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FUNDAMENTAL MEASUREMENT

FOR HEALTH SYSTEMS RESEARCH

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ABSTRACT

Science advances on the basis of linear measurement systems that are invariant across different experimental conditions. However, most survey-based health sciences research employs measures that have never been tested for invariance. This study describes and demonstrates the mathematical properties underlying the additive formulations of probabilistic conjoint measurement (PCM), which provide the necessary and sufficient conditions for fundamental measurement. As a demonstration, PCM was applied to data from 23,767 persons who responded to the Medical Expenditure Panel Survey (MEPS) conducted by the Agency for Healthcare Research and Quality (AHRQ; formerly the Agency for Health Care Policy and Research (AHCPR)). This demonstration includes tests of five hypotheses: (1) that the quality of care (QOC) variable is quantitative, (2) that the data's ordering of the survey items and rating categories is meaningful, (3) that the respondent measurement order remains constant over item subsamples, and that the item calibration order remains constant over respondent subsamples, (4) that there are no differences among the measures associated with respondents belonging to one or another of several different forms of insurance coverage (Private HMO, Public HMO, and Medicare), and (5) that each of three approaches to measurement (PCM, Item Response Theory, and the commonly used method of summated ratings) have specific scientific strengths and weaknesses that make them appropriate for general use as scientific measurement methods. None of the hypotheses were rejected, though (3) and (5) are strongly qualified. The results indicate two general conclusions: (1) PCM offers new insights into theories of QOC, and (2) it is possible to construct fundamental measurement systems of QOC variables. Finally, since QOC fundamental measurement systems can be constructed, it will be possible to equate and interpret the responses of different QOC surveys in the same metric.

INTRODUCTION

> "It is a scientific platitude that there can be neither precise control nor prediction of phenomena without measurement…. It is nearly useless to have an exactly formulated quantitative theory if empirically feasible methods of measurement cannot be developed for a substantial portion of the quantitative concepts of the theory. Given a physical concept like that of mass or a psychological concept like that of habit strength, *the point of the theory of measurement is to lay bare the structure of a collection of empirical relations which may be used to measure the characteristic of empirical phenomena corresponding to the concept.* Why a collection of relations? From an abstract standpoint a set of empirical data consists of a collection of relations between specified objects …The major source of difficulty in providing an adequate theory of measurement is to construct relations which have an exact and reasonable numerical interpretation and yet also have a technically practical interpretation" (Scott & Suppes, 1958).

The research reported here asks what empirical relations in quality of care survey data might provide the measurement quality needed for both exact numerical and technically practical interpretations. If a variable is to be measured precisely, then it needs to be 1) measurable on a linear (interval or ratio) scale, where 2) the measurement of objects does not depend on which measurement instrument is used, and 3) the properties of the measuring instrument do not change with the object measured. For instance, a person's height 1) represents a linear magnitude, 2) remains the same, no matter which ruler is used, and 3) the markings on the ruler remain in the same positions, regardless of who is measured.

That the three basic requirements of measurement can be met in the physical sciences is strikingly obvious. That these requirements can be attained in the health sciences is not so obvious, since most health science variables (e.g., pain, quality of life, etc.) are latent. Common practice assigns numeric values to observations of these variables and analyzes them as though they represent linear magnitudes. Such assignments arbitrarily depend on the perceptions of the experimenter and the particular persons surveyed. Measurements based on such assignments are never linear or generalizable across laboratories.

For example, summed rating scale responses are typically treated as linear measures. Consider a 3-category response format *Disagree (1) / Neutral (2) / Agree (3)* employed for a particular survey. Although the assigned numbers show a difference of 1 between successive categories, their *true distances are unknown and may be highly variable* (Wilson 1971; Duncan 1984; Merbitz, Morris & Grip 1989; Wright & Linacre 1989; Michell 1990, 1997, 1999, 2000; Fisher 1993; Stucki, Daltroy, Katz, Johannesson & Liang 1996; Zhu 1996). The existence of a single unit difference between categories is typically assumed, with no effort made at producing supporting evidence, even though multiple studies have shown that rating categories usually are not equally spaced. In short, there is no more justification for assuming stable category distances of 1 than there is for assuming variable distances of 0.5 and 1.75, or 3.0 and 0.9. Experimenter-defined numerical assignments to observations *can be only ordinal,* and *never represent true magnitudes*. Treating ordinal data as interval or ratio has been referred to as laying the "foundations of misinference" and is generally recognized as unscientific (Merbitz, et al. 1989).

Measurement theoreticians have shown conclusively that it is possible to construct interval quantitative measures using ordinal information (Scott & Suppes 1958; Rasch 1960; Luce & Tukey 1964; Andersen 1977; Wright 1977b, 1985, 1999; Falmagne 1979; Perline, Wright & Wainer 1979; Wright & Stone 1979; Andrich 1988, 1989; Suppes, Krantz, Luce & Tversky 1989). If ordinal data embody relationships prescribed by the axioms of additive conjoint measurement (ACM), then measures are linearly scaleable, and are as mathematically rigorous as physical measures. Accord with ACM axioms confirms that the relevant properties of the measured objects are invariant with respect to the measurement instrument, and that the properties of the measuring instrument are invariant with respect to the objects measured.

MEASUREMENT THEORY

Consider the case of a survey/test data set containing dichotomous responses (e.g., Yes/No; Agree/Disagree; True/False; Correct, Incorrect). Let $n \in N$ represent a person from the person set, and $i \in I$ an item from the item set; X_{ni} is a person-item response from the set {0,1}, and P_{ni1} is the probability of a $X_{ni}=1$ response, $0 \leq P_{ni1} \leq 1$. From the N×I matrix of responses, a person's ability (β_n) and an item's calibration (δ_i) are functions of

$$\beta_n \approx \left(r/(I-r)\right) \qquad\qquad \delta_i \approx \left((N-s)/s\right) \qquad (1a\text{-}1b)$$

where the "person score" r and the “item score” s are sufficient statistics for β and δ, respectively (Andersen 1977). The set of person abilities $\langle\beta\rangle$ and item difficulties $\langle\delta\rangle$ are obtained by maximizing the likelihood function

$$L\left(\left(X_{ni}\right)_{N\times I} \;\middle|\; \langle\beta\rangle, \langle\delta\rangle\right) = \prod_{n=1}^{N} \prod_{i=1}^{I} P_{ni1}^{X_{ni}} \left(1 - P_{ni1}\right)^{1-X_{ni}} \qquad (1c)$$

See Wright & Masters (1982) for a survey of various maximum likelihood estimation algorithms.

ACM can be expressed probabilistically (Rasch 1960; Falmagne 1979; Perline, et al. 1979; Wright 1985; Andrich 1988; Suppes, et al. 1989; Wright & Linacre 1989) with a model that specifies the log-odds (λ_{ni1}) of a $X_{ni}=1$ response as an additive function of β_n and δ_i:

$$\ell n\left(P_{ni1}/P_{ni0}\right) = \lambda_{ni} = \beta_n - \delta_i \qquad (2)$$

where

$$P_{ni1} = \left[1 + \exp(-\lambda)\right]^{-1} \qquad (3)$$

and $$P_{ni0} = \left(1 - P_{ni1}\right) = \left[1 + \exp(\lambda)\right]^{-1} \qquad (4)$$

It can be inferred from (2-4) that ($\lambda_{ni1} > 0$) ⇒ ($P_{ni1} > .5$), ($\lambda_{ni1} < 0$) ⇒ ($P_{ni1} < .5$), and ($\lambda_{ni1} = 0$) ⇒ ($P_{ni1} = .5$).

The process at which the model relates to the N×I response matrix is illustrated by the following example. Table 1 shows a hypothetical data set created by six persons responding to a four-item survey. Persons and items are ordered by total score, and the set of person abilities $\langle\beta\rangle$ and item calibrations $\langle\delta\rangle$ are estimated by (1a) and (1b), respectively. This ordering approximates a pattern of overlapping triangles of 1s and 0s in the response matrix. An exception that proves (tests) the rule is provided by person *f* who unexpectedly endorsed *z*, the most difficult item, despite having a low score. Unexpected responses such as this provide opportunities for data quality assessment and improvement, and can stimulate construct clarifications by raising questions as to whether all respondents and items belong to the same respective populations (Wright 1977b; Wright & Stone 1979; Wright & Masters 1982; Smith, Schumacker & Bush 1998).

Since survey responses occur probabilistically and not deterministically, it is useful to evaluate the observed data matrix as response probabilities. Table 2 presents the person-item responses as a P_{ni1} matrix, obtained through substitution of the corresponding β and δ values into (3). The responses of persons *a, b, c, d,* and *e* agree with the model. In each of these cases, responses coded as 1 occurred when $P_{ni1} > .50$, and responses coded as 0 occurred when $P_{ni1} < .50$. The responses of persons *c* and *d* to items *x* and *y* contain a mix of 0 and 1 responses, plausible events considering P_{ni1}=.50. The surprising "1" response of person *f* to item *z* has a .06 probability. The low probability follows from the fact that this person’s measure (-1.1) is almost three logits below the item’s calibration (1.6). The improbability of this observation indicates a contradiction of the measurement intention, and would require further investigation into possible data entry errors, sampling, or construct issues.

Why is it necessary for data to approximate modeled probabilities? For convenience, consider persons *a* and *b* in measure group *p*, *c* and *d* into group *q*, and *e* and *f* into group *r*, as shown in the left side of Figure 1. The β estimates specify that the person relations are $p > q > r$, and the δ estimates specify that the item relations are $w > x = y > z$. According to the matrix of P_{ni1} values, the person order $p > q > r$ is preserved for all items, and the item order $w > x = y > z$ holds for all person measures. In other words, the *person measurement order is invariant, regardless which items are used for measurement*, and the *item calibration order is invariant, regardless of which persons are measured*. Removing responses to items *w* and *y* still preserves the P_{ni1} order of $p > q > r$ for all i, and removing responses from persons with measure *p* preserves the P_{ni1} order for $w > x = y > z$ for all n. These properties satisfy the *independence axiom* of ACM, a condition that requires person measurement to be free from the properties of the measuring instrument, and the properties of the measuring instrument remain invariant no matter which persons are measured.

Though measurement invariance is attained by data adhering to the independence axiom, this property alone does not transform ordinal observations into interval measures. All measures at the quantitative (interval/ratio) level have additive characteristics, so both double cancellation and independence are needed for additivity. Double cancellation is illustrated in the right of Figure 1: Since the antecedents ($px \geq qz$ and $qw \geq rx$) and the consequent ($pw \geq rz$) are satisfied, the probability matrix satisfies double cancellation.

