to *allocated* to *product*, the decision trade space will concomitantly shift to the more detailed design implementations. Thus, this dynamic and (probabilistically) more accurate LCC should arguably be managed, updated, and referenced as often as the program schedule.

Current Expected Value Life Cycle Cost. To capture the utility of this improved LCC concept, we offer the appellation, CEVLCC, which stands for *Current Expected Value Life Cycle Cost*. The name is intended to convey a couple of key distinctions from the standard LCC and Brown's stochastic LCC. The "Expected Value" phrase discriminates CEVLCC from the standard LCC as a more probabilistically accurate measurement of system cost; whereas the word "current" is intended to connote the fact that the CEVLCC would be employed as a living, continually updated decision analysis tool. The notion that an LCC estimate might be applied dynamically, and at lower levels of system design, is distinct from Brown's view that the stochastic LCC could only be useful for "preliminary trade space exploration" and not for value determinations "below the architectural level" [28].

For clarity, here are the specific assumptions that must hold for this approach to be valid—

1. As programs mature, there will be unpredictable deviations from the APB that affect the system's LCC
2. It is possible, on average, to provide a more accurate LCC estimate through probabilistic modeling of the stochastic processes that cause deviations in the APB
3. The cost of the effort required to calculate a more accurate LCC is more than offset by the value obtained by the more accurate LCC
4. Given the CEVLCC cost accounting methodology, as long as each design meets all of its threshold requirements, then its relative value can be inferred from its cost

In addition, the proposed methodology is straightforward, consisting of the following steps:

- Establish system design options
- Construct time-phased probability distribution functions (PDFs) associated with all existing key cost, schedule, and technical performance parameters of the program
- Assign time-phased probabilities for potential new capabilities of the system
- Estimate standard (i.e., traditional) LCC cost
- Estimate costs associated with modifications (consistent with PDFs) to baseline cost, schedule, and technical performance parameters
- Estimate costs associated with the addition of new capabilities
- Calculate CEVLCC for each system design option and select alternative associated with the lowest CEVLCC

Hypothetical Use Case. To appreciate the process and potential utility of CEVLCC, we illustrate its application using a hypothetical missile defense scenario. For simplicity, we will only consider technical performance as part of this analysis.

Assume we have a requirement to protect a high-value facility in a sensitive overseas location, which must conform to the following four Key Performance Parameters (KPPs).

Table 1: KPPs for Missile Defense Scenario

#	Key Performance Parameter	Threshold	Objective
1	Protect facility from ballistic missile attack with X% assurance	X=95	X=99
2	Engage only missiles if > X% confident they represent an imminent threat to the facility	X=90	X=95
3	No evidence of military presence w/in X mi. of facility	X=25	X=40
4	Be able to engage X missile(s) simultaneously	X=1	X=5

The second CEVLCC assumption states that it is possible to formulate probabilistic modeling of the stochastic processes that cause deviations in the APB. One way to accomplish this is to treat the value for each performance parameter—in this case, each threshold KPP value—as a random variable, and construct the probability function. To do this with any semblance of confidence would likely require extensive empirical data from a variety of different requirement categories, program types, program levels, acquisition strategies, etc. Furthermore, the PDFs would be valid only at a single point in time, so they would need to be revised as the program matures and new information becomes available. Clearly, construction and maintenance of the CEVLCC would require significant effort; nevertheless, it could be done, and would be worth doing if the third assumption above holds. Figure 1 below provides examples of what those PDFs might look like in the case of KPPs #1 and #3:

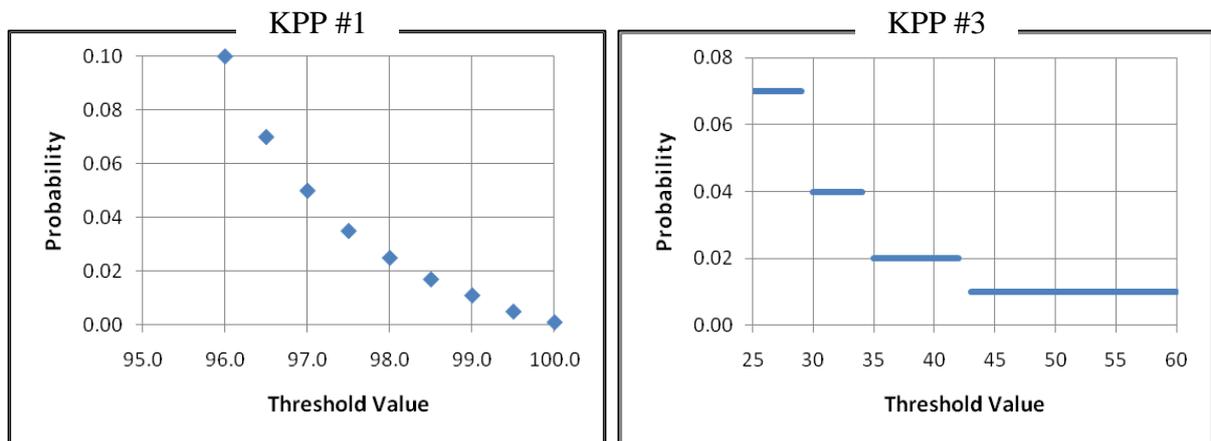


Figure 1: Notional PDFs of Missile Defense Scenario KPPs

In both cases, the x-axis is the random variable (i.e., the KPP threshold value), and the y-axis is the probability associated with a particular value of the random variable. For simplicity, we have chosen not to depict the probability that the variable will remain the same or decrease, but in a comprehensive model, these probabilities would likely need to be determined as well.

