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A Monte Carlo approach for exploring the generalizability of performance standards

James Thomas Coraggio
University of South Florida

A Monte Carlo Approach for Exploring the Generalizability of Performance Standards

by

James Thomas Coraggio

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Major Professor: John M. Ferron, Ph.D.
Jeffrey D. Kromrey, Ph.D.
Robert F. Dedrick, Ph.D.
Stephen Stark, Ph.D.

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Dedication

This dissertation is dedicated to my immediate family: my wife, Penny, and my three children, Sydnie, Allyson, and Austin. Penny has been there for me every step of the way. The journey has at times been difficult and demanding, but her support has been unwavering. Through it all we have had three children, and earned three degrees and a national board certification between us. Without her support, understanding, and devotion, I would not have been able to complete the first page of this dissertation, much less completed my first class. My two girls have had to endure countless hours away from their father during the 'early' years of their lives. I dedicate this dissertation to both of them in hopes that the importance of education will be engrained on them for the rest their lives. Austin is my little 'miracle' child. His recent birth has kept me grounded as I have worked to complete this dissertation. From him, I have learned that God truly does work in mysterious ways. We can plan all we like, but fate untimely intervenes. As this journey comes to a completion, I am sure that another will begin very soon. My formal education is finally over and I hope to make up all that 'lost' time with my family. Girls…daddy's finally home from school!

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ABSTRACT

While each phase of the test development process is crucial to the validity of the examination, one phase tends to stand out among the others: the standard setting process. The standard setting process is a time-consuming and expensive endeavor. While it has received the most attention in the literature among any of the technical issues related to criterion-referenced measurement, little research attention has been given to generalizing the resulting performance standards. This procedure has the potential to improve the standard setting process by limiting the number of items rated and the number of individual rater decisions. The ability to generalize performance standards has profound implications both from a psychometric as well as a practicality standpoint. This study was conducted to evaluate the extent to which minimal competency estimates derived from a subset of multiple choice items using the Angoff standard setting method would generalize to the larger item set. Individual item-level estimates of minimal competency were simulated from existing and simulated item difficulty distributions. The study was designed to examine the characteristics of item sets and the standard setting process that could impact the ability to generalize a single performance standard. The characteristics and the relationship between the two item sets included three factors: (a) the item difficulty distributions, (b) the location of the 'true' performance standard, (c) the number of items randomly drawn in the sample. The characteristics of the standard setting

process included four factors: (d) number of raters, (e) percentage of unreliable raters, (f) magnitude of 'unreliability' in unreliable raters, and (g) the directional influence of group dynamics and discussion. The aggregated simulation results were evaluated in terms of the location (bias) and the variability (mean absolute deviation, root mean square error) in the estimates. The simulation results suggest that the model of using partial item sets may have some merit as the resulting performance standard estimates may 'adequately' generalize to those set with larger item sets. The simulation results also suggest that elements such as the distribution of item difficulty parameters and the potential for directional group influence may also impact the ability to generalize performance standards and should be carefully considered.

Chapter One:

Introduction

Background

In an age of ever increasing societal expectations of accountability (Boursicot & Roberts, 2006), measuring and evaluating change through assessment is now the norm, not the exception. With the establishment of the No Child Left Behind Act of 2001 (NCLB; P.L. 107-110) and the increasing number of "mastery" licensing examinations (Beretvas, 2004), outcome validation is more important than ever and criterion-based testing has been the instrument of choice for most situations. Each phase of the test development process must be extensively reviewed and evaluated if stakeholders are to be held accountable for the results.

While each phase of the test development process is crucial to the validity of the examination, one phase tends to stand out among the others: the standard setting process. It has continually received the most attention in the literature among any of the technical issues related to criterion-referenced measurement (Berk, 1986). This is largely due to the fact that determining the passing standard or the acceptable level of competency is one of the most difficult steps in creating an examination (Wang, Wiser, & Newman, 2001). Little research attention, however, has been given to generalizing the resulting performance standards. In essence, can the estimate of minimal competency that is established with one subset of items be applied to the larger set of items from which it

was derived? The ability to generalize performance standards has profound implications both from a psychometric as well as a practical standpoint.

Appropriate Standard Setting Models

Of the 50 different standard setting procedures (Wang, Pan, & Austin, 2003; for a detailed description of various methods see Zieky, 2001), the Bookmark method would seem the method best suited for this type of generalizability due to its use of item response theory (IRT). In fact, Mitzel, Lewis, Patz, and Green (2001) suggested that the Bookmark method can "accommodate items sampled from a domain, multiple test forms, or a single form" as long as the items have been placed on the same scale (p. 253). Yet, there has been no identifiable research conducted on the subject using the Bookmark method (Karantonis & Sireci, 2006). While the IRT-based standard setting methods do use a common scale, they all have a potential issue with reliability. Raters are only given one opportunity per round to determine an estimate of minimal competency as they select a single place between items rather than setting performance estimates for each individual item as in the case of the Angoff method (Angoff, 1971).

The Angoff method and its various modifications are currently one of the most popular methods of standard setting among licensure and certification organizations (Impara, 1995; Kane, 1995; Plake, 1998). While the popularity of the Angoff method has declined since the introduction of the IRT-based Bookmark method, the Angoff method is still one of the "most prominent" and "widely used" standard setting methods (Ferdous & Plake, 2005). The Angoff method relies on the opinion of judges who rate each item according to the probability that a "minimally proficient" candidate will answer a specific

item correctly (Behuniak, Archambault, & Gable, 1982). The ratings of the judges are then combined to create an overall passing standard. The Angoff method relies heavily on the opinion of individuals and has an inherent aspect of subjectivity that can be of concern when determining an appropriate standard.

Some limited research on the Angoff method has supported the idea of generalizing performance standards (Ferdous, 2005; Ferdous & Plake, 2005, 2007; Sireci, Patelis, Rizavi, Dillingham, & Rodriguez, 2000), and other researchers have suggested the possibility of generalizing performance standards based only on a subset of items (Coraggio, 2005, 2007), but before such a process can be implemented, issues such as the characteristics of the item sets and the characteristics of the standard setting process must be evaluated for their impact on the process.

Characteristics of the Item Sets

Before a performance standard based on a subset of multiple choice items can be generalized to a broader set of items, characteristics of the item sets should be addressed. In other words, how well do the characteristics of the larger item set, the characteristics of the smaller subset of items, and the relationship between the two item sets impact the ability to draw inferences from the subset of items? One efficient way to address this question is to place all the items on the same scale, and the use of item response theory seems an appropriate psychometric method for this type of analysis. In fact, van der Linden (1982) suggested that item response theory (IRT) may be useful in the standard setting process. He suggested that IRT can be used to set estimates of true scores or expected observed scores for minimally competent examinees (van der Linden, 1982). In

fact, some limited research has been conducted placing minimally competency estimates on an IRT theta scale (see Coraggio, 2005; Reckase, 2006a). In addition to characteristics of the item sets, the characteristics of the standard setting process may also impact the ability to accurately generalize performance standards.

Characteristics of the Standard Setting Process

Almost from the introduction of standard setting (Lorge & Kruglov, 1953), controversy has surrounded the process. Accusations relating to fairness and objectivity have constantly clouded the standard setting landscape, regardless of the imposed method. Glass (1978) conducted an extensive review of the various standard setting methods and determined that the standard setting processes were arbitrary or derived from arbitrary premises. Jaeger (1989) and Mehrens (1995) found that it was unlikely for two different standard setting methods to result in comparable standards. Behuniak, Archambault, and Gable (1982), after researching two popular standard setting models (Angoff and Nedelsky), had similar results determining that different standard setting methods produce cut scores that are "statistically and practically different" and even groups of judges employing the same standard setting method should not be expected to set similar passing standards (p. 254). "The most consistent finding from the research literature on standard setting is that different methods lead to different results" (National Academy of Education, 1993, p. 24). In various research studies, the item difficulty estimates from raters have been at times inaccurate, inconsistent, and contradictory (Bejar, 1983; Goodwin, 1999; Mills & Melican, 1988; Reid, 1991; Shepard, 1995; Swanson, Dillon, & Ross, 1990; Wang et al., 2001). One element that has impacted rater

reliability has been the inability for raters to judge item difficulty. While the literature is well documented with the cause(s) of rater inconsistency, the primary focus of this research is to explore the resulting impact of rater inconsistency, specifically, as it relates to the ability to generalize performance standards.

Statement of the Problem

The standard setting process is a time-consuming and expensive endeavor. It requires the involvement of a number of professionals both as participants such as subject matter experts (SME) as well as those involved in the test development process such as psychometricians and workshop facilitators. The standard setting process can also be cognitively taxing on participants and this has been a criticism of the Angoff method (Lewis, Green, Mitzel, Baum, & Patz, 1998).

While IRT-based models such as the Bookmark and other variations have been created to addresses the deficiencies in the Angoff method, research suggests that these new IRT-based methods have inadvertently introduced other flaws. In a multimethod study of standard setting methodologies by Buckendahl, Impara, Giraud, & Irwin (2000), the Bookmark did not produce levels of confidence and comfort with the process that were very different than the Angoff method. Reckase (2006a) conducted a simulation study of standard setting processes which attempted to recover the originating performance standard in the simulation model. He studied the impact of rounding error on the final estimates of minimal competency for a single rater during a single round of estimates. His study simulated data using the Angoff and Bookmark methods, and found that error-free conditions during the first round of Bookmark cut scores were statistically

lower than the simulated cut scores (Reckase, 2006a). The estimates of the performance standard from his research study were "uniformly negatively statistically biased" (Reckase, 2006a, p. 14). This trend continued after simulating error into rater's judgments. These results are consistent with other Bookmark research (Green, Trimble, & Lewis, 2003; Yin & Schulz, 2005). While the IRT-based standard setting methods do use a common scale, they all have a potential issue with reliability. Raters are only given one opportunity per round to determine an estimate of minimal competency as they select a single place between items rather than setting performance estimates for each individual item as in the case of the Angoff method. Shultz (2006) suggested a modification to the Bookmark process that involves the selection of a range of items, but there is currently little research on this new proposed modification.

Setting a performance standard with the Angoff method on a smaller sample of multiple choice items and accurately applying it to the larger test form may address some of these standard setting issues (e.g., cognitively taxing process, high expense, time consuming). In fact, it may improve the standard setting process by limiting the number of items and the individual rater decisions. It also has the potential to save time and money as fewer individual items would be used in the process. Before the generalizability process can be applied, however, the various issues and implications involved in the process must be evaluated.

Purpose

The primary purpose of this research was to evaluate the extent to which a single minimal competency estimate derived from a subset of multiple choice items would be

able to generalize to the larger item set. In this context there were two primary goals for this research endeavor: (1) evaluating the degree to which the characteristics of the two item sets and their relationship would impact the ability to generalize minimal competency estimates, and (2) evaluating the degree to which the characteristics of the standard setting process would impact the ability to generalize minimal competency estimates.

First, the characteristics and the relationship between the two item sets were evaluated in terms of their effect on generalizability. This included the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard, and the number of items randomly drawn from the larger item set. Second, the characteristics of the standard setting process were evaluated in terms of their effect on generalizability, specifically, elements such as the number of raters, the 'unreliability' of individual raters in terms of the percentage of unreliable raters and their magnitude of 'unreliability', and the influence of group dynamics and discussion. The following research questions were of interest:

Research Questions

1. To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?
    a. To what extent does the distribution of item difficulties in the larger item set influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the placement of the 'true' performance standard influence the ability to generalize the estimate of minimal competency?

c. To what extent does the number of items drawn from the larger item set influence the ability to generalize the estimate of minimal competency?

2. To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?
    a. To what extent does the number of raters in the standard setting process influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the percentage of 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    c. To what extent does the magnitude of 'unreliability' in the designated 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    d. To what extent do group dynamics and discussion during the second round of the standard setting process influence the ability to generalize the estimate of minimal competency?

Research Hypotheses

1. The following three research hypotheses were related to the research questions involving the extent to which the characteristics and the relationship between the two item sets would impact the ability to generalize minimal competency estimates.
    a. The distribution of item difficulties in the larger item set will influence the ability to generalize the estimate of minimal competency. Item difficulty distributions with a smaller variance in item difficulty parameters will generalize better than item difficulty distributions with a larger variance.

b. The placement of the ‘true’ performance standard will influence the ability to generalize the estimate of minimal competency. A ‘true’ performance standard which is closer to the center of the item difficulty distribution will generalize better than a placement further away.

c. The number of items drawn from the larger item set will influence the ability to generalize the estimate of minimal competency. The larger the number of items drawn the better the generalizability of the estimate of minimal competency.

2. The following four hypotheses are related to the research questions involving the extent to which the characteristics of the standard setting process would impact the ability to generalize minimal competency estimates.

   a. The number of raters in the standard setting process will influence the ability to generalize the estimate of minimal competency. The larger the number of raters involved in the standard setting process the better the generalizability of the estimate of minimal competency.

   b. The percentage of ‘unreliable’ raters will influence the ability to generalize the estimate of minimal competency. Standard setting situations involving a lower percentage of ‘unreliable’ raters will be able to generalize the estimate of minimal competency better than those containing a higher number of ‘unreliable’ raters.

   c. The magnitude of ‘unreliability’ in the designated ‘unreliable’ raters will influence the ability to generalize the estimate of minimal competency. Standard setting situations involving a low magnitude of ‘unreliability’ in the designated

'unreliable' raters will be able to generalize the estimate of minimal competency better than those containing a high magnitude of 'unreliability' in the designated 'unreliable' raters.

d. The group dynamics and discussion during the second round of the standard setting process will influence the ability to generalize the estimate of minimal competency. Group dynamics and discussion that influence the raters towards the center of the rating distribution will generalize better than group dynamics and discussion that influence the raters towards the outside of the rating distribution.

Procedures

This research simulated the individual item level estimates of minimal competency using a Monte Carlo Approach. This approach allowed the control and manipulation of research design factors. The Monte Carlo study included seven factors in the design. These factors were (a) shape of the distribution of item difficulties in the larger item set, (b) the placement of the 'true' performance standard, (c) the number of items randomly drawn from the larger item set, (d) the number of raters in the standard setting process, (e) the percentage of 'unreliable' raters, (f) the magnitude of 'unreliability' in the designated 'unreliable' raters, and (g) the influence of group dynamics and discussion during the second round of the standard setting process. The number of levels for each factor will be described in Chapter Three: Methods.

The ability to 'adequately' generalize the performance standard was evaluated in terms of the differences between the performance standard derived with the larger item

set and the performance standard derived with the smaller subset of multiple choice items. The difference between the originating performance standard and the performance standard derived with the smaller subset of items was also reviewed. The simulation results were evaluated in terms of the location of the performance standard (bias) and the variability of the performance standard (mean absolute deviation, root mean square error).

Limitations

Based on the design of the study and the level of rater subjectivity involved in the standard setting process, there are a number of limitations that must be considered when evaluating the final results of this study. While this study has contained a number of factors to simulate the standard setting process, additional factors affecting the subjectiveness of individual raters such as content biases, knowledge of minimal competency, and fatigue may play a role in determining the final passing standard. These issues would likely affect the other raters in the standard setting process as well. Another inherent limitation of the study is the number of levels within each factor. These levels were selected to provide a sense of the impact of each factor. They were not, however, intended to be an exhaustive representation of all the possible levels within each factor.

Importance of Study

Many factors must be evaluated before concluding the quality of a standard setting process. While standard setting issues such as the dependability and replicability continue to populate the literature, other important issues have been underrepresented. The issue of generalizability is one such issue, and it is important for two reasons. First, it

has the potential to improve the quality of the process by limiting the number of items and individual rater decisions. By reducing the number of items that a rater needs to review, the quality of their ratings might improve as the raters are "less fatigued" and have "more time" to review the smaller dataset (Ferdous & Plake, 2005, p. 186). Second, it has the potential to save time and money for the presenting agency as well as the raters, who are generally practitioners in the profession. This savings may then be spent on improving other areas of the test development process. Reducing the time it takes to conduct the standard setting process may also result in a different class of more qualified raters who may have been unable to otherwise participate due to time constraints. In general, the ability to accurately generalize performance standards may have important implications for improving the quality of the standard setting process and the overall validity of the examination.

Definitions

Angoff Method. A popular method of standard setting proposed by William Angoff in 1971. While Angoff did not originally propose the idea of estimating the proportion of examinees that correctly respond to an item (see Lorge & Kruglov, 1953), his original idea (or versions of it) is still one of the most popular models of standard setting today (Impara, 1995; Kane, 1995; Plake, 1998). The popularity of the Angoff method has decreased slightly over recent years due to the popularity of the IRT-based methods.

Angoff Values. The proportion or number (depending on methodology) of minimally competent examinees predicted to correctly respond to a given item. The

individual Angoff values are usually averaged across raters and added across items to produce a minimum passing score.

Bookmark Method. The IRT-based Bookmark method (Lewis, Mitzel, & Green, 1996) developed by CTB/McGraw Hill was specifically designed to address the deficiency in the Angoff Method (Horn, Ramos, Blumer, & Maduas, 2000). It is one of a family of IRT-based rational methods, which include the Bookmark method (Lewis et al., 1996), the Item Mapping method (Wang et al., 2001), and the Mapmark method (Schultz & Mitzel, 2005). The Bookmark method was intended to work well with multiple item types (selected and constructed response) and simplify the cognitive task for raters (Lewis et al., 1998). It is a multi-round process, similar to the Angoff method. However, instead of presenting the items in administration order, the Bookmark method uses IRT *b*-paramters to order the items according to difficultly in an Ordered Item Booklet (OIB) from easiest to hardest. The Bookmark method only requires that the rater select the specific location in the OIB that separates one level of ability from another (Horn et al., 2000) as opposed to the item-by-item review as in the case of the Angoff method.

Facilitator. The person or persons who conduct the standard setting process. These test development professionals are often psychometricians.

Minimally Competent Candidate (MCC). A candidate or test taker that possesses a minimal level of acceptable performance. It is this individual who is conceptualized by standard setting participants when evaluating test content.

Performance Standard. The performance standard is the "conceptual version of the desired level of competence" (Kane, 1994, p. 426). The passing score of an

examination can be expressed as the "operational version" (Kane, 1994). The performance standard has also been referred to as the minimal performance level (MPL).

Standard Setting. A process for determining a "passing score" or minimal acceptable level of performance (Cizek, 1996).

Subject Matter Experts (SME). Individuals who have an expertise in a given subject area and are "qualified to make judgments" concerning the content (Cizek, 1996, p. 22). SMEs participate in standard setting workshops and judge items for minimal performance levels. It is also preferred that SMEs are familiar with one or more individuals who possess a minimal level of acceptable performance. Subject matter experts are also referred to as raters, judges, or standard setting participants.

Theta-cut or theta_{mc}. The performance standard represented on a theta scale. A theta represents an unobservable construct (or latent variable) being measured by a scale. The theta scale is generally normally distributed, $N(0,1)$, and estimated from item responses given to test items that have been previously calibrated by an IRT model. The theta_{mc} is calculated using a procedure designed to link item ratings and estimates of minimal competency with a common scale (Coraggio, 2005).

Chapter Two:

Literature Review

Introduction

The primary purpose of this research was to evaluate the extent to which a single minimal competency estimate from a subset of multiple choice items could be generalized to a larger set. Specifically, the two primary goals in this research endeavor were (1) evaluating the degree to which the characteristics of the two item sets and their relationship impact the ability to generalize minimal competency estimates, and (2) evaluating the degree to which the characteristics of the standard setting process impact the ability to generalize minimal competency estimates. The literature review is separated into three major sections: types of standard setting methods, issues within the standard setting process, and previous research studies in the areas of standard setting simulation and generalizing performance standards.

Standard Setting Methodology

As previously alluded to in the introduction of the paper, measuring and evaluating change through assessment is now the norm in our society, not the exception. Test developers and psychometricians are now held to tight levels of accountability and legal defensibility. Every stage of the test development process is evaluated for its contribution to the reliability of the resulting scores and the validity of the interpretation of those scores. Of all the stages, the standard setting process has received the most

attention in the literature (Berk, 1986). It has been documented that the standard setting process is one of the most difficult steps (Wang et al., 2001) and may also be one of the most unreliable (Jaeger, 1989b).

While some standards are still set unsystematically without consideration of a particular criterion (Berk, 1986), such as setting an arbitrary predetermined passing score (e.g., score of 70) or establishing a passing standard with a relative standard (quota or an examinee's normative performance level) (Jaeger, 1989a), the current accepted standard setting practices involve the use of an absolute or criterion-referenced process to evaluate the examination items and set an appropriate passing standard. Reckase (2005) stated that "a standard setting method should be able to recover the intended standard for a panelist who thoroughly understands the functioning of the test items and the standard setting process, and who makes judgments without error" (p. 1). Some researchers, however, do not share in Reckase's perspective and warn that a "true" standard or a "best" standard setting practice may not actually exist (Wang et al., 2003).

Almost from the introduction of standard setting (Lorge & Kruglov, 1953), controversy has surrounded the process. Accusations relating to fairness and objectivity have constantly clouded the standard setting landscape, regardless of the imposed methodology. Glass (1978) conducted an extensive review of the various standard setting methods and determined that the standard setting processes were either arbitrary or derived from arbitrary premises. Jaeger (1989b) and Mehrens (1995) found that it was unlikely for two different standard setting methods to result in comparable standards. Behuniak, Archambault, and Gable (1982), after researching two popular standard setting

methods of the time (Angoff and Nedelsky), had similar results determining that different standard setting methods produce cut scores that are "statistically and practically different" and even groups of raters employing the same standard setting method should not be expected to set similar passing standards (p. 254).

Current Standard Setting Methods

In 1986, Berk claimed that there were more than 38 different methods developed to estimate passing standards; by 2003, Wang et al. claimed that there were more than 50 different standard setting procedures (For a detailed description of various methods see Zieky, 2001). Yet, with all the methods available, which methods provide the best results? "The most consistent finding from the research literature on standard setting is that different methods lead to different results" (National Academy of Education, 1993, p. 24).

Due to their increased psychometric rigor and legal defensibility, the absolute or criterion-based methods are currently the most widely applied standard setting methods. Three of the most popular types of absolute or criterion-based methods include the classical rational methods, based on evaluation of test content such as the Nedelsky (1954) and the Angoff (1971) method (or modified variations); the IRT-based rational methods such as the Bookmark method (Lewis et al., 1996), Item Mapping method (Wang et al., 2001), and Mapmark method (Schultz & Mitzel, 2005); and the empirical methods, based on the examinee distribution on some external criterion such as the Comparison Groups method, (Livingston & Zieky, 1982) and the Borderline Groups method (Livingston & Zieky, 1982). Due to the lack of an existing external criterion in

most instances, the focus of this research will be on one of the classical rational methods.

Classical Rational Methods

The classical rational methods rely on the expert judgment of raters. These raters conduct a detailed analysis of each item on the examination in order to establish the minimal performance standard (Muijtjens, Kramer, Kaufman, & Van der Vleuten. 2003).

Nedelsky. The Nedelsky method has declined in popularity since the introduction of the IRT-based standard setting methods. It focuses on the minimally competent candidate and requires a review of every item on the examination, similar to the Angoff method. Rather than estimating a probability based on the overall difficulty of the item, the rater instead focuses on the individual item's multiple choice options and eliminates those that a minimally competent candidate would recognize as incorrect. An individual item probability is then determined from the remaining items (e.g., two remaining options would result in a .50 probability). The individual item probabilities are then averaged across raters and then summed across items to determine the passing standard. This process is sometimes conducted over multiple rounds.

The Nedelsky method, while less cognitively taxing for raters than other methods, does have some inherent weaknesses. It results in a limited number of item probabilities based on the number of multiple choice options. This may not reflect normal test taking behavior by a minimally competent candidate. It is also limited to use with multiple choice style examinations. In comparisons between the Nedelsky and Angoff methods, The Angoff method produced less variability among individual rater estimates (Brennan & Lockwood, 1979).

Angoff and Modified Angoff Method. Angoff's method is still one of the most popular models of standard setting today (Impara, 1995; Kane, 1995; Plake, 1998), though the popularity of the Angoff method has decreased in recent years due to the popularity of the IRT-based methods.

Angoff proposed his idea for standard setting in a book chapter entitled, *Educational Measurement*. It is important to note that Angoff "unfailingly attributed" the development of his standard setting method to Ledyard Tucker even though the method and its modified versions are given only his namesake (Smith & Smith, 1988, p. 259). An original description of the Angoff procedure is reproduced here (Angoff, 1971, p. 515).

> A systematic process for deciding on the minimum raw scores for passing and honors might be developed as follows: Keeping the hypothetical 'minimally acceptable person' in mind, one could go through the test item by item and decide whether such a person could answer correctly each item answered correctly by the hypothesized person and a score of zero is given for each item answered incorrectly by that person, the sum of the item scores will equal the raw score earned by the 'minimally acceptable person.' A similar procedure could be followed for the hypothetical 'lowest honors person.'

This original Angoff method has been described as the Angoff Yes/No method. While it has been used with some success (see Impara & Plake, 1997), it is not as popular as his next suggestion. In a footnote on that same page, Angoff described a variation to the procedure that became known as the Modified-Angoff

Approach to standard setting (Reckase, 2000). Below, the footnote from that page is reproduced (Angoff, 1971, p. 515).

> A slight variation of this procedure is to ask each judge to state the probability that the 'minimally acceptable person' would answer each item correctly. In effect, the judges would think of a number of minimally acceptable persons, instead of only one such person, and would estimate the proportion of minimally acceptable persons who would answer each item correctly. The sum of these probabilities, or proportions, would then represent the minimally acceptable score.

Impara and Plake (1997) conducted a study of both versions that Angoff originally proposed. Their results indicated that the Angoff Yes/No version, while not as popular, produced similar cut score results, was easier to understand for raters, and was easier to use (Impara & Plake, 1997).

Angoff provided no rationale for either of his standard setting methods (Impara & Plake, 1997) and this omission may have led to the many variations of his method that exist today. The Angoff method has been continually adjusted and modified during its history. In fact, Reckase (2000) stated that "there is no consensus on the definition for the modified Angoff process" (p. 3).

As shown from the passages, the Angoff models rely on the opinion of raters who rate each item according to the probability that a "minimally proficient" candidate will answer a specific item correctly (Behuniak et al., 1982). This can be seen as an advantage or as a weakness in this particular method. The Angoff method consists of an item-by-

item rating method similar to the Nedelsky method, but instead of eliminating options, the Angoff method requires participants to indicate the proportion of minimally qualified students who would answer each item correctly based on the difficulty of the item (Reckase, 2000). As with the Nedelsky method, the item ratings are then averaged across raters and combined to create an overall passing standard. The Angoff method generally involves a multi-round process that involves individual ratings as well as group discussion to achieve the final passing standard.

Regardless of the modification, the Angoff method relies heavily on the opinion of individuals and has an inherent aspect of subjectivity that can be of concern when determining an appropriate standard. In fact, it has been described as "fundamentally flawed" in an evaluation of the standard setting process used with the National Assessment of Educational Progress (Pellegrino, Jones, & Mitchell, 1999). IRT-based models were created to address the limitations in the Angoff-based standard setting models.

IRT-Based Rational Methods

In 1982, van der Linden suggested that item response theory (IRT) may be useful in the standard setting process. Yet, limitations with computer technology at the time may have limited the usefulness of IRT during the standard setting workshop process. Modern computer processing speeds and advancement in software have allowed the development of IRT-based standard setting methods designed to improve on the weaknesses in the Angoff method. The IRT-based Bookmark method was specifically designed to address the item-by-item review of the Angoff method (Horn et al., 2000).

Bookmark Method. The Bookmark method was intended to work well with multiple item types (selected and constructed response) and simplify the cognitive task for raters (Lewis et al., 1998). It is a multi-round process, similar to the Angoff and Nedelsky methods. However, instead of presenting the items in the order of administration, the Bookmark method uses IRT parameters to order the items according to difficultly from easiest to hardest in an Ordered Item Booklet (OIB). The Bookmark method only requires that the rater select the specific location in the OIB that separates one ability level from another (Horn et al., 2000) as opposed to the item-by-item review as in the case of the Angoff method. Specifically, the rater is to select the item location for which a minimally competent examinee is expected to have mastered the items below, and conversely, not have mastered the items above (Karantonis & Sireci, 2006). This location is based on a response probability (RP). The RP is the location selected by the standard setting participant where the examinee "has a .67 (2/3) probability of success with guessing factored out" (Lewis et al., 1998, p. 3). By selecting a location where the Bookmark is at the "furthest most item" where this RP is true, a unique location on the ability scale can be estimated and a cut score established (Lee & Lewis, 2001, p. 2). The RP of .67 has been traditionally used due to its ease of understanding for participants (Williams & Schultz, 2005), and its maximizing of the information function in the 3PL IRT model (Huynh, 2000).

The Bookmark method has become increasingly popular for its simplicity. Raters only need to focus on the performance of the "barely proficient" examinee without concern in estimating item difficulty, and raters can perform the required tasks in a much shorter amount of time (Buckendahl, Impara, Giraud, & Irwin, 2000). The Bookmark

method has rapidly grown in popularity from use in 18 states in 1996 (Lee & Lewis, 2001) to use in 31 states in 2005 (Perie, 2005).

In a study comparing the Bookmark and the Angoff methods, Buckendahl et al. (2000) found that while the two methods produced a similar cut score and similar levels of confidence and comfort with the process, the Bookmark method had a lower standard deviation. While they did not conduct a statistical significance test on the differences, they did suggest that this lower standard deviation would indicate a higher level of inter-rater agreement to a policy making body (Buckendahl et al., 2000). One element that may have impacted their results was that their study used Classical Test Theory p-values to create the OIB as opposed to IRT parameters. Other multiple method studies indicate that the Bookmark method consistently produces the lowest cut score among standard setting methods (Green et al., 2003; Yin & Schultz, 2005).

Bookmark Variations. One variation of the Bookmark method is the Item Mapping Method (Wang et al., 2001). In this method, items are sorted according to difficulty (using the IRT b-paramters) based on the Rasch IRT model. A rater examines the items and determines which items a minimally competent candidate would have a .50 probability of answering correctly as opposed to the .67 response probability associated with the Bookmark method.

Another very recent variation of the Bookmark method is the Mapmark method (Schultz & Mitzel, 2005) developed by ACT, Inc. It was recently implemented on the Grade 12 National Assessment of Educational Progress (NAEP) Math test, perhaps in response to the reported "flaws" in the Angoff method. The Mapmark uses "item maps"

(graphical relationships of the items to the proficiency distribution, arranged by content domains) and content domain scores to assist in "significant" discussion about what knowledge, skills, and abilities (KSAs) are being measured (Karantonis & Sireci, 2006). Due to its recent development and single implementation at this point, research on the Mapmark method has been limited.

Yin and Schultz (2005) conducted a study and compared the Mapmark method with the Angoff-based method. Their results suggest that the Mapmark cut scores are lower than those from the Angoff-based method. These results are similar to research findings from the Bookmark method (Green et al., 2003; Yin & Schultz, 2005). Yin and Schultz (2005) also discovered that the individual rater cut scores from the Mapmark method were not normally distributed and contained more extreme scores. In fact, due to the differences, the median cut score has been used as the final performance standard as opposed to the mean cut score (Yin & Schultz, 2005). One weakness of all the IRT-based standard setting methods is that they require large amounts of prior performance data in order to calibrate the items and create the OIB. The specific amount of required prior performance data depends on the IRT model employed (e.g., 3PL vs. Rasch).

Standard Setting Implications

Even if the standard setting process has been properly conducted, the resulting passing standard may have an overall impact (pass/fail rate) that is inconsistent with the expectations of the raters and/or the policy makers (Buckendahl et al., 2000). It is the policy makers, not the raters, who determine the final performance standard (Shepard, 1995). It is critical that policy makers take into account "uncertainty" associated with cut

scores before adopting a new performance standard (Lewis, 1997).

Often these policy makers may change the resulting cut score only a few raw score points. While this may seem trivial, a change of a few raw score points may have significant implications. For example, a change of two raw-score points on a statewide administration of the National Teaching Examination (NTE) mathematics subtest in April 1983 would have resulted in an additional 13% of examinees not passing the assessment (Busch & Jaeger, 1990). Another consideration is the impact (pass/fail rate) on minority groups generally referred to as differential selection. Testing can result in differential selection rates from groups with different group means (Stark, Chernyshenko, & Drasgow, 2004). Setting a high cut score can result in an adverse impact on minority groups and the resulting underselection can result in "contentions" of discrimination (Stark et al., 2004, p. 497).

From a measurement perspective, this issue of differential selection becomes one of discerning between differences due to ability (referred to as "impact" in the measurement literature) and differences due to some assessment measurement bias. The discussion of measurement bias and the differences between impact and bias are outside the scope of this particular paper (for a detailed discussion on measurement bias and impact, see Stark et al., 2004).

From a legal perspective, the issue of differential selection is the perception of discrimination. This legal discussion specifically addresses the use of performance standards in certification and licensure applications.

The federal *Uniform Guidelines on Employee Selection Procedures* state the following:

> A selection rate for any race, sex, or ethnic group which is less than four-fifths (4/5) (or eighty percent) of the rate for the group with the highest rate will generally be regarded by the Federal enforcement agencies as evidence of adverse impact, while a rate greater than four-fifths rate will generally not be regarded by Federal enforcement agencies as evidence of adverse impact (Equal Employment Opportunity Commission, 1978).

The 4/5th rule, as it is commonly known, basically states that the proportion of examinees selected from a minority (focal) group at the selected cut score when compared with the proportions of examinees selected from a majority (reference) group at the selected cut score can not exceed a ratio of .80. This rule places a significant amount of legal burden and potential liability on the policy makers in the fields of licensing and certification in terms of the location of the final performance standard. While there was no direct evidence in the literature of this rule currently being applied to the performance standards in educational testing, it might only be a matter of time in this environment of ever increasing accountability.

Aside from this issue of differential selection, policy makers are also very concerned with the perceptions of their stakeholders: specifically, the taxpayers in the case of educational testing, and licensees in the case of accreditation testing. The resulting performance standard may be "impractical," providing a performance standard that is set too high (low pass rate) or too low (high pass rate). This may bring the validity

of the examination program in to question.

Issues in the Standard Setting Process

Kane (1994, 2001) proposed three types of validity evidence to be used for validating performance standards. They were procedural, internal, and external. Procedural validity evidence focuses on the appropriateness of procedures and quality of implementation (Kane, 1994). Internal validity evidence focuses on the ability of raters to translate the performance standard into a passing score (Kane, 1994). He suggested examining this evidence empirically through the standard error of the cut score. External validity evidence involves external comparisons such as consistency of cut scores across different methods or congruence with external examinee classifications (Kane, 1994).

Kane's validity model claims to provide a way to evaluate the evidence of validity in the standard setting process, but it focuses primarily on the consistency and reliability of the process with only limited consideration to whether the resulting performance standard is truly valid.

Rater Reliability

Individual rater differences were defined in the literature as a threat to standard setting validity (Sato 1975; van der Linden, 1982; Jaeger, 1988). In discussing these differences, van der Linden (1982) coined the term 'intrajudge inconsistency' when referring to individual rater error. This term specifically refers to the inconsistency between item ratings and their associated difficulty (Plake, Melican, & Mills, 1991). For example, a rater assigns a low probability of success to an easy item and a high probability of success to a hard item. Engelhard and Cramer (1992) concluded that most

of the variation was related to judge inconsistency and this related directly to the inconsistency within the ratings of specific judges. Their research raised a concern about the subjectivity of specific judges (Engelhard & Cramer, 1992). Berk (1996) suggested using intrajudge reliability across consecutive rounds as one criterion for establishing the quality of the standard setting process. Berk also suggested evaluating rater variance in the final cut score as another criterion of success (Berk, 1996).

Another approach to identifying rater unreliability was used by Plake and Impara (2001). They sought to understand how well the raters could estimate the item-level performance of examinees at the established minimal performance standard. Their study compared the raters' estimation of the item-level minimal passing standard with the item-level performance of actual candidates who had scores close to the raters' overall estimation of the minimal performance standard. Their research indicates that the average difference between actual and anticipated performance was -.002 with a standard deviation of .09 (Plake & Impara, 2001). One limitation of this approach is that actual performance values are used as opposed to a 'true' value of the minimal performance standard, which is usually never known. Reckase (2006a) in his simulation study used these estimates to postulate that raters have an error distribution with a standard deviation of .1. He used this assumption to stimulate rater error in his simulation model. Operationally, he proposed that the rater had a 95% chance of estimating the probability of a correct response within .2 of the intended performance standard or cutscore (Reckase, 2006a). In other words, if the estimated probability of a correct response for a given item on the IRT theta scale was .7 based on a true performance standard, then the

raters estimate would be between .5 and .9 95% of the time.

One element that has impacted rater reliability (or rater unreliability) has been the inability for raters to judge individual item difficulty. In various research studies, the item difficulty estimates from raters have been at times inaccurate, inconsistent, and contradictory (Bejar, 1983; Goodwin, 1999; Mills & Melican, 1988; Reid, 1991; Shepard, 1995; Swanson et al., 1990; Wang et al., 2001). While raters are able to distinguish between which items were easy and which items were hard, they have had difficulty correctly estimating item difficulty (Shepard, 1995).

This is especially true of estimating item difficulty for minimally competent examinees and this has resulted in either overestimations of minimally competent performance or underestimations of minimally competent performance depending on the items on the examination (Shepard, 1995). One of the main concerns, especially in the Angoff model, has been the ability of raters to "predict" the performance of minimally competent candidates (Irwin, Plake, & Impara, 2000). Raters tend to only think of average examinees as opposed to those that are minimally competent (Bowers & Shindoll, 1989). Shepard (1995) felt raters were essentially unable to estimate the response of the minimally competent candidates. Impara and Plake (1997) conducted a study of two different versions of the Angoff method. Their research suggests that raters found the conceptualization of a 'single' minimally competent examinee easier to comprehend than imagining a 'group' of minimally competent examinees as is prescribed in the method.

Another issue may be a rater's preconceived perceptions of impact prior to the workshop. Buckendahl et al. (2000) examined the consequences of a rater's advanced estimates of impact. They first asked raters their perceptions of a passing rate and then provided passing rate information from previous administrations. What they found was limited evidence to suggest that these early estimates may influence the change and directionality of ratings between rounds (Buckendahl et al., 2000). Jaeger (1982) found evidence to suggest that a rater's background may also have some influence on rater consistency: specifically, the relationship between the rater's background and specific content on the examination (Plake et al., 1991).

Incorporating IRT into the standard setting process may improve the precision of the process by assisting in examining rater variability (van der Linden, 1982). IRT can be used to set estimates of true score or expected observed score for minimally competent examinees (van der Linden, 1982). IRT can also be used to identify the variability of item difficultly estimates for individual raters. Raters with extreme ratings and raters who were inconsistent in terms of their definition of minimal competency based on their item ratings can be identified using IRT (Kane, 1987).

Influence of Group Dynamics

Variability among group participants may account for differences in standard setting results. Livingston (1995) in a study of the Angoff method reported a likely group-influenced biasing effect of regression to the mean. Hertz and Chin (2002), after studying group variability, proposed that standard setting studies should focus on the interaction among groups as well as on group instruction (training) and individual rater

differences. They proposed that the best model for standard setting should be one that minimizes the effect of the group and simplifies the process (Hertz & Chin, 2002). One unique study by Wiley and Guille (2002) looked at an occasion effect for participants that established their judgments individually without any type of group interaction. While the mean difference between the collaborative group and the "at-home" raters was only 1.20 points, the "at-home" ratings did have more variability and resulted in a slight item-occasion interaction. The design of the study may also have tempered the results. "At-home" raters were experienced with the Angoff standard setting process and had access to 13 anchor items that had previously been rated. While additional research is needed on the subject of group influence, this study suggests an impact of group interaction on the resulting performance standard. Group variability is likely influenced by the social interactions during the standard setting discussion process.