Because of their additive formulations, probabilistic conjoint measurement (PCM) models *always* specify response probability matrices that satisfy independence and double cancellation. PCM's approach is analogous to ANOVA, in that the axioms require "person" and "item" factors to produce *additive and non-interactive effects* on the response probabilities. Item curves (based on P_{ni1} across β) must be parallel with equal slopes. Item response theory (IRT) models that do not specify such curves cannot produce measures with invariance properties (Andrich 1988, 1989; Cliff 1989, 1992; Fisher 1994; Karabatsos 1999a, 1999b; Wright 1984, 1999). Although models that allow item curves to cross (Hambleton & Swaminathan 1985) may be appealing for their accommodation of inconsistent data, the very fact that their structure contradicts the three basic measurement requirements renders goodness-of-fit analysis irrelevant (Karabatsos 1999a). One class of these models takes the formulation α_i (β_n - δ_i), where α_i is the slope of the item characteristic curve at $\beta_n = \delta_i$ (Hambleton & Swaminathan 1985). Such item "weighting" violates independence by distorting the conjoint order of response probabilities (e.g., Table 2). These models thus inherently entail sample-and test-*dependent* measurement.

The degree to which measurement objectivity is attained depends on how well data accord with the measurement principles specified in a PCM model. The standardized response residual represents the basis for all item and person fit statistics:

$$Z_{ni} = (x_{ni} - E_{ni})^2 \Big/ \sqrt{W_{ni}} \tag{5}$$

where Xn_i is the observed response, and E_{ni} is the expected response, and W_{ni} is the variance of the expected response. For dichotomous responses, $E_{ni} = P_{ni1}$ and $W_{ni} = P_{ni1}(P_{ni0})$. Z_{ni} is expected to approximate a N(0,1) distribution. The fit of person and item response sets is calculated using mean-square statistics (MS), which have an expectation of 1. Outfit mean-square (OMS) is the average Z^2 across responses:

$$\text{OMS} = \sum Z_{ni}^2 \Big/ \sum 1 \tag{6}$$

a statistic very sensitive to extreme unexpected responses, i.e., when $\beta_n >> \delta_i$ or $\beta_n << \delta_i$. Infit mean-square is more sensitive to unexpected responses for close β_n and δ_i distances:

$$\text{IMS} = \sum W_{ni} Z_{ni}^2 \Big/ \sum W_{ni} \qquad (7)$$

In common practice, MS$\geq$1.3 diagnoses an item or person with inconsistent responses; MS$\leq$.7 signals overfit, an improbable degree of consistency. Type I error rates for MS statistics are available using the null hypothesis of data-model fit (Smith 1988, 1991; Smith, et al. 1998).

Misfitting items and persons are usually set aside for subsequent analyses, since such data distort the transitivity among person (β) measures and item calibrations (δ). This step in the calibration of a measurement system has been controversial (Goldstein & Blinkhorn 1977; Goldstein 1979; Gustafsson 1980; Phillips 1986; Smith 1986), largely because its focus on the validity of the construct goes against the grain of traditional true-score measurement theory, which places higher value on content validity (Fisher 1994; Cherryholmes 1988; Messick 1975). Because data are commonly deemed valid on the basis of a test or survey's content, the standard paradigm has been to fit models to data, rejecting the model when it does not fit the data. From within this paradigm, changing the data, or the way in which observations are made, in order to obtain model fit has been viewed as an artificially contrived and unethical tampering necessarily unacceptable in scientific work.

But when data are too inconsistent to calibrate a ruler, the researcher should not give up and make do with data that will not support scientific generalization, which is what is often currently recommended (van der Linden & Hambleton 1997). When data must be removed from the process of calibrating an instrument, the item or person involved is not simply deleted. Rather, the characteristics of that item or person are examined for reasons why the measurement effort failed (Wright 1977b, 1999; Wright & Stone 1979; Woodcock 1999). Is the item ambiguously phrased, or does it offer more than one interpretation? Does the item perhaps belong to a different construct that ought to be measured with its own instrument? Was there a data entry error? Was this person mistakenly sampled from a different population, as might be inferred from particular response patterns shared by an identifiable subsample that shares a consistency different from the rest of the sample? Did the respondent mistakenly reverse the rating scale options? Far from being a mindless trashing of data, explanations of these kinds open the door to improving the quality of the data and the precision and accuracy of the measuring instrument.

The degree to which measurement objectivity is attained is also a function of measurement reliability. As a part of model fit optimization, it is also useful to evaluate how well item calibrations discriminate among the measured persons, and vice-versa, using separation (G) statistics (Wright & Masters 1982). The person separation (G_n) ratio describes the number of performance levels that the test distinguishes in the sample of respondents; the item separation (G_i) ratio indicates how well items spread along the variable. G is calculated by:

$$G = ASD / RMSE \tag{8}$$

where RMSE (root mean square error) is the error variance of the measures, and ASD is the standard deviation of the measures adjusted for RMSE, or

$$ASD^2 = SD^2 - RMSE^2 . \tag{9}$$

The relationships between the Kuder-Richardson reliability statistic (R) and separation is

$$R = G^2 / (1+G^2) \qquad\qquad G = [\ R / (1 - R)\ ]^{1/2} . \tag{10}$$

In principle, either R or G can be used. G is more descriptive, since 1) every unit increase represents an equal increment of change in the ratio of variation to error, and 2) G has an infinite range, $0 \leq G \leq +\infty$, while the range of R is constrained in [0,1].

HYPOTHESES

1 The first hypothesis tests whether the data support the contention that the quality of care variable is quantitative (Michell 1990). The quantitative hypothesis will be rejected or provisionally confirmed via tests of model fit, the extent to which the data satisfy the requirements of independence and double cancellation (Luce & Tukey 1964; Krantz, et al. 1971; Michell 1990, 1997), as implemented in a probabilistic conjoint measurement (PCM) model (Brogden 1977; Perline, et al. 1979; Wright 1985, 1997). Experimental procedures for maximizing model fit will be demonstrated in detail.

2 Provided satisfactory model fit is established, then the hypothesis that meaningful substantive relations are associated with hypothesis 1's quantitative structure can be tested. The fundamental issue of interest is construct validity. The empirical item order on the scale is examined in order to answer the question, "Do the data arrange the MEPS QOC item estimates reasonably and meaningfully, so that services with low calibrations are easy to be satisfied with, services with high calibrations are difficult to be satisfied with, and persons with low measures are less satisfied than those with high measures?"

3 Following the investigation of construct validity, the person measures and item calibrations will be checked for robustness via analyses of randomized and demographically defined sub-samples of the data. The hypotheses tested will require that person measures be the same (linearly convertible, with at least a .85 correlation) no matter which particular items are used to produce them, and that the item calibrations remain the same no matter which particular persons respond to them. Item calibration robustness will establish that very large samples are not needed for many measurement and scale development tasks, and that item difficulty order and the substantive meaning of the measures can be interpreted as generally characteristic of the population. Person

measurement robustness will demonstrate that 1) missing data can be accommodated, 2) measurement technology can be adapted to the needs of the respondent, instead of requiring respondents to adapt to the needs of the technology (Choppin 1968; Lunz, Bergstrom & Gershon 1994), and 3) different instruments measuring the same variable could be equated to measure in a single reference-standard quantitative metric (Fisher 1997a, 1997b, 1998, 2000a). If broadly realized in health services research, robust measures could greatly improve the efficiency and meaningfulness of that research, as well as its implementation.

4 Fourth, as an elementary demonstration of the use of PCM logits in a statistical analysis, the QOC measures are studied submitted to an analysis of variance to test an hypothesis concerning variation in amounts of QOC across public and private HMOs, and Medicare. Do persons paying more for their health care experience substantially higher QOC?

5 Fifth, PCM will be evaluated relative to summated ratings and IRT analyses of the same MEPS QOC data. PCM, IRT, and the summated ratings approach are all often used in practice as though they produce interval measures; this study will evaluate the extent to which the results of each actually meet the long-established requirements for fundamental measurement applications in science and commerce, as articulated by Campbell (1920), Fisher (1922), Guttman (1950), Rasch (1960), Luce and Tukey (1964), (Suppes, Krantz, Luce & Tversky (1989), and others, as summarized by Michell (Michell 1990, 1999, 2000) and Wright (Wright 1999).

SURVEY RESPONDENTS AND INSTRUMENT

A PCM model was applied to data from 23,767 families who responded to the Medical Expenditure Panel Survey (MEPS), conducted by the Agency for Healthcare Research and Quality (AHRQ; formerly the Agency for Health Care Policy and Research (AHCPR)) (Cohen, et al. 1996-97). The dataset (HC-002) used in these analyses was downloaded from the AHRQ website in 1998. The analysis focused on 11 survey items pertaining to quality of care (QOC), listed below (item response options are shown in parentheses). Earlier questions in the interview establish the identity of the persons obtaining care from a particular provider. The provider's name is then used in the following questions, and pronouns (they, them) refer to the persons seeking care.

1. [AC17] How difficult is it to get appointments with your provider on short notice, for example, within one or two days? (Very / Somewhat / Not too / Not at all Difficult)
2. [AC18] If they arrive on time for an appointment, about how long do they usually have to wait before seeing a medical person? (>2 hrs./1-2 hrs./31-59 mins./16-30 mins./5-15 mins./<5 min.)
3. [AC19] How difficult is it to contact a medical person at your provider over the telephone about a health problem? (Very / Somewhat / Not too / Not at all Difficult)
4. [AC19A] Does your provider generally listen to them and give them the information needed about health and health care? (No/Yes)
5. [AC19b] Does the provider usually ask about prescription medications and treatments other doctors may give them? (No/Yes)
6. [AC19C] Are they confident in the provider's ability to help them when they have a medical problem? (No/Yes)

7. [AC19D] How satisfied are they with the professional staff at the providers' office? (Not at all / Not too / Somewhat / Very Satisfied)
8. [AC19E] Overall, how satisfied are they with the quality of care received from the provider? (Not at all / Not too / Somewhat / Very Satisfied)
9. [AC24] During the last year, did any family member not receive a doctor's care or prescription medications because the family needed the money to buy food, clothing, or pay for housing? (Yes/No)
10. [AC24A] Overall, how satisfied are you that members of your family can get health care if they need it? (Not at all / Not too / Somewhat / Very Satisfied)
11. [AC25] In the last 12 months, did anyone in the family experience difficulty in obtaining care, delay obtaining care, or not receive health care they thought they needed due to any of the reasons listed on this card [list of 18 reasons shown to respondent]? (Yes/No)

In the original AHRQ documentation, these items are included in the Access to Care (AC) section of the household survey. With each question, respondents were given the opportunity to say they did not have an answer. Note that the set of items employ four different groups of rating categories, three of which use rating scale options. Therefore, for this experimental design, it is necessary to employ a different PCM model for each rating scale, which can be expressed as:

$$\lambda_{ni(j,j-1)k} = \ell n\left(P_{nijk} / P_{ni(j-1)k}\right) = \beta_n - \delta_i - \tau_{jk} \qquad (11)$$

where τ is the response threshold between response category j and j-1, k is the rating category group (k={1,2,3,4}), and $\lambda_{ni(j,j-1)k}$ is the log-odds of response category j being chosen versus category j–1.

