Test Taking Strategies in Computer Adaptive Testing that will Improve Your Score: Fact or Fiction?

Jennifer L. Ivie

Submitted to the Department of Psychology
and the faculty of the Graduate School of the University of Kansas
in partial fulfillment of the requirements for the degree of Doctor of Philosophy

_____
Co-Chair

_____
Co-Chair

_____

_____

_____

Date defended: _____

The Dissertation Committee for Jennifer L. Ivie certifies
that this is the approved version of the following dissertation:

Test Taking Strategies in Computer Adaptive Testing that will Improve Your Score:
Fact or Fiction?

Committee:

_____
Co-Chair

_____
Co-Chair

Date approved _____

ABSTRACT

Jennifer L. Ivie, Ph.D.
Department of Psychology
University of Kansas

The purpose of this study was to examine the validity of the claim made by test review companies that spending more time and attention on the first five or ten items on a computer adaptive test will improve an examinee's final ability estimate. Study 1 examined the effects of different amounts of information about how the test works and/or how to improve your score. In this study, it was found that having information on how to perform better on an exam does result in higher scores. Study 2 was a series of simulation studies that examined the stability of a computer adaptive test and the actual theta estimate when certain test parameters were varied: item bank parameters (item pool size, discrimination parameters, and guessing parameters); examinee parameters (whether or not the examinee has an artificially boosted ability level); and testing algorithm parameters, in particular how the first items are selected. Overall, evidence was found to support this test taking strategy taught to improve test scores. Finally, these results were compared to current average GRE scores for graduate schools across the United States. It was found that this artificial boost can result in admittance when the true theta might have resulted in non-admittance.

Acknowledgments

Blah blah blah

Table of Contents

List of Tables

List of Figures

1. Introduction

High stakes testing is a large area of research and debate in today's society, especially in the United States. With most institutions of higher education requiring the reporting of test scores, such as the GRE, SAT, ACT, LSAT, MCAT, etc., students spend a lot of time worrying about and studying for these tests prior to application into institutions of higher education. Many of these students will do anything necessary or suggested to improve their scores on these exams, often paying hundreds of dollars to tutors and/or test review companies for classes or books that are supposed to help students prepare for these tests. Many of these tests have more recently become computer adaptive (CAT), a format that is administered on a computer and adapts itself to the apparent ability level of the examinee.

The purpose of this study is to examine a particular test taking strategy taught by such test review companies that claim the strategy will improve the overall score for the individual on such a computer adaptive test. This strategy involves paying more attention to the items at the beginning of the test. This includes spending more time on these items, sacrificing the items near the end of the test. The claim is made that the adaptive nature of the test is such that larger changes or adaptations are made earlier in the test as the program is attempting to narrow in on an examinee's ability level. Thus, it is easier for an examinee to improve his/her score at this point in the test. Later in the test, it is claimed that these jumps are smaller and thus will result in less change in the ability estimate. This claim was tested through two studies looking at the problem from two different perspectives.

The goal of the first study was to examine the possibility that examinees who have learned this test taking strategy will score higher on an adaptive test than their counterparts who have not been taught the strategy. To reach this goal, college-aged participants were administered a computer adaptive abstract reasoning test. Some of the participants were taught the strategy while others were not.

This first study aimed to find differences due to knowledge of a strategy to "beat" the test. It could be hypothesized that just knowledge on that a person can beat a test will improve his/her score because it will lower test anxiety and increase test motivation. Research has also shown that test anxiety and test performance have an indirect relationship (Shermis & Lombard, 1998). Research has shown a direct relationship between test motivation and test performance (Kim & McLean, 1995; Cohen, 1998). found that increased test motivation improved test scores. Finally, research has shown that strategy training has a direct relationship with test performance (Embretson, 1992).

The goal of the second study was to examine the stability of a computer adaptive test at different places throughout the test to examine the claim that larger jumps are made earlier in the test, with smaller jumps later in the test. Also, in the second study, differences in final ability estimates between subjects with some sort of artificial boost (in terms of this series of studies, the artificial boost refers to knowledge on how to beat the test) and those without this boost in ability level was examined under varying testing algorithm conditions. This goal was reached using a CAT simulator that simulated examinee responses to items that were selected and

"administered" through adaptive procedures. The adaptive algorithm was varied on starting rule (i.e., how the first item(s) was selected), item discrimination and guessing parameter levels, and item pool size. Simulated participants' true ability levels ranged from -3.25 to 3.25.

Research has been conducted on the role of the starting rule used in a CAT algorithm. Much of this research has focused on issues of test security rather than the affect on test stability. Arguments have been made for randomly selecting the first item from a particular number of items or particular item difficulty range (Hulin, Drasgow, & Parsons, 1983; Embretson & Reise, 2000). Others have argued for an extension of the previous suggestion—that is, to randomly select the first 5 or 10 items from a given number of items (McBride, Wetzel & Hetter, 2001). Finally, others have argued for fixed testlets that could be used at the beginning of a CAT (Wainer & Kiely, 1987).

Many researchers have examined the affects of differing discrimination and guessing parameters on the accuracy of ability estimates in a computer adaptive framework (e.g., Vale & Weiss, 1975; Urry, 1974, 1975; Jensema, 1974; Chang, 1999; and Hau & Chang, 2001). Most research has shown that higher discrimination parameters and lower guessing parameters are ideal. Finally, research has shown that larger item pools are better in terms of ensuring more accurate ability estimates (Embretson & Reise, 2000).

As stated above, the purpose of this set of studies is to examine the efficacy of a commonly taught test taking strategy for high-stakes computer adaptive tests. In

Chapter 2 previous research on issues related to the purpose of this study are outlined including: the history of adaptive testing, previous research on the development and administration of computer adaptive tests, the item response theory concepts utilized by CAT algorithms, and information on the abstract reasoning test used in Study 1. In Chapter 3 and 4 the methods, results and discussion of Study 1 and Study 2 (respectively) are described. And, finally, the overall conclusions are discussed in Chapter 5.

2. Literature Review

Written proficiency testing became widespread throughout the United States and Western Europe in the mid-nineteenth century. In the early 1900s, testing began the shift from individualized testing to mass testing. This allowed for more efficient testing and more homogenous testing environments. In a paper-and-pencil group testing situation, the entire group receives the same items. As a result, enough items of all proficiency levels must be present on the exam to ensure close estimation of person parameters for all people taking the test. However, most paper-and-pencil (P&P) exams have a majority of items near average proficiency levels because most of the population falls within two standard deviations of the average proficiency level. In addition, ensuring valid and reliable P&P tests requires test developers to include a large number of items on the test (Wainer, 2000).

Creating more accurate and reliable P&P tests typically includes adding more items to the test at different ability levels. For this reason, as well as others which I will expand upon later, research on adaptive testing formats began as early as the 1950's. The first sign of adaptive testing was in the late 1950s, 1960s and early 1970s when several kinds of branching tests were designed. These were tests for which trees of items were developed that branched from one item to the next based on the examinee's answer to the previous item, that is whether or not the examinee answered the item correctly (Krathwol & Huyser, 1956; Hansen, 1969; and Hulin, Drasgow & Parsons, 1983). While many of today's adaptive tests are computer-based, many early

inventive formats were developed to implement adaptive procedures without the use of computers.

In the early 1970's researchers began developing and examining testing systems that allowed a examinee to take a shorter test with better measurement precision by giving examinees items based on their estimated proficiency level. Four examples of these sorts of tests include the flexilevel test (Lord, 1971a), the two-stage test (Lord, 1971b), the pyramidal test (Larkin & Weiss, 1974, as cited in McBride, 2001b), and a stratadaptive test (Weiss, 1974, as cited in McBride, 2001b). Each of these tests, as well as other predecessors of the computer adaptive test, will be discussed now.

Lord (1971a) designed a P&P test called the self-scoring flexilevel test which required the examinee to adjust their progress based on the accuracy of their answers. This test requires complex instructions because the adjusted scoring is the job of the examinee. On the other hand, this test allows for adaptive testing without the use of computationally complex scoring algorithms (Thissen & Mislevy, 2000). In the flexilevel test, each examinee responds to half of the items on the complete test. The selected half of the items depends on the examinee's proficiency level; difficult items are answered by more proficient examinees while easier items are answered by the less proficient.

An example of a flexilevel test is as follows. Items are ordered by difficulty level on a two-column sheet of paper with the item of middle difficulty centered at the top of the page, the more difficult items listed in the right-hand column increasing

in difficulty, and the easier items listed in the left-hand column decreasing in difficulty. The examinee begins with the middle difficulty question at the top of the page. The examinee marks the sheet, if they answer correctly, the answer turns a particular color signifying he/she to proceed to the first item in the more difficult column. If the examinee answers incorrectly, the color shown signifies to proceed to the first item in the easier column. The examinee then proceeds to the next available item in the column signified by the color shown with each answer. All examinees answer the same number of questions, and their final score is based on the difficulty of the last item answered (Lord, 1971a). Simply put, each time an examinee answers a question, he or she is routed to an easier or harder item based on the correctness of his/her previous answer (Schoonman, 1989).

Olivier (1974) conducted a study comparing the flexilevel test to conventional P&P tests. He found that the flexilevel test had lower reliability and validity than the conventional test. He also found about 15 percent of the examinees who took the flexilevel test had to be removed from the study due to errors made in the self-scoring mechanism (as cited in McBride, 2001b). Betz and Weiss (1975) also compared the flexilevel test to a conventional test. However, unlike the previous study, the tests were administered on the computer to remove self-scoring errors. In this study, they found that both tests demonstrated the same degree of test-retest reliability (as cited in McBride, 2001b).

The two-stage adaptive testing system, another predecessor proposed by Lord (1971b), involves giving examinees two sets of items. The examinee's performance

on the first set of items decides whether he/she will receive the harder or easier second set of items. Betz and Weiss (1973) compared a computer-based, conventional 40-item test, to a computer-based, two-stage test with two 20-item sets. They found similar test-retest reliability between these two types of tests. Possibly degrading these results, they also found that the first set of items in the two-stage test were too easy for the sample of examinees used in this study (as cited in McBride, 2001b).

Another testing system that utilizes this adaptive concept is that of a fixed branching system. This system is known as pyramidal or staircase adaptive testing (Larkin & Weiss, 1974). In a test of this type, one item is always the starting point. This item branches into two other items: one harder and one easier. These items branch into two more items each—one harder and one easier per item—continuing on to result in a lattice-like branching system. This continues on for as many levels as the number of items the test developer would like the examinee to answer. This differs from the systems of interest in this study because the branching order is pre-specified and everyone with the same response pattern receives the same items.

There are some important disadvantages to the pyramidal testing system. One major disadvantage is the enormous number of items required. If an administrator wants the examinees to answer n items, the number of items necessary for the branching system is equal to $(2^n - 1)$. Thus, an 8 item test would require 255 items. Another disadvantage is that the first few items in the test would have much higher exposure rates than the items at the end of the test, leading to decreased security on these items, which could also lead to inflated final scores. Also, if an examinee makes

simple errors on the first few items by answering the first couple of items incorrectly, he/she cannot increase his/her score beyond this lower set of items. This is a major drawback of any fixed-item selection adaptive testing system (Schoonman, 1989). Larkin and Weiss (1974) compared this test to the two-stage test discussed previously. They found both tests to have comparable test-retest reliability, but they found that the two-stage test resulted in higher proportion correct scores due to the tailoring properties of the test (as cited in McBride, 2001b).

Similar to the pyramidal test, with a prespecified branching system, the stratadaptive (or stratified adaptive) method branches from one set of items to another, rather than from one item to another. In this method, proposed by Weiss (1974), the item pool must first be sorted into strata, or mutually exclusive groups, based on item difficulty. Rather than branching from one item to the next depending on correctness of answer, the test branches (in the same manner as the pyramidal test) from one strata to the next. The examinee is given only one item from each strata. In the original design, Weiss (1974) proposed this test as a variable length, variable entry test. Waters (1974) conducted a study comparing three forms of the stratadaptive test to a conventional computer based test. He found the reliability and external validity to be higher for the stratadaptive tests than for the conventional test. He also found these tests to be 36 to 60 percent shorter on average than the 50-item conventional test.

Vale and Weiss (1975) also conducted an empirical study comparing the stratadaptive test to the conventional test. They found the stratadaptive test to be 34%

shorter than the conventional test. Also, they found the stratadaptive test to have higher internal consistency (.94 versus .91), and similar test-retest reliability. They followed this empirical study with a simulation study in which they systematically varied item discriminations, test length, and the availability and quality of prior examinee ability information. They conducted separate studies of fixed and variable length tests. In the fixed length test simulation study, they found the fidelity coefficient for the peaked conventional test to be superior to that of the stratadaptive test for items with low discriminating power (α = .50). However, at the higher levels (α = 1.0 and 2.0), the stratadaptive test had higher fidelity than the conventional test. In the variable length studies, they found similar results. At the lower discriminating items (α = .50 and 1.0), they found that the stratadaptive test had lower fidelity coefficients than the conventional, even with more than 40 items. Yet, at the high discrimination parameters (α = 2.0), they found much higher fidelity coefficients for the stratadaptive tests even though the tests were much shorter (28 items) than the 40-item conventional test (as cited in McBride, 2001b).

Another early adaptive testing system is the Implied Orders Tailored Testing, originally developed by Cliff (1975). While this method is more valid than the fixed item selection methods discussed above, it lacks the psychometric model of the outcome when a person of a certain proficiency level meets an item with certain characteristics; this method as well as the others mentioned above does not utilize the IRT methods used by current CAT systems to explain the interactions between persons and items (Cudeck, McCormick, & Cliff, 1980; Schoonman, 1989). Thus,

further research was conducted in attempts to utilize these IRT methods to better adjust item administration to the proficiency level of the examinee.

Based on the principles of this new testing system, in the early 1980s military researchers began creating computer-based tests that would choose items at the appropriate proficiency level for the examinees. The tests would administer items based on the accuracy of the previously answered items and adapt themselves to the examinee's performance. Thus, they have been named Computer Adaptive Tests or CATs (Wainer, 2000). Unlike these previous tests, these new systems would utilize IRT methods for scoring tests and choosing items. They found that utilizing CAT methodology in their recruitment testing procedure would improve the person-job match due to increased validity of the test. This 12-year military research project which began in 1979 on CAT (Martin & Hoshaw, 2001) will be expanded upon in later sections. While this was the first large scale CAT research program, other smaller CAT research programs will also be discussed in future sections.

Due to this wealth of research, CATs are beginning to replace traditional P&P tests in many fields of measurement. In education, research on converting the GRE to CAT form began in the late 1980's (Schaeffer et al., 1995). As well, tests like the Graduate Management Admissions Test (GMAT) is currently in CAT form. In psychology, cognitive measures (e.g., the ART developed by Embretson, 2005) as well as personality measures have been transferred to computer format. In the medical field, some questionnaires have been developed in CAT form (e.g., the HIT

developed by Bjorner, Kosinski & Ware, 2003). As mentioned above, in the military, the largest high stakes CAT program has been developed (CAT-ASVAB).

Computer adaptive tests have many advantages over the traditional P&P tests: shorter tests, enhanced measurement precision, testing on demand, and immediate test scoring and reporting, to name a few (Meijer & Nering, 1999). In terms of test construction, computer-administration of tests allows for easy pilot testing of new items and immediate removal of faulty items. Another advantage to a CAT is that, because it is administered on the computer, graphics, sounds, running video, and text can be combined to present tasks that resemble real-life tasks (Green, 1983). It is important to note that the first advantage, shorter tests, might seem contradictory to the fundamental principle of test development which states that longer tests provide more reliable estimates of trait level. But, because of the IRT procedure that is followed in a CAT, the proficiency level is more accurately estimated with fewer items (Straetmans & Eggen, 1998). I will outline this procedure in the next section.

Considering the advantages of CAT over traditional P&P tests, there are still some practical aspects that can be seen as disadvantages to examinees who are used to P&P tests. First, while computers are becoming more and more available in today's society, many examinees are still unfamiliar with the use of computers which could give them a disadvantage over those who are familiar with the computer. A second possible disadvantage of CAT when compared with P&P tests is that examinees are no longer given all items at one time allowing them to return to items they might have skipped or to change their answers on items they have already attempted. Rather, they

are given one item at a time, and must choose a final answer before continuing on to the next item. Another possible disadvantage to taking the test in the CAT format is one of motivation and anxiety—if the examinees understand the process the computer goes through to pick subsequent items, and they are given an item that they consider to be easy, they could assume they got the previous question incorrect. This could affect their future performance on the test.

Part of this knowledge about the inner workings of CATs comes from training courses and books like those put out by Princeton Review and Kaplan. One piece of advice that is given by these companies, especially for taking high stakes tests like the GRE, is that "one of the most important things to know is that the first few questions are the most important on the test (Kaplan, 1997, pp. 174)." They continue on to explain how paying extra attention to the first "five or so" problems, leaving the later questions to guessing if necessary, will improve your score. This is based on the assumption that earlier in the test bigger adjustments are made to the estimated proficiency level and increasing your score at the beginning makes it more difficult to get a lower score than starting off with a lower score and returning to a higher score in the end. Or as Princeton Review (Still, 2003) states it:

> "The computer weighs your performance on earlier questions more heavily than it does later ones. Early in the test, your score will move up and down (hopefully, up!) in large increments, but as you near the end, your score will change only by small amounts…This means you'll need to concentrate hardest on answering the early questions correctly, even if this means

> spending more time on them than you'd like. You can make this time up by moving more quickly on later questions, when you'll affect your score less dramatically (p. 10-11)."

This test-taking strategy leads to multiple questions. Will an examinee get a better overall score if they only get the first items correct versus a steadier pattern of correctness throughout the test? Or in a more real-world sense, will an examinee get a better overall score if they pay much closer attention to the first items on the test giving them a sort of boost in their proficiency level earlier on the test? This question is important to the testing community because of the impact it will have on test taking strategies as more and more tests become computer adaptive. These questions were the focus of this set of studies.

I have briefly outlined the advantages and disadvantages to using a CAT system for testing, as well as some of the history of adaptive testing. The rest of this chapter will focus first on the psychometric theory behind a working CAT, including the Item Response Theory (IRT) methodology underlying this system. Secondly, it will look at the actual procedure that the testing algorithm follows when administering a CAT, as well as some of the issues in test design that affect this process. The third section will be dedicated to studies that have been conducted on CATs: for example, human factors and computer adaptive testing and simulation studies on particular testing algorithms. Finally, there will be a short section that focuses on the Abstract Reasoning Test that is currently in CAT form.

2.1 Item Response Theory

In a traditional P&P test, in line with Classical Testing Theory (CTT), a person's proficiency score is usually number correct or some linear transformation of number correct. With a CAT, examinees receive different items, and in some CATs even a different number of items. Some examinees receive harder items while others receive easier items. Thus, it would be inequitable if examinees receive scores based solely on number of correct responses. To deal with this problem, Item Response Theory (IRT) is used to calculate scores. Item Response Theory "presents a mathematical characterization of what happens when an individual meets an item (Wainer, 2000, pp. 12)"; IRT is a mathematical modeling methodology that allows researchers to compare a person's proficiency with the item's difficulty in order to predict the probability of a correct response on the item (Wainer, 2000).

One major advantage to IRT methods over CTT methods is parameter invariance. In CTT, the proportion correct or easiness parameter of an item is based solely on the subpopulation that took the test. The methods used to estimate item and person parameters in IRT remedy this dependence. The property of parameter invariance refers to the independence of the ability distribution of the examinees from the item parameters, that is the true value estimation of the person's proficiency level is not dependent on the particular set of test items administered and an item's true value parameters are not the result of the subpopulation of examinees used to estimate these parameters (Hambleton, Swaminathan & Rogers, 1991).

In summary, IRT is a model-based methodology that can be used to estimate the parameters of each item in an item pool, a person's proficiency level, the reliability and precision of a test, as well as the validity of the item selection algorithm (Wainer, 2000). IRT also allows us to deal with three challenges in adaptive testing. The first is to find a useful way to characterize the variation among items in the item pool. The second challenge is to determine efficient rules for item selection during test administration. The third challenge is arriving at proficiency scores on a common scale regardless of the subset of items the examinees received (Wainer & Mislevy, 2000).

2.1.1 IRT Assumptions

Before getting into the particulars of the methodology, I will discuss the assumptions that must be met when using IRT: item fungibility, test unidimensionality, local item independence, known item parameters, and no differential item functioning (DIF). The first assumption, item fungibility, relates to the order of the items. This assumption states that regardless of the order in which you present the items, the person's proficiency estimate should not be affected (Wainer & Mislevy, 2000).

Unidimensionality refers to the assumption that all items in the test (or subtest) measure only one ability or trait. This assumption is never strictly met due to outside cognitive, personality, and test-taking factors that can affect test performance. (If there are other significant factors playing a role in the measure, they can be modeled using Multidimensional Item Response Theory (MIRT).) For purposes of

modeling the data, what is required is that all items have a dominant factor or component that influences performance on the items. This dominant factor is what test developers can then claim the test measures. While, most IRT models require that this assumption is met, more recently, multidimensional IRT models have been developed (Hambleton, Swaminathan & Rogers, 1991). This chapter will discuss unidimensional models.

Local independence refers to the assumption that when the abilities influencing performance on the test remain constant, an examinee's responses to any pair of items are statistically independent of each other. In other words, performance on one item is not influenced by performance on another item. Mathematically speaking, local independence holds true if the following equation holds true:

$$P(u_1, u_2, ..., u_n \mid \theta_s) = P(u_1 \mid \theta_s)P(u_2 \mid \theta_s)...P(u_n \mid \theta_s) = \prod_{i=1}^{n} P(u_i \mid \theta_s) \,, \quad \text{(Eq. 2.1)}$$

where i = item number = 1, 2, …, n,

u_i = response to item i, and

θ_s = ability level of person s.

That is, the probability of a particular response pattern for an examinee with a given ability level is equal to the product of the probabilities of each individual response to each item, regardless of item order (Hambleton, Swaminathan & Rogers, 1991). Another definition is that local independence is obtained when the relationship between items in a test is fully characterized by the IRT model (Embretson & Reise, 2000).

The local independence assumption can also be called conditional independence. This name refers to the fact that the independence of item responses is only considered independent after you take into consideration the person's ability level. In other words, after you statistically partial out the ability level, the examinee's responses should be independent; an examinee's responses are independent after conditioning on ability (Hambleton, Swaminathan & Rogers, 1991).

There are three IRT models that will be focused on in this paper: the one-, two- and three-parameter logistic models. The next assumption requires that the item parameters required for each model are known, and that these item parameters are the only item parameters that influence examinee performance on that item (Hambleton, Swaminathan & Rogers, 1991). This assumption can also be described in terms of the item characteristic curve (ICC). The ICC is a non-linear probability distribution that demonstrates the relationship between ability level and probability of a correct response on the item. This assumption states that the ICC has a specified form, determined by the item parameter(s) in the model (Embretson & Reise, 2000).

The final assumption that should be met for each item is that items must display no differential item functioning (DIF). That is, an item must perform the same for each person regardless of the subgroup of the population they belong to. Stated another way, "an item shows DIF if individuals having the same ability, but from different groups, do not have the same probability of getting the item right (Hambleton, Swaminathan & Rogers, 1991, pp. 110)."

2.1.2 Item calibration

As stated above, IRT is a model-based methodology. The first challenge to developing a successful CAT, finding a useful way to characterize the variation among items in the item pool, is met through the use of mathematical models that allow us to estimate the item's difficulty, discrimination and guessing parameter. A mathematical model is one that specifies the scale for the observations (dependent variables), specifies the design variables (independent variables), and specifies the numeric combination of how the dependent variables are predicted by the independent variables. These mathematical models are graphically displayed by an S-shaped logistic or normal ogive curve (the ICC) whose properties are defined by the item parameters (Embretson & Reise, 2000).

The first item parameter of mention is the location or difficulty of the item. Item difficulty is the point of inflection on the ICC, or the point where the probability of answering the item correct is equal to the probability of answering incorrectly. The slope of the line is defined by the second parameter—item discrimination. The greater the slope, the more the probability of getting the item correct is affected by the exact ability level of the examinee. The third parameter is the guessing parameter. This parameter defines a lower asymptote of the ICC. The better the probability of answering the item correctly by chance alone, the farther from zero this asymptote will become (Embretson & Reise, 2000).

Each item can be defined in terms of one of three IRT models that utilize some or all of these item parameters. The first of these three models is the simplest—

the one-parameter logistic (1PL) or Rasch model. This model defines the probability of success on an item by the item's difficulty level. Equation 2.2 shows this relationship:

$$P(U_{is} = 1 \mid \theta_s, \beta_i) = \frac{\exp(\theta_s - \beta_i)}{1 + \exp(\theta_s - \beta_i)}, \qquad \text{(Eq. 2.2)}$$

where β_i = difficulty level of item *i*,

θ_s = ability level of person *s*, and

$P(U_{is} = 1)$ = the probability that person *s* responds correctly to item *i*.

This equation is derived from the log odds ratio of the probability of getting an item correct to that of getting the item incorrect as a function of the relationship between the person's ability level and the item's difficulty level. This equation can be seen in Equation 2.3, where P_{is} is the probability of success on the item *i* for person *s*:

$$\ln\left(\frac{P_{is}}{1 - P_{is}}\right) = \theta_s - \beta_i. \qquad \text{(Eq. 2.3)}$$

When the difference between trait and difficulty levels is equal to zero, the odds of success versus failure is 1.0 or 50/50. When the difference is positive, the numerator of the ratio is larger than the denominator, implying that there is a greater chance of success if a person's trait level exceeds the item's difficulty level. The opposite is true if the difference is negative (Embretson & Reise, 2000).

The next model incorporates item discrimination as well as item difficulty. Thus, it is aptly called the two-parameter logistic (2PL) model. The 2PL model can be seen in Equation 2.4:

$$P(U_{is}=1 \mid \theta_s, \beta_i, \alpha_i) = \frac{\exp(1.7\alpha_i(\theta_s - \beta_i))}{1+\exp(1.7\alpha_i(\theta_s - \beta_i))}\ , \qquad \text{(Eq. 2.4)}$$

where α_i = discrimination of item *i*.

The item discrimination is a multiplier of the difference between trait level and item difficulty. The impact of this difference depends on the discriminating power of the item; with highly discriminating items this difference has greater impact on the probability of success (Embretson & Reise, 2000).

The final model includes a third item parameter—a guessing parameter (γ_i). This parameter allows for a lower asymptote for the ICC due to the chance of guessing the correct answer on a multiple choice item. Yet, estimates of this parameter typically come out smaller than the value that would result from random guessing on an item. For this reason, this parameter is sometimes called the pseudo-chance-level parameter. The addition of this new parameter into the 2PL model can be seen in Equation 2.5 for the 3PL model (Embretson & Reise, 2000).

$$P(U_{is}=1 \mid \theta_s, \beta_i, \alpha_i, \gamma_i) = \gamma_i + (1-\gamma_i)\frac{\exp(1.7\alpha_i(\theta_s - \beta_i))}{1+\exp(1.7\alpha_i(\theta_s - \beta_i))}\ , \qquad \text{(Eq. 2.5)}$$

where γ_i = guessing parameter for item *i*.

The relationship between these three models can be seen by the fixing of one or two of the parameters. The 1PL model is the same as the 2 and 3PL models with α_i fixed at 1.0 and γ_i fixed at 0.0. Example ICCs for each of these models can be seen in Figure 2.1. Items 1 and 2 were estimated with the 1PL model (β_1 = 0.0, β_2 = 1.0). Items 3 and 4 were both estimated using the 2PL model (β_3 = -2.0, α_3 = 0.4, and β_4 = 0.0, α_4 = 0.5). Item 5 used the 3PL model (β_5 = -2.0, α_5 = 1.0, γ_5 = 0.2). While looking

at this example, it should be noted that, in general, item difficulties tend to range between -3 and 3, with item discriminations usually between 0.2 and 2.0. Guessing parameters are rarely greater than 0.3.

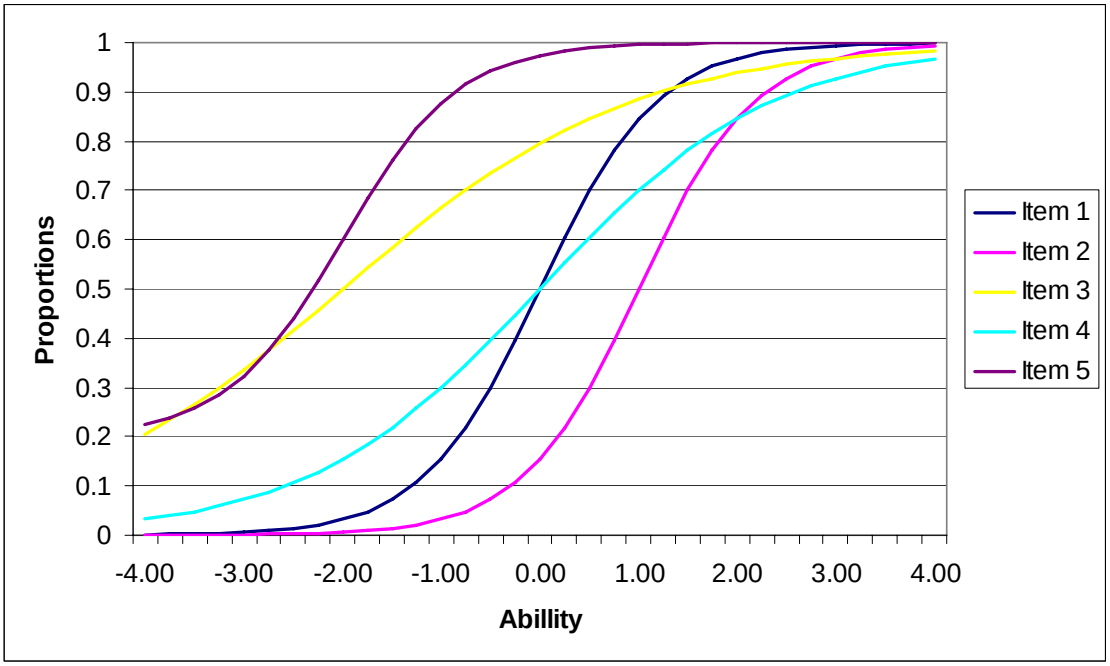


Figure 2.1. Example ICCs for five items.

To estimate these parameters, items must be administered to many examinees with known ability estimates (θ_s). Then, a log-likelihood function (Equation 2.6) can be obtained from the responses to the item by the N examinees. This likelihood function is the product of the probabilities of a correct/incorrect response by each examinee as a function of their ability level and the item's parameters (Embretson & Reise, 2000).

$$\ln L(u_1, u_2, ..., u_N \mid \theta_s, \alpha_i, \beta_i, \gamma_i) = \sum_{s=1}^{N} \left[u_s \ln P_s + (1 - u_s)(1 - P_s) \right], \quad \text{(Eq. 2.6)}$$

where u_s = the response to item s (1=correct, 0=incorrect), and

P_s = the probability of a correct answer to item s.

Maximum likelihood estimation (MLE) is then used to estimate the most likely values for the parameters. When estimating the 3PL model for an item, the most likely value for all three parameters must be estimated simultaneously. Thus, MLE is an iterative procedure that attempts to locate the maximum value of a surface (represented by the likelihood function) in three dimensions (Hambleton, Swaminathan & Rogers, 1991). This estimation procedure will be discussed further in the next section.

A successful CAT should have available to it an extensive and calibrated item pool. Item calibration should be done prior to item use, with each item being tested on a large number of examinees—Wainer and Mislevy (2000) suggest upwards of 1,000 examinees with a proficiency distribution similar to the difficulty distribution of the items being calibrated. After the item parameters have been estimated, we can then use this information to calculate proficiency scores for examinees.

2.1.3 Person calibration

We denote a person's proficiency level with the Greek capital letter, θ. As with item difficulties, person trait levels tend to range between -3 and 3, with the majority of the population falling between -2 and 2. As well, trait levels are estimated

from a similar likelihood function (see Equation 2.7) based on the same probability functions as item parameters (Embretson & Reise, 2000).

$$\ln L(u_1, u_2, ..., u_n \mid \theta_s, \alpha_i, \beta_i, \gamma_i) = \sum_{s=1}^{n} \left[u_s \ln P_s + (1 - u_s)(1 - P_s) \right] \quad \text{(Eq. 2.7)}$$

We can estimate θ using a maximum likelihood method or using a Bayes Modal estimation procedure. The maximum likelihood procedure is essentially a Bayes Modal estimator with a uniform prior. The maximum likelihood estimate of θ is the mode or the maximum value of the likelihood function (Wainer & Mislevy, 2000). The Bayes Modal estimator is also known as the Maximum A Posteriori (MAP) (Embretson & Reise, 2000).

When finding the maximum value of the log-likelihood function above, it would be intuitive to find the first derivative of the function and set that to zero and solve for θ_s. This, however, results in an unsolvable equation. Thus, maximum likelihood estimation is a procedure that utilizes the Newton-Raphson scoring algorithm. The first step in this procedure is to specify a start value for θ_s (e.g., θ_s = 0.0). The next step is to calculate the first and second derivatives of the log-likelihood function at this value of θ_s. The ratio of the first derivative to the second derivative , which we denote as ε, is calculated. This new value is then subtracted from the original estimate of θ_s. This value is then used as the new start value for θ_s. This iterative procedure is repeated until ε is less than some small value (e.g., $\varepsilon < 0.001$) (Embretson & Reise, 2000).

Maximum likelihood estimates have well-known asymptotic properties. That is, as test length increases, the MLE of θ_s ($\hat{\theta}$) becomes distributed normally with a mean equal to the true value of θ_s and a standard error that is a function of the test information, $I(\theta_s)$ (see Equation 2.12 for $I(\theta_s)$) (Hambleton, Swaminathan & Rogers, 1991):

$$SE\left(\hat{\theta}\right) = \frac{1}{\sqrt{I(\theta_s)}}. \qquad \text{(Eq. 2.8)}$$

When a person answers all items correctly or all items incorrectly, the MLE procedure will be unable to calculate an accurate estimate, and rather will estimate the trait level as equal to positive or negative infinity. Other aberrant response patterns can result in this same estimation problem when using the 3PL model. This problem can be overcome using the Bayes Modal estimation method. This method incorporates prior information about the ability parameters into the likelihood function. It is important to note that when a uniform prior distribution is used for all examinees, the estimate computed will be numerically identical to the MLE found (Hambleton, Swaminathan & Rogers, 1991).

For this method, prior information is expressed in terms of a density function denoted as $f(\theta)$. The posterior density function found using this method is seen in Equation 2.9:

$$f\left(\theta \mid \underline{u}\right) \propto L\left(\theta \mid \underline{u}\right) f\left(\theta\right), \qquad \text{(Eq. 2.9)}$$

where $\underline{u}$ = the vector of responses to all items on the test.

The mode of this new function is the most probable value for θ, and is then used as an estimate for θ (Hambleton, Swaminathan & Rogers, 1991).

The mean can also be calculated for this distribution by approximating the posterior distribution of θ by forming a frequency distribution with k values of θ. The frequency at any given value of θ is given by the posterior density function. The mean can then be calculated as follows (Bock & Mislevy, 1982):

$$\mu(\theta \mid \underline{u}) = \frac{\sum_{j=1}^{k} \theta_j f(\theta_j \mid \underline{u})}{\sum_{j=1}^{k} f(\theta_j \mid \underline{u})}. \qquad \text{(Eq. 2.10)}$$

This estimate is called the Expected A Posteriori (EAP) estimate. Wainer and Thissen (1987) found that the EAP estimate of ability had the smallest mean squared errors when compared to the other methods.

The accuracy of the proficiency estimate is a measurement of the width of the posterior likelihood distribution. If the distribution is very narrow, then the proficiency estimate is considered more accurate than if the distribution is very broad. Adaptive testing tends to decrease the width of this distribution by judiciously selecting each item on the test. When comparing the two methods of proficiency estimation, the maximum likelihood estimator to the Bayes Modal estimator, it is seen that the Bayes Modal estimate is typically more precise than MLE (Wainer & Mislevy, 2000).

2.1.4 Joint Person and Item Calibration

When ability estimates are known, the item parameters can be estimated. When item parameters are known, the ability estimates can be estimated. When neither are known, they must be estimated jointly. In this situation, the data for all items and all examinees must be considered at the same time. This is done using the following joint likelihood function (Hambleton, Swaminathan & Rogers, 1991):

$$L\left(\underline{u} \mid \underline{\theta}, \underline{\beta}, \underline{\alpha}, \underline{\gamma}\right) = \prod_{s=1}^{N} \prod_{i=1}^{n} P_{is}^{u_{is}} \left(1 - P_{is}\right)^{1-u_{is}} \quad , \qquad \text{(Eq. 2.11)}$$

where $\underline{u}$ = the vector of responses for each person,

$\underline{\theta}$ = the vector of ability estimates,

$\underline{\beta}$ = the vector of item difficulty estimates,

$\underline{\alpha}$ = the vector of item discrimination estimates, and

$\underline{\gamma}$ = the vector of item guessing parameter estimates.

Estimation for these parameters begins with a major issue: item difficulty and item discrimination are both arbitrary scaling constants, which means that there is no unique maximum for the likelihood function. This issue of indeterminacy can be dealt with by first choosing an arbitrary scale for either the ability estimates or for the item difficulty. Typically, the mean and standard deviation for the *N* ability estimates are set to 0 and 1, respectively. Then, the procedure joint maximum likelihood estimation (JMLE) can be used to estimate the unknown parameters.

JMLE is completed in two cyclical stages. The first stage is to choose initial values for the ability parameters. This is calculated as the standardized logarithm of

the ratio of the number correct to the number wrong for each examinee. Then, treating the ability parameters as known, the item parameters are estimated. In the second stage, these known item parameters are used to estimate the ability parameters. This cycle is repeated using the estimated ability parameters to estimate item parameters, and so forth, until there is no change in the estimates.

As with MLE, JMLE cannot estimate parameters for people with perfect or zero scores or for items that everyone got correct or everyone got incorrect. Another disadvantage is that JMLE does not yield consistent person or item parameter estimates when using the 2- or 3PL model. For the 3PL, this procedure may fail entirely if some restrictions are not placed on the ability or item parameters.

This first disadvantage, that of perfect or zero scores, can be overcome with Bayesian methods as with MLE. The problem of inconsistent estimates can be overcome using a method called marginal maximum likelihood estimation (MMLE). This procedure requires specifying a distribution for the ability parameters and integrating them out of the likelihood function before estimating the item parameters. This requires a large pool of examinees from which to estimate this ability distribution. Once the item parameters have been estimated, they can be used to then estimate the ability parameters. MMLE can fail when it is necessary to estimate the guessing parameter. This issue can be dealt with through placing priors on the guessing parameter (Embretson & Reise, 2000). Bayesian methods, however, do not encounter this problem (Hambleton, Swaminathan & Rogers, 1991).

2.1.5 The role of IRT in CAT

2.1.5.1 Ability level estimation. This summary of IRT demonstrates some of the psychometric properties used in developing and implementing a CAT. The combination of the availability of computers and the usefulness of IRT allows test developers to create tests that will find more precise estimates of a person's proficiency level with fewer items. As well, computer adaptive testing uses the IRT invariance property to create an algorithm by which examinees can take a test that appropriately measures their ability level. These use of IRT methods versus CTT methods also provide more useful information for distinguishing between examinees within a certain range of a trait. Examinees are given items that are more appropriate and, thus, more defining at their proficiency level (Embretson & Reise, 2000). In other words, IRT provides a basis for tailoring the difficulty of the test to the ability level of the examinee, locating items and examinees on the same scale, and expressing all scores on the same scale even when examinees have taken tests consisting of different items. These advantages of IRT result in much more efficient adaptive tests than those based on CTT methods (McBride, 2001a).

Further, IRT plays three important roles in the process an adaptive test follows. These include (1) estimating the examinee's ability level, (2) selecting items sequentially, and (3) deciding when to stop testing. Within the IRT-based adaptive testing system framework, there have been two ability estimation methods used extensively: maximum likelihood estimation (MLE) (Lord, 1980), and Bayesian

sequential estimation (Owen, 1969, 1975; Urry, 1983). Both of which were discussed above and will be touched on further in later sections.

2.1.5.2 Item bank development. Another issue that should be touched upon in this discussion of the role of IRT in CAT deals with developing the item bank. The important question here is which of the three aforementioned models to use when characterizing your items (1-, 2-, or 3-PL models). If enough item response data is available to estimate an item's parameters, it is advisable to use the 3PL model when your test items are in multiple-choice format (McBride, 2001a). Lord (1970) demonstrated that the 3PL model is more efficient than using the 1PL model to estimate a person's proficiency score on a multiple choice test, because the 1PL model sacrifices measurement precision, making the results less reliable. Urry (1974) also demonstrated this increase in reliability when comparing the 3PL to the 1PL model. He found this to be especially true when all items in the adaptive item bank had discrimination parameters equal to or above .80.

In summary, when developing a CAT testing program, as previously alluded to, there are some important components that must be taken into consideration. The first is developing a firm psychometric foundation—that is, a valid, defensible theoretical basis for administering different questions to different people, yet expressing all results on a single scale. The second consideration is that of the item bank. A large set of items, which measure the domain of interest and has psychometric characteristics that will make them useful for adaptive testing, must be developed or available. A third important component is that of choosing a strategy or

set of procedures for sequentially choosing which item to administer at each stage of the test. A fourth component in developing a successful CAT is providing a body of research that justifies the usefulness and validity of adaptive testing as an alternative to the conventional version (McBride, 2001a). While the first two components have been discussed in this section, these third and fourth components will be expanded upon in the next section.

The next section will explain the procedure a CAT follows when administering a test. Many of the properties of IRT that were discussed here will be helpful for understanding this explanation.

2.2 Computerized Adaptive Testing

2.2.1 Introduction

With a conventional paper-and-pencil test, one major assumption is that all examinees receive the same or parallel items. As a result, many of the items that an examinee receives are not very informative at their proficiency or trait level. For example, if they are more proficient, items at the lower end of the difficulty scale do not tell the administrator much about their proficiency. Using the IRT principles described in the previous section, CAT addresses these inefficiencies by attempting to administer each examinee items for which their chance of answering correctly is approximately 0.50 (Embretson & Reise, 2000).

Before discussing the actual procedure a CAT algorithm can follow to administer items, I will discuss some of the issues that must be dealt with prior to developing a successful CAT program. One such issue is item pool development and

testing. As with P&P testing, it is very important that items are carefully written, that item content does not discriminate against particular subsets of the population and does not function differently for different subsets of the population, a problem known as differential item functioning. It is also important these items are not flawed in some way, because individual items impact a person's proficiency score much more in a CAT than in a P&P test due to the fact that people receive less items and each item helps direct the test toward a particular score (Wainer, 2000).

Another issue to consider is the extensiveness of the item pool. Ideally, an item pool should include enough highly discriminating items with difficulty parameters over the entire trait range. This is necessary to ensure that the entire trait range is measured well (Weiss, 1982). Ree (1977) suggested a ratio of 5 to 10 calibrated items in the item bank to every 1 item an examinee will have to encounter. Embretson and Reise (2000) suggest a rough estimate of around 100 highly discriminating items, with difficulty parameters spread widely across the trait range, for dichotomously scored items. Urry (1971) suggested that an ideal item bank should consist of items with a wide and uniform distribution of difficulty parameters, with high discrimination (none less than .80), and low guessing parameters (none greater than .33). Jensema (1974) demonstrated that the fidelity coefficient varied directly with the magnitude of the discrimination parameter, inversely with the size of the guessing parameter, and directly with the test length.

A third issue surrounding the item pool is one of item calibration. It is important to consider how the item parameters were estimated for the items in the

item pool. Research has shown that item parameters estimated using results from a P&P test are not directly translatable to computer administered tests (e.g., Green et al., 1984; Mead & Drasgow, 1993; Neuman & Baydoun, 1998; Spray, Ackerman, Reckase & Carlson, 1989). Part of this issue could be due to possible order effects. With P&P tests, all examinees receive the same items in the same order. With CATs, the order is variable for all examinees. Thus, when using a CAT, researchers must make the assumption that presentation-order does not affect item parameter estimates (Embretson & Reise, 2000).