Most standard setting methods include some type of social interaction among participants, with many of the models requiring multiple rounds of discussion before the final minimum passing score is determined. Multiple consecutive rounds of ratings are designed to "foster convergence of views" as the workshop progresses (Karantonis & Sireci, 2006). Some researchers even suggest providing normative information on examinee test performance to assist raters in adjusting judgments between rounds (Cizek, 1996). Research on this issue suggests that providing this information to raters will produce "small and inconsistent" changes in the overall mean performance standard, but will result in lower rater variability (Busch & Jaeger, 1990, p. 148). Rater cut score variability has been used as criterion for determining the quality of the standard setting

process (Berk, 1996).

One relevant factor that may provide some of the inconsistencies in standard setting results is social influences (Hertz & Chinn, 2002). Group discussion and interaction can dramatically change individual perceptions about the difficulty of an item, and some individuals are more prone than others to change their perceptions. Some raters have reported feeling pressured to change their original ratings (McGinty, 2005). In fact, the opinion of the group is on average more extreme after the group discussion, than it is before the group discussion (Fitzpatrick, 1989). The minority position also may have a difficult time convincing the majority position during a discussion. The "most likely result" is for the minority group to give in to the majority position (Hertz & Chinn, 2002, p. 6). Group discussion has resulted in lower rating variability, and this lower variability has been traditionally used by practitioners as one measure of standard setting quality. Raters in the same group employing the same standard setting method had ratings that were more similar than raters in different groups employing the same method (Behuniak et al, 1982). One meta-analysis on different variations of the Angoff method resulted in higher degree of consensus and a higher overall minimal passing standard when participants focus on a common definition of minimal competency and discuss their individual estimates as a group (Hurtz & Auerbach, 2003). This higher degree of consensus may be due to the influence of the dominant interactions by the majority group. Improving the reliability does not necessarily imply improving the validity of the passing standard. An issue even bigger than the reliability of the standard setting process, may be the validity (McGinty, 2005). Researchers tend to focus more on reliability

because it can be “more easily” established and researchers are more “comfortable” with the idea of replicability.

The literature on standard setting has presented several suggestions for improving the reliability and replicability of the standard setting process. These suggestions include selecting qualified raters, using proper rater training on procedures and providing a clear definition of minimal candidate competency (Mills, Melican, & Ahluwalia, 1991; Plake et al., 1991); providing preexisting item performance data (Plake et al., 1991; Kane, 1994); and ensuring that judges have an expertise in their domain (Jaeger, 1991). Many of these “suggestions,” however, do not guarantee valid results (McGinty, 2005).

McGinty (2005) suggests that while convergence is often the goal of standard setting processes, it may have two major flaws. First, the resulting convergence may be “artificially” derived and, second, the resulting convergence may be the result of “undesirable” influences. Berk (1995) discussed the subjectivity and imprecision involved in the process, while van der Linden (1995) emphasized “feelings of arbitrariness” (p. 100). Overall, there have been a limited number of research studies attempting to examine the cognitive process of standard setting participants (Ferdous & Plake, 2005).

Participant Cognitive Processes

Most judgmental standard setting methods are cognitively taxing for raters. Each method requires raters to develop some type of hypothetical construct related to the content and the minimally competent examinee (Demauro, 2003). This hypothetical construct consists of either a knowledge and skills domain with criteria for inclusion or a

body of knowledge and skills within a hypothetical minimally competent examinee (Demauro, 2003).

Skorupski and Hambleton (2005) used a variety of questionnaires at various times during the standard setting process to examine what raters were thinking when they were participating in standard setting studies. Based on the results of the study, raters reported that they felt rushed and they also seemed to report more confidence and understanding in the standard setting process than they actually had (Skorupski & Hambleton, 2005). The study reported that the raters arrived at the standard setting workshop with different ideas about why they were there, the importance of the process, and the definitions of the performance level descriptors.

Giraud, Impara, and Plake (2005) conducted a study examining teachers' conceptions of the target examinee and found that teachers had a similar characterization of minimally competent students even in different workshops, with different content, different grade levels, and different school districts. This suggested that some outside influence was affecting the teachers' perceptions of minimal competence. The authors felt this result was due to either a common idea of competency across teachers or some aspect of the workshop process (Giraud et al., 2005).

One issue repeatedly referenced in the literature is the 'basis' for the judgment of minimal competency. Even workshop facilitators have been inconsistent when recommending whether raters should base their ratings on "how minimally competent examinees *should* perform" rather than "how they *could* perform" (McGinty, 2005). Angoff evidently made no distinction in how raters should address this perception when

using his own method (Zieky, 1995). Ambiguity in this interpretation could have negative implications for the resulting performance standard. Raters generally believe that *should* represents a higher standard than *would* (Impara & Plake, 1997). This issue while important to the process of standard setting has been mentioned very infrequently in the literature.

McGinty (2005), after conducting a qualitative study on the perceptions of standard setting participants, described the entire standard setting process as "elusive and fraught with subjectivity" (p. 270). She continued by describing the process as including many features that are not "amenable to psychometric analysis" (p. 270). The findings from her were presented as three primary themes:

1. Panelists had difficulty with the Angoff method, and the difficulty lay primarily in the confusion between prediction and value judgment,
2. Panelists felt a tension between the desire to set high standards and the desire to be viewed by the public as doing a good job, and
3. Many panelists were skeptical about how their input would actually be used (McGinty, 2005, p. 278).

The validity evidence in the standard setting process should be focused on each stage of the process: inputs, process, outputs, and consequences (McGinty, 2005). McGinty (2005) suggests that most "direct and compelling evidence" of validity in the standard setting process would be associated with consequences of the process (p. 271).

Identifying Sources of Error

Kane (1987) recommended a study that would identify variability due to different sources of variance such as item variance and rating variance. Lee and Lewis (2001) conducted a generalizability study using the Bookmark method. Their results suggest that small group and participants effects are 'non-negligible' and, that for a fixed number of raters, increasing the number of small groups will likely increase the reliability of cut scores (Lee & Lewis, 2001). Few studies have also been conducted that examine the issues related to the standard error of the cut score. These include studies of the Angoff and Nedelsky methods (Brennan & Lockwood, 1980; Kane & Wilson, 1984) as well as a generalizability based study of the Bookmark method (Lee & Lewis, 2001).

Previous Simulation and Generalizability Studies

The existing literature on standard setting simulations and the generalizability of performance standards is sparse. This may be the result of the subjective nature of the standard setting process.

Previous Simulation Studies

Reckase has been more involved than most researchers in the area of standard setting simulation. He published an article in *Educational Measurement: Issues and Practice* that generated some attention (Reckase, 2006a). The editor of the journal issue referred to Reckase's article as "generating controversy" and suggested that researchers may be moving towards a "unifying theory" of standard setting that addresses "social interaction processes" and "social psychology findings" on human behavior and decision-making (Ferrara, 2006, p. 2). Reckase's (2006a) study simulated data using the Angoff

and Bookmark methods, and found that error-free conditions during the first round of Bookmark cut scores were statistically lower than the simulated cut scores (Reckase, 2006a). This trend continued after simulating error into rater's judgments. These results are consistent with other Bookmark research (Green et al., 2003; Yin & Schulz, 2005).

As one might expect, Reckase's article resulted in immediate commentary. Schultz (2006) published an article in the very next issue of *Educational Measurement: Issues and Practice.* Reckase (2006b) also published a rejoinder in that same issue as well. Reckase's original article proposed a conceptual framework that he described as a "psychometric theory of standard setting" (Reckase, 2006a, p. 4). He suggested that this theory was closely related to the true score theory used in psychometrics. That is to say, a "standard setting method should be able to recover an intended cut score (ICS)" (Reckase, 2006a, p. 4). He proposed three criteria for evaluating standard setting procedures: (1) whether the ICS could be recovered if there was no error in the process, (2) whether the process used for estimating the cut score was statistically unbiased, and (3) whether the resulting estimates of the cut scores have small standard errors (Reckase, 2006a). One issue in the research design was that only the initial round of ratings for a single rater was simulated. This study did not take in consideration any social interactions between participants that generally occur after the first round. It was this issue along with the ability of the simulations to represent "actual outcomes" of Bookmark and Angoff procedures that was the focus of Schultz's (2006) commentary. In his response, Shultz proposed a different modified version of the Bookmark procedure that uses multiple selections of items by raters (Shultz, 2006). In simulation, this

modification showed "considerable promise" over the traditional Bookmark procedure (Reckase, 2006b). Shultz also proposed a different rater error model for the Angoff method that involved uniform regression across the scale to some fixed value (Shultz, 2006). Schultz proposed that this fixed value might be 0.5 based on his review of previous standard setting studies (see Shepard, 1995; Heldsinger, Humphry, & Andrich, 2005). Reckase (2006b) in his rejoinder, proposed the following adjustment formula to address this potential uniform regression:

$$rating = .5 + (rating - .5) * .8$$

This adjustment when simulated may suggest an initial overestimation of probabilities in the early rounds of the standard setting process and a subsequent downward adjustment as raters get feedback in later rounds of the process.

Previous Studies of Performance Standard Generalizability

Sireci et al. (2000) conducted a study involving the setting of performance standards using only partial item sets. The study evaluated the differences between three different Angoff based methods of standard setting that were used to set standards for Computer Adaptive Testing (CAT) items (Sireci et al., 2000). The three models included a more traditional modified Angoff method along with two newer Angoff based methods designed with time-saving modifications. In the world of Computer Adaptive Testing (CAT), the available set of items is the entire bank as opposed to a single set of examination items. Generalizability of performance standards can be very important as it relates to saving time and expense in the standard setting process. One additional criterion of their study was to evaluate the consistency of derived cut scores over the item

subsets. The item subsets were evaluated as thirds of the total set (112 items). The results of the study suggest that two of the three subsets or 2/3 of the total items produced standard setting results "relatively similar" to the entire item set (Sireci et al., 2000, p. 24). The maximum cut score deviation for all but one of the analyses was about a tenth of a standard deviation. For the Angoff method, it was just 2.49 and 2.06 score points different depending on the instrument (Introductory Algebra and Intermediate Algebra). Conversely, using just one of their three subsets (or a third of the items) it was 4.94 and 3.71 points and about two-tenths of a standard deviation. Based on the results, Sireci et al. (2000) suggested estimating performance standards with only partial items sets is "promising and deserves further study" (p. 28). These results suggest the feasibility of performance standard generalization. One limitation of their study, however, is that it was conducted with only a single panel (thirteen raters) and a single test instrument.

Ferdous and Plake (2005) conducted a later study that provided an even greater promise of the feasibility of generalizing performance standards. Their research study included two different Angoff standard setting studies from a mental health program conducted in 1995 and 2000, and one Angoff standard setting study from a financial analyst program conducted in 2001 (Ferdous & Plake, 2005). Eight subsets of items were extracted from the original standard setting studies using a stratified sampling technique. Item difficulty categories were stratified to match the proportion of item difficulties on the full length tests. The minimum passing scores were evaluated for each sample and compared to the full length test. A subset of half the items was consistently within one point of the minimum passing score of the full test. To validate their results, the

researchers repeated the process two times for each test form and produced similar results. The results of their study suggest that a stratified sample of 50% of the items may be 'sufficient' to estimate a minimum passing score (Ferdous & Plake, 2005).

An index for intrajudge inconsistency was also calculated for the full test and the subsets using a procedure developed by Chang (1999). The formula is shown below:

$$\bar{d}_j = \sum_i \left| P_{ij} - P_{ie} \right| / n_i$$

Where,

P_{ij} is the item performance estimate for judge j, item i;

P_{ie} is the empirical p value for item i; and

n_i is the number of items.

When the mean intrajudge inconsistencies were compared between the 50% item subset and the full test items, the results were almost identical. For the 1995 mental health program, the intrajudge consistency was 0.13 (SD = 0.05) as compared to 0.12 (SD = 0.05); for the 2000 mental health program, the intrajudge consistency was 0.08 (SD = 0.03) as compared to 0.08 (SD = 0.04); and for the 2001 financial analyst study, the intrajudge consistency was 0.36 (SD = 0.02) as compared to 0.36 (SD = 0.02). While this study examined the stability of standard setting results across subject areas and occasions for multiple groups, it was limited by the fact that the same standard setting group was used for each test form (Ferdous & Plake, 2005). In other words, the same group participated in the standard setting process for the full set of test items. Samples were then derived from this larger set of items. A model in which different raters participated in only rating subsets of items may produce different results. Also the raters

might consider their ratings differently if they were permitted more time with fewer items to evaluate.

Summary of the Literature Review

There are currently a number of standard setting options available. Some are set unsystematically, while others use a predefined process to evaluate the examination content. The more widely accepted methods are the rational methods which evaluate item content to determine a passing standard. While the Bookmark method has rapidly grown in popularity since its introduction (Lee & Lewis, 2001; Perie, 2005), the Angoff method is still one of the "most prominent" and "widely used" standard setting methods (Ferdous & Plake, 2005). The Angoff and Bookmark methods; however, still carry a weight of controversy.

The primary indicator of standard setting quality is reliability and consistency. With this type of focus, issues such as rater reliability (or unreliability), group dynamics, and the cognitive complexity of the standard setting process have largely dominated the literature. Little research has been conducted on attempting to understand the impact and replicate the effect of some of these issues. Reckase (2006a, 2006b) with his 'psychometric theory of standard setting,' and Shultz (2006) with his detailed criticisms and suggestions have contributed to this area of standard setting simulation. This study is designed to further research on standard setting simulation by attempting to incorporate rater reliability and group dynamics into the simulation model. The few studies that have researched the feasibility of generalizing performance standards have produced favorable results (Sireci et al., 2000; Ferdous & Plake, 2005). This research will also attempt to

expand on the limited research currently available on the ability to generalize performance standards.

Chapter Three:

Method

Purpose

The primary purpose of this research was to evaluate the extent to which a single minimal competency estimate from a subset of multiple choice items would generalize to the larger item set. Within this context there were two primary goals in this research endeavor: (1) evaluating the degree to which the characteristics of the two item sets and their relationship would impact the ability to generalize minimal competency estimates, and (2) evaluating the degree to which the characteristics of the standard setting process would impact the ability to generalize minimal competency estimates. The following research questions were of interest:

Research Questions

1. To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?
    a. To what extent does the distribution of item difficulties in the larger item set influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the placement of the 'true' performance standard influence the ability to generalize the estimate of minimal competency?
    c. To what extent does the number of items drawn from the larger item set influence the ability to generalize the estimate of minimal competency?

2. To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?
    a. To what extent does the number of raters in the standard setting process influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the percentage of 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    c. To what extent does the magnitude of 'unreliability' in the designated 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    d. To what extent do group dynamics and discussion during the later rounds of the standard setting process influence the ability to generalize the estimate of minimal competency?

Research Hypotheses

1. The following three research hypotheses were related to the research questions involving the extent to which the characteristics and the relationship between the two item sets would impact the ability to generalize minimal competency estimates.
    a. The distribution of item difficulties in the larger item set will influence the ability to generalize the estimate of minimal competency. Item difficulty distributions with a smaller variance in item difficulty parameters will generalize better than item difficulty distributions with a larger variance.
    b. The placement of the 'true' performance standard will influence the ability to generalize the estimate of minimal competency. A 'true' performance standard

which is closer to the center of the item difficulty distribution will generalize better than a placement further away.

c. The number of items drawn from the larger item set will influence the ability to generalize the estimate of minimal competency. The larger the number of items drawn the better the generalizability of the estimate of minimal competency.

2. The following four hypotheses are related to the research questions involving the extent to which the characteristics of the standard setting process would impact the ability to generalize minimal competency estimates.

   a. The number of raters in the standard setting process will influence the ability to generalize the estimate of minimal competency. The larger the number of raters involved in the standard setting process the better the generalizability of the estimate of minimal competency.

   b. The percentage of 'unreliable' raters will influence the ability to generalize the estimate of minimal competency. Standard setting situations involving a lower percentage of 'unreliable' raters will be able to generalize the estimate of minimal competency better than those containing a higher number of 'unreliable' raters.

   c. The magnitude of 'unreliability' in the designated 'unreliable' raters will influence the ability to generalize the estimate of minimal competency. Standard setting situations involving a low magnitude of 'unreliability' in the designated 'unreliable' raters will be able to generalize the estimate of minimal competency better than those containing a high magnitude of 'unreliability' in the designated

'unreliable' raters.

d. The group dynamics and discussion during the second round of the standard setting process will influence the ability to generalize the estimate of minimal competency. Group dynamics and discussion that influence the raters towards the center of the rating distribution will generalize better than group dynamics and discussion that influence the raters towards the outside of the rating distribution.

Simulation Design

This research simulated the individual item-level estimates of minimal competency using a Monte Carlo Approach. This type of approach allowed the control and manipulation of research design factors. Every simulation study begins with various decision points. These decision points represent the researcher's attempt to ground the simulation process in current theory and provide a foundation for the creation of 'real life' data and results that can be correctly generalized to specific populations. The initial decision points involved in this simulation were the type of standard setting method, the type of IRT model, and the number of items to be evaluated.

The two most popular standard setting methodologies are the Angoff and Bookmark methods. The Angoff method was selected over the Bookmark method as the standard setting method for this study due to its popularity of use (Ferdous & Plake, 2005), stronger ability to replicate the performance standard (Reckase, 2006a), and greater amount of general research as well as research on the ability to generalize performance standards. In fact, the Bookmark method is "based on the least amount of

research of any (standard setting) method" (R. Hambleton, NCME session, April 10, 2007). The IRT method selected was based on the characteristics of the items. Multiple choice items were used and the three-parameter IRT model which incorporates a pseudo guessing parameter is the most appropriate IRT model for this type of item. The decision to use a large sample of items for the larger sample was based on the research questions. There would be less economic value in dividing a small sample of items into even smaller samples. After deciding on the initial elements or decision points in the simulation process, the design of the process was formulated.

From a conceptual standpoint, the simulation took place in two distinct steps: data generation and data analysis. The data generation step consisted of simulating the standard setting participant's individual estimates of minimal competency and calculating the resulting item-level estimates of minimal competency. These minimal competency estimates were simulated using 143 IRT item parameters and a pre-established 'true' performance standard.

The second step or data analysis step of the simulation process consisted of forming a smaller item set by drawing a stratified random sample from the larger item set. The resulting performance standard established with this smaller item set was then compared to the performance standard from the larger item set as well as the 'true' performance standard used to originally simulate the data. The process was repeated across the different levels of the factors in the simulation process.

Simulation Factors

The simulation factors were evaluated in terms of their impact on generalizability. The simulation factors were separated into two areas: those related to the characteristics and relationship between the item sets, and those related to the standard setting process.

The characteristics and the relationship between the two item sets included three factors; a) the distribution of item difficulties in the larger item set, b) the placement of the 'true' performance standard, and c) the number of items randomly drawn from the larger item set.

a. *Distribution of item difficulties in the larger item set.* An ideal examination instrument is most effective "when the test items are neither too difficult nor too easy" for the examinee (Lord, 1980, p. 150). From an IRT perspective where the items and examinees are placed on the same theta scale, the shape of the item difficulty distribution is often a function of the intended purpose and use of the examination results. A wider distribution of item difficulties would be preferred in the case of an examination that intended to measure a wide variety of abilities such as academic placement tests (e.g., SAT, GRE, ACT, etc.). This would allow a maximum amount of information (low standard error) to be collected across a large number of different ability levels. Conversely, a more narrow distribution would be more appropriate in the case of a credentialing examination, such as a certification or licensing examination, where "measurement precision" is required at the point of the performance standard (Gibson & Weiner, 1998 p. 299). This would provide a maximum amount of information, and hence a lower

standard error, at the point of the performance standard. In order to capture the impact of both of these situations as well as to address some of the issues related to simulated vs. real data, this factor included four levels. The first level of this factor was the difficulty distribution of 143 actual items [published in an Educational Measurement: Issues and Practice article (Reckase, 2006a)]. This 'real' item difficulty distribution had the second largest standard deviation and largest range of item difficulty values (*b*) of the four distributions. It was clearly designed to measure a wide range of abilities. The next three distributions were simulated. Various models have been proposed to simulate IRT parameters. These include sampling from uniform, beta, normal, and lognormal distributions. These simulations are often used to create items that cover a wide range of items with realistic or sometimes non-realistic characteristics to test various assumptions (see Gao & Chen, 2005 for an example of simulating parameters using uniform and four parameter beta distributions). To create simulated data as close as possible to actual data, the three simulated item parameter distributions were based on item parameter distributions from an existing examination program. The second distribution was a distribution of item difficulties based on the marginal distributions from the SAT. This simulated SAT distribution had the second smallest standard deviation and second smallest range of item difficulty values (*b*). A test of this nature would be designed to measure a wide range of abilities. The distributions of the IRT item parameter distributions for the second level were $a \sim N(0.8,0.2^2)$, $b \sim N(0, 1)$, and $c \sim N(0.2, 0.03^2)$ [Wainer, Bradlow,

& Du, 2000]. The third distribution was similar to the second with a reduced variance for the *b*-parameters. The *a*- and *c*- parameters distributions were the same as the second distribution and the b-parameter distribution was $b \sim N(0, 0.5)$. This second simulated SAT distribution with the lower variance in item difficulty parameters had the smallest standard deviation and the smallest range of item difficulty values (*b*). A test of this nature would be designed to measure a more narrow range of abilities as in the case of a licensure or certification examination. Ideally, an examination or bank of items for mastery testing would consist of items with item difficulty parameters around the performance standard (Embretson & Reise, 2000). This would provide a maximum amount of information (or conversely a low standard error) around the performance standard. The fourth distribution was based on the *a*- and *c*- parameters distributions from the SAT examination, but with a uniform distribution, *UNIFORM* (-3, 3), to simulate the *b*-parameters. This fourth simulated SAT distribution with the uniform item difficulty distribution had the largest standard deviation and the second largest range of item difficulty values (*b*). A test of this nature would be designed to measure a wide range of abilities. To avoid any unusual parameter estimates, the *a*–parameter was left truncated at 0.3, and the c-parameter was left truncated a 0.0 and right truncated at 0.6 (Wang, Bradlow, & Wainer, 2002). The four factors of the distribution of item difficulty were the 'real' item difficulty distribution, the simulated SAT item difficulty distribution, the simulated SAT item difficulty distribution with lower variance, and the

simulated SAT uniform item difficulty distribution.

b. *Placement of the 'true' performance standard.* While considerable research has been conducted on the process of developing performance standards in terms of creating definitions and descriptions of minimal competent performance (Fehrmann, Woehr, & Arthur, 1991; Giraud, Impara, & Plake, 2000; Reid, 1985), little has been conducted on the impact of the specific placement of that standard on a common theta scale. This is largely due to the limited simulation research in the area of standard setting. Reckase (2006a), one of the few researchers conducting simulation studies involving the use of a theta scale for determining minimal performance, proposed that the minimal performance level or cutscore is "analogous to the true score in true score theory" (p. 5). He referred to the minimal performance standard as a "hypothetical construct" that is the "ideal operationalization" of the rater's interpretation of policy. He provided an example where 66% of the population would be deemed above proficient. This percent was selected since it is the typical percent above Proficient for states reviewed by the Ad Hoc Committee on Confirming Test Results (2002) appointed by the National Assessment Governing Board. Reckase proposed that this standard would be equivalent to -0.4 on the IRT theta scale. In a standard normal distribution, 66% of the distribution is above this point. In Reckase's study, he used a variety of performance standards from -3.00 to 3.00 or the majority of the theta scale (Reckase, 2006a). Due to the scope and complexity of this research in terms of the number of factors and their associated levels, this

factor included three levels in the central region of the theta scale, θ_{mc} = -1.0, 0, and 1.0. While not addressing all possible levels of theta, -∞ to +∞, these three levels of the theta scale addressed the large percentage of examines that fall within the center of the ability distribution.

c. *Number of items randomly drawn from the larger item set.* As previously mentioned, the larger item set contained 143 items. For the individual subsets, there were six levels of this factor: 36, 47, 72, 94, 107, and 143 items. The full item set was included as part of the comparison to the "true" originating theta value as well as a quality control check in the simulation model. These item sets represented approximately 25%, 33%, 50%, 66%, 75%, and all of the total number of items.

The characteristics of the standard setting process included elements such as the number of raters, the 'unreliability' of individual raters in terms of the percentage of unreliable raters and their magnitude of 'unreliability', and the influence of group dynamics and discussion.

a. *Number of raters.* The size of the panel should be large enough to provide a precise estimation of the passing standard that would be recommended by the entire population of raters (Jaeger, 1989a). The number of recommended raters for an Angoff method standard setting approach varies throughout the literature. Livingston and Zieky (1982) suggested as few as five participants can be adequate. Mehrens and Popham (1992) suggested that 20 to 25 raters should be involved in the standard setting process. Brandon (2004) after a review of a

number of Angoff-based standard setting studies proposed that the number should be at least 10, and 15 to 20 in ideal circumstances. Jaeger (1989a) proposed a method for calculating the required number of raters using a comparison between the standard error of the mean recommend cut score and the test's standard error of measurement. He proposed that 13 raters would have been sufficient in the majority of standard setting that he reviewed (Jaeger, 1989a). Based on the recommended research on the issue of the number of raters, this study could potentially have levels of the number of raters factor that are as few as five (Livingston and Zieky, 1982) and as many as twenty-five (Mehrens and Popham, 1992). However, since one premise of this research is to explore the potential savings of a standard setting model which includes fewer overall items, this study will use a more conservative stance on the number of raters in line with the potential economic advantage of the proposed generalizability model. In keeping with the recommendations of the majority of researchers and at the same representing a sufficient range of the number of raters, the factor for the number of raters will have three levels: 8 raters, 12 raters, and 16 raters.

b. *Percentage of unreliable raters.* Evidence exists suggesting that some raters tend to be unreliable in their individual estimates of minimal performance (Engelhard & Cramer, 1992). Schultz (2006) stated that "item rating errors are an acknowledged component of variation" in Angoff standard setting cut scores (p. 5). Shepard (1995) suggested that rater judgments were "internally inconsistent and contradictory" (p. 151). Some raters have difficulty estimating hard and easy

items (Lorge & Kruglov, 1953; Mattar, 2000; Shepard, Glaser, Linn, & Bohrnstedt, 1993). Mattar (2000) proposed that one reason for this rater unreliability may be the tendency to make central judgments of difficulty regardless of the difficulty of the items. Recent research in the area of standard setting simulation with the inclusion of error has only been conducted on a single rater (Reckase, 2006a; Reckase, 2006b; Schultz, 2006), rather than an evaluation of the cumulative impact of multiple raters. This simulation research attempted to model error for multiple 'fallible' raters. While all raters in the simulation were simulated to contain some minimal level of unreliability (ρ_{XX} = .95), this factor simulated those raters deemed to be 'fallible' in the simulation study. This percentage of unreliable raters factor contained three levels: 25% of the total raters, 50% of the total raters, and 75% of the total raters.

c. *Magnitude of 'unreliability' in unreliable raters.* Item difficulty estimates have been inaccurate, inconsistent, and contradictory (Bejar, 1983; Goodwin, 1999; Mills & Melican, 1988; Reid, 1991; Shepard, 1995; Swanson, 1990; Wang et al., 2001). Raters have had trouble 'predicting' the performance of minimally competent candidates (Irwin et al, 2000). Raters also tend to think of average examinees as opposed to minimally competent examinees (Bowers & Shindoll, 1989). Other issues such as preconceived perceptions of impact (Buckendahl et al., 2000) and a rater's background in relation to specific content on the examination (Plake et al., 1991) may impact a rater's reliability. Factors such as the training of standard setting participants and a well-developed definition of

minimal competency may also impact rater reliability. Researchers have suggested using models such as generalizability theory and rater judgments/p-values differences to determine rater reliability. Checking for rater accuracy, however, requires a known "true" value of minimal competency and some researchers argue that such a value does not exist (Schultz, 2006; Wang et al., 2003). Without the ability to determine a known "true" value of the minimal performance, it is difficult to assess the number and magnitude of unreliable raters in a standard setting process. This factor had three levels of reliability: ρ_{XX} = .65, .75, and .85. These levels were selected based on general acceptable levels of reliability in testing.

d. *Influence of group dynamics and discussion.* Consecutive rounds of ratings are designed to "foster convergence of views" as the workshop progresses (Karantonis & Sireci, 2006). Extreme raters are given the opportunity to support their positions during the discussion phase of the Angoff standard setting process (Cizek, Bunch, & Koons, 2004). This discussion provides raters the opportunity to discuss and share pertinent item and examinee information related to examinee performance (Fitzpatrick, 1989). Livingston (1995) in a study of the Angoff method reported a likely group-influenced biasing effect of regression to the mean. Fitzpatrick (1989) suggested a group polarization effect during the discussion phase. Group polarization is described as a moderate group position becoming more extreme in that same direction after group interaction and discussion (Myers & Lamm, 1976). To simulate these possible social influences

in discussion round of the standard setting process, there were three levels of this factor: lowest rater influence, highest rater influence, and average rater influence. To address the directional influence of dominant raters, individual rater performance estimates were adjusted directionally based on the influence of one of the three levels of the factor using the following formula:

$$rating = \varepsilon + (rating - \varepsilon) * influence\_factor$$

Where, ε is the rating of the influencing rater and the *rating* represents the individual item rating of the standard setting participant. The *influence_factor* is an estimate of the rater's level of influence. It was calculated using random variables sampled from a normal distribution, $N(0,1)$. These sampled values were multiplied by a standard deviation of 0.1 and added to a mean of 0.7. This influence factor was assigned to each individual rater and used systematically in each of their item ratings. This adjustment is based on one proposed by Reckase (2006b) in which ε was a constant of .5. His proposed adjustment was specifically designed to address uniform rating regression and contained a constant of .8 for the influence factor. The values in the equation were chosen to represent actual changes that occur in ratings during the discussion round of the standard setting process. Brandon (2004) conducted a review of Angoff–based standard setting research and found a mean reduction in variation of 31% (SD=21.0) for 17 of the 19 examinations that he reviewed. As a result of this reduction in Angoff estimates, Brandon suggests that the second round of the process involving discussion and a review of empirical data, "positively affect

agreement on item estimates" (Brandon, 2004, p. 79).

Table 1

Simulation Factors and the Corresponding Levels

Factor	Levels
1. Characteristics and the Relationship between the Two Item Sets	
a) item difficulties distribution (larger set)	'real', simulated SAT, simulated SAT with lower variance, simulated uniform difficulty
b) 'true' performance standard	θ_{mc} = -1.0, 0, 1.0
c) number of items randomly drawn	36, 47, 72, 94, 107, 143 items
2. Standard Setting Process Characteristics	
a) number of raters	8, 12, 16
b) number of unreliable raters	25%, 50%, 75%
c) magnitude of 'unreliability' in unreliable raters	ρ_{XX} = .65, .75, .85.
d) influence of group dynamics and discussion	Lowest rater, highest rater, average rater

By crossing the seven factors in this simulation model, a total of 5,832 conditions were simulated. Aggregating results over a number of replications has been shown to produce more stable and reliable findings resulting in more precision in the estimated parameters (Dawber, Rodgers, & Carbonaro, 2004). Thus, increasing the number of replications is a recommended technique for reducing the variance of estimated

parameters (Harwell, Stone, & Kirisci, 1996).

The selected number of replications for each condition was based on balancing time to simulate with the precision of the estimates. Table 1 provides a list of each of the factors and their corresponding levels.

Preliminary estimates suggested that a single condition should take approximately 3 seconds to complete. Simulation studies using a proportion as an outcome variable have provided adequate precision with one thousand replications (see Robey & Barcikowski, 1992). While this study does not contain a proportion as an outcome variable, this number of replications served as the starting value in the simulation model. Outcome variables were monitored to ensure adequate precision in the estimates. One model that was used to monitor the precision of the estimates was a review of the variability across different subsets of replications.

Table 2

Example Comparison of Estimated RMSE across Replication Sizes

Replications	RMSE between Samples	RMSE Between True and Large Sample	RMSE Between True and Small Sample
100	0.033	0.156	0.177
200	0.031	0.156	0.177
300	0.029	0.155	0.175
400	0.031	0.156	0.177
500	0.031	0.156	0.177
600	0.029	0.158	0.178
700	0.029	0.157	0.177
800	0.029	0.156	0.176
900	0.029	0.155	0.175
1000	0.029	0.154	0.174

Table 2 provides such an example from preliminary work on the simulation

model. The outcome variables such as the estimated root mean squared error (RMSE) was compared across different sets of replications, 100 to 1,000 in increments of 100 in this example. This change in the RMSE estimates across the different sets of replications can be used to determine whether an appropriate level of precision has been achieved in the estimates. For example, the estimated RMSE 'across' the two samples results in no change (three decimal places) from 600 through 1,000 replications. The change in the estimated RMSE for the other comparisons (between true and large; between true and small) shows a difference of 0.004 over the same sets of replication sizes. Based on the number of conditions and the 1,000 replications for each condition, the total number of simulations was 5,832,000.

The original estimate was that the full simulation would take 3,037.5 hours of computer time or roughly 31.6 days to complete the simulations using three computers running non-stop 24 hours per day. This original estimate was very close to the actual time it took to run the simulations.

Simulation Procedures

Figure 1 displays a flowchart containing each phase of the simulation process, including the two rounds involved in data generation, the creation of the datasets, and the evaluation of the results. In addition to the phases in the simulation process, the simulation constants and various design factors points in the process are noted as well.

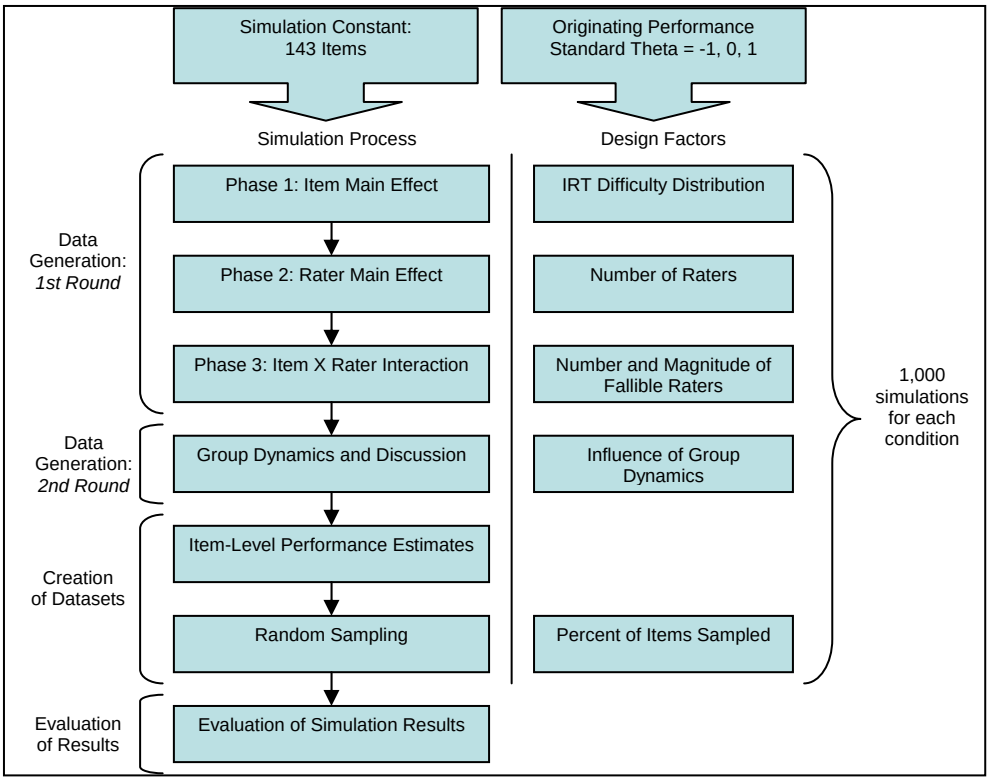


Figure 1. Simulation flowchart

Data Generation

The first round in the Angoff standard setting process was conceptualized as containing three possible sources of error; hence, the initial data generation phase took place in a three-phase process. These three sources of error included the items, the raters, and the interaction between the two. Consider a measurement model for the individual sources of error as shown below.

$$Angoff\_Rating_{ij} = Grand\_Mean + Item\_Main\_Effect_j + Rater\_Main\_Effect_i + Item/Rater\_Interaction_{ij}$$

Each rater's item estimate of minimal competency ($Angoff\_Rating_{ij}$) was composed of a grand mean (*Grand_Mean*), an item main effect ($Item\_Main\_Effect_j$), a

rater main effect ($Rater\_Main\_Effect_i$), and an interaction effect between items and raters ($Item/Rater\_Interaction_{ij}$). A data generation phase was developed through simulation to reflect each of the two main effects and the interaction effect with the overall effect cumulative across the three phases. That is to say, the last phase of the simulation contained all of the three sources of model error. Variance components were calculated at each phase of the simulation process in order to validate the infused sources of error.

Phase 1: Item Main Effect

In the first phase, the IRT parameters were used to establish rater Angoff values. The IRT parameters and a "true" passing standard were established using one of the four levels of item difficulty distributions ('real' item difficulty distribution, the simulated SAT item difficulty distribution, the simulated SAT item difficulty distribution with lower variance, and the simulated uniform difficulty distribution) and the initial passing standard was set to one of the three levels of this factor (θ_{mc} = -1.0, 0, and 1.0). Figure 2 graphically displays the four item difficulty distributions and Table 3 through Table 6 present the descriptive statistics for each of the item difficulty distributions.

Table 3

Mean, Standard Deviation, Minimum, and Maximum values of the IRT Parameters for the Real Distribution

IRT Parameter	Mean	SD	Minimum	Maximum
A	0.68	0.27	0.11	1.69
B	0.44	1.07	-3.85	3.32
C	0.16	0.09	0.00	0.31

Table 4

Mean, Standard Deviation, Minimum, and Maximum values of the IRT Parameters for the Simulated Distribution based on the SAT

IRT Parameter	Mean	SD	Minimum	Maximum
A	0.81	0.20	0.30	1.38
B	-0.07	0.93	-2.29	2.08
C	0.20	0.03	0.14	0.28

Table 5

Mean, Standard Deviation, Minimum, and Maximum values of the IRT Parameters for the Simulated Distribution based on the SAT with Lower Variance in b-parameters

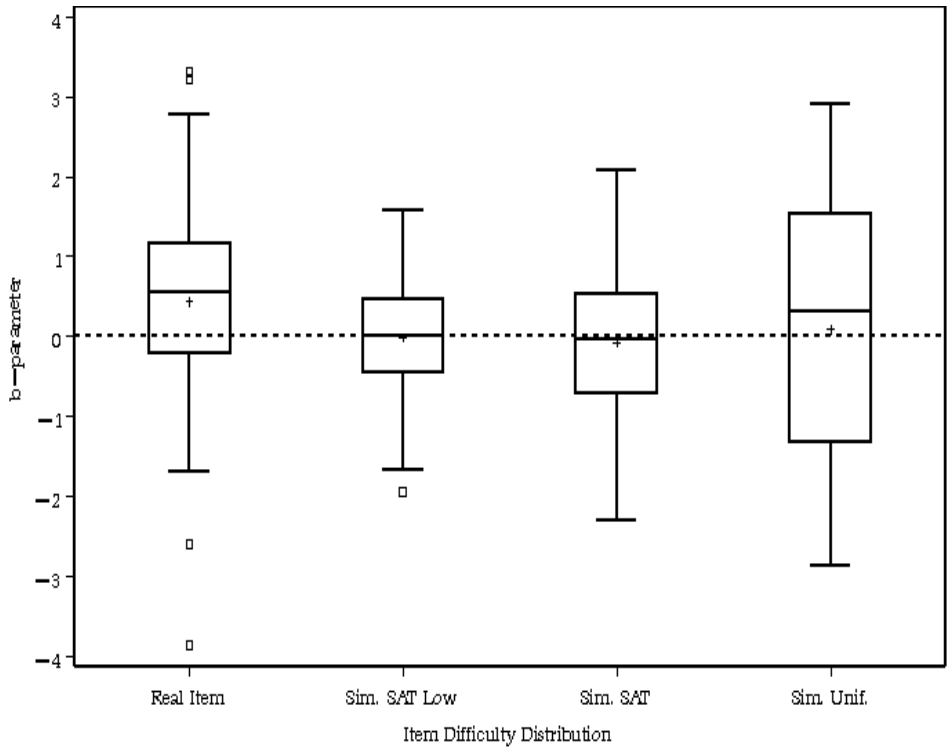
IRT Parameter	Mean	SD	Minimum	Maximum
A	0.81	0.21	0.33	1.28
B	-0.01	0.70	-1.94	1.58
C	0.20	0.03	0.10	0.26

Table 6

Mean, Standard Deviation, Minimum, and Maximum values of the IRT Parameters for the SAT Uniform Difficulty Distribution

IRT Parameter	Mean	SD	Minimum	Maximum
A	0.78	0.21	0.30	1.41
B	0.09	1.69	-2.86	2.90
C	0.20	0.03	0.13	0.28

The three simulated distributions were created using the WinGen Software (Han, 2007) with population characteristics described in the simulation factors section of this document. To establish the individual item ratings, the true performance standard (θ_{mc}) was transformed into a probability for each item using a single point estimate of a three-parameter IRT model with a known theta value and known item parameters.