It is also important to notice that the rating scales differ in the direction they point. The original coding of item 1 above, for instance, assigned a rating of 1 to very difficult, indicating low QOC, and a 4 to not at all difficult, indicating high QOC. With the very next question in the interview, however, the direction of the coding is reversed, such that waiting times of less than five minutes are rated as 1, and waits of more than two hours, 6.

In order to evaluate the capacity of the ordinal observations to support interval measurement, the ratings need to consistently point in the same direction, so that any rating in a higher category always indicates more QOC. The data were therefore recoded so that a high sum of ratings for a respondent would indicate the overall high QOC associated with a high measure, and a high sum of ratings for an item would indicate that it represents the relatively low QOC hurdle that calibrates toward the bottom of the scale. The order of the codings as they were employed in the analyses is shown in the listing of the items above.

Accordingly, scoring the one 6-point item (2), the five 4-point items (1, 3, 7, 8, and 10), and the five dichotomous items (4, 5, 6, 9, and 11) as consecutive integers, 1-6, 1-4, and 1-2, respectively, results in a minimum possible score of 11, and a maximum possible score of 36, given complete data (responses to all items).

RESULTS

The following results are provided by the PCM program WINSTEPS (Wright & Linacre 1999). Table 3 shows the summary statistics for the 23,767 cases, of which 208 lacked responses, 3,383 obtained the maximum score of 36, and 165 obtained the minimum score of 11, leaving 20,011 analyzable cases. Persons and items with maximum and minimum scores are not analyzable because they provide no information as to their position on the scale, being, in effect, shorter than the first unit on the ruler, or taller than the last unit.

The top part of Table 3 provides a global summary of person (top) and item (bottom) statistics. The item scale is centered at zero (refer to the mean ITEM MEASURE). The average person measure of 2.08 is therefore over two errors (0.85) above the center of the scale, indicating that, relative to the administered items, the sample receives high QOC. The average REAL ERROR for the persons is .85, and is .02 for the items. The small item calibration error is due to the large sample size. As the number of measures increases and the unit of measurement is successfully repeated, error decreases, variation remains constant or increases, and reliability increases.

Hypothesis 1: The Quality of Care variable is quantitative

The first hypothesis is not rejected. As shown in Table 3, on average, the person and item fit statistics are satisfactory, although the variance and range of the mean-square statistics indicate that there are some persons with unexpected responses. In general, however, the independence and double cancellation axioms are not violated. As is described in greater detail under Hypothesis 3a, below, a series of experiments were conducted in order to determine the instrument's optimal characteristics.

In these experiments, model fit was not markedly improved, since it was satisfactory at the start. Modeled reliability was improved from .65 to .82, but this seems to have been inflated by the appearance of a nearly 3-logit gap in the middle of the item calibration distribution. The effect of the gap in the item calibrations on the respondent measures would be to increase variation without simultaneously increasing error; hence the increase in reliability. A less extreme removal of inconsistent observations resulted in no change in reliability, equivalent model fit, and improved construct definition, as presented below.

Hypothesis 2: The Quality of Care items and rating categories fall in a meaningful order and position relative to the measures

This hypothesis is taken up under the subheadings of the overall item hierarchy, the instrument targeting, and the rating scale step hierarchy.

The Item Hierarchy. The second hypothesis is not rejected. Figure 2 provides important information about the QOC construct. The positions of the measures and calibrations in this figure are taken from the first analysis of all measurable 20,011 cases. After these are described, they are compared with the results of the optimized analysis.

The item at the bottom of Figure 2's variable map involves the provider's ability to listen to the patient and provide relevant information. Immediately above this item are two others involving the family's ability to obtain care, and confidence in the provider. These issues constitute the overriding purposes of the encounter, so it seems of some theoretical importance that the measurement of QOC would start here.

At the other end of the scale, at the top of Figure 2, are items concerning waiting time, phone access, and making appointments. Respondents are least likely to rate these items in high categories, suggesting that only the highest quality care providers will receive positive responses in these areas. It is of some interest to note that the items are distinguished along the measurement continuum by general and specific expressions of QOC. With the exception of the easiest item, the bottom seven items are relatively abstract, while the top four involve specific services provided. This contrast should prove of theoretical value in the development of improved QOC instruments.

Since a higher person measure indicates higher QOC, the probability of satisfaction decreases as item calibrations increase. To illustrate, hypothetical responses from fictional people named “Tom,” “Mary,” and “Cindy” were inserted into Figure 2. Mary’s position on the QOC variable (above 6) indicates that she is likely to be satisfied with or agreeable relative to all the items. Cindy’s position (at about –2) on the variable indicates that she is not likely to be satisfied with any of the items. Tom’s position (≈0) indicates that he is likely to be very satisfied with or agreeable relative to items 4, 6, and 9, not satisfied with 2, 3, 1, and 5, with about a .50 probability that he is satisfied with 10, 7, 11, and 8.

When all items fit a PCM model, and are ordered in a meaningful hierarchy, they all measure the same variable. Failures of invariance are invitations to investigate construct validity, with the aim of better understanding and improving the item hierarchy. The MEPS QOC items 2 and 5 provoke responses that are less consistent (MS > 1.2) than the other items’ responses. The other items involve issues that are crucial components of QOC, addressing basic issues of access, provider skill, and information availability. In contrast, responses to questions concerning appointment waiting time (item 2), and whether the provider asks about other treatments (item 5), do not vary with the same consistency as the other items in the scale. From the perspective of item fit, the MS statistics suggest that these two items are not consistent components of the unidimensional QOC construct, and should therefore be set aside for subsequent analyses.

These two items should not automatically be considered ineffective, and mechanically deleted from further consideration. They might 1) be inordinately affected by only a very few especially unexpected responses from persons with extremely high or extremely low measures; 2) be useful for measuring a different unidimensional aspect of QOC; or 3) signal the existence of a special respondent subsample especially interested or uninterested in these aspects of QOC.

Perhaps the item "Provider asks about other treatments" is relatively more important for a group of respondents who have particular conditions with well-known alternative treatments, or for persons with multiple co-morbidities who may be at risk for medication interactions.

And it may be impossible for certain high-QOC patients to have short waiting times in the particular clinics they visit. It could be that these clinics are crowded because of the high quality care they deliver. No matter whether response inconsistencies occur as systematic functions of persons or items, further investigation of the standardized residuals (as calculated in equation 5) is warranted. Factor analysis of the standardized residuals appears to be an approach that is useful for detailed construct investigations (Wright & Linacre 1999; Linacre 1998).

An alternative to setting aside noisy persons/items is the removal of individual responses (single person-item interactions) with high residuals. This is possible since the probabilistic properties of PCM models enable the handling of missing data with little or no consequence to person and item measurements, even though greater numbers of missing responses necessarily increase the error of measurement. Employing this strategy on these data caused the removal of all responses in the most positive category of the most difficult item, involving waits of less than five minutes to see the doctor at appointments. Responses in this extremely positive category were so improbable that they were inherently inconsistent with all other responses. Removing them then caused the item to become less difficult and its relative position shifted from the top of the hierarchy to fourth from the top. In addition, the spread of the other items on the variable increased from about 3 logits to over 7 logits. Almost all of that change, however, was introduced in the form of a gap between the top four and bottom seven items, suggesting that the elimination of the waiting time and other treatment inconsistencies set the stage for a clearer expression of the difference between the more agreeable, general aspects of the construct at the bottom of the scale, and the less agreeable, specific aspects at the top.

The advantage of removing individual inconsistent responses is that the analyst is able to retain as much information as possible from the data set, without the unreasonable dependence on complete person-response sets. For most other approaches to measurement, a missing response leads to the deletion of a person or item from the analysis, or the application of some test-dependent scoring algorithm to replace the missing response(s) (Bernhard, et al. 1998; Hahn, Webster, Cella & Fairclough 1998; Troxel, Fairclough, Curran & Hahn 1998). Within the PCM framework, if it is of concern that missing responses occurred as a function of non-random factors, then the item calibrations can be compared across respondent groups exhibiting different amounts or types of missing responses, in order to detect item bias.

The person-item variable map is considered from a different perspective in Figure 3, which shows the expected responses across all of the items for individual measures. The distribution of all measures is shown at the bottom of the figure, with the number of measures at each point along the logit ruler indicated by the vertical columns of numbers. There are, for instance, 2,004 measures at the mean (about 2.1 logits), indicated by the M at the bottom of the figure (S indicates one SD; T, two).

To take a specific example, Tom, who measures about 0 on the QOC scale, is expected to have a response string, listed in item difficulty order (from the bottom up), of 22232331222 (Tom’s expected responses are underlined in Figure 3). Mary, who experiences higher QOC and a measure of about 4, is expected to respond 2224442445 (Mary’s expected responses are circled.) The various patterns of responses associated with each possible score-measure combination constitute the modeled expectations against which observations are compared.

Because the modeled expectations of consistent variation embody the criteria of sufficiency, Guttman reproducibility, conjoint additivity, and parameter separation (Wright 1999), the difference between those expectations and the observations is of primary value in the estimation of model fit. When any given measure is associated with a model fit statistic in a satisfactory range, observations and expectations conform to one another sufficiently well for the measure, understood as effective within a range defined by plus or minus two errors, to be interpreted as meaningfully approximating the ideal.

Accordingly, Figure 3 could be used as the basis for a redesigned assessment form. For instance, clinician managers could use this type of figure as an assessment form (i.e., KEYFORM) to measure and qualitatively assess QOC at the point of care (Connolly, Nachtman & Pritchett 1971; Fisher, et al. 1995a; Linacre 1997; Ludlow & Haley 1995; Ludlow, et al. 1992; Sparrow, Balla & Cicchetti 1984; Woodcock 1973, 1999). To obtain a patient's QOC measure, the patient simply responds to the questions by circling the appropriate categories, and the provider then draws a line through roughly the middle of the circled responses, and refers to the patient's measure (and error, raw score, percentile rank, or whatever other statistic might be deemed relevant and worth including) at the bottom of the form. Once the analyst has reached the point at which item calibrations are stable, calibrated forms can be provided to physicians, nurses, and other care providers as diagnostic tools. This is one method of integrating measurement methodology with clinical practice, making the enhanced interpretability established via instrument calibration immediately available at the point of care with no need for further time-consuming computerization and data analysis.

Instrument Targeting. The mistargeting of items to persons is illustrated in the person-item variable map in Figure 2, and in Figure 3. More than 61% of the sample is likely to be satisfied with every item (β>1.75), and more than 90% persons measure above the center of the item scale (δ=0). Given the instrument’s 36 response categories, Rasch generalizability theory (Linacre 1993) predicts a measurement error of about .50 for an on-target sample, in contrast to the present sample’s .85 average error. With an average measurement error of .50 and this sample’s measurement standard deviation of about 1.4, one would then expect a person measurement separation of about 2.7 and associated reliability of .88.

Figure 2 shows only the average step difficulty for the items. For the items with dichotomous, yes-no response formats, the sole step difficulty is the same as the item difficulty. But for the five items rated on multicategory scales, calibration estimates are obtained at each step transition. The construct definition and the instrument targeting are then enhanced, in this case only slightly, by the additional information provided by the rating scale step estimates, as shown in Figure 3.