Another issue that must be dealt with in a CAT system is that of item exposure control. One of the first methods for dealing with this issue was proposed by McBride and Martin (1983). In this method, the program chooses the first item of a test at random from the five most appropriate possibilities, the second item is chosen from the four remaining best possibilities, the third from the three best, and the fourth from the two best. Then, beginning with the fifth item, the best possible item is chosen for the remainder of the test. This allows for 5 x 4 x 3 x 2 x 1 = 120 possible item response patterns for any likelihood of response at any given ability level.

A more complex method involves the calculation of an exposure control parameter, k_i, for each item (Sympson & Hetter, 1985, as cited in Thissen & Mislevy, 2000). Each item in the pool is assigned an intended maximum probability of exposure value, r. This value is the maximum proportion of the examinee population that should receive this item. The smaller the value of k_i, the less likely it is that item i is administered. Thus, when any item is chosen as the most informative at the current

estimate of θ, a random number between 0 and 1 is also chosen; if that number is larger than k_i, then the item is administered; if it is smaller, then the item is not administered and the next most informative item is chosen. This value of k_i is empirically derived through simulation studies to ensure that the use of the item by a randomly selected examinee is approximately equal to r.

Content balancing is another important issue to be considered when designing a CAT. A parameter for the content of the item must be included in tests of ability that covers a range of aspects. In a fixed-length test, this can be done by separating the item pool into bundles of items based on content and then setting the testing algorithm to choose the most informative item from the content bundles for pre-specified locations on the test. This also ensures that all examinees receive items of particular content in the same order, ridding the outcome measure of any ordering effects. For a variable-length test, the testing algorithm can be set to rotate through the bundles to help ensure equal content balancing throughout the test (Thissen & Mislevy, 2000).

Because the *first* high-stakes testing program to take CAT form was the Armed Services Vocational Aptitude Battery (ASVAB) much of that research will be discussed throughout the following sections. It is important to note that this development program that began in 1979 included a number of "firsts" in CAT research. The CAT-ASVAB research and development (R&D) program was the first to develop a complete multiple-aptitude battery of adaptive tests. They were the first to develop a micro-computer based adaptive testing system, which was capable of

displaying graphical test items. They were the first to deliver adaptive tests on a network of personal computers. In terms of evaluating adaptive tests, they were also the first to demonstrate the construct equivalence of conventional and adaptive multitest batteries, establish the predictive validity of a battery of adaptive tests, develop technical standards for evaluating adaptive tests, and develop and apply technology for equating conventional and adaptive tests (McBride, 2001a).

I will now discuss this procedure through which a CAT administers appropriate items to examinees to better estimate their proficiency or trait level.

2.2.2 The procedure

In short, the administration of a computer adaptive test is a cyclical process that follows a basic three-step procedure. In the first step, the computer administers an item at the difficulty level matched to the current estimate of the examinee's trait level. After the examinee attempts the item, the trait level is re-estimated based on this new information (e.g., whether or not the response was correct on an achievement battery). Then the program administers an item at this newly estimated trait level. These steps are repeated until a prespecified stopping criterion is met (Wise & Kingsbury, 2000). The general logic behind an adaptive test can be seen in Figure 2.2.

I will now expand upon this process. When developing a CAT, one must choose the testing algorithm that consists of three major parts: starting rules, continuing rules, and stopping rules (Wainer, 2000). Each step will be discussed in more detail.

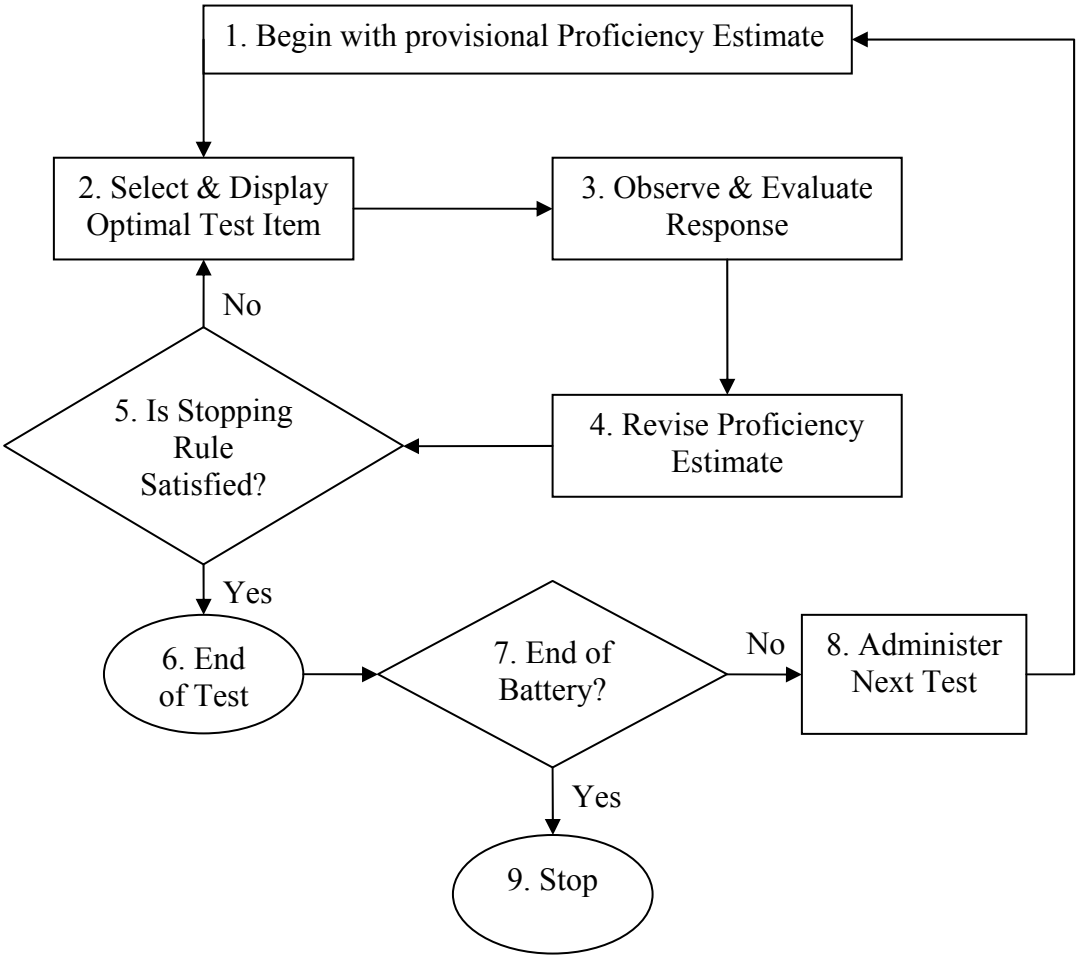


Figure 2.2. Flowchart representing an adaptive test (adapted from Thissen & Mislevy, 2000).

2.2.2.1 Step 1 – The starting rule. There are multiple rules to choose from when deciding on the first item to be administered. Given that the program uses information about the examinee to choose an item, the first starting rule could be to use some prior information on the examinee and find an item that has a difficulty level close to the trait level previously identified for the examinee (Straetmans & Eggen, 1998). This method can be utilized through the use of scores on tests similar to the current test being administered. This can be done by exploiting the relationships between the tests in question—that is, using the examinee's score on the previous exam and converting that score to the scale of the current exam and using this score as a starting point for choosing the first item(s) for the test (Thissen & Mislevy, 2000).

Another version of this rule is to use prior information from the tested population when there is no prior information for the examinee. An item with difficulty located at the mean proficiency level for the group of examinees who have already completed the exam is an option for a starting point. This could also be done by first specifying group memberships of the examinee through demographic information gathered on all examinees. The examinee could then receive an item located at the mean proficiency level for the identified group.

If this last method is utilized throughout the testing algorithm, it provides higher expected precision over the population of examinees but it also invokes issues of fairness. While smaller error variances result, there are expected tendencies toward certain types of biases when starting examinees with different proficiency estimates.

One type of bias is underprediction of higher-scoring individuals who belong to lower-average proficiency groups due to the combination of individual responses with the initial use of group membership information—a sort of regression towards the mean of the group the examinee belongs to. These issues of fairness disappear if this method is only used for selecting a first item for an examinee, though this will result in higher mean squared error rates (Thissen & Mislevy, 2000).

If no prior information is available for the examinee or the population the examinee is from, other starting rules should be considered. One such rule is to use an item of average difficulty level (b = 0.0). If this rule is used, the item pool must contain enough items at this average difficulty level to ensure that examinees do not receive the same first items. If the same items are given to all examinees, the items could easily be made public resulting in inflated success rates and decreased validity of the item(s) in question. Hulin, Drasgow, and Parsons (1983) suggested randomly selecting from a set of 30 or more items at or near this average ability. Another way to remedy this problem, Embretson and Reise (2000) suggested choosing an item from within the initial difficulty range of -0.5 to 0.5 if the examinee population can be assumed to be normally distributed over the trait continuum. Other methods for item exposure control could also be considered (Thissen & Mislevy, 2000).

Another rule is not based on psychometric properties but is rather based on old habit by test developers: give easy items initially to the examinees to help reduce test anxiety (Straetmans & Eggen, 1998). Many P&P tests begin with the easiest

items and get harder as the examinee proceeds through the test. This particular starting rule would conform to this sort of test format.

There are many issues to consider when choosing a starting rule. One such issue is whether different proficiency estimates and different items adversely affect final estimates. As we will see in later discussions, the method of initial item selection does not adversely affect final score estimates when using a likelihood based estimator, but could affect the estimates when using a Bayesian method (Thissen & Mislevy, 2000). It has been shown that the longer the test, however, the less the initial item will affect the final estimate of proficiency level (Lord, 1980).

As discussed previously, another consideration in choosing an algorithm starting or continuation rule is that of test security. If the same item is used to start all examinees (i.e., the item at the average ability level), then the succeeding item should also be the same for those who answer the first item correctly, as well for those who answer incorrectly. This pattern could continue, resulting in identical item administration patterns due to identical response patterns. This, in turn, could become a security issue (and item exposure rate issue) if this information is shared among examinees. One way to combat this issue is through a fixed-set size or shrinking set size selection procedure (mentioned in the previous section). McBride, Wetzel and Hetter (2001) summarized these two methods. In the fixed-set size procedure, rather than choosing the one item that provides the maximum information for the examinee, the item would be randomly chosen from *k* items that would come close to maximizing this information function. This could be continued for the first 5 or so

items. One important thing to remember is while that the larger value of *k*, the more random the sequence of items administered, but also the larger the loss of precision. Another version of this method is to use a shrinking set size. For this method, the first item would be randomly chosen from *k* items that come close to maximizing the information function at that ability level. For the succeeding items, *k* would be reduced by some increment until each item administered was *the* best item at that ability level.

2.2.2.2 Step 2 – The continuation rule. After the examinee responds to the first item, the adaptive algorithm usually begins. Using the parameters of the IRT model, the computer now administers items based on the examinee's previous pattern of correct/incorrect responses. There are two decisions that must be made: how to score the responses and how the next item is chosen for administration (Embretson & Reise, 2000). Thus, after the first item is presented, there are also multiple methods for choosing the succeeding items. Before detailing these methods, some important issues should be discussed.

All of the methods described in this section choose the single "best" item at each stage for administration. However, while there are multiple psychometric methods for item selection, there are other item selection constraints that should be considered. The first, most obvious constraint that must be applied to the testing algorithm is that no item, though it may be the best item, should be given to the same examinee twice. Another issue that can be dealt with through item exposure constraints is that the methods listed below tend to choose items with high

discrimination values. This results in overexposure of those items in comparison to items with lower discrimination values. The third constraint that should be considered, which is especially important in the achievement/aptitude testing fields, is placed upon item content. It is very important in these fields that a certain number of topics are covered on the test to ensure a broad enough measure of a person's proficiency in that domain (Thissen & Mislevy, 2000). Many studies have been conducted that look at item exposure constraints (see, Stocking and Swanson, 1993; Kingsbury & Zara, 1991; Theunissen, 1986).

The choices available for how to score responses on items include Maximum Likelihood Estimation (MLE), Maximum A Posteriori (MAP), and Expected A Posteriori (EAP). As mentioned in the previous section, MLE is not ideal when all items have been answered correctly or all have been answered incorrectly, thus MLE cannot be used for scoring responses until an examinee has gotten at least one item correct and one incorrect. Bock and Mislevy (1982) found that EAP can be used to avoid this problem when scoring examinees, because this algorithm allows the estimation of examinees' trait levels based on just one response by using prior information distributions. Some researchers disagree with the use of prior information due to the possible effect the prior information might have on outcome scores, like regression toward the mean when a limited number of items are administered (Embretson & Reise, 2000).

Dodd (1990) discussed another method for overcoming this problem. In this step-wise method, the responses on the initial items are not scored. Rather, a

particular step-size, or change in difficulty level, is chosen (e.g., increments 0.25). If an examinee gets the item incorrect they are given an item chosen randomly from the item pool with a difficulty level 0.25 less than the current item. If they answer correctly, they receive an item with a difficulty level that is increased by 0.25, thus, starting the test with a more pyramidal-type testing system. After enough have been administered, the MLE method can then be used to begin estimating an examinee's score after each item.

One of the earliest versions of a maximum likelihood approach to adaptive testing was proposed by Urry (1977). In this method, at each stage of the test, the next item to be selected was chosen by matching the item ability level to the item difficulty level. Another early version of a maximum likelihood approach was proposed by Lord (1977). The Broad Range Tailored Test (BRTT) utilized MLE to estimate the examinee's ability level after each item. The closest discrete ability level to this estimate is located, and the first unused item in the sorted list that corresponds to this ability level is administered. These MLE approaches do not always yield the maximum information at $\hat{\theta}$ (Weiss, 1982). Thus, the maximum information approach was proposed for this process.

The maximum information approach corresponds to the MLE approach for estimating examinee score by choosing the item that provides the most information at the current estimate of ability. When unconstrained, this approach selects an item *i* that maximizes the item information (see Equation 2.12) evaluated at the provisional proficiency estimate for examinee *s* after *n* preceding items.

$$I_i\left(\hat{\theta}_s\right) = \frac{\left[P_i'\left(\hat{\theta}_s\right)\right]^2}{\left\{P_i\left(\hat{\theta}_s\right)\left[1 - P_i\left(\hat{\theta}_s\right)\right]\right\}}, \qquad \text{(Eq. 2.12)}$$

where $\hat{\theta}_s$ = the provisional proficiency estimate for examinee *s*,

$P_i\left(\hat{\theta}_s\right)$ = the probability of a correct response to item *i*, and

$P_i'\left(\hat{\theta}_s\right)$ = the first derivate of $P_i(\theta)$ with respect to θ evaluated at $\hat{\theta}$.

Thus, the next item is chosen that will maximize this equation. As with MLE, this approach fails when no finite value of θ exists (usually due to perfect or zero scores). This procedure is computationally burdensome, but very efficient (Thissen & Mislevy, 2000).

A less efficient, but also less computationally burdensome approach uses an Info Table, which lists all items and their information at a number of proficiency levels scanning the continuum of proficiency scores. This approach chooses the item that maximizes information for proficiency levels at or around the particular scores listed in the table. Equation 2.12 is used to calculate the information for each item at each of the predetermined values of θ, and these values are then listed in the Info Table (Lord, 1980).

Another approach, the Bayesian or maximum expected precision attempts to minimize the standard error of the examinee's expected posterior. This approach is most often accredited to Owen (1975). Through this method, the posterior distribution of θ after *n* preceding items is calculated after each stage of the test. The selection of

the next item to be administered is based on maximizing the expected a posterior precision:

$$E_u\left[Var^{-1}\left(\theta \mid \underline{s}_n, i, u_{n+1}\right)\right] = \sum_{u=0}^{1} Var^{-1}\left(\theta \mid \underline{s}_n, (i,u)\right) \cdot \int P(u+1=u \mid \theta, \alpha_i, \beta_i, \gamma_i) p\left(\theta \mid \underline{u}_n\right) d\theta, \quad \text{(Eq. 2.13)}$$

where u = the response to item i,

$\underline{s}_n$ = the information available for the person on all preceding items,

$\underline{u}_n$ = the vector of responses to items 1 through n, and

$p(\theta|\underline{u}_n)$ = the posterior distribution of θ.

In a group of simulation studies, Vale (1975) compared several adaptive testing strategies as well as two conventional test designs. In his study, in terms of test information throughout the normal range of ability, he found the fixed-length Bayesian strategy superior to all other testing strategies (as cited in McBride, 2001b).

Jensema (1974) conducted a simulation study comparing the fixed- and variable-length Bayesian adaptive format. He found that, in the fixed-length tests, the magnitude of fidelity coefficient was a function of the discriminating power of the items, while in the variable-length tests, this coefficient was determined by the target posterior variance. McBride and Weiss (1976) examined the role that the relationship of difficulty and discrimination parameters played in variable-length Bayesian adaptive testing. They found that when these two parameters were positively correlated, the test length decreased as ability level increased. For negatively related difficulty and discrimination parameters, they found the opposite: test length increased as ability level decreased (as cited in McBride, 2001b).

The maximum information approach and the maximum expected precision approach do not always yield the same results (Embretson & Reise, 2000). Many CAT systems are hybrids of these methods. Many of the currently used CAT systems use Owen's EAP method for estimating θ after each item and use maximum information for item selection (Thissen & Mislevy, 2000).

After a starting and continuation rule has been decided upon, a stopping rule must also be chosen. I will now outline some possible stopping rules.

2.2.2.3 Step 3 – The stopping rule. The final decision that has to be made when designing a CAT algorithm is what criteria to use to end the test. The stopping criterion is usually a predetermined number of items, a predetermined time limit, a desired level of measurement precision, or a combination of any of these (Wise & Kingsbury, 2000). At the most basic level, a mixture of measurement precision and limited number of items must be used, otherwise, a examinee could exhaust the entire item pool without reaching the desired precision level (Thissen & Mislevy, 2000).

Using the fixed test length method, that is a predetermined number of items, has some advantages over the other methods. First, it is easy to implement. Second, item usage rates can be more easily predicted. However, this comes at the price of measurement precision. Using this method allows for varying degrees of measurement precision, and this variability should increase the farther from average a person's proficiency score is. Simulation studies can look at this effect of test length on measurement precision for any given set of items (Thissen & Mislevy, 2000).

When the chosen stopping rule is a particular level of measurement precision, the test administrator can choose any level of precision. This results in a variable number of items each examinee might encounter, but a constant measurement precision across all ability levels (McBride, 2001a). In the maximum information framework, this precision would be reached when a target response pattern information is reached. In the EAP or MAP framework, this target is reached based on the target posterior precision. Simulation studies can be conducted to determine the expected length of a test necessary to reach this precision for examinees at various proficiency levels.

This stopping method has one major advantage—by measuring all examinees to the same level of precision, the data then conforms to the traditional test theory assumption of equal measurement error variance (Thissen & Mislevy, 2000). As well, Bock and Mislevy (1982) found that when using the EAP-based stopping rule, the reliability of the estimates is the same for all examinees, as long as enough items have been administered.

The third method, a fixed time limit, seems only to be appropriate for speeded tests. For power tests, choosing this method alone can defeat the purpose of the test (Thissen & Mislevy, 2000). Yet, this method is used in most standardized CATs. Typically, the predetermined amount of time allowed is based on previous studies that allow for the majority of examinees to finish the exam. As well, with most CATs used for achievement or aptitude testing, all examinees have the potential to receive

the same number of items, but are limited in the amount of time in which they have to complete the test.

It should be noted that the method used to estimate the final proficiency score need not be the same method that is used throughout the test to estimate current proficiency level in order to select the next item. Though there are some definite advantages (better stability in estimates and better use of available information during testing) to using the Bayesian estimators, rarely do the final estimates from an MLE method and a Bayesian method differ significantly. This is found to be especially true after at least 20 items have been administered (Thissen & Mislevy, 2000).

2.2.3 Previous Research on CAT

Since its inception, CAT has been the focus of much research in the field of testing and measurement. Its advantages over other testing methods make it a prime target for researchers. Here I will outline some of this recent research on this testing method including research comparing CAT to other computer based testing designs, research comparing IRT models within CAT, research on other testing algorithms, and research on practical issues within computer adaptive testing. As well, probably some of the most large scale research that has been conducted on CAT that has been by the military in developing a CAT-ASVAB (Armed Services Vocational Aptitude Battery) will be discussed (Sellman & Arabian, 2001).

2.2.3.1 Technical issues within CAT. Earlier in this chapter, methods for large scale testing were outlined and discussed. As well, technical issues dealing with the role of IRT in CAT and the algorithms used in CAT were discussed. A few studies

have been conducted directly comparing CAT with other methods for computer-based testing. As well, some studies have been conducted comparing IRT models and various testing algorithms within the CAT framework. Here I will discuss some of these studies.

Jodoin (2005) compared the linear fixed (LFT), multi-stage (MST, Luecht & Nungester, 1998), and adaptive (CAT) computer-based testing methods under varying levels of item pool quality, test length, and exposure control expected. He conducted a simulation study looking at multiple indices of unconditional measurement precision. The LFT design is a computer-based version of a P&P test. Items are delivered in the same order, one at a time, and can be skipped and returned to later in the testing session. The MST design is similar to the two-stage branching design discussed earlier. Blocks or modules of items are administered to the examinee, then, based on their performance on that block of items, the examinee is directed to another module of easier items, harder items, or items of the same difficulty level. Thus, unlike CATs, the test is adapted after a *collection of items* is administered. Similar to CATs, where item exposure rates are partially dealt with through creating items that are matched on content and statistical specification, MSTs deal with item exposure rates through matching modules on content and statistical specification. MSTs provide a balance between LFTs and CATs in that they allow a examinee to return to items within a module before moving on to the next module (Jodoin, 2005).

In his comparison of these three designs, Jodoin (2005) found that test reliability was uniformly high (.91 to .98) across all conditions; test reliability was

highest for CAT, second highest for MST, and lowest for LFT. Within test designs, more stringent exposure control resulted in lower reliability. He also found in the simulations that to gain comparable reliability as a CAT, the LFT design needed to be over twice as long.

Lu and colleagues (2003) conducted a simulation study comparing the 1-, 2- and 3-PL IRT models between CAT and P&P testing formats, with or without set trimming (that is, removing items from the CAT item sets that exhibited poor model-data fit and those that differed from other items within the set in difficulty). They reported a reliability of 0.85 for the P&P test. When compared to the CAT with and without trimming the reliability for the three IRT models increased from the 1-PL model (0.78 for both CATs) to the 2-PL model (0.84 with trimming vs. 0.83 without trimming), and then leveled out at the 3-PL model (0.85 with trimming vs. 0.84 without trimming). These findings suggest that the 2-PL model is the most parsimonious model; the 2-PL model is much more reliable than the 1-PL model, and the increase in reliability with the 3-PL model is not large enough to justify adding a third parameter to the model. Other findings from this study include a great reduction in bias through set trimming, consistent exposure rate frequencies across the three IRT models, very little difference in exposure rate frequencies between trimmed and non-trimmed sets, slightly better measurement precision through set trimming, and fewer violations of content constraints through non-trimming.

Deng and Ansley (2003) conducted a simulation study that compared the item usage and test efficiency of four item selection algorithms used in CATs. The four

methods they examined included: the maximum Fisher information procedure (F), the a-stratified multistage CAT (STR), a refined stratification procedure that allows more items from the high *a* strata and fewer items from the low *a* strata (USTR), and a completely random item selection procedure (RAN). They compared these methods over multiple conditions that varied the level of exposure constraints, the test lengths, and the target maximum exposure rates. They found that RAN yielded the best item usage, yet had the lowest test efficiency, over most conditions. Under no item constraints, F proved to be more efficient that STR and USTR, but had poor item usage. USTR resulted in lower error variances over STR with little compromise in item usage. USTR also improved item usage with comparable efficiency when compared to F under certain circumstances: the exposure control only condition, long tests, or a stringent security criterion.

Many adaptive tests use a method of content balancing within the item selection algorithm to account for topics to be covered within the test. Davey (2005) presented an alternative to this method—bin-structured item selection. This process requires first constructing equivalence classes, or item bins. These bins contain items that meet the same criteria for content constraints and thus can be interchanged with other items in the bin to meet requirements of content coverage without over exposure. Then, the test is designed by selecting one item from each bin. The number of bins necessary is equal to the number of each type of item that fulfills each desired item type.

Davey (2005) outlined five advantages to the bin-structured design over typical CAT designs. (1) Bin-structured pools are simple and straightforward. (2) Test sequencing can be constant across examinees. (3) Bins do not interact with one another; thus, item enemies can be put in the same bin and then do not become a problem in test administration. (4) Exposure rates can be made analytically rather than empirically. (5) Finally, bin-structured pools allow for simplified item pool development, as a result of more predictable test outcomes due to the above-mentioned advantages. If built correctly, the bin-structured design allows for a more appropriate and balanced sampling of the domain. Thus, with this sort of design, items are chosen from within previously decided upon bins, but are still adapted to the ability level of the examinee.

2.2.3.2 Practical issues within CAT. The previous section outlines some studies that have been completed examining various technical issues with computer adaptive testing. Now, I will outline some studies that have been done on the more practical side of computer adaptive testing, such as scale stability over time, racial bias within testing, and the effects of guessing on test scores.

One of the issues in any large scale testing program is that of scale stability over time. Guo and Wang (2003) conducted a study using both real data and simulated data to detect scale drift that might result from errors in item calibration and parameter scaling procedures over time in CAT. If the scale does drift over time, the original interpretation of scores may become invalid. In the real data portion of their study, Guo and Wang found little drift from the first administration to the

second when looking at the ICCs, implying little change in item performance between the two time points. When looking at the items' RMSDs, they found very little change in performance over a 20-month period. At the test level, they did, however, find the test to be slightly harder at the second time point, though this difference was not significant. Guo and Wang had similar findings from the simulation portion of their study. Their simulated sample of examinees scored slightly lower at the second time point suggesting the first test was harder. They found slight negative bias from time point 1 to time point 2.

In 2003, Freedle argued that the SAT's known bias toward white males could be overcome by scoring only the harder questions on the test. His theory followed the controversial idea that while, African Americans and other minorities tend to score lower overall on high-stakes tests, such as the SAT, they tend to score equal to, if not better than, their White counterparts on the more difficult questions on these tests. Bridgeman and Burton (2005) challenged this claim through a real data study of the SAT. They identified a sample of students who took the CAT SAT in May of their junior year of high school and then again in November of their senior year. When looking at the students who scored in the 200-400 range (out of 800) on the verbal section, and then only scoring the hard items on the test (which they called the SAT Hard Test), they found a test-retest reliability of only .07, suggesting that low scoring examinees ability on harder items is a product of random guessing. They also looked at the rate of minority students who scored above a 600 on the regular test and those

who scored above a 600 on the Hard Test. The number of students scoring above 600 only increased by less than 1% when scoring the Hard Test only.

Bridgeman and Cline (2004) conducted a study that examined the impact of guessing on the final items of a CAT due to time constraints. In their study, they inspected the results from examinees who took the Graduate Record Examination Analytical section (GRE-A). They found that not only did examinees whose tests ended with a string of guesses result in lower test scores for those examinees, but that examinees with higher ability levels tended to receive items which required more time to complete. This forced these examinees to guess on their final questions, resulting in lower scores. Thus, examinees who received more time-consuming items, earned lower scores than examinees with equal ability levels who received less time-consuming items. They found that taking the examinees estimated ability level after 29 items was more accurate than accounting for all 35 items that were administered on the test, thus reducing the effect of guessing on the final items due to time constraints. They found that 3.4 to 34% of examinees had over a 0.5 drop in their theta level due to guessing on the last 6 questions. This suggests that these additional items "add more noise than signal (p. 144)" to the estimate of theta for examinees.

2.2.3.3 Research and development of the CAT-ASVAB. Some of the most notable research done in the field of computerized adaptive testing was conducted by the United States military beginning in the late 1970's. A series of studies, both empirical and simulated will be discussed now.

In the early 1980's, the first adaptive tests were administered to military recruits. McBride and Martin (1983) conducted an empirical study to test the feasibility of CAT-ASVAB in the recruit population, and to substantiate the theoretical advantages of adaptive testing. In their study, recruits were given either a 50-item conventional ASVAB test, or a 30-item adaptive test, which used a Bayesian sequential adaptive testing procedure to select subsequent items. After receiving their respective tests, recruits were also administered a criterion measure. McBride and Martin found that the adaptive tests had considerably higher alternate forms reliability when the only looking at the first 5 to 20 items administered. At the longer test lengths, the difference in reliability was reduced dramatically, but the adaptive tests still had slightly higher reliability.

Moreno and colleagues (1983) conducted a study comparing a battery of adaptive tests to their conventional ASVAB counterparts. In this empirical study of over 300 Marine Corps recruits, a factor analysis of the data demonstrated that all three tests in the CAT battery behaved the same as those in their conventional ASVAB counterparts. The loadings on the CAT tests were almost identical to the loadings on the factors derived from the P&P conventional version (as cited in McBride, 2001c).

McBride, Wetzel and Hetter (2001) summarized a series of simulation studies that looked at the role of the size and characteristics of the item banks as well as the distribution of ability for choosing the best item selection strategy. In the first study, they evaluated whether the mathematical strategies maintained their superiority in

conditions characterized by realistic errors in item parameter estimates. In this study, data for a conventional peaked test, Bayesian sequential algorithm, a hybrid Bayesian algorithm and the stratadaptive procedure were compared on the test information provided at each level of ability. The Bayesian hybrid algorithm utilized the Bayesian estimation procedure to estimate an ability score after each item, but the next item was chosen by selecting an item that best matched this ability level from an Information Table of items (a method discussed in an earlier section). Each simulated test was 15 items long. The difference between the two Bayesian methods was minimal at all levels of ability. At the lower and higher ability levels, the TIF was much greater for every adaptive strategy when compared with the conventional peaked test.

The second study in this series compared refinements to enhance test security. As mentioned previously, test security could be breached if examinees learned the appropriate responses to items resulting in the same patterns of item administration, thus inflating test scores. Avoiding this predictability of item sequence, especially in the earlier items, might help to remedy this situation. Because of the results of the first study, the testing algorithm chosen for this study was the Bayesian hybrid algorithm. For this study, five fixed-set size item selection procedures (1, 5, 10, 20, and 40 items), and two shrinking set size procedures ("5-4-3-2-1" and "10-8-6-4-2") were compared. Again, the TIFs for each test type were compared. Though the TIF patterns across the entire range of ability levels were similar; the 40-item, fixed-set size resulted in the lowest TIF, followed by the 20-item, fixed-set size, and then the

10-item, fixed-set size. The shrinking set sizes and 5-item and 1-item, fixed set sizes, resulted in almost identical TIFs.

The third study summarized by McBride, Wetzel and Hetter (2001) examined the differences between fixed- and variable length tests. In this study, they compared the final test lengths of two variable-length tests stopped with a stopping criterion of a Bayes posterior variance equal to .0638 and .0526 to a fixed-length test with 15 items over a simulated ability level range between -2.25 and +2.25. They found that the more rigorous stopping rule (.0526) resulted in an average test length between 10 and 30. This stopping rule resulted in an average test length of less than 15 for ability levels from -2.25 to +1.75, with a steep increase in test length for higher ability levels. The less rigorous stopping criterion (.0638) had an average test length between 10 and 30 as well. However, for ability levels below +0.25, the average test length was around 18; for ability levels between +0.25 and +1.75, the average test length dropped below that of the 15-item fixed-length test; and for ability levels above +1.75, the test length increased dramatically as ability level increased.

Overall the studies conducted by the CAT-ASVAB R&D program reached the following general conclusions. First, mathematically complex strategies are more reliable and efficient than simpler, mechanical strategies. Second, the Bayesian sequential and maximum likelihood procedures are equally efficient, but each comes with its own technical problems. Third, these technical problems can be overcome using a hybrid technique combining the best properties of both. Fourth, this hybrid technique has its own issues when dealing with test security. Thus, the shrinking-set

size procedure should be used in conjunction with this hybrid strategy. Finally, the variable-length test yields the same efficiency as a fixed-length test overall, and is not overly advantageous. Thus, a fixed-length test can be used (McBride, Wetzel, & Hetter, 2001).

Another test that has been converted into a CAT format is the Abstract Reasoning Test (ART, Embretson, 1995, 1998). The next section will discuss this test in further detail.

2.3 Abstract Reasoning Test

The Abstract Reasoning Test (ART, Embretson, 1995, 1998) was developed as a non-verbal measure of reasoning ability. The test is similar in form to the Raven's Advanced Progressive Matrices (RAPM, Raven, 1962, 1976) test, which is considered one of the best measures of general intelligence (Prabhakaran et al., 1997; Carpenter, Just, & Shell, 1990; Burke, 1958). Before getting into the details of the ART, I will define intelligence and introduce other similar matrices tests.

2.3.1 What is intelligence?

Since the early 1900's, measuring the construct known as intelligence has become a major focus for some researchers. Even as far back as the philosophers of ancient Greece, people have asked the questions: What is intelligence? How can a person become more intelligent? And, finally, how can we measure the level of a person's intelligence? Without a strong definition of the construct, we can hardly begin attempting to develop measurements of intelligence. Thus, we must first look at the definitions of this construct.

Some researchers have defined intelligence unidimensionally. Thorndike and colleagues (1927) defined intelligence as the ability to give responses that are true or factual. Colvin (1921), Pintner (1927), and Thurstone (1938) all defined intelligence in terms of an ability to adapt to new information or new experiences in a productive manner. Henmon (1921), Woodrow (1921), and Dearborne (1921) defined intelligence as the ability to acquire information or the capacity to which a person could acquire information. Terman (1916) defined intelligence in terms of the ability to carry on abstract thinking. Finally, Raven defined intelligence as the ability to reason by analogy from awareness of relations between experienced characters (Burke, 1958).

Other definitions of intelligence have taken a more factorial approach. Spearman's (1927) factorial model of intelligence gave way to a hierarchy of factors. At the top stratum, Stratum I, is general intelligence, with the following specific factors at Stratum II: fluid intelligence, crystallized intelligence, general memory and learning, broad visual perception, broad auditory perception, broad retrieval ability, broad cognitive speededness, and processing speed. Each of these factors in Stratum II is divided into even more specific abilities. Thurstone (1938) divided intelligence into seven "primary mental abilities": verbal comprehension, verbal fluency, numerical reasoning, spatial visualization, memory, reasoning, and perceptual speed. Guilford (1982) proposed that there are 150 possible factors to human intelligence—each composed of some degree of each of three categories: operation, content, and product.

In 1936, Raven set out to design a test to measure such a construct. His purpose was to develop a test that was easy to administer and interpret and was not dependent upon language development. The result, the Raven's Progressive Matrices (RPM) scale was developed to measure eductive ability, which is one of the two main components of general cognitive ability, according to Spearman (1927). Eductive ability is the ability to make meaning out of confusion, to generate high-level schemata making it easy to handle complexity. This ability is at the center of Guttman's radex of intelligence and has been found to mediate between verbal, numerical, and spatial abilities (Raven, 2000). Other tests similar to the RPM and Raven's Advanced Progressive Matrices (RAPM) have been designed to measure this non-verbal reasoning ability (e.g., Naglieri & Das, 1997; ART, Embretson, 1995, 1998).

2.3.2 Measures of Intelligence

2.3.2.1 Raven's Progressive Matrices. Most researchers agree, RPM is considered to be one of the best measures of general cognitive ability (*g*). It can be administered to large numbers of people and used for guidance purposes, clinical practice, and many research settings (Ward & Fitzpatrick, 1973). Due to its nonverbal attribute, it is also considered to be one of the only measures of *g* with a low level of culture-loading (Arthur et al., 1999). The RPM scale correlates between .4 and .7 with other measures of intelligence, and occurs as an independent variable or covariate in multiple experimental studies of cognitive ability (Hunt, 1974).

Hornke and Habon (1986) attempted to identify rules that could be used to solve items from the RPM. They devised the following rules: identity, addition, subtraction, intersection, unique addition, seriation, variation of closed Gestalts, variation of open Gestalts, separated components, integrated components and embedded components. They used the rules to build a pool of items similar to the RPM item pool.

Carpenter and colleagues (1990) simplified Hornke and Habon's rules into five that could be used, together or separately, to solve most RPM items. Those rules include: identity, progression, figure addition or subtraction, distribution of 3 and distribution of 2. The first rule, identity, is defined by having a constant object or attribute across a row and/or a column (see Figure 2.3a). The second rule, progression, occurs when there is an attributal increment or decrement (change in size, number, shading, position, etc.) occurring between adjacent entries across a row or down a column (see Figure 2.3b). Figure addition or subtraction occurs when a figure from the first entry is either added to or subtracted from an adjacent figure to make the third figure in the row or column (see Figure 2.3c). Distribution of 3 exists when three values from a categorical attribute (figure type, fill color, etc.) are distributed throughout every row and column (see Figure 2.3d). The final rule, distribution of 2, occurs when one value from a categorical attribute is found twice in a row and the third value is null (see Figure 2.3e).

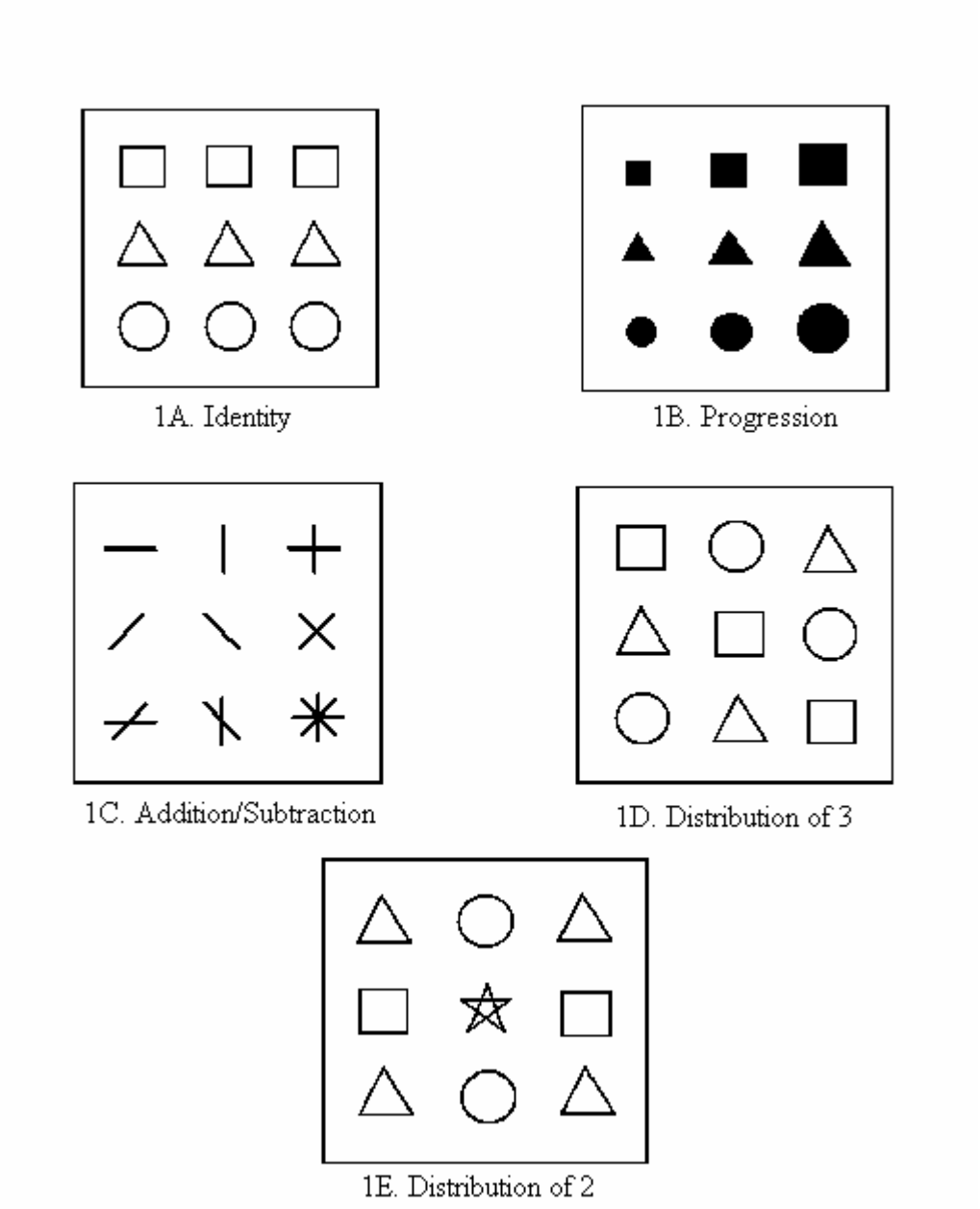


Figure 2.3. Examples of each of the five rules for solving RPM test items.

In a study of performance on items of this type, Carpenter and colleagues (1990) found that processing of these items was an incremental task. They also found that error rates for individuals were affected by the number of rules necessary to solve an item. But, their results were unable to explain the wide variety of individual differences in performance on this test. In this same study, they found a relationship of $r(43) = .77$, $p < .01$, between error rates on the RPM and error rates on the Tower of Hanoi puzzle. The errors on both were related to the load on working memory to solve the particular item or puzzle. A larger number of errors were made on items involving multiple rules or multiple occurrences of the same rule on the RPM. Additionally, a larger number of errors were made when there were more moves involved in solving the puzzle. This research suggests that not only is the RPM a measure of general intelligence or cognitive ability, but it is also a measure of the ability to generate and maintain goals in working memory. Based on their findings, they proposed two possible sources of individual differences: working memory and abstraction complexity.

While Carpenter and colleagues were unable to find a definitive source of the individual differences exhibited in their study, Verguts and De Boeck (2002) proposed two factors of individual differences in problem solving ability on the RPM. The first factor was the number of rules the examinee has cognitively available at any point in the test. The second factor was the working memory load, or number of sub-routines the examinee can store simultaneously. Rules used early in the test were usually found easily by the examinee and were then available for the rest of the test.

The test is commonly setup to become progressively more difficult, with the harder rules, more of the rules per item, and more abstract use of the rules, as the examinee nears the end of the test. Verguts and De Boeck found that learning effects played a role in solving problems of this type, that is, the more a person encounters a particular rule the easier problems with those rules become.

2.3.2.2 Abstract Reasoning Test. Rather than applying these rules to already designed tests, Embretson (1998) was able to generate a bank of matrix items (she called Abstract Reasoning items) through cognitive design system theories using Carpenter and colleagues' research on these tests. The cognitive design system approach uses both conceptual and procedural information as a framework for interfacing item design principles with test validity. She utilized this method to design items similar to those found on the RPM using the five rules defined by Carpenter and colleagues. Embretson found evidence to support the proposed sources of individual differences, working memory load and abstraction complexity. Embretson designed her item pool to maximize the use of rules individually as well as in combination with other rules. As a result, she found that working memory load was a function of the number of rules necessary to complete the item. She also found that the abstraction complexity load stems from relationships involving null values (distribution of 2 rule), relationships based on attributes rather than objects, and the distortion of corresponding objects.

2.4 Conclusion

In this review of the literature, it is apparent that computerized adaptive testing has been a major emphasis in testing and measurement research, especially since the late 1970's. While multiple research projects examining issues in CAT were summarized in this paper, the most notable research was conducted through the United States military that produced the largest working CAT in use today. As well, many ground-breaking findings were brought about through this research. These findings should be put to use when developing a computerized adaptive testing program in any field (be it educational, psychological, or medical). This body of research also lends itself to many more questions that need to be answered in future CAT research endeavors.

The purpose of this study was to look at the affects of test taking strategies on outcome scores in computer adaptive tests. To examine this issue, two studies were conducted. The first was an empirical study looking at real world differences that might occur due to instructional differences. The second study was a series of simulation studies that looked at the stability of testing algorithms at different ability levels over different test lengths.

3. Study 1

In Study 1, differences in trait estimates due to varying levels of test information given prior to an examinee was examined by administering a computer adaptive test with three different sets of instructions. It could be hypothesized that the amount of information an examinee has on how a test is scored and administered should improve their performance on the test. Also, it could be hypothesized that an increased amount of information on how an examinee could improve his/her score should result in a higher score.