*Fi*gure *2.* Distribution of item difficulty parameters (*b*) for each level of the item difficulty distribution factor.

The equation for the 3-parameter model as well as the description for each of the model parameters is stated below.

$$P(\theta) = c_i + (1 - c_i)\{1 + \exp[-D\, a_i(\theta_{mc} - b_i)]\}^{-1}$$

Where,

a_i is the discrimination parameter for the ith item,

b_i is the difficulty parameter for the ith item,

c_i is the pseudo guessing parameter for the ith item,

D is 1.702, and

θ_{mc} is the minimal competency theta estimate.

The three IRT parameters in the model are further described as follows: (1) the *a*-parameter is the discrimination index of the item, (2) the *b*-parameter is the difficulty of the item and, (3) the *c*-parameter is the index of the pseudo guessing parameter for the item.

The derived probability, *p(θ)*, was then averaged across all items and this value represented the grand mean (*Grand_Mean*) in the model. To establish the individual items means, the grand mean was subtracted from the overall item effect. The scale of the Angoff ratings was then changed to reflect practice (multiplied by 100). Raters are normally instructed to contemplate 100 minimally competent examinees and determine the number out of a hundred that would correctly respond to the item as opposed to determining a probability of success.

An example has been constructed to demonstrate the changes to the data structure through each phase in the simulation process. Each phase will be displayed as a table displaying six items with their corresponding item IRT parameters items, simulated Angoff values, estimated item-level theta performance estimates ($\hat{\theta}_{mci}$) as well as the estimated overall theta performance estimate for the six-item set ($\hat{\theta}_{mck}$). The estimated overall theta performance estimate for the six items, $\hat{\theta}_{mck}$, is the mean $\hat{\theta}_{mci}$ across items. The calculation of the item level performance estimates, $\hat{\theta}_{mci}$, will be discussed later in this section. For example purposes, the originating theta in the sample tables will be 0 and the calculated grand mean is 0.476.

Before infusing an item main effect (*Item_Main_Effect*$_j$) into the model, conceptually we can consider a model consisting of no item related differences. Table 7 displays such an example with six essentially parallel items each with the same IRT parameters.

Table 7

Simulated Data Sample for Parallel Items

IRT Parameters				Rater												
A	B	C	Item #	1	2	3	4	5	6	7	8	9	10	11	12	$\hat{\theta}_{mci}$
0.60	-0.69	0.21	20	74	74	74	74	74	74	74	74	74	74	74	74	0.01
0.60	-0.69	0.21	40	74	74	74	74	74	74	74	74	74	74	74	74	0.01
0.60	-0.69	0.21	60	74	74	74	74	74	74	74	74	74	74	74	74	0.01
0.60	-0.69	0.21	80	74	74	74	74	74	74	74	74	74	74	74	74	0.01
0.60	-0.69	0.21	100	74	74	74	74	74	74	74	74	74	74	74	74	0.01
0.60	-0.69	0.21	120	74	74	74	74	74	74	74	74	74	74	74	74	0.01
															$\hat{\theta}_{mck}$ =	0.010

In this first conceptual stage, all raters have identical Angoff values (or estimates of the performance standard) both across items and across raters. The differences between $\hat{\theta}_{mck}$ and the originating theta value of zero are the result of rounding error in the model. Table 8 displays six representative items from phase one in the preliminary simulation results. The table presents the item-level differences resulting from phase one in the simulation process. Since this first phase of the model is designed to address item error only, no rater error has been introduced in this example and all the individual raters are assigned the same Angoff rating for a given item. The differences between $\hat{\theta}_{mck}$ and the originating theta value (zero in this example) were anticipated to be small and are primarily the result of rounding error in the model.

Table 8

Simulated Data Sample from Phase One: Item Main Effect

IRT Parameters				Rater												
A	B	C	Item #	1	2	3	4	5	6	7	8	9	10	11	12	$\hat{\theta}_{mci}$
0.60	-0.69	0.21	20	74	74	74	74	74	74	74	74	74	74	74	74	0.01
0.51	-0.13	0.00	40	53	53	53	53	53	53	53	53	53	53	53	53	0.01
0.46	0.26	0.22	60	57	57	57	57	57	57	57	57	57	57	57	57	-0.01
0.43	0.67	0.19	80	50	50	50	50	50	50	50	50	50	50	50	50	0.04
0.81	0.98	0.21	100	37	37	37	37	37	37	37	37	37	37	37	37	0.00
1.55	1.42	0.20	120	22	22	22	22	22	22	22	22	22	22	22	22	0.01
															$\hat{\theta}_{mck}$ =	0.009

Phase 2: Rater Main Effect

The number of raters is a factor in the simulation model. This factor has three levels: eight, twelve, and sixteen raters. To estimate the level of rater leniency or severity, random variables were sampled from a normal distribution, $N(0,1)$. These sampled values were then multiplied by a standard deviation of 6.8 and added to a mean of 0 to represent the assumed systematic bias of individual raters. This standard deviation was selected to achieve an "acceptable" range of rater variability as suggested by Taube (1997). He proposed that the differences between the highest and lowest raters should be less than 20% of the possible Angoff values (or 20 points). The resulting value, or each rater's main effect, was then added to a rater's set of item ratings to reflect their individual variation. For example, Rater 2 had a calculated rater main effect of -2.1 which was added to Rater 2's Angoff rating for Item 20 from the last phase (74) for a resulting Angoff value of 72.

Table 9 displays six representative items from phase two in the preliminary simulation results. Once again, the differences between $\hat{\theta}_{mck}$ and the originating theta value (zero in this example) were anticipated to be small and are primarily the result of rounding error in the model.

Table 9

Simulated Data Sample from Phase Two: Rater Main Effect

IRT Parameters				Rater												
A	B	C	Item #	1	2	3	4	5	6	7	8	9	10	11	12	$\hat{\theta}_{mci}$
0.60	-0.69	0.21	20	78	72	80	69	67	79	58	87	80	79	67	70	0.00
0.51	-0.13	0.00	40	57	51	59	48	46	58	37	66	59	58	46	49	0.00
0.46	0.26	0.22	60	62	55	63	52	51	63	42	71	64	62	51	54	0.02
0.43	0.67	0.19	80	54	47	56	45	43	55	34	63	56	55	43	46	0.02
0.81	0.98	0.21	100	41	35	43	32	31	43	21	50	43	42	31	34	0.01
1.55	1.42	0.20	120	26	20	28	17	16	28	6	35	28	27	16	19	0.04
															$\hat{\theta}_{mck}$ =	0.016

Phase 3: Item X Rater Interaction

The final source of error variance in the model was the interaction effect between the items and raters. It is this stage of the process that would reflect the first round of an Angoff standard setting workshop. After a training process, standard setting participants would individually review and rate the items. Participants would evaluate how many out of 100 minimally competent would correctly respond to a given item. These ratings or Angoff values would have elements of the first three phases of the simulation process (item main effect, rater main effect, and item X rater interaction effect). To achieve this unreliability, random variables were sampled from a normal distribution. These values were then multiplied by a predefined standard deviation and added to a mean of 0. For the majority of raters (non-fallible raters), the standard deviation of 6.4 was used to

estimate a reliability of .95 (see Coraggio, 2006, 2007). This was done to simulate normal variability in the rating process. To simulate the 'fallible' raters, the value of the standard deviation reflected one of the three levels of the magnitude of 'unreliability' in the simulation model (ρ_{XX} = .65, .75, and .85). These deviations scores were then added to each of the 'unreliable' rater's Angoff values to simulate unreliability. The number of 'unreliable' raters was based on one of the three levels of the percentage of unreliable rater's factor: 25% of the raters, 50% of the raters, and 75% of the raters.

For example, Rater 5 had a calculated interaction error of -11.0 for Item 1 which was added to Rater 5's Angoff rating for Item 20 from the last phase (67) for a resulting Angoff value of 56. Table 10 displays the six representative items from phase three in the preliminary simulation results.

Table 10

Simulated Data Sample from Phase Three

IRT Parameters				Rater												
A	B	C	Item #	1	2	3	4	5	6	7	8	9	10	11	12	$\hat{\theta}_{mci}$
0.60	-0.69	0.21	20	74	73	71	70	56	71	54	98	78	73	66	72	-0.14
0.51	-0.13	0.00	40	49	47	46	57	51	55	37	72	65	60	40	52	-0.01
0.46	0.26	0.22	60	62	48	61	55	40	67	56	63	69	52	51	51	-0.06
0.43	0.67	0.19	80	55	61	63	41	49	57	31	72	67	54	42	47	0.26
0.81	0.98	0.21	100	46	31	45	33	42	33	14	62	47	46	31	26	0.06
1.55	1.42	0.20	120	33	17	30	10	27	30	9	33	26	23	6	19	-0.01
															$\hat{\theta}_{mck}$ =	0.018

While there were no 'unreliable' raters and only a small amount of unreliability was simulated in this preliminary simulation (ρ_{XX} = .95) across all raters, the differences between $\hat{\theta}_{mck}$ and the originating theta value (zero in this example) were generally larger

at this phase depending on the number of 'unreliable' raters and the magnitude of their unreliability. Even in this preliminary simulation, however, differences as a result of the unreliability can be seen for specific items such as Item 80. This is largely due to the restricted number of raters and items in the simulation model.

Group Dynamics and Discussion

After the conditions for the initial standard setting round was complete, the second round or discussion round was simulated. The discussion round of the Angoff standard setting workshop usually includes a group discussion regarding those items that did not meet some predetermined level of group consensus. This process usually involves an item-by-item review of those highlighted items with a statement from the highest and the lowest rater regarding their justifications for their individual ratings. Other participants generally add to the discussion as well. Finally, standard setting participants are asked to review their individual ratings for a given item and are permitted to change their ratings if they so choose. Standard setting research on second round performance suggests that providing information to raters will generally produce "small and inconsistent" changes in the overall mean performance standard, but will result in lower rater variability (Busch & Jaeger, 1990, p. 148). Brandon (2004) after conducting a review of the empirical literature on modified Angoff standard setting also concluded that the variability of rater estimates decreases after raters engage in between-round activities. Researchers have also suggested a group-influenced biasing effect of regression to the mean (Livingston, 1995) in addition to a group polarization effect during the discussion phase (Fitzpatrick, 1989). To address the uniform rating regression and the directional

influence of dominant raters, individual rater performance estimates were adjusted directionally based on one of the three levels of the influence factor: the lowest rater influence, the highest rater influence, and the average rater influence. The following formula based on an adjustment proposed by Reckase (2006b) was used to simulate this group influence during the discussion phase of the standard setting process:

$$rating = \varepsilon + (rating - \varepsilon) * influence\_factor$$

Where, ε is the rating of the influencing rater (lowest rater, the highest rater, or the average rater) as mentioned in the three levels of the influence factor, and the *rating* represents the individual item rating of the standard setting participant. To estimate the level of variability for the rater's level of influence, an *influence_factor* was calculated using random variables sampled from a normal distribution, $N(0,1)$. These sampled values were then multiplied by a standard deviation of 0.1 and added to a mean of 0.7. These values were selected to provide an acceptable amount of variability in the degree of influence for each rater. This *influence_factor* was assigned to each individual rater and used systematically in each of their item ratings for the discussion phase of the simulation.

For example, the average rater was the level of the directional influence for the preliminary simulation and the mean rating for Item 1 was 70.67. Rater 7 had a calculated *influence_factor* of 0.84 and a rating for Item 1 of 54. The resulting calculation was 70.67 + (54 -70.67) * 0.84 or 57. Table 11 displays six representative items from the discussion phase in the preliminary simulation results. While there was little difference between the $\hat{\theta}_{mck}$ calculated in the third phase and the $\hat{\theta}_{mck}$ calculated in the discussion

phase, this is largely due to the selection of the directional influence towards the average rater. The differences between $\hat{\theta}_{mck}$ and the originating theta value (zero in this example) were generally much larger at this phase depending on the level of the directional influence factor.

Individual Item Performance Standard Estimates

To create the individual item performance standard estimates for each item from the simulated data, $\hat{\theta}_{mci}$ was calculated for each item using a formula based on an IRT procedure proposed by Coraggio (2005, 2007). This procedure was designed to link item ratings and estimates of minimal competency with a common theta scale. Details regarding the basis for the formula and the transformation are located in Appendix A.

Table 11

Simulated Data Sample from Discussion Phase

IRT Parameters				Rater												
A	B	C	Item#	1	2	3	4	5	6	7	8	9	10	11	12	$\hat{\theta}_{mci}$
0.60	-0.69	0.21	20	73	72	71	70	60	71	57	87	76	72	67	72	-0.17
0.51	-0.13	0.00	40	50	49	48	56	51	54	40	64	62	57	42	52	-0.03
0.46	0.26	0.22	60	60	52	60	55	45	62	56	60	66	54	52	52	-0.06
0.43	0.67	0.19	80	54	58	60	45	50	55	35	64	63	54	44	48	0.21
0.81	0.98	0.21	100	43	34	43	34	41	35	18	52	45	43	32	28	0.02
1.55	1.42	0.20	120	29	19	28	13	26	26	11	28	25	23	9	20	-0.13
															$\hat{\theta}_{mck}$ =	-0.028

The final transformation of the formula for $\hat{\theta}_{mci}$ is as follows:

$$\hat{\theta}_{mc_i} = \frac{\ln\left(\frac{\bar{\gamma}_i - c_i}{1 - \bar{\gamma}_i}\right) + Da_i b_i}{Da_i}$$

Where,

$\bar{\gamma}_i$ is the mean rater Angoff rating for the ith item,

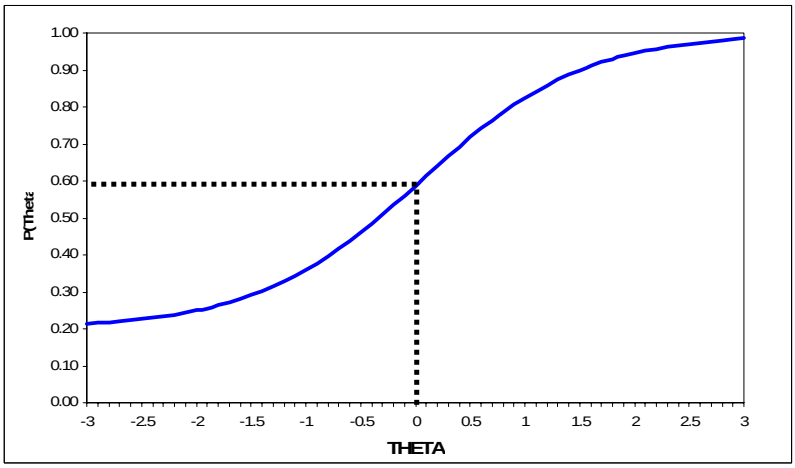
a_i is the *a*-parameter for the *i*th item,

b_i is the *b*-parameter for the *i*th item,

c_i is the *c*-parameter for the *i*th item, and

D is a scaling factor of 1.702 used in the 3-parameter IRT model.

Figure 3 displays a graphical representation of the relationship between the Angoff ratings (probabilities) and the minimal competency estimates on the theta scale.



*Fi*gure *3.* Relationship between the Angoff ratings (probabilities) and the minimal competency estimates (theta) for a given item.

The simulation program produced 143 item-level performance standard estimates ($\hat{\theta}_{mc\,i}$). A sampling macro, *samplethetas2*, was run to perform stratified random sampling on the full 143-item dataset. A stratified sampling model was designed to create a sample that was similar in terms of item difficulty and item discrimination as the original 143-item set. This model is consistent with the current literature in item selection strategies for standard setting. Ferdous and Plake (2007) used a similar sampling method that stratified samples according to content, item p-values, and item discrimination levels. Smith and Ferdous (2007) found that sampling models that stratified on p-value intervals and p-value density assist in reducing the standard error of the cutscore. The previous methods have predominately used classical statistics with pre-existing datasets in employing their stratified sampling models. This study sought to build on this previous research by employing a sampling model that stratified on item difficulty (*b*-parameters) and item discrimination (*a*-parameters).

Item difficulty distributions were separated into thirds by item difficulty and item discrimination parameters. A three-by-three stratification matrix was then constructed for each individual item difficulty distribution. The corresponding percentages of items in each cell were used along with the surveyselect procedure in SAS to create the individual samples. This ensured that the samples were similar to the full 143-item set in terms of item difficulty and item discrimination. The size of the sample varied depending on the level of the factor. The number of items sampled was 36, 47, 72, 94, and 107. The sample sizes represented approximately 25%, 33%, 50%, 66%, and 75% of the total number of items. The full 143-item set was also included as a comparison group. The difference

between the sample performance estimate, the performance estimate derived from the complete 143 item set, and the 'true' originating performance estimate was stored and aggregated across the simulations. Appendix B contains the SAS code for one set of conditions from the generalizability study.

Simulation Model Validation

The first criticism of any simulation procedure is whether the results can be generalized to 'real life' situations. Without any real life generalizability, the results of the simulation study add little or no contribution to the body of research literature. The ideal approach is to begin with a simulation model that has already been validated through previous research. Unfortunately, little research exists in the area of standard setting simulation studies and the basis for this proposed simulation model has only recently been explored. In order to begin to establish the validity of this proposed simulation process, multiple sources of internal and external validity evidence are presented below. While individually each source may not provide enough evidence to validate the model on its own merit, cumulatively they begin to provide a solid basis of support.

Internal Sources of Validity Evidence

Sources of Error. To validate the sources of error in the model (item, rater, and item/rater interaction), variance components were used to verify that error was appropriately applied to the originating source. These variance components were calculated as an element of the simulation process and were periodically reviewed during preliminary simulations to ensure that the process was operating as intended.

Recovery of Originating Performance Standard. While there has been considerable attention in the literature regarding the existence of a 'true' performance standard (see Schultz, 2006; Wang et al., 2003); the ability for a standard setting methodology to recover an originating standard in a relatively error-free environment seems like a logical assumption (Reckase, 2005; Reckase, 2006a). Preliminary simulations of the simulation model indicate the simulation model's ability to recover the originating performance standard. The simulation results had a mean bias of 0.051 with an originating performance standard of 0 after 1,000 replications, where the number of raters was twelve, the reliability for all raters was .95, and the full 143 item set was used with the 'real' IRT parameters. Under similar conditions with an originating performance standard of -1, the mean bias was -0.051 after 1,000 replications.

Standard Setting Model Fit to IRT Model. While van der Linden (1982) suggested the use of IRT in the analyzing data, there is no assurance that the minimum passing levels (Angoff ratings) produced by the standard setting raters adequately fit an IRT model (Kane, 1987). Kane (1987) proposed a test of IRT model fit for standard setting ratings.

His model is shown by the formula:

$$\sum_i Z_{iR}^2 = \sum_i \left[\left(M_{iR} - P_i(\theta^*) \right) / \sigma_i(M_{iR}) \right]^2$$

Where,

M_{iR} is the mean Angoff probability rating on Item i for k raters, and

$P(\theta^*)$ is the probability value for θ^* on the item characteristic curve that characterizes minimal competency for Item i.

The resulting value is distributed as a chi-square with *n* -1 degrees of freedom. While Kane (1987) did not provide an example using actual data, he did suggest that the issue of independence could be "problematic when all items are reviewed by the same raters" (p. 336). He felt that the independence assumption should be robust as long as the correlated errors are small compared to the random errors, specifically, the variation in specific raters rating specific items over different occasions. Preliminary simulation results were evaluated using Kane's IRT fit model. Results suggest that these standard setting data correctly fit an IRT framework. This result is not completely unexpected as IRT was used to initially determine the estimates, however, it does provide additional validation evidence for the simulation model.

External Sources of Validity Evidence

Research Basis for Simulation Factors and Corresponding Levels. When possible, each factor and its associated levels were related to actual standard setting conditions as discussed earlier in the simulation factors section of this document. For example, the 'real' IRT parameters were previously published parameters and the other simulated distributions were established from published information on the SAT examination. Other examples include the number of raters and the influence of group dynamics conditions.

Similarity of Simulation Data Characteristics with Performance Data in the Literature. Simulation data characteristics were similar to those presented in the research literature. For example, the variance in rater estimates decreased between the first and second round while little change occurred in overall performance standard estimate. This

finding is consistent with published research (Busch & Jaeger, 1990; Hurtz & Auerbach, 2003).

Review by Content Expert. A preliminary study using a similar version of the simulation model (Coraggio, 2007) was reviewed by a notable expert in the area of standard setting and generalizing performance standards, his comments regarding the methodology were very favorable with no suggested changes to the simulation methodology (S. G. Sireci, personal interview, April 11, 2007).

Comparisons to 'Real' Standard Setting Datasets. The availability of Angoff datasets is limited in the existing research for reasons of privacy and test security. For purposes of model validity, comparisons were made between an actual two round Angoff dataset (provided by S. G. Sireci) and simulated dataset with similar characteristics. The actual Angoff dataset contained 13 raters. One rater was randomly selected and removed in order to match simulation parameters.

Comparisons were made between the phase 3 of the simulation (item X rater interaction phase) and the initial round (independent ratings) of the actual Angoff dataset. Since the simulation was designed to represent a number of factors across various conditions, research was conducted to find the condition which had the closest representation to the actual Angoff dataset. Due to the nature of simulation study, individual conditions contained replication results which had a certain amount of variability. Therefore, multiple replications were also simulated for conditions which produced similar results.

The simulation condition closest to the actual Angoff dataset had the following factor levels: directional influence = 'lowest rater', item difficulty distribution = 'real', sample size = '143', number of raters = '12', percentage of fallible raters = '75%', reliability of fallible raters = '.75', and location of the originating theta = '-1'. The results of the comparison between the actual Angoff dataset and the cumulative results of 12 replications of the simulation condition are included in Table 12. The variance for the item main effect (51.8%) was very close to the mean for the twelve simulation runs (56.3%) and the variance for the item by rater interaction was a little more than 1 percentage point difference (35.9% to 37.0%).

Table 12

Comparison of Simulated Angoff Variance Percentages with 'Real' Angoff Dataset during Round 1

Outcome	Actual Data	Simulation Results[a]			
		Mean	SD	Min	Max
Var(Item)	51.8%	56.3%	1.8%	53.7%	59.4%
Var(Raters)	12.3%	6.7%	2.5%	2.0%	10.1%
Var(Item*Raters)	35.9%	37.0%	1.7%	34.4%	40.9%

[a] n=12 replications with the simulation condition that included the following factor levels directional influence = 'lowest rater', tem difficulty distribution = 'real', sample size = '143', number of raters = '12', percentage of fallible raters = '75%', reliability of fallible raters = '.75', and location of the originating theta = '-1'

Comparisons were also made between the selected condition at the discussion phase of the simulation and the second round (after group discussion) of the actual Angoff dataset. These results are included in Table 13.

The variance for the item main effect (65.9%) was only one percentage point away from the mean for the twelve simulation runs (66.9%). The difference in variance for the item by rater interaction was less than 1 percentage point difference (26.1% to

26.9%). These results suggest that the simulated data function similarly to the actual Angoff data.

Table 13

Comparison of Simulated Angoff Variance Percentages with 'Real' Angoff Dataset during Round 2

Outcome	Actual Data	Simulation Results[a]			
		Mean	SD	Min	Max
Var(Item)	65.9%	66.9%	3.5%	62.0%	73.2%
Var(Raters)	8.0%	6.2%	2.5%	1.5%	9.8%
Var(Item*Raters)	26.1%	26.9%	2.6%	22.2%	31.5%

[a] n=12 replications with the simulation condition that included the following factor levels directional influence = 'lowest rater', tem difficulty distribution = 'real', sample size = '143', number of raters = '12', percentage of fallible raters = '75%', reliability of fallible raters = '.75', and location of the originating theta = '-1'

Programming

This research was conducted using SAS version 9.1.3 SP 4. Conditions for the study were run under the Windows Vista Business platform. Normally distributed random variables were generated using the RANNOR random number generator in SAS. A different seed value for the random number generator was used in each execution of the program. For each condition in the research design, 1,000 samples were simulated.

Analysis

The ability to 'adequately' generalize the performance was evaluated in terms of the differences between the performance standard derived with the larger item set and the performance standard derived with the smaller subset of multiple choice items. The difference between the sample and the originating performance standard (θ_{mc}) was also evaluated. The aggregated simulation results were evaluated in terms of the location

(bias) and the variability (mean absolute deviation, root mean square error) in the estimates.

Location was identified by calculating the bias or mean error (ME). Bias is the mean difference between the sample performance standard ($\hat{\theta}_{mc_k}$) and the full 143-item set performance standard ($\hat{\theta}_{mc_{143}}$).

$ME = \frac{1}{n}\sum_{k=1}^{n}\left(\hat{\theta}_{mc_k} - \hat{\theta}_{mc_{143}}\right)$, where the summation is over the 1,000 replications.

The difference between the sample ($\hat{\theta}_{mc_k}$) and the originating performance standard (θ_{mc}) was also evaluated.

$ME = \frac{1}{n}\sum_{k=1}^{n}(\hat{\theta}_{mc_k} - \theta_{mc})$, where the summation is over the 1,000 replications.

Variability was identified by calculating the root mean squared error (RMSE). RMSE is the square root of the sum of squares divided by the number of samples. The sum of squares was calculated with the difference between the sample performance standard ($\hat{\theta}_{mc_k}$) and the full 143-item set performance standard ($\hat{\theta}_{mc_{143}}$).

$$RMSE = \sqrt{\frac{\sum_{k=1}^{n}\left(\hat{\theta}_{mc_k} - \hat{\theta}_{mc_{143}}\right)^2}{n}}$$

The RMSE difference between the sample ($\hat{\theta}_{mc_k}$) and the originating performance standard (θ_{mc}) was also evaluated.

$$RMSE = \sqrt{\frac{\sum_{k=1}^{n}(\hat{\theta}_{mc_k} - \theta_{mc})^2}{n}}$$

Variability was also identified by calculating the mean absolute deviation (MAD). MAD is the sum of the absolute differences divided by the number of samples. The MAD was calculated between the sample performance standard ($\hat{\theta}_{mc_k}$) and the full 143-item set performance standard ($\hat{\theta}_{mc_{143}}$).

$$MAD = \frac{\sum_{k=1}^{n} \left| \left(\hat{\theta}_{mc_k} - \hat{\theta}_{mc_{143}} \right) \right|}{n}$$

The MAD between the sample ($\hat{\theta}_{mc_k}$) and the originating performance standard (θ_{mc}) was also evaluated.

$$MAD = \frac{\sum_{k=1}^{n} \left| (\hat{\theta}_{mc_k} - \theta_{mc}) \right|}{n}$$

Results were analyzed by computing eta-squared (η^2) values. Critical factors were identified using eta-squared (η^2) to estimate the proportion of variance associated with each effect (Maxwell & Delaney, 1990). Cohen (1977, 1988) proposed descriptors for interpreting eta-squared values; (a) small effect size: η^2 = .01; (b) medium effect size: η^2 = .06, and (c) large effect size: η^2 = .14. For this research study, the Critical factors were identified using Cohen's medium effect size criteria, η^2 = .06.

Research Question 1

Research Question 1, evaluating the impact of the characteristics and the relationship between the two item sets in the ability to generalize minimal competency estimates, was addressed by examining proportion of variance associated with each effect (η^2) using Cohen's medium effect size criteria, η^2 = 0.06. The outcomes were averaged over all conditions and averaged separately for each level of the associated factors being

examined (the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard, and the number of items randomly drawn from the larger item set). If there were significant interactions between factors in research question 1, graphs were constructed to display these relationships.

Research Question 2

Research Question 2, evaluating the impact of the characteristics of the standard setting process in the ability to generalize minimal competency estimates, were addressed by examining proportion of variance associated with each effect (η^2) using Cohen's medium effect size criteria, $\eta^2 = 0.06$. The outcomes were averaged over all conditions and averaged separately for each level of the associated factors being examined (the number of raters, the 'unreliability' of individual raters in terms of the percentage of unreliable raters and their magnitude of 'unreliability', and the influence of group dynamics and discussion). If there were significant interactions between factors in research question 2, graphs were constructed to display these relationships.

Chapter Four:

Results

This chapter presents the results of the study as they relate to each of the individual research questions. The chapter initially begins by describing how the results were evaluated and then presents the results in two sections, one section for each generalizability comparison. The first generalizability comparison is evaluating the difference between the small sample performance estimate and the performance estimate derived from the complete 143-item set. The second generalizability comparison is evaluating the difference between the small sample performance estimate and the 'true' originating performance estimate. Each generalizability comparison section will be subdivided by the outcome measures (bias, mean absolute deviation, and root mean square error) and results will be presented in the order of the research questions. Following the discussion on the results of the generalizability comparisons, performance standards derived from the simulation study will be compared to performance standards set with 112 Angoff values from an actual standard setting study. Random stratified sampling will be performed on this population of Angoff values and then compared with the results of the simulation. The last section of the chapter will be a summary of the results presented.

The two research questions relate to the extent to which various factors impact the ability to generalize minimal competency estimates. The first research question involves factors related to the characteristics and the relationship between the two item sets. The

second research question involves factors related to the standard setting process. The following research questions are addressed by the results:

Research Questions

1. To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?
    a. To what extent does the distribution of item difficulties in the larger item set influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the placement of the 'true' performance standard influence the ability to generalize the estimate of minimal competency?
    c. To what extent does the number of items drawn from the larger item set influence the ability to generalize the estimate of minimal competency?
2. To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?
    a. To what extent does the number of raters in the standard setting process influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the percentage of 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    c. To what extent does the magnitude of 'unreliability' in the designated 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    d. To what extent do group dynamics and discussion during the later rounds of the standard setting process influence the ability to generalize the estimate of

minimal competency?

Results Evaluation

There were 5,832 conditions simulated using the seven factors of this Monte Carlo study. The seven factors were the item difficulty distributions in the larger 143-item set ('real' item difficulty distribution, simulated SAT item difficulty distribution, simulated SAT item difficulty distribution with reduced variance, and simulated uniform item difficulty distribution), location of the 'true' performance standard (θ_{mc} = -1.0, 0, 1.0), number of items randomly drawn in the sample (36, 47, 72, 94, 107, and the full item set), number of raters (8, 12, 16), percentage of unreliable raters (25%, 50%, 75%), magnitude of 'unreliability' in unreliable raters (ρ_{XX} = .65, .75, .85), and the directional influence of group dynamics and discussion (lowest rater, highest rater, average rater). This resulted in 4 (item difficulty distributions) x 3 (originating performance standards) x 6 (item sample sizes) x 3 (rater configurations) x 3 (percentage of unreliable raters) x 3 (directional group dynamics) = 5,832 conditions.

The results of the simulation were evaluated using PROC GLM in SAS such that the dependent variables were Bias, RMSE, and MAD and the independent variables were the seven different factors. The effect size, eta-squared (η^2), was calculated to measure the degree of the association between the independent variables main effects and the dependent variables along with the two-way and three-way interaction effects between the independent variables and the dependent variables. Eta-squared is the estimated proportion of variability in each of the outcomes associated with each factor in the simulation design. It is calculated as the ratio of the effect variance (SS_{effect}) to the total

variance (SS_{total}).

$$\eta^2 = \frac{SS_{effect}}{SS_{total}}$$

Generalizability Comparison I

Each generalizability comparison section will be subdivided by the outcome measures (bias, mean absolute deviation, and root mean square error) and results will be presented in the order of the research questions. The first generalizability comparison evaluated the difference between the small sample performance estimate and the performance estimate derived from the complete 143-item set. The first research question involves the extent to which the characteristics and the relationship between the two item sets impacted the ability to generalize minimal competency estimates. This is followed by the second research question which involves the extent to which the characteristics of the standard setting process impacted the ability to generalize minimal competency estimates. The text of the research questions will be repeated verbatim in each section in order to provide a proper reference for the reader. Table 14 displays the descriptive statistics for each of the outcome measures across the 5,832 conditions for Generalizability Comparison I.

The mean for estimated bias was 0.000 (SD = 0.004) with a range from -0.022 to 0.024. The mean for estimated RMSE was 0.035 (SD = 0.028) with a range from 0.000 to 0.178 and the mean for estimated MAD was 0.026 (SD = 0.020) with a range from 0.000 to 0.130.

Table 14

Mean, Standard Deviation, Minimum, and Maximum Values for Outcomes Associated with Generalizability Comparison I (N=5832)

Outcome	Mean	SD	Min	Max
Bias	0.000	0.004	-0.022	0.024
RMSE	0.035	0.028	0.000	0.178
MAD	0.026	0.020	0.000	0.130

Figure 4 is a graphical representation of the distributions for each of the three outcome variables.

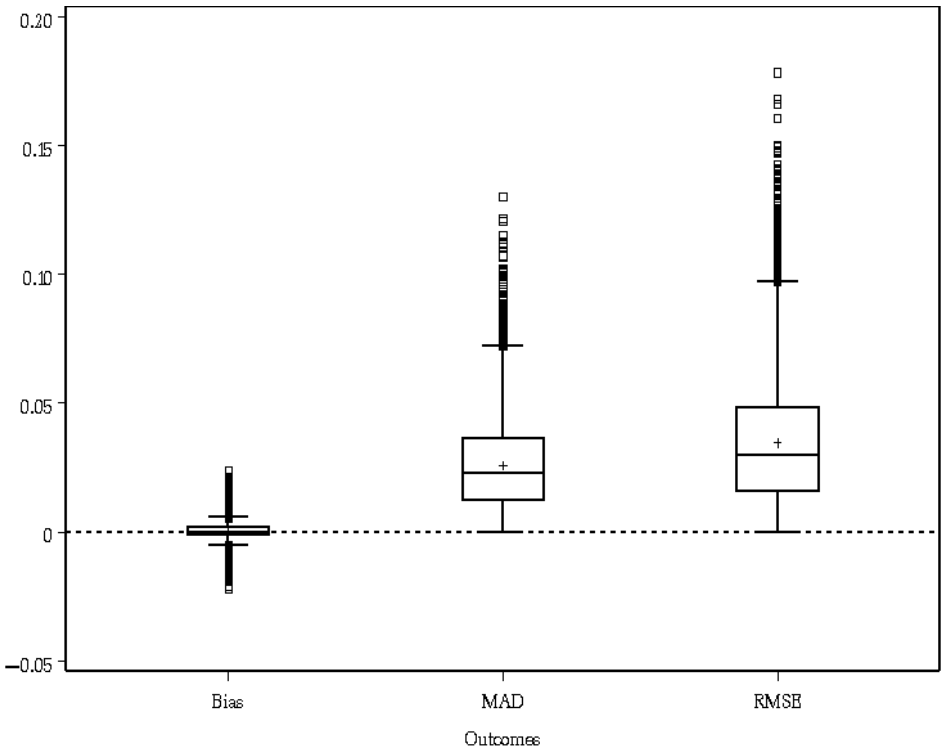


Figure 4. Outcome distributions for Generalizability Comparison I

The results of the simulation were evaluated using SAS PROC GLM. The dependent variables in the model were the three outcome variables, Bias, RMSE, and MAD. The seven independent variables were the seven different factors from the

simulation model. Three different models were evaluated, main effects model, two-way interaction model, and three-way interaction model. For the bias outcome, only 18.9% of the variability was explained by the main effects of the seven simulation factors. In terms of RMSE and MAD outcomes, 84.6% and 86.3% of the variability was explained, respectively, by the main effects of the seven simulation factors.

Table 15 displays the eta-squared values for each of the main effects for generalizability comparison I. Using the pre-established standard of Cohen's medium effect size criteria ($\eta^2 = 0.06$), the only note worthy bias main effect was the sample size factor ($\eta^2 = 0.17$). In terms of the RMSE and MAD main effect, the same four of the factors had eta-squared values resulting in at least a medium effect. These included the directional influence factor, the item difficulty distribution factor, number of sample item factor and the location of the 'true' performance standard factor.

Table 15

Eta-squared Analysis of the Main Effects of the Factors in the Simulation for Generalizability Comparison I

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct	0.01	0.06*	0.07*
Dist	0.01	0.07*	0.07*
SampleN	0.17*	0.63*	0.60*
RaterN	0.00	0.01	0.02
Fallible%	0.00	0.01	0.01
ρ_{XX}	0.00	0.01	0.01
θ_{mc}	0.01	0.07*	0.08*

* Eta-squared value at or above Cohen's medium effect size criteria of 0.06

Note. Direct = directional influence, Dist = item difficulty distribution, SampleN = sample size, RaterN = number of raters, Fallible% = percentage of fallible raters, ρ_{XX} = reliability of fallible raters, and θ_{mc}=location of the originating theta

The amount of explained variability in the bias outcome increased substantially to 68.3% in the two-way interaction model. The RMSE and MAD outcomes experienced more modest increases in explained variability with 97.4% and 98.1%, respectively in the two-way interaction model. Table 16 displays the eta-squared values for each of the two-way interaction effects for Generalizability Comparison I.

Table 16

Eta-square Analysis of the Two-way Interaction Effects of the Factors in the Simulation for Generalizability Comparison I

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct x Dist	0.02	0.01	0.01
Direct x SampleN	0.04	0.02	0.03
Direct x RaterN	0.00	0.00	0.00
SampleN x Dist	0.38*	0.03	0.03
RaterN x Dist	0.00	0.00	0.00
RaterN x SampleN	0.00	0.01	0.01
Fallible% x Direct	0.00	0.00	0.00
Fallible% x Dist	0.00	0.00	0.00
Fallible% x SampleN	0.00	0.00	0.00
Fallible% x RaterN	0.00	0.00	0.00
Fallible% x ρ_{XX}	0.00	0.00	0.00
Fallible% x θ_{mc}	0.00	0.00	0.00
ρ_{XX} *x Direct*	0.00	0.00	0.00
ρ_{XX} *x Dist*	0.00	0.00	0.00
ρ_{XX} *x SampleN*	0.00	0.01	0.00
ρ_{XX} *x RaterN*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc}	0.00	0.00	0.00
θ_{mc} *x Direct*	0.00	0.01	0.02
θ_{mc} *x Dist*	0.02	0.01	0.01
θ_{mc} *x SampleN*	0.03	0.03	0.03
θ_{mc} *RaterN*	0.00	0.00	0.00

***** Eta-squared value at or above Cohen's medium effect size criteria of 0.06

Note. Direct = directional influence, Dist = item difficulty distribution, SampleN = sample size, RaterN = number of raters, Fallible% = percentage of fallible raters, ρ_{XX} = reliability of fallible raters, and θ_{mc}=location of the originating theta

Using the pre-established standard of Cohen's medium effect size criteria (η^2 = 0.06), the only note worthy bias interaction effect was the two-way interaction between the sample size factor and the item difficulty distribution factor (η^2 = 0.38). In terms of the RMSE and MAD main effect, there were no two-way interactions that met the pre-established criteria.

The amount of explained variability in the bias outcome increased slightly to 70.1% in the three-way interaction model. In terms of the RMSE and MAD outcomes, almost all of the variability was explained in the three-way interaction model with 98.6% and 99.1% of the variability explained by the model, respectively.