Rating Scale Step Hierarchies. Four different groups of response categories were calibrated. For the purposes of this demonstration, attention will be focused on the “Very Satisfied / Somewhat Satisfied / Not Too Satisfied / Not At All Satisfied” rating scale of items 7, 8, and 10. The analysis of its rating scale structure is provided in Table 4. The “Observed Count” column summarizes the number of responses made within each rating category, and the “Average Measure” column indicates the average person measures responding to each category. The results are satisfactory, as an increasing score on the rating scale corresponds to an increase in both the average measures and the category thresholds. Both MS statistics indicate that there are some unexpected "1" responses, which suggests that the two misfitting items contain responses of 1 when their expected ratings were much higher.

The figure at the bottom of Table 4 illustrates that the category response thresholds (τ_{jk}) correspond to the locations where curves j and j-1 cross. Therefore, -.55 is the response threshold between categories 1 and 2, -.42 is the threshold between categories 2 and 3, and .98 is the threshold between categories 3 and 4, as indicated in Table 4, which means that a higher QOC rating contributes to higher patient QOC measure, as intended.

Results for the remaining items' step structures are shown in Tables 5-7 and are in general satisfactory. Overall, the data exhibit, item by item and step by step, the conjoint response pattern expected by the PCM model. But notice that each of the three rating scales includes one very small step, suggesting that it may be necessary to collapse adjacent ones together.

Collapsing categories is recommended when (a) category responses exhibit statistically significant inconsistencies (MS>1.2), (b) a category has a low observed count, (c) most of the category's response curve lies under an adjacent category's curve, or (d) when the size of the step from the previous category is less than 1.4 logits (Linacre 1999). Such an optimization of the rating scale usually improves fit, and will also improve separation/reliability to the extent that error is lowered relative to variation. In this case, an experimental recoding the data resulted in the elimination of statistically significant inconsistencies, and had no effect on separation/reliability.

Hypothesis 3a: Measures are invariant across item subsamples

Hypothesis 3a is not rejected, but provisionally, with a strong need for improved data quality. The person separation statistic for the initial analysis of all 20,011 available cases indicates that there is about as much error variance as measurement variance (G_n=1.17), as is associated with low person reliability (R=.58). In other words, the highest measure cannot be distinguished from the lowest measure to a statistically significant degree because the instrument lacks the power it needs to spread the respondents out along a number line. In order to give numeric expression to quantitative magnitudes, an instrument should discriminate 2+ measurement levels of QOC (G_n >2, R_n> .80). High stakes applications can require reliabilities above .95, as is required for consistent and reliable reproduction of a measure across instruments.

It is sometimes possible to establish an instrument's maximum possible reliability by removing cases with significant inconsistencies (OMS > 1.4), or with significant amounts of missing data. After the instrument's best performance is observed, the cases that had been removed are replaced, and measures for them are produced by anchoring the items at their established calibrations. If the error introduced by the inconsistencies or the missing data decreases relative to the variation in the measures, reliability increases.

With the MEPS QOC data, however, iterative analyses removing ultimately 9,999 cases exhibiting the strongest inconsistencies had negligible effect on the measurement reliability coefficient. The main effect of these analyses was to remove cases with responses at the extreme low end of the rating scale for the item involving appointment waiting times (i.e., persons indicating that they had waited over two hours before seeing a doctor). This in turn caused the item to drift progressively lower down the scale from its original position as the most difficult indicator of QOC to a new position fourth from the top. In these analyses, the positions of the other items remained stable, but the mistargeting of the instrument was exacerbated, with the mean measure increasing from 2.08 to 3.18.

Figure 4 shows the results of a direct test of measurement invariance across item subsamples. These results were produced by dividing the QOC items into two groups defined by the number of available response categories, and using these as separate instruments each measuring the QOC experienced by the entire (second, reduced) sample of respondents. The overall correlation between the two sets of measures is .71.

Further tests of this hypothesis are summarized in the correlations shown in Table 8. Three subsets of items, chosen based on their difficulty calibrations (low, medium, and high), and two mutually exclusive random sets of items, were calibrated in separate WINSTEPS analyses on the original data set’s 20,011 measurable cases. Correlations range from about .26 to .89. The lowest values are associated with comparisons of item groups that vary greatly in their difficulties (low vs. high), and the highest values, with groups of items that are similar in difficulty (middle, high, and the two random selections).

Overall, the low number of items available for scaling makes it difficult to obtain highly reliable measures. Were there more items, and had they been designed with the intention of establishing invariant measures in mind (i.e., taking up the recommendations of Fisher (2000b) and others), larger subsets of more reliable items would be available, and better able to provide measures in a uniform metric. To obtain the same measure using different brands or configurations of instruments known to measure the same thing would require that the correlations be higher and the associated plots would show a narrower range of variation nearer to the identity line.

Hypothesis 3b: Calibrations are invariant across respondent subsamples

Hypothesis 3b is not rejected, since the item calibrations spread along the scale very well (G_i =51, R_i=1.0). As a demonstration of the scale invariance over respondent subsamples, data from five randomly chosen (with replacement) groups of respondents, and from two other groups distinguished by their measures (high and low), were used to produce seven separate sets of item calibration estimates. Sample sizes for the randomly selected groups were 500, 250, 100, with two other groups of 35. The sample sizes of the high median and low median groups were 4,915 and 6,094, respectively.

Correlations for these groups of item difficulty estimates are shown in Table 9; the values correlated are plotted in Figure 5. Sixteen of the 21 correlations (about 80%) are greater than or equal to .84, and about half of them (10 of 21) are over .90. With the exception of the item at the far right of Figure 5, involving difficulty in receiving health care, the items calibrate in the same order across samples, even with sample sizes as low as 35.

A similar pattern and similar correlations, here averaging about .95, were produced by respondent subsamples defined by their participation in public or private HMOs, or in Medicare (see Table 10 and Figure 6).

Hypothesis 4: Measures do not vary across demographic groups

This traditional statistical hypothesis of no difference in measures across groups is rejected on the basis of a significant F-test (see Table 11). In substantive measurement terms, however, the differences in the mean measures are very small (0.06, 0.27, and 0.33 logits), with all of the differences less than half the average estimated measurement error (.85 for the total sample). In other words, when the differences in the measures are interpreted in terms of an actual change in responses to the questions asked, no such change is seen as likely.

Hypothesis 5: PCM, IRT, and summated ratings are each appropriate in QOC measurement

Though retainable in certain limited respects, overall, this hypothesis has to be rejected. The superior data quality assessment and interval measurement characteristics of PCM, relative to IRT and summated ratings, are irrefutable. A summary of PCM, IRT, and summated ratings features is provided in Table 13. We will now take up a more detailed examination of summated ratings and IRT analyses of the MEPS QOC data.

Summated Ratings. Reconsidering again Figure 3, we see that not only are the distances between adjacent categories variable, but the position of a given category on the ruler changes with the difficulty of the items. Figure 7 shows another view of the data shown in Table 7 for the MEPS QOC item 2, involving waiting time for an appointment. The distances between the step thresholds reported in Table 7 are shown in Figure 7 in such a way that the "foundations of misinference" (Merbitz, et al. 1989) on which so much research is based become graphically apparent. For instance, a change in waiting times that would be expressed as a shift in the average response from 2.5 to 3.5 raw score units corresponds with a one-logit difference on the measurement scale, but the same single unit shift from 4.5 to 5.5 in the expected score corresponds with more than three logits on the measurement scale.

Figures 8-10 display other ways in which the nonlinearity of ordinal scores is misleading.

Figure 8 plots the entire range of possible scores (11-36), given complete data, against their logit-linear transformations (the approximate β estimates for the minimum score 11 and the maximum score 36 are based on hypothetical scores of 11.3 and 35.7). The relationship takes the shape of an ogive since scores are always bounded by their minimum and maximum values, while their associated logit estimates range from -∞ to +∞. Figure 8 shows the consequences of using nonlinear raw scores for statistical inference. A 2-point difference from 21 to 23 translates to a 1-logit increase, but the same 2-point gain from 32 to 34 is a 2-logit increase. Hence, the score difference between 32 and 34 is quantitatively twice more than the apparently equal difference between 21 and 23.

The crux of Likert's (1932) argument against Thurstone was that, with complete data, the middle of the score distribution is nearly additive. The problem that is especially pertinent to the mistargeted MEPS QOC instrument is that the natural logarithm's two-stretch transformation of the score distribution causes the size of a raw score unit to vary by up to 400% on the logit metric (Wright 1999) depending on where that unit is positioned. That is, the difficulty of advancing a single score unit at the extreme ends of the measurement continuum is much more challenging than it is in the middle of the ruler. Raw scores from different parts of the ruler are almost always compared with no cognizance of this difference, so the inferences made are inherently flawed.

Figures 9 and 10 show plots of all raw scores versus their associated logit measures for the measures and calibrations, respectively. These figures highlight the effect of missing data on the raw scores. Notice that the upper edge of the points plotted in Figure 9 represents the same complete-data curve shown in Figure 8. The points below that upper edge represent score groups formed on the basis of responses to fewer than all of the available questions on the instrument. Although competent users of survey instruments know better than to compare raw scores summed from different collections of questions, this plot draws attention to the inconvenience of methods unable to account for missing data, and the advantages of odds ratios as a component of fundamental measurement theory and practice for adapting instruments to the needs of people, instead of forcing people to adapt to the needs of instruments.

Without checking the internal consistency of the data, users of the summated ratings approach also have no means of determining whether raw scores are sufficient statistics, or if important information about the variable is left embedded in interactions hidden in the data. Unfortunately, reliability statistics are often mistakenly interpreted as measures of internal consistency (Green, Lissitz, & Mulaik 1977; Ottenbacher & Tomchek 1993), and the more appropriate mean square model fit statistics (Smith, Schumacker, & Bush 1998) are rarely used.

Table 12, taken from the WINSTEPS output, lists the MEPS QOC items provoking the least consistent responses. As shown in Figure 2, the items with the highest OMS (Outfit Mean Square) statistics, concerning items 5 (the provider's questions about other treatments), and 2 (appointment waiting time), calibrate in the upper half of the difficulty distribution, at 0.66 and 1.59 logits, respectively. Two particularly unexpected responses of 1 (No) to item 5, the worst fitting item, stand out in Table 12. These responses come from respondents 21388 and 21387 (as can be seen by reading the vertical column of numbers immediately above the responses in Table 12).

Respondents 21388 and 21387 provided identical answers to all questions, and have the same measure of 3.5, IMS of 3.3, and OMS of 9.9, based on responses to all 11 items and raw scores of 34. Table 12 also shows that these same two persons also responded in category 6 to item 2, indicating that they typically wait less than five minutes to see the doctor when they arrive on time for an appointment. An additional inconsistency indicated in Table 12 are the responses of No to question 9, concerning the ability of everyone in the family to obtain needed care. Examination of an additional table available in the WINSTEPS output indicates that these are the only observations affecting these two respondents' measures to a statistically significant degree.