Schaeffer and colleagues (1998) conducted a study that demonstrated that the test taking strategy described in Chapter 2 (spending more time on and attention on the first items) worked on the earlier scoring methods used by the CAT-GRE. Under what was called the 80% scoring method, scores were based only on the first 80% of the test so examinees were not punished for not finishing the test. This resulted in inflated estimates for examinees who spent more time on the beginning items. As a result, the CAT-GRE was changed to use the proportional scoring method which allows for not finishing the test to factor into an examinee's final estimate.

Other research has shown that an examinee's score on a test is a result of not only the ability level of that examinee on the content of the test, but also motivation and anxiety levels. Powers (2001) compared test anxiety and confidence differences between the GRE-CAT and the traditional P&P GRE. He found no significant difference between the two testing methods on multiple anxiety and confidence measures. Vispoel (1998) found that feedback on a test results in better scores for

examinees with higher levels of test anxiety. Shermis and Lombard (1998) found that test anxiety significantly lowered performance on a computer-based math and reading placement test. Kim and McLean (1995) found that test motivation reduced test anxiety and improved estimate ability.

In this study, the impact of instructions about the testing procedure and about how to improve scores was examined, using an experimental procedure. Examinees were randomly assigned to a control condition (i.e., standard instructions), a condition with additional testing-procedure information, or a condition with additional test-taking strategy information. Differences in final theta estimates were compared between the three groups, taking into account possible confounding effects of prior CAT experience, to test the two hypotheses outlined above. Due to the nature of a computer adaptive test, another hypothesis that was tested was that there would be no significant difference in proportion of items answered correctly between the conditions. Finally, it was hypothesized that the more information an examinee is given prior to the test, the longer it will take them to read the instructions.

3.1 Methods

3.1.1 Participants

Participants were college students participating for credit in an introduction to psychology course at a large Midwestern university. Because the population of interest is college age adults who would be likely to take a high-stakes tests (for admission to college, or graduate school, or other post graduate programs), the participant pool was expanded to include students from junior level classes (statistics

in psychology, upper-level English and linguistics classes). Students from these classes participated for extra credit in their respective courses. The participants were randomly assigned to each of three conditions ($n_1 = 71$, $n_2 = 64$, $n_3 = 71$).

3.1.2 Materials and Apparatus

3.1.2.1 Item Pool. A pool of Abstract Reasoning Test (Section 1.3) items was used for this study. The item pool consisted of 150 items, 30 sets of 5 items each. Each set of items was generated following a design template that designated which and how many of each rule would be used to create the item set. As discussed in the previous chapter, these rules include Identity, Addition/Subtraction, Progression, Distribution of 3, and Distribution of 2. The items were randomly generated based on these designated templates. This resulted in items with difficulties ranging from -3.39 to +3.54. Each item was a 3 x 3 matrix, with 1 correct answer and 7 distractors, similar to the example in Figure 3.1.

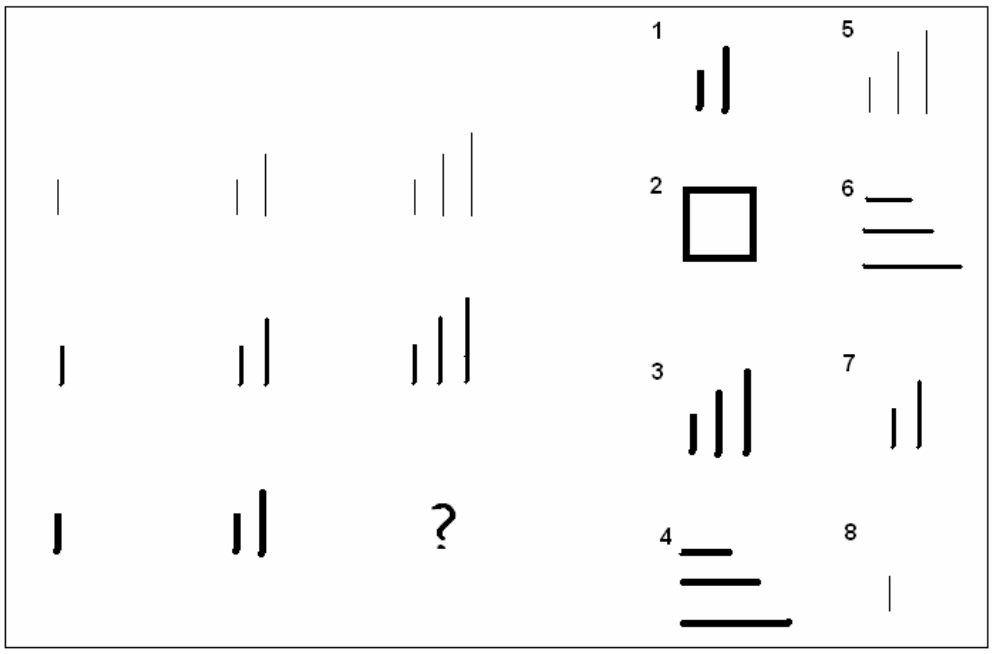


Figure 3.1. Example Abstract Reasoning Test item similar to those created for Study 1. The correct answer is 3.

3.1.2.2 Testing Program. A computer adaptive test version of ART was developed using a trial version of FastTEST Professional version 1.6 (Assessment Systems Corporation, 2002). This program allowed for many testing algorithm choices. Based on these choices, the design of the test used is described next.

For this study, Maximum Likelihood Estimation (discussed in the previous chapter) was used with a 1-PL model. Each examinee received 25 items and had a 35-minute time limit to complete the test. Only one participant did not complete the test in the allotted time. Through pilot testing, it was found that assuming an average

initial theta value resulted in the administration of the same first item for all examinees. Though test security issues were not of interest for this study, they are very important in the field of testing. Thus, the program was set to randomly choose an initial theta value between -1.00 and 1.00. The fixed set size starting rule discussed in the previous chapter was used to help deal with over item exposure rates for test security purposes. For the first 10 items of the test, the item administered was selected randomly from the 10 items that gave the most information at the current estimate of theta.

Items were displayed individually on the screen. Participants were not allowed to return to items they had already answered, and they were forced to choose an answer before moving on to the next item. The mouse was used to select their answer and to move on to the next item (as discussed in more detail later). Instructions and other pertinent test information were presented prior to the items, and a thank you screen was presented when the test was completed. The amount of time left on the test was visible on the computer monitor, however the item number was not. The exact procedure and instructions are discussed next.

3.1.2.3 Instructions. Every participant received the same adaptive test (though the items administered varied based on ability level and response pattern). The only difference between the three conditions was the instructions the examinee received. The entire set of instructions, including example problems and the rules are available in Appendix A. Each of the instructions particular to a specific condition are outlined next.

Participants in Condition 1 were told only that they were taking a computer adaptive abstract reasoning test: "This is a computer adaptive test that measures your ability to reason on a nonverbal matrix completion task… You will not be allowed to skip any items or return to any items you have already answered."

Participants in Condition 2 were told that they were taking a CAT-ART, and were briefly told how a CAT procedure adapts to the test takers performance. These instructions were as follows:

> "This is a computer adaptive test that measures your ability to reason on a nonverbal matrix completion task… A computer adaptive test "adapts" itself to test takers by selecting the next item to be presented on the basis of performance on preceding items. This means that each item you receive on the test is chosen from a large number of items and the choice is made based on whether you answered previous questions correctly or incorrectly. This also means that the items you receive may not be the same items or they may not be given to you in the same order as others taking this test. Because of the nature of this test, you will not be allowed to skip any items or return to any items you have already answered."

Participants in Condition 3 received the longest set of instructions. These instructions included the same explanation about how a CAT works (the paragraph above), but in addition the test taking strategy as taught by Kaplan for "beating" the computer adaptive GRE was explained as follows:

> "One test taking strategy that you should keep in mind when taking this test is as follows... Because of the nature of a computer adaptive test, you will want to spend time and concentration on the first ten questions of the test. The reason for this is that a computer adaptive test relies heavily on the first ten questions in determining your score. This is because the computer knows nothing about your ability before you start the test. Because of the short length of this test, the needs to use pretty big jumps in judgment in the first ten questions and then use the remaining questions to "fine-tune" your score."

As stated above, the three conditions only differ in the extent to which a CAT is explained. What is shown here is only the parts that differ, the rest of the instructions can be seen in Appendix A.

3.1.3 Procedure

The ART-CAT was installed on the computers in a free-use computer lab. All three conditions were run during each time slot and participants were randomly assigned to each condition. This was done by rotating which computers had which conditions and allowing the subjects to select the computer they took the test on. Because the log in screen looked alike for all conditions, the participants were unaware that there were multiple conditions to select from.

When the participants arrived, they were asked to have a seat at any computer, to read and sign a consent form, and to log in to the program on the computer. They were then given a brief description of the study, including a statement that the computer would present the test instructions. They were also told that they could

summon the researcher if they had any questions before starting the test and that they should also summon the researcher when the test was complete.

Then, the participants logged in to the computer (with their name and ID number), the computer displayed the instructions page corresponding to their test condition. At the bottom of the instructions page, they had to use the mouse to click on the button stating that "by clicking this button you have given their consent to participate in this study." Then, the participant needed to hit "Next" to move to the next screen. The next two screens presented the following two yes/no questions:

(1) Have you ever taken a computer adaptive test before?

(2) Have you ever taken a class (e.g., Kaplan, Princeton Review, etc.) to help prepare for a computer adaptive test?

This provided information as to whether the participants had already been exposed to these test taking strategies. This information is especially important for the participants in Conditions 1 and 2 who did not receive instructions that included these test taking strategies.

Once the participants answered those questions, they were shown an example ART item. Next, they received a demonstration of each of the five rules and then they were shown the original example and asked to attempt to solve it. After attempting to solve the example item, they were shown the correct answer and how to apply the rules to that particular item. The reason for teaching the rules prior to the test was done to equalize the participants on the familiarity of knowledge necessary to succeed on the test. This is consistent with the high-stakes tests of interest, which measure

knowledge that the test takers should either possess or at least be familiar with through previous coursework or studying. These slides are presented in Appendix A.

Once they finished the instruction slides, the test was started. The clock on the screen was set for 35 minutes, and began counting down when the first item was on screen. They received 25 items. Each item appeared individually on the screen. The examinees used the left mouse button to select the alternative they believed to be correct, and then they used the left mouse button to select the "Next" button to move on to the next item.

Once they reached the end of the test, a thank you screen appeared. After which, they received their respective class credit (participation credit for introductory psychology courses, and extra credit for the other courses).

3.2 Results

Analyses were conducted to determine if there were differences between the three conditions in overall performance on the CAT. Multiple versions of this outcome measure (MLE theta estimate, CTT percent correct, etc.) were investigated. To examine whether the instructions were read and/or followed, the amount of time spent on the instructions, the scored items, and the entire test were considered as outcome measures in these analyses. Unfortunately, due to the nature of the trial version of the testing program used, individual response time data per item were not available. The independent variable for these analyses was condition. Possible covariates examined were whether or not participants had attended a class that

covered how a CAT works and whether participants had taken a CAT. Due to the small sample size in this study, a more lenient alpha level ($\alpha = .10$) was used.

3.2.1 Descriptive Statistics

Prior to testing the hypotheses, descriptive statistics were run for the entire group of participants, and within each condition. These statistics can be seen in Table 3.1. No variables were found to be significantly skewed or kurtotic. However, under the dependent variable, MLE theta, one outlier in each condition was found and deleted from the samples. Sample size slightly varied between groups, due to the randomization procedure.

As can be seen in Table 3.1, the three conditions differed little in proportion correct scores. This was to be expected with a CAT due to the nature of the procedure. It should also be noted that there was no noticeable difference in standard errors of the theta estimate. This suggests that the length of the test was appropriate to ensure stable measurement precision across conditions. These standard errors are similar to the resulting root mean squared error found at the end of the thirty-five item test simulated in Study 2.

As expected by the design of the study, it can be seen in Table 3.1 that the average amount of time spent on the instructions for the study differed little between Condition 1 and Condition 2. However, participants in Condition 3, on average, spent more time reading the instructions. This is consistent with the differences in length of the instructions between the conditions. This same pattern was seen when looking at

the amount of time spent answering scored items (that is, taking the actual computer adaptive ART). Tests for significant differences are discussed in the next section.

A chi-square test of independence was conducted to examine the presence of participants within each condition who might have had prior exposure to computer adaptive testing. The breakdown within each question by condition is displayed in Table 3.2. As can be seen in this table, very few people had previous exposure to a CAT. The proportions of participants in each group are virtually equal across conditions. Also, neither of these variables was found to be significant covariates for the outcome variables of interest with p-values ranging between .427 and .975. As a result, they were left out of further analyses. However, it should be noted that given the small sample sizes, the true impact of these possible covariates may not have been uncovered.

Table 3.1

Descriptive statistics for Study 1 (after removing outliers)

		Condition		
DV	Statistic	1 (N = 70)	2 (N = 63)	3 (N = 70)
Proportion correct				
	Mean	.547	.529	.562
	(SD)	(.138)	(.129)	(.144)
	Median	.560	.560	.560
	Minimum	.200	.200	.300
	Maximum	.800	.800	.800
MLE theta				
	Mean	.983	.863	1.204
	(SD)	(.958)	(.904)	(.838)
	Median	1.081	1.096	1.257
	Minimum	-.919	-1.556	-.462
	Maximum	2.932	2.445	3.268
MLE theta standard error				
	Mean	.263	.263	.270
	(SD)	(.017)	(.018)	(.027)
	Median	.256	.259	.261
	Minimum	.244	.242	.247
	Maximum	.314	.344	.392
Total test time				
(in minutes)	Mean	20.204	19.138	22.722
	(SD)	(7.871)	(6.683)	(7.909)
	Median	19.850	18.071	22.522
	Minimum	10.138	7.075	10.454
	Maximum	39.290	41.429	42.490
Time for scored items				
(in minutes)	Mean	16.454	15.368	18.416
	(SD)	(7.483)	(6.097)	(7.686)
	Median	15.736	13.663	18.061
	Minimum	6.463	4.982	6.635
	Maximum	35.461	35.390	35.471
Time for instructions				
(in minutes)	Mean	3.750	3.770	4.306
	(SD)	(1.245)	(1.072)	(1.100)
	Median	3.525	3.794	4.253
	Minimum	1.890	1.587	2.505
	Maximum	8.839	6.039	9.095

Table 3.2

Crosstabs analysis of the proportion of each sample (and number of participants from each sample) who answered yes/no to the questions regarding prior exposure to a CAT

		Condition		
Question	Answer	1 (N = 70)	2 (N = 63)	3 (N = 70)
Have you ever taken a computer adaptive test before?				
	Yes	.057 (4)	.079 (5)	.071 (5)
	No	.943 (66)	.921 (58)	.929 (65)
Have you ever taken a class (e.g., Kaplan, Princeton Review, etc.) to help prepare for a computer adaptive test?				
	Yes	.186 (13)	.222 (14)	.186 (13)
	No	.814 (57)	.778 (49)	.814 (57)

3.2.2 Inferential Statistics

Two hypotheses were outlined above: 1) the amount of information an examinee has on how a test is scored and administered should improve their performance on the test; and 2) an increased amount of information on how an examinee could improve his/her score should result in a higher score when compared to examinee's who only know how the test works. These hypotheses were examined for all dependent measures, proportion correct, MLE theta estimate, standard error of the estimate, time spent on the instructions and time spent on the scored items, through planned comparisons. The first compared Condition 1 (the control condition)

to Condition 2 and 3 combined, and the second compared Condition 2 (the how-a-test-works condition) to Condition 3 (the how-to-beat-the-test condition).

In examining the overall F-tests for each dependent variable, there were no significant differences between conditions in the percentage of items scored ($F(2, 200) = 1.027$, $p = .360$). There was a significant difference found for each of the other dependent variables: time spent reading the instructions ($F(2, 200) = 5.240$, $p = .006$, partial-$\eta^2 = .050$); MLE theta ($F(2, 200) = 2.472$, $p = .087$, partial-$\eta^2 = .024$); and time spent on scored items ($F(2, 200) = 3.128$, $p = .046$, partial-$\eta^2 = .030$). The results from the planned comparisons are discussed next.

Two comparisons were made: 1) Condition 1 to Condition 2 and 3 combined; and 2) Condition 2 to Condition 3. For each of the differences reported as significant, Cohen's d was calculated to give an estimate of effect size. Also, a 90% confidence interval was calculated and reported around this effect size. A summary of these findings can be seen in Table 3.3.

For Comparison 1, the only significant difference was found on time spent reading instructions. Examinees spent significantly more time reading the instructions in Conditions 2 and 3 than those in Condition 1. This is consistent with the length of the instructions. For Comparison 2, Condition 2 was found to result in significantly higher values than Condition 3 on all dependent variables except for proportion scored as correct. As discussed in the previous section, no differences are expected for proportion correct on a computer adaptive test. Also, as expected given the design of the test, examinees in Condition 3 spent more time reading the instructions than

those in Condition 2. The findings for this dependent variable suggest that the participants were reading the instructions. Also, participants who received information on how to "beat" the test spent more time on test items than those who were only told how the test works. Information on how to "beat" the test also resulted in higher estimates of theta and standard errors of theta than those who only received information on how the test works. This supports the claim that this test taking strategy does result in higher final estimates of ability.

Table 3.3

Planned comparison significance tests and effect sizes for each dependent variable.

DV	Comp.	M_D	SE_{MD}	T	df	p	d	d_{UB}	d_{LB}
Proportion Scored Correctly									
	1	-0.002	0.040	-0.061	200	0.952			
	2	0.034	0.023	1.433	200	0.153			
MLE Theta									
	1	0.101	0.266	0.378	200	0.706			
	2	0.341	0.156	2.178	200	0.031	0.366	0.192	0.539
Theta SE									
	1	0.006	0.006	1.034	200	0.302			
	2	0.008	0.004	2.111	200	0.036	0.052	-0.122	0.225
Time on scored items									
	1	0.876	2.115	0.414	200	0.679			
	2	3.049	1.243	2.453	200	0.015	1.158	0.984	1.331
Time on instructions									
	1	0.576	0.338	1.705	200	0.090	0.281	0.133	0.428
	2	0.536	0.199	2.698	200	0.008	0.514	0.340	0.688

3.3 Discussion

In this study, real-world implications of different levels of information about the testing procedure were examined. The goal of this study was to add to the literature on how the amount of knowledge an examinee has about the testing

procedure impacts the testing outcome. Examinees differed only in the information about the testing procedures, not the domain the test was measuring. Briefly, significant differences were found in the outcome measure of interest, especially between those who were told only how a computer adaptive test works, and those who were also given a strategy to improve their scores on a computer adaptive test. These findings are discussed below.

As expected, no significant difference was found between conditions in the proportion of items answered correctly. While this might seem contradictory to individuals who are unfamiliar with CAT principles, if there is a significant difference between any conditions on the theta estimate, proportion correct scores are still expected to be equal. An adaptive test tends to bounce back-and-forth between items a person can and cannot answer correctly to attempt to narrow in on the items of a difficulty level that a person would have a 50/50 chance of answering correctly. Thus, it is not surprising that, on average, the participants answered correctly slightly more than 50 percent of the items they were administered regardless of their condition.

In agreement with the design of the study, it was also found that the longer the instructions were the more time participants spent reading the instructions. These results also were expected due to the difference in length between the instructions for the conditions. Condition 2 was longer than Condition 1 by one paragraph and Condition 3 was longer than Condition 2 by one paragraph. These results suggest that the participants were reading the instructions prior to taking the test.

Significant differences were found in the final theta estimates between Condition 2, the testing procedure knowledge condition, and Condition 3, the test-taking strategy knowledge condition. As can be seen in Table 3.1, theta estimates for Condition 3 were higher than those for Condition 2. This suggests that extra knowledge about the test procedure somehow improves performance on a test. There are at least two plausible explanations for this effect. First, the differences could be due to motivational or anxiety issues, consistent with general findings on test anxiety (e.g., Powers, 2001; Vispoel, 1998; Shermis & Lombard, 1998; Cohen, 1998; Kim & McLean, 1995; etc.). That is, examinees may perform better on the exam because they feel more comfortable knowing a trick to "beat" the exam rather than assuming they will not do well. However, a second explanation is that the instructions changed the test-taking strategy, which then impacted CAT estimation. This is supported by the significantly greater amount of time spent on the test for the examinees in the test-taking strategy condition. That is, the particular CAT algorithm used in the study might lend itself to inflated theta estimates if performance is better on the first set of items than if performance was consistent across items.

Something to note from these findings is that Condition 1, the control condition, mean estimated theta fell in between Condition 2, the testing-procedure knowledge condition, and Condition 3, the test-taking strategy knowledge condition. This suggests that having knowledge about the testing program's procedure may, in fact, be detrimental to performance on the exam. Examinees in the first condition were told only that they were taking a computer adaptive test. Without any prior

knowledge, this could mean nothing to an examinee, and the information would be treated the same as if it were just part of the title of the exam. This suggests that there might be a motivational or emotional reaction affecting performance on this exam. Perhaps receiving information about how the procedure works, without receiving information about how to approach the procedure, increases anxiety, or decreases motivation. Elliot and Dweck (1988) found that children who received feedback about mistakes reacted in a learned helplessness manner. They believed that the mistake they had made implied a lack of ability. This is consistent with the findings of Study 1, in that students who are informed about how an adaptive test functions know that if they get an item wrong, they will get an easier item. In other words, if an examinee receives an easier item (especially early in the test were bigger jumps are made) he/she can assume that his/her answer on the previous item was incorrect. Thus, receiving an easier item could be interpreted by the examinee as proof of a lack of ability, resulting in less motivation for the remainder of the exam.

Overall, these findings are consistent with current literature on test anxiety and motivation. As discussed earlier in this chapter, Kim and McLean (1995) found that increased test motivation improved test scores. Shermis and Lombard (1998) found that lower anxiety scores were associated with better performance on both math and reading tests. Cohen (1998) found motivation to be associated with learning test taking strategies. Embretson (1992) found that strategy training improved performance on the spatial learning ability test (SLAT).

The pattern of response time differences between the conditions further supports this explanation. Small significant differences were found between Condition 2 and 3 in amount of time spent on scored items. No significant differences were found between these two conditions and the control condition. The amount of time spent on items in the control condition was one minute more than the testing procedure knowledge condition and two minutes less than the test-taking strategy knowledge condition. These differences could become more apparent with future studies involving more than 25 items. This also supports the motivation theory mentioned above that those with a particular strategy are actually more motivated to do the test than others. The examinees who were given instructions on how to beat the test, spent more time on the test as a whole than those who were just told how the test works. Those who were given no special instructions or information spent more time than those who were told how the test works, but less time than those who were told how to "beat" the test.

Vispoel (1998) conducted a study in which students were given answer correctness feedback after each item. He found that higher test anxiety resulted in lower final theta estimates. He also found that when participants were given answer feedback, they took significantly less time on the test than when they were not given answer feedback. This is consistent with the findings of this study. Those who were in the testing procedure knowledge condition knew that if they answered incorrectly they would receive an easier item, and vice-versa. Those participants took less time overall on the test than those who had no information to this effect. While the

participants in the third condition received this same information, they were also told to spend more time on the items, which should counteract the answer feedback effect found by Vispoel (1998).

Again, power was relatively low for this study (ranging from .17 to .48). Future studies should use larger sample sizes to see if these findings are representative of the testing taking population as a whole. Also, future studies should add a fourth condition where participants are given the strategies for beating the test without being told the procedure of an adaptive test to distinguish any interactions between these two pieces of information. Also, future studies should use longer tests to see if those time differences become larger.

How instructions impact examinee behavior and performance on a test were examined here in Study 1. In Study 2, the testing algorithm and the claim made by these study guides as to how the tests work were examined. The results from the set of simulation studies will help to determine which testing algorithm might be more susceptible to these strategies. If certain testing algorithms are found to be more stable earlier during the course of the exam, these algorithms might be more preferable to developers of high stakes tests. These studies might also add to the literature on the length of a test necessary to ensure accurate estimation of a person's ability level.

4. Study 2

In the first study, differences were found between examinees who received information on a test-taking strategy that should increase a person's ability estimate. These differences help support the claim of the efficacy of this test-taking strategy. However, the first study was limited to only one testing procedure and the 1-PL model. Also, it was limited by the fact that true ability estimates were unknown, thus making a true causal inference was impossible. A simulation study was conducted next to overcome these limitations.

In this second study, the impact of an artificial boost on ability estimates, such as those obtained from adaptive test-taking strategies, was examined using simulation methods. Trait estimates were compared at different stages of the test from a selected set of adaptive testing algorithms. It was hypothesized that less stable testing algorithms would result in inaccurate estimates of ability level. If the test is less stable earlier on, then it should be easier to increase your score earlier in the test (i.e., there is more movement up or down in the theta estimate). Kaplan and other test review companies inform examinees that less stable estimates earlier in the test provide an opportunity to increase scores at this point if extra effort is allocated. As the test continues and stability in the estimate increases, it will be harder for the estimate to decrease. Thus, if this test-taking strategy works, the final estimate of an ability level should be inflated. If evidence is found to support this hypothesis, administrators making decisions for admissions, licensure and/or scholarships based on test scores may have to re-examine their criteria. To test this hypothesis, a series of simulation

studies were conducted varying the starting rules to examine the stability of the theta estimate at different test lengths.

4.1 Methods

For this portion of the study, multiple conditions were tested using a program written to simulate a computer adaptive test. There were multiple components in this study. The first component was the ability boost for the first ten items that varied in the population of examinees. The second component was the composition of the item bank, with varied size and item difficulty parameters. The 3-PL model was used with specified discrimination and guessing parameters. Finally, the third component of the study consisted of varying starting rules. Each of these components will be discussed further in later sections.

The outcome measure of interest is the stability and accuracy with which the particular testing algorithm estimates true theta at each point in the test. To examine this, each condition was replicated over 1000 examinees with true theta values at each of 29 intervals of theta values from -3.5 to +3.5 (interval size = .25) and random boost levels (discussed later in Section 4.1.5.2). This resulted in N = 29,000 examinees under each of the 288 conditions discussed later. These conditions will be referred to as "boost" conditions. To examine whether this stability is significantly different from conditions with no artificial boost, these 288 conditions were replicated over 10 examinees with true theta values at each of the same 29 intervals (N = 290). These conditions will be referred to as "null" conditions. Fewer replications were used for

the null conditions because there was less variability in response patterns when all simulated examinees at a particular theta level had equal boosts of zero.

4.1.1 Materials and Apparatus

As stated above, a computer program was adapted from a program written by Xiangdong Yang that would simulate a computer adaptive test. This program was written in C++ to interactively compute trait level (using MLE) and its standard error and then select an item from the bank that provides maximum information at the current estimated trait level. This is done by calculating an index for all items. This index is the difference between an item's difficulty level and the target difficulty level (see Equation 4.1). The target difficulty level is a function of the current theta estimate and the item's guessing and discrimination parameters (see Equation 4.2). The item with the smallest index value (called Minimum Index) is administered. These equations were as follows:

$$Index = b_i - b_{target}, \qquad \text{(Eq. 4.1)}$$

$$b_{\text{target}} = \theta_{\text{current}} - \log\left(\frac{1 + \sqrt{1 + 8c_i} \big/ 2}{1.7a_i} \right), \qquad \text{(Eq. 4.2)}$$

where b_i = difficulty parameter,

θ_{current} = current estimate of theta,

c_i = guessing parameter, and

a_i = discrimination parameter.

A bank of items was generated through the program by first designating the size of the bank and whether the items were randomly generated or set to constant values. The item bank size could be designated in this program (see Section 4.1.5.1). The parameters could be randomly generated or set constant for all items. For this study, the difficulty parameter was randomly generated by the program while the other two parameters were set constant depending on the condition (see Section 4.1.5.1). The starting rule (see Section 4.1.2) and stopping rule (see Section 4.1.4) could also be designated in this program. The program was written to output the theta estimate and standard error for each item administered. The item pool parameters were also outputted.

4.1.2 Starting rules

As discussed in Chapter 2, the first step in developing a CAT is to decide how to start the test. There are many options for this starting rule. For this study, many of these possibilities were examined to see which starting rule would provide more stable estimates throughout the test. For this study, it was assumed that there was no prior information about the examinee.

The first starting rule with no prior information discussed in Chapter 2 was to begin with an item of average difficulty level, or an item selected randomly from a range of difficulty levels. The first three conditions fell into this category, which will be referred to as "Random" for this study. The first condition, "Random, b=0," involved choosing the first item as one with a difficulty level close to or equal to 0. The second condition, "Random, -.5<b<.5," involved randomly choosing the first

item from all items with difficulty levels between -0.5 and +0.5 (as suggested by Embretson and Reise, 2000). The third condition, "Random, -1<b<1," in this set involved randomly selecting the first item from all items with difficulty levels ranging from -1.0 to +1.0. For these three conditions, the second item will be selected as the "best" item based on the MLE estimation procedure used.

The second set of starting rules involved choosing items randomly from a certain number of items that are considered best estimates, and continuing this process for the first so many items of the test. This type of starting rule is used to help ensure test security by reducing the exposure of the first items used. For this set of conditions, both the fixed-set size ("Fixed") and shrinking-set size ("Shrinking") procedures were looked at, as summarized by McBride, Wetzel, and Hetter (2001). For the fixed-set size procedure, three fixed-set sizes (10, 20, or 30 items) were examined over the first five items ("Fixed, 10," "Fixed, 20," and "Fixed, 30," respectively). In other words, the best fitting item was selected randomly from a certain number of items that all provide close to maximum information at the current estimate of theta for the first five items on the test.

The second version of this starting rule used was the shrinking-set size procedure. In this procedure, the first item was selected randomly from a designated number of items at the current estimate of theta. The second item was selected randomly from a smaller designated number of items at the current estimate of theta. The third item was selected randomly from an even smaller number of items at the current theta estimate, and so on. For this set of conditions, two different shrinking-

set sizes were examined: the first item is selected from 20 items, then reducing the set size by 4 for each consecutive item ("Shrinking, 20 to 4"); and the first item is selected from 10 items, then reducing the set size by 2 for each consecutive item ("Shrinking, 10 to 2"). Once the set size is reduced to one item, the continuation rule selects the best item at the current theta estimate. The shrinking set size condition has one advantage over the fixed set size, as discussed in Chapter 2. When an item is selected from a set of items that provide close to maximum information, rather than choosing the item that provides the most information, there is a loss of precision in the estimate. By using a shrinking set size, more and more precision is gained with each succeeding item.

A third set of starting rules examined was to give a constant, miniature linear fixed test at the beginning and then use an estimate based on all items in the initial fixed item test ("Linear"). This condition is similar to the starting rule described by Straetmans and Eggen (1998). There were three conditions under this starting rule. These conditions were based on the difficulty level (easy, easy-to-medium, or medium) of the items. Each of these linear fixed tests was 5 items long. For ease of programming, these conditions were created by picking a first item number and adding an increment of 5 to the item number each time to pick the next item. Because the item pool was ordered by item difficulty, the earlier items provided for easier linear tests.

For the 1000-item pool, the easy set consisted of item numbers 100, 105, 110, 115, and 120 ("Linear, easy"); the easy-to-medium set consisted of item numbers

250, 255, 260, 265, and 270 (“Linear, easy-to-medium”); and the medium level set consisted of item numbers 500, 505, 510, 515, and 520 (“Linear, medium”). For the 500-item pool, the easy set consisted of item numbers 25, 30, 35, 40, and 45; the easy-to-medium set consisted of item numbers 100, 105, 110, 115, and 120; and the medium set consisted of item numbers 200, 205, 210, 215, and 220. Because of the limited number of items in the 100-item pool, only an easy set and medium set were used. The easy set consisted of item numbers 5, 10, 15, 20, and 25, and the medium set consisted of item numbers 20, 25, 30, 35, and 40.

Considering that the test taking strategy under investigation involves the role of the first set of items, the majority of variation in this test was in the starting rule. There were 11 possible starting rules considered in this study.

4.1.3 Continuation rule

The continuation rule refers to the theta estimation algorithm used throughout the test. For this study, the Maximum Likelihood Estimation procedure was used. As mentioned in the earlier chapters, this procedure utilizes information from the previously answered items to estimate a theta level at any particular point in the exam. This MLE procedure was selected to stay consistent with Study 1.

4.1.4 Stopping rule

Because the outcome measure of this particular set of simulation studies is the precision of an estimate at different points in the test, the stopping rule was not varied. Rather, the stability and accuracy of the theta estimate was examined at every

fifth item up to 35 items. In other words, each condition was examined on each of three outcome measures (discussed below) at the 5th, 10th, 15th,…, and 35th item.

4.1.5 Data Generation

4.1.5.1 Item parameter generation. Consistent with computer adaptive tests, the item parameters were treated as known. The CAT program used simulated item data with difficulty levels varying from -4 to +4. The discrimination and guessing parameters were treated as constant across all items but were varied as an independent variable across conditions to see if these parameters would influence the stability of the test. Three levels of discrimination (a = 1.0, 1.5, and 2.0) and three levels of guessing (c = 0, .1, and .2) were used. This resulted in 9 item parameter conditions (3 a values x 3 c values). Finally, 3 item pools were generated differing in the size of the item pool (100, 500, and 1000 items). Ultimately, these variations resulted in 27 possible conditions times the 11 variations of starting rules. Thus, there were 291 minus the three easy-to-medium linear test conditions not simulated for the small item pool, resulting in 288 conditions overall.

4.1.5.2 Examinee parameter generation. To create the artificial boost in the examinees ability level for the first 10 items, a second theta level was randomly generated for each replication from a log-normal distribution ($M = 1$, $SD = .8$). A log-normal distribution was used because of its non-negative property. This theta level was then divided by two times the item number (for items 1 through 10) and added to the actual ability level estimated at each item to create a diminishing artificial boost in ability level. A diminishing boost was created to attempt to mimic what would

happen in the real world. It is unlikely that an examinee who was taught this strategy would focus more attention consistently for the first ten items. Rather, they would focus more attention to the first item, and a little less to the second and so forth. This would be especially true if the examinee was unaware of which item he/she was on. The average boost for each theta level can be found in Appendix B.

There were no significant differences in this boost between each true theta value group ($F(28, 28971) = .982$, $p = .492$). Also, there was no significant difference over the ten items between the theta value groups in the amount of boost they received ($F(252, 260739) = .982$, $p = .571$). Average boost levels at each item (1 through 10) can be seen in Table 4.1. Also, these mean boosts can be seen for each theta level in Figure 4.1. (For means and standard deviations at each theta level, see Appendix B.)

Table 4.1

Descriptive statistics for ability level boosts for items 1 through 10

	Minimum	Maximum	Mean	SD
$Boost_0$	.04	32.34	1.364	1.293
$Boost_1$	.02	16.17	.682	.646
$Boost_2$	.01	8.09	.341	.323
$Boost_3$	.01	5.39	.227	.215
$Boost_4$	.01	4.04	.171	.162
$Boost_5$	.00	3.23	.136	.129
$Boost_6$	.00	2.70	.114	.108
$Boost_7$	.00	2.31	.098	.092
$Boost_8$	.00	2.02	.085	.081
$Boost_9$	.00	1.80	.076	.072
$Boost_{10}$	.00	1.62	.068	.065

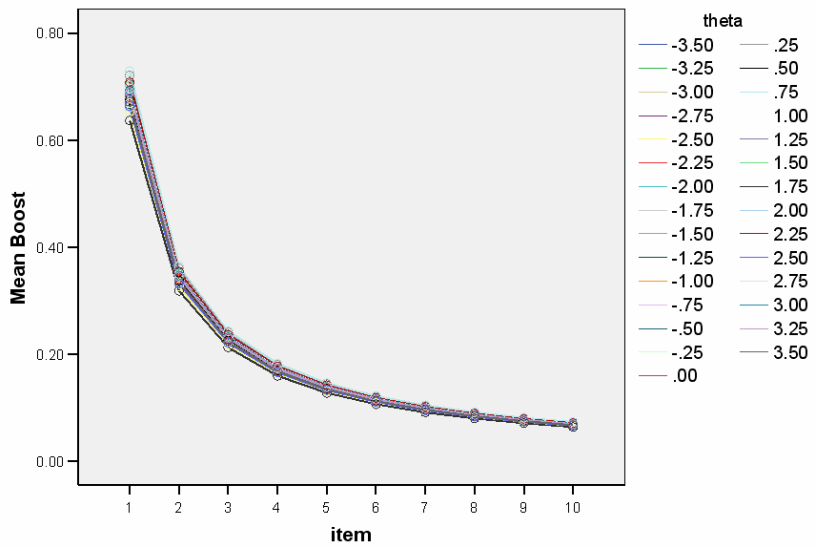


Figure 4.1. Spaghetti plot of mean ability level boost for each theta level for Items 1 through 10.

4.1.5.3 Adaptive test generation. Responses were generated interactively depending on the parameters of the item that was selected and the simulee's trait level given that item difficulty levels were known for every item in the pool. Each item was "administered" to the "examinee" based on the conditions outlined above. To review, for each item, there is an item characteristic curve that is the function of the known item parameters. Depending on the theta level of the examinee, the probability of answering the item correctly is equal to the *y*-axis coordinate for the particular theta value. To generate item response patterns, a random probability was drawn, from 0 to 1. If that random probability is equal to or less than the known probability of correctly answering the item, then the program will treat that as a correct answer and adapt

accordingly. If the random probability is greater than the known probability, the program will consider the examinee to have answered incorrectly, and adapt accordingly. For example, if an examinee has an ability level of 1.5, and he/she has a 0.85 chance of answering an item correctly, by choosing a random probability, 85% of these drawings should result in a correct answer.

This process was repeated for each item over 35 items for all replications resulting in a distribution of pattern responses and final theta estimates. These final theta estimates were then be compared to the known theta estimates for each examinee. This gave outcome measures that were used to evaluate the stability of CAT algorithms at the different test lengths.

4.1.6 Outcome Measures

The dependent variable for this study was the amount of variability between the true theta level and the estimated theta level at each fifth item interval of the test. This dependent variable was measured in three ways: total variability as measured by the average root mean squared error over the replications; systematic variability as measured by the average bias over the replications; and random variability as measured by the average standard deviation over the replications. Each of these is discussed in more detail next.

The first measure of discrepancy between the known theta values and the final theta estimates used was the root mean square error (RMSE). The RMSE is a measure of total variability; this measure of the recovery of the examinee's true ability level is the total of random variability and systematic variability as follows:

$$RMSE_{\theta_i}^2 = BIAS_{\theta_i}^2 + SD_{\theta_i}^2, \quad \text{(Eq. 3.3)}$$

where the RMSE is the measure of total variation of ability estimates over *R* replications. This total variability ($RMSE^2$) is equal to the sum of the systematic variability ($BIAS^2$) and the random variability (SD^2).

Computationally, the RMSE within each theta interval is equal to the square root of the average squared error of estimation over *R* replications:

$$RMSE_{\theta_i} = \left[\frac{\sum_{r=1}^{R} \left(\hat{\theta}_{ir} - \theta_i \right)^2}{R} \right]^{1/2}. \quad \text{(Eq. 3.4)}$$

The average RMSE over *N* examinees yields an overall measure of error:

$$RMSE = \frac{\sum_{i=1}^{N} RMSE_{\theta_i}}{N}. \quad \text{(Eq. 3.5)}$$

The second measure of estimate recovery used was that of bias. Bias gives a measure of the systematic variability. Bias gives a direct average of error in the estimation of θ_i, including the direction and magnitude of this error. An overall measure of this systematic variability over *R* replications within each theta value was found as follows:

$$BIAS_{\theta_i} = \left[\frac{\sum_{r=1}^{R} \left(\hat{\theta}_{ir} - \theta_i \right)}{R} \right]. \quad \text{(Eq. 3.6)}$$

An overall average bias for all true theta values was calculated as follows:

$$BIAS = \frac{\sum_{i=1}^{N} BIAS_{\theta_i}}{N}. \quad \text{(Eq. 3.7)}$$

The third outcome measure was the standard deviation, which measures the random variability of the theta estimates. The SD of θ_i is equal to the standard deviation of estimates over R replications:

$$SD_{\theta_i} = \left[\frac{\sum_{r=1}^{R} \left(\hat{\theta}_{ir} - \bar{\hat{\theta}}_{ir} \right)^2}{R} \right]^{1/2}. \quad \text{(Eq. 3.8)}$$

Then, to test for differences between conditions, an overall measure of standard deviation was calculated over all N examinees:

$$SD = \frac{\sum_{i=1}^{N} SD_{\theta_i}}{N}. \quad \text{(Eq. 3.9)}$$

The analyses involved looking at the value of these measures of error at Item 5, 10,…, 35 for each condition. Repeated-measures factorial ANOVAs were conducted to test for significant differences between conditions in each of the outcome measures. Also, a series of repeated-measures factorial ANOVAs was conducted to test for significant differences in the amount of variability from all sources discussed about between the null and boost conditions.

Also of interest in this study was whether the final estimate of theta was inflated due to this artificial boost. A repeated-measures factorial ANOVA was conducted to test for significant differences in theta estimation between the boost and null conditions.

4.2 Results

There were three measures of the stability of ability estimates in this study: 1) root mean square error, 2) bias, and 3) standard deviation. As mentioned in the previous section, each of these is a measure of the differences between estimated theta and true theta. These three outcome measures were used to test for significant differences between null versus boost conditions at each true theta level. Also, the three outcome measures were used to test for significant differences between null versus boost conditions, starting rule condition types, item pool sizes, discrimination levels, guessing parameters, and any possible interactions between the null versus boost condition variable and the other 4 condition variants. Each of these measures was examined at each interval of 5 items throughout the test to demonstrate the course of a computer adaptive test. To test for these differences between conditions over the course of the test, a repeated-measures factorial ANOVA was run for each of the outcome measures. Also, the theta estimate at each of seven points throughout the test was examined. The null and boost conditions were compared using a repeated-measures factorial ANOVA to see if there was significant inflation in the theta estimate over the course of the test in the boost conditions. These results are explained in the following sections. Due to the large number of simulated examinees, an alpha level of .01 was used.

4.2.1 Descriptive Statistics for Final Theta Estimation

First, to demonstrate the affect of this boost, descriptive statistics were run from the average theta estimate for each of the 288 boost conditions. Appendix C

summarizes these findings. Also, Appendix D shows the mean and 95% confidence intervals for the estimated theta at each true theta interval.

As can be seen in Appendix C and Figure 4.2a, there is an apparent regression toward the mean for the estimate at Item 5. Negative true theta values have higher estimates, and positive true theta values have lower estimates of theta. By Item 10, the average theta estimate boost is positive (as in Figure 4.2a). This boost does slowly reduce by the end of the test, but there seems to be an asymptote for boost around 0.03 with a mean of 0.046 (*SD* = .008). Also, at Item 5, Figure 4.2b shows that the standard deviation of the average theta estimate ranges from around 0.18 to as high as 1.7. After Item 10, this variability drops below .1 (*M* = .019, *SD* = .005).