After establishing the PDFs for the parameter values of existing capabilities, we next need to account for the probability that the system will be required to support new (and obviously foreseeable) capabilities. For instance, we might conceive of the following two potential new

capabilities, along with their estimated likelihoods:

- Protect against cruise missile threats (15%)
- Protect against unconventional ordnance attacks (e.g., suicide bomber) (2%)

Each of these probability functions will require a temporal dimension as well. In other words, these estimations of probability are associated with a given time horizon, and will necessarily vary depending on that horizon. For this scenario, we might estimate that if a requirement related to the first new capability (i.e., protect against cruise missiles) has not been introduced by the Preliminary Design Review (PDR), then its likelihood of being imposed between the PDR and the Critical Design Review (CDR) is three percent, and its likelihood of being imposed between the CDR and the Test Readiness Review (TRR) is one percent, and so on. Viewed in this way, we recognize a certain similarity between these various PDFs and traditional risk burn-down plans. This is an important point, as the PDFs would need to be managed in a similar manner, and could reasonably be integrated with traditional risk analyses.

In both cases (i.e., the modification of existing capabilities and the addition of new capabilities), the assigned probabilities will admittedly be estimates, perhaps quite rough ones. Will they be exactly right? Absolutely not. If our stochastic models are at all valid, are they likely to be closer to reality than the assumption that nothing will change over the remaining life of the program? Almost certainly.

Next is the cost assessment step. This is executed in the context of whatever design options we have available to us at any given time. Let's assume, based on earlier assessments, that the program has chosen a defensively-oriented architecture that engages ballistic missile threats during the terminal phase. Like all true decisions, the program has made an irrevocable allocation of resources as a result, and has, to some extent, necessarily constrained their design space going forward. Nevertheless, the commitment to the terminal phase option still leaves a number of fundamental design decisions open to them. We then postulate the following list of architectural possibilities being considered by the program:

1. Terrestrial interceptor system stationed at least 25 miles from facility
2. Concealed (e.g., underground) terrestrial interceptor system
3. Airborne interceptor system
4. Terrestrial directed-energy system stationed at least 25 miles from facility
5. Concealed (e.g., underground) terrestrial directed-energy system
6. Airborne directed-energy system
7. Hardened structure that ensures survivability of facility
8. Force field

Each of these architectures has relative strengths and weaknesses based on the KPPs as written. And each of these designs has its own inherent costs to implement. All else being equal, under the traditional conception of LCC, the option above with the lowest LCC that also meets all threshold requirements is typically the one that will be selected. This is the crux of the problem, as this traditional approach does not account for the value of the flexibility embedded within certain architectural options.

The CEVLCC, however, requires that additional cost estimating be performed against the range of potential new KPP threshold values as well as the potential new capabilities. Clearly, some of these options are better poised to accommodate changes in the KPP thresholds. For instance, the concealed terrestrial architectures (i.e., options #2 and #5) will have no additional cost should there be an increase associated with the threshold value of KPP #3, whereas the non-concealed versions (i.e., options #1 and #4) would likely have an enormous cost impact. Similarly, some architectures can more easily accommodate new capabilities. If the

program is directed to incorporate the capability to protect the facility against cruise missiles, then the airborne interceptor system can be modified much more easily than the underground interceptor system (i.e., the airborne system is more *flexible*). And the hardened structure option will not have to be modified at all, as the capability to withstand the cruise missile strike was already embedded in its design (i.e., it's *overcapacitized*).

Once we've determined the estimated costs for the potential changes to the system, we calculate all of the expected values for each design with respect to each change. So suppose that for all three directed energy architectures, we estimated the following additional costs for the potential range of changes in the value of the KPP#1 threshold.

Table 2: Marginal Probability Costs for Directed Energy Architectures

Index ()	KPP#1 Threshold (X)	Additional Cost to Implement ()	Probability (from Figure 1)
1	96.0%	\$0.0M	10.0%
2	96.5%	\$0.0M	7.0%
3	97.0%	\$0.0M	5.0%
4	97.5%	\$1.0M	3.5%
5	98.0%	\$1.0M	2.5%
6	98.5%	\$1.0M	1.7%
7	99.0%	\$3.0M	1.1%
8	99.6%	\$6.0M	0.5%
9	99.9%	\$20.0M	0.1%

Using the standard formula for expected value, then architectures #4, #5, and #6 (i.e., the directed energy architectures) have an expected value of \$160k with respect to KPP #1. We repeat this process for each architecture for the remaining KPPs. We then account for the potentially new capabilities in the same manner, although the expected value calculation is a trivial weighted probability with a single term. So for each architecture, a separate CEVLCC is calculated by summing its baseline LCC, its summation of expected values for the modifiable capabilities, and its summation of expected values for the new capabilities, i.e.—