Table 17

Eta-square Analysis of the Three-way Interaction Effects of the Factors in the Simulation for Generalizability Comparison I

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct x RaterN x Dist	0.00	0.00	0.00
Direct x RaterN x SampleN	0.00	0.00	0.00
RaterN x SampleN x Dist	0.00	0.00	0.00
Fallible% x ρ_{XX} *x Direct*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x Dist*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x SampleN*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x RaterN*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x* θ_{mc}	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct x Dist*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct x SampleN*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct x RaterN*	0.00	0.00	0.00
θ_{mc} *x Direct x Dist*	0.01	0.00	0.01
θ_{mc} *x Direct x SampleN*	0.01	0.01	0.01
θ_{mc} *x Direct x RaterN*	0.00	0.00	0.00

***** Eta-squared value at or above Cohen's medium effect size criteria of 0.06

Note. Direct = directional influence, Dist = item difficulty distribution, SampleN = sample size, RaterN = number of raters, Fallible% = percentage of fallible raters, ρ_{XX} = reliability of fallible raters, and θ_{mc}=location of the originating theta

Table 17 displays the eta-squared values for each of the three-way interaction effects for generalizability comparison I. Using the pre-established standard of Cohen's medium effect size criteria ($\eta^2 = 0.06$), there were no note worthy three-way interactions.

Bias in Generalizability Comparison I

Research Question 1. The first research question, "To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?" focuses on the characteristics and the relationship between the two item sets. This question is specifically addressed by the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard influence, and the number of items drawn from the larger item set.

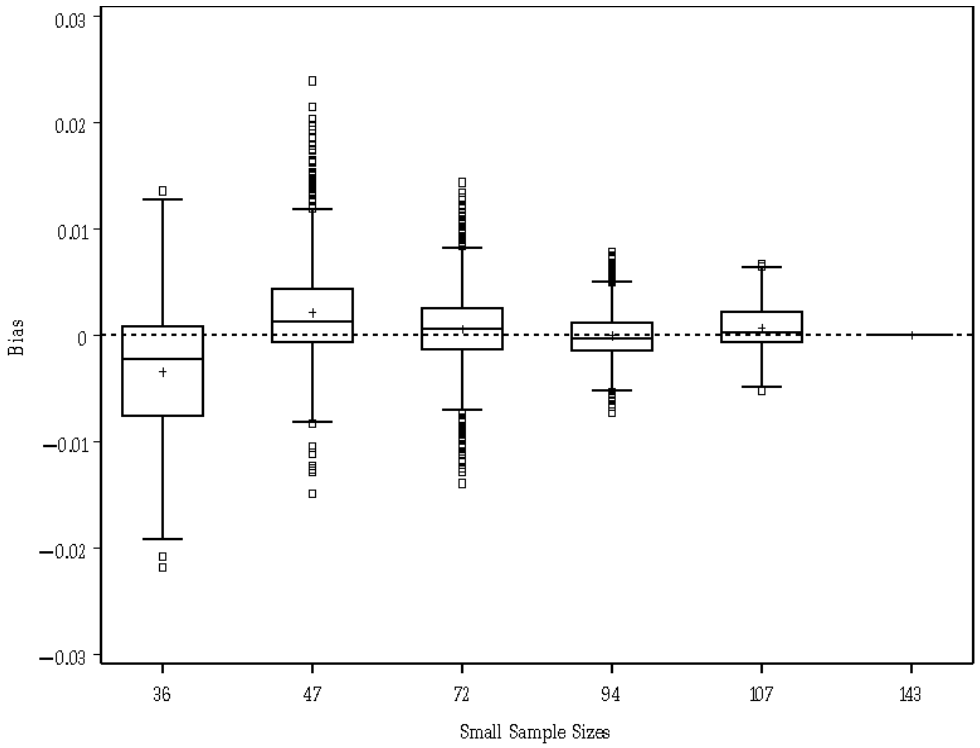


Figure 5. Estimated bias for small sample size bias for Generalizability Comparison I.

The number of sample items factor was the only factor of the three in research question 1 that resulted in a medium or greater effect size for eta-squared. In fact, the bias in theta estimates for the 'true' performance standard factor resulted in a large effect size ($\eta^2 = 0.17$). The six levels of this factor included sample sizes of 36, 47, 72, 94, 107, as well as the full 143-item set. Figure 5 displays the box plots for bias for each of the six sample sizes.

Though the bias estimates were generally very small (+/-0.003), the mean bias and the variability in bias estimates decreased as the number of items in the small sample size increased. The bias mean, standard deviation, minimum, and maximum values are shown in Table 18.

Table 18

Bias Mean, Standard Deviation, Minimum and Maximum for Small Sample Size Factor Associated with Generalizability Comparison I (n=972)

Sample Size	Mean	SD	Min	Max
36	-0.003	0.006	-0.022	0.014
47	0.002	0.005	-0.015	0.024
72	0.001	0.004	-0.014	0.014
94	0.000	0.002	-0.007	0.008
107	0.001	0.002	-0.005	0.007
143	0.000	0.000	0.000	0.000

The sample size factor also interacted with the item difficulty distribution factor ($\eta^2 = 0.38$). The item difficulty distribution factor had four levels which included a distribution of 'real' items, a simulated distribution based on the SAT, a second simulated distribution based on the SAT with reduced variance, and a simulated uniform

distribution. The bias in theta estimates for the item difficulty distributions factor was relatively small ($\eta^2 = 0.01$). Figure 6 graphically displays this two-way interaction between the sample size and item difficulty distribution factor. The results of the simulation suggest that there was more variability in average bias estimates when the sample size was small with the variability in average bias estimates decreasing as the sample size increased. The average bias was the greatest in the simulated uniform distribution, which initially was negatively bias in the 36-item small and was positively bias for the remaining samples.

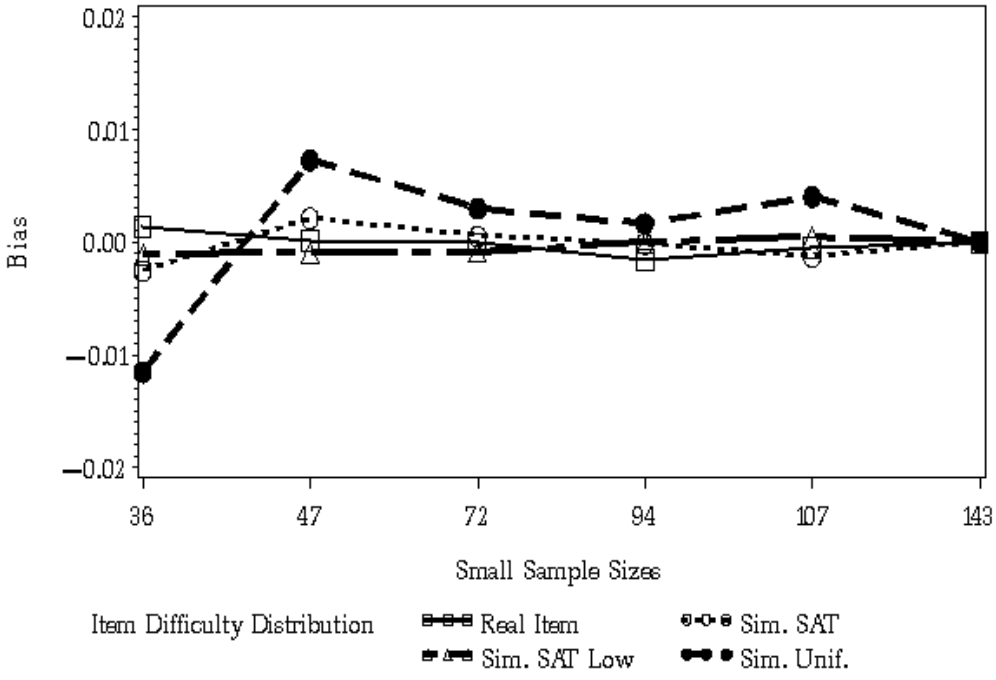


Figure 6. Two-way bias interaction between item difficulty distributions and small sample size for Generalizability Comparison I.

The last factor in research question 1, placement of the 'true' performance standard, had three levels which included an originating theta of -1, 0, and 1. The variance in bias in theta estimates associated with the 'true' performance standard factor was relatively small ($\eta^2 = 0.01$).

Research Question 2. The second research question, "To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?" focuses on the characteristics of the standard setting process. This question is specifically addressed by the number of raters, the percentage and magnitude of 'unreliable' raters, and the impact of group dynamics and discussion during the later rounds of the standard setting process.

None of the four factors in research question 2 resulted in a medium or larger effect size in eta-squared. The number of raters factor had three levels which included 8, 12, and 16 raters involved in the standard setting process. The variance in estimated bias in theta estimates associated with the number of raters factor was very small ($\eta^2 = 0.00$). The percentage of unreliable raters factor also had three levels which included 25%, 50%, and 75% of raters which were unreliable in their estimates. The effect size for the estimated bias in theta estimates associated with the percentage of unreliable raters factor was also small ($\eta^2 = 0.00$). The magnitude of 'unreliability' factor had three levels which included reliabilities (ρ_{XX}) of .65, .75, and .85. The variance in estimated bias in theta estimates associated with the magnitude of 'unreliability' factor was also very small ($\eta^2 = 0.00$). The directional influence factor had three levels of this factor for the directional impact of group dynamics and discussion. They included influence towards

the lowest rater, highest rater, and average rater. Similar to the last three factors, the variance in estimated bias in theta estimates associated with the directional influence factor was not notable ($\eta^2 = 0.01$).

Root Mean Square Error in Generalizability Comparison I

Research Question 1. The first research question, "To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?" focuses on the characteristics and the relationship between the two item sets. This question is specifically addressed by the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard influence, and the number of items drawn from the larger item set.

Table 19

RMSE Mean, Standard Deviation, Minimum, and Maximum for Item Difficulty Distribution Factor Associated with Generalizability Comparison I (n=1458)

Item Difficulty Distribution	Mean	SD	Min	Max
Real Item	0.04	0.03	0.00	0.18
Sim. SAT Low	0.03	0.02	0.00	0.12
Sim. SAT	0.03	0.02	0.00	0.14
Sim. Unif.	0.04	0.03	0.00	0.13

All three of the factors in research question 1 for RMSE had eta-squared values that resulted in a medium effect or greater. The estimated RMSE in theta estimates associated with the item difficulty distribution factor exceeded the pre-established standard with an eta-squared (η^2) of 0.07. Table 19 displays the mean, standard deviation, minimum, and maximum for the four levels of the item difficulty distribution

factor for Generalizability Comparison I.

While real item difficulty distribution and the simulated uniform distribution had slightly higher RMSE means and standard deviations than the other two distributions, one noticeable difference between the item difficulty distributions was the higher range of the RMSE estimates for the real item difficulty distribution as opposed to the other three simulated distributions as shown in Figure 7.

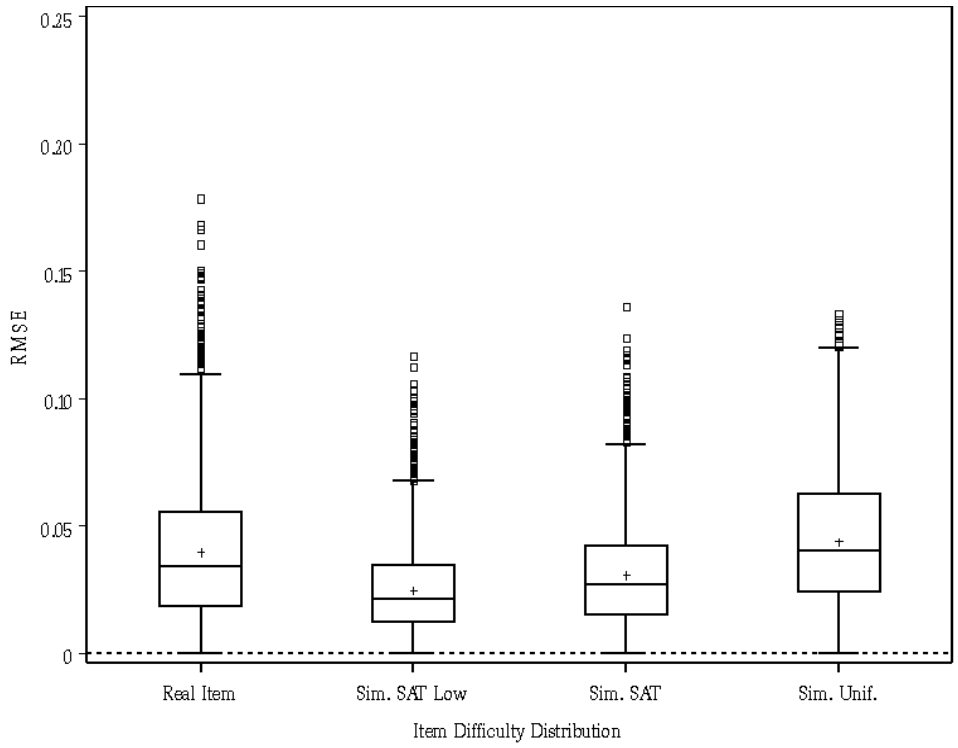


Figure 7. Estimated RMSE for item difficulty distributions for Generalizability Comparison I.

The effect size for the estimated RMSE in theta estimates associated with the placement of the 'true' performance standard factor ($\eta^2 = 0.08$) also exceeded the pre-established standard. The estimated mean RMSE for an originating theta of -1 was

higher (0.05) than the other two estimated mean RMSE values (0.03) as shown in Table 20.

Table 20

RMSE Mean, Standard Deviation, Minimum, and Maximum for Placement of the 'True' Performance Standard Factor Associated with Generalizability Comparison I (n=1944)

Originating Theta	Mean	SD	Min	Max
-1	0.05	0.03	0.00	0.18
0	0.03	0.03	0.00	0.13
1	0.03	0.02	0.00	0.11

In addition to the estimated RMSE mean difference among samples, the upper most limit of each originating theta value was different with -1 having the highest (0.18) of the three values as visually displayed in Figure 8.

The variance in the estimated RMSE in theta estimates associated with the number of sample items factor had the highest eta-squared value ($\eta^2 = 0.60$) of any of the RMSE effects in Generalizability Comparison I. The estimated mean RMSE decreased as the size of the sample increased as shown in Table 21.

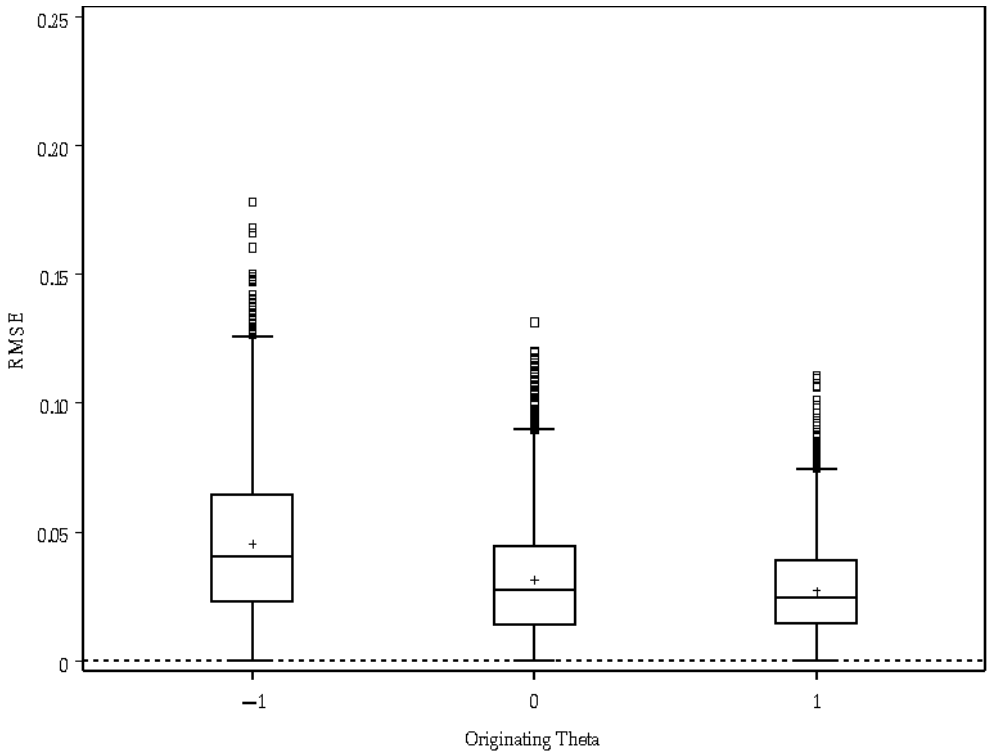


Figure 8. Estimated RMSE for the placement of the 'true' performance standard for Generalizability Comparison I.

Table 21

RMSE Mean, Standard Deviation, Minimum, and Maximum for Number of Sample Items Factor Associated with Generalizability Comparison I (n=972)

Sample Size	Mean	SD	Min	Max
36	0.07	0.03	0.02	0.18
47	0.06	0.02	0.02	0.15
72	0.04	0.02	0.01	0.10
94	0.03	0.01	0.01	0.08
107	0.02	0.01	0.01	0.06
143	0.00	0.00	0.00	0.00

Figure 9 provides a graphical representation of the change as the mean, standard deviation, and range of the estimated RMSE decreases when the size of the sample is

reduced.

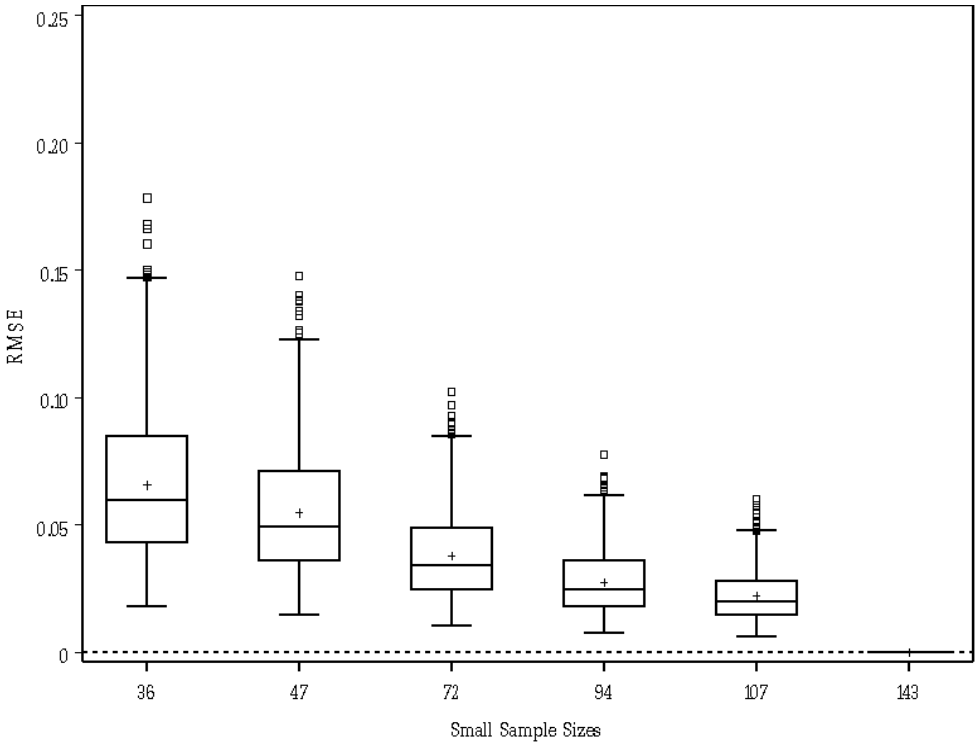


Figure 9. Estimated RMSE for small sample sizes for Generalizability Comparison I.

Research Question 2. The second research question, "To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?" focuses on the characteristics of the standard setting process. This question is specifically addressed by the number of raters, the percentage and magnitude of 'unreliable' raters, and the impact of group dynamics and discussion during the later rounds of the standard setting process.

Only one of the four factors in research question 2 for RMSE had eta-squared values that resulted in a medium effect or greater, the directional influence factor. The variance in estimated RMSE in theta estimates associated with the directional influence

factor was note worthy ($\eta^2 = 0.07$). The estimated RMSE for directional influence towards the lowest rater was higher than the other two directional values as shown in Table 22.

The upper most limit of the lowest rater's estimated RMSE (0.18) was also considerable higher than the other two directional influences as visually displayed in Figure 10.

Table 22

RMSE Mean, Standard Deviation, Minimum, and Maximum for Directional Influence Factor Associated with Generalizability Comparison I (n=1944)

Directional Influence	Mean	SD	Min	Max
Lowest Rater	0.05	0.03	0.00	0.18
Average Rater	0.03	0.03	0.00	0.13
Highest Rater	0.03	0.02	0.00	0.11

The remaining three factors in research question 2 had variance in estimated RMSE in theta estimates that was small and did not exceed the pre-established criteria of a medium effect size or greater, the number of raters factor ($\eta^2 = 0.02$), the percentage of unreliable raters factor ($\eta^2 = 0.01$), and the magnitude of 'unreliability' factor ($\eta^2 = 0.00$).

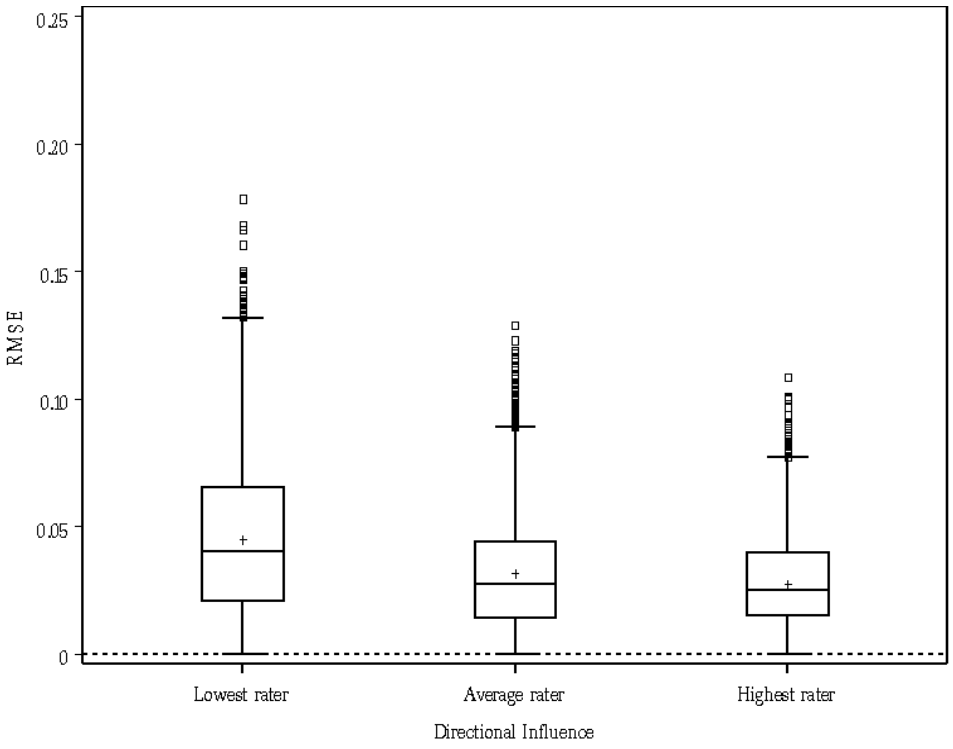


Figure 10. Estimated RMSE for the directional influences for Generalizability Comparison I.

Mean Absolute Deviation in Generalizability Comparison I

Research Question 1. The first research question, "To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?" focuses on the characteristics and the relationship between the two item sets. This question is specifically addressed by the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard influence, and the number of items drawn from the larger item set.

All three of the factors in research question 1 for MAD had eta-squared values that resulted in a medium effect or greater. The variance in estimated MAD in theta estimates associated with the item difficulty distributions factor ($\eta^2 = 0.07$) exceeded the pre-established standard. Table 23 displays the mean and standard deviations for the four levels of the item difficulty distribution factor.

Table 23

MAD Mean, Standard Deviation, Minimum, and Maximum for Item Difficulty Distribution Factor Associated with Generalizability Comparison I (n=1458)

Item Difficulty Distribution	Mean	SD	Min	Max
Real Item	0.03	0.02	0.00	0.13
Sim. SAT Low	0.02	0.02	0.00	0.09
Sim. SAT	0.02	0.02	0.00	0.10
Sim. Unif.	0.03	0.02	0.00	0.10

While real item difficulty distribution and the simulated uniform had slightly higher MAD means than the other two distributions, one noticeable difference between the item difficulty distributions was the higher range of the MAD estimates for the real item difficulty distribution (0.13) as opposed to the three simulated distributions as shown in Figure 11.

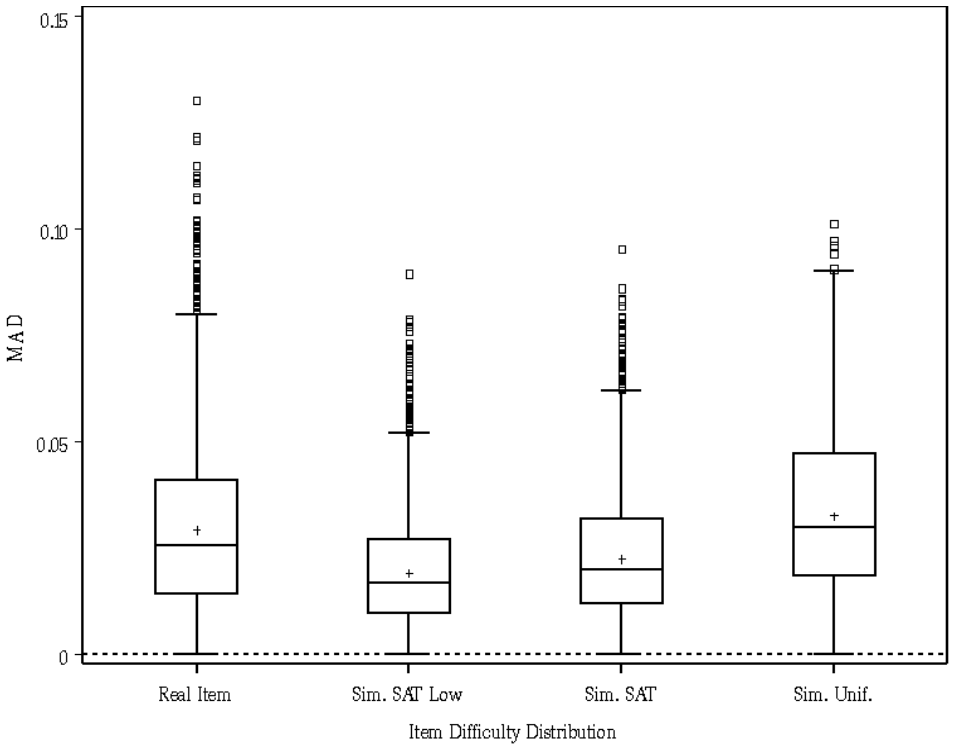


Figure 11. Estimated MAD for item difficulty distributions for Generalizability Comparison I.

The variance in estimated MAD in theta estimates associated with the placement of the 'true' performance standard factor ($\eta^2 = 0.07$) also exceeded the pre-established standard. The estimated mean MAD for an originating theta of -1 was higher (0.03) than the other two theta values (0.02) as shown in Table 24.

In addition to the estimated MAD mean difference among samples, the upper most limit of each originating theta value was different with -1 having the highest (0.13) of the three values as visually displayed in Figure 12.

Table 24

MAD Mean, Standard Deviation, Minimum, and Maximum for Placement of the 'True' Performance Standard Factor Associated with Generalizability Comparison I (n=1944)

Originating Theta	Mean	SD	Min	Max
-1	0.03	0.02	0.00	0.13
0	0.02	0.02	0.00	0.09
1	0.02	0.02	0.00	0.08

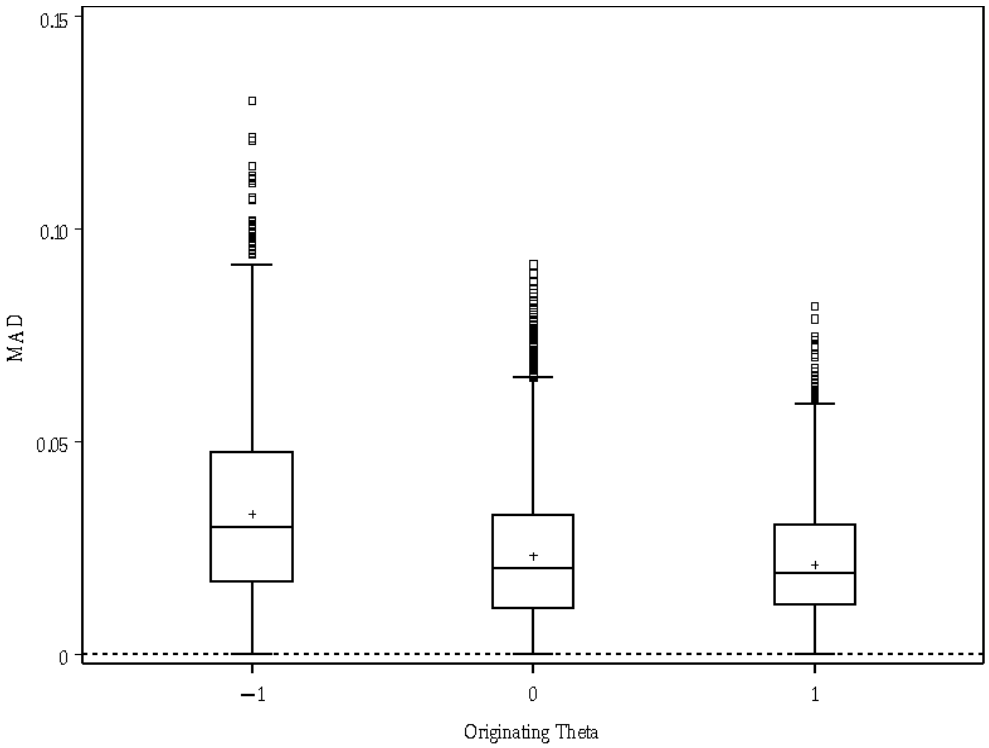


Figure 12. Estimated MAD for the placement of the 'true' performance standard for Generalizability Comparison I.

The variance in estimated MAD in theta estimates for the number of sample items factor had the highest eta-squared value ($\eta^2 = 0.63$) of any of the MAD effects in

Generalizability Comparison I. The estimated mean MAD decreased as the size of the sample increased as shown in Table 25.

Table 25

MAD Mean, Standard Deviation, Minimum, and Maximum for Number of Sample Items Factor Associated with Generalizability Comparison I (n=972)

Sample Size	Mean	SD	Min	Max
36	0.05	0.02	0.01	0.13
47	0.04	0.02	0.01	0.11
72	0.03	0.01	0.01	0.08
94	0.02	0.01	0.01	0.06
107	0.02	0.01	0.01	0.04
143	0.00	0.00	0.00	0.00

Figure 13 provides a graphical representation of this change as the mean, standard deviation, and range of the estimated MAD decreases when the size of the sample is reduced.

Research Question 2. The second research question, "To what extent will the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?" focuses on the characteristics of the standard setting process. This question is specifically addressed by the number of raters, the percentage and magnitude of 'unreliable' raters, and the impact of group dynamics and discussion during the later rounds of the standard setting process.

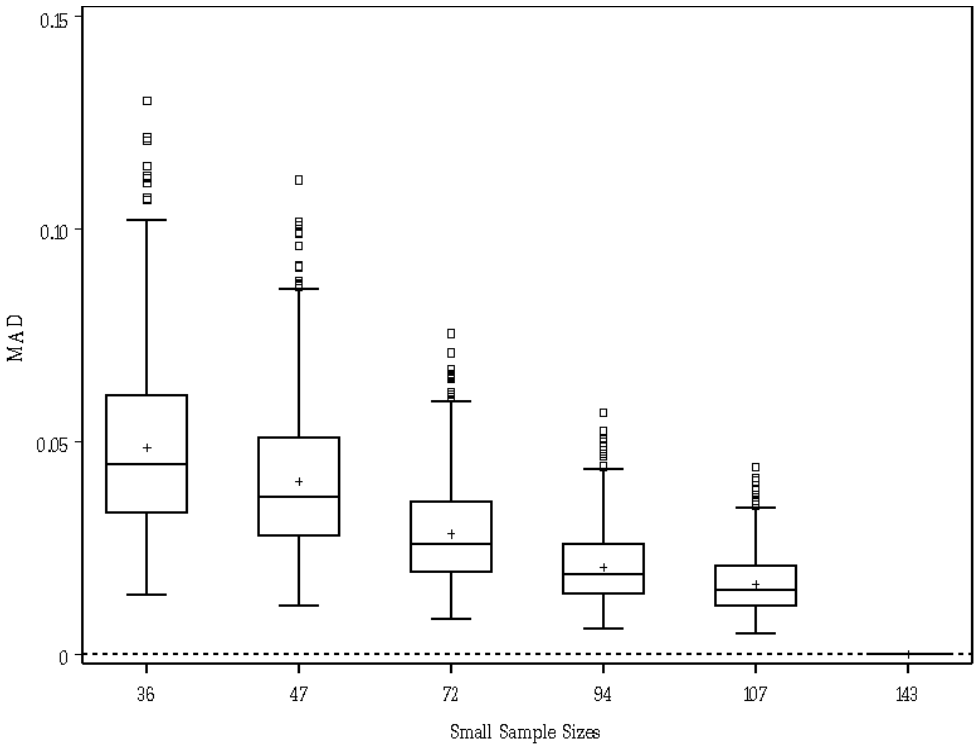


Figure 13. Estimated MAD for small sample sizes for Generalizability Comparison I.

Only one of the four factors in research question 2 for MAD had eta-squared values that resulted in a medium effect or greater, the directional influence factor. The effect size for the estimated MAD in theta estimates associated with the directional influence factor was note worthy ($\eta^2 = 0.06$). The mean estimated MAD for directional influence towards the lowest rater was higher than the other two directional values as shown in Table 26.

The upper most limit of the lowest rater's estimated MAD (0.13) was also considerable higher than the other two directional influences as visually displayed in Figure 14.

Table 26

MAD Mean, Standard Deviation, Minimum, and Maximum for Directional Influence Factor Associated with Generalizability Comparison I (n=1944)

Directional Influence	Mean	SD	Min	Max
Lowest Rater	0.03	0.02	0.00	0.13
Average Rater	0.02	0.02	0.00	0.09
Highest Rater	0.02	0.02	0.00	0.08

The remaining three factors in research question 2 had an effect size for estimated MAD in theta estimates that was small and did not exceed the pre-established criteria of a medium effect size or greater, number of raters factor ($\eta^2 = 0.01$), the percentage of unreliable raters factor ($\eta^2 = 0.01$), and the magnitude of 'unreliability' factor ($\eta^2 = 0.01$).

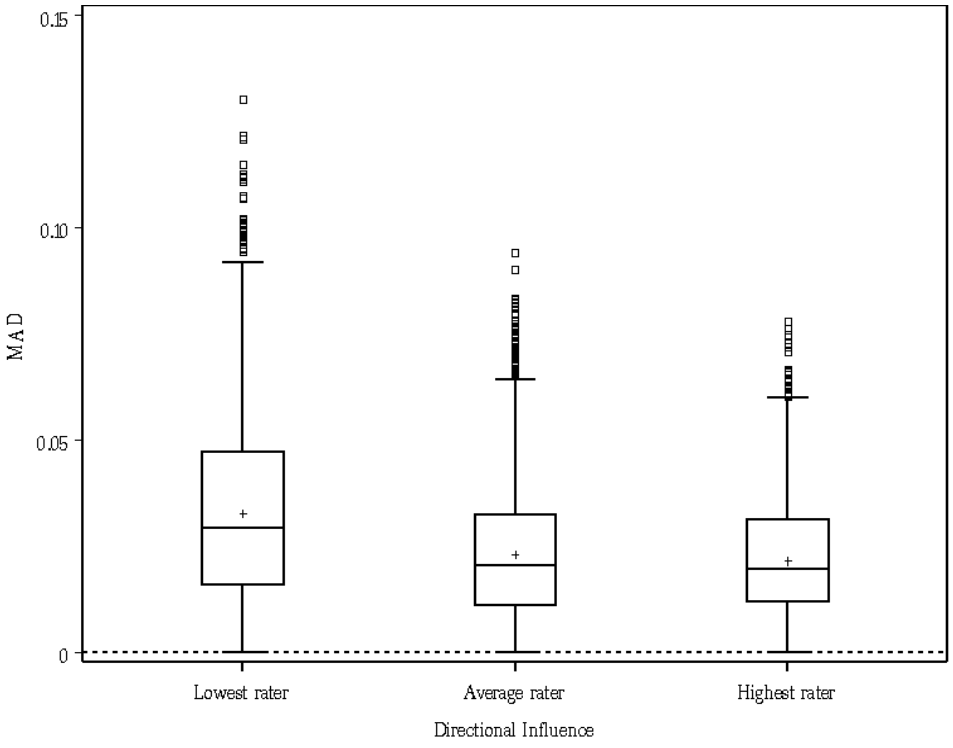


Figure14. Estimated MAD for the directional influences for Generalizability Comparison

I.

Generalizability Comparison II

The second generalizability comparison evaluated the difference between the small sample performance estimate and the 'true' originating performance estimate. Table 27 displays the mean, standard deviation, minimum, and maximum values for each of the outcome measures across the 5,832 conditions for Generalizability Comparison II.

Table 27

Mean, Standard Deviation, Minimum, and Maximum values for Outcomes Associated with Generalizability Comparison II (N=5832)

Outcome	Mean	SD	Min	Max
Bias	-0.10	0.41	-1.14	0.96
RMSE	0.39	0.22	0.08	1.15
MAD	0.37	0.22	0.07	1.14

The mean for estimated bias was -0.10 (SD = 0.41) with a range from -1.14 to 0.96. The mean for estimated RMSE was 0.39 (SD = 0.22) with a range from 0.08 to 1.15 and the mean for estimated MAD was 0.37 (SD = 0.22) with a range from 0.07 to 1.14. Figure 15 is a graphical representation of the distributions for each of the three outcome variables for Generalizability Comparison II.

Thirty-one conditions had at least one outcome for Generalizability II that was equal to -1 or less (bias) or equal to 1 or greater (RMSE, MAD). All thirty-one identified conditions had a directional influence towards the lowest rater, an originating theta of 1, and a SAT simulated uniform item difficulty distribution as shown in Table 28.

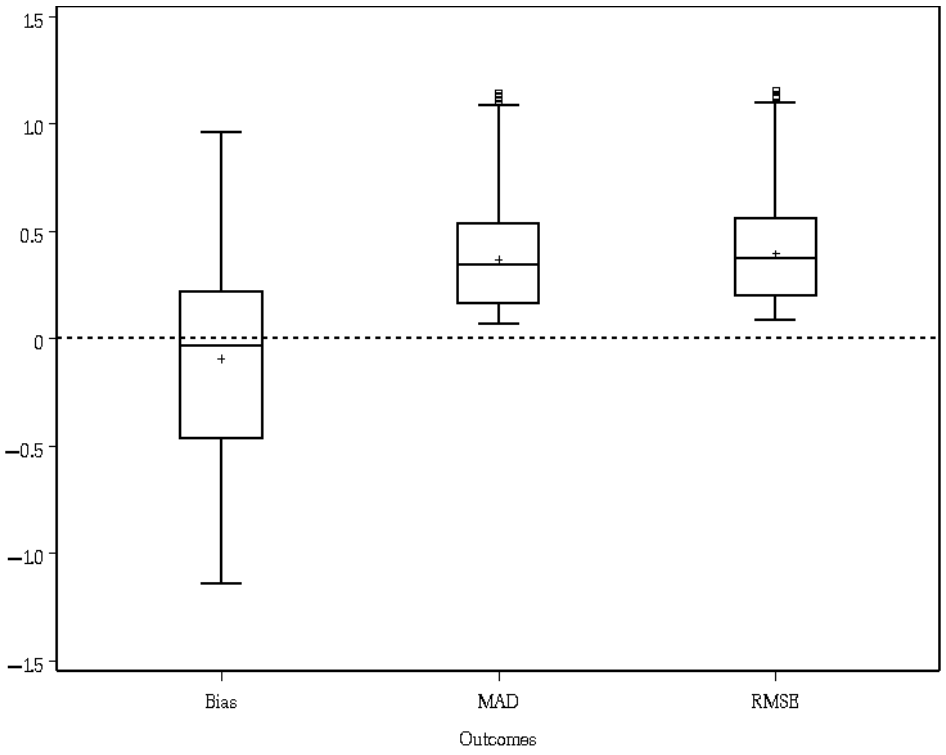


Figure 15. Outcome distributions for Generalizability Comparison II

The results of the simulation were evaluated using SAS PROC GLM. The dependent variables in the model were the three outcome variables, Bias, RMSE, and MAD. The seven independent variables were the seven different factors from the simulation model. Three different models were evaluated, main effects model, two-way interaction model, and three-way interaction model. For the bias outcome, 91.0% of the variability was explained by the main effects of the seven simulation factors. This was considerable higher than the 19% of variability explained for bias in the main effects model for Generalizability Comparison I. In terms of RMSE and MAD outcomes, 73.3% and 72.3% of the variability was explained, respectively, by the main effects of the seven simulation factors.