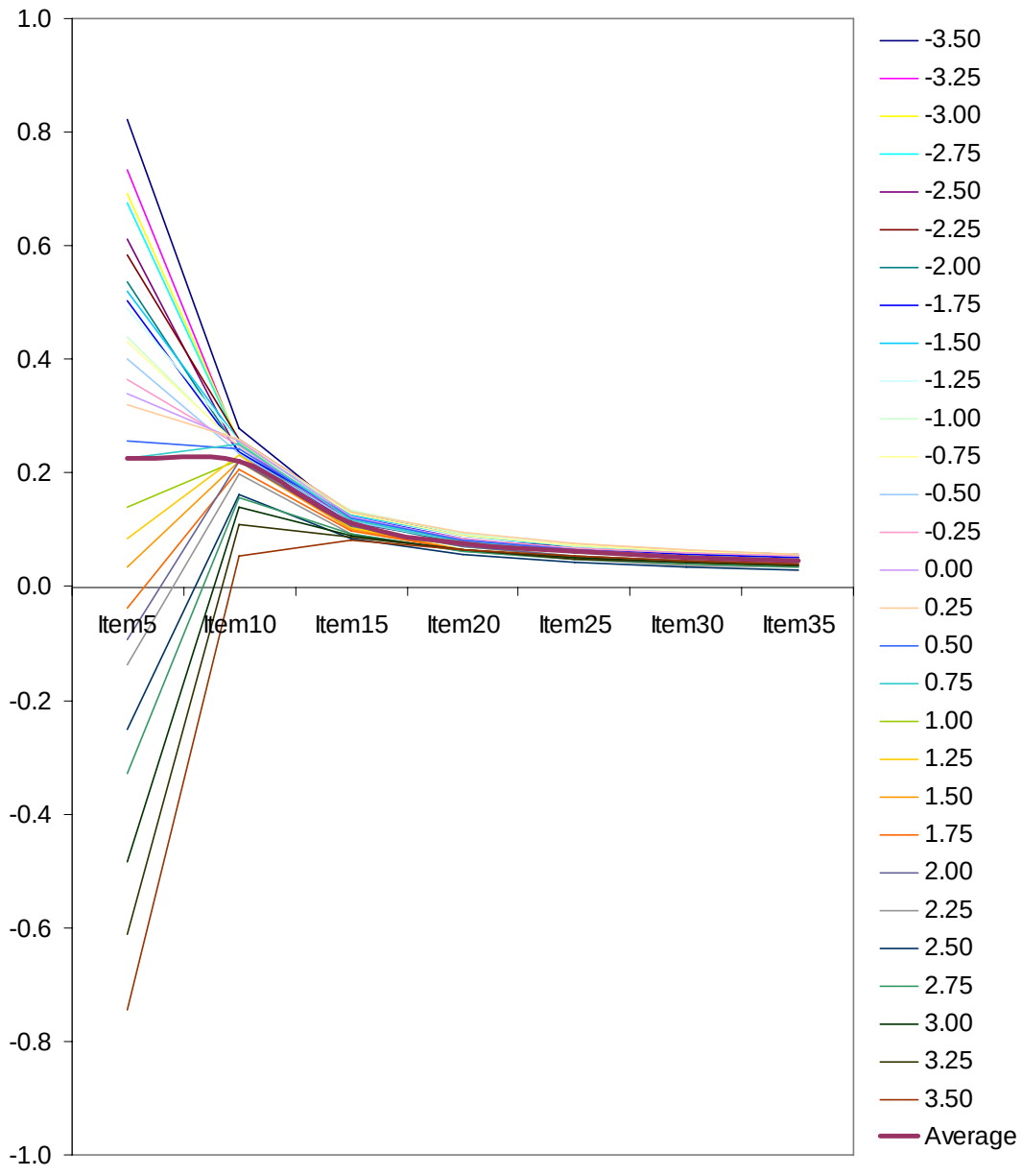


Figure 4.2a. Average theta estimate for boost condition at each theta interval at each item interval of the test.

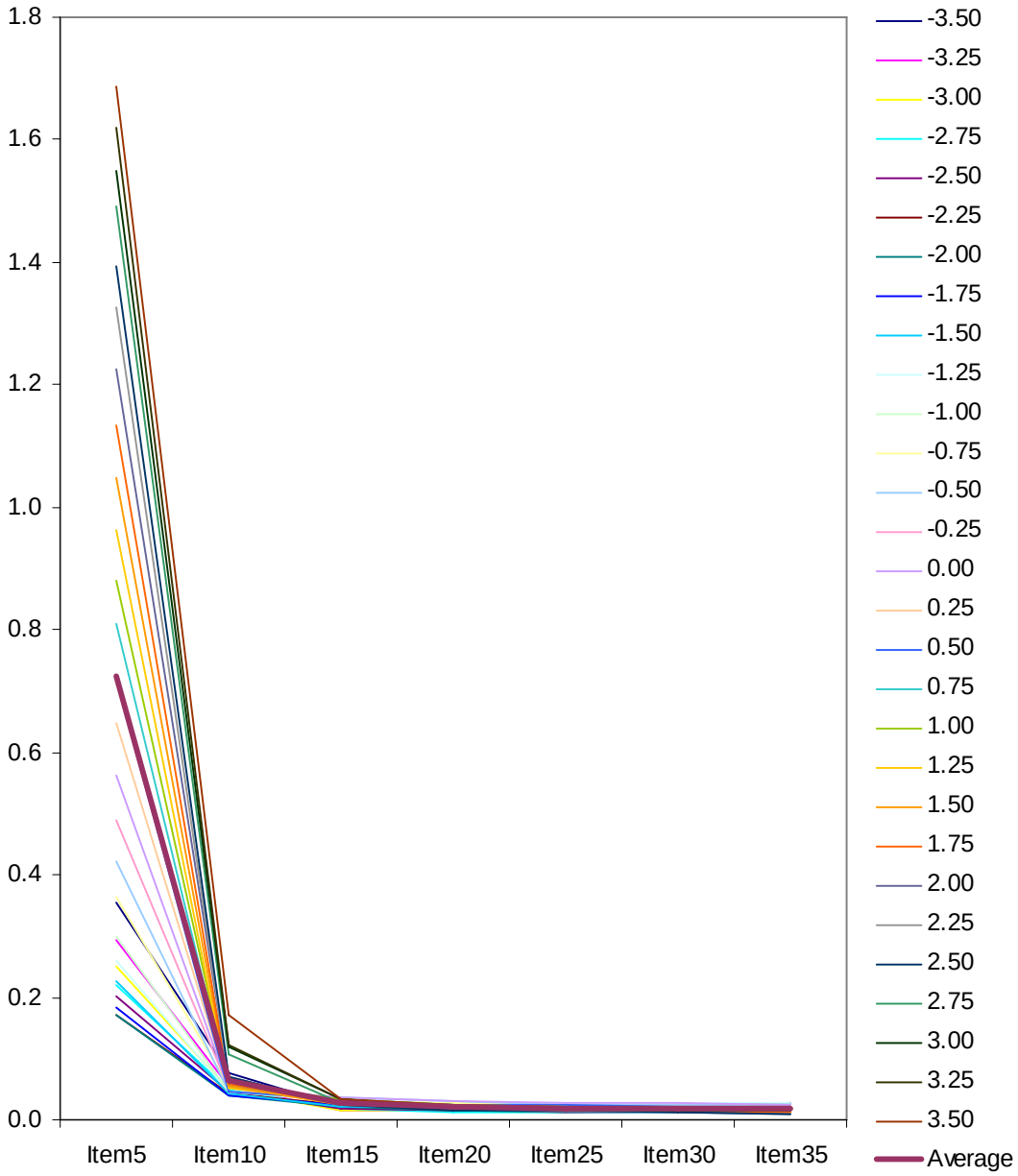


Figure 4.2b. Standard deviation of the theta estimate for boost conditions at each theta interval at each item interval of the test.

4.2.2 Overall Differences in Dependent Variables

4.2.2.1 Root mean squared error. A repeated-measures ANOVA was conducted. A large significant reduction in root mean squared error over time was found for all conditions ($F(6, 3324) = 7059.848$, $p = .000$, partial-$\eta^2 = .927$). As stated above, root mean squared error is a measure of total variability in the estimate of theta. As expected with a CAT, there was significantly more variability earlier in the test when there was greater error in the estimate. The following sections outline the differences found due to the independent variables of interest in this study.

4.2.2.2 Bias. The second outcome measure of interest was bias. As the difference between estimated theta and true theta, bias is the directional measure of systematic variability. As expected with a CAT, a small significant reduction in bias over time was found ($F(6, 3318) = 44.115$, $p = .000$, partial-$\eta^2 = .074$). The following sections outline the differences found due to the independent variables of interest in this study.

4.2.2.3 Standard deviation. As stated above, standard deviation is a measure of random variability in the estimate of theta. As expected with a CAT, a very large, significant reduction in the standard deviation over time was found for all conditions ($F(6, 3324) = 11484.961$, $p = .000$, partial-$\eta^2 = .954$). As the test continued, the amount of error between the individual's theta estimate and the group's theta estimate became smaller. The following sections outline the differences found due to the independent variables of interest in this study.

4.2.3 Null versus Boost Conditions

The first test conducted for this study was whether there was a significant difference between the simulated examinees with no boost and those with an artificial boost.

4.2.3.1 Root mean squared error. A repeated-measures factorial ANOVA was conducted to answer this question. The boost condition was found to have significantly higher RMSE than the null condition ($F(1, 554) = 373.741$, $p = .000$, partial-$\eta^2 = .403$), indicating that the estimates were further from the true theta in the boost condition. There was also a small significant interaction in terms of the difference in this change over time between the null and the boost conditions ($F(6, 3324) = 81.819$, $p = .000$, partial-$\eta^2 = .129$). This difference can be seen in Figure 4.3.

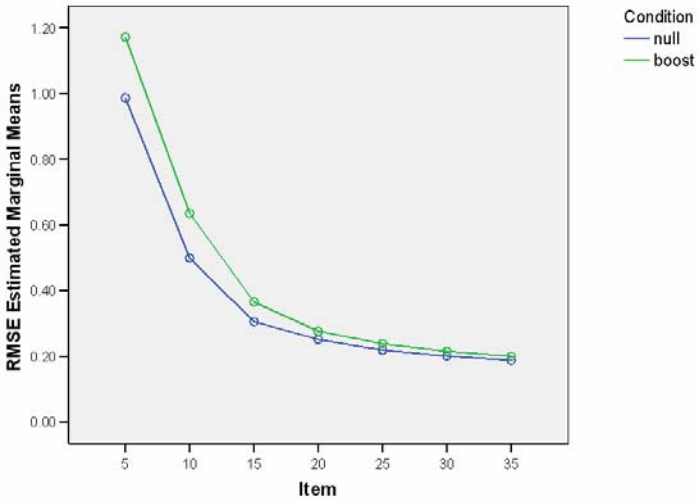


Figure 4.3. Estimated marginal means for RMSE for the null versus boost conditions over the 35 item test.

4.2.3.2 Bias. The first test conducted for this study was whether there was a significant difference between the simulated examinees with no boost and those with an artificial boost. A repeated-measures factorial ANOVA was conducted. A significant difference was found between the null and boost conditions in bias ($F(1, 553) = 530.016$, $p = .000$, partial-$\eta^2 = .489$). There was a significant interaction of time with the null and boost conditions ($F(6, 3318) = 65.121$, $p = .000$, partial-$\eta^2 = .105$). As can be seen in Figure 4.4, the null conditions started out with negative bias compared to a large amount of positive bias for the boost conditions. By the tenth item, both the null and boost conditions had positive bias, however, the boost conditions consistently had significantly more positive bias than the null conditions. This is important to note, because it suggests that examinees with a boost will have a consistent positive error in their score estimates in comparison to those without the boost.

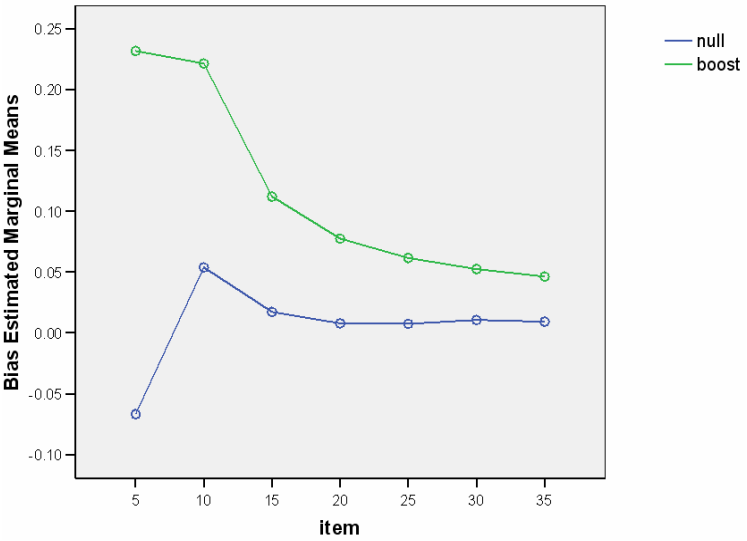


Figure 4.4. Estimated marginal means for the bias between the null and boost conditions.

4.2.3.3 Standard deviation. The first test conducted for each independent variable was whether there was a significant difference between the simulated examinees with no boost and those with an artificial boost. A repeated-measures factorial ANOVA was conducted. A large, significant difference was found between these two groups in SD ($F(1, 554) = 1970.239$, $p = .000$, partial-$\eta^2 = .781$). There was also a significant interaction between reduction in SD over time and the null versus boost condition variable ($F(6, 3324) = 145.560$, $p = .000$, partial-$\eta^2 = .208$). As can be seen in Figure 4.5, a large portion of the RMSE found earlier is apparent in the SD. As well, the boost conditions consistently had a higher standard deviation than the null conditions.

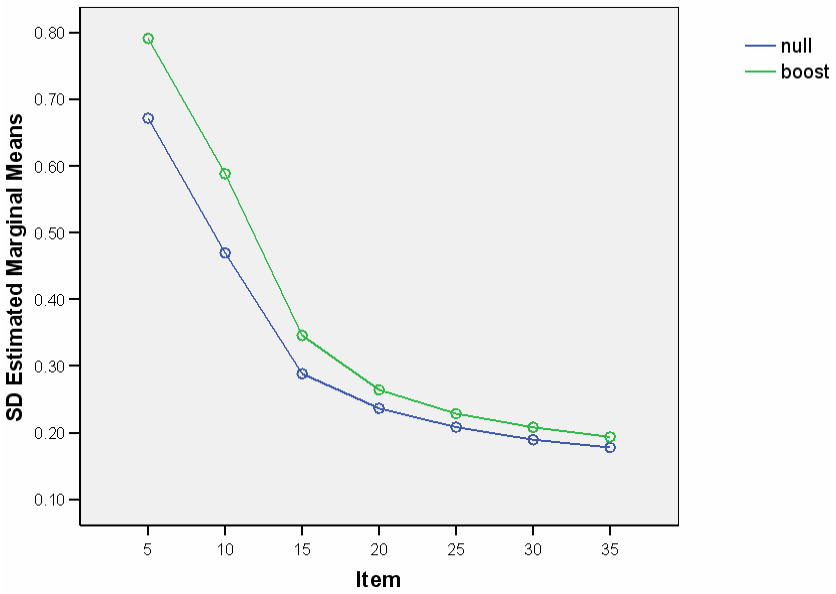


Figure 4.5. Estimated marginal means for differences in standard deviation between null and boost conditions.

4.2.4 Condition Type

The first independent variable of interest was type of starting rule used for the computerized adaptive test. As stated above, there were four types of starting rules varied: 1) RANDOM, i.e., the first item selected randomly with difficulty level close or equal to zero (b = 0, -.5 to .5, or -1.0 to 1.0); 2) FIXED, i.e., a fixed set size for the first five items (set size = 10, 20, or 30); 3) SHRINKING, i.e., a shrinking set size for the first five items (set size = 10 to 2 or 20 to 4); and 4) LINEAR, i.e., a linear fixed five-item test (difficulty level = easy, easy-to-medium, or medium).

4.2.4.1 Root mean squared error. A significant difference was found in overall RMSE due to condition type for the conditions ($F(3, 554) = 264.464$, $p = .000$,

partial-η^2 = .589). There was a very small significant interaction between condition type and null versus boost (NULLBOOST) conditions (F(3, 554) = 8.331, p = .000, partial-η^2 = .043). Also, a significant interaction in terms of the change over time of RMSE due to condition type was found for the conditions (F(18, 3324) = 132.861, p = .000, partial-η^2 = .418). This significant effect was also found to interact significantly with the null and boost conditions (F(18, 3324) = 4.661, p = .000, partial-η^2 = .025).

Through post hoc comparisons it was found that the linear fixed test resulted in significantly more RMSE than all three other condition types. Figure 4.6a and 4.6b show that RMSE was greater for the linear fixed test until the twenty-fifth item at which point the amount of RMSE was virtually the same for all condition types. Also, comparing Figure 4.6a to 4.6b elucidates the sources of the interaction: it can be seen that the LINEAR test type resulted in more RMSE than the other test types, and the boost conditions resulted in more RMSE at the earlier items than did the null conditions.

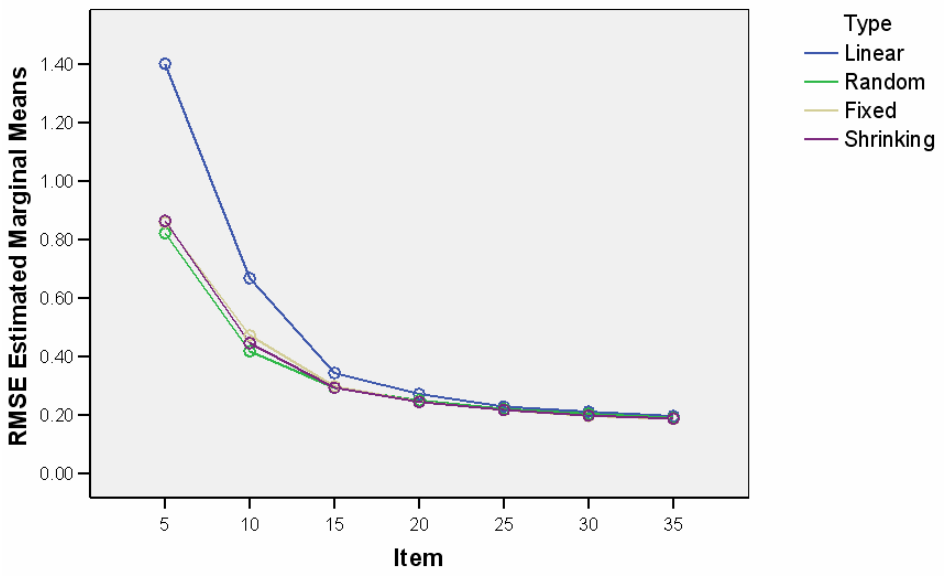


Figure 4.6a. Estimated marginal means for the root mean squared error for each null condition type for items over the test.

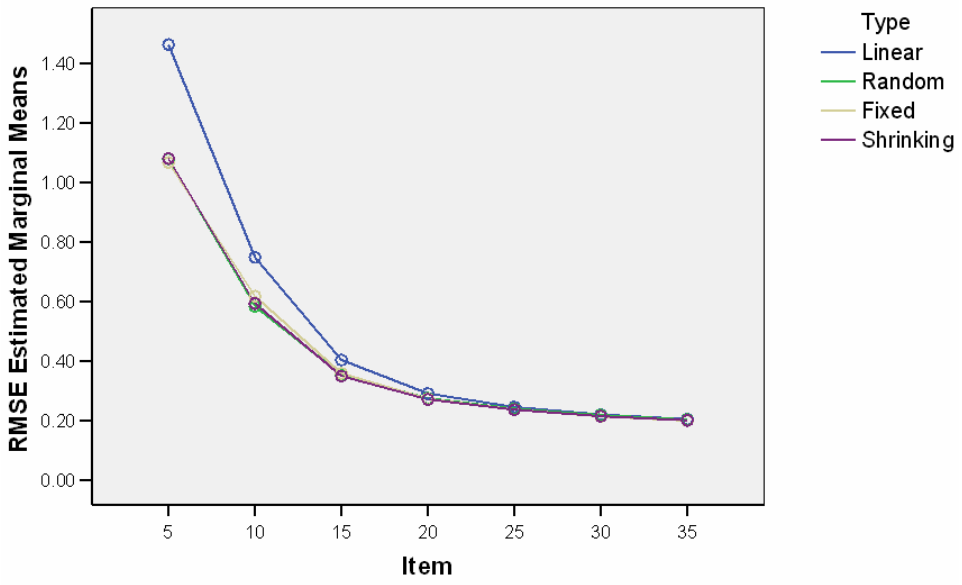


Figure 4.6b. Estimated total root mean squared error for each boost condition type for items over the test.

In closer examination of the different versions of the condition types, no significant differences were found between the first three condition types for the conditions. However, a large significant difference was found between the linear fixed test condition types ($F(2, 132) = 436.831$, $p = .000$, partial-$\eta^2 = .869$). This difference did not interact with the NULLBOOST variable. That is, the null and boost conditions showed the same pattern of differences between the LINEAR starting rules. Through post hoc comparisons, it was found that all three LINEAR starting rules were significantly different from each other. The easy test resulted in significantly more RMSE than the medium test ($M_D = .079$, $SE_D = .005$, $p = .000$), and the medium test resulted in significantly more RMSE than the hard test ($M_D = .056$, $SE_D = .005$, $p = .000$). These differences can be seen in Figures 4.7a and 4.7b.

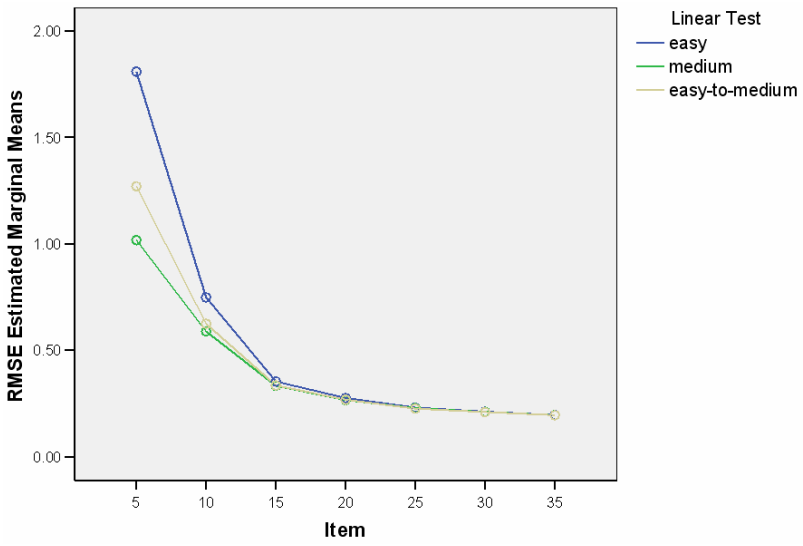


Figure 4.7a. Estimated marginal means for the RMSE at each item interval for the linear test null conditions.

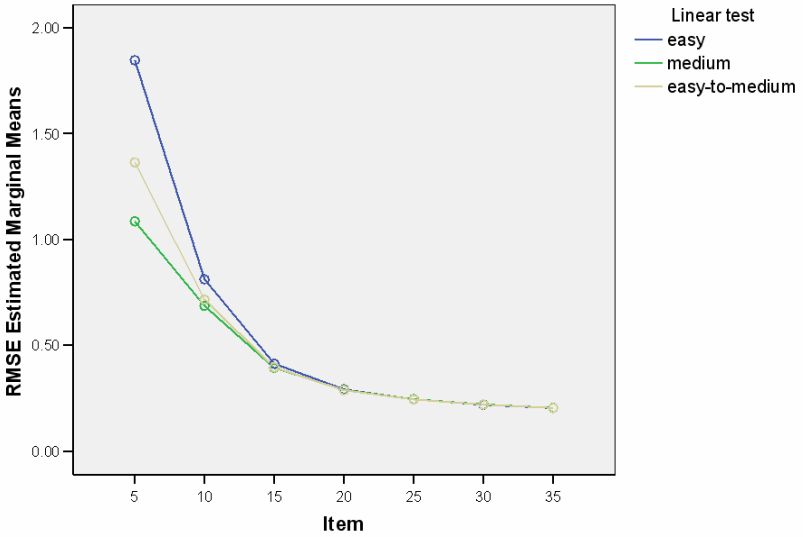


Figure 4.7b. Estimated marginal means for the RMSE at each item interval for the linear test boost conditions.

4.2.4.2 Bias. A significant difference in bias was found due to starting rule condition type ($F(3, 553) = 336.168$, $p = .000$, partial-$\eta^2 = .646$). A small significant difference between null and boost conditions in bias due condition type was found ($F(3, 553) = 10.903$, $p = .000$, partial-$\eta^2 = .056$). A significant difference in change of bias over time was found due to condition type ($F(18, 3318) = 312.918$, $p = .000$, partial-$\eta^2 = .629$). A very small, significant interaction between change of bias over time due to condition type was also found between the null and boost conditions ($F(18, 3318) = 3.203$, $p = .000$, partial-$\eta^2 = .017$). Overall, the amount of bias was more dramatic for the boost conditions than the null conditions, especially for Items 5 and 10.

Through post hoc comparisons it was found that the linear fixed test condition resulted in significantly less bias than all other condition types. This can be seen in Figures 4.8a and 4.8b. However, the amount of bias becomes almost equal to zero for all condition types after Item 5.

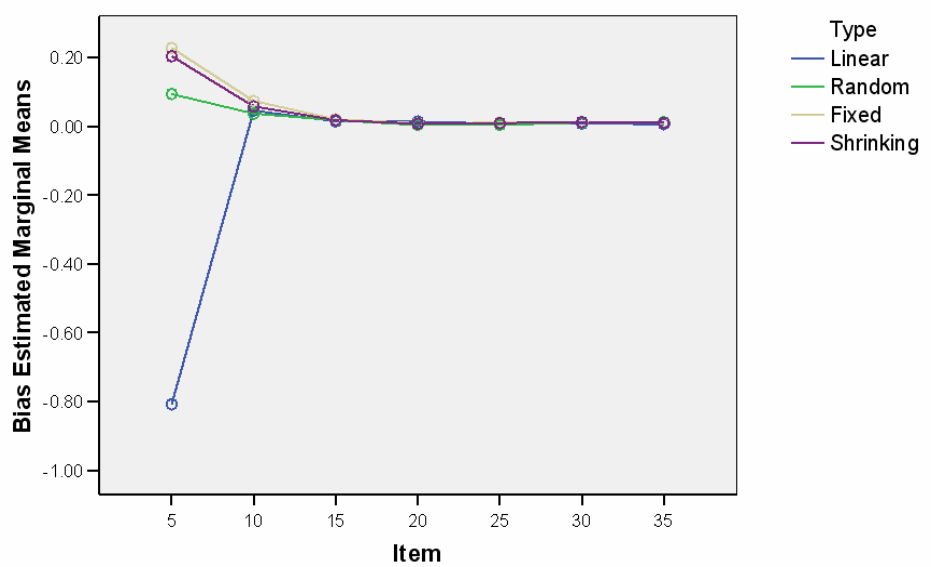


Figure 4.8a. Estimated marginal means for bias between condition types for the null conditions.

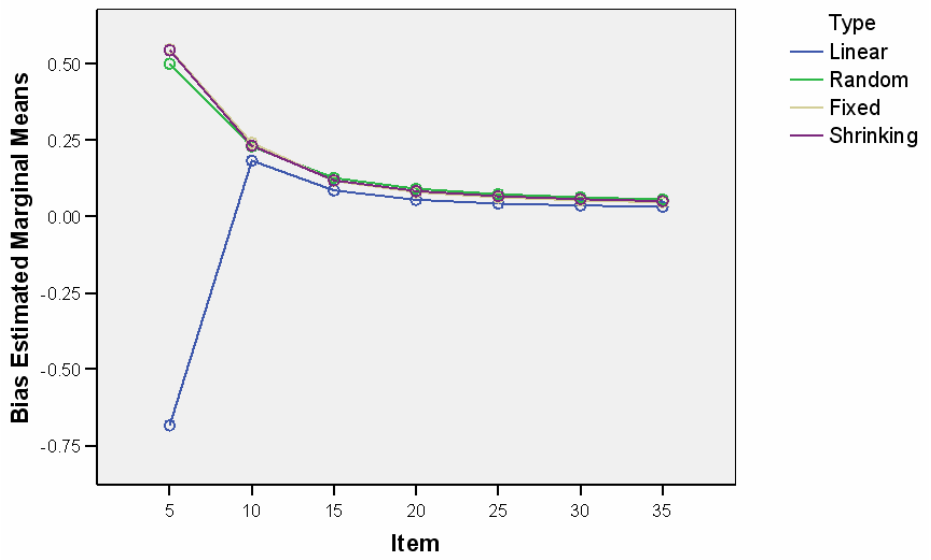


Figure 4.8b. Estimated marginal means for bias between condition types for the boost conditions.

In examining possible differences due to the versions of each of the condition types, no significant differences were found between the versions of the first three starting rule condition types. However, significant differences were found between the linear fixed test conditions ($F(2, 132) = 404.317$, $p = .000$, partial-$\eta^2 = .860$). Through post hoc comparisons it was found that all three linear fixed tests conditions were significantly different from each other. The easy test resulted in significantly more negative bias than the easy-to-medium test ($M_D = -.075$, $SE_D = .009$, $p = .000$) and the medium test ($M_D = -.215$, $SE_D = .008$, $p = .000$). The easy-to-medium test resulted in significantly more negative bias than the medium test ($M_D = -.139$, $SE_D = .009$, $p = .000$). As can be seen in Figures 4.9a and 4.9b, this difference only exists

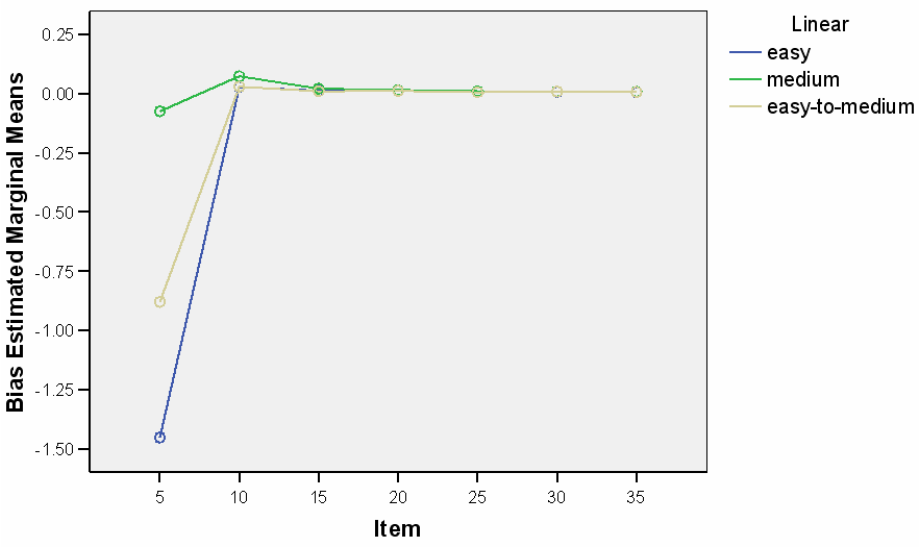


Figure 4.9a. Estimated marginal means for bias between linear fixed test conditions for null conditions.

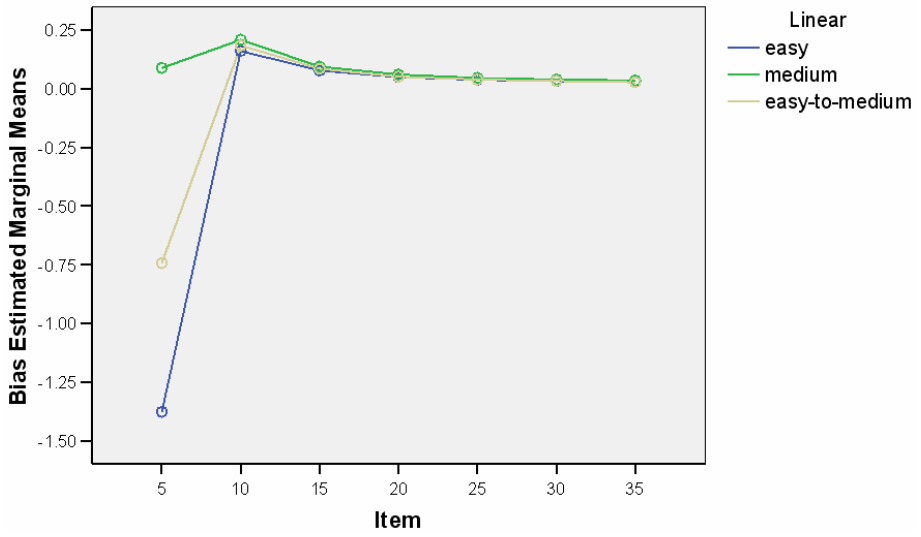


Figure 4.9b. Estimated marginal means for bias between linear fixed test conditions for boost conditions.

at Item 5—linear fixed test with average difficulty level items (medium linear test condition) resulted in less bias than the other two types. The easy test resulted in the most bias. Bias begins to converge between these condition types within the null and boost conditions around Item 10. However, at Item 10, the boost conditions had slightly higher positive bias than the null conditions. Both the null and boost conditions had virtually zero bias after Item 10.

4.2.4.3 Standard deviation. A significant difference was found between the four types of conditions for starting rules ($F(3, 554) = 113.692$, $p = .000$, partial-$\eta^2 = .381$). A small, significant interaction was found between the condition types and the null versus boost condition variable ($F(3, 554) = 11.147$, $p = .000$, partial-$\eta^2 = .057$). A large, significant interaction was also found between the change of SD over time and the type of condition ($F(18, 3324) = 736.995$, $p = .000$, partial-$\eta^2 = .800$). Finally, a significant interaction between change over time and condition type and the null versus boost condition variable was found ($F(18, 1608) = 6.307$, $p = .000$, partial-$\eta^2 = .033$). Overall, the amount of standard deviation is greater for the boost conditions than the null conditions.

Through post hoc comparisons it was found that over the entire test, the linear fixed test starting condition resulted in significantly lower SD than the other condition types. However, in looking at Figures 4.10a and 4.10b, it can be seen that this result is misleading. At the fifth item, the linear fixed test has significantly lower SD. At the tenth item, the linear fixed test has significantly higher SD. Beginning around the

twenty-fifth item, the SD of the linear fixed test begins to converge with the other starting conditions. This is true for both the null and boost conditions.

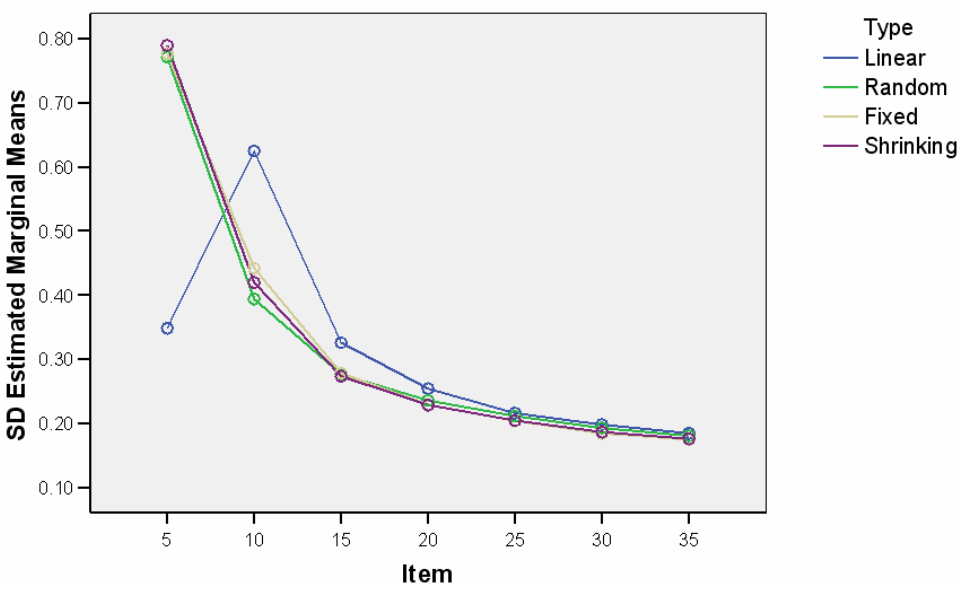


Figure 4.10a. Estimated marginal means for standard deviation between condition types for null conditions.

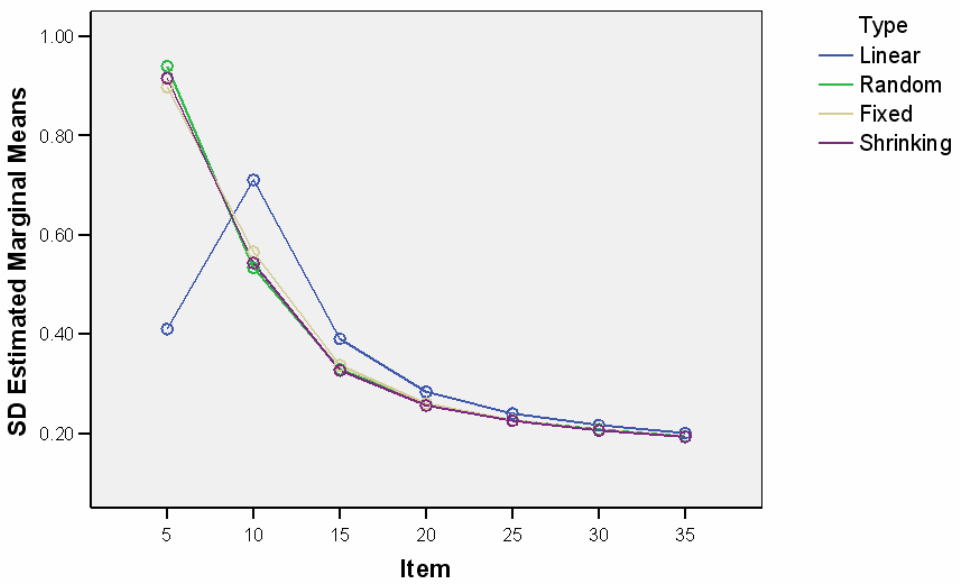


Figure 4.10b. Estimated marginal means for standard deviation between condition types for boost conditions.

The repeated-measures, factorial ANOVA was conducted within each starting condition type to look for differences between each version of the starting rule condition. No significant differences were found between the versions of the starting rules including administering a first item with difficulty level equal or close to zero (RANDOM), a fixed set size (FIXED), or a shrinking set size (SHRINKING). However, a significant difference was found between the three types of linear fixed tests for the null conditions ($F(2, 132) = 41.999$, $p = .000$, partial-$\eta^2 = .389$).

Through a post hoc comparison of these linear test types for the null conditions it was found that all three test levels were significantly different from each other. Figure 4.11a shows that at Item 5, the medium level test has significantly more

SD than the other two. However, at Item 10, the easy test resulted in more SD with the easy-to-medium and medium tests having similar amounts of SD. At Items 15 and 20, the easy test still has slightly more SD and then converges with the other two around Item 25. As can be seen in Figure 4.11b, the SD is higher for the medium difficulty test at Item 5, then lower at Item 10. Finally, the SD becomes very similar for all tests beginning at the fifteenth item. However, there is slightly more SD in the medium difficulty test after the twenty-fifth item.

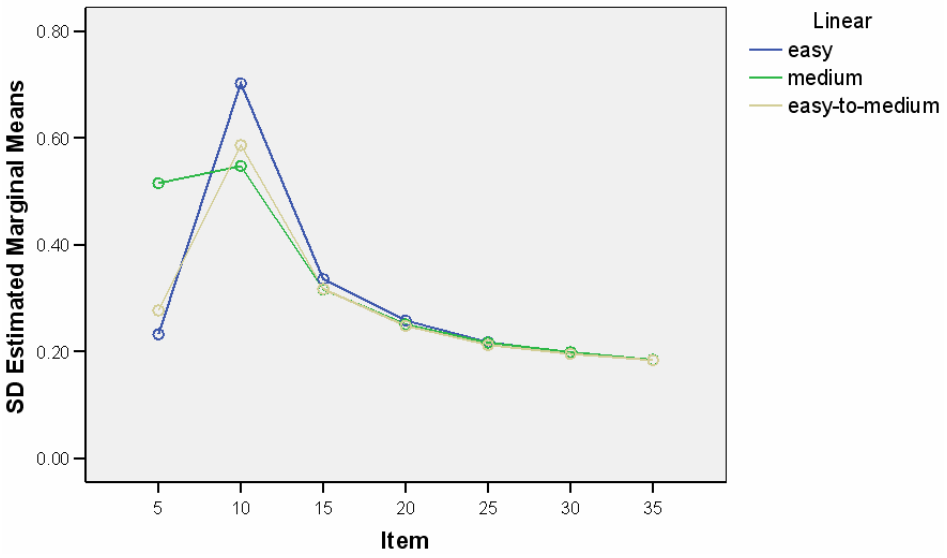


Figure 4.11a. Estimated marginal means for standard deviation between linear test types for null conditions.

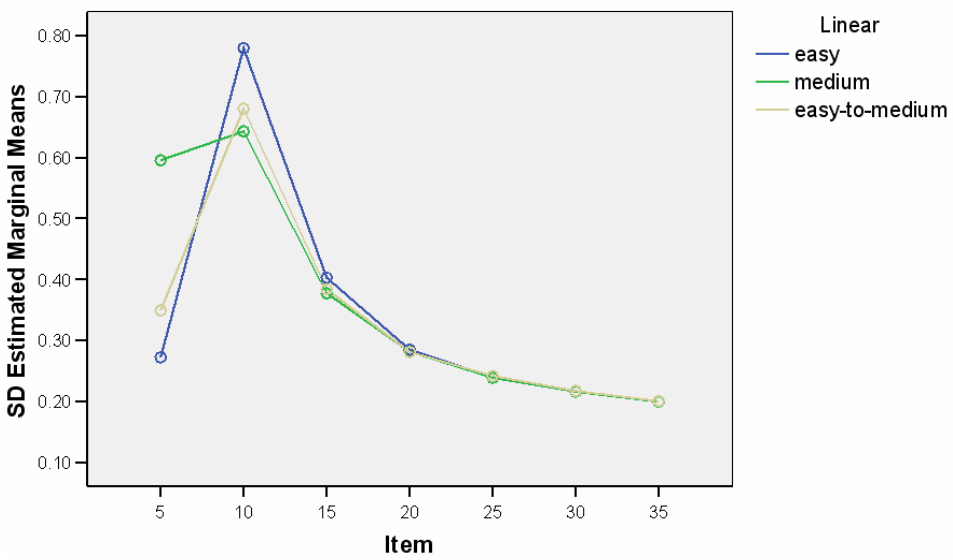


Figure 4.11b. Estimated marginal means for standard deviation between linear test types for boost conditions.

4.2.5 Item Pool Size

As discussed previously, three item pool sizes (n_i = 100, 500, and 1000) were examined for possible differences in RMSE, bias and standard deviation of the theta estimate at different points in the test.

4.2.5.1 Root mean squared deviation. There was a very small significant main effect of pool size (F(2, 554) = 11.200, p = .000, partial-η^2 = .039). There was a marginally significant interaction between the null versus boost conditions variable and item pool size (F(2, 554) = 3.464, p = .032, partial-η^2 = .012). The RMSE was higher at Items 5 and 10 for the boost conditions than for the null conditions.

Through post hoc comparisons it was found that the largest item pool (n_i = 1000) resulted in significantly less RMSE than the medium item pool (n_i = 500) (M_D = -.012, SE_D = .004, p = .002) and the small item pool (n_i = 100) (M_D = -.019, SE_D = .004, p = .000). Also, there was a significant interaction between change over time and the item pool sizes (F(12, 3324) = 23.186, p = .000, partial-η^2 = .077). This difference is apparent in Figures 4.12a and 4.12b which show that after the 15th item, the 100-item pool results in significantly more RMSE than the other two pool sizes. Also, it should be noted that for the null conditions, there is less RMSE earlier in the test than for the boost conditions. After the 15th item, this difference is no longer noticeable.

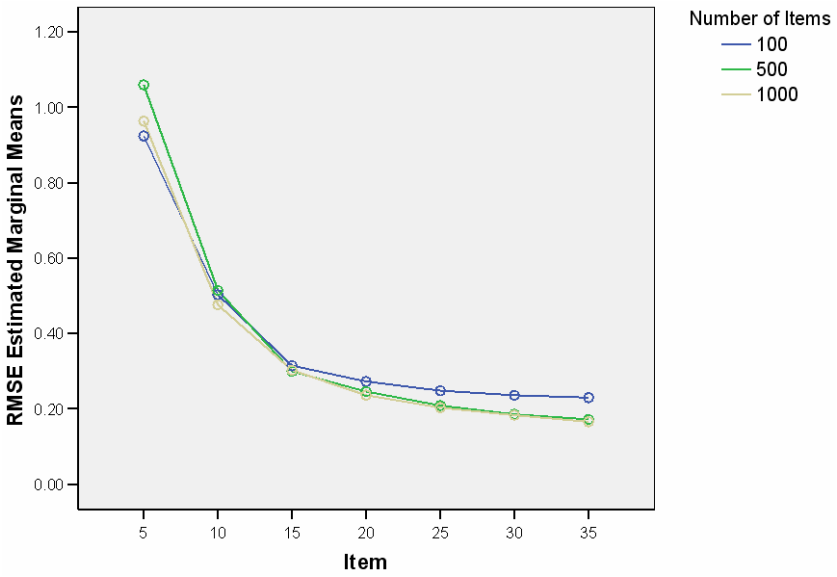


Figure 4.12a. Estimated marginal means for RMSE for the item pool size for the null conditions.