(2)

This leads to the fourth assumption. As long as each design meets all threshold requirements, then its relative value can be inferred from its cost. Ordinarily, this would not be valid, but given our cost formulation methodology, we implicitly accounted for those discriminators that would otherwise have contributed to the value side of the equation. Specifically, there is no need to assess how much to “credit” a particular design for its ability to exceed a KPP threshold or its capacity to accommodate future changes. Both of these inherent design values are captured (albeit in complementary fashion) in our marginal probability cost estimates within CEVLCC. In other words, if a particular design option were able to more inexpensively accommodate a capability change—whether via flexibility or via overcapacitization—its weighted cost would be less than the competing designs, and its *value* would be greater. Note that this is why it is only valid to compare systems that meet all threshold requirements, as there needs to be a value baseline to reference. Based on these assumptions, then, we can now

assert that *the system that is the best value is simply the one with the lowest CEVLCC*.

As promising as CEVLCC might be, we recognize there are also a number of potential drawbacks to this technique, most of which are tied to the model assumptions. For instance, the fundamental nature of defense acquisition may be more *chaotic* than *stochastic*, thus preventing accurate predictive modeling over a reasonable time horizon, and fully precluding analysis of unforeseeable changes (violation of assumption 2). Also, to be most effective, CEVLCC would need to be comprehensive and current, which results in a large number of permutations to account for, thus potentially making its implementation cumbersome. Even if the resource investment is deemed to be worthwhile very early in the program (i.e., when design decisions are most impacting), it is possible that the return on investment will not be sufficient to justify its use much further into the program (violation of assumption 3). Importantly, CEVLCC, as currently conceived, also cannot effectively provide a relative evaluation of design options that do not meet threshold requirement levels (violates assumption 4). Finally, the CEVLCC does not entirely sidestep the problem of valuing capability, as excess capability above the threshold often does have value that must be accounted for. This technique does not properly account for the temporal benefit that an overcapacitized solution provides, i.e., having a newly desired capability immediately (or more quickly) available vice waiting for development and implementation.

Conclusion

There is consensus that uncertainty is a principal reason that DOD programs continue to struggle mightily with respect to their ability to adhere to cost and schedule projections. While acquisition policies and strategies that aim to abate uncertainty are admirable and often useful, ultimately they can only help so much. Since uncertainty is a certainty, programs may be better served by infusing their systems with an inherent ability to effectively respond to uncertainty. The singular term most commonly associated with such an ability is *flexibility*. While flexibility is arguably the single best term for this concept, even it is insufficient to capture the full range of capability responsiveness we would like our systems to have. We may also need them to be *versatile* and/or *overcapacitized*. However, making a system flexible, versatile, or overcapacitized inevitably requires additional investment that must be justified. The only viable way to provide that justification is to quantify the value of the capabilities that can be more easily achieved because of the investment. For military weapons systems, this task is, at best, extremely challenging, and, at worst, simply not feasible.

Consequently, a fundamentally different approach is needed—one that does not rely on an explicit valuation of potential capabilities, and is capable of evaluating design options more strategically, thus shifting the focus from the somewhat narrow view of just flexibility, per se, to the broader view of capabilities, regardless of how they are achieved. Thus, we propose the CEVLCC, a top-down, *intrinsic* value model based on the familiar notion of life cycle cost. The idea is premised on the notion that the need for capability changes in a program arises in a stochastic manner that can be modeled and incorporated into a continually updated, expected value model of total program cost. We believe CEVLCC potentially offers a number of advantages over current practices—

- An inherent focus on capability *in toto* that serves to automatically assimilate relevant capability concepts, such that discriminatory design considerations like *overcapacity* and *versatility* become irrelevant
- An inherent ability to incorporate cost and schedule components of a program, thereby obviating the distinction between *design* flexibility and *process* flexibility
- Being comprised of concepts already familiar to the acquisition community (i.e., life

- cycle cost and risk analysis), thereby greatly reducing cultural entry barriers
- Having a simple premise and an intuitive output (i.e., cost), both of which encourage adoption among stakeholders across the acquisition community
- Not being subject to criticisms specific to real options analysis
- Being able to mostly sidestep theoretical and practical challenges associated with valuing military capabilities

Currently, the CEVLCC concept is largely notional, and significant research effort remains to determine its validity and/or utility. Most of the near-term work is intended to validate, or at least characterize the limitations of, the CEVLCC assumptions. Specifically, we plan to—

- Analyze/characterize APB behavior for historical programs and examine concomitant LCC behavior with the intent of identifying salient factors that drive perturbations.
- Construct a basic CEVLCC model based on these salient factors.
- Compare the LCC accuracy for historical programs over time to the corresponding CEVLCC, and conduct tradeoff analyses to determine when the return on investment in the CEVLCC model is no longer worthwhile.
- Identify/develop alternate methodology to address CEVLCC weakness with respect valuing existing excess capability.
- Refine CEVLCC model and validate via historical case-studies

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