Table 28

Conditions for Generalizability II with an Outcome (bias, RMSE, MAD) equal to -1 or Less, or 1 or Greater (All Conditions Included Directional Influence = Lowest Rater, Originating Theta = 1, Item Difficulty Distribution = SAT Simulated Uniform)

Sample Size	Number of Raters	Rater Reliability	Fallible Raters (%)	Bias	RMSE	MAD
36	8	.85	75	-1.02	1.04	1.02
	12	.75	75	-0.99	1.01	0.99
	12	.85	50	-0.99	1.00	0.99
	12	.85	75	-1.09	1.10	1.09
	16	.75	75	-1.04	1.05	1.04
	16	.85	50	-1.05	1.06	1.05
	16	.85	75	-1.14	1.15	1.14
47	12	.85	75	-1.05	1.06	1.05
	16	.75	75	-1.02	1.03	1.02
	16	.85	50	-1.02	1.03	1.02
	16	.85	75	-1.11	1.12	1.11
72	8	.85	75	-1.00	1.01	1.00
	12	.85	75	-1.07	1.08	1.07
	16	.75	75	-1.04	1.05	1.04
	16	.85	50	-1.04	1.05	1.04
	16	.85	75	-1.12	1.13	1.12
94	8	.85	75	-0.99	1.01	0.99
	12	.85	75	-1.08	1.09	1.08
	16	.75	75	-1.03	1.04	1.03
	16	.85	50	-1.04	1.05	1.04
	16	.85	75	-1.12	1.13	1.12
107	8	.85	75	-0.99	1.00	0.99
	12	.85	75	-1.07	1.08	1.07
	16	.75	75	-1.03	1.04	1.03
	16	.85	50	-1.03	1.04	1.03
	16	.85	75	-1.13	1.14	1.13
143	8	.85	75	-1.00	1.01	1.00
	12	.85	75	-1.08	1.09	1.08
	16	.75	75	-1.03	1.04	1.03
	16	.85	50	-1.04	1.05	1.04
	16	.85	75	-1.13	1.14	1.13

Table 29 displays the eta-squared values for each of the main effects for Generalizability Comparison II. Using the pre-established standard of Cohen's medium effect size criteria ($\eta^2 = 0.06$), the only note worthy bias main effect was the directional influence factor ($\eta^2 = 0.84$).

Table 29

Eta-squared Analysis of the Main Effects of the Factors in the Simulation for Generalizability Comparison II

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct	0.84*	0.57*	0.57*
Dist	0.03	0.06*	0.07*
SampleN	0.00	0.00	0.00
RaterN	0.00	0.01	0.00
Fallible%	0.00	0.02	0.01
ρ_{XX}	0.00	0.02	0.02
θ_{mc}	0.04	0.05	0.06*

* Eta-squared value at or above Cohen's medium effect size criteria of 0.06

Note. Direct = directional influence, Dist = item difficulty distribution, SampleN = sample size, RaterN = number of raters, Fallible% = percentage of fallible raters, ρ_{XX} = reliability of fallible raters, and θ_{mc}=location of the originating theta

In terms of the RMSE, three of the factors had eta-squared values resulting in at least a medium effect, directional influence ($\eta^2 = 0.57$), item difficulty distribution ($\eta^2 = 0.07$), and the location of the 'true' performance standard ($\eta^2 = 0.06$). Two factors of these same factors had at least a medium effect for the MAD outcome, they were directional influence ($\eta^2 = 0.57$) and item difficulty distribution ($\eta^2 = 0.06$). Almost all of the variability in the bias outcome is explained by the two-way interaction model with 99.6% of explained variability in the model. The amount of explained variability in the RMSE and MAD outcome measures increased to 92.7% and 92.9%, respectively.

Table 30

Eta-square Analysis of the Two-way Interaction Effects of the Factors in the Simulation Generalizability Comparison II

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct x Dist	0.01	0.06*	0.05
Direct x SampleN	0.00	0.00	0.00
Direct x RaterN	0.01	0.01	0.01
Sample x Dist	0.00	0.00	0.00
RaterN x Dist	0.00	0.00	0.00
RaterN x SampleN	0.00	0.00	0.00
Fallible% x Direct	0.01	0.01	0.01
Fallible% x Dist	0.00	0.00	0.00
Fallible% x SampleN	0.00	0.00	0.00
Fallible% x RaterN	0.00	0.00	0.00
Fallible% x ρ_{XX}	0.00	0.00	0.00
Fallible% x θ_{mc}	0.00	0.00	0.00
ρ_{XX} *x Direct*	0.01	0.01	0.01
ρ_{XX} *x Dist*	0.00	0.00	0.00
ρ_{XX} *x SampleN*	0.00	0.00	0.00
ρ_{XX} *x RaterN*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc}	0.00	0.00	0.00
θ_{mc} *x Direct*	0.03	0.11*	0.10*
θ_{mc} *x Dist*	0.03	0.01	0.01
θ_{mc} *x SampleN*	0.00	0.00	0.00
θ_{mc} *RaterN*	0.00	0.00	0.00

* Eta-squared value at or above Cohen's medium effect size criteria of 0.06

Note. Direct = directional influence, Dist = item difficulty distribution, SampleN = sample size, RaterN = number of raters, Fallible% = percentage of fallible raters, ρ_{XX} = reliability of fallible raters, and θ_{mc}=location of the originating theta

Table 30 displays the eta-squared values for each of the two-way interaction effects for Generalizability Comparison II. Using the pre-established standard of Cohen's medium effect size criteria ($\eta^2 = 0.06$), there were no note worthy two-way interactions related to bias. In terms of the RMSE, one two-way interaction exceeded the pre-established threshold; the interaction between the location of the 'true' performance standard factor and the directional influence factor ($\eta^2 = 0.10$). This same interaction was

also identified for the MAD outcome ($\eta^2 = 0.11$). The MAD outcome also had a second two-way interaction that exceeded the pre-established threshold, the interaction between the directional influence factor and the item difficulty distribution factor ($\eta^2 = 0.06$).With almost all of the variability explained in the two-way interaction model, the bias outcome only had a modest increase to 99.9% of the variance explained in the three-way interaction model. The RMSE and MAD outcomes also had almost all of the variability explained in the three-way interaction model with 99.3% and 99.3% of the variability explained by the model, respectively.

Table 31 displays the eta-squared values for each of the three-way interaction effects for Generalizability Comparison II. Using the pre-established standard of Cohen's medium effect size criteria ($\eta^2 = 0.06$), there were no note worthy three-way interactions for the bias outcome measure. The RMSE and MAD outcome measures each had one three-way interaction which exceeded the pre-established medium effect threshold. That interaction for both outcomes was between the 'true' performance standard factor, the directional influence factor, and the item difficulty distribution factor (Both RMSE and MAD: $\eta^2 = 0.06$).

Bias in Generalizability Comparison II

Research Question 1. The first research question, "To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?" focuses on the characteristics and the relationship between the two item sets. This question is specifically addressed by the distribution of item difficulties in the larger item set, the placement of the 'true'

performance standard influence, and the number of items drawn from the larger item set.

Table 31

Eta-square Analysis of the Three-way Interaction Effects of the Factors in the Simulation for Generalizability Comparison II

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct x RaterN x Dist	0.00	0.00	0.00
Direct x RaterN x SampleN	0.00	0.00	0.00
RaterN x SampleN x Dist	0.00	0.00	0.00
Fallible% x ρ_{XX} *x Direct*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x Dist*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x SampleN*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x RaterN*	0.00	0.00	0.00
Fallible% x ρ_{XX} *x* θ_{mc}	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct x Dist*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct x SampleN*	0.00	0.00	0.00
ρ_{XX} *x* θ_{mc} *x Direct x RaterN*	0.00	0.00	0.00
θ_{mc} *x Direct x Dist*	0.00	0.06*	0.06*
θ_{mc} *x Direct x SampleN*	0.00	0.00	0.00
θ_{mc} *x Direct x RaterN*	0.00	0.00	0.00

* Eta-squared value at or above Cohen's medium effect size criteria of 0.06

Note. Direct = directional influence, Dist = item difficulty distribution, SampleN = sample size, RaterN = number of raters, Fallible% = percentage of fallible raters, ρ_{XX} = reliability of fallible raters, and θ_{mc}=location of the originating theta

None of the three factors in research question 1 for bias resulted in a medium or greater effect size for eta-squared. The variance in bias in theta estimates associated with the item difficulty distributions factor ($\eta^2 = 0.03$), the 'true' performance standard factor ($\eta^2 = 0.04$), and the number of sample items factor ($\eta^2 = 0.00$) were all below the pre-established threshold.

Research Question 2. The second research question, "To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?" focuses on the characteristics of the standard setting process.

This question is specifically addressed by the number of raters, the percentage and magnitude of 'unreliable' raters, and the impact of group dynamics and discussion during the later rounds of the standard setting process.

Only one of the four factors in research question 2 for bias had eta-squared values that resulted in a medium effect or greater, the directional influence factor. In fact, the resulting effect size was large ($\eta^2 = 0.84$). The estimated bias for directional influence towards the lowest rater was negative and substantially lower than the other two directional values as shown in Table 32. All values of the influence towards the lowest rater were negatively bias. Conditions which were equal to -1 or less are located in Table 28. All identified conditions had an originating theta of 1 and a SAT simulated uniform item difficulty distribution.

Table 32

Bias Mean, Standard Deviation, Minimum, and Maximum for Directional Influence Factor Associated with Generalizability Comparison II (n=1944)

Directional Influence	Mean	SD	Min	Max
Lowest Rater	-0.57	0.16	-1.14	-0.25
Average Rater	-0.07	0.12	-0.48	0.17
Highest Rater	0.35	0.20	-0.08	0.96

The upper most limit of the highest rater's estimated bias (0.96) was considerable higher than the other two directional influences as visually displayed in Figure 16. The remaining three factors in research question 2 had variance in estimated bias in theta estimates that was small and did not exceed the pre-established criteria of a medium effect size or greater, number of raters factor ($\eta^2 = 0.00$), the percentage of

unreliable raters factor ($\eta^2 = 0.00$), and the magnitude of 'unreliability' factor ($\eta^2 = 0.00$).

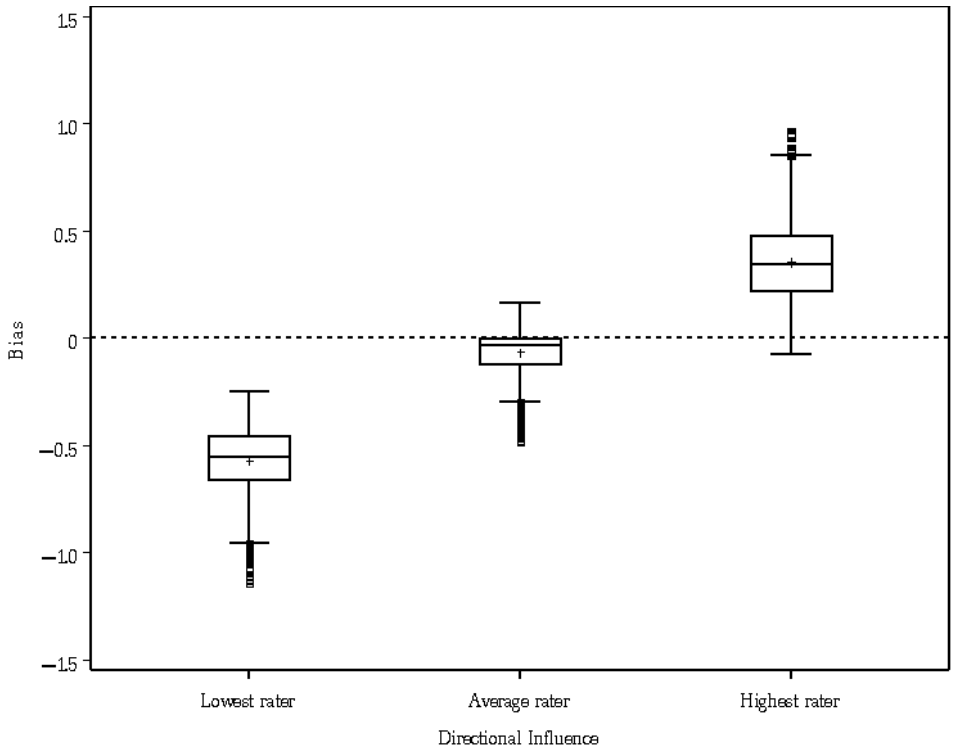


Figure 16. Estimated bias for the directional influences for Generalizability Comparison II.

Root Mean Square Error in Generalizability Comparison II

Research Question 1. The first research question, "To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?" focuses on the characteristics and the relationship between the two item sets. This question is specifically addressed by the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard influence, and the number of items drawn from the larger item set.

Two of the three factors in research question 1 for variance in estimated RMSE had eta-squared values that resulted in a medium effect or greater, the item difficulty distributions factor and the placement of the 'true' performance standard factor. The variance in estimated RMSE in theta estimates associated with the item difficulty distributions factor ($\eta^2 = 0.07$) exceeded the pre-established standard. Table 33 displays the RMSE descriptive statistics for each of the four levels of the item difficulty distribution factor.

Table 33

RMSE Mean, Standard Deviation, Minimum, and Maximum for Item Difficulty Distribution Factor Associated with Generalizability Comparison II (n=1458)

Item Difficulty Distribution	Mean	SD	Min	Max
Real Item	0.42	0.21	0.11	0.97
Sim. SAT Low	0.33	0.18	0.08	0.83
Sim. SAT	0.35	0.19	0.10	0.81
Sim. Unif.	0.48	0.26	0.10	1.15

While real item difficulty distribution and the simulated uniform had slightly higher RMSE means and standard deviations than the other two distributions, one noticeable difference between the item difficulty distributions was the higher range of the RMSE estimates for the simulated uniform item difficulty distribution as opposed to the other three item difficulty distributions as shown in Figure 17. Conditions which were equal to 1 or greater are located in Table 28. All identified conditions had a directional influence towards the lowest rater and an originating theta of 1. The item difficulty distribution factor was also involved in a three-way interaction that met the

pre-established medium effect criteria. This three-way interaction was with the placement of the 'true' performance standard factor and the directional influence factor ($\eta^2 = 0.06$). This result will be discussed in more detail in the directional influence factor section.

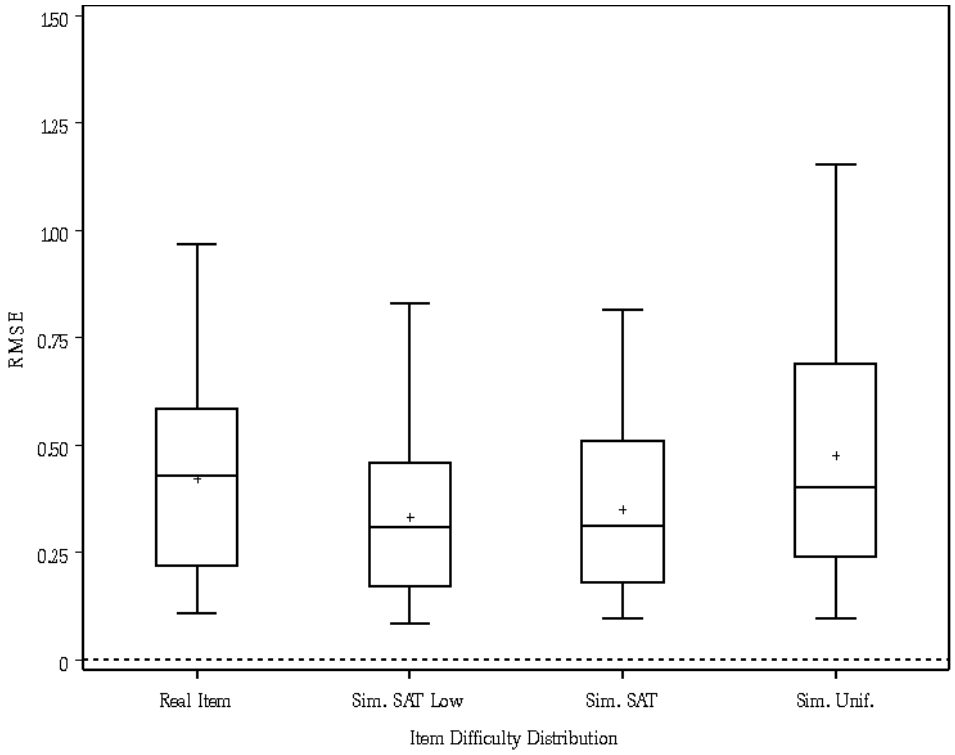


Figure 17. Estimated RMSE for item difficulty distributions for Generalizability Comparison II.

The variance in estimated RMSE in theta estimates associated with the placement of the 'true' performance standard factor ($\eta^2 = 0.06$) also exceeded the pre-established medium effect standard. The estimated mean RMSE for an originating theta of -1 was higher (0.47) than the other two estimated mean RMSE values (0.36) as shown in Table 34.

Table 34

RMSE Mean, Standard Deviation, Minimum, and Maximum for Placement of the 'True' Performance Standard Factor Associated with Generalizability Comparison II (n=1944)

Originating Theta	Mean	SD	Min	Max
-1	0.47	0.22	0.12	0.97
0	0.36	0.19	0.08	0.97
1	0.36	0.23	0.10	1.15

In addition to the estimated RMSE mean difference among samples, the upper most limit of each originating theta value was different with 1 having the highest (1.15) of the three values as visually displayed in Figure 18.

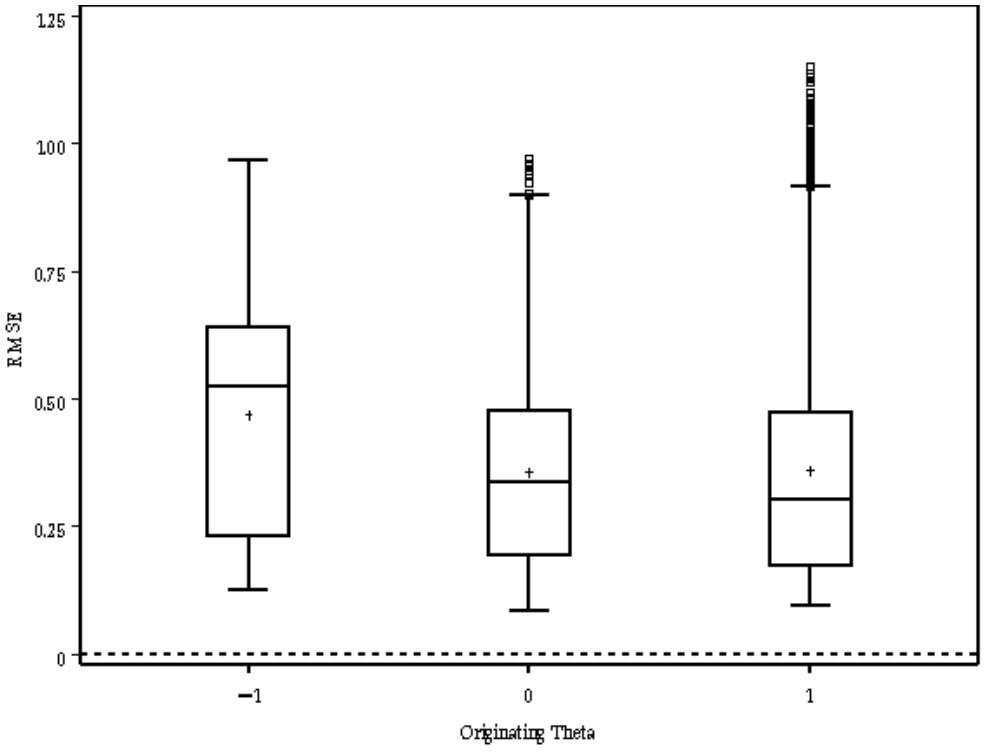


Figure 18. Estimated RMSE for the placement of the 'true' performance standard for Generalizability Comparison II.

Conditions which were equal to 1 or greater are located in Table 28. All identified conditions had a directional influence towards the lowest rater and a SAT simulated uniform item difficulty distribution.

The placement of the 'true' performance standard factor also had a notable two-way interaction with the directional influence factor ($\eta^2 = 0.10$) and a three-way interaction with the directional influence factor and the item difficulty distribution factor ($\eta^2 = 0.06$). These results will be discussed in more detail in the directional influence factor section. The variance in estimated RMSE in theta estimates associated with the number of sample items factor was very small ($\eta^2 = 0.00$) and did not exceed the pre-established medium effect standard.

Research Question 2. The second research question, "To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?" focuses on the characteristics of the standard setting process. This question is specifically addressed by the number of raters, the percentage and magnitude of 'unreliable' raters, and the impact of group dynamics and discussion during the later rounds of the standard setting process.

Only one of the four factors in research question 2 for RMSE had eta-squared values that resulted in a medium effect or greater, the directional influence factor. The variance in estimated RMSE in theta estimates for the directional influence factor had the highest eta-squared value ($\eta^2 = 0.57$) of any of the other factors in Generalizability Comparison II. The estimated RMSE for directional influence towards the lowest rater was higher than the other two directional values as shown in Table 35.

Table 35

RMSE Mean, Standard Deviation, Minimum, and Maximum for Directional Influence Factor Associated with Generalizability Comparison II (n=1944)

Directional Influence	Mean	SD	Min	Max
Lowest Rater	0.60	0.15	0.28	1.15
Average Rater	0.19	0.08	0.08	0.50
Highest Rater	0.39	0.18	0.10	0.97

The upper most limit of the lowest rater's estimated RMSE (1.15) was also considerably higher than the other two directional influences as visually displayed in Figure 19.

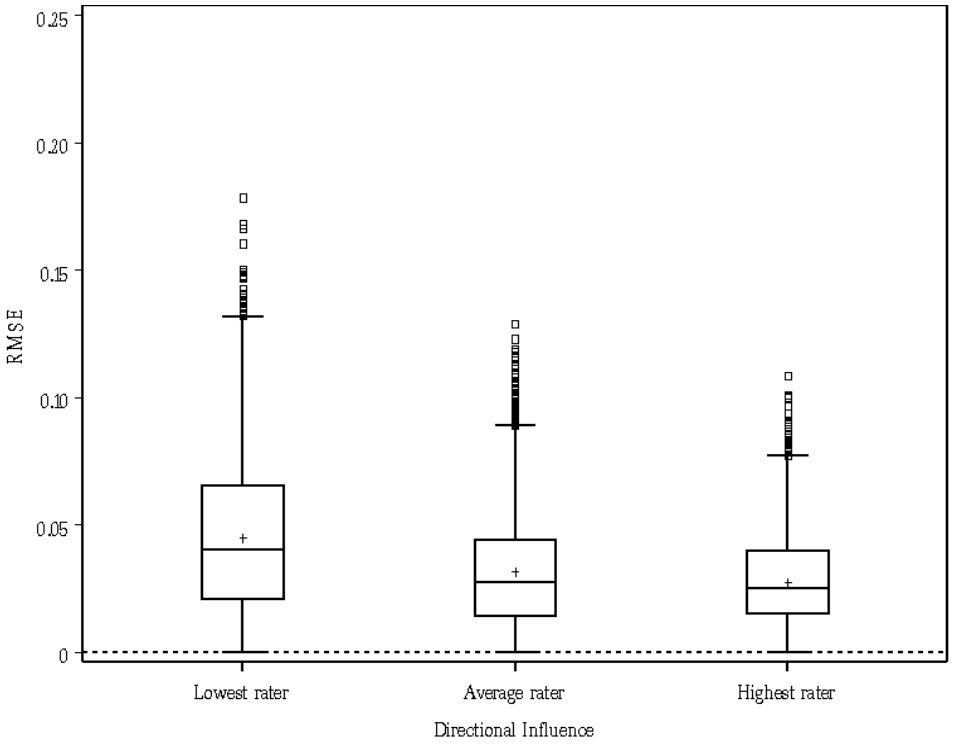


Figure 19. Estimated RMSE for the directional influences for Generalizability Comparison II.

Conditions which were equal to 1 or greater are located in Table 28. All identified conditions had an originating theta of 1, and a SAT simulated uniform item difficulty distribution. As mentioned previously, the placement of the 'true' performance standard factor interacted with the directional influences factor ($\eta^2 = 0.10$). Figure 20 graphically displays this two-way interaction. The results suggest that while the directional influence towards the lowest and average rater were impacted similarly by the various originating theta, the directional influence towards the highest rater was impacted differently.

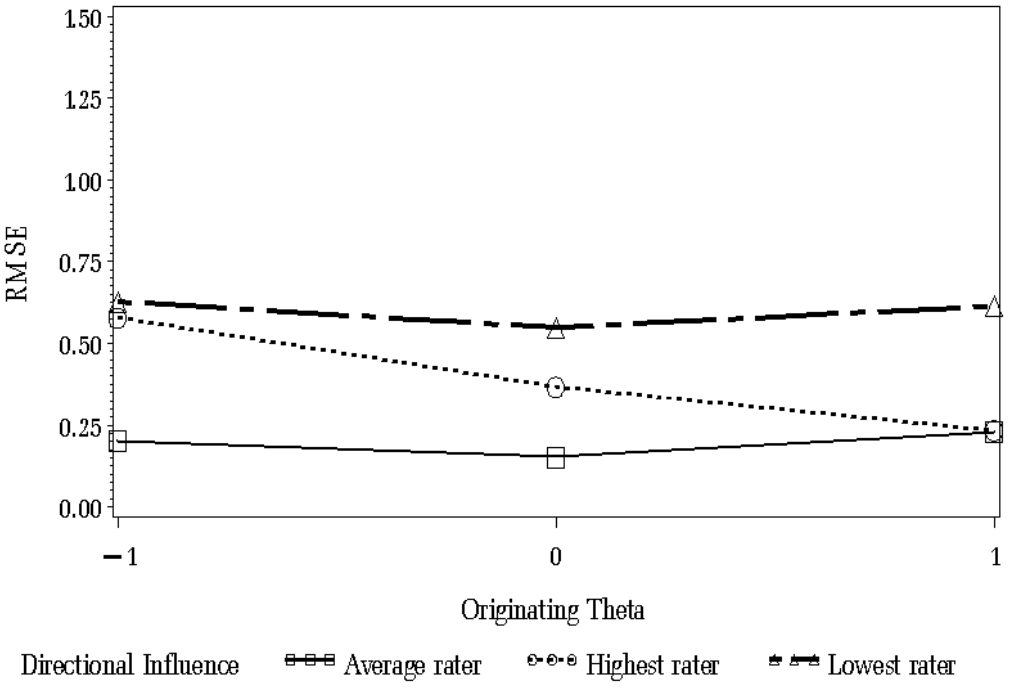


Figure 20. Estimated RMSE two-way interaction between the placement of the 'true' performance standard factor and the directional influences factor for Generalizability Comparison II.

Table 36 displays the results of the two-way interaction between the placement of the 'true' performance standard factor and the directional influences factor by displaying the estimated RMSE as a function of the placement of the 'true' performance standard factor and the directional influences factor.

In addition to the two-way interaction, the directional influence factor was also involved in a three-way interaction with the placement of the 'true' performance standard factor and the item difficulty distribution factor ($\eta^2 = 0.06$). This three-way interaction is graphically displayed in Figures 21-23 with separate figures for each level of the originating theta.

Table 36

Estimated RMSE as a Function of the Placement of the 'True' Performance Standard Factor and the Directional Influences Factor Associated with Generalizability Comparison II (n=648)

	Originating Theta		
Directional Influence	-1	0	1
Lowest Rater	0.63	0.55	0.61
Average Rater	0.20	0.15	0.23
Highest Rater	0.58	0.37	0.23

When the originating theta is -1, the four item difficulty distributions converge in terms of mean RMSE when the direction influence is to the lowest rater. When the directional influence is towards the average rater, the real item and simulated uniform distributions converge in terms of mean RMSE, while the simulated SAT and simulated SAT with lower variance distributions converge at a lower mean RMSE. The relationship between item difficulty distributions is even more pronounced when the directional

influence is towards the highest rater.

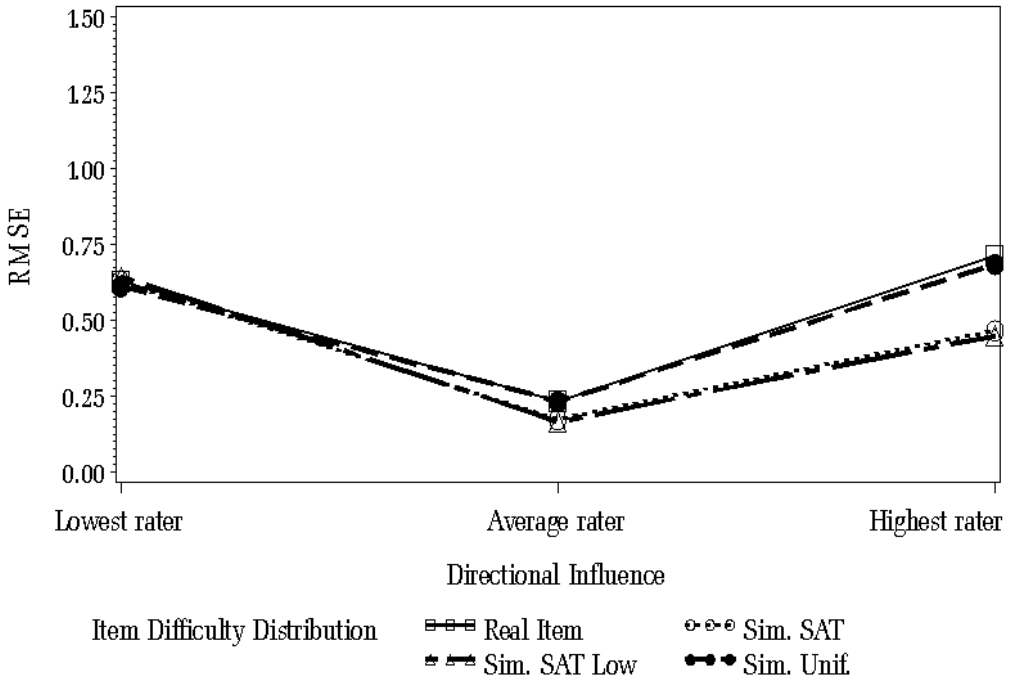


Figure 21. Estimated RMSE interaction between item difficulty distribution factor and the directional influences factor at originating theta=-1 for Generalizability Comparison II.

When the originating theta is 0, the four item difficulty distributions have the least amount of convergence in terms of mean RMSE when the directional influence is to the lowest rater. The simulated SAT and simulated SAT with lower variance distributions have the most similar mean RMSE as compared to the other item difficulty distributions. The simulated uniform distribution has the highest mean RMSE at all directional influences except for the highest rater influence where the real item difficulty distribution has the highest mean RMSE.

When the originating theta is 1, the four item difficulty distributions also have the least amount of convergence in terms of mean RMSE when the directional influence is to the lowest rater. Again, the simulated SAT and simulated SAT with lower variance distributions have the most similar mean RMSE as compared to the other item difficulty distributions.

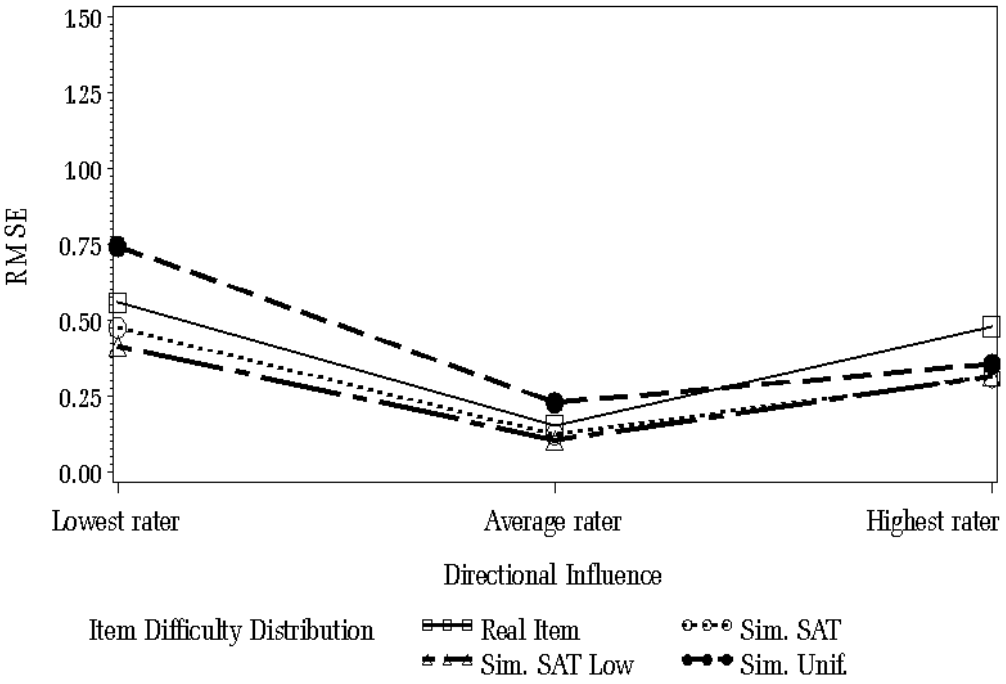


Figure 22. Estimated RMSE interaction between item difficulty distribution factor and the directional influences factor at originating theta=0 for Generalizability Comparison II.

The simulated uniform distribution also continues to have the highest mean RMSE at all directional influences except for the highest rater influence where the real item difficulty distribution again has the highest mean RMSE. However, the mean

RMSE for the simulated uniform distribution is impacted different from the other three item difficulty distributions as it has even more separation when the originating theta is 1.

The remaining three factors in research question 2 all had effect sizes for the estimated RMSE in theta estimates that were small and did not exceed the pre-established criteria of a medium effect size or greater, the number of raters factor ($\eta^2 = 0.00$), the percentage of unreliable raters factor ($\eta^2 = 0.01$), and the magnitude of 'unreliability' factor ($\eta^2 = 0.02$).

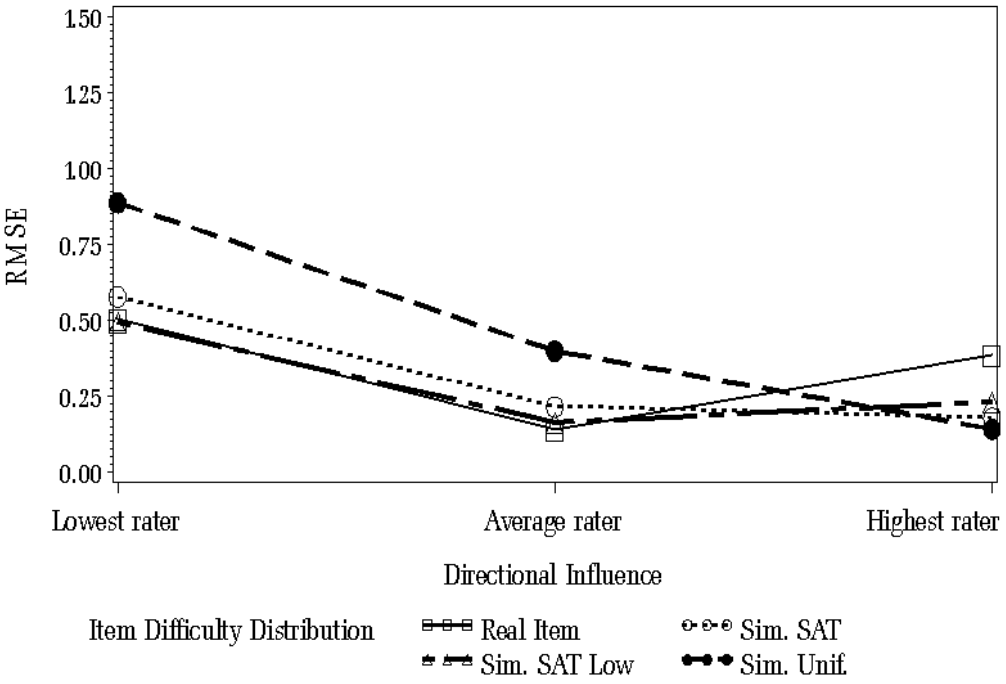


Figure 23. Estimated RMSE interaction between item difficulty distribution factor and the directional influences factor at originating theta=1 for Generalizability Comparison II.

Mean Absolute Deviation in Generalizability Comparison II

Research Question 1. The first research question, "To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?" focuses on the characteristics and the relationship between the two item sets. This question is specifically addressed by the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard influence, and the number of items drawn from the larger item set.

Only one of the three factors in research question 1 for variance in estimated MAD had an eta-squared value that resulted in a medium effect or greater, the item difficulty distributions factor. The variance in estimated MAD in theta estimates associated with the item difficulty distributions factor ($\eta^2 = 0.06$) exceeded the pre-established standard. Table 37 displays the mean and standard deviations for the four levels of the item difficulty distribution factor.

Table 37

MAD Mean, Standard Deviation, Minimum, and Maximum for Item Difficulty Distribution Factor for Associated with Generalizability Comparison II (n=1458)

Item Difficulty Distribution	Mean	SD	Min	Max
Real Item	0.39	0.21	0.08	0.96
Sim. SAT Low	0.31	0.18	0.07	0.81
Sim. SAT	0.32	0.19	0.08	0.80
Sim. Unif.	0.45	0.27	0.08	1.14

While real item difficulty distribution and the simulated uniform distribution had slightly higher MAD means and standard deviations than the other two distributions, one

noticeable difference between the item difficulty distributions was the higher range of the MAD estimates for the simulated uniform item difficulty distribution as opposed to the other three simulated distributions as shown in Figure 24.

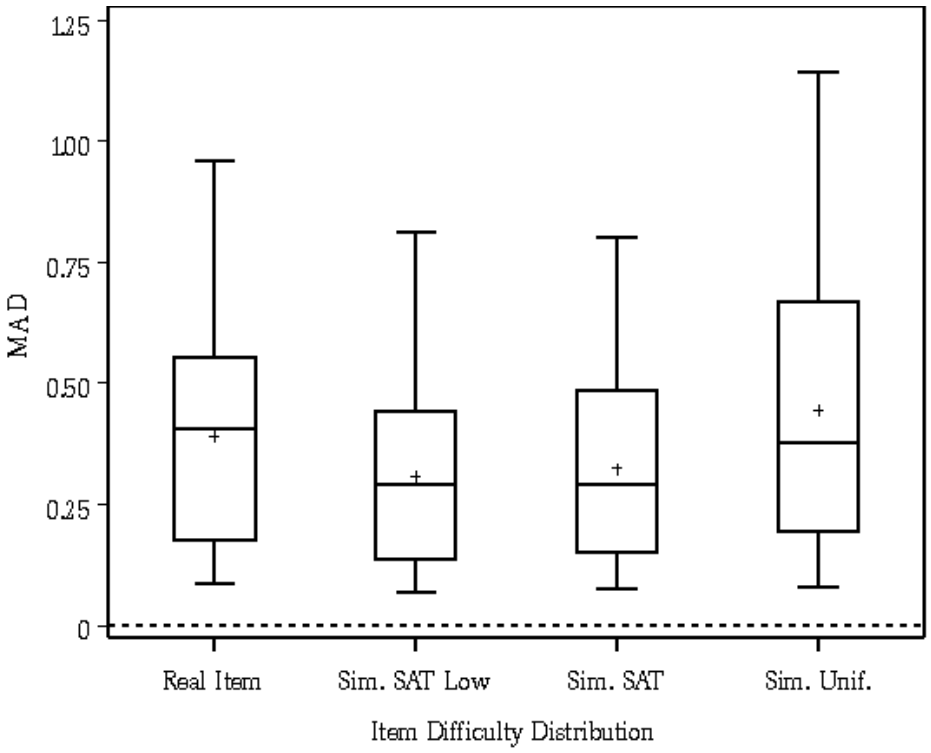


Figure 24. Estimated MAD for item difficulty distributions for Generalizability Comparison II.