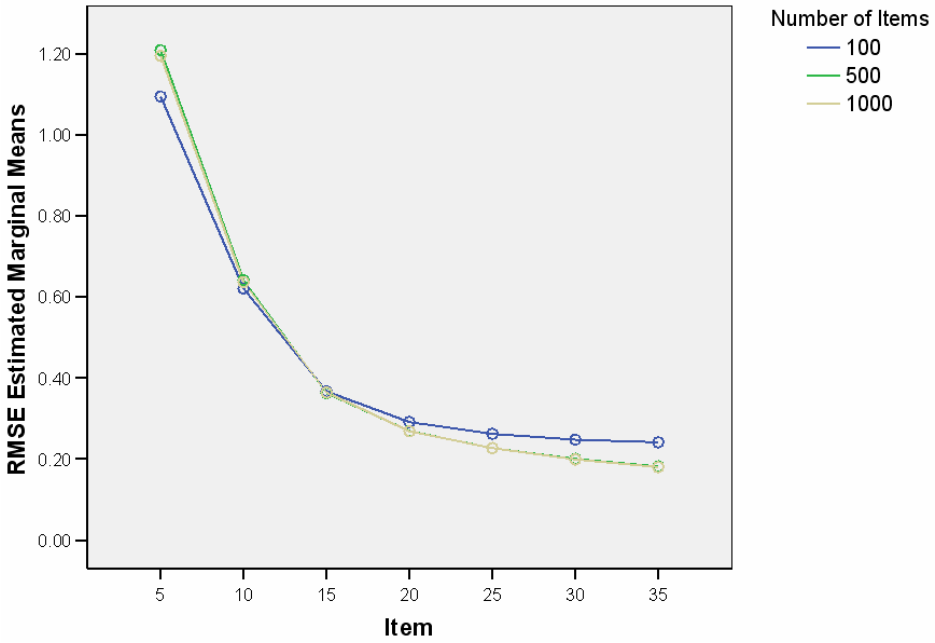


Figure 4.12b. Estimated marginal means for RMSE for the item pool size for the boost conditions.

4.2.5.2 Bias. Item pool size was varied to examine possible differences in error of theta estimation due to having enough items. As outlined in above, three variants of item pool size were examined: 100, 500, and 1000 items. No significant differences were found due to pool size.

4.2.5.3 Standard deviation. Item pool size was varied to examine possible differences in error of theta estimation due to having enough items. Three variants of item pool size were examined: 100, 500, and 1000 items. A significant difference between item pool sizes was found ($F(2, 554) = 156.856$, $p = .000$, partial-$\eta^2 = .362$). This difference was also found to significantly interact with the null and boost

conditions variable ($F(2, 554) = 25.892$, $p = .000$, partial-$\eta^2 = .085$). As with the overall differences in SD found, the null conditions resulted in less SD than the boost conditions. Also, a small, significant interaction between reduction of SD over time and item pool size was found ($F(12, 3324) = 26.010$, $p = .000$, partial-$\eta^2 = .086$). This change over time in SD was found to slightly interact with the null and boost conditions variable ($F(12, 3324) = 4.055$, $p = .000$, partial-$\eta^2 = .014$). Overall, the amount of standard deviation was greater for the boost conditions.

Through post hoc comparisons it was found that all three pool sizes resulted in significantly different levels of SD for the null conditions. It can be seen in Figure 4.13a that for the null conditions the medium sized item pool resulted in more SD for the first 15 items, and the smallest item pool resulted in more SD for the remainder of the test. This same pattern can be seen in Figure 4.13b for the boost conditions, however, the amount of SD earlier in the test is larger for these conditions in comparison to the null conditions.

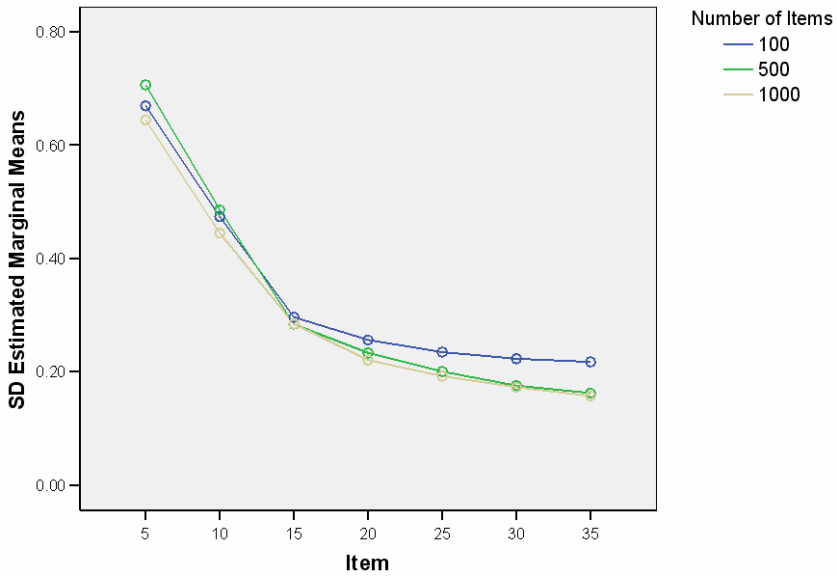


Figure 4.13a. Estimated marginal means for standard deviation between item pool sizes for null conditions.

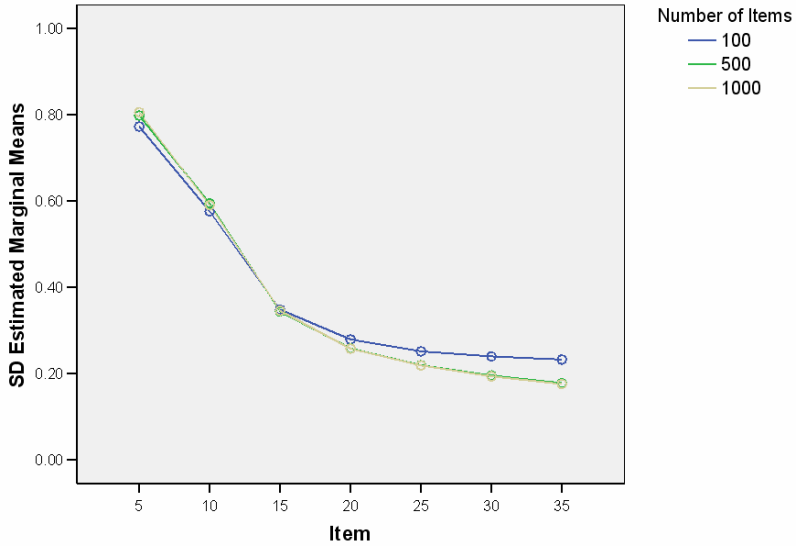


Figure 4.13b. Estimated marginal means for standard deviation between item pool sizes for boost conditions.

4.2.6 Discrimination Parameter Levels

To examine differences in the error of the estimation of theta value due to the discriminating power of an item, three variants for the a-parameter were used: 1.0, 1.5, and 2.0.

4.2.6.1 Root mean squared error. The discriminating power of the item was examined for its effect on the amount of RMSE in the theta estimate throughout the test. A significant difference in RMSE was found due to the discrimination parameter ($F(2, 554) = 804.373$, $p = .000$, partial-$\eta^2 = .744$). There was no significant differences interaction between the null versus boost condition variable and the discrimination parameters. However, a small significant interaction was found between change over time of the RMSE differing and the discrimination parameter levels ($F(12, 3324) = 9.131$, $p = .000$, partial-$\eta^2 = .032$). The differences found between discrimination parameter levels is greater during the middle part of the test than at the beginning and end of the test.

Through post hoc comparisons it was found that all three parameter levels were significantly different from each other. As seen in Figures 4.14a and 4.14b, significantly less RMSE resulted from larger discrimination parameters. Also, in comparing these two figures, it can be seen that the null conditions had less RMSE overall when compared to the boost conditions regardless of the level of discrimination.

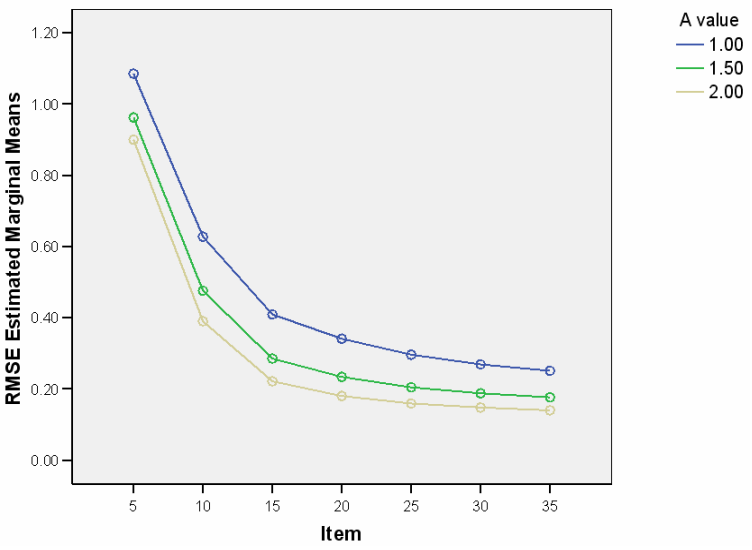


Figure 4.14a. Estimated marginal means for RMSE between discriminating parameters for the null conditions.

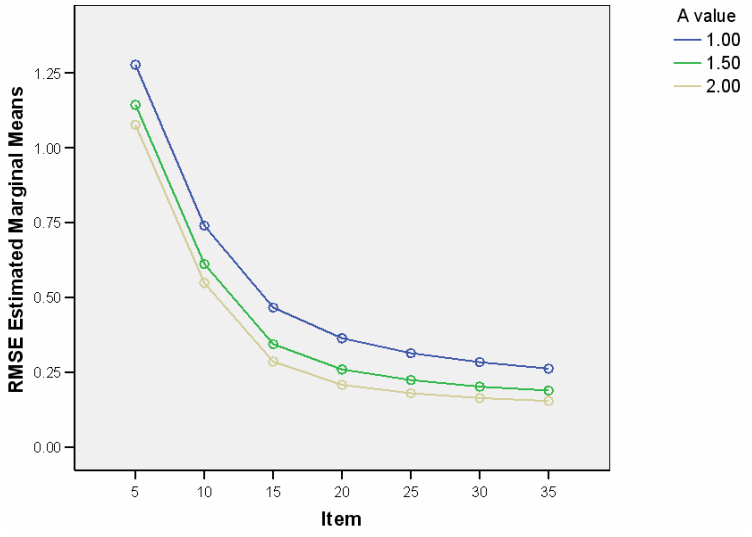


Figure 4.14b. Estimated marginal means for RMSE between discriminating parameters for the boost conditions.

4.2.6.2 Bias. A very small, significant difference was found between levels of the discrimination parameters ($F(2, 553) = 15.243$, $p = .000$, partial-$\eta^2 = .052$). Also, there was a very small significant interaction between the amount of reduction over time in bias and the discriminating power of the items ($F(12, 3318) = 10.119$, $p = .000$, partial-$\eta^2 = .035$). The differences due to discriminating power were much greater at Item 5 than the rest of the items.

Post hoc comparisons were conducted to find where these differences existed. All three levels of discrimination were significantly different from each other. As can be seen in Figures 4.15a and 4.15b, larger discrimination parameters resulted in significantly lower bias. For the null conditions, estimates at Item 5 were much lower than the true estimate for discrimination levels of 1.5 and 2.0. At Item 10, all three levels resulted in average estimates slightly higher than the true theta estimate. After Item 15, the differences between the estimates and the true thetas were virtually equal to zero. For the boost conditions, average bias was never negative. Lower discrimination parameters resulted in higher biases. At Item 10, these biases converged. They continued to decrease over the length of the test, reaching an apparent asymptote around a positive bias of .05.

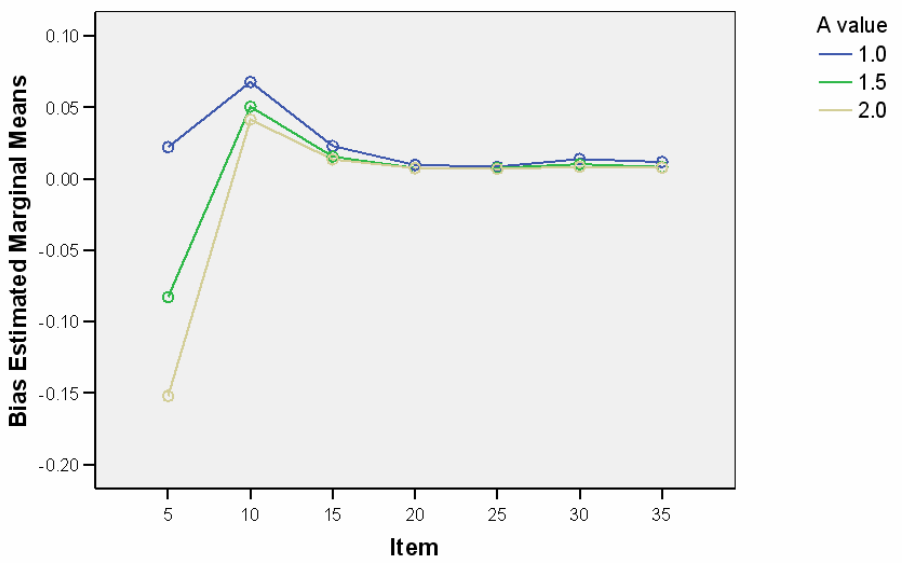


Figure 4.15a. Estimated marginal means for bias between discrimination parameter levels for null conditions.

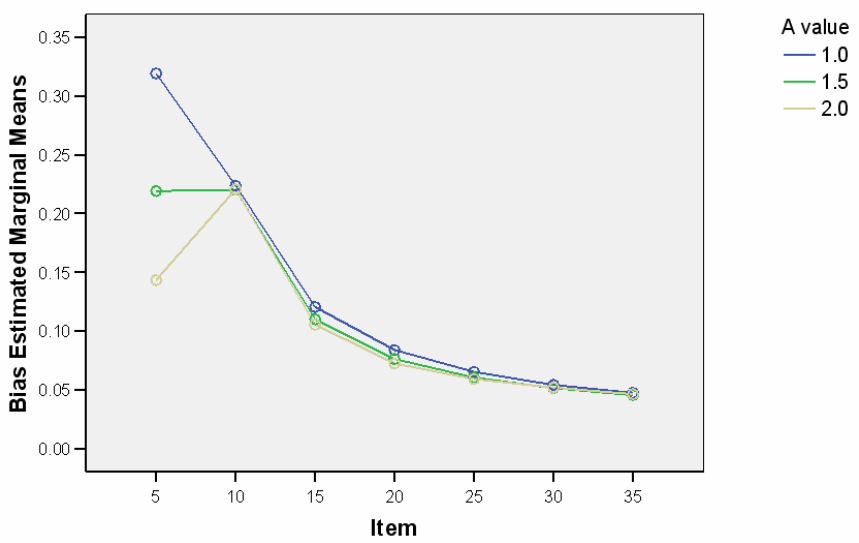


Figure 4.15b. Estimated marginal means for bias between discrimination parameter levels for boost conditions.

4.2.6.3 Standard deviation. A large, significant difference in SD due to discriminating ability of the items was found ($F(2, 554) = 7695.878$, $p = .000$, partial-$\eta^2 = .965$). A very small, significant interaction between the SD due to discrimination level and the null and boost conditions variable was found ($F(2, 554) = 6.915$, $p = .001$, partial-$\eta^2 = .024$). Also, a significant interaction between change in SD over time and discriminating ability of the items was found ($F(12, 3324) = 149.059$, $p = .000$, partial-$\eta^2 = .350$). This change was also found to significantly interact with the null and boost conditions variable ($F(12, 3324) = 3.437$, $p = .000$, partial-$\eta^2 = .012$). Overall, the amount of standard deviation was greater for the boost conditions than for the null conditions.

Through post hoc comparisons it was found that all three discrimination levels were significantly different from each other. As can be seen in Figures 4.16a and 4.16b, the less discriminating an item, the more SD there is associated with the resulting theta estimate at any point in the test. As with the other independent variables, the amount of SD was greater for the boost conditions than for the null conditions. This is consistent with the design of the boost conditions.

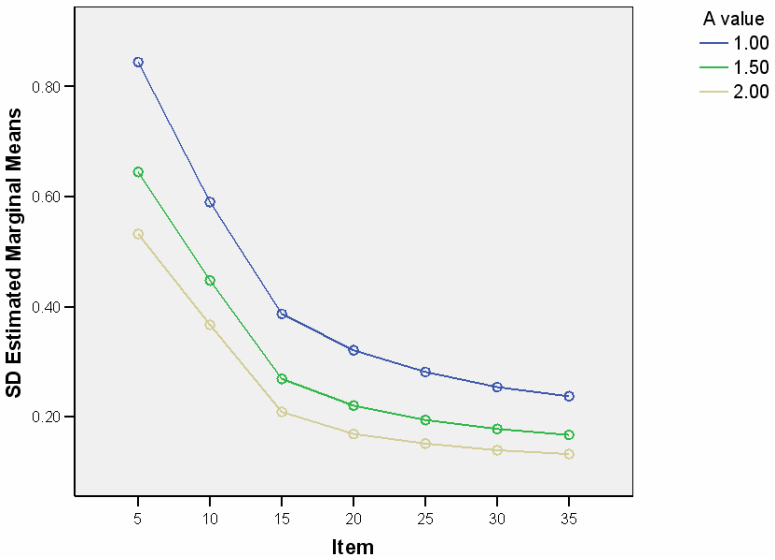


Figure 4.16a. Estimated marginal means for standard deviation between discrimination parameter levels for null conditions.

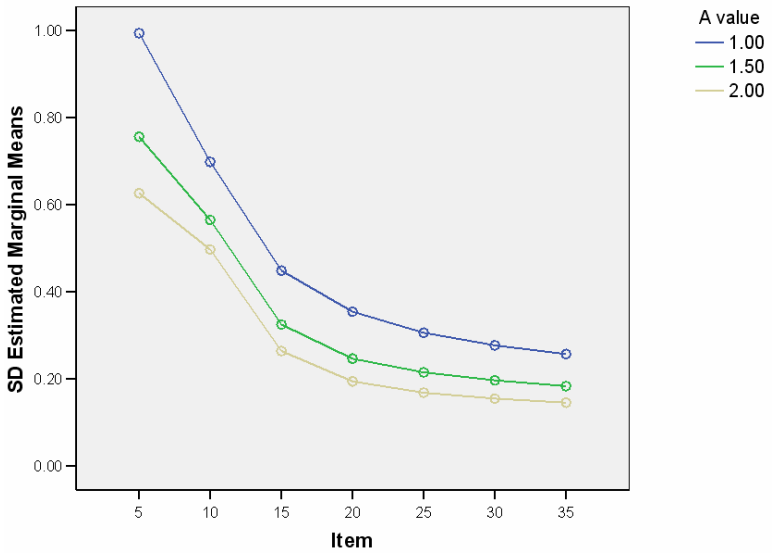


Figure 4.16b. Estimated marginal means for standard deviation between discrimination parameter levels for boost conditions.

4.2.7 Guessing Parameter Levels

Analyses were conducted to examine differences in RMSE due to three different levels of the guessing parameter (c = 0.0, 0.1, and 0.2).

4.2.7.1 Root mean squared error. Different guessing parameters resulted in significantly different amounts of RMSE ($F(2, 554) = 306.434$, $p = .000$, partial-$\eta^2 = .525$). These differences were found to be consistent for both the null and boost conditions; there was no interaction between the null versus boost condition variable and the guessing parameter variable. However, a small but significant interaction was found between change in RMSE over time and guessing parameter levels ($F(12, 3324) = 30.291$, $p = .000$, partial-$\eta^2 = .099$). Differences in RMSE due to guessing parameter levels were greater during the first half of the test. Those differences grew smaller later in the test.

Through post hoc comparisons it was found that all three values of the guessing parameter were significantly different from each other. Figures 4.17a and 4.17b show that the larger guessing parameters resulted in significantly more RMSE. Also, in comparing these two figures, it can be seen that the null conditions had less RMSE overall when compared to the boost conditions regardless of the level of the guessing parameter.

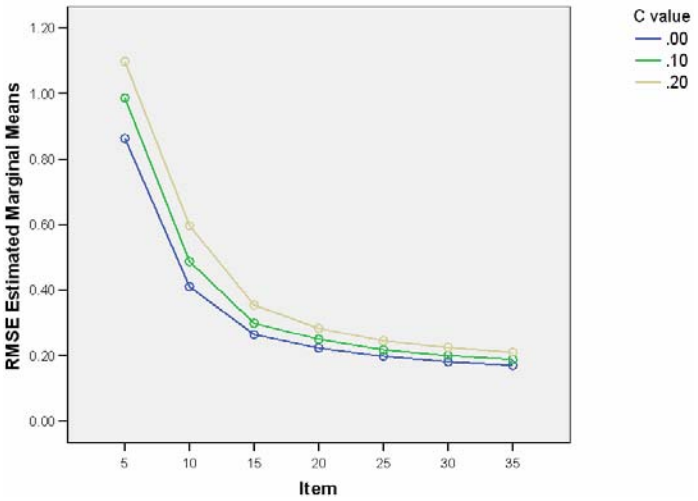


Figure 4.17a. Estimated marginal means for the RMSE between guessing parameters for the null conditions.

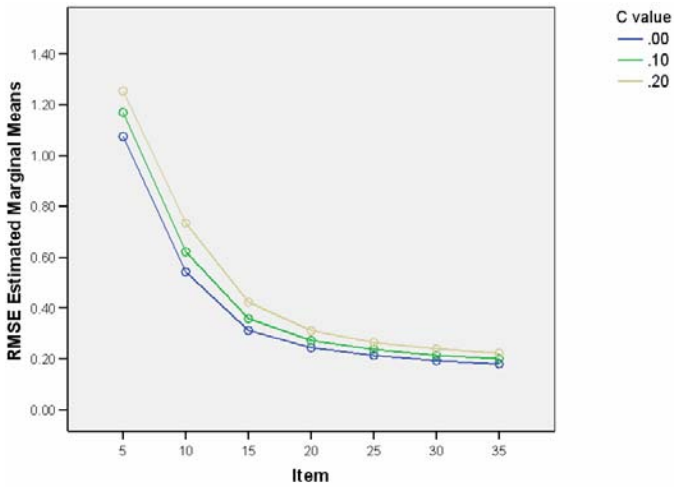


Figure 4.17b. Estimated marginal means for the RMSE between guessing parameters for the boost conditions.

4.2.7.2 Bias. A very small, significant difference was found in bias due to magnitude of the guessing parameter ($F(2, 553) = 12.276$, $p = .000$, partial-$\eta^2 = .043$). A very small, significant interaction between change over time and guessing parameter level was also found ($F(12, 3318) = 3.102$, $p = .000$, partial-$\eta^2 = .011$). Differences in bias due to guessing level were greater at Items 5 and 10 than the rest of the test.

Post hoc comparisons were conducted to see where these differences existed. The largest discrimination parameter (c = .20) resulted in significantly higher bias levels than the middle level (c = .10, $M_D = .016$, $SE_D = .006$, $p = .005$) and the lowest level (c = .00, $M_D = .028$, $SE_D = .006$, $p = .000$). As can be seen in Figures 4.18a and 4.18b, similar differences existed for the guessing parameters as those found for the discrimination parameter. For the null conditions (Figure 4.18a), all three levels of the guessing parameter resulted in negative bias (that is, lower estimates of theta when compared to the true theta value) at Item 5. At Item 10, these biases became slightly positive. After Item 15, these biases leveled out around zero. For the boost conditions (Figure 4.18b), the bias was very positive for the first ten items. Bias began to drop over the subsequent items and began to level out around .05 during the last 10 items or so.

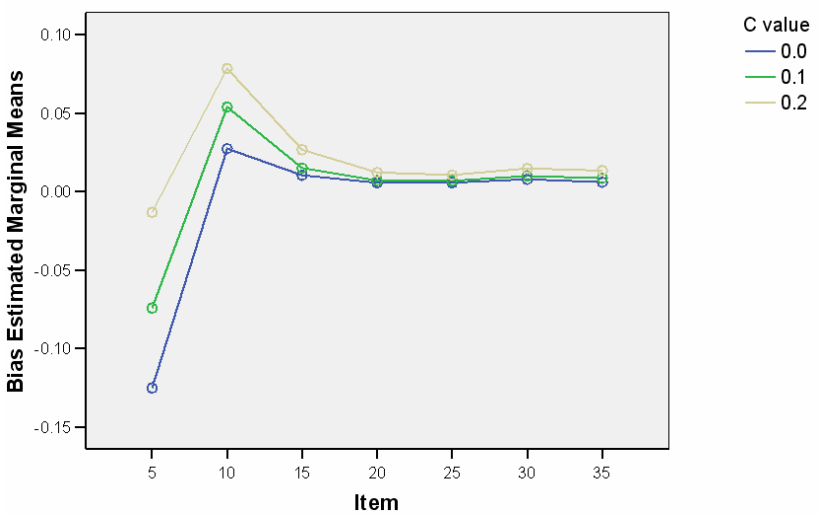


Figure 4.18a. Estimated marginal means for bias between guessing parameter levels for null conditions.

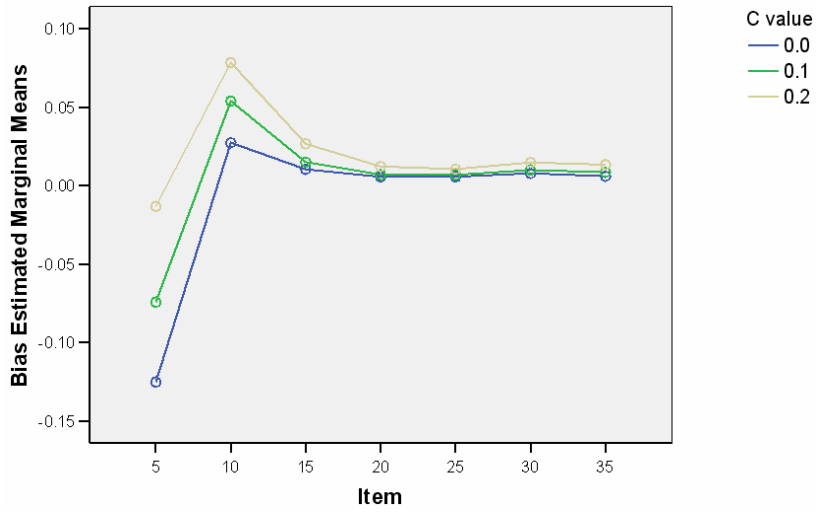


Figure 4.18b. Estimated marginal means for bias between guessing parameter levels for boost conditions.

4.2.7.3 Standard deviation. A significant difference in SD due to the guessing parameter was found ($F(2, 554) = 1983.091$, $p = .000$, partial-$\eta^2 = .877$). This difference was consistent between the null and boost conditions, that is, there was no significant interaction between guessing parameter and the NULLBOOST variable. However, a significant interaction between the change in SD over time and the size of the guessing parameter was found ($F(12, 3324) = 88.191$, $p = .000$, partial-$\eta^2 = .241$). This difference was found to slightly interact with the null and boost conditions variable ($F(12, 3324) = 4.230$, $p = .000$, partial-$\eta^2 = .015$). Overall, the amount of standard deviation was slightly greater for the boost conditions than the null conditions.

Through post hoc comparisons it was found that all parameter levels were significantly different from each other. As can be seen in Figures 4.19a and 4.19b, the higher guessing parameters resulted in a higher level of SD. For both the null and boost conditions, the SD levels out around .20. However, for the first half of the test, consistent with the design of the study, the SD is higher for the boost conditions than for the null conditions.

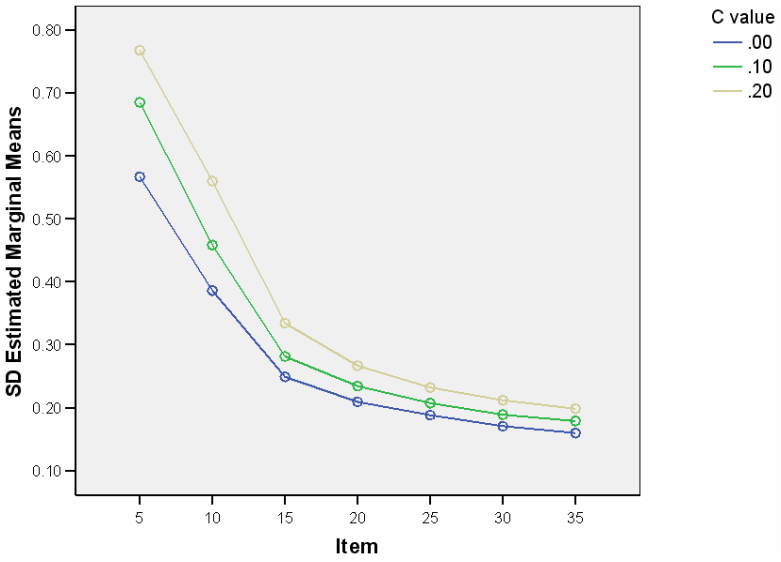


Figure 4.19a. Estimated marginal means for standard deviation between guessing parameter levels for null conditions.

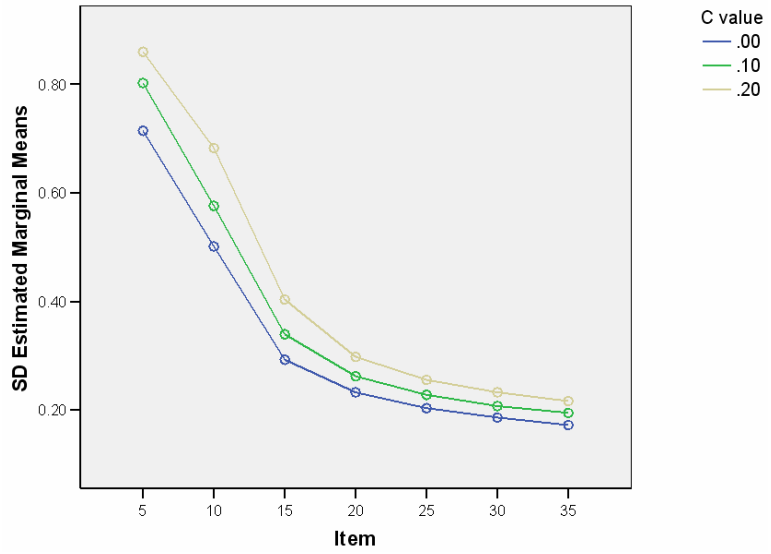


Figure 4.19b. Estimated marginal means for standard deviation between guessing parameter levels for boost conditions.

4.2.8 Differences between True Theta Levels

Differences in the outcome measures over the course of the test between true theta levels were also examined using a repeated-measures factorial ANOVA. As discussed above, there were 29 intervals of theta ranging from -3.25 to 3.25 (interval size = .25).

4.2.8.1 Root mean squared error. A very small, but significant difference was found between true theta levels in root mean squared error ($F(28, 16675) = 20.311$, $p = .000$, partial-$\eta^2 = .033$). This difference was found to significantly interact with the null versus boost conditions variable ($F(28, 16675) = 5.154$, $p = .000$, partial-$\eta^2 = .009$). The effect sizes suggest that the significant differences found were due to sample size. It can be seen in Figures 4.20a and 4.20b that after Item 10, the pattern of RMSE between true theta levels is virtually equal. However, earlier in the test, the more extreme theta levels resulted in more root mean squared error. Also, it should be noted that the boost conditions resulted in smoother lines due to more replications.

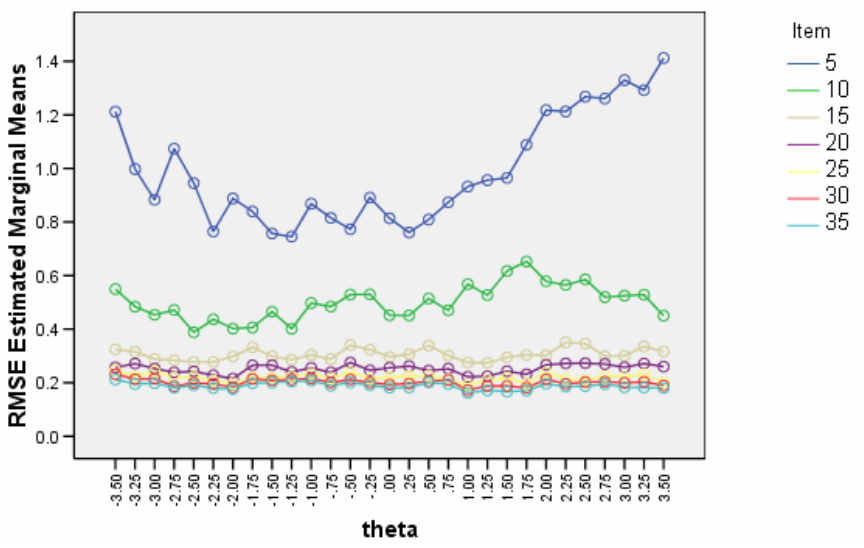


Figure 4.20a. Estimated marginal means for root mean squared error for each true theta level across the test for the null conditions.

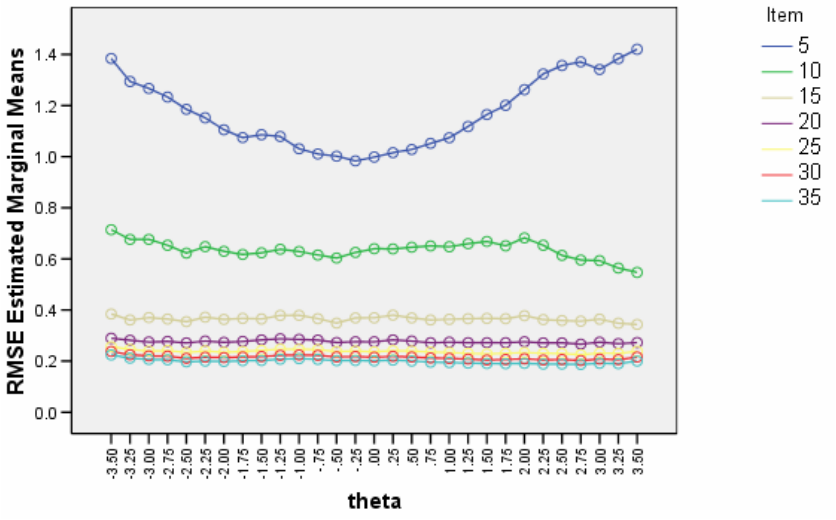


Figure 4.20b. Estimated marginal means for root mean squared error for each true theta level across the test for the boost conditions.

4.2.8.2 Bias. Differences in bias over the course of the test between true theta levels were also examined using a repeated-measures factorial ANOVA. A small, significant difference was found between true theta levels in amount of bias ($F(28, 16675) = 120.319$, $p = .000$, partial-$\eta^2 = .169$). A slightly significant interaction was also found between true theta levels and the null versus boost conditions variable ($F(28, 16675) = 6.959$, $p = .000$, partial-$\eta^2 = .012$). The effect sizes suggest that the differences found were due to sample size rather than the main effects. It can be seen in Figures 4.21a and 4.21b that at Item 5, for both the null and boost conditions, there was regression to the mean in terms of bias. That is, lower theta levels were biased in a positive direction, and higher theta levels were biased in a negative direction. By Item 10, all bias was in the positive direction. By Item 35, there were no noticeable differences in amount of bias between the theta levels. It should also be noted that the amount of bias for the boost conditions is consistently greater than that of the null conditions. Also, it should be noted that the boost conditions resulted in smoother lines due to more replications.

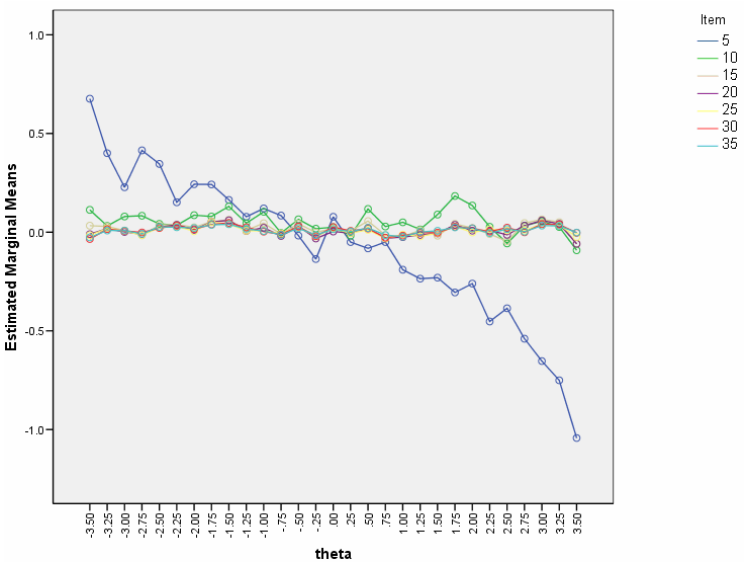


Figure 4.21a. Estimated marginal means for bias throughout the test for each true theta level for the null conditions.

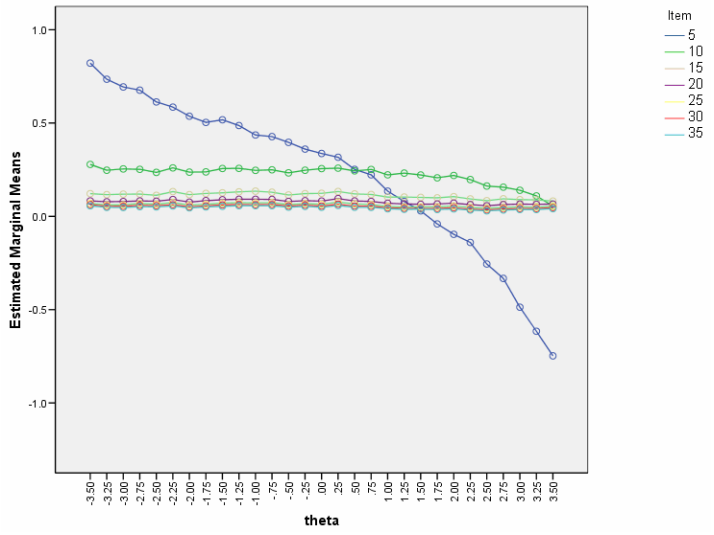


Figure 4.21b. Estimated marginal means for bias throughout the test for each true theta level for the boost conditions.

4.2.8.3 Standard deviation. A very small, but significant difference was found between true theta levels in squared deviation ($F(28, 16675) = 24.562$, $p = .000$, partial-$\eta^2 = .040$). This difference was found to significantly interact with the null versus boost conditions variable ($F(28, 16675) = 6.294$, $p = .000$, partial-$\eta^2 = .011$). Overall, the standard deviation across theta levels is greater for the boost conditions than it is for the null conditions. The effect sizes suggest that the significant differences found were due to sample size. It can be seen in Figures 4.22a and 4.22b that for both the null and boost conditions, there is less standard deviation for the greater theta levels early in the test. At the tenth item, standard deviation becomes virtually equal across theta levels. Also, it should be noted that the boost conditions resulted in smoother lines due to more replications.

These results are discussed in more detail in the next section.

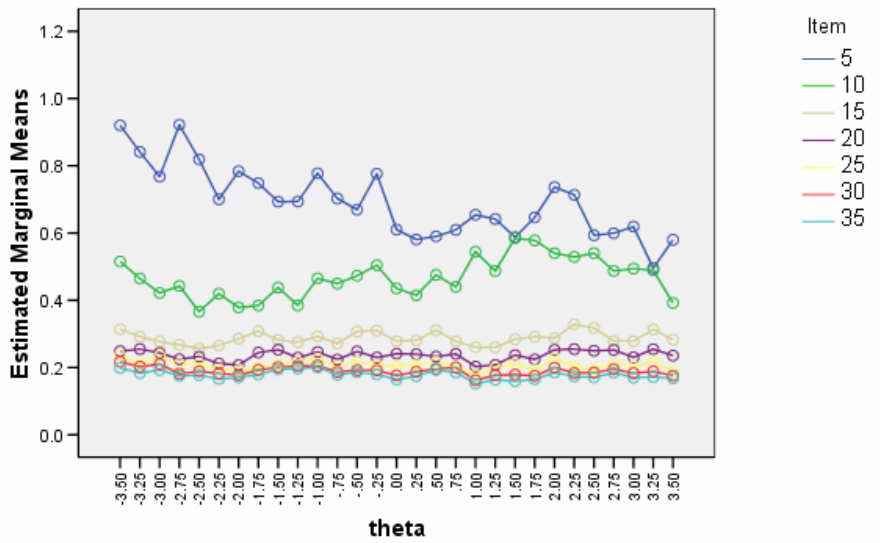


Figure 4.22a. Estimated marginal means for squared deviation throughout the test for each true theta level for the null conditions.

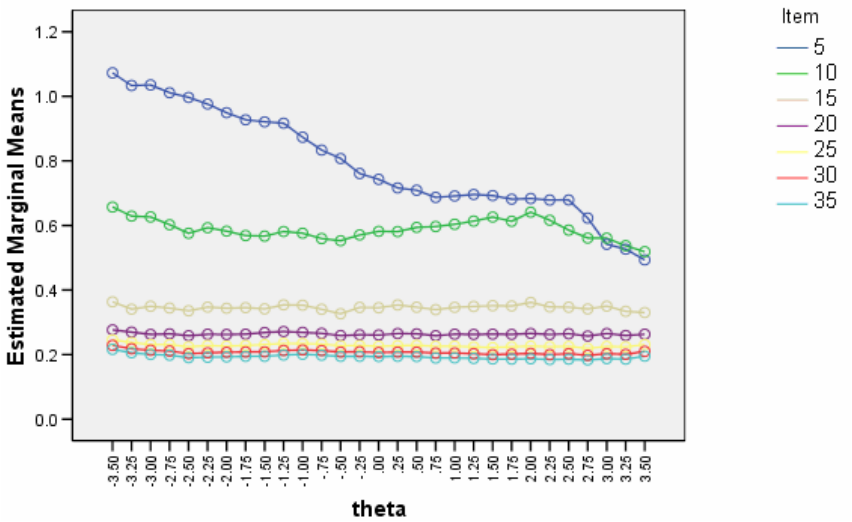


Figure 4.22b. Estimated marginal means for squared deviation throughout the test for each true theta level for the boost conditions.

4.3 Discussion

In this study, possible differences in stability of latent trait estimate due to varying testing parameters were examined to determine the impact of an artificial theta boost for the first 10 items of an exam. Four testing parameters were varied: the starting rule, the size of the item pool, the discriminating power of the items, and the guessing parameter of the items. The patterns of differences within most of these testing parameters were found to be slightly different between the null and the boost conditions. Prior to discussing these testing parameters, the differences between the null and boost conditions are discussed.

As described in Section 4.2.1, on average, the final theta estimates for the boost conditions was about .05 points above the true theta value. As discussed later, it can be seen that this difference reflects the systematic variability (bias) between the boost and null conditions. These results imply that by the end of a CAT, the estimated theta is slightly increased by the artificial boost at the beginning of the test. The boost also impacted the stability of the latent trait estimates. Also, the general pattern of this instability was regression toward an average theta level. That is, examinees with negative theta levels were given much higher estimates for their ability level, while those with very positive theta levels were estimated as much lower. The closer to an average theta level (around zero), the less difference there was between the estimated values and the true values. After the tenth item, the boost in estimated theta level was pretty stable, but small. This suggests that this difference might not be worth worrying about in practice. However, the actual effect of this boost might need to be

taken into account depending on the scale of the actual test used. Inferential tests are now discussed in terms of the three outcome measures.