Referring again to Figure 3, we see that the step threshold for category 1 (No) on item 5, the point along the measurement continuum where this response to this item is expected relative to category 2 (i.e., there is a greater than .50 probability of a No response), is at about 0.6 logits, with the probabilities of a negative response increasing for lower measures (to the left), and decreasing for higher measures (to the right). With measures of 3.5, respondents 21388 and 21387 are over 2.9 logits above the point on the scale where responses of No to item 5 are expected, so the odds of these observations occurring are more than 20 to 1 against them (see Wright & Stone 1979, p. 16, for a table relating logit differences to response odds and probabilities).

Even more dramatic is the difference between the measures of 3.5 and the response of No to question 9, involving a difference of about 4.5 logits and contrary odds of more than 55 to 1. Similarly, Table 7 and Figure 3 show that the expected measure for respondents using category 6 on item 2 is about 6 logits. Respondents 21388 and 21387 are then 2.5 logits below the item's calibration at the step from category 5 to 6, and the odds of these responses occurring are then less than 1 in 10.

Improbable response inconsistencies become evident only in a frame of reference established by a theory of measurement that demands sufficient statistics, conjoint additivity, and parameter separation. As shown in Figure 3, measures in the range of 3 to 4 logits are not generally associated with responses indicating that the care provider does not ask about other treatments, or that not everyone in the family has access to care. Were questions such as these routinely asked of persons seeking care, in a touch-screen kiosk or scannable form environment utilizing a survey formatted so as to graphically convey the essential information (Connolly, Nachtman & Pritchett 1971; Woodcock 1973, 1999; Wright & Stone 1979, Chapter 8; Sparrow, Balla & Cicchetti 1984; Fisher, et al. 1995a; Linacre 1997; Ludlow & Haley 1995; Ludlow, et al. 1992), response inconsistencies could be quickly identified and used as a basis for further investigation at the point of care, perhaps identifying and preventing errors and omissions before they happen.

PCM model fit statistics then not only serve the purpose of flagging individuals with unusual interpretations of the item hierarchy, but the also identify sub-samples of respondents with common critical characteristics. These responses provide an example of the way in which PCM analysis can provide a detailed account of person-item response patterns, and can facilitate the investigation of random and systematic errors, possibilities not available in the summed ratings environment.

Item Response Theory. Though there are some similarities that hold up across Item Response Theory and PCM (Rasch measurement), the criteria defined by fundamental measurement theory separates implementations of these approaches into two very different groups according to their application of various requirements for rigorous, meaningful quantification. Unfortunately, it is often nearly impossible to predict a research report's relation to that criterion from the key words chosen for use in the title and abstract, with IRT or latent trait theory, for instance, sometimes encompassing Rasch models and conjoint measurement, and sometimes not.

Even more troubling is the fact that the approaches' various relative strengths and weaknesses are often mistakenly associated with the wrong approach, as when IRT's need for large sample sizes is attributed to Rasch's probabilistic conjoint measurement models, and when the parameter separation characterizing fit to one of Rasch's models is attributed to IRT models (Streiner & Norman 1995; McDowell & Newell 1996; McHorney 1997; Revicki & Cella 1997), mistakes that are made even though the correct attributions are readily available in the IRT literature (Hambleton & Cook 1977; Lord 1983). Finally, even when these errors are avoided, there are so few models for conducting and reporting studies of these kinds that the quality of the work performed is widely variable, to the point that reliability statistics are left unreported

(Haley, McHorney & Ware 1994), and the additive, equal-interval measures resulting from a reported Rasch analysis are abandoned in favor of the nonlinear, ordinal raw scores in statistical comparisons (Whiteneck, Charlifue, Gerhart, Overholser & Richardson 1992).

The dominant paradigm in survey research is statistical, meaning that the typical approach to mathematical modeling is to fit models to data, instead of vice versa (Meehl 1967; Guttman 1985, Michell 1986). The goal in this paradigm is to describe data with the model that fits best, and that is best able to account for variation in the data by adjusting for the effects of interactions between variable characteristics of the questions asked or of the respondents. Application of Item Response Theory models (Hambleton & Swaminathan 1985; van der Linden & Hambleton 1997) then focuses on accounting for variance by adding parameters intended to describe the functioning of the items, but which also sacrifice statistical sufficiency, conjoint additivity, and parameter separation, in the process.

The alternative, measurement paradigm, in contrast, is prescriptive rather than descriptive. It accordingly specifies models possessing the characteristics necessary for objective scientific inference and the calibration of quantitative constants, and fits data to these. Now, in this paradigm, in instances of less than optimal data-model fit, the data, not the model, are considered suspect (although alternative models that maintain sufficiency, additivity, etc. should certainly be considered).

It is well established that the mathematics of summated ratings and IRT parameter estimates make them insufficient for establishing the bases of comparison required for accountability and patient-centeredness in health care since their axioms limit their empirical possibilities to sample- and instrument-dependent results (Andrich 1988, 1989; Embretson 1996; Karabatsos 1999a, 1999b; Wright 1984, 1999). The phenomenon of over-parameterized models is not unique to IRT, but has been recognized as a problem whenever data are described so precisely that their value as support for general conclusions is destroyed (Busemeyer & Wang 2000).

Explanations for the continued popularity of IRT and summated ratings models may also extend beyond the logic of human sciences research methodologies to cultural assumptions about the nature of objectivity and existence (Fisher 1992b, 1994; Michell 1990, 1999, 2000). For instance, the IRT focus on fitting models to data may be motivated by the mistaken assumption that it is not possible to modify scales without making new data incommensurable with old. On the contrary, however, because PCM models are probabilistic they can account for missing data and could play a vital role in bringing continuous quality improvement methods to bear on tests and surveys (Wright & Stone 1979; Holm & Kavanagh 1985). This feature is in fact the basis for recent advances in adaptive measurement and instrument equating (Lunz, Bergstrom, & Gershon 1994; Wolfe 2000). The following analyses are undertaken in the hope that some may be swayed by empirical comparisons of the PCM and IRT methods.

Among the most well known IRT models are the 2-parameter logistic (2PL) and the 3-parameter logistic (3PL) (Birnbaum 1968; Hambleton 1983; Hambleton & Swaminathan 1985; Hulin, et al. 1983; Lord & Novick 1968; Lord 1980; van der Linden & Hambleton 1997). Limiting attention to dichotomous outcomes, 2PL specifies that the probability of person n responding positively to item i is governed by

$$\Pr[\, x_{ni} = 1 \mid \beta_n, \delta_i , \alpha_i \,] \equiv e[\alpha_i (\beta_n - \delta_i)] / \gamma_{ni} \qquad (12)$$

where α_i is a second item parameter denoting the discrimination index of item i, and $\gamma_{ni} = 1 + e[\alpha_i (\beta_n - \delta_i)]$. The inclusion of the discrimination parameter is motivated by the idea that some items discriminate between lower and higher ability groups more effectively than others. 3PL includes the pseudo-chance parameter (η_i) of item i,

$$\Pr[\, x_{ni} = 1 \mid \beta_n, \delta_i , \alpha_{i,} \eta_i \,] \equiv \eta_i + (1 - \eta_i)[e[\alpha_i\, (\beta_n - \delta_i\,)] \,/\, \gamma_{ni}] \qquad (13)$$

Here, η_i is an adjustment for low ability persons improbably obtaining correct responses to difficult items.

One source of misunderstanding comes from the argument that the Rasch PCM models are a “special case” of 2PL and 3PL models. The logic of this argument follows from the fact that, when there are no surprising responses ($\eta_i = 0$), and all items have equal discrimination ($\alpha = 1$), 2PL and 3PL mathematically reduce to a Rasch PCM model. From this perspective, 2PL and 3PL are seen as offering more explanatory power than PCM models, because 2PL and 3PL do not rest on the “restrictive” assertion that all items have the same discrimination, and that guessing is minimal (Hambleton & Swaminathan 1985).

PCM advocates, however, show that 2PL and 3PL estimates are not based on sufficient statistics, and that the discrimination parameter cannot be estimated independently of the person parameter (Smith 1992; Wright 1977a, 1984, 1999). As a result, 2PL/3PL estimates will often not converge (Stocking 1989; Hambleton & Cook 1977), which requires an upper limit to be imposed on α (to prevent it from approaching infinity), and the assumption that β_n is a normally-distributed random variable. Furthermore, values of the discrimination parameters are symptomatic of item bias against different examinees (Masters 1986), which means that bias can go undetected within this framework. In addition, 2PL and 3PL demand very large samples of persons and many test items (Hambleton 1983; Lord 1983), and even then, there is doubt as to the accurate estimation of the pseudo-chance parameter (Hulin, et al. 1983).

These problems, however, are overlooked by many 2PL and 3PL practitioners. In fact, the design of these models to fit problematic data is seen as an advantage over PCM models by IRT:

> A controversial aspect of analyzing test items for model fit has always been whether the case of a misfitting item should lead to removal of the item from the test or to a relaxation of the model. In principle [according to IRT], both strategies are possible....*a relaxation of the Rasch [PCM] model is available in the form of the two-and three-parameter logistic models, which may better fit problematic items* (van der Linden & Hambleton 1997, p. 12).

From the perspective of the rigorous axioms of ACM theory, however, to “relax” the requirements of measurement to “better fit problematic items” amounts to giving up the measurement effort. Preliminary results (Karabatsos 1999a, 1999b) show that the probability values estimated by 2PL and 3PL cannot possibly satisfy either independence or double cancellation, from which it follows that 2PL and 3PL *cannot* construct interval or ratio scale measurement. This is because the presence of α or η within the models severely distorts the scalability of persons and items. These findings confirm Cliff’s (Cliff 1992, p. 188) hypothesis:

> I have argued (Cliff 1989) that the 3-PL model cannot define interval scales of ability because the model itself contradicts the order-consistency principle [independence]. The presence of the discrimination parameter means that item difficulty order changes with ability; the probability of a correct response is not in the same order for all persons. Thus, order consistency is contradicted, so interval scaling is not possible. Such a difficulty does not arise if there is only one item parameter (Roskam & Jansen 1984), leading to the Rasch [PCM] model.

Furthermore, IRT models cannot provide instrument-free measurement because they incorporate parameters, intended to describe item functioning, that “destroy the possibility of explicit invariance of the estimates” (Andrich 1988, p. 67). Or, as another researcher (Wood 1978, p. 31) has put it, "I am persuaded by Lumsden (Lumsden 1978) that two- and three-parameter models are not the answer - test scaling models are self-contradictory if they assert both unidimensionality and different slopes for the item characteristic curves,” meaning that scale-dependencies can and do remain in data fitting IRT models with two or three item parameters.

Table 13 facilitates detailed comparison of specific features of PCM, IRT, and summated ratings analyses as typically performed using available software.