Conditions which were equal to 1 or greater are located in Table 28. All identified conditions had a directional influence towards the lowest rater and an originating theta of 1. The item difficulty distribution factor was also involved in a two-way interaction with the directional influence factor ($\eta^2 = 0.06$) and a three-way interaction which also included the placement of the 'true' performance standard factor ($\eta^2 = 0.11$). These results will be discussed in more detail in the directional influence

factor section.

The variance in estimated MAD in theta estimates associated with the placement of the 'true' performance standard factor ($\eta^2 = 0.05$) did not exceed the pre-established medium effect standard, though the placement of the 'true' performance standard factor did have a notable two-way interaction with the directional influence factor ($\eta^2 = 0.11$). This result will be discussed in more detail in the directional influence factor section. The variance in estimated MAD in theta estimates associated with the number of sample items factor was very small ($\eta^2 = 0.00$) and also did not exceed the pre-established medium effect standard.

Research Question 2. The second research question, "To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?" focuses on the characteristics of the standard setting process. This question is specifically addressed by the number of raters, the percentage and magnitude of 'unreliable' raters, and the impact of group dynamics and discussion during the later rounds of the standard setting process.

Only one of the four factors in research question 2 for variance in estimated MAD had eta-squared values that resulted in a medium effect or greater, the directional influence factor. The variance in estimated MAD in theta estimates associated with the directional influence factor had the highest eta-squared value ($\eta^2 = 0.57$) of any of the other factors in Generalizability Comparison II. The estimated MAD for directional influence towards the lowest rater was higher than the other two directional values as shown in Table 38.

Table 38

MAD Mean, Standard Deviation, Minimum, and Maximum for Directional Influence Factor Associated with Generalizability Comparison II (n=1944)

Directional Influence	Mean	SD	Min	Max
Lowest Rater	0.57	0.16	0.25	1.14
Average Rater	0.16	0.08	0.07	0.48
Highest Rater	0.37	0.18	0.08	0.96

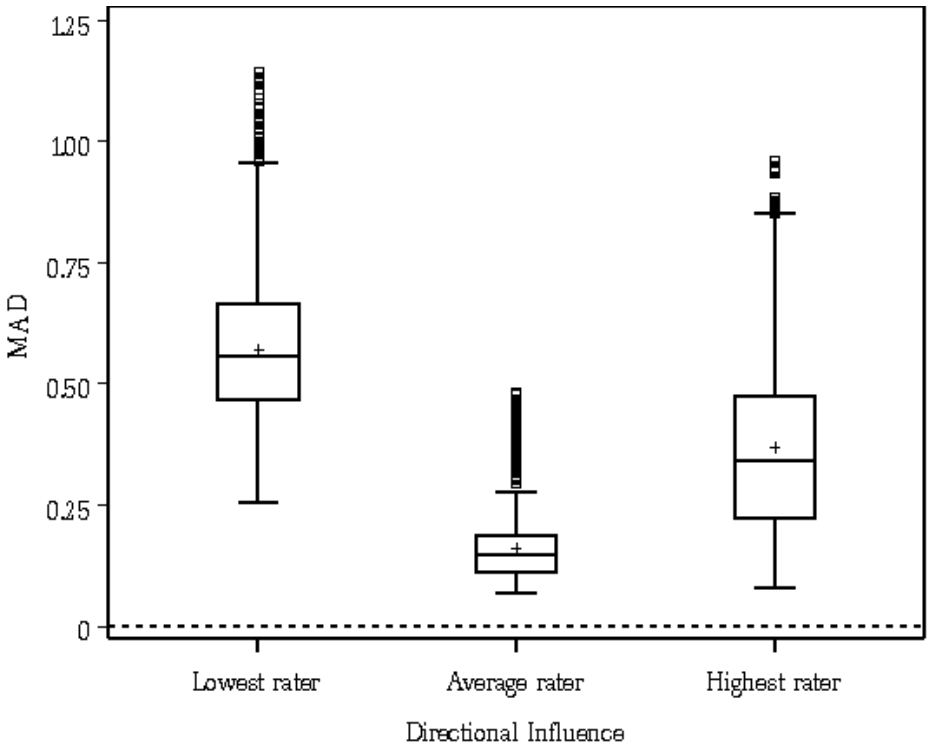


Figure 25. Estimated MAD for the directional influences for Generalizability Comparison II.

The upper most limit of the lowest rater's estimated MAD (1.14) was also considerable higher than the other two directional influences as visually displayed in Figure 25. Conditions which were equal to 1 or greater are located in Table 28. All

identified conditions had an originating theta of 1 and a SAT simulated uniform item difficulty distribution. As mentioned previously, the directional influences factor interacted with placement of the 'true' performance standard factor ($\eta^2 = 0.11$) and the item difficulty distribution factor ($\eta^2 = 0.06$).

Figure 26 graphically displays the two-way interaction between the placement of the 'true' performance standard factor and the directional influences factor. The results suggest that while the directional influence towards the lowest and average rater was impacted similarly by the various originating theta, the directional influence towards the highest rater was impacted differently.

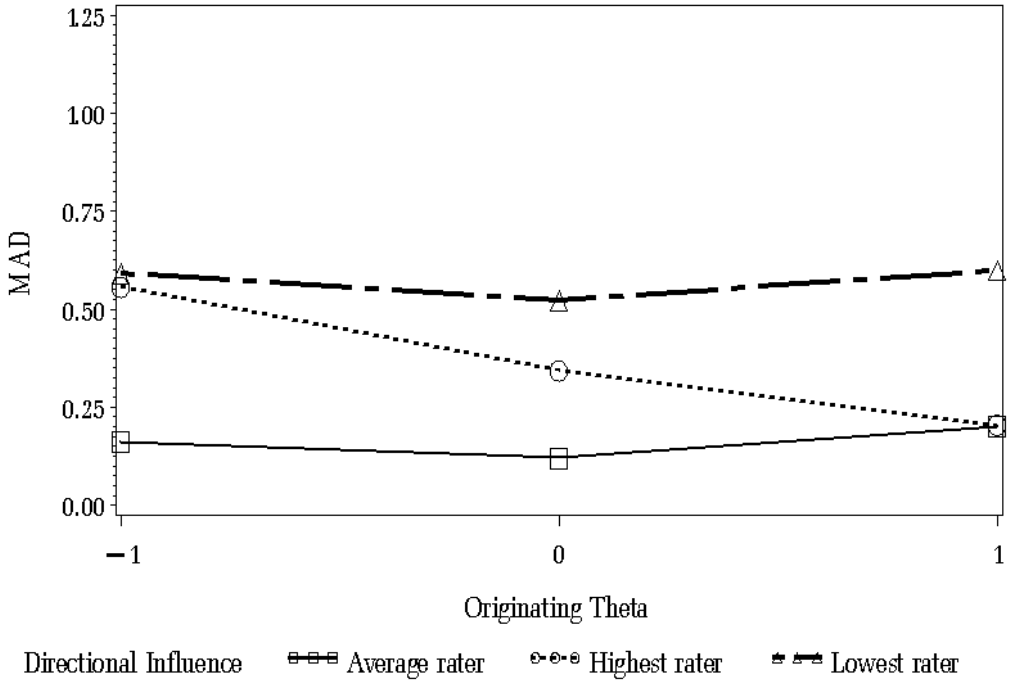


Figure 26. Estimated MAD two-way interaction between the placement of the 'true' performance standard factor and the directional influences factor for Generalizability

Comparison II.

Table 39 displays the estimated MAD as a function of the placement of the 'true' performance standard factor and the directional influences factor. Figure 27 provides the graphical representation of this two-way interaction between the item difficulty distribution and the directional influences factor.

Table 39

Estimated MAD as a Function of the Placement of the 'True' Performance Standard Factor and the Directional Influences Factor Associated with Generalizability Comparison II (n=648)

	Originating Theta		
Directional Influence	-1	0	1
Lowest Rater	0.59	0.52	0.60
Average Rater	0.16	0.12	0.20
Highest Rater	0.56	0.34	0.20

The results suggest that while the directional influence towards the lowest and average rater were impacted similarly by the various item difficulty distributions (with the uniform difficulty distribution having the largest mean MAD), the directional influence towards the highest rater was impacted differently with the real item distribution moving away from the remaining three item difficulty distribution grouping.

Table 40 also includes the results of the interaction between the item difficulty distribution factor and the directional influences factor. The directional influence factor was also involved in a three-way interaction with the placement of the 'true' performance standard factor and the placement of the 'true' performance standard factor ($\eta^2 = 0.06$).

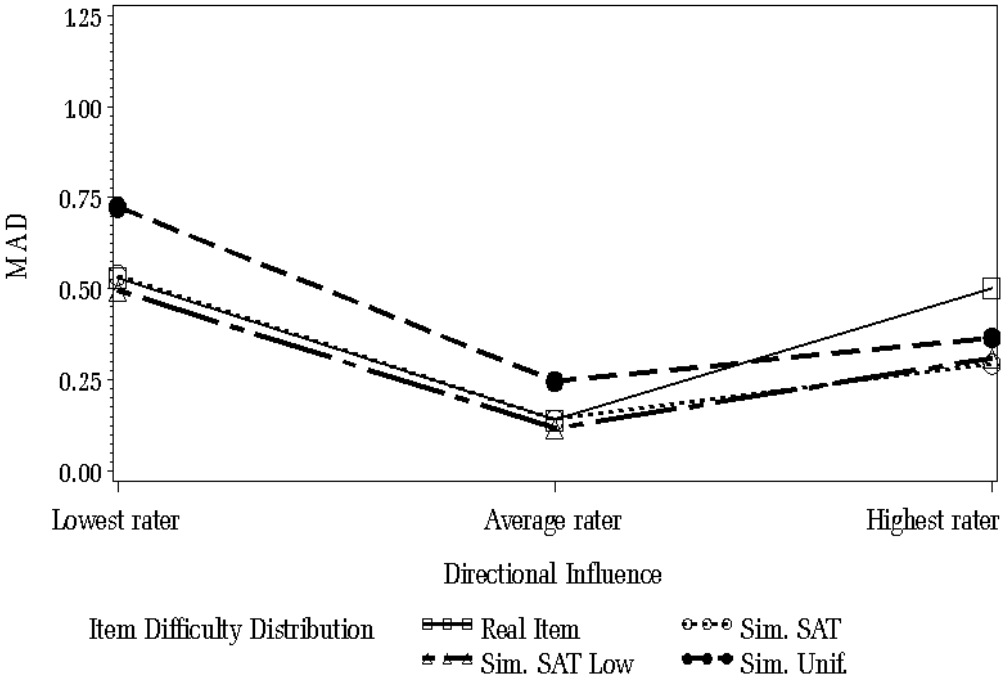


Figure 27. Estimated MAD two-way interaction between the item difficulty distribution and the directional influences factor for Generalizability Comparison II.

Table 40

Estimated MAD as a Function of the Item Difficulty Distribution Factor and the Directional Influences Factor Associated with Generalizability Comparison II (n=486)

Directional Influence	Item Difficulty Distribution			
	Real Item	SAT Sim	SAT Sim Low	Uniform
Lowest Rater	0.53	0.54	0.49	0.72
Average Rater	0.14	0.14	0.12	0.25
Highest Rater	0.50	0.29	0.31	0.37

Figures 28-30 display the results of this three-way interaction for each level of the originating theta. When the originating theta is -1, the four item difficulty distributions converge in terms of mean MAD when the direction influence is to the lowest rater.

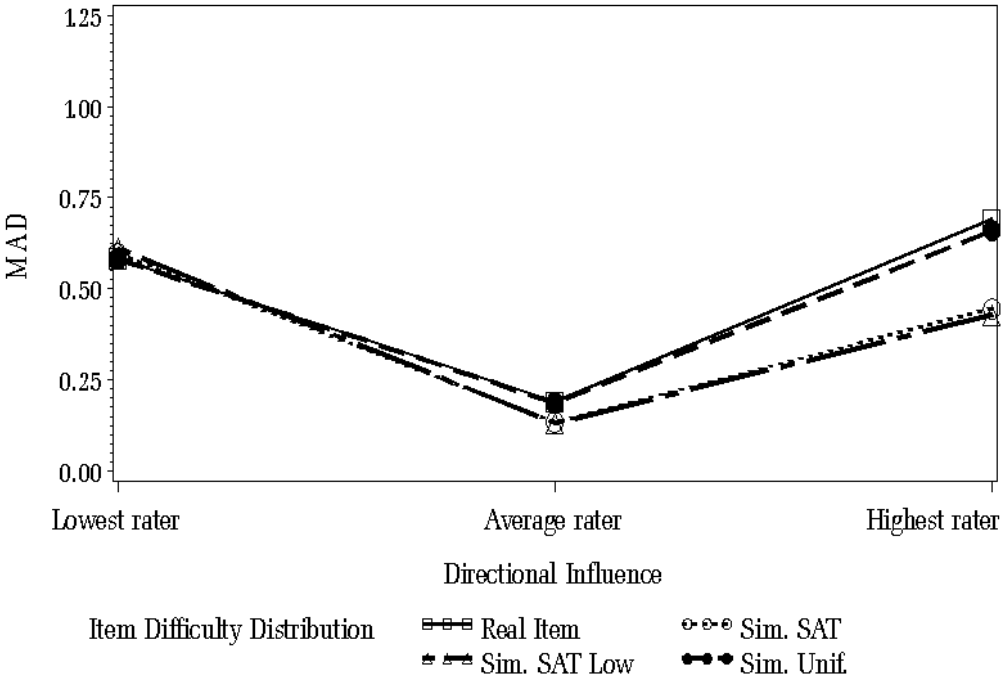


Figure 28. Estimated MAD interaction between item difficulty distribution factor and the directional influences factor at originating theta=-1 for Generalizability Comparison II.

When the directional influence is towards the average rater, the real item and simulated uniform distributions converge in terms of mean MAD, while the simulated SAT and simulated SAT with lower variance distributions converge at a lower mean MAD. The relationship between item difficulty distributions is even more pronounced when the directional influence is towards the highest rater. When the originating theta is 0, the four item difficulty distributions have the least amount of convergence in terms of

mean MAD when the directional influence is to the lowest rater. The simulated SAT and simulated SAT with lower variance distributions have the most similar mean MAD as compared to the other item difficulty distributions. The simulated uniform distribution has the highest mean MAD at all directional influences except for the highest rater influence where the real item difficulty distribution has the highest mean MAD.

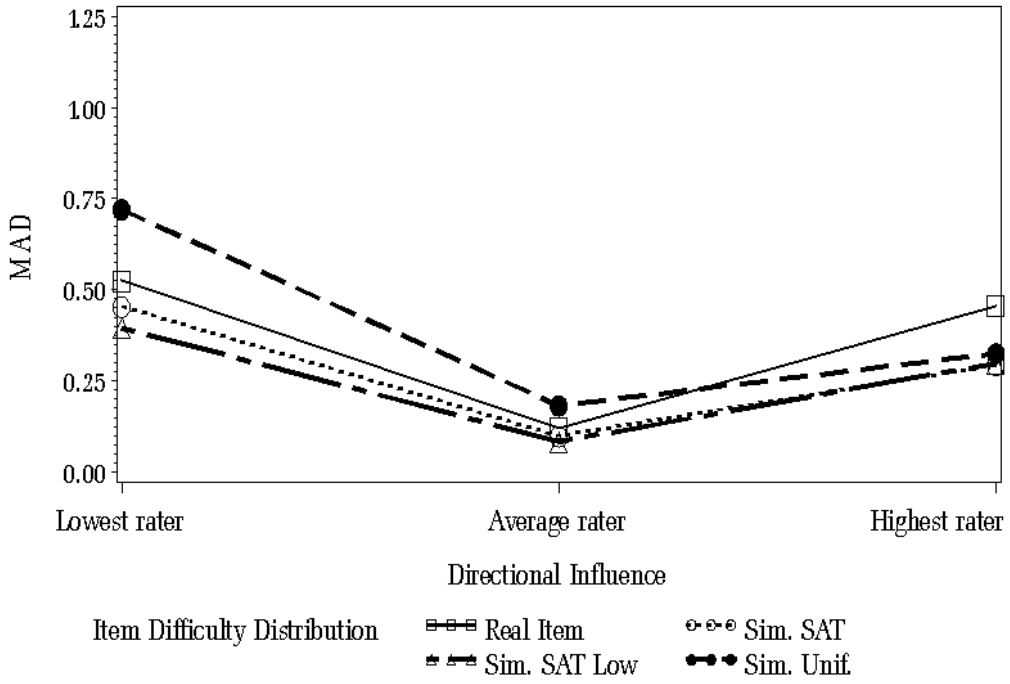


Figure 29. Estimated MAD interaction between item difficulty distribution factor and the directional influences factor at originating theta=0 for Generalizability Comparison II.

When the originating theta is 1, the four item difficulty distributions also have the least amount of convergence in terms of mean MAD when the directional influence is to the lowest rater. Again, the simulated SAT and simulated SAT with lower variance distributions have the most similar mean MAD as compared to the other item difficulty

distributions. The simulated uniform distribution also continues to have the highest mean MAD at all directional influences except for the highest rater influence where the real item difficulty distribution again has the highest mean MAD. However, the mean MAD for the simulated uniform distribution is impacted different from the other three item difficulty distributions as it has even more separation when the originating theta is 1.

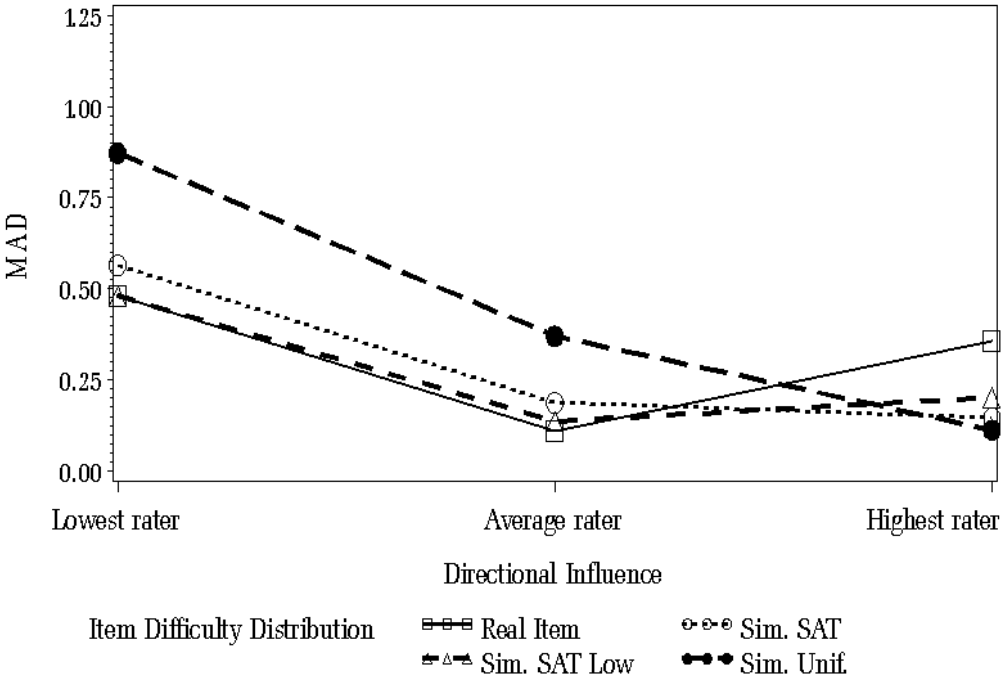


Figure 30. Estimated MAD interaction between item difficulty distribution factor and the directional influences factor at originating theta=1 for Generalizability Comparison II.

The remaining three factors in research question 2 had effect sizes for variance in estimated MAD in theta estimates that was small and did not exceed the pre-established criteria of a medium effect size or greater, the number of raters factor ($\eta^2 = 0.01$), the

percentage of unreliable raters factor ($\eta^2 = 0.02$), and the magnitude of 'unreliability' factor ($\eta^2 = 0.02$).

Actual Standard Setting Results Comparison

Results from an actual Angoff standard setting process were used as a 'pseudo' population. Samples were then drawn using a similar stratified random sampling methodology and comparisons were made to the results of the simulation study. Comparisons were made between an actual 112-item Angoff dataset (provided by S. G. Sireci) and the simulation results. The actual Angoff dataset contained 13 raters. One rater was randomly selected and removed in order to match the simulation parameters for rater size. The 112-item set contained one-parameter IRT values. The mean b-parameter was 0.01 (SD = 0.91) with a minimum value of -3.83 and a maximum value of 1.61.

The ability to generalize the performance standard was evaluated using a model similar to that used in the simulation. Since the items were calibrated under a one-parameter IRT model, only the difficulty parameters could be used for the stratification. The individual item difficulty parameters (b-values) were separated into three groups and stratified random samples were extracted based on one of the three item difficulty groupings. This ensured representative groups of item difficulty in the drawn samples. The sample sizes were based on the sample size factor used in the simulation. To match the characteristics of the simulation design and ensure stable results, one thousand samples were taken from each sample size. The three outcomes (bias, RMSE, and MAD) were calculated for each sample size across the one thousand samples.

Bias in Actual Angoff Dataset Comparison

Table 41 displays the estimated bias for each sample size as well as the descriptive statistics for the bias outcome from the simulation results for Generalizability Comparison I. The estimated bias calculated from the actual results falls within the ranges from the simulation study at each sample size. For example, the estimated bias for a sample size of 25% (0.011) falls within the range of estimated bias from the simulation results (-0.022 to 0.014).

Table 41

Bias for Small Sample Size in the Actual Angoff Dataset

Sample Size	Actual Results[a]	Simulation Results[b]			
		Mean	SD	Min	Max
25%	0.011	-0.003	0.006	-0.022	0.014
33%	0.009	0.002	0.005	-0.015	0.024
50%	0.006	0.001	0.004	-0.014	0.014
66%	0.003	0.000	0.002	-0.007	0.008
75%	-0.004	0.001	0.002	-0.005	0.007
100%	0.000	0.000	0.000	0.000	0.000

[a] n=1,000 replications at each sample size
[b] n=972 conditions at each sample size (1,000 replications each condition)

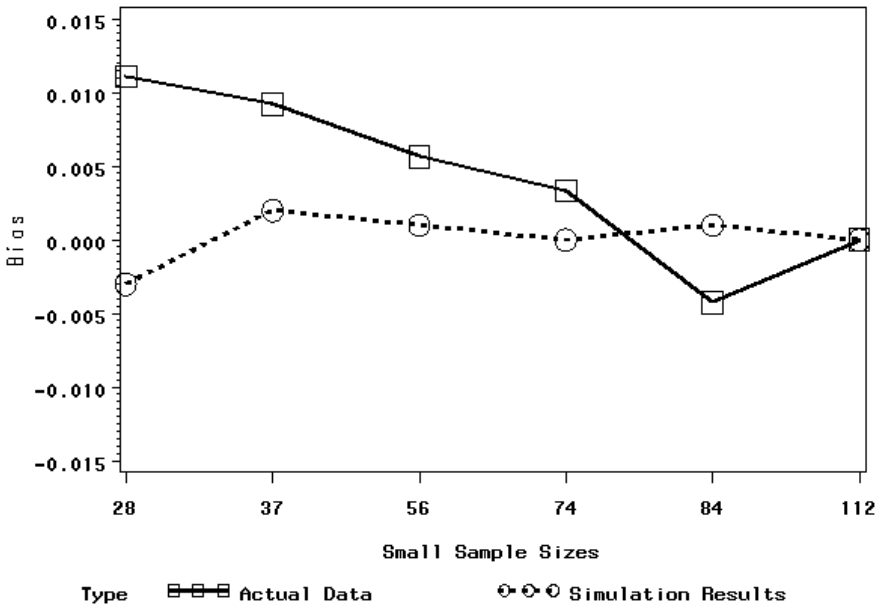


Figure 31. Estimated bias for the small sample sizes for actual Angoff and simulated datasets.

The estimated bias calculated from the actual results displays a reduction in the estimated bias as the sample size increases as shown in Figure 31.

RMSE in Actual Angoff Dataset Comparison

Table 42 displays the estimated RMSE for each sample size as well as the descriptive statistics for the RMSE outcome from the simulation results for Generalizability Comparison I. The estimated RMSE calculated from the actual results falls when in the range from the simulation study. For example, the estimated RMSE for a sample size of 50% (0.06) falls within the range of estimated bias from the simulation results (0.01 to 0.10).

Table 42

RMSE for Small Sample Size in the Actual Angoff Dataset

Sample Size	Actual Results[a]	Simulation Results[b]			
		Mean	SD	Min	Max
25%	0.10	0.07	0.03	0.02	0.18
33%	0.08	0.06	0.02	0.02	0.15
50%	0.06	0.04	0.02	0.01	0.10
66%	0.04	0.03	0.01	0.01	0.08
75%	0.03	0.02	0.01	0.01	0.06
100%	0.00	0.00	0.00	0.00	0.00

[a] n=1,000 replications at each sample size
[b] n=972 conditions at each sample size (1,000 replications each condition)

The estimated RMSE calculated from the actual results displays a similar reduction in the estimated RMSE as the sample size increases as shown in Figure 32.

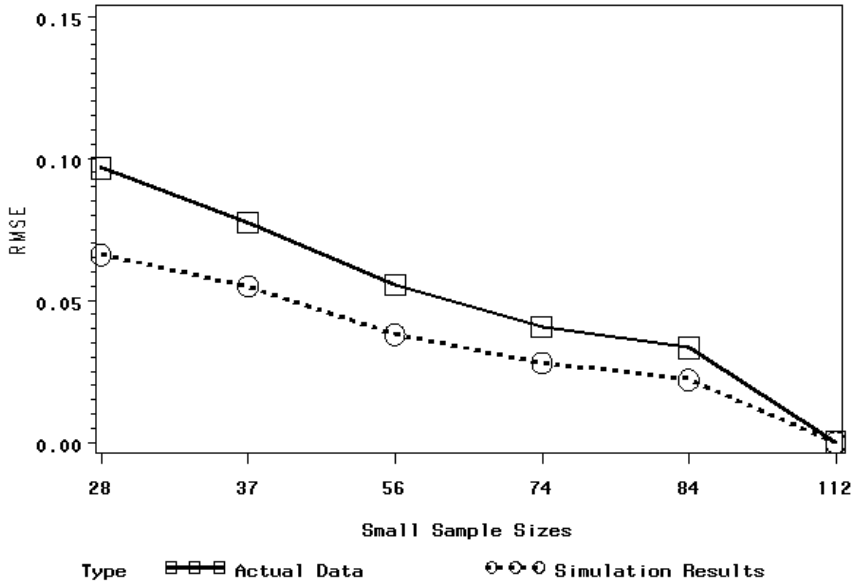


Figure 32. Estimated RMSE for the small sample sizes for actual Angoff and simulated datasets.

MAD in Actual Angoff Dataset Comparison

Table 43 displays the estimated MAD for each sample size as well as the descriptive statistics for the MAD outcome from the simulation results for Generalizability Comparison I. The estimated MAD calculated from the actual results falls when in the range from the simulation study. For example, the estimated MAD for a sample size of 75% (0.03) falls within the range of estimated bias from the simulation results (0.01 to 0.04). Similarly to the RMSE results, the estimated MAD calculated from the actual results displays a reduction that is very similar to the simulations study results. This reduction in the estimated MAD as the sample size increases is graphically displayed in Figure 33.

Table 43

MAD for Small Sample Size in the Actual Angoff Dataset

Sample Size	Actual Results[a]	Simulation Results[b]			
		Mean	SD	Min	Max
25%	0.08	0.05	0.02	0.01	0.13
33%	0.06	0.04	0.02	0.01	0.11
50%	0.04	0.03	0.01	0.01	0.08
66%	0.03	0.02	0.01	0.01	0.06
75%	0.03	0.02	0.01	0.01	0.04
100%	0.00	0.00	0.00	0.00	0.00

[a] n=1,000 replications at each sample size
[b] n=972 conditions at each sample size (1,000 replications each condition)

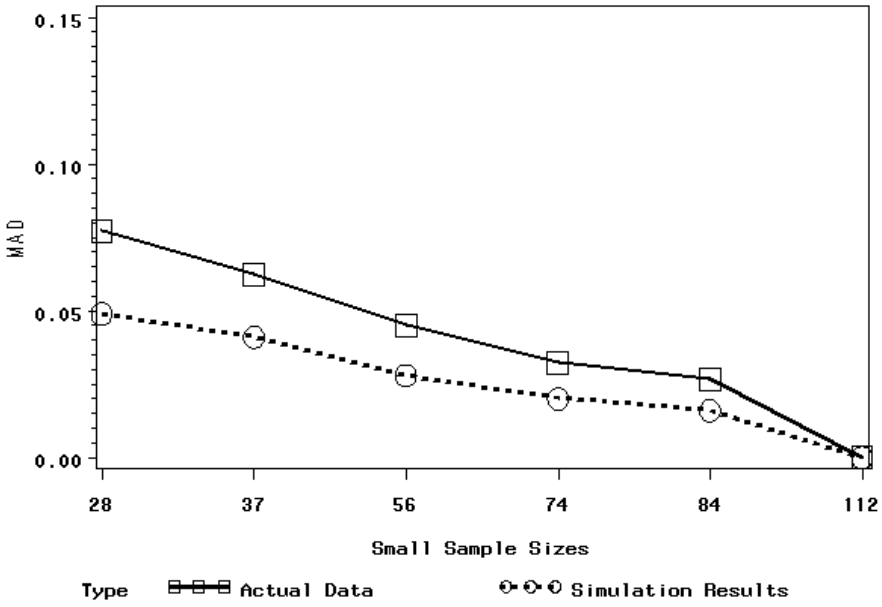


Figure 33. Estimated MAD between the small sample sizes for actual Angoff and simulated datasets.

Results Summary

The results were evaluated individually for each generalizability comparison. The first generalizability comparison evaluated the difference between the small sample performance estimate and the performance estimate derived from the complete 143-item set. The second generalizability comparison evaluated the difference between the small sample performance estimate and the 'true' originating performance estimate. Each generalizability comparison section was evaluated by the study outcome measures (bias, mean absolute deviation, and root mean square error) and the corresponding research questions. The two research questions relate to the extent to which various factors impact the ability to generalize minimal competency estimates. The first research question involved those factors related to the characteristics and the relationship between the two

item sets. The second research question involved those factors related to the characteristics of the standard setting process. Finally, the simulation results were compared to an existing set of 112 Angoff values from an actual standard setting study.

Results were analyzed by computing eta-squared values to estimate the proportion of variability in each of the outcomes (bias, RMSE, and MAD) associated with each factor in the simulation design. Critical factors were identified using eta-squared (η^2) to estimate the proportion of variance associated with each effect. Cohen (1977, 1988) proposed descriptors for interpreting eta-squared values; (a) small effect size: $\eta^2 = .01$; (b) medium effect size: $\eta^2 = .06$, and (c) large effect size: $\eta^2 = .14$. Critical factors were determined as those that had an eta-squared effect size of medium or greater.

Results Summary for Generalizability Comparison I

Table 44 displays the eta-squared medium and large effect sizes for all three outcomes in Generalizability Comparison I. For the bias outcome, the only factor of the seven in Generalizability Comparison I that had a medium or larger eta-squared effect size was the sample size factor from research question 1.

This factor also interacted with the item difficulty distribution factor which resulted in a large effect. The MAD and RMSE outcomes had the same pattern of medium and large eta-squared effects. The medium effects included the item difficulty distribution factor and the location of the originating performance standard factor from research question 1, and the directional influence factor from research question 2.

Table 44

Eta-squared Analysis of the Medium and Large Effect Sizes of the Factors in the Simulation for Generalizability Comparison I

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct		Medium	Medium
Dist		Medium	Medium
SampleN	Large	Large	Large
θ_{mc}		Medium	Medium
SampleN x Dist	Large		

Note. Direct = directional influence, Dist = item difficulty distribution, SampleN = sample size, and θ_{mc}=location of the originating theta

The sample size factor from research question 1 had the only large eta-squared effect size of the study factors. Neither MAD nor RMSE had any interaction effects that were note worthy.

Results Summary for Generalizability Comparison II

Table 45 displays the eta-squared medium and large effect sizes for all three outcomes in Generalizability Comparison II. For the bias outcome, the directional influence factor from research question 2 was the only one of the seven study factors that had a medium or larger eta-squared effect size. The eta-squared effect size for the directional influence factor was large.

The RMSE outcome had medium eta-squared effects for the item difficulty distribution factor and the location of the originating performance standard factor from research question 1. The MAD outcome had a medium eta-squared effect for the item difficulty distribution factor. Both RMSE and MAD had a large eta-squared effect for the directional influence factor from research question 2. RMSE and MAD also had

combinations of two-way and three-way interactions between the item difficulty distribution factor, the location of the originating performance standard factor, and directional influence factor.

Table 45

Eta-squared Analysis of the Medium and Large Effect Sizes of the Factors in the Simulation for Generalizability Comparison II

Outcome	*Bias* η^2	*MAD* η^2	*RMSE* η^2
Direct	Large	Large	Large
Dist		Medium	Medium
θ_{mc}			Medium
Direct x Dist		Medium	
θ_{mc} *x Direct*		Medium	Medium
θ_{mc} *x Direct x Dist*		Medium	Medium

Note. Direct = directional influence, Dist = item difficulty distribution, and θ_{mc}=location of the originating theta

Results Summary for the Actual Angoff Dataset Comparison

Results from an actual Angoff standard setting process were used as a 'pseudo' population. Samples were then drawn using a similar stratified random sampling methodology and comparisons were made to the results of the simulation study. Comparisons were made between an actual 112-item Angoff dataset (provided by S. G. Sireci) and the simulation results. The ability to generalize the performance standard was evaluated using a model similar to that used in the simulation. The sample sizes were based on the sample size factor used in the simulation. To match the characteristics of the simulation design and ensure stable results, one thousand samples were of each sample size. The three outcomes (bias, RMSE, and MAD) were calculated for each sample size across the one thousand samples. The estimated outcome measures

calculated from the actual results all fell when in the range from the simulation study. The outcome measures from the actual results also displayed similar reductions to the simulation study as the samples increased in size.

Chapter Five:

Conclusions

Summary of the Study

While each phase of the test development process is crucial to the validity of the examination, one phase tends to stand out among the others; the standard setting process. It has continually received the most attention in the literature among any of the technical issues related to criterion-referenced measurement (Berk, 1986). Little research attention, however, has been given to generalizing the resulting performance standards. In essence, can the estimate of minimal competency that is established with one subset of multiple choice items be applied to the larger set of items from which it was derived? The ability to generalize performance standards has profound implications both from a psychometric as well as a practicality standpoint.

The standard setting process is a time-consuming and expensive endeavor. It requires the involvement of number of professionals both in the context of participants such as subject matter experts (SME) as well as those involved in the test development process such as psychometricians and workshop facilitators. The standard setting process can also be cognitively taxing on participants (Lewis et al., 1998). Generalizing performance standards may improve the quality of the standard setting process. By reducing the number of items that a rater needs to review, the quality of their ratings might improve as the raters are “less fatigued” and have “more time” to review the

smaller dataset (Ferdous & Plake, 2005, p. 186). Reducing the time it takes to conduct the process also translates into a savings of time and money for the presenting agency as well as the raters, who are generally practitioners in the profession.

While IRT-based models such as the Bookmark and other variations have been created to addresses some of these the deficiencies, research suggests that these newer IRT-based methods have inadvertently introduced other flaws. In a multimethod study of standard setting methodologies by Buckendahl et al. (2000), the Bookmark standard setting method did not produce levels of confidence and comfort with the process that were very different than the popular Angoff method. Reckase (2006a) conducted a simulation study of standard setting processes using Angoff and Bookmark methods which attempted to recover the originating performance standard in the simulation model. He found that error-free conditions during the first round of Bookmark cut scores were statistically lower than the simulated cut scores (Reckase, 2006a). The Bookmark estimates of the performance standard from his research study were 'uniformly negatively statistically biased' (Reckase, 2006a, p. 14). These results are consistent with other Bookmark research (Green et al., 2003; Yin & Schulz, 2005). While the IRT-based standard setting methods do use a common scale, they all have a potential issue with reliability. Raters are only given one opportunity per round to determine an estimate of minimal competency as they select a single place between items rather than setting performance estimates for each individual item as in the case of the Angoff method.

Setting a performance standard with the Angoff method on a smaller sample of items and accurately applying it to the larger test form may address some of these

standard setting issues (e.g., cognitively taxing process, high expense, time consuming). In fact, it may improve the standard setting process by limiting the number of items and the individual rater decisions. It also has the potential to save time and money as fewer individual items would be used in the process.

The primary purpose of this research was to evaluate the extent to which a single minimal competency estimate derived from a subset of multiple choice items would generalize to the larger item set. There were two primary goals for this research endeavor: (1) evaluating the degree to which the characteristics of the two item sets and their relationship impact the ability to generalize minimal competency estimates, and (2) evaluating the degree to which the characteristics of the standard setting process impact the ability to generalize minimal competency estimates.

First, the characteristics and the relationship between the two item sets were evaluated in terms of their effect on generalizability. This included the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard, and the number of items randomly drawn from the larger item set. Second, the characteristics of the standard setting process were evaluated in terms of their effect on generalizability: specifically, elements such as the number of raters, the 'unreliability' of individual raters in terms of the percentage of unreliable raters and their magnitude of 'unreliability', and the influence of group dynamics and discussion.

Individual item-level estimates of minimal competency were simulated using a Monte Carlo approach. This type of approach allowed the control and manipulation of research design factors. Every simulation study begins with various decision points.

These decision points represent the researcher's attempt to ground the simulation process in current theory and provide a foundation for the creation of 'real life' data and results that can be correctly generalized to specific populations. The initial decision points involved in this simulation are the type of standard setting method, the type of IRT model, and the number of items evaluated. The Angoff method was selected over the Bookmark method as the standard setting method for this study due to its popularity of use (Ferdous & Plake, 2005), stronger ability to replicate the performance standard (Reckase, 2006a), and greater amount of general research as well as research on the ability to generalize performance standards. The IRT method selected was based on the characteristics of the items. Multiple choice items were used and the three-parameter IRT model which incorporates a pseudo guessing parameter was the most appropriate IRT model for this type of item. The decision to use a large number of items for the larger item set was based on the research questions. There would be less economic value in dividing a smaller number of items into even smaller samples.

The simulation took place in two distinct steps: data generation and data analysis. The data generation step consisted of simulating the standard setting participant's individual estimates of minimal competency and calculating the resulting item-level estimates of minimal competency. The second step or data analysis step of the simulation process consisted of forming a smaller item set by drawing a stratified random sample from the larger item set. The resulting performance standard established with this smaller item set was then compared to the performance standard from the larger item set as well as the 'true' performance standard used to originally simulate the data. The Monte Carlo

study involved seven factors. The simulation factors were separated into two areas: those related to the characteristics and relationship between the item sets, and those related to the standard setting process. The characteristics and the relationship between the two item sets included three factors: (a) the item difficulty distributions in the larger 143-item set ('real' item distribution, simulated SAT item distribution, simulated SAT item distribution with reduced variance, and simulated uniform difficulty), (b) location of the 'true' performance standard (θ_{mc} = -1.0, 0, 1.0), (c) number of items randomly drawn in the sample (36, 47, 72, 94, 107, and the full item set). The characteristics of the standard setting process included four factors: (a) number of raters (8, 12, 16), (b) percentage of unreliable raters (25%, 50%, 75%), (c) magnitude of 'unreliability' in unreliable raters (ρ_{XX} = .65, .75, .85.), and (d) and the directional influence of group dynamics and discussion (lowest rater, highest rater, average rater).