One important finding to discuss is that the boost conditions resulted in significantly greater total variability (RMSE) than did the null conditions. This effect arose from both systematic variability (bias) and random variability (SD). However, the larger impact on the RMSE found was due to the random variability. Because the boost was randomly varied across replications, it logically follows that this set of conditions would have more random variability than the null set with no variation in boost (i.e., all boosts were equal to zero). Bias is an average measure of the difference between the estimated theta and the true theta. The difference in this measure of bias suggests that boost and null conditions differed in the average disparity between the estimated theta and the true theta by the amount suggested by the descriptive statistics. Also, the pattern of differences due to starting conditions differed only slightly between both the null and boost sets of conditions. The effect size was small enough to suggest that the significant differences found were a function of the large sample sizes.

For all measures of variability, the only starting rule condition that resulted in significantly more variability were the linear fixed test conditions. Within these linear test condition types, all three were significantly different from each other. The medium level difficulty test always resulted from the least amount of variability. This evidence of possible differences in test outcomes when using small fixed tests (also known as testlets) has been addressed before (Wainer & Kiely, 1987). For all other

conditions, the stability of the test over time was the same. This suggests that the starting rule does not affect the outcome of a computer adaptive test. By the twentieth item of the test, the variability measure converged on the same value across all conditions. Also, it should be noted that bias was close to zero after the fifteenth item; most of the RMSE was due to random variability (SD) after the fifteenth item. These results suggest that choosing a linear fixed test that would be too easy for the majority of the population could result in less stable estimates earlier in the test. The main instability is due to systematic variability after the first five items. Overall, these findings suggest that any of the starting rules tested in this study will result in the same outcome measure of theta after about the twentieth item.

The second independent variable in this study was the size of the item pool. The findings of this study suggest that early on, a smaller item pool resulted in more stable estimates, but as that pool gets smaller due to items being removed, the theta estimates become less stable. This is probably due to a lack of items that give maximum information at an estimated theta level after many of the items have been removed. There was no evidence to suggest any stability differences between larger item pool sizes. These findings give support for the argument for large item pools when developing a computerized adaptive test (e.g., Embretson & Reise, 2000). These differences were only apparent for the RMSE and SD measures. There were no differences in bias (systematic variability) due to item pool sizes. It should also be mentioned that these patterns were the same for both null and boost conditions. Thus,

estimates that might be affected by an artificial boost will not differ from those without a boost due to item pool sizes.

The third independent variable of interest was the discrimination parameter of the items. Three variants were tested (a = 1.0, 1.5, or 2.0). As would be expected, the more discriminating items resulted in more stable estimates of theta. The discrimination parameter gives the ability of an item to differentiate examinees with close estimates of ability. It would logically follow that estimates based on more discriminating items would result in more accurate estimates. Early adaptive testing research found evidence to suggest that more discriminating items result in more reliable theta estimates for adaptive tests (Vale & Weiss, 1975). Urry (1974, 1975) found the 3-PL model to be more reliable than the 1-PL model for estimating theta as long as the discrimination parameters were at least greater than .80. Jensema (1974) also found that reliability of a test increased with larger discrimination parameters.

These findings are similar to those by other researchers who suggest using low discriminating items earlier in the test when there is more room for error and highly discriminating items later in the test when the CAT is fine-tuning a score. This helps deal with the issue of item exposure rates (Chang, 1999; Hau & Chang, 2001). The only differences found between the null and boost conditions were in the SD (random variability) and were very slight. These results suggest that the affect that the discrimination parameter has on the stability of the CAT is the same for both null and boost conditions.

Finally, the fourth independent variable of interest was the guessing parameter of the item. This study showed that larger guessing parameters resulted in less stable estimates. Similar to the logic of the discrimination parameter, this makes intuitive sense. If there is a better chance of guessing the correct answer without the true ability to answer correctly, then this should affect the estimate of the ability level. In this study, strong evidence was found to support the idea that creating items with less chance for guessing the item correctly will result in more stable estimates of theta. Jensema (1974) found that the reliability of an adaptive test increased as the size of the guessing parameter decreased.

This inverse relationship between the reliability or stability of an estimate and the guessing parameter is also consistent with the findings of Bridgeman and Cline (2004) that guessing later in the test can decrease an examinee's score. In this study, it was found that higher guessing parameters decreased the stability of a test at all points in the test, which supports their findings that guessing would have a potential to dramatically affect an examinee's final theta estimate. These results in combination with the findings for the discrimination parameter variable suggest that creating items with more discriminating power and less guessing possibility is optimal for stable theta estimates. These findings were consistent for both the null and boost conditions.

Analyses were conducted to look at possible differences in these outcome measures at different levels of true latent trait level. Although the analyses found significant differences on all accounts, effect sizes found suggest that the significance found was due to the number of degrees of freedom (which are a function of the

number of conditions examined) used to determine the significance level. Considering that the simulated item pool was created from a normal distribution of difficulty parameters, this might seem counterintuitive. It could be assumed that since there are fewer items at the more extreme levels of ability, the estimates of trait at these later points in the test would be less stable than those at a more average ability level. However, results from this study found evidence to suggest that this is not the case.

Overall, evidence was found to suggest that given enough items (at least 20 to 25) the maximum likelihood estimation procedure results in stable estimates of theta no matter the starting rule selected. This is consistent with Jensema's (1974) findings that the reliability of an adaptive test is directly related to the length of the test. Also, when developing an item pool for a computerized adaptive test, care should be given to create enough items with good discriminating power and low possibilities for guessing. If the evidence for stability of the estimates leveling out after twenty items is coupled with the findings of Bridgeman and Cline (2004), this would suggest that tests with 35 items could be unnecessarily long. There is also evidence that an artificial boost to an examinee's ability level can affect the outcome estimate of ability level, but whether this difference is large enough is discussed next.

The important question arising from the simulation study is whether or not this boost has a meaningful impact on scores. To examine this, GRE subtest score and selection ratio information was collected from *The Princeton Review Complete Book of Graduate Programs* (Princeton Review, 2005). Of the 1482 graduate programs

outlined in this book, GRE-Quantitative, GRE-Verbal and percent accepted data were only reported for 340 programs. The average GRE score for each subtest and other descriptive data can be found in Table 4.2. In this table, it can be seen for the GRE-V (M = 467, SD = 118) and GRE-Q (M = 591, SD = 148) that more selective schools have higher average scores on both subtests.

Figures 4.23 and 4.24 superimpose the results of bias from a boost on various cutlines for selection. The mean ability estimate obtained for each true theta level from the null and boost conditions is plotted, along with the 67% confidence interval. These means are then compared to the average GRE-V (Figure 4.23) and GRE-Q (Figure 4.24) for schools at certain selection ratio levels. The whiskers for the null conditions are noticeably longer than those for the boost conditions. Considering that there were fewer replications for the null conditions, this should be expected.

For the GRE-V, it can be seen that at a 20% selection rate, the average GRE score falls between the null conditions and the boost conditions. Also, at the 50-90% selection rates, examinees in the null conditions are scoring lower than the average score, and the examinees in the boost conditions are scoring above the average score. Given the frequency data in Table 4.2, this would affect decisions for around 200 schools. For the GRE-Q, it can be seen that this same pattern is occurring for the 30, 50 and 100% selection rate programs. At these three selection rates, there are almost 100 schools affected by this boost difference.

While these data are based on average information, they indicate that this boost can impact selection into graduate programs. Thus, although the boost created

only a small inflation in final score estimates, this slight increase could very well be enough to enhance an applicant's chances of acceptance at a more selective school. Given the high priority placed on test scores in our society for admissions, licensure and scholarship purposes, future research is necessary to support this study.

Table 4.2

Descriptive statistics for average GRE-Q and GRE-V scores for 340 graduate programs broken down by selection ratio

	Selection Ratio	N	Mean	SD	Min.	Max.
GRE-V	0.10	3	609.3	35.92	579	649
	0.20	17	588.3	48.92	528	730
	0.30	29	564.4	43.82	465	621
	0.40	40	542.8	43.31	475	650
	0.50	35	500.9	45.09	377	598
	0.60	53	499.8	54.98	300	601
	0.70	57	495.7	57.61	300	659
	0.80	41	488.1	56.81	323	632
	0.90	33	486.8	47.40	402	581
	1.00	28	472.4	60.97	344	600
GRE-Q	0.10	3	649.0	78.89	600	740
	0.20	17	676.3	64.08	550	773
	0.30	29	636.4	73.58	451	760
	0.40	40	611.2	57.76	500	750
	0.50	35	596.3	80.80	392	757
	0.60	53	577.3	73.56	350	770
	0.70	57	585.6	79.16	380	760
	0.80	41	577.0	88.14	400	728
	0.90	33	531.7	57.29	450	638
	1.00	28	528.9	61.57	376	614

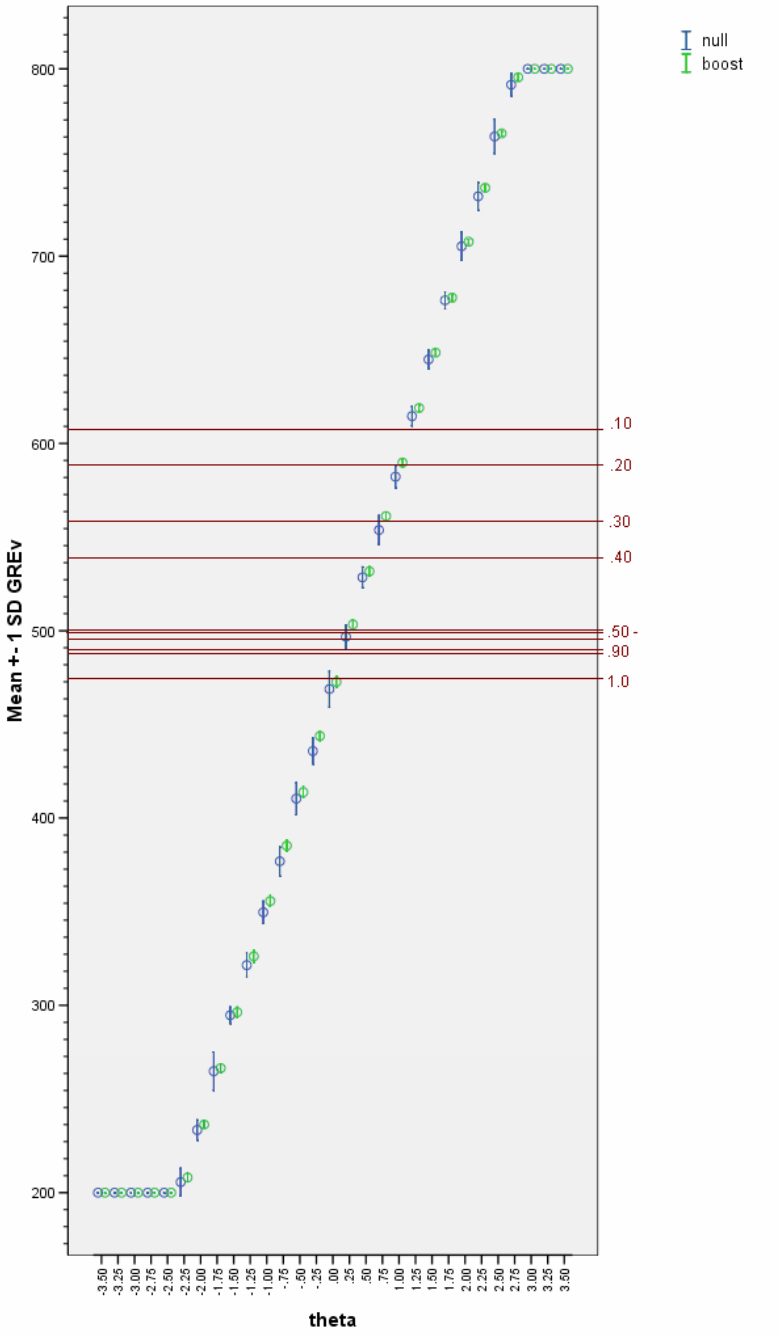


Figure 4.23. GRE-V scores for all true theta values compared to average GRE-V scores corresponding to each college selection ratio.

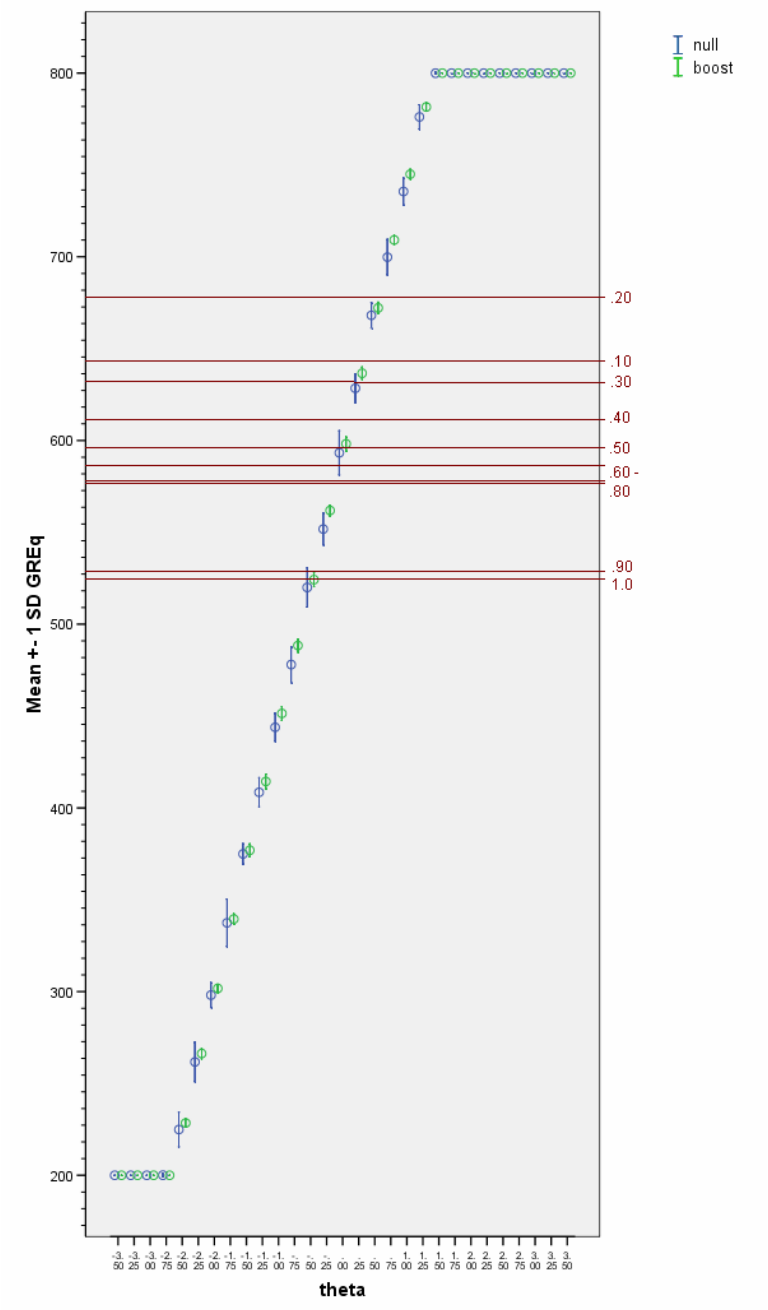


Figure 4.24. GRE-Q scores for all true theta values compared to average GRE-Q scores corresponding to each college selection ratio.

5. Conclusion

In this set of studies, the impact of a test taking strategy on computerized adaptive testing outcomes was examined. The test taking strategy of interest was one that is taught by companies like Kaplan and Princeton Review to help "beat the test." Briefly, the strategy is to spend more time ensuring a correct answer on the first five or ten items to help improve the final estimation of your ability level.

Two studies were conducted to test possible effects of this test taking strategy. The first study looked at real world effects of knowledge of test taking strategies by comparing three groups of examinees who differed only in the amount of instructions they received prior to taking the same computer adaptive test. The second study looked at the effects of an artificial boost in ability on computer adaptive test outcomes under varying test procedure conditions.

In Study 1, differences were found in final trait estimates between conditions that. Those examinees who were taught the test taking strategy performed better than those without this knowledge. The examinees who performed the worst were those who knew how the test works but were given no information on how to "beat" the test. As discussed in Chapter 3, these differences could be due to outside examinee factors like motivation and/or anxiety levels that might result from the knowledge, or lack there of, of this test taking strategy. This is consistent with current literature on test anxiety and motivation.

In Study 2, it was found that test stability patterns only differed slightly between the two types of examinees. There was a significant difference in random

variability of theta estimates between the two groups of examinees, and there were slightly significant differences in systematic variability or bias between the two groups. This suggests that the biggest amount of differences found were due to the variability in the initial theta boost. It was found that examinees with an artificial boost in their ability at the beginning of the test did, on average, have an inflated final theta estimate when compared to examinees without an artificial boost. While this difference was significant, it was small. However, when applying these differences to average GRE scores of graduate programs in the U.S., for schools with selection rations especially around 50-100% this difference could result in the acceptance of some students whose scores are not true reflections of their quantitative or verbal reasoning ability levels.

It was also found in Study 2 that larger item pools resulted in more stable estimates of theta. Items with higher discrimination parameters and lower guessing parameters resulted in more stable estimates. These results are consistent with the literature on computer adaptive testing. Also, if a test developer is interested in using a fixed linear test to begin an adaptive test, it was found in this study that a test with items at a more average difficulty level will result in more stable estimates of theta. Finally, as long as tests are at least 20 items in length, the stability of the theta estimates is virtually equal no matter the starting rule used or the presence of an artificial boost in ability level early in the test.

Separately, these two studies lend to some interesting results. But, combining the findings in Study 1 with those in Study 2 begins to suggest this test taking strategy

on average does result in inflated final ability estimates. It was found in Study 1 that those who knew the test taking strategy had significantly better ability estimates than those who only knew how the test works; in Study 2 that an artificial boost resulted in significantly higher theta estimates. These results support the claim by the test review companies that spending more time at the beginning of the test will increase your score on the test. However, there were limitations in this set of studies that require further attention. These limitations are discussed next.

The first of these limitations is apparent in Study 1. This study lacked the capability to detect if the participants in the third group really followed the test taking strategy explained to them at the beginning of the test. That is, there was no data to see if those examinees spent more time on the first items in the test than the other participants did. So, while there were differences found, there is no way in this study to know for sure that these differences were due to following the test taking strategy taught prior to the test or if they were the effects of lowered anxiety and/or higher motivation levels.

Secondly, this study was limited to the maximum likelihood estimation procedure. This estimation procedure is limited to examinees who answer at least one item correctly and at least one item incorrectly. Also, this is not the only estimation procedure in use by CAT developers and administrators. Future simulation studies should look at other estimation procedures (e.g., EAP).

Finally, only one test taking strategy was examined in this study. In the simulation study, this test-taking strategy was mimicked in only one way. Future

simulation studies should vary how this boost is applied to the true trait level. Future studies should also attempt to isolate other human factors that could affect ability estimates for high-stakes tests. This last point is of greatest importance given the large role high-stakes testing plays in our society today.

References

Arthur, Jr., W., Tubre, T. C., Paul, D. S., & Sanchez-Ku, M. L. (1999). College-sample psychometric and normative data on a short form of the Raven Advanced Progressive Matrices test. *Journal of Psychoeducational Assessment*, *17*, 354-361.

Assessment Systems Corporation. (2002). *FastTEST Professional Testing System, version 1.6*.

Betz, N. E., & Weiss, D. J. (1973). *An empirical study of computer-administered two-stage ability testing* (RR 73-4). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota.

Betz, N. E., & Weiss, D. J. (1974). *Simulation studies of two-stage ability testing* (RR 74-4). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota.

Betz, N. E., & Weiss, D. J. (1975). *Empirical and simulation studies of flexilevel ability testing* (RR 75-3). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota.

Bjorner, J. B., Kosinski, M., & Ware, Jr., J. E. (2003). Calibration of an item pool for assessing the burden of headaches: An application of item response theory to the Headache Impact Test (HIT). *Quality of Life Research, 12*, 913-933.

Bock, R. D. & Mislevy, R. J. (1982). Adaptive EAP estimation of ability in a microcomputer environment. *Applied Psychological Measurement, 6*, 431-444.

Bridgeman, B., & Burton, N. (2005, April). *Does scoring only the hard questions on the SAT make it fairer?* Paper presented at the annual meeting of the American Educational Research Association, Montreal.

Bridgeman, B., & Cline, F. (2004). Effects of differentially time-consuming tests on computer-adaptive test scores. *Journal of Educational Measurement, 41*(2), 137-148.

Burke, H. R. (1958). Raven's progressive matrices: A review and critical evaluation. *Journal of Genetic Psychology, 93*, 199-228.

Carpenter, P. A., Just, M. A., & Shell, P. (1990). What one intelligence test measures: A theoretical account of processing in the Raven Progressive Matrices Test. *Psychological Review, 97,* 404-431.

Chang, H. H., & Ying, Z. (1999). A-stratified multistage computerized adaptive testing. *Applied Psychological Measurement, 23* (3), 211-222.

Cliff, N. (1975). Complete orders from incomplete data: Interactive ordering and tailored testing. *Psychological Bulletin, 82,* 289-302.

Colvin, S. S. (1921). Intelligence and its measurement. *Journal of Educational Psychology, 12,* 136-139.

Cudeck, R., McCormick, J., & Cliff, N. (1980). Implied orders tailored testing: simulation with the Stanford-Binet. *Applied Psychological Measurement, 4,* 157-163.

Davey, T. (2005, April). *An introduction to bin-structured adaptive testing.* Paper presented at the annual meeting of the National Council on Measurement in Education, Montreal.

De Ayala, R. J. (1992). The nominal response model in computerized adaptive testing. *Applied Psychological Measurement, 16,* 327-343.

Dearborne, D. F. (1921). Intelligence and its measurement: A symposium. *Journal of Educational Psychology, 12,* 123-147.

Deng, H., & Ansley, T. (2003, April). *To stratify or not: An investigation of CAT item selection procedures under practical constraints*. Paper presented at the annual meeting of the National Council on Measurement in Education, Chicago, IL.

Dodd, B. G. (1990). The effect of item selection procedure and stepsize on computerized adaptive attitude measurement using the rating scale model. *Applied Psychological Measurement, 14,* 355-366.

Elliot, E. S., & Dweck, C. S. (1988). Goals: An approach to motivation and achievement. *Journal of Personality and Social Psychology, 54* (1), 5-12.

Embretson, S. E. (1992). Measuring and validating cognitive modifiability as an ability: A study in the spatial domain. *Journal of Educational Measurement, 29* (1), 25-50.

Embretson, S. E. (1995). Working memory capacity versus general central processes in intelligence. *Intelligence, 20,* 169-189.

Embretson, S. E. (1998). A cognitive design system approach to generating valid tests: Application to abstract reasoning. *Psychological Methods, 3*(3), 380-396.

Embretson, S. E., & Reise S. P. (2000). *Item response theory for psychologists.* Mahwah, NJ: Lawrence Erlbaum Associates, Publishers

Erdfelder, E., Faul, F., & Buchner, A. (1996). GPOWER: A general power analysis program. *Behavior Research Methods, Instruments, & Computers, 28,* 1-11.

Freedle, R. O. (2003). Correcting the SAT's ethnic and social-class bias: A method for reestimating SAT scores. *Harvard Educational Review, 73*(1), 1-43.

Green, B. F., Bock, R. D., Humphreys, L. G., Linn, R. L., & Reckase, M. D. (1984). Technical guidelines for assessing computerized adaptive tests. *Journal of Educational Measurement, 21,* 347-360.

Green, B. F., Jr. (1983). The promise of tailored tests. In H. Wainer & S. Messick (Eds.), *Principals of modern psychological measurement.* Hillsdale, NJ: Lawrence Earlbaum Associates.

Guilford, J. P. (1982). Cognitive psychology's ambiguities: Some suggested remedies. *Psychological Review*, *89*, 48-59.

Guo, F., & Wang., L. (2003, April). *Online calibration and scale stability of a CAT program*. Paper presented at the annual meeting of the National Council on Measurement in Education, Chicago, IL.

Hambleton, R. K., Swaminathan, H., & Rogers, H. J. (1991). *Fundamentals of item response theory.* Newbury Park, CA: SAGE Publications, Inc.

Hambleton, R. K., Zaal, J. N. & Pieters, J. P. M. (1991). Computerized adaptive testing: Theory, application, and standards. In R. K. Hambleton & J. N. Zaal (Eds.), *Advances in educational and psychological testing: Theory and applications.* Boston, MA: Kluwer Academic Publishers.

Hansen, D. N. (1969). An investigation of computer-based science testing. In R. C. Atkinson & H. A. Wilson (Eds.), *Computer-assisted instruction: A book of readings* (pp. 209-226). New York: Academic Press.

Hau, K. T., & Chang, H. H. (2001). Item selection in computerized adaptive testing: Should more discriminating items be used first? *Journal of Educational Measurement, 38* (3), 249-266.

Henmon, V. A. C. (1921). Intelligence and its measurement: A symposium. *Journal of Educational Psychology, 12,* 195-198.

Hornke, L. F. & Habon, M. W. (1986). Rule-based item bank construction and evaluation within the linear logistic framework. *Applied Psychological Measurement, 10*(4), 369-380.

Hulin, C. L., Drasgow, F., & Parsons, C. K. (1983). *Item response theory: Application to psychological measurement.* Homewood, IL: Dow-Jones Irwin.

Hunt, E. B. (1974). Quote the Raven? Nevermore. In L. W. Gregg (Ed.), *Knowledge and cognition*. Hillsdale, NJ: Erlbaum. 129-158.

Jensema, C. J. (1974). An application of latent trait mental test theory. *British Journal of Mathematical Statistical Psychology, 27,* 29-48.

Jodoin, M. G. (2005, April). *A comparison of linear fixed, multi stage and adaptive test designs.* Paper presented at the annual meeting of the American Educational Research Association, Montreal.

Kaplan. (1997). *GRE: Power strategies to help you score higher, 1997-98 Ed.* New York: Kaplan Educational Centers and Simon & Schuster.

Kim, J., & McLean, J. E. (1995, April). *The influences of examinee test-taking motivation in computerized adaptive testing.* Paper presented at the annual meeting of the National Council on Measurement in Education, San Francisco, CA.

Kingsbury, G. G. & Zara, A. R. (1991). A comparison of procedures for content-sensitive item selection in computerized adaptive tests. *Applied Measurement in Education, 4,* 241-261.

Krathwol, D. R. & Huyser, R. J. (1956). The sequential item test (SIT). *American Psychologist, 2,* 419.

Larkin, K. C., & Weiss, D. J. (1974). *An empirical investigation of computer-administered pyramidal ability testing* (RR 74-3). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota. (NTIS No. AD-A006 733)

Lord, F. M. (1970). Some test theory for tailored testing. In W. Holtzman (Ed.), *Computer-assisted instruction, testing, and guidance* (pp. 139-183). New York, NY: Harper & Row.

Lord, F. M. (1971a). The self-scoring flexilevel test. *Journal of Educational Measurement, 8,* 147-151.

Lord, F. M. (1971b). A theoretical study of two-stage testing. *Psychometrika, 36,* 227-241.

Lord, F. M. (1977). A broad range tailored test of verbal ability. *Applied Psychological Measurement,* 95-100.

Lord, F. M. (1980). *Application of item response theory to practical testing problems.* Hillsdale, NJ: Lawrence Erlbaum Associates.

Lu, Y., Pitoniak, M., Rizavi, S., Way, W. D., & Steffen, M. (2003, April). *Evaluating computer adaptive testing design for the MCAT with realistic simulated data.* Paper presented at the annual meeting of the National Council on Measurement in Education, Chicago, IL.

Luecht, R. M., & Nungester, R. (1998). Some practical examples of computer-adaptive sequential testing. *Journal of Educational Measurement, 35,* 239-249.

Maier, C. (2005). *Complete Book of Graduate Programs, 2006 Ed.* New York, NY: The Princeton Review, Inc.

Martin, C. J., & Hoshaw, C. R. (2001). Policy and program management perspective. In W. Sands, B. K. Waters, and J. R. McBride (Eds.), *Computerized adaptive testing: From inquiry to operation* (pp. 11-20). Washington, DC: American Psychological Association.

McBride, J. R. (2001a). Technical Perspective. In W. Sands, B. K. Waters, and J. R. McBride (Eds.), *Computerized adaptive testing: From inquiry to operation* (pp. 29-44). Washington, DC: American Psychological Association.

McBride, J. R. (2001b). Research antecedents of applied adaptive testing. In W. Sands, B. K. Waters, and J. R. McBride (Eds.), *Computerized adaptive testing: From inquiry to operation* (pp. 47-57). Washington, DC: American Psychological Association.

McBride, J. R. (2001c). The marine corps exploratory development project: 1977-1982. In W. Sands, B. K. Waters, and J. R. McBride (Eds.), *Computerized adaptive testing: From inquiry to operation* (pp. 59-67). Washington, DC: American Psychological Association.

McBride, J. R., & Martin, J. T. (1983). Reliability and validity of adaptive ability tests in a military setting. In D.J. Weiss (Ed.), *New horizons in testing: Latent trait theory and computerized adaptive tests* (pp. 223-236). New York: Academic Press.

McBride, J. R., & Weiss, D. J. (1976). *Some properties of a Bayesian adaptive ability testing strategy* (RR 76-4). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota. (NTIS No. AD-A022 964)

McBride, J. R., Wetzel, C. D., & Hetter, R. D. (2001). Preliminary psychometric research for CAT-ASVAB: Selecting an adaptive testing strategy. In W. Sands, B. K. Waters, and J. R. McBride (Eds.), *Computerized adaptive testing: From inquiry to operation* (pp. 83-95). Washington, DC: American Psychological Association.

Mead, A. D. & Drasgow, F. (1993). Equivalence of computerized and paper-and-pencil cognitive ability tests: A meta-analysis. *Psychological Bulletin, 114,* 449-458.

Meijer, R. R. & Nering, M. L. (1999). Computerized adaptive testing: Overview and introduction. *Applied Psychological Measurement, 23*(3), 187-194.

Moreno, K. E., Wetzel, C. D., McBride, J. R., & Weiss, D. J. (1983). *Relationship between corresponding Armed Services Vocational Aptitude Battery (ASVAB) and computerized adaptive testing (CAT) subtests* (NPRDC TR 83-27). San Diego, CA: Navy Personnel Research and Development Center. (NTIS AD-A131 683)

Naglieri, J. A., & Das, J. P. (1997). *Das-Naglieri cognitive assessment system.* Itasca, IL: Riverside Publishing.

Neuman, G., & Baydoun, R. (1998). Computerization of paper-and-pencil tests: When are they equivalent? *Applied Psychological Measurement, 22,* 71-83.

Owen, R. J. (1969). *A Bayesian approach to tailored testing* (RB-69-92). Princeton, NJ: Educational Testing Services.

Owen, R. J. (1975). A Bayesian sequential procedure for quantal response in the context of adaptive mental testing. *Journal of the American Statistical Association, 70,* 351-356.

Pintner, R. (1927). *Intelligence testing: Methods and results.* New York: H. Holt and Company.

Powers, D. E. (2001). Test anxiety and test performance: Comparing paper-based and computer-adaptive versions of the Graduate Record Examination (GRE) General Test. *Journal of Educational Computing Research, 24* (3), 249-273.

Prabhakaran, V., Smith, J. A. L., Desmond, J. E., Glover, G. H., & Gabrieli, J. D. E. (1997). Neural substrates of fluid reasoning: An fMRI study of neocortical activation during performance of the Raven's Progressive Matrices Test. *Cognitive Psychology, 33*(1), 43-63.

Raven, J. (2000). The Raven's Progressive Matrices: Change and stability over culture and time. *Cognitive Psychology, 41*, 1-48.

Raven, J. C. (1962). *Advanced progressive matrices Sets I and II.* London: HK Lewis.

Raven, J. C., & Raven, J. (1976). *Advanced progressive matrices.* Oxford: Oxford Psychologists Press.

Ree, M. J. (1977). Implementation of a model adaptive testing system at an Armed Forces Entrance and Examining Station. In D. J. Weiss (Ed.), *Proceedings of the 1977 Computerized Adaptive Testing Conference* (pp. 216-220). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota.

Schaeffer, G. A., Bridegman, B., Golub-Smith, M. L., Lewis, T. P., & Steffen, M. (1998). *Comparability of paper-and-pencil and computer adaptive test scores on the GRE General Test* (RR 98-38). Princeton, NJ: Educational Testing Services. (GRE Board Professional Report No. 95-08P)

Schaeffer, G. A., Steffen, M., Golub-Smith, M. L., Mills, C. N., & Durso, R. (1995). *The introduction and comparability of the computer adaptive GRE General Test* (RR 95-20). Princeton, NJ: Educational Testing Services. (GRE Board Professional Report No. 88-08aP)

Schoonman, W. (1989). *An applied study on computerized adaptive testing.* Amsterdam: Swets & Zeitlinger B.V.

Sellman, W. S. & Arabian, J. M. (2001). Foreword. In W. Sands, B. K. Waters, and J. R. McBride (Eds.), *Computerized adaptive testing: From inquiry to operation* (pp. xv-xvii). Washington, DC: American Psychological Association.

Shermis, M. D., & Lombard, D. (1998). Effects of computer-based test administration on test anxiety and performance. *Computers in Human Behavior, 14* (1), 111-123.

Spearman, C. (1927). *The abilities of man*. New York: Macmillan.

Spray, J. A., Ackerman, T. A., Reckase, M. D., & Carlson, J. E. (1989). Effect of the medium of item presentation on examinee performance and item characteristics. *Journal of Educational Measurement, 26,* 261-271.

Sternberg, R. J. (1985). General intellectual ability. In R. J. Sternberg (Ed.) *Human abilities: An information processing approach.* New York: W.H. Freeman and Company, 5-29.

Still, C. (2003). *The Princeton Review Crash Course for the GMAT: The Last-Minute Guide to Scoring High, 2nd Ed* (pp. 10-11). New York: Random House, Inc.

Stocking, M. L., & Swanson, L. (1993). A method for severely constrained item selection in adaptive testing. *Applied Psychological Measurement, 17,* 277-292.

Straetmans, G. J. J. M., & Eggen, T. J. H. M. (1998). Computerized adaptive testing: What it is and how it works. *Educational Technology, January-February,* 45-52.

Sympson, J. B., & Hetter, R. D. (1985). Controlling item-exposure rates in computerized adaptive testing. *Proceedings of the 27th annual meeting of the Military Testing Association* (pp. 973-977). San Diego, CA: Navy Personnel Research and Development Center.

Terman, L. M. (1916). *The measurement of intelligence.* Boston, MA: Houghton Mifflin.

Theunissen, T. J. J. M. (1986). Some applications of optimization algorithms in test design and adaptive testing. *Applied Psychological Measurement, 10,* 381-389.

Thissen, D., & Mislevy, R. J. (2000). Testing algorithms. In H. Wainer (Ed.), *Computer adaptive testing: A primer, 2nd Ed.* (pp. 101-133). Mahwah, NJ: Lawrence Erlbaum Associates.

Thorndike, E. L., Bregman, E. O., Cobb, M. V., & Woodyard, E. (1927). *The measurement of intelligence.* New York: Bureau of Publications.

Thurstone, L. L. (1938). *Primary mental abilities.* Chicago: University of Chicago Press.

Urry, V. W. (1974). Approximations to item parameters of mental test models and their uses. *Educational and Psychological Measurement, 34,* 253-269.

Urry, V. W. (1977). Tailored testing: A successful application of item response theory. *Journal of Educational Measurement, 14,* 181-196.

Urry, V. W. (1983). *Tailored testing and practice: A basic model, normal ogive models, and tailored testing algorithms.* Washington, DC: Office of Personnel Management.

Vale, C. D. (1975). Problem: Strategies of branching through an item pool. In D. J. Weiss (Ed.), *Computerized adaptive trait measurement: Problems and prospects* (RR 75-5). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota. (AD-A018 675)

Vale, C. D., & Weiss, D. J. (1975). *A simulation study of stradaptive ability testing* (RR 75-6). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota. (AD-A020 961)

Verguts, T. & De Boeck, P. (2002). The induction of solution rules in Raven's Progressive Matrices Test. *European Journal of Cognitive Psychology, 14*(4), 521-547.

Vispoel, W. P. (1998). Psychometric characterisitcs of computer-adaptive and self-adaptive vocabulary tests: The role of answer feedback and test anxiety. *Journal of Educational Measurement, 35* (2), 155-167.

Wainer, H. (2000). Introduction and History. In H. Wainer (Ed.), *Computerized adaptive testing: A primer, 2nd Ed* (pp. 1-22). Mahwah, NJ: Lawrence Erlbaum Associates.

Wainer, H., & Kiely, G. L. (1987). Item clusters and computerized adaptive testing: A case for testlets. *Journal of Educational Measurement, 24* (3), 185-201.

Wainer, H., & Mislevy, R. J. (2000). Item Response Theory, item calibration, and proficiency estimation. In H. Wainer (Ed.), *Computerized adaptive testing: A primer, 2nd Ed* (pp. 23-36). Mahwah, NJ: Lawrence Erlbaum Associates.

Wainer, H., & Thissen, D. (1987). Estimating ability with the wrong model. *Journal of Educational Statistics, 12,* 339-368.

Ward, J., & Fitzpatrick, T. F. (1973). Characteristics of matrices items. *Perceptual and Motor Skills, 36*, 987-993.

Waters, B. K. (1974). *An empirical investigation of the stradaptive testing model for the measurement of human ability.* Unpublished doctoral dissertation, Florida State University, Tallahassee, FL.

Weiss, D. J. (1974). *The stratified adaptive computerized ability test* (RR 73-3). Minneapolis, MN: Psychometric Methods Program, Department of Psychology, University of Minnesota. (AD 768 376)

Weiss, D. J. (1982). Improving measurement quality and efficiency with adaptive testing. *Applied Psychological Measurement, 6,* 473-492.

Wise, S. L., & Kingsbury, G. G. (2000). Practical issues in developing and maintaining a computerized adaptive testing program. *Psicologica, 21,* 135-155.

Appendix A

Instructions for Conditions

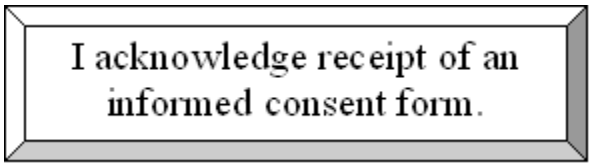
Introduction – Condition 1 – Screen 1

Thank you for agreeing to participate in this study on abstract reasoning ability. This is a computer adaptive test that measures your ability to reason on a nonverbal matrix completion task.

The test you are about to take has 25 matrix completion items. Before beginning the test, you will be given examples of the rules necessary to complete all items in the test. You will also be given an example item to practice these rules before beginning the test.

You will not be allowed to skip any items or return to any items you have already answered.

Please click on the button below to acknowledge that you have signed and received a copy of the informed consent form with information on how to contact the researchers in this study at any point after completion of this study. Then, click "Next" in the upper, right-hand corner of the screen to move on.



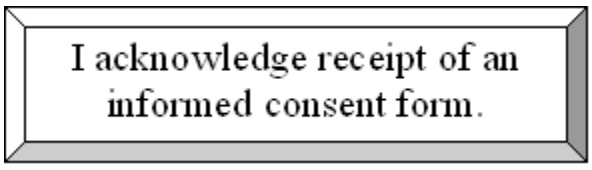
Introduction – Condition 2 – Screen 1

Thank you for agreeing to participate in this study on abstract reasoning ability. This is a computer adaptive test that measures your ability to reason on a nonverbal matrix completion task.

The test you are about to take is computer adaptive and has 25 matrix completion items. Before beginning the test, you will be given examples of the rules necessary to complete all items in the test. You will also be given an example item to practice these rules before beginning the test.

A computer adaptive test "adapts" itself to test takers by selecting the next item to be presented on the basis of performance on preceding items. This means that each item you receive on the test is chosen from a large number of items and the choice is made based on whether you answered previous questions correctly or incorrectly. This also means that the items you receive may not be the same items or they may not be given to you in the same order as others taking this test. Because of the nature of this test, you will not be allowed to skip any items or return to any items you have already answered.

Please click on the button below to acknowledge that you have signed and received a copy of the informed consent form with information on how to contact the researchers in this study at any point after completion of this study. Then, click "Next" in the upper, right-hand corner of the screen to move on.



Introduction – Condition 3 – Screen 1

Thank you for agreeing to participate in this study on abstract reasoning ability. This is a computer adaptive test that measures your ability to reason on a nonverbal matrix completion task.

The test you are about to take is computer adaptive and has 25 matrix completion items. Before beginning the test, you will be given examples of the rules necessary to complete all items in the test. You will also be given an example item to practice these rules before beginning the test.

A computer adaptive test "adapts" itself to test takers by selecting the next item to be presented on the basis of performance on preceding items. This means that each item you receive on the test is chosen from a large number of items and the choice is made based on whether you answered previous questions correctly or incorrectly. This also means that the items you receive may not be the same items or they may not be given to you in the same order as others taking this test. Because of the nature of this test, you will not be allowed to skip any items or return to any items you have already answered.

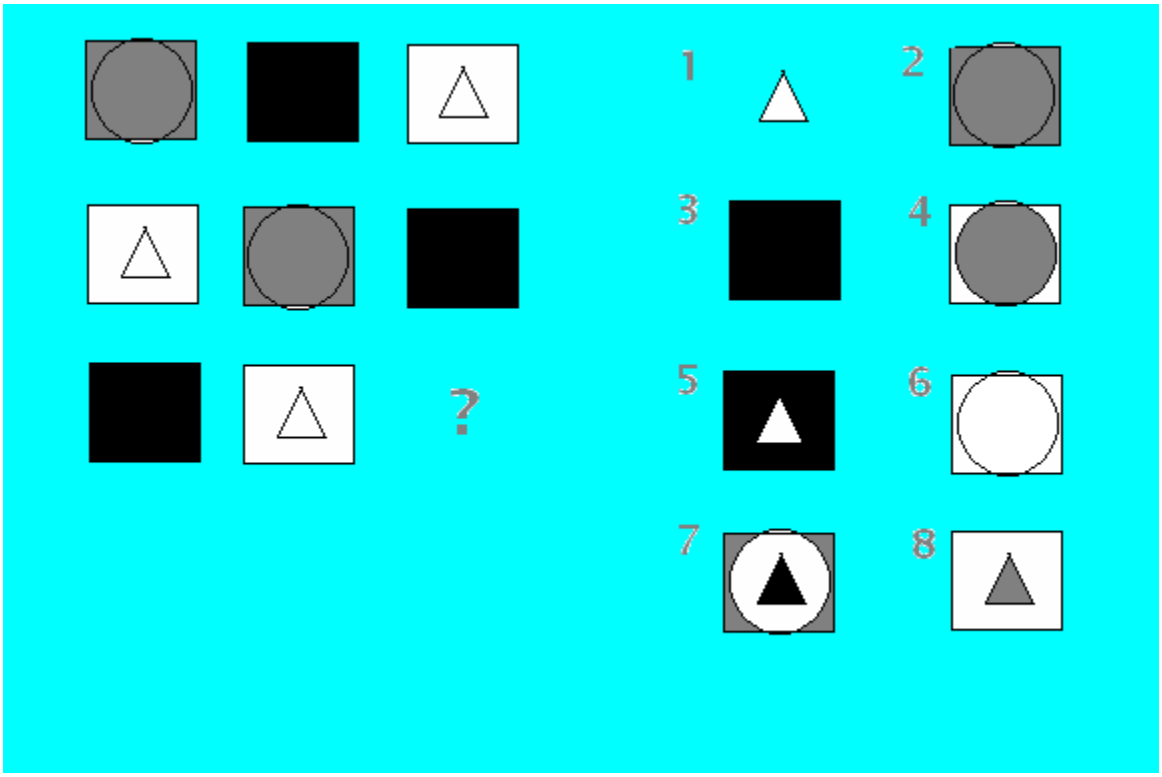
One test taking strategy that you should keep in mind when taking this test is as follows... Because of the nature of a computer adaptive test, you will want to spend time and concentration on the first ten questions of the test. The reason for this is that a computer adaptive test relies heavily on the first ten questions in determining your score. This is because the computer knows nothing about your ability before you start the test. Because of the short length of this test, the needs to use pretty big jumps in judgment in the first ten questions and then use the remaining questions to "fine-tune" your score.