Comparison of the PCM and IRT approaches to measurement began by attempting to establish the extent to which 1PL IRT models produce measures equivalent to those produced by PCM. From there, more than 25 additional IRT models involving various numbers and combinations of item parameters, constraints, estimation methods, and MEPS item groupings were specified and the parameters estimated. All IRT results were obtained using the MULTILOG program (Thissen 1991). All analyses were of the 23,767 available MEPS QOC cases, less the 208 with no data, meaning that 23,559 cases were actually in use for most analyses.

Figure 11 shows a slightly curved, but narrow and nearly linear, relationship between measures made using the WINSTEPS PCM software, and those made fitting a 1PL IRT model (Samejima’s Graded Response model) using MULTILOG. For the 1PL model, discrimination was held constant at 1.0, and the guessing parameter was not estimated. The PCM measures have a wider distribution, and the measures are not on the identity line, but the correlation is very high (.96).

Figure 12 shows a similar comparison, but in this case the MULTILOG analysis had the item difficulty estimates anchored at the WINSTEPS values. With this extra information in hand, MULTILOG was able to effect a near-perfect repetition of the WINSTEPS measures, though the correlation remains the same as it was for the first comparison (.96).

Figure 13 shows the results obtained when the 1PL model's discrimination was set to 1.8 for all items, employing the average discrimination actually obtained across the items. In this comparison, the variation in discrimination shows an effect on the measures, with a looping deviation in the distribution accentuating a smaller and less noticeable similar shift that was present in the first comparison (Figure 11). Again, the correlation between the two groups of measures is quite high (about .95).

Additional comparisons included a plot of the MULTILOG and WINSTEPS estimates for the individual step transition parameters of a partial credit model. The 24 steps included in the analysis correlated .83 (p<.01).

Because MULTILOG can estimate 2PL and 3PL parameters only for dichotomous data, data from the six multicategory MEPS QOC items were recoded. Figure 14 shows again that the two programs produce very similar results in the context of the IRT 1PL model, with a virtual 1.0 correlation between the item calibration estimates. The estimates are not on the identity line, but are parallel to it, because of differences in the way the two programs center the scale.

Figure 15 shows a comparison of the measures made by the two programs on the dichotomous data. Though the 1PL measures fall in a 4-logit range, and the PCM measures in a nearly 10-logit range, the two sets of measures in general fall together along a very narrow line. The measures made using the WINSTEPS and MULTILOG 1PL dichotomous and mixed models were generally consistent with themselves and each other, with high correlations.

Figure 16 shows the item calibrations from WINSTEPS plotted against 2PL MULTILOG estimates. Note that the introduction of the discrimination parameter has affected the difficulty estimates of five items in the lower left corner of the plot to the point that these five items have nearly reversed their difficulty order. These are the five items that were originally formatted as dichotomies. It may not be surprising to find that the discriminations for these five questions are different from those of the rating scale items, but the question that has to be answered is whether it is ultimately more productive to incorporate the variation in data consistency in the item difficulty estimates, or to require a single consistency and pursue deviations from expectation by means of residuals analyses and fit statistics.

Figure 17 shows the calibrations from WINSTEPS plotted against 3PL MULTILOG item difficulty estimates. Allowing the difficulty estimates to be affected by unexpected extreme responses has reversed even more the direction of the five originally dichotomous items than was the case in the 2PL analysis.

Figure 18 shows the measures from the WINSTEPS and 2PL MULTILOG analyses. In contrast with Figure 15’s comparisons involving the measures made using a 1PL IRT model, this plot shows that significant additional variation in the measures has been introduced by the 2PL discrimination parameter.

Figures 19 and 20 plot the 3PL measures against the WINSTEPS PCM and the 2PL measures, showing that additional nonlinearities are introduced by the addition of the third item parameter. Both figures show a floor effect evident in the 3PL measures, and Figure 20’s plot of the 3PL and 2PL measures shows a horizontal line crossing the middle of the rest of the distribution, apparently evidence of an inordinate adjustment made to the measures that include the inconsistent responses to items 5 and 2, noted in the model fit analysis above.

The problem of knowing which item difficulty order is the right one in the context of IRT's multiple item parameters is aggravated by the fact that the discriminations, and so also the item difficulty and person measurement orders, do not remain constant across models or data input methods. Figure 21 is a plot of the discrimination values obtained for the MEPS QOC items when different data input methods were used with the same 3PL model, and Figure 22 is a plot of the discrimination values obtained by the 2PL and 3PL models when the same data input method was used. In both instances, discriminations vary markedly; by necessity, so then also will the associated item difficulty estimates vary accordingly.

RECOMMENDATIONS FOR MEPS QOC INSTRUMENT IMPROVEMENT

Instruments should be designed so as to produce internally consistent data with a high ratio of variation to error (Fisher 2000b). In general, these features are easier to achieve if:

- the items each address a single concept, and so do not involve dependent (if-then) clauses or conjunctions (and, or, but), since these introduce multiple concepts that cannot later be disentangled from the data;
- the items all share a common response structure, since
    - respondents can be confused by frequent changes in the available categories; and
    - effective variation in the responses is easier to plan in this framework;
- the needed reliability is determined in advance, and the number of items and response categories included in the survey are chosen so as to provide that reliability or better, according to the well-known mathematical relationship between variation and error (Linacre 1993); and

- if items are intentionally written so as to provoke even respondents with very high or very low measures to use all of the available response options.

All of these recommendations are relevant to the measurement of QOC as achieved by the MEPS. The MEPS has particular problems with targeting and reliability, problems that could be effectively addressed by rethinking the questions asked in light of these analyses, analyses of similar instruments (Fisher & Karabatsos 2000a, b; Karabatsos & Fisher 2000), and PCM theory.

DISCUSSION

Though the MEPS QOC items generally fit the PCM model, their calibrations need to better target the sample, as is indicated in Figure 2 and by the low person separation/reliability. This "negative" result does not mean that the measurement effort should be abandoned. Rather, the information provided by this analysis should be used to improve the instrument. For instance, additional difficult items, representing higher degrees of QOC, should be constructed, for the top of the measurement range. Such items would enable the survey to better target the sample, increasing person separation reliability and reducing opportunities for wildly inconsistent responses by lessening the overall difference between the measures and calibrations.

The theoretically achievable reliability of .88 should be attainable with no increase in the number of items or survey categories. All items calibrating at levels lower than one standard deviation below the mean of the person measures should be dropped or modified such that an equal number of items that calibrate at levels above the mean of the person measures are created. The desired effect might be achieved simply by rephrasing existing questions or by adding more response options, such as “Extremely Satisfied”, with the aim of making it more difficult for respondents to employ the most positive category.

The summed ratings method fails to extract most of the relevant and useful information available in the rating scale data. Raw scores misdirect inference on the supposition that the ordinal scores can be safely treated as interval. They are also unable to tolerate missing data, which puts out of reach practical advantages such as instrument equating, adaptive administration, and the mediation of population-based construct differences.

The most important point relevant to the summated ratings method is that the arithmetic logic of most statistical comparisons of sum scores requires that the intervals between rating categories be evenly spaced. Surveys are often designed to suggest as much in the way they are printed so as to represent the inter-category intervals as equally spaced. But the summing of scores into a total has repeatedly been shown to be meaningful only when the numbers added up represent something that exhibits a consistent structural magnitude (Fisher 1922; Thurstone 1926, 1928; Guttman 1985; Wilson 1971; Andersen 1977; Cliff 1982, 1989; Wright 1985, 1999; Mundy 1986; Michell 1986, 1990, 1997, 1999; Merbitz, et al. 1989).

It may, of course, be impossible to design rating scales capable of performing so as to exhibit the needed additive properties without additional treatments, primarily involving the application of the natural logarithm to the distribution-free transformation of the raw scores into odds ratios (Rasch 1960; Andersen 1977; Wright 1977b, 1985, 1999). Maor (1994) documents the many different aspects of mathematics that the number *e*, on which the natural logarithm is based, ties together: the law of compound interest in finance, the spirals of snail shells and galaxies, equal-tempered musical scales, the area under a hyperbola, the Weber-Fechner psychophysical law of perception, and the golden ratio exhibited in the structural proportions of leaf patterns, the human body, and classic art. We should not be surprised to find that the number *e* and the natural logarithm appear again in the structures of survey-based measurement variables.

The comparisons of item and respondent subsample scalings reported under the headings of hypotheses 3 and 4 present the rarely appreciated point that high reliability is not desirable for purely theoretical reasons, but for the pragmatic end of providing reproducible measures. Two instruments measuring the same thing will produce the same measure for the same amount of the variable only within the range of error (i.e., to the extent that their measurement precision (reliability) allows for it). When a low number of poorly designed items taken altogether measure with a reliability below .70, as do the MEPS QOC items, and these are then divided into two groups that measure with even lower reliabilities, low correlations (.50 - .70) between the resulting sets of measures is to be expected.

When, in contrast, highly reliable calibrations of .94 to .96 are obtained for two different instruments measuring the same thing, it can be expected that the measures produced will be quite stable, and will correlate highly. The value of the Rasch separation reliability statistic (Wright & Masters 1982) becomes apparent in this context. For a reliability of about .80 and separation of 2.0, subsample comparison studies can be expected to produce correlations of about .75 to .80, since, as the separation statistic indicates, the relationship of variation to error in the measures causes the measures to vary within the bounds defined by two groups, one high and one low. The two groups lie like oblong, slightly overlapping ellipses, end-to-end with one another on the measurement continuum, with centers 4 errors apart. The large amount of error relative to the variation in the measures means that treatment effects would have to be very large to be detectable. Even in the context of relatively imprecise measurement, it remains nonetheless true that, as long as there is more variation than error (and separation is greater than 2), measures from different instruments experimentally established as measuring the same thing will fall within four errors of each other more than 99% of the time.

But consider now a different situation involving two instruments addressing the same construct, and with measurement reliabilities of .97 and separations of 6. These instruments define six overlapping ellipses with centers 4 errors apart spread along the ruler. Just as was the case with the lower reliability, more imprecise instruments, a measure made with one of the highly precise instruments can be expected to fall within four errors of a measure of the same amount from the other instrument better than 99% of the time. The advantage gained from the greater precision is not only that different instruments provide more nearly the same measures for the same amounts, but that the same numerical difference in imprecise and precise measurement contexts will be more interpretable in the precision context, since there will be more items articulating the substantive meaning of the score differences all along the measurement continuum. Thus the power to make highly reliable quantitative distinctions not only makes the detection of faint effect sizes much more likely, but it also makes them more meaningful, since the presence of multiple items provoking the score differences provide a language and theory of the variable.

But when results are as uncontrolled as those found in the application of the multiparameter IRT models, where are criteria for knowing which difficulty order is right to come from? The only place they can come from is from the identification of constant, additive relationships capable of supporting generalizable comparisons. In actual practice, multiparameter IRT estimation procedures require the estimation of a PCM model every other iteration in order to prevent the estimates from diverging to infinity (Stocking 1989). And when instruments have to be equated so that measures can be compared, it is again PCM, in effect, that is put to use, since the need for stable estimates across samples is the well-accepted point of equating. If we are then to engineer quantitative measures of QOC, functional status, quality of life, or other variables that effect what Roche (1998) calls the full integration of measurement and mathematics in scale-free metrics, there would seem to be no point in employing analytic methods and theories like IRT and summated ratings that do not efficiently facilitate the realization of that goal.