The ability to 'adequately' generalize the performance was evaluated in terms of the differences between the performance standard derived with the larger item set and the performance standard derived with the smaller subset of multiple choice items. The difference between the originating performance standard and the performance standard derived with the smaller subset of items was also examined. The aggregated simulation results were evaluated in terms of the location (bias) and the variability (mean absolute deviation, root mean square error) in the estimates. The examining proportion of variance associated with each effect (η^2) was evaluated using Cohen's medium effect size criteria, $\eta^2 = 0.06$.

Research Questions

1. To what extent do the characteristics and the relationship between the two item sets impact the ability to generalize minimal competency estimates?
    a. To what extent does the distribution of item difficulties in the larger item set influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the placement of the 'true' performance standard influence the ability to generalize the estimate of minimal competency?
    c. To what extent does the number of items drawn from the larger item set influence the ability to generalize the estimate of minimal competency?
2. To what extent do the characteristics of the standard setting process impact the ability to generalize minimal competency estimates?
    a. To what extent does the number of raters in the standard setting process influence the ability to generalize the estimate of minimal competency?
    b. To what extent does the percentage of 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    c. To what extent does the magnitude of 'unreliability' in the designated 'unreliable' raters influence the ability to generalize the estimate of minimal competency?
    d. To what extent do group dynamics and discussion during the later rounds of the standard setting process influence the ability to generalize the estimate of minimal competency?

Summary of Results

Generalizability Comparison I

For the bias outcome, the only factor of the seven in Generalizability Comparison I that had a medium or larger eta-squared effect size was the sample size factor from research question 1. This factor also interacted with the item difficulty distribution factor which resulted in a large effect. The MAD and RMSE outcomes had the same pattern of medium and large eta-squared effects. The medium effects included the item difficulty distribution factor and the location of the originating performance standard factor from research question 1, and the directional influence factor from research question 2. The sample size factor from research question 1 had the only large eta-squared effect size of the study factors. Neither MAD nor RMSE had any interaction effects that were note worthy.

Generalizability Comparison II

The directional influence factor from research question 2 was the only one of the seven study factors that had a medium or larger eta-squared effect size. The eta-squared effect size for the directional influence factor was large. The RMSE outcome had medium eta-squared effects for the item difficulty distribution factor and the location of the originating performance standard factor from research question 1. The MAD outcome had a medium eta-squared effect for the item difficulty distribution factor. Both RMSE and MAD had a large eta-squared effect for the directional influence factor from research question 2. RMSE and MAD also had combinations of two-way and three-way interactions between the item difficulty distribution factor, the location of the originating

performance standard factor, and directional influence factor.

Actual Angoff Dataset Comparison

Results from an actual Angoff standard setting process were used as a 'pseudo' population. Samples were then drawn using a similar stratified random sampling methodology and comparisons were made to the results of the simulation study. Comparisons were made between the minimal competency estimates derived from the simulation results and those derived from an actual 112-item Angoff dataset (provided by S. G. Sireci). The ability to generalize the performance standard was evaluated using a model similar to that used in the simulation. The sample sizes were based on the sample size factor used in the simulation. To match the characteristics of the simulation design and ensure stable results, one thousand samples were taken from each sample size. The three outcomes (bias, RMSE, and MAD) were calculated for each sample size across the one thousand samples. The outcome measures calculated from the actual results were all within the range of the simulation study results. The outcome measures from the actual results also displayed similar reductions in variance as the sample size increased.

Discussion

Previous research studies related to using subsets of items to set performance standards have only been conducted on existing Angoff datasets. Little or no previous research exists evaluating the extent to which various standard setting factors impact the generalizability of performance standards. This simulation study sought to explore these various factors within the standard setting process and their impact on generalizability.

Two different generalizability comparisons were made as a result of the study. The first generalizability comparison evaluated the difference between the small sample performance estimate and the performance estimate derived from the complete 143-item set. The second generalizability comparison evaluated the difference between the small sample performance estimate and the 'true' originating performance estimate. Because of the uniqueness of each of the generalizability comparisons, each will be discussed separately as they relate to the research questions and their associated study factors and then differences will be compared at the end of the section.

Generalizability Comparison I

Three factors were associated with the characteristics and the relationship between the two item sets as stated in research question 1. All three factors were postulated to impact the ability to generalize minimal competency estimates between the small sample performance estimate and the performance estimate derived from the complete 143-item set. These three factors were the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard, and the number of items drawn from the larger item set.

It was hypothesized that item difficulty distributions with a smaller variance in item difficulty parameters will generalize better than item difficulty distributions with a larger variance. The study results suggest that there is some value to this hypothesis. While little bias was present in the item difficulty factor of the simulation study, the variability in theta estimates (RMSE and MAD) was very noticeable. The mean RMSE (0.025) was the smallest in the simulated SAT Low item difficulty distribution (lower

variance in item difficulty parameters). This item difficulty distribution also had the lowest variability in item difficulty parameters (SD = 0.70). This suggests that the tighter the item difficulty distribution, the better the generalizability of performance estimates. Conversely, the item difficulty distribution with the largest variability in difficulty parameters, the simulated SAT uniform distribution (SD = 1.69), had the highest mean RMSE (0.044) of the four item difficulty distributions.

In terms of location of the 'true' performance standard, it was suggested that a 'true' performance standard which is closer to the center of the item difficulty distribution will generalize better than a placement further away. The simulations study results also suggest that this hypothesis has some merit. While little bias was present in the location of the 'true' performance factor, the variability (RMSE and MAD) in theta estimates was very noticeable. Of the three originating theta values, an originating theta value of 1 had the lowest mean RMSE (0.027) and lowest range of RMSE values (0.111) of the three originating theta values. The mean item difficulty parameters (*b*-parameter) for the four item difficulty distributions were -0.01(Simulated SAT Low), -0.07 (Simulated SAT), 0.09 (Simulated SAT Uniform), and 0.44 (Real Item). While a strong interaction between the originating theta factor and the item difficulty distribution factor was not present, this could explain why an originating theta of 1 had a lower mean RMSE than an originating theta of -1. An originating theta of 0 had the second lowest mean RMSE (0.031) of the three originating theta values.

It was also suggested that the larger the number of sample items drawn from the 143-item sample the better the generalizability of the estimate of minimal competency. This was true in the study both in terms of the bias and the variability (RMSE and MAD)

in theta estimates. In fact, this factor had the largest outcome measure effect sizes of the seven study factors in Generalizability Comparison I. The results of the simulation study suggest that the larger the sample size the less bias and variability of theta estimates. This result is consistent with the current literature (Coraggio, 2007; Ferdous & Plake, 2005, 2007; Sireci et al., 2000). The number of sample items factor also interacted with the item difficulty distribution factor. The ability to generalize performance estimates increased as the sample size increased, but not at the same rate for all four item difficulty distributions. The item difficulty distribution that was impacted the most was the SAT Uniform distribution which interestingly also had the most variability (SD = 1.69) in item difficulty parameters (*b*-parameters).

Four factors were associated with the characteristics of the standard setting process as stated in research question 2. All four factors were postulated to impact the ability to generalize minimal competency estimates between the small sample performance estimate and the performance estimate derived from the complete 143-item set. These four factors were the number of raters, the percentage of 'unreliable' raters, magnitude of 'unreliability' in the designated 'unreliable' raters, and the group dynamics and discussion during the second round of the standard setting process.

It was hypothesized that the larger the numbers of raters in the standard setting process the better the generalizability of the estimate of minimal competency. This was based on the literature suggesting that at least 10 and ideally 15 to 20 should participate (Brandon, 2004). The three levels in this study were selected to be representative and at the same time economical based on the nature of the research topic. The number of raters

factor in the study did not produce any notable results in terms of the bias and variability (RMSE and MAD) of theta estimates.

It was also suggested that the consistency of raters and magnitude of consistency would impact the generalizability of the performance estimates. While this is also suggested in the literature (Schultz, 2006; Shepard, 1995), the results of this study did not support this hypothesis. None of these rater related factors produced notable results in terms of the bias and variability (theta) of theta estimates. This included the percentage of 'unreliable' raters and the magnitude of unreliability in the fallible raters.

The group dynamics and discussion during the second round of the standard setting process did produce noticeable results in the study. It was suggested that group dynamics and discussion that influence the raters towards the center of the rating distribution would generalize better than group dynamics and discussion that influence the raters towards the outside of the rating distribution. Fitzpatrick (1989) had suggested that a group polarization effect that occurs during the discussion phase of the Angoff workshop and Livingston (1995) had reported that this effect was towards the mean rating. The results of this study suggest that the directional influence towards the highest rater had best generalizability of theta estimates. The directional influence towards the highest rater had the lowest mean RMSE (0.027) and lowest range of RMSE values (0.109). While it was hypothesized that the directional influence towards the average rater would have the best generalizability of theta estimates, the reason for the slight advantage in the directional results towards the highest rater is not immediately apparent. Further research on the issue of the impact of directional influence should be conducted

to investigate this outcome. The directional influence towards the average rater was a very close second with a mean RMSE of 0.032.

Generalizability Comparison II

Three factors were associated with the characteristics and the relationship between the two item sets as stated in research question 1. All three factors were postulated to impact the ability to generalize minimal competency estimates between the small sample performance estimate and the 'true' originating performance estimate. These three factors were the distribution of item difficulties in the larger item set, the placement of the 'true' performance standard, and the number of items drawn from the larger item set.

It was hypothesized that item difficulty distributions with a smaller variance in item difficulty parameters will generalize better than item difficulty distributions with a larger variance. The study results suggest that this hypothesis may be accurate for Generalizability Comparison II as well as Generalizability Comparison I. While little bias was present in the item difficulty factor of the simulation study, the variability (RMSE and MAD) in theta estimates was very noticeable. The mean RMSE was the smallest (0.33) in the simulated SAT with low item difficulty variance. This item difficulty distribution also had the lowest variability in item difficulty parameters (SD = 0.70). Conversely, the item difficulty distribution with the largest variability in difficulty parameters, the SAT Uniform distribution (SD = 1.69), had the highest mean RMSE (0.48). The item difficulty distribution also had two note-worthy interactions, one with the directional influence factor, and one with the directional influence factor and the

originating theta factor. In both cases, the SAT Uniform distribution displayed less generalizability of performance estimates than the other four item difficulty distributions.

In terms of location of the 'true' performance standard, it was also suggested that a placement of the 'true' performance standard closer to the center of the item difficulty distribution will generalize better than a placement further away. The simulation study results also suggest that this hypothesis has some merit. While little bias was present in the location of the 'true' performance factor of the simulation study, the variability (RMSE) in theta estimates was very noticeable. Of the three originating theta values, an originating theta value of 0 had the lowest mean RMSE (0.36) and lowest standard deviation of RMSE (0.19) of the three originating theta values. As mentioned earlier, there was an interaction between the originating theta factor and the item difficulty distribution factor. The mean item difficulty parameters (*b*-parameter) for the four item difficulty distributions were -0.01(Simulated SAT Low), -0.07 (Simulated SAT), 0.09 (Simulated SAT Uniform), and 0.44 (Real Item). All four item difficulty distributions center around an originating theta of 0 with a slight skewness towards an originating theta of 1. An originating theta of 1 had the second lowest mean RMSE (0.39) with a standard deviation of RMSE (0.23).

Regarding the number of items drawn, it was hypothesized that the larger the number of items drawn the better the generalizability of the estimate of minimal competency. The results of this study did not support this hypothesis in Generalizability Comparison II. The sample size factor did not produce any notable results in terms of the bias and the variability (RMSE and MAD) of theta estimates. This factor was very

noteworthy in Generalizability Comparison I for all three outcome measures, when comparing the generalizability between the small sample and the full 143-item set. However, this generalizability comparison was between the small sample and the originating or 'true' theta. Little research exists on the concept of a true' theta and researchers are only able to determine the 'true' originating theta in simulation studies. Some researchers even argue the existence of a 'true' originating theta (Schultz, 2006; Wang et al., 2003). One possible reason for this difference in results between the generalizability comparisons is that other factors (such as the directional influence factor) may have accounted for such large shares of the explained variance in the outcome measures that they essentially drown out the impact of the sample size factor in Generalizability Comparison II.

Four factors were associated with the characteristics of the standard setting process as stated in research question 2. All four factors were postulated to impact the ability to generalize minimal competency estimates between the small sample performance estimate and the 'true' originating performance estimate. These four factors were the number of raters, the percentage of 'unreliable' raters, the magnitude of 'unreliability' in the designated 'unreliable' raters, and the group dynamics and discussion during the second round of the standard setting process.

It was hypothesized that the larger the numbers of raters in the standard setting process the better the generalizability of the estimate of minimal competency. While the literature suggested minimum and recommended levels of the number of raters (Brandon, 2004), the three levels used in this study (8, 12, and 16) did not produce any notable

results in terms of the bias and variability (RMSE and MAD) of theta estimates for Generalizability Comparison II. It was also hypothesized that the consistency of raters and magnitude of consistency would impact the generalizability of the performance estimates. While this is also suggested in the literature (Schultz, 2006; Shepard, 1995), the results of this study did not support this hypothesis for Generalizability Comparison II. As with the results of the first generalizability comparison, none of the rater related factors produced notable results in terms of the bias and variability (RMSE and MAD) of theta estimates.

It was also suggested that group dynamics and discussion that influence the raters towards the center of the rating distribution would generalize better than group dynamics and discussion that influence the raters towards the outside of the rating distribution. The simulation study results suggest that this is an accurate hypothesis as the directional influence towards the average rater had the lowest mean bias (-0.07) and mean RMSE (0.19). This is consistent with the rater regression to the mean effect discussed in the literature (Livingston, 1995). Directional influence towards the lowest rater was negatively bias (-0.57), while directional influence towards the highest rater was positively bias (0.35). This result was different than the result for the other generalizability comparison. This factor had the largest outcome measure effect sizes of the seven study factors in Generalizability Comparison II.

Limitations

Based on the design of the study, there are a number of limitations to consider in relation to this research study. The simulation method implemented in this study provides

control of a number of factors intended to investigate performance in specific situations. This benefit of control in simulation studies is also a limitation as it tends to limit the generalizability of the study findings. Thus, the seven controlled factors (a) the item difficulty distributions, (b) location of the 'true' performance standard, (c) number of items randomly drawn in the sample, (d) number of raters, (e) percentage of unreliable raters, (f) magnitude of 'unreliability' in unreliable raters, and (g) directional influence of group dynamics and discussion dictate the types of standard setting environments to which the study results can be generalized. Another inherent limitation of the simulation study is the number of levels within each factor. These levels were selected to provide a sense of the impact of each factor. They were not intended to be an exhaustive representation of all the possible levels within each factor.

Another restriction on the ability to generalize the study results is related to the study's initial decision points. While the researcher attempted to ground the simulation process in current theory and provide a foundation for the creation of 'real life' data in order to generalize to specific populations, the initial decision points also provided limitations. For example, the Angoff method was selected as the standard setting model. The use of other models such as the Bookmark method may produce very different results. The other two decisions points of IRT method (three-parameter) and larger item sample size (143 items) also provide similar limitations on the generalizability of study results.

The final consideration of limited generalizability is the level of rater subjectivity involved in the standard setting process. While this study has contained a number of

factors to simulate the standard setting process, additional factors affecting the subjectiveness of individual raters such as content biases, knowledge of minimal competency, and fatigue may also play a role in determining the final passing standard. These issues would likely affect the other raters in the standard setting process as well.

Implications

Implications for Standard Setting Practice

The intent of this research was to evaluate the model of setting performance standards with partial items sets. This line of research has important implications for standard setting practice as using a subset of multiple choice items to set the passing standard has the potential to save time and money as well as improve the quality of the standard setting process. This could be accomplished through limiting the number of items and the number of individual rater decisions required for the process. The quality of individual ratings might also improve as the raters are "less fatigued" and have "more time" to review the items (Ferdous & Plake, 2005, p. 186). Financial savings could be redirected to improving other areas of the test development process such as validation.

This simulation research made two comparisons of generalizability. The first addressed the differences between the performance standard derived with the larger item set and the performance standard derived with the smaller subset of multiple choice items. This first comparison has implications that are directly apparent for practitioners as they generally start with the larger set of items from which to subset. The implications for the second comparison may not be as immediately apparent, but may be just as important. It was the difference between the 'true' originating performance standard and

the performance standard derived with the smaller subset of multiple choice items. The 'true' performance standard is never known in practice and some researchers have even questioned its existence (Schultz, 2006; Wang et al., 2003). It was simulated as a factor in this study and has direct implications in terms of the ability of a standard setting model to reproduce the intended standard (Reckase, 2006a, 2006b).

The simulation results suggest that the model of using partial item sets may have some merit for practitioners as the resulting performance standard estimates may generalize to those set with the larger item set. The results for the comparison between the large and small item sets indicate large effect sizes (η^2) for the sample size factor both in terms of bias and variability (RMSE and MAD). The results also suggest that sample sizes between 50% and 66% of the larger item set may be adequate. The estimated mean bias for the sample size of 50% was 0.001 (SD=0.004) with an RMSE of 0.04 (SD=0.02), while the mean bias for the sample size of 66% was less than 0.000 (SD=0.002) with an RMSE of 0.03 (SD=0.01). This finding is consistent with non-simulated research (Ferdous & Plake, 2005, 2007; Sireci et al., 2000). Interestingly enough the second generalizability comparison, which evaluated the difference between the small sample performance estimate and the 'true' originating performance estimate, did not produce any note worthy results in the outcome measures. This suggests that the smallest sample and the largest sample generalized to the 'true' originating performance standard equally as well. These results do not seem very intuitive and may require additional research.

This simulation study by design has explored the conditions that may impact the generalizability of performance standards. Previous research studies related to using

subsets of items to set performance standards have only been conducted on existing datasets. This simulation study sought to explore various factors within the standard setting process and their specific impact on generalizability. This included characteristics related to the item sets as well as those related to the standard setting process. In fact, the simulation results suggest that some elements of the process should be carefully considered before attempting to set standards with subsets of items. Elements such as the type of the item difficulty distribution in the larger item set (or original test form); the direction of the group influence during the group discussion phase; and the location of the 'true' performance standard may adversely impact generalizability.

The simulation results suggest that the item difficulty distribution can impact the ability to generalize performance standards. The simulation study results suggest that item difficulty distributions with a tighter variance such as those created for certification and licensure examinations have better generalizability of performance standards. A test of this nature would be designed to measure a more narrow range of abilities. Ideally, an examination or bank of items for mastery testing would consist of items with item difficulty parameters around the performance standard (Embretson & Reise, 2000). This would provide a maximum amount of information (or conversely a low standard error) around the performance standard. While the specific issues of computer adaptive testing (CAT) are outside the realm of this paper (see van der Linden & Glas, 2000 for more detail), different item selection, scoring (i.e., ML, MAP, EAP), and termination procedures may require a wider range of item difficulty parameters than reflected by the SAT simulated item difficulty distribution with low variance used in this study. This item

difficulty distribution factor had medium effect sizes (η^2) in terms of variability (RMSE and MAD) for both generalizability comparisons. This factor also interacted with the sample size factor in Generalizability Comparison I as well as the directional influence factor and the location of the originating performance standard in Generalizability Comparison II.

The simulation results also suggest that directional influence by raters during the discussion round can impact the ability to generalize performance standards. This result is consistent with current research. Some researchers have suggested a group-influenced biasing effect of regression to the mean (Livingston, 1995) during group discussion. Other researchers have suggested a group polarization effect (Fitzpatrick, 1989) in which a moderate group position becomes more extreme in that same direction after group interaction and discussion (Myers & Lamm, 1976). Group discussion has resulted in lower rating variability, and this lower variability has been traditionally used by practitioners as one measure of standard setting quality. Lower variability, however, may not guarantee valid results (McGinty, 2005). One question that has been periodically explored in the literature is the need for the discussion round in the standard setting process. The impact of the directional influence towards the lowest and highest raters in Generalizability Comparison II suggests the need to revisit this question. This directional influence factor had medium effect sizes (η^2) in terms of variability (RMSE and MAD) for Generalizability Comparison I and large effect sizes (η^2) in terms of bias and variability (RMSE and MAD) for Generalizability Comparison II. The results suggest that directional influence while being an important consideration in terms of generalizing

across item sets may have an even bigger implication in terms of the ability of the standard setting process to replicate the intended originating performance standard. This factor also interacted with the item difficulty distribution factor and the location of the originating performance standard factor in Generalizability Comparison II.

In addition to the item difficulty distribution and directional influence factors, the simulation results suggest that the location of the originating performance standard factor may also impact the ability to generalize performance standards. The simulation study results suggest that a 'true' originating performance standard which is closer to the center of the item difficulty distribution will generalize better than a placement which is further away. This factor had medium effect sizes (η^2) in terms of variability (RMSE and/or MAD) for both generalizability comparisons. As previously mentioned, this factor interacted with the item difficulty distribution factor and the directional influence factor in Generalizability Comparison II.

This issue of a 'true' performance standard is controversial among standard setting researchers. One way to operationalize this concept in terms of standard setting practice is analogize it to a 'true' score in test theory. Normally, one would assume that the location of the 'true' performance standard for a given program would not change over time just as 'true' score would not change in test theory. A practitioner could carefully consider the location of the performance standards from previous standard settings. The average of these previous performance standards could then be considered a 'true' performance standard and taken into consideration when creating new test forms and conducting future standard settings.

An interesting outcome of this study was the lack of noteworthy results regarding the number and fallibility of standard setting participants. The number of raters within the standard setting process did not seem significant in terms of impacting the generalizability of the performance standard. Perhaps there is some validity to Livingston and Zieky's (1982) suggestion that as few as five participants may be adequate to set performance standards. The study results also suggest that truly random rater error has little impact on the ability to generalize performance standards at least in terms of the levels used within this simulation study. The issue of non-random rater error was not as extensively explored in this study with the exception of the factor related to directional influence during the discussion phase of the standard setting process.

While the findings of this study are consistent with other non-simulated generalizability research (Ferdous & Plake, 2005, 2007; Sireci et al., 2000), questions of policy must be explored before implementing this partial item set standard setting model in a 'high-stakes' testing environment. There have been few partial item set strategies used operationally (see NAGB, 1994 for example). Questions regarding the 'fairness' of setting performance standards with only partial item sets have been raised by other researchers (Ferdous & Plake, 2007). Hambleton suggested that performance standards set with only partial item sets would never be acceptable under today's environment of increased accountability (R. Hambleton, NCME session, April 10, 2007). Other 'high stake' examination models have been established using partial item sets such as computer adaptive testing (CAT) in which examinees are only presented partial item sets before a determination of competency. CAT models have withstood judicial legislation. CAT

assessment models gained 'acceptance' after several decades of research (Embretson & Reise, 2000). It is hoped that this study will contribute to the current limited body of research study on setting performance standards with partial item sets.

Suggestions for Future Research

Future research should be conducted with additional combinations of raters with different levels of fallibility to see if these rater-related results are consistent across studies. Another suggestion for future research is to conduct standard setting research with other item difficulty models such as items calibrated with different IRT models (one-parameter, two-parameter, etc.) and p-value models. It would be interesting to see if these other models produced comparable results. Clearly, the very use of IRT is a limitation as IRT models require substantial quantities of examinee responses in order to calibrate items. Many smaller testing programs do not have a sufficient test incident level (responses) required for IRT.

Further research on different types of item difficulty distributions would also be of interest. Clearly, while there were some differences in the mean and standard deviation of the *b*-parameter distributions used in this study, the slight differences impacted the results of the study. In addition, it would be interesting to further investigate the impact of directional rater bias. This study evaluated systematic directional influence towards another rater. Other directional bias error models should be considered. Such as models that allow an individual rater to be randomly influenced. For example, influenced towards the 'highest' rater on one item and then influenced towards the 'lowest' rater on another item. It might also be interesting to evaluate the impact of a single or group of

raters that had a predetermined preference towards making the final performance standard either high or low. Lastly, it would be interesting to conduct similar studies with other types of standard setting methods such as the Bookmark method to see if they would produce comparable results.

Conclusions Summary

The primary purpose of this research was to evaluate the extent to which a single minimal competency estimate derived from a subset of multiple choice items would be generalizable to the larger item set. The limited research on the subject of generalizability of performance standards has concentrated on evaluating existing datasets. This study sought to add to the current body of research on the subject in two ways: 1) by examining the issue through the use of simulation and 2) by examining factors within the standard setting process that may impact the ability to generalize performance standards.

The simulation results suggest that the model of setting performance standards with partial item sets may have some merit as the resulting performance standard estimates may generalize to those set with larger item sets. This finding was consistent with the other non-simulated research (Ferdous & Plake, 2005, 2007; Sireci et al., 2000). The simulation results also suggest that elements such as the item difficulty distribution in the larger item set (or original test form) and the impact of directional group influence during the group discussion phase of the process can impact generalizability. For example, item difficulty distributions with a tighter variance and directional influence during the discussion phase that was towards the average rater had the most favorable results though there was often an interaction with the location of the originating

performance standard.

The simulation method implemented in this study provided control of a number of factors intended to investigate performance in specific situations. However, this benefit of control in simulation studies can also be a limitation. The seven controlled factors and their associated levels dictate the types of standard setting environments to which the study results can be generalized. The study's initial decision points selected as an attempt to ground the simulation process in current theory also created limitations. The results of this study can only be generalized to similar environments (Angoff standard setting method, larger item sample sizes, and three-parameter IRT models). The final consideration of limited generalizability is the level of rater subjectivity involved in the standard setting process. While this study has contained a number of factors to simulate the standard setting process, additional factors affecting the subjectiveness of individual raters such as content biases, knowledge of minimal competency, and fatigue may also play a role in determining the final passing standard.

The number and fallibility of standard setting participants in this study had little impact in terms of generalizability of performance standards. Future research should be conducted with additional combinations of raters and different levels of fallibility to see if these results are consistent across studies. Future research should also be conducted with other item difficulty models such as items calibrated with other IRT models (one-parameter, two-parameter, etc.) and p-value models. This study evaluated directional influence towards another rater. It might be interesting to evaluate the impact of a single or group of raters that had a predetermined preference towards making the final

performance standard either high or low. Lastly, it would be interesting to conduct similar studies with other types of standard setting methods (e.g., Bookmark method).

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Appendices

Appendix A: Deriving the Individual Item Performance Estimates

Individual item performance standard estimates ($\hat{\theta}_{mc_{ij}}$) are established by transforming the probability of a correct response to the "log-odds". The transformation (see Hambleton & Swaminathan, p. 57-60; Hambleton, Swaminathan, & Rodgers, p. 83) begins with

$$P(\theta) = c_i + (1 - c_i)\{1 + \exp[-D a_i(\theta - b_i)]\}^{-1}$$

And

$$\frac{P_i(\theta) - c_i}{Q_i(\theta)} = \exp\left[Da_i(\theta - b_i)\right]$$

Where,

a_i is the discrimination parameter,

b_i is the difficulty parameter,

c_i is the pseudo-guessing parameter,

D is 1.702, and

θ is the minimal competency theta estimate.

The minimal competency estimate is then derived by using the natural logarithm (to the base e = 2.718).

$$\ln\left[\frac{P_i(\theta) - c_i}{Q_i(\theta)}\right] = Da_i\left(\theta - b_i\right)$$

$$\ln\left[\frac{P_i(\theta) - c_i}{Q_i(\theta)}\right] = Da_i\theta - Da_ib_i$$

$$\ln\left[\frac{P_i(\theta)-c_i}{Q_i(\theta)}\right]+Da_ib_i=Da_i\theta$$

$$\frac{\ln\left[\frac{P_i(\theta)-c_i}{Q_i(\theta)}\right]+Da_ib_i}{Da_i}=\theta$$

The mean Angoff value for a given item ($\bar{\gamma}_i$) is then substituted for $P_i(\theta)$ and $Q_i(\theta)$ is converted to 1- $\bar{\gamma}_i$ as shown. The resulting equation as proposed by Coraggio (2005) for the theta-cut using the three parameter IRT model is as shown.

$$\hat{\theta}_{mc_{ij}}=\frac{\ln\left(\frac{\bar{\gamma}_i-c_i}{1-\bar{\gamma}_i}\right)+Da_ib_i}{Da_i}$$

Appendix B: Example SAS code

```
options pageno=1;
libname SS_SIM
'C:\Users\jcoraggio\Documents\Classes\Dissertation\SAS';

*++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+
Simulation program for Angoff Standard Setting
Created by JTC
++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++++
+++;

*Clear Old dataset;
proc datasets nolist; delete Phase4_Stand_Rep;
*options mprint;

/*****************************************************************
****
IRT Real Item Parameters from Mark Reckase's EMIP Paper
[With A/1.702 adjustment]
*****************************************************************
***/
data IRT;
Input bank Name$ A B C;


datalines ;
1     item1     0.743    -0.299    0.186
2     item2     1.224    0.816     0.000
3     item3     0.617    0.376     0.255
4     item4     0.643    -0.003    0.244
5     item5     0.857    0.267     0.191
6     item6     0.934    1.077     0.127
7     item7     0.788    -0.149    0.000
8     item8     0.518    0.241     0.213
9     item9     0.894    0.939     0.127
10    item10    1.154    0.977     0.216
11    item11    0.416    -1.232    0.217
12    item12    0.459    0.352     0.180
13    item13    0.462    -2.597    0.201
14    item14    0.495    -1.470    0.164
15    item15    0.669    -1.047    0.186
16    item16    0.457    -0.063    0.238
17    item17    0.222    1.199     0.248
18    item18    0.652    -1.119    0.207
19    item19    0.304    -0.096    0.000
20    item20    0.335    -0.045    0.270
21    item21    0.699    -0.469    0.194
22    item22    0.853    0.777     0.215
23    item23    0.635    -0.069    0.246
24    item24    1.077    0.914     0.000
25    item25    0.799    0.556     0.202
26    item26    0.435    0.594     0.187
27    item27    0.713    0.427     0.133
```