Please click on the button below to acknowledge that you have signed and received a copy of the informed consent form with information on how to contact the researchers in this study at any point after completion of this study. Then, click "Next" in the upper, right-hand corner of the screen to move on.

I acknowledge receipt of an informed consent form.

Instructions – Screen 2

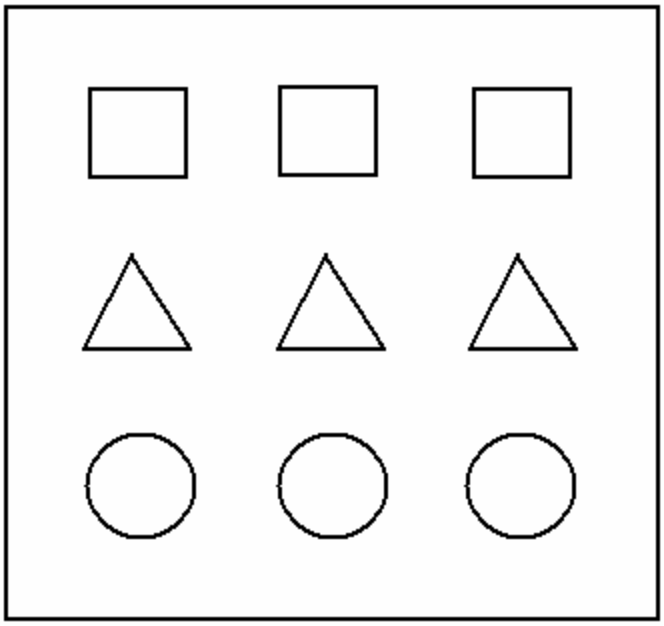
The matrix completion items you will be given throughout this test are similar the one below. There are three rows and three columns of pictures that follow particular patterns. You must first figure out the pattern then choose the correct answer (numbered 1 through 8) that fulfills this pattern to fill in the ninth spot (represented by the "?").



Please click "Next" to learn the rules necessary to complete these items.

Instructions – Screen 3

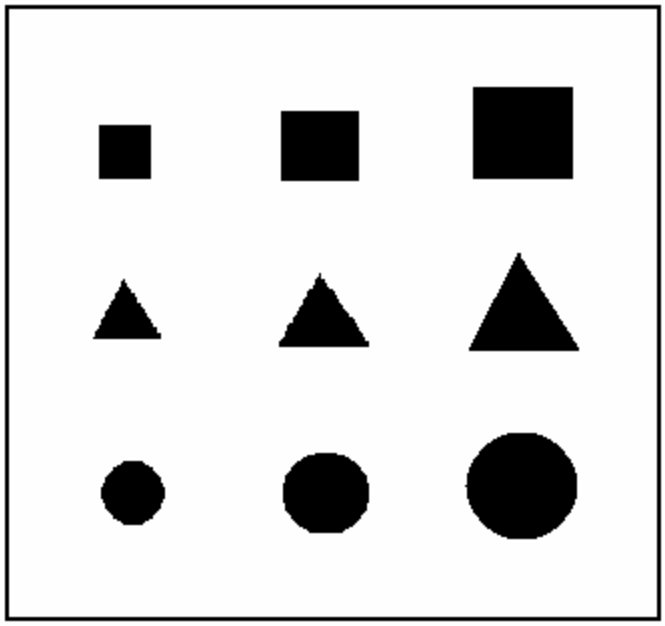
The first rule is known as Identity. This rule states that the same element is found across the rows and/or down the columns. As in the picture below, there is a square in every element of the first row, a triangle in every element of the second row and a circle in every element of the third row. This rule can be applied down the columns as well if there were a same attribute down every element of a column.



Please click "Next" to move on to the next rule.

Instructions – Screen 4

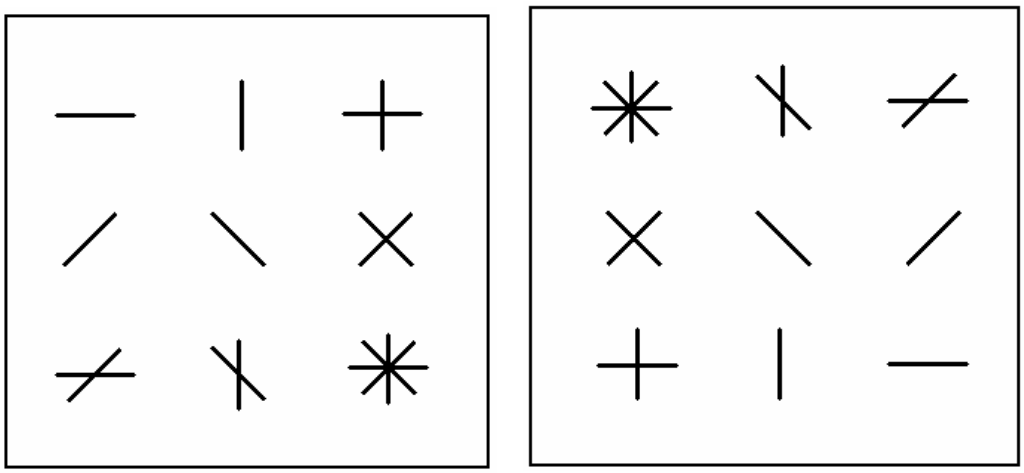
The second rule for these items is Progression. As can be seen in the example below, this rule implies a change in the attribute from the first to the third element in that row or column. For this example, the shapes get bigger as you move from left to right. This rule could consist of changes in number of shapes (e.g., one square to three squares) or shade of the shape (e.g., white to grey to black).



Please click "Next" to move on to the next rule.

Instructions – Screen 5

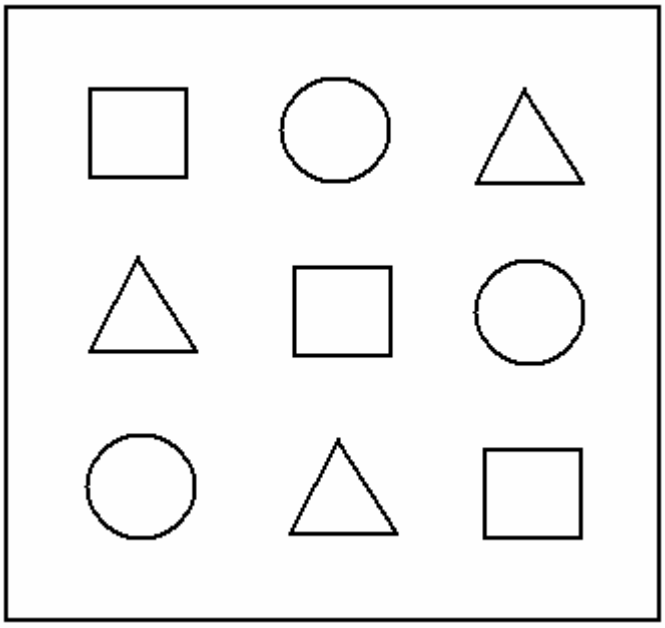
The third rule for these items is Addition/Subtraction. As can be seen in the first example below, this rule results in adding the attributes in each of the first elements of each row AND column to create the third element (e.g., the horizontal line added to the vertical line creates the plus sign at the end of the first row). Or, in the second example, looking at the first column, if you take the asterisk looking element, and remove from it the diagonal lines (the second element in the column), the resulting element is the plus sign.



Please click "Next" to move on to the next rule.

Instructions – Screen 6

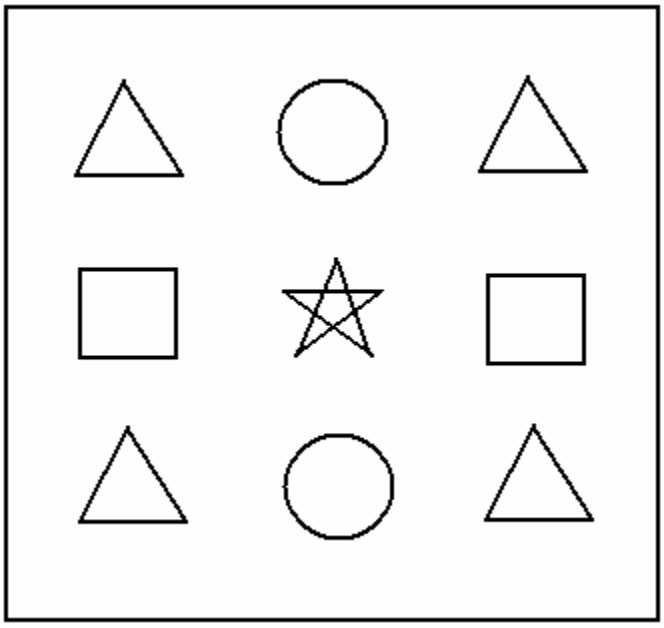
The fourth rule for these items is Distribution of 3. As can be seen in the example below, each row and column contains one of three attributes that are evenly distributed over every row and column. Each row and column contains one square, one circle and one triangle.



Please click "Next" to move on to the final rule.

Instructions – Screen 7

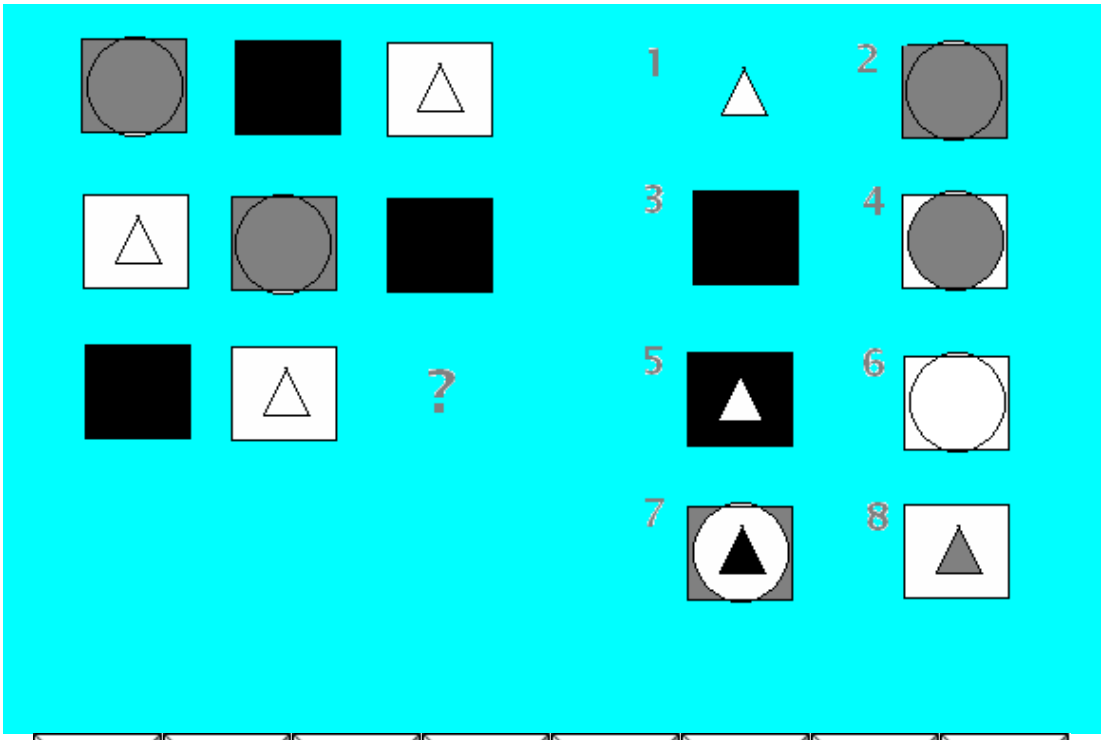
The final rule for these items is Distribution of 2. As can be seen in the example below, each row and column has two elements with identical attributes and the third element consists of some contrasting attribute. For example, the first row consists of two elements with triangles and the third element contains a circle.

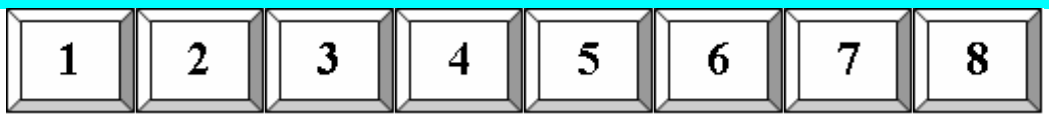


Please click "Next" to move on to an example item.

Instructions – Screen 8

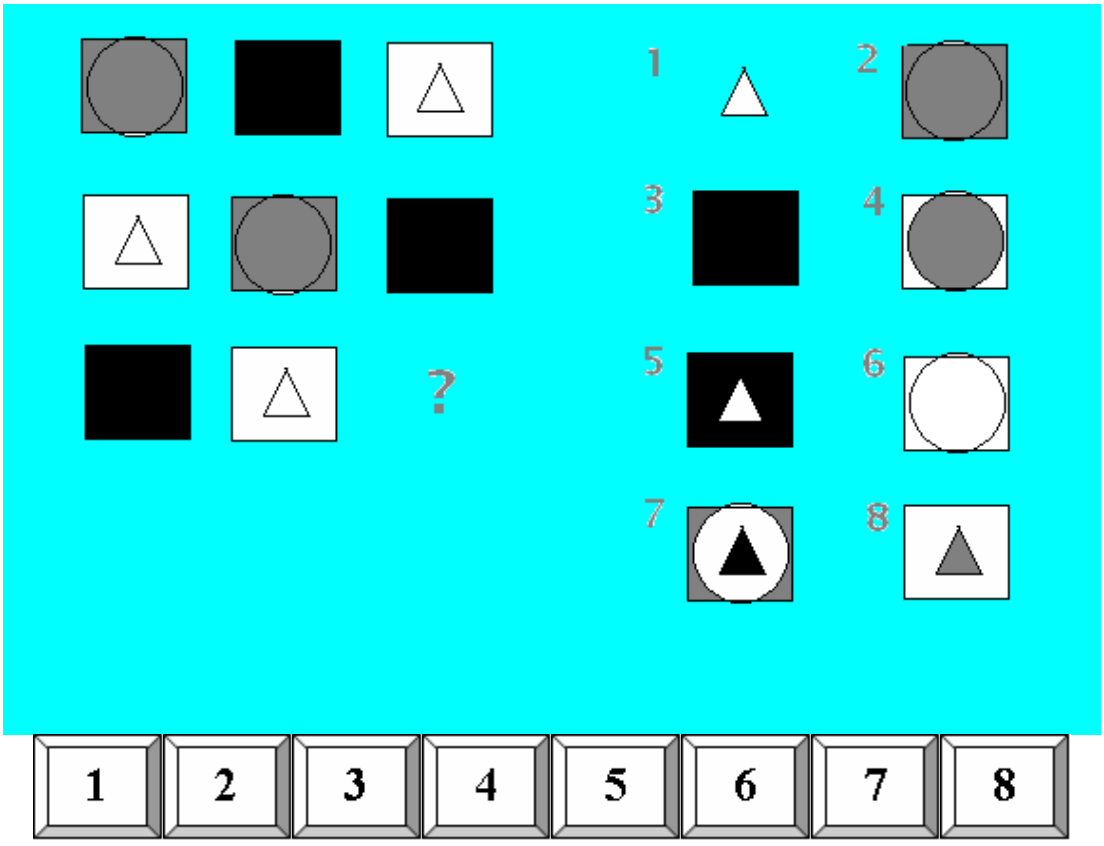
Given what you know about the rules, attempt to solve the item below. Once you have decided on an answer, use your mouse to click on the numbered button below the item.





Instructions – Screen 9

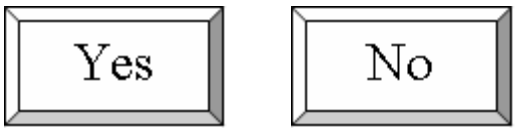
If you answered 2, you are correct. Remembering the rules from earlier, we can see that the identity rule is used (a square in every row and column). As well, there are two Distribution of 3 rules in play. Each row and column has one element that's grey, one that's white and one that's black. As well, each row and column has one element containing a circle, one containing a square and one containing a triangle.



Instructions – Survey Question 1 – Screen 10

Have you ever taken a course to improve your score on a major test (i.e., the GRE, SAT, ASVAB, etc.) in which they discussed test taking strategies for computer adaptive tests?

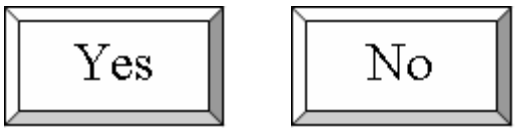
Please use your mouse to choose "Yes" or "No" below, then click "Next".



Instructions – Survey Question 1 – Screen 11

Have you ever taken a computer adaptive test before (e.g., the GRE, ASVAB, etc.)?

Please use your mouse to choose "Yes" or "No" below, then click "Next".



Appendix B

Means (and standard deviations) for theta boosts for each level of theta

Theta	Item1	Item2	Item3	Item4	Item5	Item6	Item7	Item8	Item9	Item10
-3.50	.687	.343	.229	.172	.137	.114	.098	.086	.076	.069
	(.650)	(.325)	(.217)	(.163)	(.130)	(.108)	(.093)	(.081)	(.072)	(.065)
-3.25	.675	.337	.225	.169	.135	.112	.096	.084	.075	.068
	(.618)	(.309)	(.206)	(.154)	(.124)	(.103)	(.088)	(.077)	(.069)	(.062)
-3.00	.670	.335	.223	.167	.134	.112	.096	.084	.074	.067
	(.577)	(.289)	(.192)	(.144)	(.115)	(.096)	(.082)	(.072)	(.064)	(.058)
-2.75	.690	.345	.230	.173	.138	.115	.099	.086	.077	.069
	(.653)	(.326)	(.218)	(.163)	(.131)	(.109)	(.093)	(.082)	(.073)	(.065)
-2.50	.653	.327	.218	.163	.131	.109	.093	.082	.073	.065
	(.708)	(.354)	(.236)	(.177)	(.142)	(.118)	(.101)	(.089)	(.079)	(.071)
-2.25	.682	.341	.227	.170	.136	.114	.097	.085	.076	.068
	(.614)	(.307)	(.205)	(.154)	(.123)	(.102)	(.088)	(.077)	(.068)	(.061)
-2.00	.667	.334	.222	.167	.134	.111	.095	.083	.074	.067
	(.676)	(.338)	(.225)	(.169)	(.135)	(.113)	(.097)	(.085)	(.075)	(.068)
-1.75	.675	.338	.225	.169	.135	.113	.096	.084	.075	.068
	(.627)	(.313)	(.209)	(.157)	(.125)	(.104)	(.090)	(.078)	(.070)	(.063)
-1.50	.697	.349	.232	.174	.139	.116	.100	.087	.078	.070
	(.602)	(.301)	(.201)	(.150)	(.120)	(.100)	(.086)	(.075)	(.067)	(.060)
-1.25	.667	.334	.222	.167	.133	.111	.095	.083	.074	.067
	(.580)	(.290)	(.193)	(.145)	(.116)	(.097)	(.083)	(.072)	(.064)	(.058)
-1.00	.662	.331	.221	.165	.132	.110	.095	.083	.074	.066
	(.633)	(.317)	(.211)	(.158)	(.127)	(.106)	(.090)	(.079)	(.070)	(.063)
-.75	.693	.347	.231	.173	.139	.116	.099	.087	.077	.069
	(.703)	(.352)	(.234)	(.176)	(.141)	(.117)	(.100)	(.088)	(.078)	(.070)
-.50	.705	.352	.235	.176	.141	.118	.101	.088	.078	.071
	(.716)	(.358)	(.239)	(.179)	(.143)	(.119)	(.102)	(.089)	(.080)	(.072)
-.25	.668	.334	.223	.167	.134	.111	.096	.084	.074	.067
	(.629)	(.314)	(.210)	(.157)	(.126)	(.105)	(.090)	(.079)	(.070)	(.063)
.00	.691	.345	.230	.173	.138	.115	.099	.086	.077	.069
	(.622)	(.311)	(.207)	(.156)	(.124)	(.104)	(.089)	(.078)	(.069)	(.062)
.25	.720	.360	.240	.180	.144	.120	.103	.090	.080	.072
	(.665)	(.332)	(.222)	(.166)	(.133)	(.111)	(.095)	(.083)	(.074)	(.066)
.50	.675	.338	.225	.169	.135	.113	.096	.084	.075	.068
	(.584)	(.292)	(.195)	(.146)	(.117)	(.097)	(.083)	(.073)	(.065)	(.058)
.75	.729	.364	.243	.182	.146	.121	.104	.091	.081	.073
	(.699)	(.350)	(.233)	(.175)	(.140)	(.117)	(.100)	(.087)	(.078)	(.070)

Appendix B (continued)

Means (and standard deviations) for theta boosts for each level of theta

Theta	Item1	Item2	Item3	Item4	Item5	Item6	Item7	Item8	Item9	Item10
1.00	.670	.335	.223	.168	.134	.112	.096	.084	.075	.067
	(.611)	(.305)	(.204)	(.153)	(.122)	(.102)	(.087)	(.076)	(.068)	(.061)
1.25	.689	.344	.230	.172	.138	.115	.098	.086	.077	.069
	(.667)	(.333)	(.222)	(.167)	(.133)	(.111)	(.095)	(.083)	(.074)	(.067)
1.50	.701	.351	.234	.175	.140	.117	.100	.088	.078	.070
	(.702)	(.351)	(.234)	(.176)	(.140)	(.117)	(.100)	(.088)	(.078)	(.070)
1.75	.636	.318	.212	.159	.127	.106	.091	.080	.071	.064
	(.574)	(.287)	(.191)	(.144)	(.115)	(.096)	(.082)	(.072)	(.064)	(.057)
2.00	.661	.331	.220	.165	.132	.110	.095	.083	.074	.066
	(.635)	(.317)	(.212)	(.159)	(.127)	(.106)	(.091)	(.079)	(.071)	(.063)
2.25	.708	.354	.236	.177	.142	.118	.101	.089	.079	.071
	(.671)	(.336)	(.224)	(.168)	(.134)	(.112)	(.096)	(.084)	(.075)	(.067)
2.50	.663	.332	.221	.166	.133	.111	.095	.083	.074	.066
	(.635)	(.317)	(.212)	(.159)	(.127)	(.106)	(.091)	(.079)	(.071)	(.063)
2.75	.696	.348	.232	.174	.139	.116	.100	.087	.077	.070
	(.670)	(.335)	(.223)	(.168)	(.134)	(.112)	(.096)	(.084)	(.074)	(.067)
3.00	.694	.347	.231	.173	.139	.116	.099	.087	.077	.069
	(.624)	(.312)	(.208)	(.156)	(.125)	(.104)	(.089)	(.078)	(.069)	(.062)
3.25	.690	.345	.230	.172	.138	.115	.099	.086	.077	.069
	(.745)	(.372)	(.248)	(.186)	(.149)	(.124)	(.106)	(.093)	(.083)	(.074)
3.50	.671	.336	.224	.168	.134	.112	.096	.084	.075	.067
	(.610)	(.305)	(.203)	(.152)	(.122)	(.102)	(.087)	(.076)	(.068)	(.061)

Appendix C

Means (and standard deviations) for the theta estimate at seven points throughout the test within each true theta level

Theta	Item5	Item10	Item15	Item20	Item25	Item30	Item35
-3.50	-2.678	-3.222	-3.379	-3.417	-3.431	-3.437	-3.444
	(.354)	(.075)	(.020)	(.020)	(.023)	(.025)	(.028)
-3.25	-2.515	-3.003	-3.134	-3.172	-3.189	-3.197	-3.202
	(.292)	(.054)	(.016)	(.016)	(.018)	(.021)	(.022)
-3.00	-2.307	-2.746	-2.881	-2.921	-2.938	-2.946	-2.953
	(.251)	(.049)	(.0167)	(.014)	(.015)	(.016)	(.019)
-2.75	-2.075	-2.498	-2.631	-2.668	-2.681	-2.691	-2.698
	(.221)	(.048)	(.017)	(.013)	(.012)	(.013)	(.014)
-2.50	-1.888	-2.264	-2.387	-2.419	-2.435	-2.443	-2.449
	(.201)	(.045)	(.019)	(.016)	(.014)	(.014)	(.015)
-2.25	-1.665	-1.990	-2.117	-2.160	-2.176	-2.188	-2.194
	(.172)	(.042)	(.023)	(.019)	(.017)	(.017)	(.019)
-2.00	-1.463	-1.763	-1.884	-1.924	-1.939	-1.949	-1.954
	(.171)	(.039)	(.022)	(.016)	(.013)	(.014)	(.015)
-1.75	-1.245	-1.512	-1.627	-1.666	-1.685	-1.694	-1.699
	(.184)	(.041)	(.024)	(.019)	(.017)	(.018)	(.019)
-1.50	-.980	-1.244	-1.374	-1.411	-1.429	-1.439	-1.446
	(.227)	(.042)	(.020)	(.020)	(.021)	(.022)	(.023)
-1.25	-.761	-.993	-1.119	-1.158	-1.177	-1.187	-1.193
	(.261)	(.046)	(.029)	(.028)	(.026)	(.026)	(.027)
-1.00	-.561	-.753	-.865	-.908	-.928	-.938	-.943
	(.299)	(.045)	(.031)	(.025)	(.023)	(.024)	(.025)
-.75	-.320	-.500	-.621	-.659	-.677	-.687	-.693
	(.363)	(.054)	(.032)	(.028)	(.025)	(.024)	(.024)
-.50	-.100	-.267	-.386	-.420	-.437	-.445	-.451
	(.423)	(.047)	(.036)	(.029)	(.025)	(.025)	(.025)
-.25	.114	-.003	-.128	-.166	-.182	-.190	-.197
	(.489)	(.051)	(.028)	(.023)	(.021)	(.020)	(.020)
.00	.341	.256	.123	.082	.065	.055	.049
	(.562)	(.053)	(.036)	(.031)	(.028)	(.027)	(.026)
.25	.570	.508	.383	.345	.327	.315	.308
	(.648)	(.052)	(.031)	(.025)	(.023)	(.022)	(.023)
.50	.755	.744	.620	.582	.566	.555	.549
	(.720)	(.047)	(.032)	(.025)	(.023)	(.021)	(.021)

Appendix C (continued)

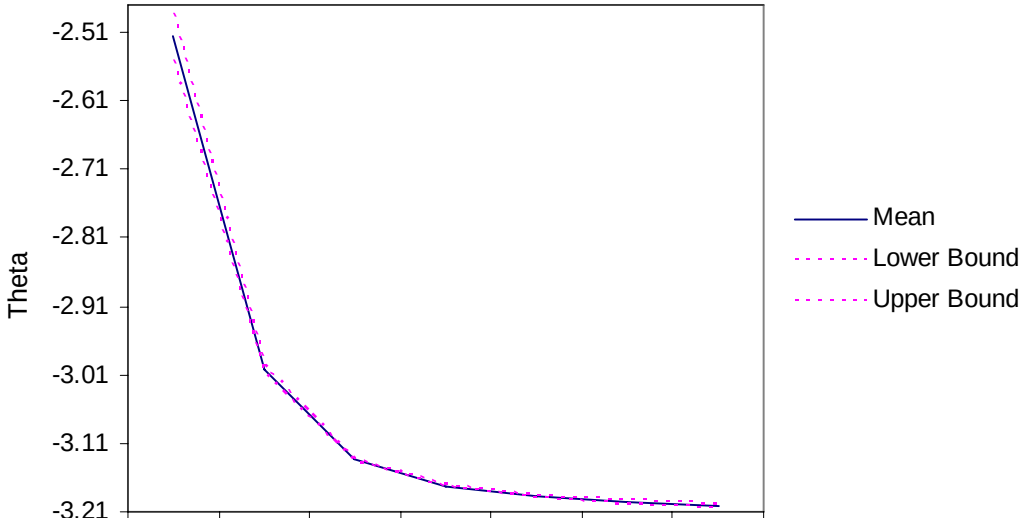
Means (and standard deviations) for the theta estimate at seven points throughout the test within each true theta level

Theta	Item5	Item10	Item15	Item20	Item25	Item30	Item35
.75	.977	1.002	.867	.830	.813	.805	.798
	(.811)	(.056)	(.030)	(.023)	(.021)	(.019)	(.017)
1.00	1.139	1.222	1.103	1.070	1.053	1.046	1.041
	(.881)	(.058)	(.034)	(.025)	(.021)	(.019)	(.018)
1.25	1.334	1.482	1.354	1.317	1.300	1.293	1.288
	(.962)	(.053)	(.031)	(.022)	(.018)	(.016)	(.015)
1.50	1.534	1.722	1.602	1.565	1.551	1.542	1.538
	(1.049)	(.056)	(.023)	(.019)	(.015)	(.015)	(.013)
1.75	1.713	1.957	1.849	1.816	1.801	1.792	1.787
	(1.133)	(.059)	(.027)	(.021)	(.017)	(.014)	(.013)
2.00	1.908	2.219	2.106	2.071	2.055	2.046	2.040
	(1.226)	(.070)	(.023)	(.017)	(.013)	(.011)	(.010)
2.25	2.114	2.447	2.343	2.313	2.298	2.288	2.284
	(1.327)	(.063)	(.032)	(.026)	(.019)	(.017)	(.015)
2.50	2.248	2.663	2.584	2.557	2.543	2.535	2.530
	(1.395)	(.071)	(.023)	(.015)	(.014)	(.011)	(.010)
2.75	2.421	2.906	2.844	2.814	2.797	2.789	2.784
	(1.490)	(.105)	(.028)	(.020)	(.016)	(.015)	(.015)
3.00	2.517	3.140	3.090	3.066	3.051	3.042	3.036
	(1.550)	(.121)	(.032)	(.022)	(.018)	(.016)	(.015)
3.25	2.638	3.360	3.338	3.315	3.299	3.292	3.286
	(1.618)	(.122)	(.033)	(.022)	(.019)	(.017)	(.018)
3.50	2.760	3.555	3.582	3.566	3.555	3.547	3.541
	(1.688)	(.172)	(.035)	(.026)	(.022)	(.020)	(.020)

Appendix D

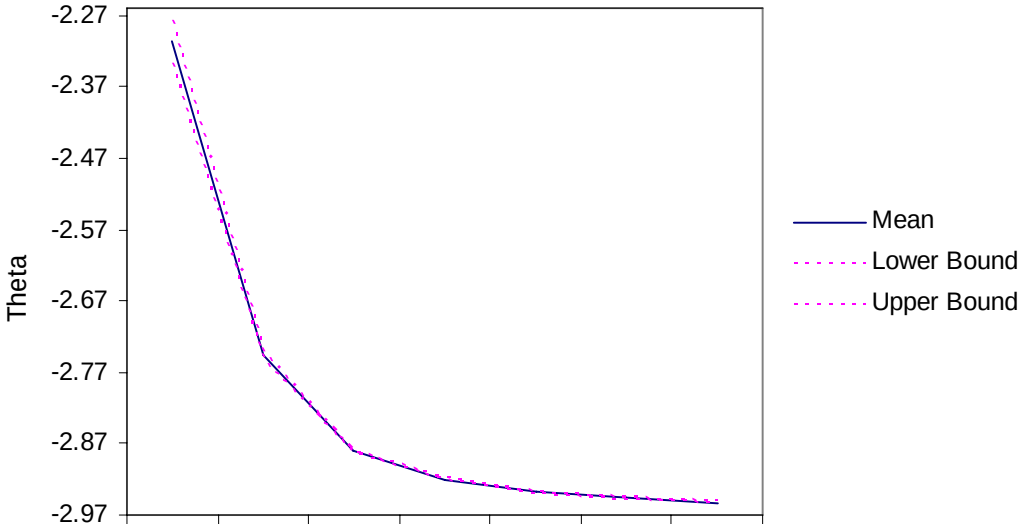
Estimated theta value mean and 95% confidence interval for each true theta for boost conditions

True theta = -3.25



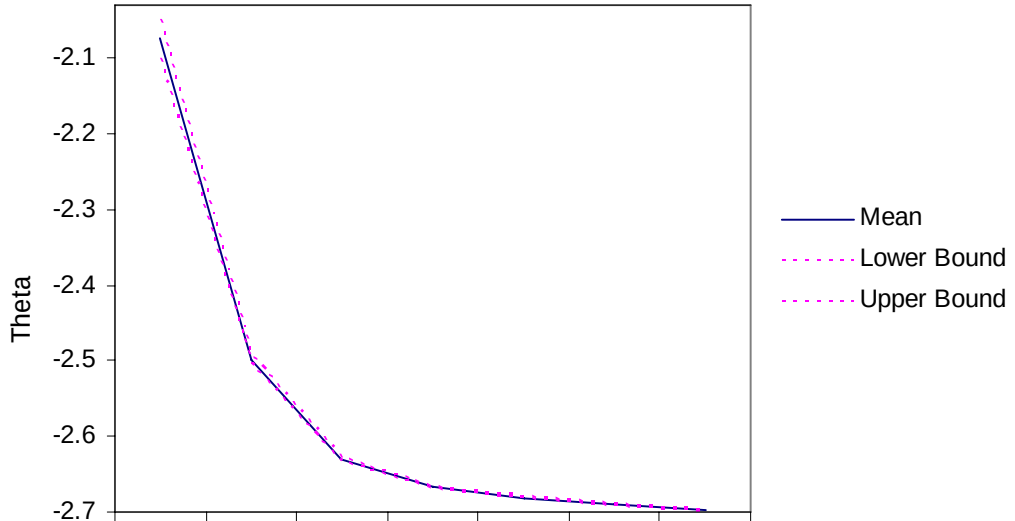
	item5	item10	item15	item20	item25	item30	item35
Mean	-2.5154	-3.0029	-3.1337	-3.1717	-3.1885	-3.1967	-3.2019
Lower Bound	-2.5492	-3.0092	-3.1356	-3.1736	-3.1905	-3.1991	-3.2045
Upper Bound	-2.4815	-2.9966	-3.1318	-3.1699	-3.1864	-3.1943	-3.1992

True theta = -3.00



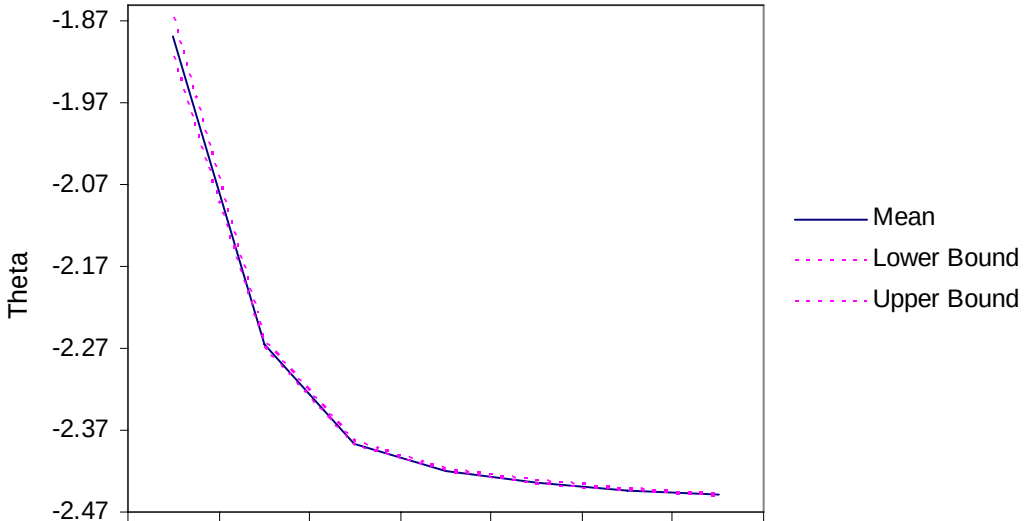
	item5	item10	item15	item20	item25	item30	item35
Mean	-2.3067	-2.7459	-2.8811	-2.9205	-2.9384	-2.9464	-2.9526
Lower Bound	-2.3358	-2.7516	-2.8831	-2.9221	-2.9401	-2.9483	-2.9548
Upper Bound	-2.2776	-2.7403	-2.8792	-2.9189	-2.9367	-2.9445	-2.9504

True theta = -2.75



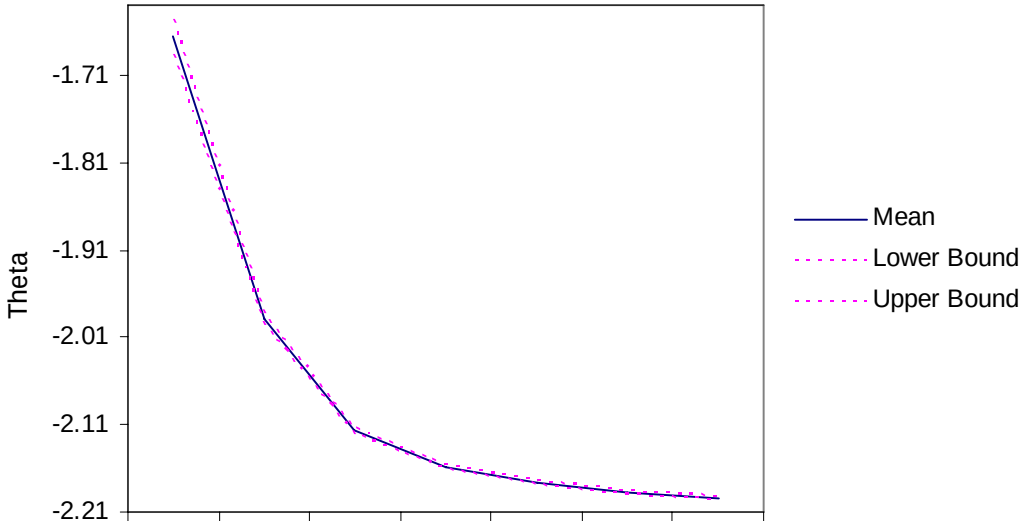
	item5	item10	item15	item20	item25	item30	item35
Mean	-2.0748	-2.4981	-2.6307	-2.6675	-2.681	-2.6907	-2.6979
Lower Bound	-2.1005	-2.5036	-2.6327	-2.6691	-2.6824	-2.6922	-2.6995
Upper Bound	-2.0492	-2.4925	-2.6287	-2.6659	-2.6796	-2.6893	-2.6962

True theta = -2.50



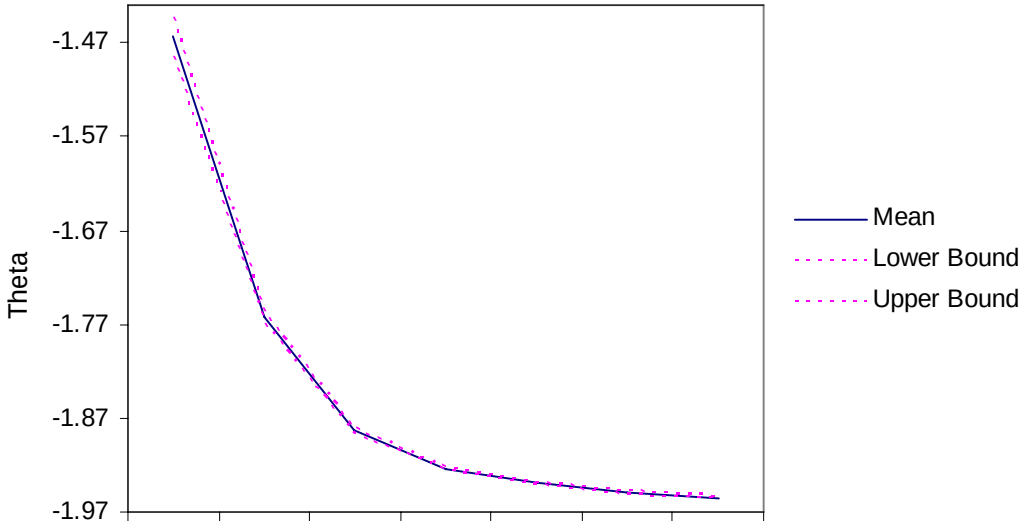
	item5	item10	item15	item20	item25	item30	item35
Mean	-1.8882	-2.2644	-2.387	-2.4188	-2.4346	-2.4427	-2.4492
Lower Bound	-1.9114	-2.2696	-2.3893	-2.4206	-2.4362	-2.4443	-2.4509
Upper Bound	-1.8649	-2.2592	-2.3848	-2.417	-2.4329	-2.441	-2.4475

True theta = -2.25



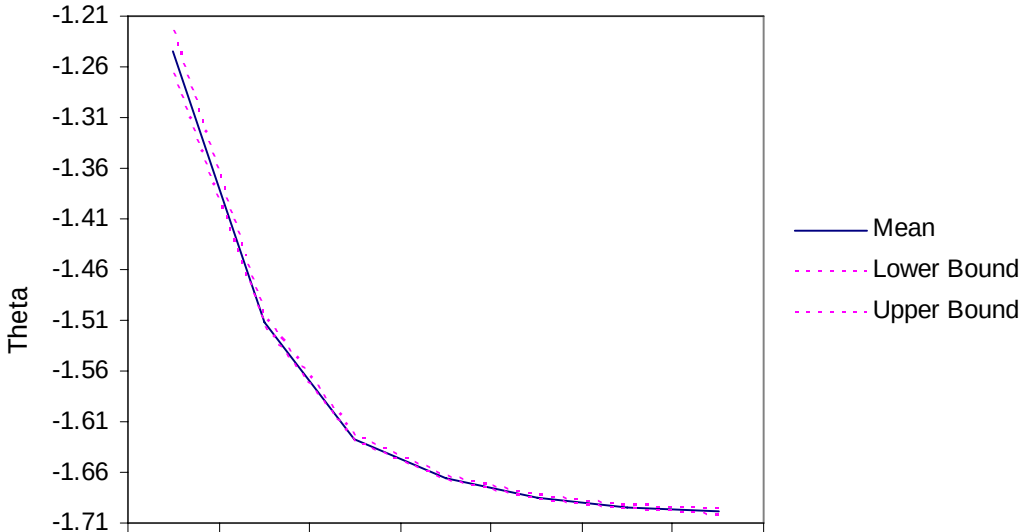
	item5	item10	item15	item20	item25	item30	item35
Mean	-1.6649	-1.9902	-2.1173	-2.1595	-2.1763	-2.1877	-2.1939
Lower Bound	-1.6849	-1.9951	-2.1199	-2.1616	-2.1783	-2.1897	-2.1961
Upper Bound	-1.6449	-1.9854	-2.1146	-2.1573	-2.1744	-2.1857	-2.1918

True theta = -2.00



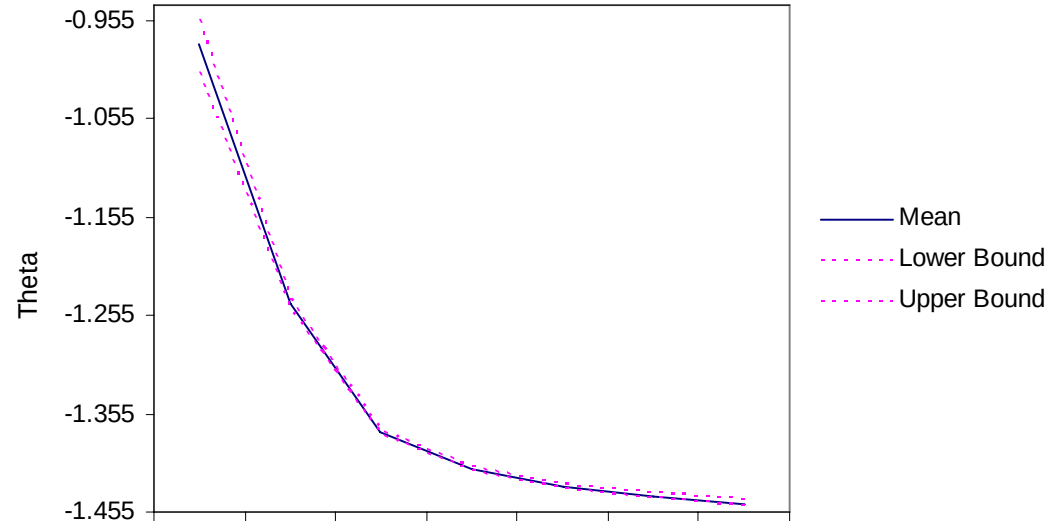
	item5	item10	item15	item20	item25	item30	item35
Mean	-1.4633	-1.7631	-1.8837	-1.9235	-1.9394	-1.949	-1.9544
Lower Bound	-1.4831	-1.7676	-1.8862	-1.9253	-1.9409	-1.9506	-1.9562
Upper Bound	-1.4435	-1.7585	-1.8811	-1.9217	-1.938	-1.9474	-1.9527