Both the history of measurement science (Heilbron 1993, Roche 1998) and developmental psychology (Bergling 1999; Dawson 2000) show that understanding evolves over time through interaction with objects. Interactions with the things themselves provoke qualitative cognitive and social shifts in understanding and behavior. Given acceptance of the criteria for fundamental measurement essential to science and commerce, the hypothesis that PCM, IRT, and summated ratings approaches each have an appropriate place in QOC measurement could be accepted, with qualifications, in the framework of an evolutionary perspective.

A paradigm shift similar to the one that provoked what Kuhn (1961) calls the "second scientific revolution" in the 19th Century may be occurring in the world of measurement methods based in tests and surveys. For instance, even though raw scores are nearly linear in the presence of complete data and in the middle of the score distribution, PCM makes it possible to obtain linear measures in the presence of missing data and across the entire range of the distribution. Even though measurement via tests and surveys seems to have functioned well enough without tests of the quantitative hypothesis, and without routine data quality assessment, perhaps demands for accountability will make those omissions less tolerable. And even though scale-dependent metrics may not have seemed terribly cumbersome over the course of the 20th Century, in the context of the 21st Century's ubiquitous computer networks and streamlined communications, scale-free metrics will soon become the new norm.

Similarly, even though the 1 PL IRT measures and calibrations are statistically identical with the PCM measures and calibrations, the IRT analysis does nothing to test Hypothesis 1, above, that the QOC variable is quantitative, since no data-model fit tests of independence and double cancellation are provided. It is also true that, in the contexts of the 2PL and 3PL models, as shown below, tests of Hypothesis 1 would be inconsistent with the mathematics structuring the estimation process, since the tests would be checking for the invariance that the models overtly compromise by shifting the item difficulty order according to the discriminatory power of each individual item. IRT criteria for model choice are accordingly based in the statistical capacity of the models to account for variation in the data. When applied to the MEPS QOC data, the 3PL and 2PL IRT models clearly fit better than the 1PL model did, with respective chi-square statistics of -293,899, -293,209, and -288,498.

These statistical results are confounded, however, by the plots clearly showing that the ordering and meaning of the measures and calibrations produced by the 2PL and 3PL IRT models vary in arbitrary and uninterpretable ways. Rather than implementing the long-proven methods of science that value measurement for revealing anomalous failures of invariance, and so facilitating data quality assessment and the identification of new constructs requiring investigation (Kruskal 1960; Kuhn 1961, Wimsatt 1981), IRT integrates anomalous interactions into the measures and calibrations, concealing unexpected observations deep within the results and destroying the possibility of quantitative comparability (Embretson 1996).

The IRT analysis also falls short of the PCM standard in not providing the graphic displays of the construct needed for assessing the interpretability of the measures and calibrations.

Referring again to those displays, in Figures 2 and 3, the overall MEPS QOC item order indicates that matters of access to care are of more concern to respondents than matters pertaining to the provider. Though not yet widely recognized, a general pattern in QOC and satisfaction with health care services has been observed across several independent studies (Angelico & Fisher 2000a, 2000b; Fisher 1992a; Fisher & Karabatsos 2000a, 2000b; Karabatsos & Fisher 2000). This pattern indicates that 1) the highest quality of care or satisfaction with services is usually associated with the physician; 2) somewhat less quality is associated with nursing, allied health staff, and general access to care; 3) less quality yet is found with regard to office staff, making appointments, and telephone access; and 4) the least quality is associated with waiting room comfort and cleanliness, and parking availability.

Plainly, the existence of this continuum does not imply that waiting room comfort and cleanliness ought to be improved so as to provoke ratings equivalent to those obtained by the physician. On the contrary, in the spirit of continuous quality assessment and improvement (McLaughlin & Kaluzny 1999), the theoretical stability of the QOC continuum across instruments, providers, and health care consumers ought to be employed as the criterion reference against which quality improvement efforts are evaluated. Instead of adopting the quality control approach's futile efforts to cut off the low quality tail of the measurement distribution, the point is instead to move the entire distribution up the ruler to an overall higher quality.

Should this as yet informally documented pattern prove to be stable across brands of instruments, as has been found to be the case with clinician-assigned ratings of physical functioning (Fisher 1997a, 1997b; Fisher, Harvey, Taylor, et al. 1995), it should be possible to calibrate a reference standard metric to which all instruments measuring QOC would be traceable, using methods adapted from the existing standard procedures of metrology (Mandel 1977 1978; Wernimont 1977, 1978; Pennella 1997).

CONCLUSION

Although the MEPS QOC instrument will need certain refinements to optimize its functioning, PCM analysis provides clear evidence that fundamental measurement systems can be created for QOC variables.

Given a sufficiently reliable QOC measurement system, it should be possible to equate different QOC measures together so as to make them traceable to a reference standard metric. Fisher and Karabatsos (2003) compare PCM analyses of the MEPS QOC items with similar analyses of the QOC-specific items from the Consumer Assessment of Health Plans Study (CAHPS, version 2.0 Adult Core items), also funded by AHRQ. Given that there are no items common to both the MEPS and the CAHPS, and that the MEPS measurement reliability is so low, equating would probably not be viable. As an alternative, a new instrument has been designed according to the PCM principles described above and following the content included in the MEPS and CAHPS QOC scales. Research on this scale continues.

In practice, much research presented in the context of IRT, Rasch measurement, latent trait theory, or conjoint analysis is, or strives, in effect, to be PCM. But the hypotheses tested in these reports vary widely, as does the quality of their execution. The present report itself is undoubtedly far from perfect, but it is hoped that it may be taken as a contribution toward the development of improved research standards in the human sciences.

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		ITEMS				Person	
		v	*w*	*y*	*z*	Score	
						(1a)	β_n
P	*a*	1	1	1	0	3	1.1
E	*b*	1	1	1	0	3	1.1
R	*c*	1	0	1	0	2	0
S	*d*	1	1	0	0	2	0
O	*e*	1	0	0	0	1	-1.1
N	*f*	0	0	0	1	1	-1.1
S							
Item Score	(1b)	5	3	3	1		
	δ_i	-1.6	0	0	1.6		

Table 1. Six persons' responses to a 4-item survey.

		ITEMS				
		v	*w*	*y*	*z*	β_n
P	*a*	.94	.75	.75	.38	1.1
E	*b*	.94	.75	.75	.38	1.1
R	*c*	.83	.50	.50	.17	0
S	*d*	.83	.50	.50	.17	0
O	*e*	.63	.25	.25	.06	-1.1
N	*f*	.63	.25	.25	.06	-1.1
δ_i		-1.6	0	0	1.6	

Table 2. The data in Table 1 expressed as probabilities.

	Person Summary (N=20,011)				*Item Summary (I=11)*			
	Measure	Error	IMS	OMS	Measure	Error	IMS	OMS
Mean	2.08	.85	.93	.88	0.00	.02	.99	.93
S.D.	1.42	.37	.74	1.10	1.02	.01	.14	.29
Min.	-2.95	.43	.00	.00	-1.49	.01	.79	.44
Max.	5.04	4.07	5.60	9.9	1.59	.04	1.28	1.55

ASD = 1.08	RMSE = .92	ASD = .02	RMSE = 1.02
Separation = 1.17	Real Reliability = .58	Separation = 51	Real Reliability = 1.0
(Model Reliability = .65)			

Table 3. Summary statistics for measures and MEPS QOC items.

Items:
7. Satisfaction with staff
8. Satisfaction with QOC
10. Satisfied that family members can get care when needed

Response Category		Observed Count	Avg. β_n	IMS	OMS	Category Threshold (τ_{jk})
1	Not at all satisfied	955	.12	1.25	1.49	
						-.55 (1 to 2)
2	Not too satisfied	2,290	.49	.80	.66	
						-.42 (2 to 3)
3	Somewhat satisfied	11,857	1.48	.83	.63	
						.98 (3 to 4)
4	Very satisfied	40,274	2.91	.83	.90	

```
        CATEGORY PROBABILITIES: MODES - Step measures at intersections
P      ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
R  1.0 +                                                    444444444+
O      |111111                                        444444         |
B      |      111                                 4444               |
A      |         11                             44                   |
B   .8 +           11                         44                     +
I      |             1                       4                       |
L      |              1                    44                        |
I      |               1                  4                          |
T   .6 +                1                4                           +
Y      |                 1              4                            |
    .5 +                  1            4                             +
O      |                   1     33333*                              |
F   .4 +                   1   33    4 33                            +
       |                    * 3     4    3                           |
R      |                2222 *22   4      33                         |
E      |              22    3 1 224         33                       |
S   .2 +            22    33   1 422          33                     +
P      |          22     3      *   22          33                   |
O      |      2222     33     44 11   22          333                |
N      |222222     3333    444     111  2222         3333333         |
S   .0 +***********44444444           111111*************************+
E      ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
       -4    -3    -2    -1     0     1     2     3     4     5     6
                       PERSON [MINUS] ITEM MEASURE
```

Table 4. Rating scale structure analysis, items 7, 8, & 10

Items

4. Provider listens and gives information
5. Provider asks about treatments other doctors may give
6. Confident in provider's ability
9. Everyone in family received care
11. No difficulty receiving health care

Category Number	Category Label	N	Average Measure	IMS	OMS
1	No	9,773	1.30	1.04	.97
2	Yes	82,421	3.13	1.07	1.05

```
P      ++------+------+------+------+------+------+------+------++
R  1.0 +1111111111111111                         2222222222222222+
O      |                1111                 2222                |
B      |                    11             22                    |
A      |                      1           2                      |
B   .8 +                       1         2                       +
I      |                        1       2                        |
L      |                         1     2                         |
I      |                          1   2                          |
T   .6 +                          1  2                           +
Y      |                           1 2                           |
    .5 +                            *                            +
O      |                           2 1                           |
F   .4 +                          2  1                           +
       |                          2   1                          |
R      |                         2     1                         |
E      |                        2       1                        |
S   .2 +                       2         1                       +
P      |                      2           1                      |
O      |                    22             11                    |
N      |                2222                 1111                |
S   .0 + 222222222222222                         1111111111111111+
E      ++------+------+------+------+------+------+------+------++
       -8     -6     -4     -2      0      2      4      6      8
                                  QOC PERSON MEASURE
```

Table 5. Response category structure, items 4, 5, 6, 9, & 11

Items

1. How difficult is it to get appointments?
3. How difficult is it to contact provider by phone?

Category Number	Category Label	N	Average Measure	IMS	OMS	Step Threshold
1	Very Difficult	2,989	–0.59	.95	.91	
						–0.72 (1→2)
2	Somewhat Difficult	4,892	0.07	.94	.84	
						–0.43 (2→3)
3	Not Too Difficult	11,630	0.82	.93	.80	
						1.16 (3→4)
4	Not at all Difficult	13,390	1.90	1.16	1.11	

```
        CATEGORY PROBABILITIES: MODES - Step measures at intersections
P      ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
R  1.0 +                                                     44444444+
O      |11111                                          444444        |
B      |     111                                   4444              |
A      |        11                               44                  |
B   .8 +          11                           44                    +
I      |            1                         4                      |
L      |             1                      44                       |
I      |              1                    4                         |
T   .6 +               1                  4                          +
Y      |                1                4                           |
    .5 +                 1        3333  4                            +
O      |                  1     33    3*                             |
F   .4 +                   1   3      4 33                           +
       |                  22*2*      4    3                          |
R      |               222  *3 22   4      33                        |
E      |             22    3 1   2 4         33                      |
S   .2 +           22     3   11  *2           33                    +
P      |         22      3      *4  22           33                  |
O      |     2222     333     44 11   22           3333              |
N      |22222      333     444     111  22222          333333        |
S   .0 +***********44444444           1111111************************+
E      ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
       -4    -3    -2    -1     0     1     2     3     4     5     6
                       PERSON [MINUS] ITEM MEASURE
```

Table 6. Response category structure, items 1 & 3

Item
2. How long is the usual wait for a medical appointment?