28	item28	1.118	0.868	0.216
29	item29	0.537	-0.973	0.197
30	item30	0.981	0.269	0.258
31	item31	0.897	-0.803	0.251
32	item32	0.632	-1.151	0.237
33	item33	0.512	-0.129	0.000
34	item34	1.071	1.404	0.182
35	item35	0.541	0.350	0.157
36	item36	0.381	-0.573	0.256
37	item37	0.438	-0.033	0.158
38	item38	0.574	-0.695	0.240
39	item39	1.020	1.993	0.291
40	item40	0.489	1.842	0.262
41	item41	0.900	0.699	0.303
42	item42	0.481	0.986	0.294
43	item43	1.035	1.783	0.081
44	item44	0.886	0.241	0.173
45	item45	0.961	1.236	0.000
46	item46	0.655	1.623	0.204
47	item47	0.441	-0.817	0.226
48	item48	0.439	-0.207	0.162
49	item49	0.488	1.023	0.129
50	item50	0.553	-0.251	0.148
51	item51	0.966	1.144	0.287
52	item52	0.353	-0.604	0.191
53	item53	0.410	-1.265	0.000
54	item54	0.406	-0.810	0.000
55	item55	0.900	0.795	0.214
56	item56	0.805	-0.025	0.234
57	item57	1.313	1.639	0.000
58	item58	0.419	-1.691	0.209
59	item59	0.622	-0.195	0.308
60	item60	0.352	-1.130	0.195
61	item61	0.582	0.702	0.126
62	item62	0.867	1.224	0.130
63	item63	0.672	1.642	0.000
64	item64	0.752	-0.573	0.000
65	item65	0.544	-1.264	0.000
66	item66	0.668	2.673	0.047
67	item67	0.468	0.145	0.170
68	item68	0.404	-1.340	0.197
69	item69	1.351	0.654	0.296
70	item70	0.330	0.789	0.277
71	item71	0.527	-0.165	0.160
72	item72	0.685	-0.292	0.178
73	item73	0.346	3.227	0.221
74	item74	0.385	0.256	0.000
75	item75	0.478	-0.220	0.198
76	item76	0.556	0.560	0.169
77	item77	0.407	2.318	0.291
78	item78	0.410	1.485	0.000

```
79    item79    0.808   0.811    0.000
80    item80    0.462   0.264    0.223
81    item81    0.392   -0.320   0.195
82    item82    0.252   1.224    0.202
83    item83    1.058   1.305    0.134
84    item84    0.357   0.079    0.219
85    item85    0.498   3.316    0.110
86    item86    0.108   -3.851   0.211
87    item87    0.645   1.159    0.165
88    item88    0.991   1.488    0.220
89    item89    0.518   -0.072   0.177
90    item90    0.543   0.711    0.239
91    item91    0.937   1.829    0.241
92    item92    0.854   1.588    0.106
93    item93    0.844   0.582    0.271
94    item94    1.004   1.597    0.040
95    item95    0.944   -0.035   0.132
96    item96    1.554   1.416    0.201
97    item97    0.528   0.994    0.172
98    item98    0.462   -0.280   0.141
99    item99    0.518   0.580    0.123
100   item100   0.415   0.115    0.198
101   item101   0.513   0.379    0.151
102   item102   1.272   1.373    0.170
103   item103   0.428   0.674    0.186
104   item104   0.504   -0.250   0.161
105   item105   0.674   2.782    0.000
106   item106   0.752   2.100    0.000
107   item107   0.599   1.231    0.259
108   item108   0.534   0.731    0.000
109   item109   0.765   1.495    0.176
110   item110   1.039   2.000    0.000
111   item111   0.864   0.062    0.000
112   item112   0.585   -0.343   0.000
113   item113   0.730   0.472    0.232
114   item114   0.467   0.013    0.180
115   item115   0.906   0.310    0.234
116   item116   0.810   0.978    0.206
117   item117   0.641   0.597    0.314
118   item118   1.084   0.899    0.089
119   item119   0.675   1.754    0.000
120   item120   1.047   1.854    0.000
121   item121   0.616   -0.241   0.162
122   item122   0.614   0.894    0.111
123   item123   1.694   1.409    0.085
124   item124   0.607   -0.293   0.211
125   item125   0.540   -0.035   0.122
126   item126   0.565   0.226    0.256
127   item127   0.394   0.544    0.169
128   item128   0.600   -0.688   0.210
129   item129   0.602   -1.065   0.184
```

```
130   item130   0.585   -1.253   0.158
131   item131   1.181   1.192    0.278
132   item132   0.667   0.798    0.185
133   item133   0.597   -0.677   0.199
134   item134   0.466   0.301    0.290
135   item135   0.654   1.075    0.193
136   item136   0.567   1.378    0.000
137   item137   0.961   0.966    0.100
138   item138   0.645   1.816    0.233
139   item139   0.664   0.292    0.155
140   item140   0.501   0.703    0.162
141   item141   0.559   0.735    0.250
142   item142   0.814   1.060    0.151
143   item143   1.086   1.465    0.136
 ;

data IRT;
Set IRT;
If A < 0.527 then IRTA = 1;Else IRTA = 2;
If A > 0.743 then IRTA = 3;
If B < -0.033 then IRTB = 10;Else IRTB = 20;
If B > 0.899 then IRTB = 30;

IRT_LEVEL = IRTA+IRTB;

proc sort data = IRT; by B;

/*****************************************************************
****
    Performance Estimate Generation Macro
*****************************************************************
***/

%macro SS_SIM (Rep=1000, Rel=0, Per=10, Theta=1, Direct = 2, Dist =
Real, Cond = 1,RaterN = 12);

proc printto log = log;
run;

%put %sysfunc(datetime(),datetime20.3).....................Rep=&Rep
Rel=&Rel Per=&Per Theta=&Theta Direct=&Direct Dist=&Dist,
Cond=&Cond);

*+-----------------------+
  Turn off the log window
 +-----------------------+;
proc printto log = junk;
run;

*+--------------------------+
```

```
  Turn off the output window
 +-------------------------+;
proc printto print = junk2;
run;

%Let Var1 = 36;
%Let Var2 = 47;
%Let Var3 = 72;
%Let Var4 = 94;
%Let Var5 = 107;
%Let Var6 = 143;
%Let Dim_Var = 6;

/*****************************************************************
****
 Parameters
            Rep = Replications
            Rel = Rater Reliability (XXXXX10.0 approx .90XXXXX, 12.5
                      approx .85, 17.5                      approx
.75, 21.0 approx .65)
            Per = Percentage of Unreliable Raters (25%, 50%, 75%)
            Theta = 'True' Originating theta_mc (-1, 0, 1)
*****************************************************************
***/

%do Rep = 1 %to &Rep;

%do I = 1 %to &Dim_Var.;

data IRT;
set IRT;
number = _n_;
theta_mc = symget('Theta');

e=(EXP(-1.7*A*(theta_mc-B)));
e=round (e,.001);
function = C+SUM((1-C)/(1+e));
Grand_Item = Function;*Grand Mean plus Item Main Effect;
*proc print data=IRT;

/* Datacheck*/
proc means N Mean Std var MIN MAX SKEW KURT data = IRT noprint;
var Grand_Item;
output out = Grand mean = Grand_mean std = Grand_std;

data Grand;
set Grand;
call symput('Grand_mean', Grand_mean);  /* Create global grand_mean
variable */
```

```
/*****************************************************************
****
    ITEM MAIN EFFECT   Phase 1
*****************************************************************
***/

Data Phase1;
set IRT;

Grand_mean = &Grand_mean;
Item_main = Grand_Item - &Grand_mean;
Rater_main = 0;
Rater_Item = 0;
e=0;

Rater1 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater1 GT 100 then Rater1= 100;If Rater1 LT 1
then Rater1= 1;
Rater2 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater2 GT 100 then Rater2= 100;If Rater2 LT 1
then Rater2= 1;
Rater3 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater3 GT 100 then Rater3= 100;If Rater3 LT 1
then Rater3= 1;
Rater4 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater4 GT 100 then Rater4= 100;If Rater4 LT 1
then Rater4= 1;
Rater5 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater5 GT 100 then Rater5= 100;If Rater5 LT 1
then Rater5= 1;
Rater6 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater6 GT 100 then Rater6= 100;If Rater6 LT 1
then Rater6= 1;
Rater7 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater7 GT 100 then Rater7= 100;If Rater7 LT 1
then Rater7= 1;
Rater8 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater8 GT 100 then Rater8= 100;If Rater8 LT 1
then Rater8= 1;
Rater9 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater9 GT 100 then Rater9= 100;If Rater9 LT 1
then Rater9= 1;
Rater10 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater10 GT 100 then Rater10= 100;If Rater10
LT 1 then Rater10= 1;
Rater11 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater11 GT 100 then Rater11= 100;If Rater11
LT 1 then Rater11= 1;
```

```
Rater12 = round ((100*(Grand_Mean + Item_main + Rater_main +
Rater_Item + e)),1);If Rater12 GT 100 then Rater12= 100;If Rater12
LT 1 then Rater12= 1;


Diff = Max(of Rater1-Rater12) - Min(of Rater1-Rater12);
Stdev = round (std(Rater1, Rater2, Rater3, Rater4, Rater5, Rater6,
Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);
Rater_Avg = round (mean(Rater1, Rater2, Rater3, Rater4, Rater5,
Rater6, Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);

* Natural Log Check;
NLogData = ((Rater_Avg*.01)-C)/(1-(Rater_Avg*.01));
If NLogData LE 0 Then NLogData = .01;
Theta_Cal = (log(NLogData)+(1.7*A*B))/(1.7*A);

Phase = 1;

Rep = symget('Rep');
Rel = symget('Rel');
Per = symget('Per');
Direct = symget('Direct');
theta_mc = symget('Theta');

*proc print noobs data=Phase1;
*Var Theta_mc Bank A B C Grand_mean Rater1 Rater2 Rater3 Rater4
Rater5 Rater6 Rater7 Rater8 Rater9 Rater10 Rater11 Rater12 Stdev
Rater_Avg Theta_Cal;

*Create True Datasets to check reliability;
/*
Data True_Check;
set Phase1;
tRater1 = Rater1;
tRater2 = Rater2;
tRater3 = Rater3;
tRater4 = Rater4;
tRater5 = Rater5;
tRater6 = Rater6;
tRater7 = Rater7;
tRater8 = Rater8;
tRater9 = Rater9;
tRater10 = Rater10;
tRater11 = Rater11;
tRater12 = Rater12;
Keep tRater1 tRater2 tRater3 tRater4 tRater5 tRater6 tRater7 tRater8
tRater9 tRater10 tRater11 tRater12;
*/

/* Datacheck*/
```

```
*proc means N Mean Std var MIN MAX SKEW KURT data = Phase1;
*var Rater1 Rater2 Rater3 Rater4 Rater5 Rater6 Rater7 Rater8 Rater9
Theta_cal;

/* Get Phase1 Standard */
proc means data = Phase1 noprint;
 var Rater_Avg Theta_cal;
 output out = Phase1_Stand median = medianT mean = meanT std = stdT;

/* Create Rater Differences*/
data Rater_Effect;
array RE[*] RE1- RE12;
do I = 1 To 12;
RE [I] = 0 + (6.8 * rannor(0));
End;
call symput('RE1', RE1);
call symput('RE2', RE2);
call symput('RE3', RE3);
call symput('RE4', RE4);
call symput('RE5', RE5);
call symput('RE6', RE6);
call symput('RE7', RE7);
call symput('RE8', RE8);
call symput('RE9', RE9);
call symput('RE10', RE10);
call symput('RE11', RE11);
call symput('RE12', RE12);

proc print data = Rater_Effect;
run;

/*****************************************************************
*********************************
    RATER MAIN EFFECT Phase 2
*****************************************************************
*********************************/
Data Phase2;
set IRT;

Grand_mean = &Grand_mean;
Item_main = Grand_Item - &Grand_mean;
Rater_main = 0;
Rater_Item = 0;
e=0;

/* Rater Main Effect*/
Rater_main1 = (.01*(&RE1));
Rater_main2 = (.01*(&RE2));
Rater_main3 = (.01*(&RE3));
Rater_main4 = (.01*(&RE4));
```

```
Rater_main5 = (.01*(&RE5));
Rater_main6 = (.01*(&RE6));
Rater_main7 = (.01*(&RE7));
Rater_main8 = (.01*(&RE8));
Rater_main9 = (.01*(&RE9));
Rater_main10 = (.01*(&RE10));
Rater_main11 = (.01*(&RE11));
Rater_main12 = (.01*(&RE12));


Rater1 = round ((100*(Grand_Mean + Item_main + (Rater_main1) +
Rater_Item + e)),1);If Rater1 GT 100 then Rater1= 100;If Rater1 LT 1
then Rater1= 1;
Rater2 = round ((100*(Grand_Mean + Item_main + (Rater_main2) +
Rater_Item + e)),1);If Rater2 GT 100 then Rater2= 100;If Rater2 LT 1
then Rater2= 1;
Rater3 = round ((100*(Grand_Mean + Item_main + (Rater_main3) +
Rater_Item + e)),1);If Rater3 GT 100 then Rater3= 100;If Rater3 LT 1
then Rater3= 1;
Rater4 = round ((100*(Grand_Mean + Item_main + (Rater_main4) +
Rater_Item + e)),1);If Rater4 GT 100 then Rater4= 100;If Rater4 LT 1
then Rater4= 1;
Rater5 = round ((100*(Grand_Mean + Item_main + (Rater_main5) +
Rater_Item + e)),1);If Rater5 GT 100 then Rater5= 100;If Rater5 LT 1
then Rater5= 1;
Rater6 = round ((100*(Grand_Mean + Item_main + (Rater_main6) +
Rater_Item + e)),1);If Rater6 GT 100 then Rater6= 100;If Rater6 LT 1
then Rater6= 1;
Rater7 = round ((100*(Grand_Mean + Item_main + (Rater_main7) +
Rater_Item + e)),1);If Rater7 GT 100 then Rater7= 100;If Rater7 LT 1
then Rater7= 1;
Rater8 = round ((100*(Grand_Mean + Item_main + (Rater_main8) +
Rater_Item + e)),1);If Rater8 GT 100 then Rater8= 100;If Rater8 LT 1
then Rater8= 1;
Rater9 = round ((100*(Grand_Mean + Item_main + (Rater_main9) +
Rater_Item + e)),1);If Rater9 GT 100 then Rater9= 100;If Rater9 LT 1
then Rater9= 1;
Rater10 = round ((100*(Grand_Mean + Item_main + (Rater_main10) +
Rater_Item + e)),1);If Rater10 GT 100 then Rater10= 100;If Rater10
LT 1 then Rater10= 1;
Rater11 = round ((100*(Grand_Mean + Item_main + (Rater_main11) +
Rater_Item + e)),1);If Rater11 GT 100 then Rater11= 100;If Rater11
LT 1 then Rater11= 1;
Rater12 = round ((100*(Grand_Mean + Item_main + (Rater_main12) +
Rater_Item + e)),1);If Rater12 GT 100 then Rater12= 100;If Rater12
LT 1 then Rater12= 1;

Diff = Max(of Rater1-Rater12) - Min(of Rater1-Rater12);
```

```
Stdev = round (std(Rater1, Rater2, Rater3, Rater4, Rater5, Rater6,
Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);
Rater_Avg = round (mean(Rater1, Rater2, Rater3, Rater4, Rater5,
Rater6, Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);

* Natural Log Check;
NLogData = ((Rater_Avg*.01)-C)/(1-(Rater_Avg*.01));
If NLogData LE 0 Then NLogData = .01;
Theta_Cal = (log(NLogData)+(1.7*A*B))/(1.7*A);

Phase = 2;

Rep = symget('Rep');
Rel = symget('Rel');
Per = symget('Per');
Direct = symget('Direct');
theta_mc = symget('Theta');

/* Datacheck
proc means N Mean Std var MIN MAX SKEW KURT data = Phase2 ;
var Rater1 Rater2 Rater3 Rater4 Rater5 Rater6 Rater7 Rater8 Rater9
Theta_cal;
*/

*proc print noobs data=Phase2;
*Var Theta_mc Bank A B C Grand_mean Rater1 Rater2 Rater3 Rater4
Rater5 Rater6 Rater7 Rater8 Rater9 Rater10 Rater11 Rater12 Rater_Avg
Stdev  Theta_Cal Diff;


/*****************************************************************
*********************************
    RATER X ITEM INTERACTION EFFECT Phase 3
*****************************************************************
*********************************/
Data Phase3;
set IRT;

Grand_mean = &Grand_mean;
Item_main = Grand_Item - &Grand_mean;
Rater_main = 0;
Rater_Item = 0;
e=0;

/* Rater Main Effect */
Rater_main1 = (.01*(&RE1));
Rater_main2 = (.01*(&RE2));
Rater_main3 = (.01*(&RE3));
Rater_main4 = (.01*(&RE4));
Rater_main5 = (.01*(&RE5));
```

```
Rater_main6 = (.01*(&RE6));
Rater_main7 = (.01*(&RE7));
Rater_main8 = (.01*(&RE8));
Rater_main9 = (.01*(&RE9));
Rater_main10 = (.01*(&RE10));
Rater_main11 = (.01*(&RE11));
Rater_main12 = (.01*(&RE12));

Rel = symget('Rel');

/* Rater X Item Interaction 1 Error */
Err_mean = 0;
Err_SD = 6.4;**************************************Sim;
I = 0;
Array Ran_Err[*] Err1 - Err12;
Do I = 1 To 12;
Ran_Err [I] = (.01*(Err_mean + (Err_SD * rannor(0))));
End;

Err_mean = 0;
Err_SD2 = Rel;**************************************Sim;

Per = symget('Per');

If Per = 25 then do;
Err3 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err6 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err9 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
end;

If Per = 50 then do;
Err1 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err3 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err5 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err7 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err9 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err11 =(.01*(Err_mean + (Err_SD2 * rannor(0))));
end;

If Per = 75 then do;
Err1 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err2 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err4 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err5 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err7 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err8 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err10 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
Err11 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
```

```
Err12 = (.01*(Err_mean + (Err_SD2 * rannor(0))));
end;

*RI_Err = .01 * (Err_mean + (Err_SD2 * rannor(0)));

Rater1 = round ((100*(Grand_Mean + Item_main + (Rater_main1) +
(Rater_Item + Err1 + e))),1);If Rater1 GT 100 then Rater1= 100;If
Rater1 LT 1 then Rater1= 1;
Rater2 = round ((100*(Grand_Mean + Item_main + (Rater_main2) +
(Rater_Item + Err2 + e))),1);If Rater2 GT 100 then Rater2= 100;If
Rater2 LT 1 then Rater2= 1;
Rater3 = round ((100*(Grand_Mean + Item_main + (Rater_main3) +
(Rater_Item + Err3 + e))),1);If Rater3 GT 100 then Rater3= 100;If
Rater3 LT 1 then Rater3= 1;
Rater4 = round ((100*(Grand_Mean + Item_main + (Rater_main4) +
(Rater_Item + Err4 + e))),1);If Rater4 GT 100 then Rater4= 100;If
Rater4 LT 1 then Rater4= 1;
Rater5 = round ((100*(Grand_Mean + Item_main + (Rater_main5) +
(Rater_Item + Err5 + e))),1);If Rater5 GT 100 then Rater5= 100;If
Rater5 LT 1 then Rater5= 1;
Rater6 = round ((100*(Grand_Mean + Item_main + (Rater_main6) +
(Rater_Item + Err6 + e))),1);If Rater6 GT 100 then Rater6= 100;If
Rater6 LT 1 then Rater6= 1;
Rater7 = round ((100*(Grand_Mean + Item_main + (Rater_main7) +
(Rater_Item + Err7 + e))),1);If Rater7 GT 100 then Rater7= 100;If
Rater7 LT 1 then Rater7= 1;
Rater8 = round ((100*(Grand_Mean + Item_main + (Rater_main8) +
(Rater_Item + Err8 + e))),1);If Rater8 GT 100 then Rater8= 100;If
Rater8 LT 1 then Rater8= 1;
Rater9 = round ((100*(Grand_Mean + Item_main + (Rater_main9) +
(Rater_Item + Err9 + e))),1);If Rater9 GT 100 then Rater9= 100;If
Rater9 LT 1 then Rater9= 1;
Rater10 = round ((100*(Grand_Mean + Item_main + (Rater_main10) +
(Rater_Item + Err10 + e))),1);If Rater10 GT 100 then Rater10= 100;If
Rater10 LT 1 then Rater10= 1;
Rater11 = round ((100*(Grand_Mean + Item_main + (Rater_main11) +
(Rater_Item + Err11 + e))),1);If Rater11 GT 100 then Rater11= 100;If
Rater11 LT 1 then Rater11= 1;
Rater12 = round ((100*(Grand_Mean + Item_main + (Rater_main12) +
(Rater_Item + Err12 + e))),1);If Rater12 GT 100 then Rater12= 100;If
Rater12 LT 1 then Rater12= 1;

Diff = Max(of Rater1-Rater12) - Min(of Rater1-Rater12);
Stdev = round (std(Rater1, Rater2, Rater3, Rater4, Rater5, Rater6,
Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);
Rater_Avg = round (mean(Rater1, Rater2, Rater3, Rater4, Rater5,
Rater6, Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);
```

```
* Natural Log Check;
NLogData = ((Rater_Avg*.01)-C)/(1-(Rater_Avg*.01));
If NLogData LE 0 Then NLogData = .01;
Theta_Cal = (log(NLogData)+(1.7*A*B))/(1.7*A);

Phase = 3;

Rep = symget('Rep');
Direct = symget('Direct');
theta_mc = symget('Theta');

/*****************************************************************
*********************************
    Group Dynamics EFFECT Phase 4
*****************************************************************
*********************************/

/* Create Rater Influence Factor*/
data Influence_Factors;
array RI[*] RI1- RI12;
do I = 1 To 12;
RI [I] = 0.7 + (0.1 * rannor(0));
End;

call symput('RI1', RI1);
call symput('RI2', RI2);
call symput('RI3', RI3);
call symput('RI4', RI4);
call symput('RI5', RI5);
call symput('RI6', RI6);
call symput('RI7', RI7);
call symput('RI8', RI8);
call symput('RI9', RI9);
call symput('RI10', RI10);
call symput('RI11', RI11);
call symput('RI12', RI12);

Data Phase4;
set Phase3;

RMIN=min(of rater1-rater12);
RMAX=max(of rater1-rater12);
RMEAN=mean(of rater1-rater12);

Direct = symget('Direct');
If Direct = 1 then XX=RMIN;
If Direct = 2 then XX=RMEAN;
If Direct = 3 then XX=RMAX;

influ1 = (&RI1);
```

```
influ2 = (&RI2);
influ3 = (&RI3);
influ4 = (&RI4);
influ5 = (&RI5);
influ6 = (&RI6);
influ7 = (&RI7);
influ8 = (&RI8);
influ9 = (&RI9);
influ10 = (&RI10);
influ11 = (&RI11);
influ12 = (&RI12);

*rating = rating + (influ - rating) * influence_factor;

Rater1 = round (XX + ((rater1 - XX)* (&RI1)),1);
Rater2 = round (XX + ((rater2 - XX)* (&RI2)),1);
Rater3 = round (XX + ((rater3 - XX)* (&RI3)),1);
Rater4 = round (XX + ((rater4 - XX)* (&RI4)),1);
Rater5 = round (XX + ((rater5 - XX)* (&RI5)),1);
Rater6 = round (XX + ((rater6 - XX)* (&RI6)),1);
Rater7 = round (XX + ((rater7 - XX)* (&RI7)),1);
Rater8 = round (XX + ((rater8 - XX)* (&RI8)),1);
Rater9 = round (XX + ((rater9 - XX)* (&RI9)),1);
Rater10 = round (XX + ((rater10 - XX)* (&RI10)),1);
Rater11 = round (XX + ((rater11 - XX)* (&RI11)),1);
Rater12 = round (XX + ((rater12 - XX)* (&RI12)),1);

Phase = 4;

Direct = symget('Direct');
Diff = Max(of Rater1-Rater12) - Min(of Rater1-Rater12);
Stdev = round (std(Rater1, Rater2, Rater3, Rater4, Rater5, Rater6,
Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);
Rater_Avg = round (mean(Rater1, Rater2, Rater3, Rater4, Rater5,
Rater6, Rater7, Rater8, Rater9, Rater10, Rater11, Rater12),.01);

* Natural Log Check;
NLogData = ((Rater_Avg*.01)-C)/(1-(Rater_Avg*.01));
If NLogData LE 0 Then NLogData = .01;
Theta_Cal = (log(NLogData)+(1.7*A*B))/(1.7*A);

/*****************************************************************
*********************************
Stratified Sampling Procedure
*****************************************************************
*********************************/
%Put Var&I;

%If &&Var&I = 36 %then %do;
```

```
%Let L1 = 6;
%Let L2 = 5;
%Let L3 = 1;
%Let L4 = 4;
%Let L5 = 4;
%Let L6 = 4;
%Let L7 = 2;
%Let L8 = 3;
%Let L9 = 7;
%end;

%If &&Var&I = 47 %then %do;
%Let L1 = 7;
%Let L2 = 7;
%Let L3 = 1;
%Let L4 = 6;
%Let L5 = 5;
%Let L6 = 5;
%Let L7 = 3;
%Let L8 = 4;
%Let L9 = 9;
%end;

%If &&Var&I = 72 %then %do;
%Let L1 = 11;
%Let L2 = 10;
%Let L3 = 2;
%Let L4 = 8;
%Let L5 = 8;
%Let L6 = 8;
%Let L7 = 5;
%Let L8 = 6;
%Let L9 = 14;
%end;

%If &&Var&I = 94 %then %do;
%Let L1 = 14;
%Let L2 = 14;
%Let L3 = 3;
%Let L4 = 11;
%Let L5 = 10;
%Let L6 = 11;
%Let L7 = 6;
%Let L8 = 7;
%Let L9 = 18;
%end;

%If &&Var&I = 107 %then %do;
%Let L1 = 16;
%Let L2 = 16;
```

```
%Let L3 = 3;
%Let L4 = 13;
%Let L5 = 11;
%Let L6 = 12;
%Let L7 = 7;
%Let L8 = 8;
%Let L9 = 21;
%end;

%If &&Var&I = 143 %then %do;
%Let L1 = 22;
%Let L2 = 21;
%Let L3 = 4;
%Let L4 = 17;
%Let L5 = 15;
%Let L6 = 16;
%Let L7 = 9;
%Let L8 = 11;
%Let L9 = 28;
%end;

proc sort;
by IRT_LEVEL;

proc surveyselect data=Phase4 method=srs rep = 1
n=(&L1 &L2 &L3 &L4 &L5 &L6 &L7 &L8 &L9) out=obsout noprint;
strata IRT_Level;
  id _all_;

Proc means N mean median std min max noprint data=obsout;
class Rep;
var Theta_cal;
output out= obsout_mean N=N_Sam Mean=Theta_cal_mean_sam
Median=Theta_cal_med_sam Std=Theta_cal_std_sam;

Data Phase4;
merge Phase4 obsout_mean;
by Rep;
if _type_=1;
run;

/* Datacheck*/
proc means N Mean Std var MIN MAX SKEW KURT data = Phase4;
var Rater1 Rater2 Rater3 Rater4 Rater5 Rater6 Rater7 Rater8 Rater9
Rater10 Rater11 Rater12 Theta_cal;
run;

*proc print noobs data=Phase3;
*Var Theta_mc Bank A B C Grand_mean Rater1 Rater2 Rater3 Rater4
Rater5 Rater6 Rater7 Rater8 Rater9 Rater10 Rater11 Rater12 Rater_Avg
Stdev Theta_Cal Diff;
```

```
/*****************************************************************
*********************************
    G-Theory Phase 1
*****************************************************************
*********************************/
/*
Data Trans1;
set Phase1;
Keep Rater1 Rater2 Rater3 Rater4 Rater5 Rater6 Rater7 Rater8 Rater9
Rater10 Rater11 Rater12 Bank;
proc sort;
by Bank;
proc transpose data=Trans1 out=Trans1_out;
   by Bank;
PROC FORMAT;
      Value $rfmt  'Rater1'=1  'Rater2'=2  'Rater3'=3 'Rater4'=4
'Rater5'=5
                   'Rater6'=6  'Rater7'=7  'Rater8'=8 'Rater9'=9
'Rater10'=10 'Rater11'=11 'Rater12'=12;
Data Trans1_out;
set Trans1_out;
Format _Name_ $rfmt. ;
Rater = _Name_;Drop Rater;
Rename _Name_ = Raters;
Rename Col1 = Score;
Rename Bank = Item;
*proc print data=long1;
*run;

proc varcomp;
class Item Raters;
model Score = Item Raters Item*Raters;
run;

*Create Observed Datasets to check reliability;
Data Obs_Check;
set Phase4;
oRater1 = Rater1;
oRater2 = Rater2;
oRater3 = Rater3;
oRater4 = Rater4;
oRater5 = Rater5;
oRater6 = Rater6;
oRater7 = Rater7;
oRater8 = Rater8;
oRater9 = Rater9;
oRater10 = Rater10;
oRater11 = Rater11;
oRater12 = Rater12;
```

```
Keep oRater1 oRater2 oRater3 oRater4 oRater5 oRater6 oRater7 oRater8
oRater9 oRater10 oRater11 oRater12;

/*****************************************************************
*********************************
    G-Theory Phase 2
*****************************************************************
*********************************/
/*
Data Trans2;
set Phase2;
Keep Rater1 Rater2 Rater3 Rater4 Rater5 Rater6 Rater7 Rater8 Rater9
Rater10 Rater11 Rater12 Bank;
proc sort;
by Bank;
proc transpose data=Trans2 out=Trans2_out;
   by Bank;
PROC FORMAT;
      Value $rfmt  'Rater1'=1  'Rater2'=2  'Rater3'=3 'Rater4'=4
'Rater5'=5
                 'Rater6'=6  'Rater7'=7  'Rater8'=8 'Rater9'=9
'Rater10'=10 'Rater11'=11 'Rater12'=12;
Data Trans2_out;
set Trans2_out;
Format _Name_ $rfmt. ;
Rater = _Name_;Drop Rater;
Rename _Name_ = Raters;
Rename Col1 = Score;
Rename Bank = Item;

proc varcomp data=Trans2_out;
class Item Raters;
model Score = Item Raters Item*Raters;
run;

/*****************************************************************
*********************************
    G-Theory Phase 3
*****************************************************************
*********************************/
/*
Data Trans2;
set Phase3;
Keep Rater1 Rater2 Rater3 Rater4 Rater5 Rater6 Rater7 Rater8 Rater9
Rater10 Rater11 Rater12 Bank;
proc sort;
by Bank;
proc transpose data=Trans2 out=Trans2_out;
   by Bank;
PROC FORMAT;
```

```
      Value $rfmt  'Rater1'=1  'Rater2'=2  'Rater3'=3 'Rater4'=4
'Rater5'=5
                   'Rater6'=6  'Rater7'=7  'Rater8'=8 'Rater9'=9
'Rater10'=10 'Rater11'=11 'Rater12'=12;
Data Trans2_out;
set Trans2_out;
Format _Name_ $rfmt. ;
Rater = _Name_;Drop Rater;
Rename _Name_ = Raters;
Rename Col1 = Score;
Rename Bank = Item;

proc varcomp data=Trans2_out;
class Item Raters;
model Score = Item Raters Item*Raters;
run;

/*****************************************************************
*********************************
    G-Theory Phase 4
*****************************************************************
*********************************/
/*
Data Trans2;
set Phase4;
Keep Rater1 Rater2 Rater3 Rater4 Rater5 Rater6 Rater7 Rater8 Rater9
Rater10 Rater11 Rater12 Bank;
proc sort;
by Bank;
proc transpose data=Trans2 out=Trans2_out;
   by Bank;
PROC FORMAT;
      Value $rfmt  'Rater1'=1  'Rater2'=2  'Rater3'=3 'Rater4'=4
'Rater5'=5
                   'Rater6'=6  'Rater7'=7  'Rater8'=8 'Rater9'=9
'Rater10'=10 'Rater11'=11 'Rater12'=12;
Data Trans2_out;
set Trans2_out;
Format _Name_ $rfmt. ;
Rater = _Name_;Drop Rater;
Rename _Name_ = Raters;
Rename Col1 = Score;
Rename Bank = Item;

proc varcomp data=Trans2_out;
class Item Raters;
model Score = Item Raters Item*Raters;
run;

/*****************************************************************
*********************************
```

```
    START Check Error
**********************************************************************
**********************************/
/*
Data rel_check&Rep;
set Obs_check;
keep oRater1 oRater3 oRater5 oRater6 oRater9 oRater12;
Rename oRater1 = oRater1R&Rep;
Rename oRater3 = oRater3R&Rep;
Rename oRater5 = oRater5R&Rep;
Rename oRater6 = oRater6R&Rep;
Rename oRater9 = oRater9R&Rep;
Rename oRater12 = oRater12R&Rep;

Data Rater_Reliability;
merge True_Check Obs_Check;

proc corr data=Rater_Reliability noprint outp=error_check;

*proc print data = Obs;
*var r_xx2 Err_SD2 RI_Err rater5 rater6;

proc corr data=Rater_Reliability;
var oRater1 oRater2 oRater3 oRater5 oRater6 oRater9 oRater12;
run;

Data error_check2;
set error_check;
if _TYPE_ = 'CORR';
if _Name_ in ('tRater1', 'tRater2', 'tRater3', 'tRater4', 'tRater5',
'tRater6', 'tRater7', 'tRater8', 'tRater9', 'tRater10', 'tRater11',
'tRater12');
Drop tRater1 tRater2 tRater3 tRater4 tRater5 tRater6 tRater7 tRater8
tRater9 tRater10 tRater11 tRater12 _type_ ;

data error_check3;
set error_check;
True =_n_;
array vars[*] oRater1- oRater12;
do Obs = 1 to 12;
if Obs = True then do;
r = vars[Obs];
output;
end;
end;
Drop oRater1- oRater12;
proc print data=error_check;
run;
/* END Check Error ***********************************/
```

```
/*****************************************************************
*********************************
    Phase Compare
*****************************************************************
*********************************/

/* Get Phase1 Standard */
proc means data = Phase1 noprint;
ID Rep Rel Per Direct theta_mc;
 var Rater_Avg Theta_cal Phase;
 output out = Phase1_Stand median = Rater_median Theta_median mean =
Rater_mean Theta_mean Phase  std = Rater_std Theta_std;

/* Get Phase2 Standard */
proc means data = Phase2 noprint;
ID Rep Rel Per Direct theta_mc;
 var Rater_Avg Theta_cal Phase;
 output out = Phase2_Stand median = Rater_median Theta_median mean =
Rater_mean Theta_mean Phase  std = Rater_std Theta_std;

/* Get Phase3 Standard */
proc means data = Phase3 noprint;
 ID Rep Rel Per Direct theta_mc;
 var Rater_Avg Theta_cal Phase;
 output out = Phase3_Stand median = Rater_median Theta_median mean =
Rater_mean Theta_mean Phase  std = Rater_std Theta_std;

/* Get Phase4 Standard */
proc means data = Phase4 noprint;
 ID Rep Rel Per Direct theta_mc;
 var Rater_Avg Theta_cal Theta_cal_mean_sam Theta_cal_med_sam
Theta_cal_std_sam N_Sam Phase;
 output out = Phase4_Stand&Rep median = Rater_median Theta_median
mean = Rater_mean Theta_mean Theta_cal_mean_sam_mean
Theta_cal_med_sam_mean Theta_cal_std_sam_mean N_Sam Phase  std =
Rater_std Theta_std ;

/* Merge data files */
PROC APPEND
     BASE=Phase1_Stand
     DATA=Phase2_Stand;

PROC APPEND
     BASE=Phase1_Stand
     DATA=Phase3_Stand;

Data Phase4_lite;
Set Phase4_Stand&Rep;
DROP Theta_cal_mean_sam_mean Theta_cal_med_sam_mean
Theta_cal_std_sam_mean N_Sam;
```

```
PROC APPEND
     BASE=Phase1_Stand
     DATA=Phase4_lite;

PROC FORMAT;
 Value cfmt 1 = "Phase 1"
            2 = "Phase 2"
            3 = "Phase 3"
            4 = "Phase 4";

data All;
RETAIN Phase Theta_mean Theta_median Theta_std Rater_mean
Rater_median;
set Phase1_Stand;
Format Phase cfmt. ;

*proc print data=All;

proc append base=Phase4_Stand_Rep data=Phase4_Stand&Rep;

*data Rel_check;
*merge Rel_check Rel_check&Rep;

*proc corr data=Rel_check;
*var oRater1R1 oRater1R2 oRater1R3 oRater1R4 oRater1R5 ORater1R6;

*proc corr data=Rel_check;
*var oRater3R1 oRater3R2 oRater3R3 oRater3R4 oRater3R5 ORater3R6;

*proc corr data=Rel_check;
*var oRater5R1 oRater5R2 oRater5R3 oRater5R4 oRater5R5 ORater5R6;

*proc corr data=Rel_check;
*var oRater6R1 oRater6R2 oRater6R3 oRater6R4 oRater6R5 ORater6R6;

*proc corr data=Rel_check;
*var oRater9R1 oRater9R2 oRater9R3 oRater9R4 oRater9R5 ORater9R6;

*proc corr data=Rel_check;
*var oRater12R1 oRater12R2 oRater12R3 oRater12R4 oRater12R5
ORater12R6;

*Clear Old dataset;
proc datasets nolist; delete Phase4_Stand&Rep;

%end;
%end;

%Let Filename = SS_Sim.&Dist&Cond;
```

```
data &Filename;
set Phase4_stand_Rep;

*Outcomes;
large_bias = theta_mean - theta_mc;
small_bias = theta_cal_mean_sam_mean - theta_mc;
between_bias = theta_cal_mean_sam_mean - theta_mean;

large_bias_sq = ((theta_mean - theta_mc)*(theta_mean - theta_mc));
small_bias_sq = ((theta_cal_mean_sam_mean -
theta_mc)*(theta_cal_mean_sam_mean - theta_mc));
between_bias_sq = ((theta_cal_mean_sam_mean -
theta_mean)*(theta_cal_mean_sam_mean - theta_mean));

large_bias_a = ABS(theta_mean - theta_mc);
small_bias_a = ABS(theta_cal_mean_sam_mean - theta_mc);
between_bias_a = ABS(theta_cal_mean_sam_mean - theta_mean);
Dist=SYMGET('Dist');
RaterN = &RaterN;
run;

data All_Rec;
set &Filename;

%mend SS_SIM;

* +--------------------------------+
   Define 'dummy' files to reroute
   the log and/or output windows
  +--------------------------------+;

filename junk dummy;
filename junk2 dummy;

/*****************************************************************
*********************************
    Calls to the Macro
*****************************************************************
*********************************/
%SS_SIM(Rel = 12.5, Per = 25, Theta = -1, Direct = 1, Cond = 1);
Run;
%SS_SIM(Rel = 12.5, Per = 25, Theta = 0, Direct = 1, Cond = 2);
Run;
%SS_SIM(Rel = 12.5, Per = 25, Theta = 1, Direct = 1, Cond = 3);
Run;

%SS_SIM(Rel = 12.5, Per = 50, Theta = -1, Direct = 1, Cond = 4);
Run;
%SS_SIM(Rel = 12.5, Per = 50, Theta = 0, Direct = 1, Cond = 5);
Run;
```

```
%SS_SIM(Rel = 12.5, Per = 50, Theta = 1, Direct = 1, Cond = 6);
Run;

%SS_SIM(Rel = 12.5, Per = 75, Theta = -1, Direct = 1, Cond = 7);
Run;
%SS_SIM(Rel = 12.5, Per = 75, Theta = 0, Direct = 1, Cond = 8);
Run;
%SS_SIM(Rel = 12.5, Per = 75, Theta = 1, Direct = 1, Cond = 9);
Run;

%SS_SIM(Rel = 12.5, Per = 25, Theta = -1, Direct = 2, Cond = 10);
Run;
%SS_SIM(Rel = 12.5, Per = 25, Theta = 0, Direct = 2, Cond = 11);
Run;
%SS_SIM(Rel = 12.5, Per = 25, Theta = 1, Direct = 2, Cond = 12);
Run;

%SS_SIM(Rel = 12.5, Per = 50, Theta = -1, Direct = 2, Cond = 13)
Run;
%SS_SIM(Rel = 12.5, Per = 50, Theta = 0, Direct = 2, Cond = 14)
Run;
%SS_SIM(Rel = 12.5, Per = 50, Theta = 1, Direct = 2, Cond = 15)
Run;

%SS_SIM(Rel = 12.5, Per = 75, Theta = -1, Direct = 2, Cond = 16)
Run;
%SS_SIM(Rel = 12.5, Per = 75, Theta = 0, Direct = 2, Cond = 17)
Run;
%SS_SIM(Rel = 12.5, Per = 75, Theta = 1, Direct = 2, Cond = 18)
Run;

%SS_SIM(Rel = 12.5, Per = 25, Theta = -1, Direct = 3, Cond = 19)
Run;
%SS_SIM(Rel = 12.5, Per = 25, Theta = 0, Direct = 3, Cond = 20)
Run;
%SS_SIM(Rel = 12.5, Per = 25, Theta = 1, Direct = 3, Cond = 21)
Run;

%SS_SIM(Rel = 12.5, Per = 50, Theta = -1, Direct = 3, Cond = 22)
Run;
%SS_SIM(Rel = 12.5, Per = 50, Theta = 0, Direct = 3, Cond = 23)
Run;
%SS_SIM(Rel = 12.5, Per = 50, Theta = 1, Direct = 3, Cond = 24)
Run;

%SS_SIM(Rel = 12.5, Per = 75, Theta = -1, Direct = 3, Cond = 25)
Run;
```

```
%SS_SIM(Rel = 12.5, Per = 75, Theta = 0, Direct = 3, Cond = 26)
Run;
%SS_SIM(Rel = 12.5, Per = 75, Theta = 1, Direct = 3, Cond = 27)
Run;

%SS_SIM(Rel = 17.5, Per = 25, Theta = -1, Direct = 1, Cond = 28)
Run;
%SS_SIM(Rel = 17.5, Per = 25, Theta = 0, Direct = 1, Cond = 29)
Run;
%SS_SIM(Rel = 17.5, Per = 25, Theta = 1, Direct = 1, Cond = 30)
Run;

%SS_SIM(Rel = 17.5, Per = 50, Theta = -1, Direct = 1, Cond = 31)
Run;
%SS_SIM(Rel = 17.5, Per = 50, Theta = 0, Direct = 1, Cond = 32)
Run;
%SS_SIM(Rel = 17.5, Per = 50, Theta = 1, Direct = 1, Cond = 33)
Run;

%SS_SIM(Rel = 17.5, Per = 75, Theta = -1, Direct = 1, Cond = 34)
Run;
%SS_SIM(Rel = 17.5, Per = 75, Theta = 0, Direct = 1, Cond = 35)
Run;
%SS_SIM(Rel = 17.5, Per = 75, Theta = 1, Direct = 1, Cond = 36)
Run;

%SS_SIM(Rel = 17.5, Per = 25, Theta = -1, Direct = 2, Cond = 37)
Run;
%SS_SIM(Rel = 17.5, Per = 25, Theta = 0, Direct = 2, Cond = 38)
Run;
%SS_SIM(Rel = 17.5, Per = 25, Theta = 1, Direct = 2, Cond = 39)
Run;

%SS_SIM(Rel = 17.5, Per = 50, Theta = -1, Direct = 2, Cond = 40)
Run;
%SS_SIM(Rel = 17.5, Per = 50, Theta = 0, Direct = 2, Cond = 41)
Run;
%SS_SIM(Rel = 17.5, Per = 50, Theta = 1, Direct = 2, Cond = 42)
Run;

%SS_SIM(Rel = 17.5, Per = 75, Theta = -1, Direct = 2, Cond = 43)
Run;
%SS_SIM(Rel = 17.5, Per = 75, Theta = 0, Direct = 2, Cond = 44)
Run;
%SS_SIM(Rel = 17.5, Per = 75, Theta = 1, Direct = 2, Cond = 45)
Run;
```

```
%SS_SIM(Rel = 17.5, Per = 25, Theta = -1, Direct = 3, Cond = 46)
Run;
%SS_SIM(Rel = 17.5, Per = 25, Theta = 0, Direct = 3, Cond = 47)
Run;
%SS_SIM(Rel = 17.5, Per = 25, Theta = 1, Direct = 3, Cond = 48)
Run;

%SS_SIM(Rel = 17.5, Per = 50, Theta = -1, Direct = 3, Cond = 49)
Run;
%SS_SIM(Rel = 17.5, Per = 50, Theta = 0, Direct = 3, Cond = 50)
Run;
%SS_SIM(Rel = 17.5, Per = 50, Theta = 1, Direct = 3, Cond = 51)
Run;

%SS_SIM(Rel = 17.5, Per = 75, Theta = -1, Direct = 3, Cond = 52)
Run;
%SS_SIM(Rel = 17.5, Per = 75, Theta = 0, Direct = 3, Cond = 53)
Run;
%SS_SIM(Rel = 17.5, Per = 75, Theta = 1, Direct = 3, Cond = 54)
Run;

%SS_SIM(Rel = 21.0, Per = 25, Theta = -1, Direct = 1, Cond = 55)
Run;
%SS_SIM(Rel = 21.0, Per = 25, Theta = 0, Direct = 1, Cond = 56)
Run;
%SS_SIM(Rel = 21.0, Per = 25, Theta = 1, Direct = 1, Cond = 57)
Run;

%SS_SIM(Rel = 21.0, Per = 50, Theta = -1, Direct = 1, Cond = 58)
Run;
%SS_SIM(Rel = 21.0, Per = 50, Theta = 0, Direct = 1, Cond = 59)
Run;
%SS_SIM(Rel = 21.0, Per = 50, Theta = 1, Direct = 1, Cond = 60)
Run;

%SS_SIM(Rel = 21.0, Per = 75, Theta = -1, Direct = 1, Cond = 61)
Run;
%SS_SIM(Rel = 21.0, Per = 75, Theta = 0, Direct = 1, Cond = 62)
Run;
%SS_SIM(Rel = 21.0, Per = 75, Theta = 1, Direct = 1, Cond = 63)
Run;

%SS_SIM(Rel = 21.0, Per = 25, Theta = -1, Direct = 2, Cond = 64)
Run;
%SS_SIM(Rel = 21.0, Per = 25, Theta = 0, Direct = 2, Cond = 65)
Run;
%SS_SIM(Rel = 21.0, Per = 25, Theta = 1, Direct = 2, Cond = 66)
Run;
```

```
%SS_SIM(Rel = 21.0, Per = 50, Theta = -1, Direct = 2, Cond = 67)
Run;
%SS_SIM(Rel = 21.0, Per = 50, Theta = 0, Direct = 2, Cond = 68)
Run;
%SS_SIM(Rel = 21.0, Per = 50, Theta = 1, Direct = 2, Cond = 69)
Run;

%SS_SIM(Rel = 21.0, Per = 75, Theta = -1, Direct = 2, Cond = 70)
Run;
%SS_SIM(Rel = 21.0, Per = 75, Theta = 0, Direct = 2, Cond = 71)
Run;
%SS_SIM(Rel = 21.0, Per = 75, Theta = 1, Direct = 2, Cond = 72)
Run;

%SS_SIM(Rel = 21.0, Per = 25, Theta = -1, Direct = 3, Cond = 73)
Run;
%SS_SIM(Rel = 21.0, Per = 25, Theta = 0, Direct = 3, Cond = 74)
Run;
%SS_SIM(Rel = 21.0, Per = 25, Theta = 1, Direct = 3, Cond = 75)
Run;

%SS_SIM(Rel = 21.0, Per = 50, Theta = -1, Direct = 3, Cond = 76)
Run;
%SS_SIM(Rel = 21.0, Per = 50, Theta = 0, Direct = 3, Cond = 77)
Run;
%SS_SIM(Rel = 21.0, Per = 50, Theta = 1, Direct = 3, Cond = 78)
Run;

%SS_SIM(Rel = 21.0, Per = 75, Theta = -1, Direct = 3, Cond = 79)
Run;
%SS_SIM(Rel = 21.0, Per = 75, Theta = 0, Direct = 3, Cond = 80)
Run;
%SS_SIM(Rel = 21.0, Per = 75, Theta = 1, Direct = 3, Cond = 81)
Run;


*+--------------------------------+
  Turn on the output window again
 +--------------------------------+;
proc printto log = log;
run;
proc printto print = print;
run;

Proc means data=All_Rec n sum mean std;
class rel per Direct N_Sam theta_mc;
```

```
var theta_mean between_bias large_bias small_bias between_bias_sq
large_bias_sq small_bias_sq between_bias_a large_bias_a
small_bias_a;
run;
```

About the Author

James Thomas Coraggio received a bachelor's degree (BA) in Mass Communications from the University of South Florida in 1994 and a master's degree (M.Ed.) in Curriculum and Instruction with an emphasis in measurement and research from the University of South Florida in 2002. He has directed test development and psychometric processes for standardized testing companies at Schroeder Measurement Technologies and Pearson. He has also worked in the area of measurement, evaluation, and assessment for Eckerd Youth Alternatives and St. Petersburg College. In addition, he consulted with doctoral students on dissertation research through the Consulting Office for Research in Education and taught undergraduate measurement courses both in face-to-face and online formats. His research has been nominated for the Florida Educational Research Association distinguished paper two times and recognized as Best Paper for the Florida Association of Institutional Research Conference. He has been listed in Marquis Who's Who in the World (2008).