True theta = -1.75



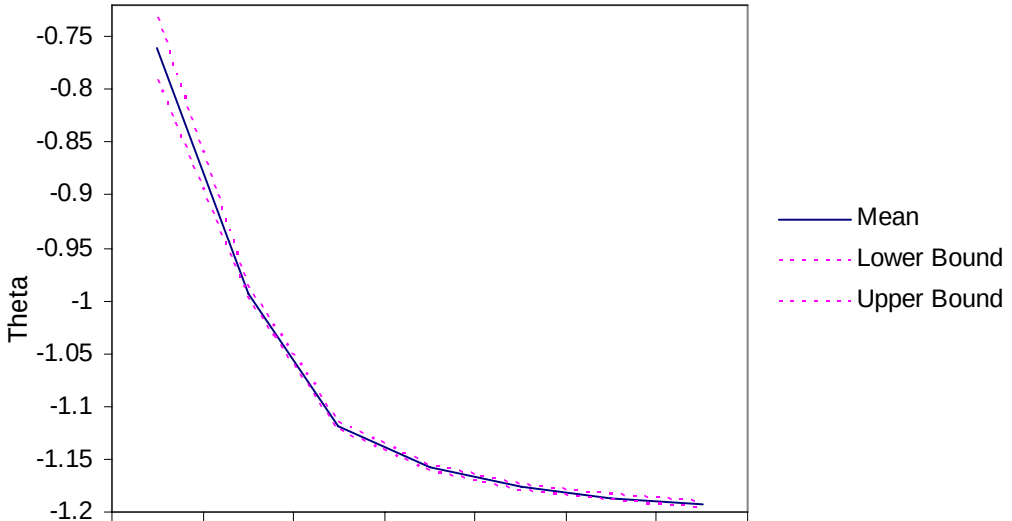
	item5	item10	item15	item20	item25	item30	item35
Mean	-1.2453	-1.5117	-1.627	-1.6657	-1.6852	-1.6939	-1.6992
Lower Bound	-1.2666	-1.5164	-1.6298	-1.6679	-1.6872	-1.696	-1.7014
Upper Bound	-1.224	-1.507	-1.6243	-1.6634	-1.6832	-1.6918	-1.697

True theta = -1.50



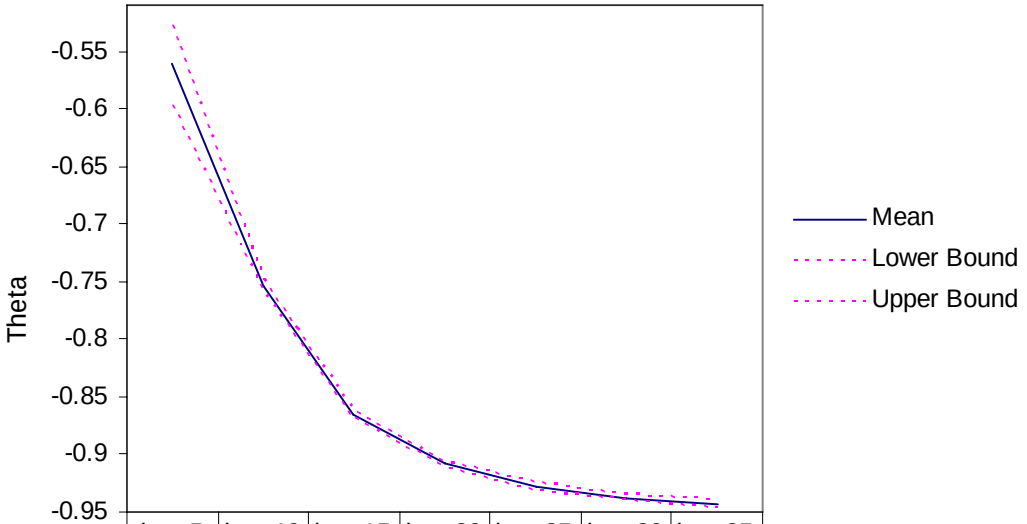
	item5	item10	item15	item20	item25	item30	item35
Mean	-0.9803	-1.2435	-1.3737	-1.411	-1.429	-1.4385	-1.4462
Lower Bound	-1.0066	-1.2484	-1.3761	-1.4134	-1.4315	-1.441	-1.4489
Upper Bound	-0.954	-1.2386	-1.3714	-1.4087	-1.4265	-1.436	-1.4435

True theta = -1.25



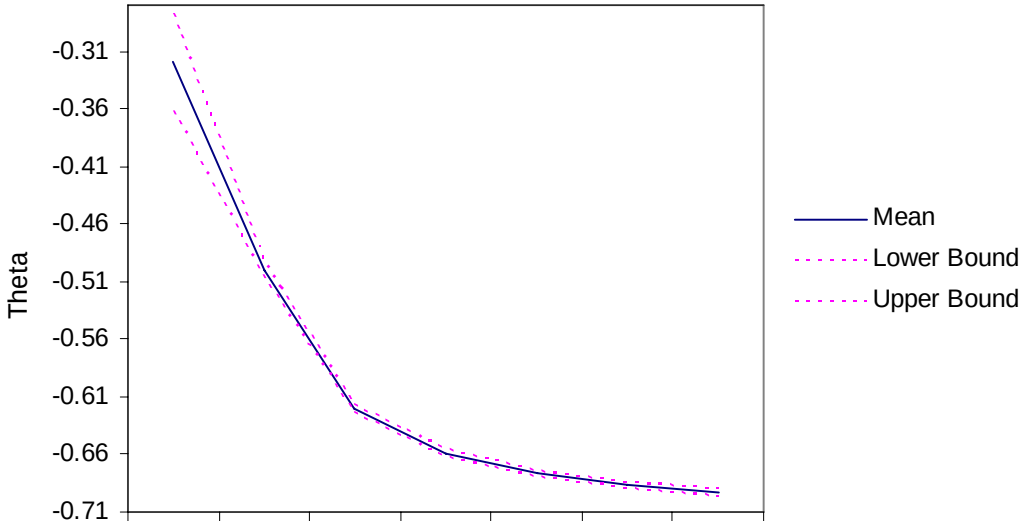
	item5	item10	item15	item20	item25	item30	item35
Mean	-0.7606	-0.9928	-1.1186	-1.1584	-1.1766	-1.1866	-1.1932
Lower Bound	-0.7908	-0.9982	-1.122	-1.1617	-1.1796	-1.1897	-1.1964
Upper Bound	-0.7304	-0.9874	-1.1152	-1.1552	-1.1735	-1.1836	-1.19

True theta = -1.00



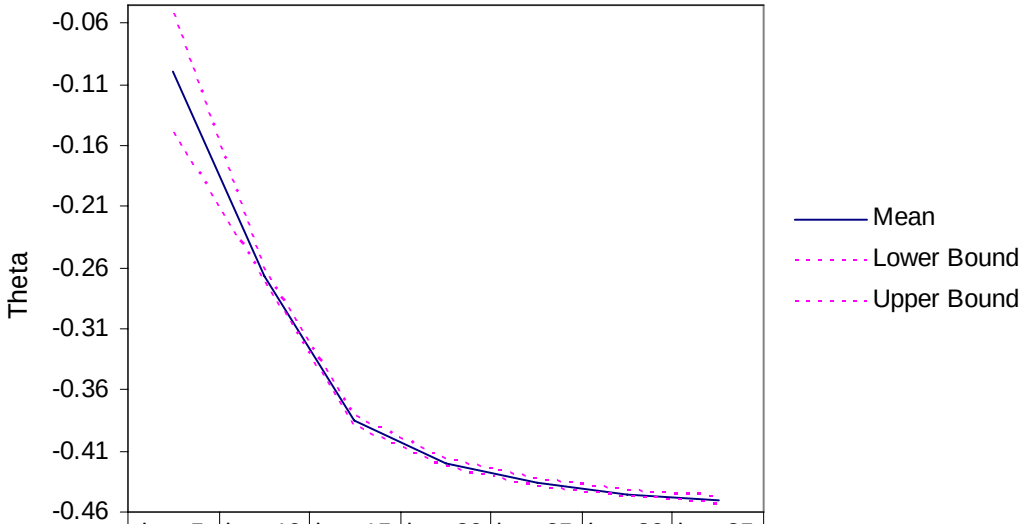
	item5	item10	item15	item20	item25	item30	item35
Mean	-0.561	-0.7533	-0.8651	-0.9082	-0.928	-0.9375	-0.9432
Lower Bound	-0.5956	-0.7585	-0.8687	-0.9111	-0.9307	-0.9404	-0.9461
Upper Bound	-0.5263	-0.748	-0.8614	-0.9052	-0.9253	-0.9347	-0.9402

True theta = -0.75



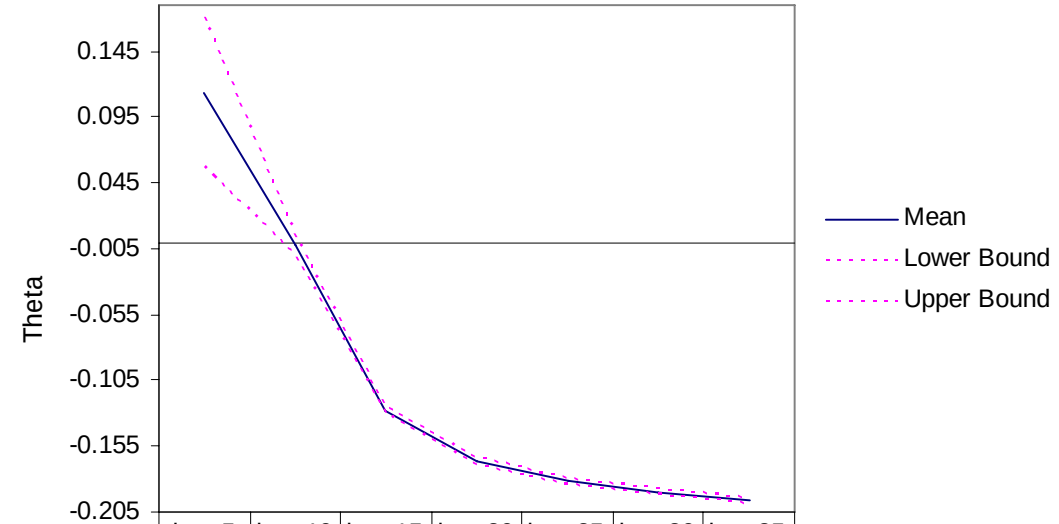
	item5	item10	item15	item20	item25	item30	item35
Mean	-0.3196	-0.5004	-0.6208	-0.6593	-0.6767	-0.6865	-0.6932
Lower Bound	-0.3617	-0.5066	-0.6245	-0.6626	-0.6795	-0.6892	-0.696
Upper Bound	-0.2775	-0.4942	-0.6171	-0.656	-0.6738	-0.6838	-0.6904

True theta = -0.50



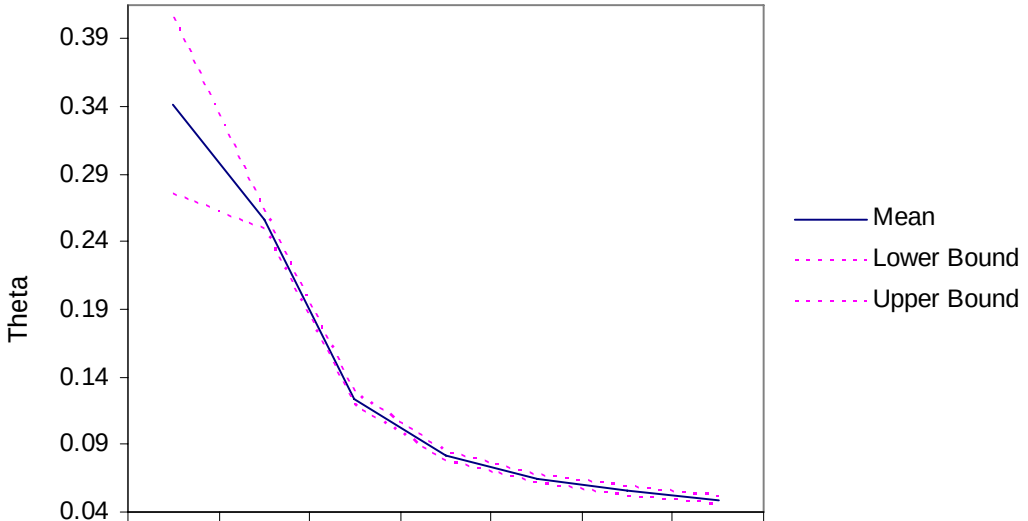
	item5	item10	item15	item20	item25	item30	item35
Mean	-0.0998	-0.2669	-0.3857	-0.4202	-0.4365	-0.445	-0.4509
Lower Bound	-0.1489	-0.2724	-0.3899	-0.4236	-0.4394	-0.4479	-0.4538
Upper Bound	-0.0508	-0.2614	-0.3815	-0.4168	-0.4336	-0.442	-0.4479

True theta = -0.25



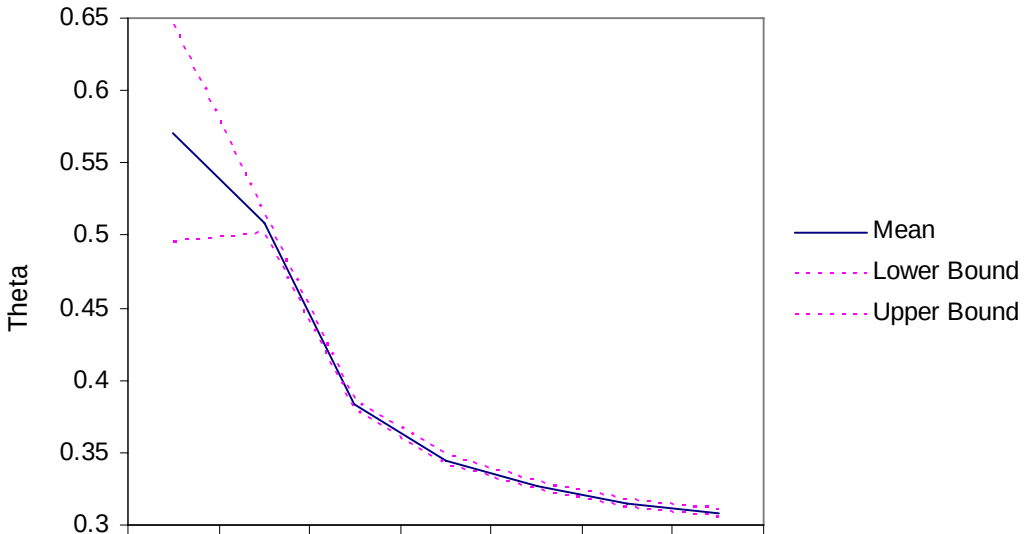
	item5	item10	item15	item20	item25	item30	item35
Mean	0.1141	-0.0028	-0.1282	-0.1662	-0.1816	-0.1901	-0.1966
Lower Bound	0.0574	-0.0088	-0.1315	-0.1689	-0.184	-0.1924	-0.1989
Upper Bound	0.1708	0.0031	-0.125	-0.1635	-0.1792	-0.1878	-0.1943

True theta = 0.00



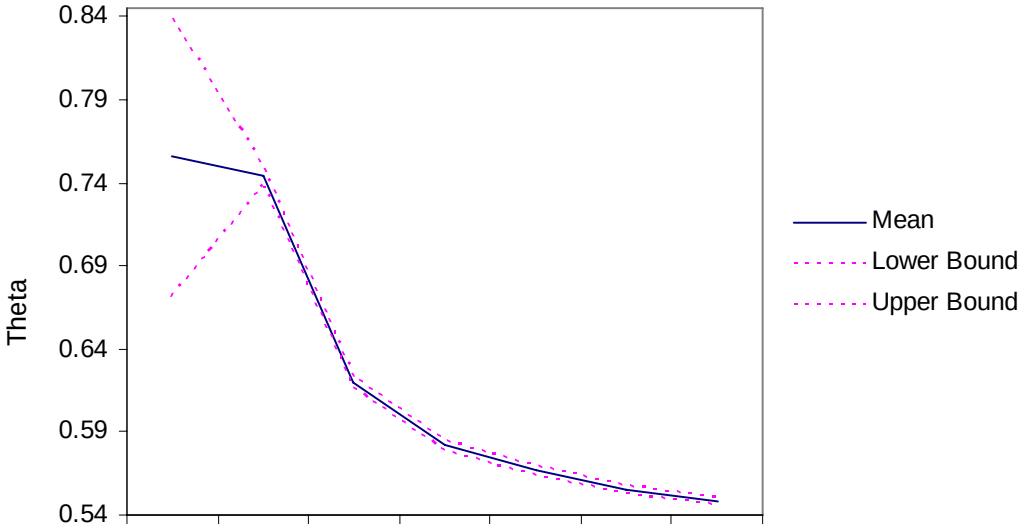
	item5	item10	item15	item20	item25	item30	item35
Mean	0.3407	0.2559	0.1233	0.0817	0.0646	0.0552	0.0486
Lower Bound	0.2756	0.2498	0.1191	0.0782	0.0613	0.0521	0.0456
Upper Bound	0.4059	0.262	0.1275	0.0853	0.0678	0.0583	0.0516

True theta = 0.25



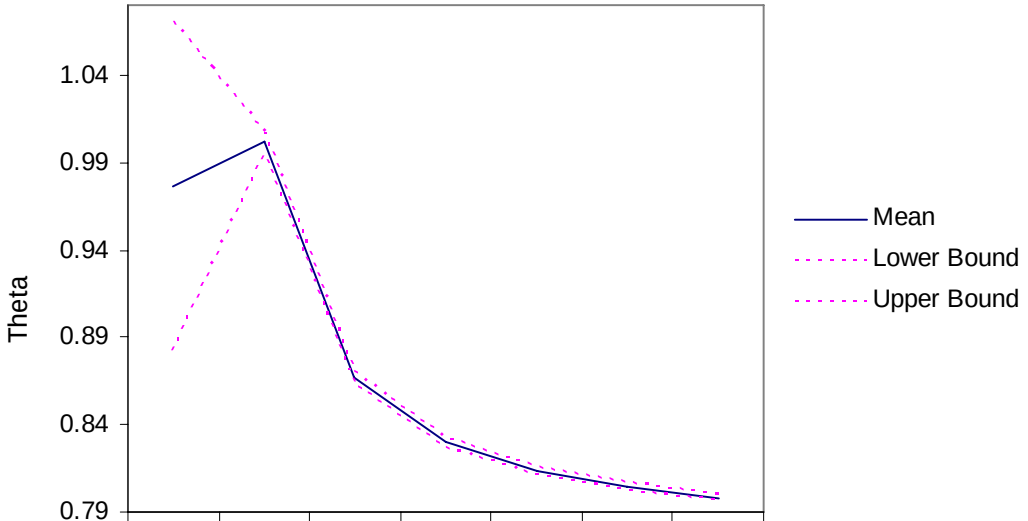
	item5	item10	item15	item20	item25	item30	item35
Mean	0.5704	0.5083	0.383	0.345	0.3265	0.315	0.3081
Lower Bound	0.4952	0.5023	0.3793	0.3421	0.3239	0.3124	0.3055
Upper Bound	0.6456	0.5143	0.3866	0.348	0.3291	0.3176	0.3107

True theta = 0.50



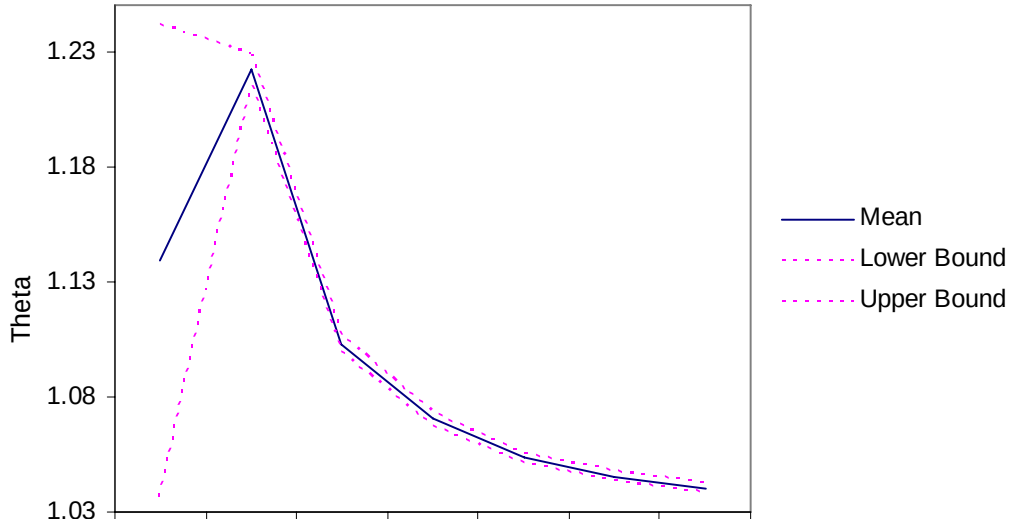
	item5	item10	item15	item20	item25	item30	item35
Mean	0.7553	0.7435	0.62	0.5822	0.5664	0.5548	0.5485
Lower Bound	0.6719	0.7381	0.6163	0.5793	0.5638	0.5524	0.546
Upper Bound	0.8388	0.749	0.6238	0.5851	0.5691	0.5573	0.5509

True theta = 0.75



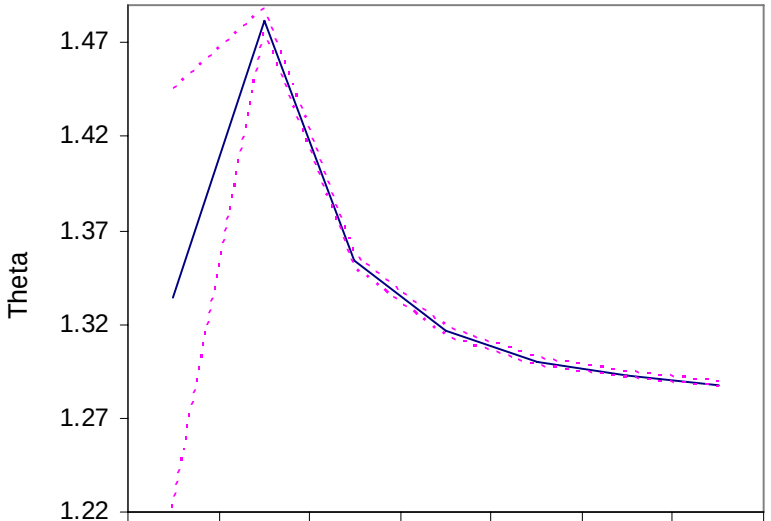
	item5	item10	item15	item20	item25	item30	item35
Mean	0.9766	1.0015	0.8674	0.8298	0.8134	0.8048	0.7983
Lower Bound	0.8826	0.995	0.8638	0.8271	0.811	0.8026	0.7963
Upper Bound	1.0706	1.008	0.8709	0.8325	0.8158	0.807	0.8003

True theta = 1.00



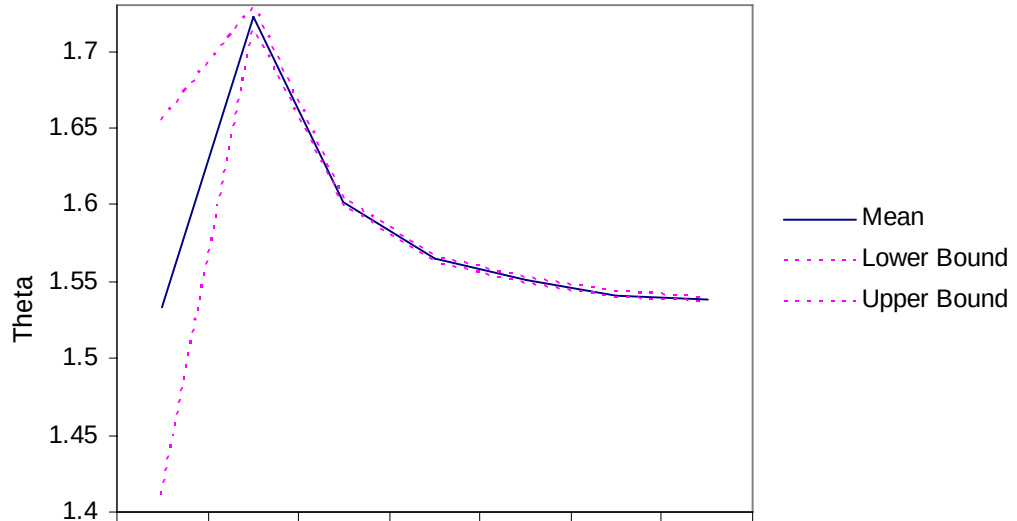
	item5	item10	item15	item20	item25	item30	item35
Mean	1.1392	1.2223	1.103	1.0704	1.0533	1.0455	1.0405
Lower Bound	1.037	1.2156	1.0991	1.0676	1.0509	1.0433	1.0384
Upper Bound	1.2414	1.229	1.1069	1.0733	1.0558	1.0477	1.0426

True theta = 1.25



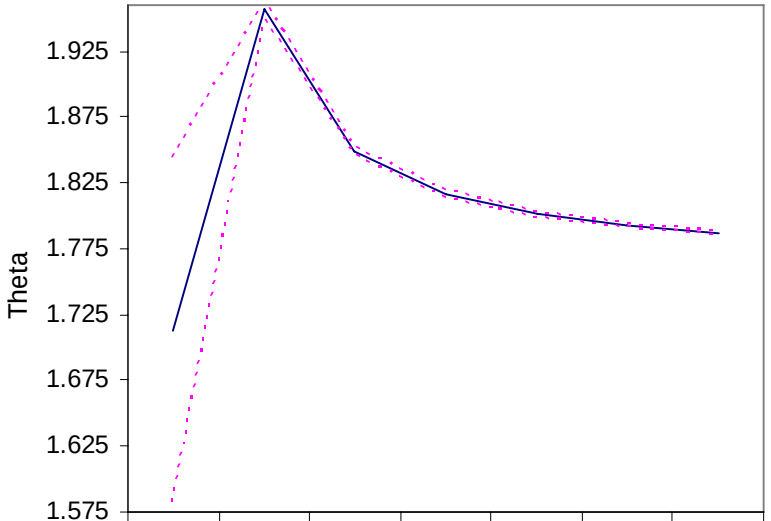
	item5	item10	item15	item20	item25	item30	item35
Mean	1.3339	1.4818	1.3535	1.3165	1.3002	1.2931	1.2877
Lower Bound	1.2223	1.4756	1.35	1.3139	1.2981	1.2913	1.286
Upper Bound	1.4454	1.4879	1.3571	1.3191	1.3023	1.295	1.2894

True theta = 1.50



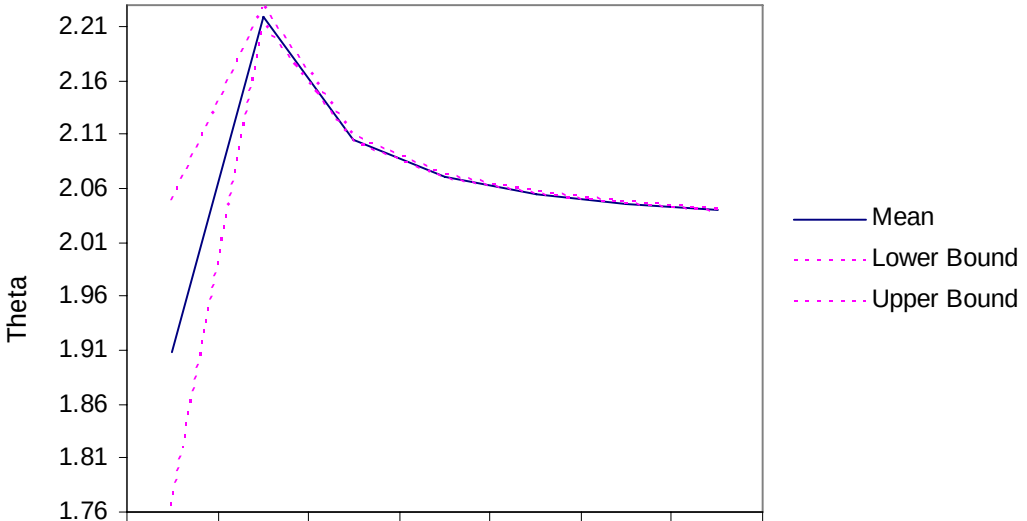
	item5	item10	item15	item20	item25	item30	item35
Mean	1.5335	1.7219	1.6016	1.5645	1.5508	1.5415	1.5382
Lower Bound	1.4118	1.7154	1.5989	1.5623	1.5491	1.5398	1.5366
Upper Bound	1.6552	1.7284	1.6043	1.5668	1.5524	1.5432	1.5397

True theta = 1.75



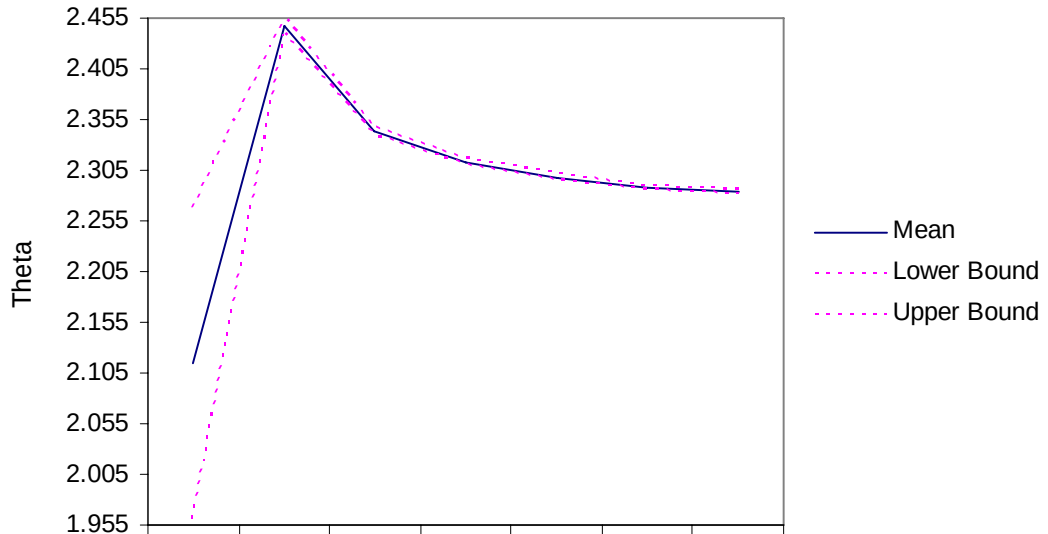
	item5	item10	item15	item20	item25	item30	item35
Mean	1.7132	1.9567	1.8491	1.8162	1.8011	1.7921	1.7868
Lower Bound	1.5818	1.9499	1.846	1.8138	1.7992	1.7905	1.7853
Upper Bound	1.8447	1.9635	1.8523	1.8186	1.8031	1.7937	1.7883

True theta = 2.00



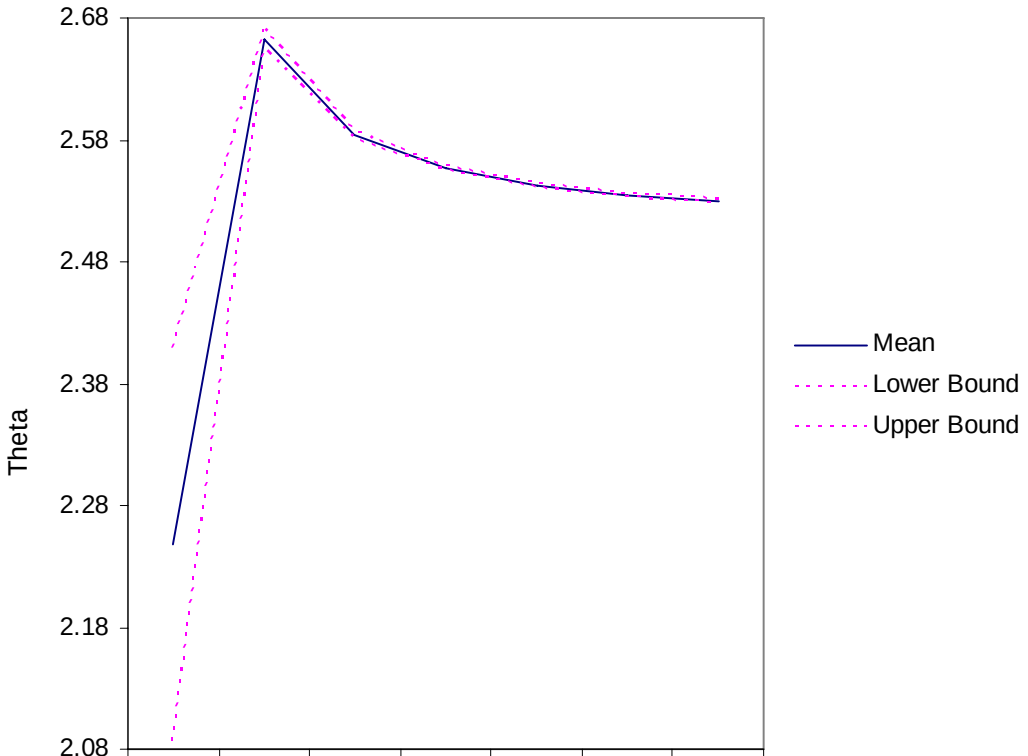
	item5	item10	item15	item20	item25	item30	item35
Mean	1.9076	2.2193	2.1055	2.0707	2.0554	2.046	2.0396
Lower Bound	1.7655	2.2111	2.1029	2.0687	2.0538	2.0447	2.0384
Upper Bound	2.0498	2.2274	2.1082	2.0727	2.0569	2.0472	2.0408

True theta = 2.25



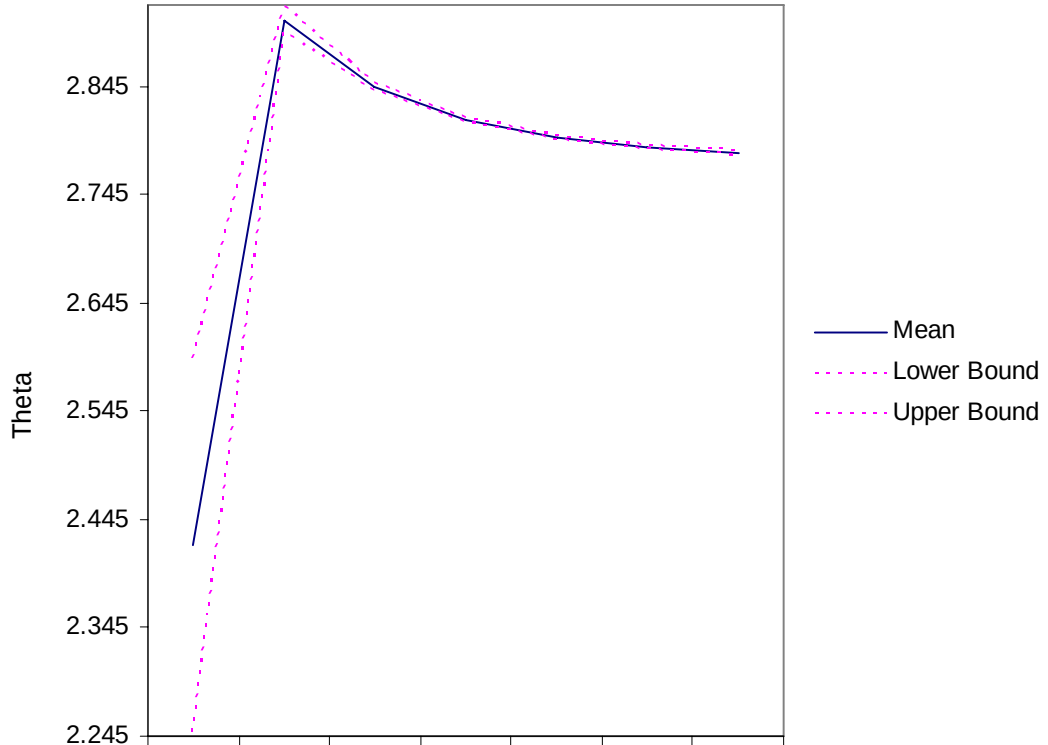
	item5	item10	item15	item20	item25	item30	item35
Mean	2.1138	2.4472	2.3432	2.3131	2.2981	2.2884	2.2838
Lower Bound	1.9598	2.4399	2.3395	2.3101	2.2959	2.2865	2.2821
Upper Bound	2.2678	2.4545	2.3469	2.3161	2.3002	2.2903	2.2856

True theta = 2.50



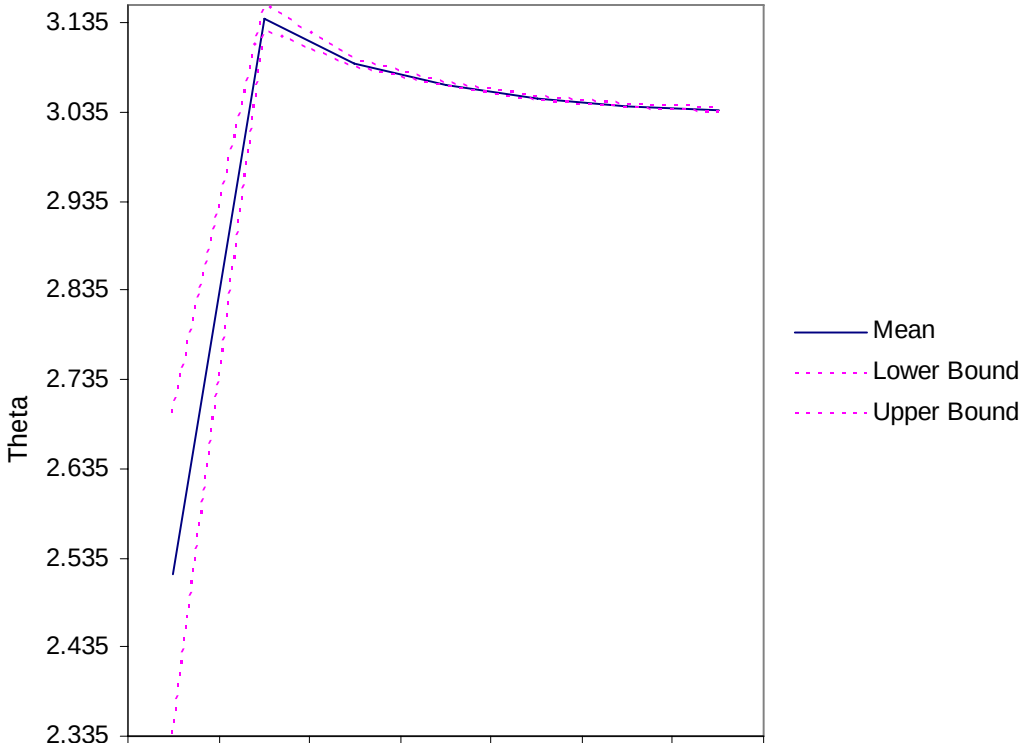
	item5	item10	item15	item20	item25	item30	item35
Mean	2.2483	2.6626	2.584	2.5566	2.5428	2.5348	2.5298
Lower Bound	2.0865	2.6544	2.5813	2.5549	2.5412	2.5335	2.5286
Upper Bound	2.4101	2.6708	2.5867	2.5584	2.5444	2.5361	2.531

True theta = 2.75



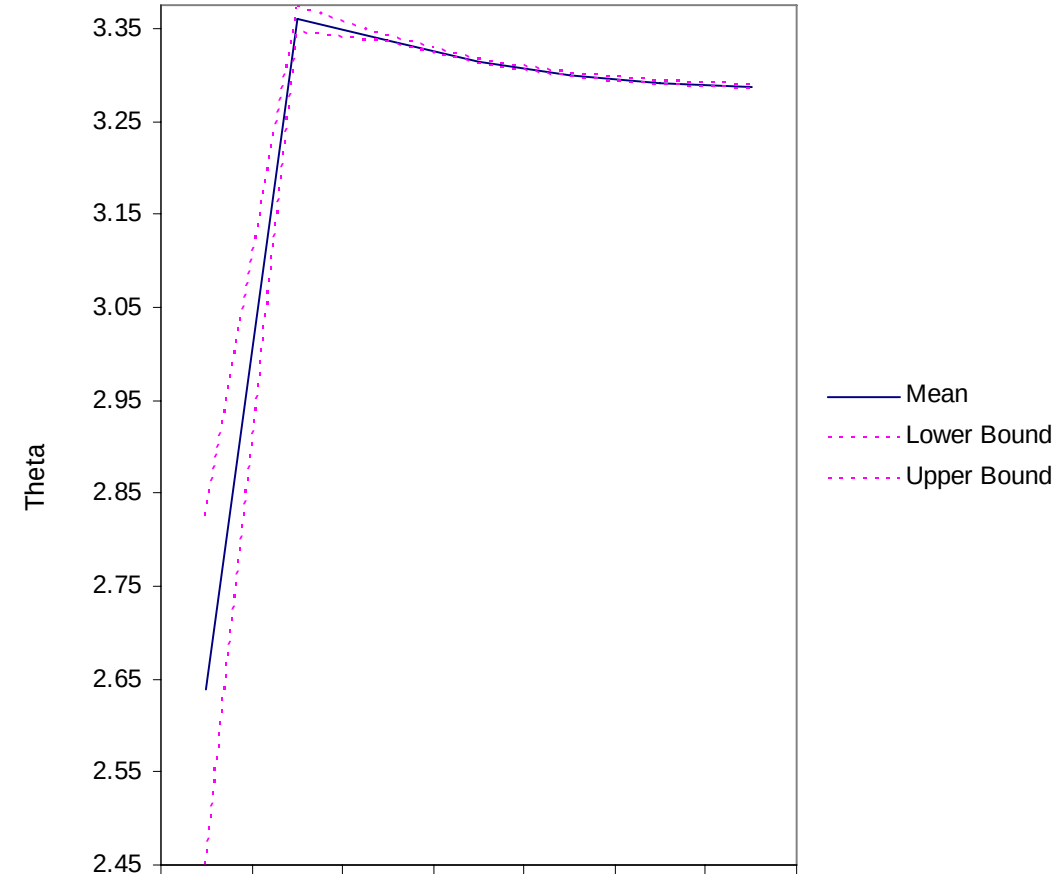
	item5	item10	item15	item20	item25	item30	item35
Mean	2.4213	2.9064	2.8439	2.8135	2.7972	2.7892	2.7839
Lower Bound	2.2485	2.8942	2.8407	2.8112	2.7953	2.7874	2.7822
Upper Bound	2.5942	2.9187	2.8472	2.8159	2.7991	2.791	2.7856

True theta = 3.00



	item5	item10	item15	item20	item25	item30	item35
Mean	2.5172	3.14	3.0895	3.0656	3.0506	3.0419	3.0364
Lower Bound	2.3374	3.126	3.0858	3.063	3.0485	3.04	3.0346
Upper Bound	2.6969	3.154	3.0932	3.0682	3.0527	3.0437	3.0382

True theta = 3.25



	item5	item10	item15	item20	item25	item30	item35
Mean	2.6381	3.3598	3.338	3.3145	3.299	3.2917	3.2863
Lower Bound	2.4504	3.3456	3.3342	3.312	3.2968	3.2897	3.2842
Upper Bound	2.8258	3.3739	3.3418	3.3171	3.3011	3.2937	3.2884