Category Number	Category Label	N	Average Measure	IMS	OMS	Step Threshold
1	>2 hours	250	0.38	1.37	1.44	
						2.69 (1→2)
2	1-2 hours	1,111	0.89	1.24	1.29	
						1.11 (2→3)
3	31 to 59 minutes	1,831	1.34	1.17	1.20	
						1.06 (3→4)
4	16 to 30 minutes	5,212	1.95	1.21	1.20	
						0.43 (4→5)
5	5-15 minutes	7,387	2.92	1.24	1.16	
						4.43 (5→6)
6	<5 minutes	669	3.33	1.82	1.26	

```
        CATEGORY PROBABILITIES: MODES - Step measures at intersections
P      ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
R  1.0 +                                                             +
O      |                                                             |
B      |                                                             |
A      |                                                            6|
B   .8 +1                                    5555                  6 +
I      | 1                                555    555             66  |
L      |  1                              5          55         66    |
I      |   11                          55             55      6      |
T   .6 +     1                        5                 5    6       +
Y      |      1                      5                   5  6        |
    .5 +       1  222               5                     5*         +
O      |        *2   22      444444*                     66 5        |
F   .4 +      22 1     2    4     5 4                   6    5       +
       |    22    1     2  4     5   44                6      5      |
R      |   2       1    3**3    5      4              6        55    |
E      | 22         1 33 42 33 5        44          66           5   |
S   .2 +2           3*  4  2  *3          44      66              55 +
P      |          33  *4    **  33          44  66                  5|
O      |       333  44 11  5  22  33         6**4                    |
N      |   3333  444   55**1    222 333366666    444444              |
S   .0 +***************66666*****************************************+
E      ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
       -4    -3    -2    -1     0     1     2     3     4     5     6
                       PERSON [MINUS] ITEM MEASURE
```

Table 7. Response category structure, item 2

ITEMS		LOWDIF	MIDDIFF	HIDIFF	43,44,47,49,51,52	42,45,46,48,50
LOWDIFF	Pearson Correlation	1.000	.718	.255	.706	.524
	Sig. (2-tailed)	.	.000	.000	.000	.000
	N	16121	16069	11476	16121	16121
MIDDIFF	Pearson Correlation	.718	1.000	.470	.875	.680
	Sig. (2-tailed)	.000	.	.000	.000	.000
	N	16069	16083	11476	16069	16069
HIDIFF	Pearson Correlation	.255	.470	1.000	.889	.799
	Sig. (2-tailed)	.000	.000	.	.000	.000
	N	11476	11476	11476	11476	11476
Random Selection 43,44,47,49,51,52	Pearson Correlation	.706	.875	.889	1.000	.813
	Sig. (2-tailed)	.000	.000	.000	.	.000
	N	16121	16069	11476	16121	16121
Random Selection 42,45,46,48,50	Pearson Correlation	.524	.680	.799	.813	1.000
	Sig. (2-tailed)	.000	.000	.000	.000	.
	N	16121	16069	11476	16121	16121

** Correlation is significant at the 0.01 level (2-tailed).

Table 8. Comparison of QOC person measures across several item-subset conditions (step-anchored analysis).

N=6,094 ANALYZABLE FOR LOW MEDIAN GROUP
N=4,915 ANALYZABLE FOR HIGH MEDIAN GROUP
Correlations

		N_500	N_250	N_100	N_35A	N_35B	MEDI_LOW	MEDI_HI
N_500	Pearson Correlation	**1.000**	**.912**	**.921**	**.943**	**.839**	**.991**	**.865**
	Sig. (2-tailed)	.	.000	.000	.000	.001	.000	.001
	N	11	11	11	11	11	11	11
N_250	Pearson Correlation	**.912**	**1.000**	**.973**	**.932**	**.770**	**.922**	**.625**
	Sig. (2-tailed)	.000	.	.000	.000	.006	.000	.040
	N	11	11	11	11	11	11	11
N_100	Pearson Correlation	**.921**	**.973**	**1.000**	**.931**	**.877**	**.942**	**.725**
	Sig. (2-tailed)	.000	.000	.	.000	.000	.000	.012
	N	11	11	11	11	11	11	11
N_35A	Pearson Correlation	**.943**	**.932**	**.931**	**1.000**	**.795**	**.945**	**.783**
	Sig. (2-tailed)	.000	.000	.000	.	.003	.000	.004
	N	11	11	11	11	11	11	11
N_35B	Pearson Correlation	**.839**	**.770**	**.877**	**.795**	**1.000**	**.854**	**.839**
	Sig. (2-tailed)	.001	.006	.000	.003	.	.001	.001
	N	11	11	11	11	11	11	11
MEDI_LOW	Pearson Correlation	**.991**	**.922**	**.942**	**.945**	**.854**	**1.000**	**.860**
	Sig. (2-tailed)	.000	.000	.000	.000	.001	.	.001
	N	11	11	11	11	11	11	11
MEDI_HI	Pearson Correlation	**.865**	**.625**	**.725**	**.783**	**.839**	**.860**	**1.000**
	Sig. (2-tailed)	.001	.040	.012	.004	.001	.001	.
	N	11	11	11	11	11	11	11

** Correlation is significant at the 0.01 level (2-tailed).
* Correlation is significant at the 0.05 level (2-tailed).

Table 9. Correlation analysis --Comparison of item calibrations across random samples, and by median split (unanchored analysis).

Correlations

		MEDIC	PRVHMO	PUB
MEDIC	Pearson Correlation	1.000	.946	.948
	Sig. (2-tailed)	.	.000	.000
	N	11	11	11
PRVHMO	Pearson Correlation	.946	1.000	.958
	Sig. (2-tailed)	.000	.	.000
	N	11	11	11
PUB	Pearson Correlation	.948	.958	1.000
	Sig. (2-tailed)	.000	.000	.
	N	11	11	11

** Correlation is significant at the 0.01 level (2-tailed).

Table 10. Correlation matrix of the item calibrations: Public HMO vs. Private HMO vs. Medicare.

Descriptives
QOC

	N	Mean	Std. Deviation	Std. Error	95% Confidence Interval for Mean		Minimum	Maximum
					Lower Bound	Upper Bound		
Priv HMO	4791	4.84	2.39	.003	4.77	4.91	-5.89	8.09
Medicare HMO	123	4.90	2.48	.22	4.46	5.34	-5.47	8.09
Public HMO	693	4.57	2.62	.10	4.37	4.76	-5.47	8.09
Priv HMO & Medicare HMO	106	5.54	1.91	.19	5.17	5.90	-.88	8.09
Private HMO & Public HMO	37	4.03	2.94	.48	3.06	5.01	-4.29	8.09
Medicare HMO & Public HMO	16	4.92	2.06	.51	3.83	6.02	1.37	8.09
Private HMO & Medicare HMO & Public HMO	7	4.80	2.49	.94	2.50	7.11	2.13	8.09
Total	5773	4.82	2.42	.03	4.75	4.88	-5.89	8.09

Test of Homogeneity of Variances---reject H0: variances are equal
Use Tamhane, Dunnett's T3, Games-Howell, and Dunnett's D3 for Post-Hoc
QOC

Levene Statistic	Df1	df2	Sig.
2.06	6	5766	.055

ANOVA
QOC

	Sum of Squares	df	Mean Square	F	Sig.
Between Groups	124.05	6	20.67	3.54	.002
Within Groups	33656.43	5766	5.84		
Total	33780.47	5772			

Private HMO and Medicare HMO > Private HMO (+.70)	T, DT3
Private HMO and Medicare HMO > Public HMO (+.97)	T, DT3
Private HMO and Medicare HMO > Private HMO and Public HMO (1.50)	T, DT3, GH,DC

Table 11. ANOVA analysis comparing person measures: Public HMO vs. Private HMO vs. Medicare.

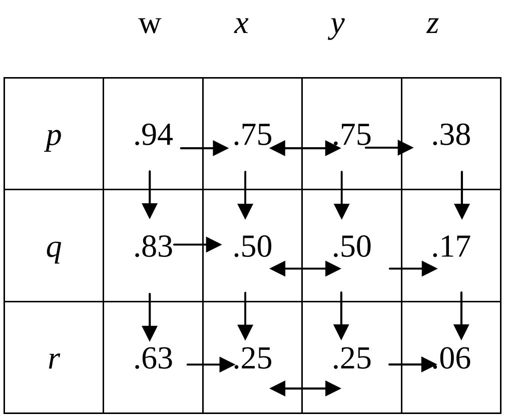
```
MOST MISFITTING RESPONSE STRINGS
ITEM                 OUTMNSQ  |PERSON
                              |2222221111           22222111111111111111111
                              |22222140009777766554403311999888877211110000522222
                              |66666689921111122416684433700662275644442222353333
                              |77777454479777600716642288877335465155447777064444
                              |87654125409210954444367687221521630454984321069875
                           high--------------------------------------------------
     5 ASKS ABOUT OTH   1.55 A|........................11........................
     2 APPT. WAITING    1.23 B|........................666.......6.6666..........
    11 [NO PROBLEMS]    1.11 C|111111.11..1111...111.....1.......................
     9 [EVERYONE HAS    1.03 E|.........11....111......111....1111111111111......
    10 SATIS FAMILY      .89 F|..................................3...............
     4 LISTENS & GIVES   .75 d|......1..............111...1111..............11111
     6 CONFIDENT IN PR   .44 c|............................................1.....
                              |----------------------------------------------low-
```

Table 12. Using fit statistics to identify inconsistent responses that do not contribute to the measurement effort.

Features	WINSTEPS PCM	MULTILOG IRT	SUMMED RATINGS
Starts from ordinal observations (counts of correct answers or sums of ratings)	Yes	Yes	Yes
Additive unit of measurement	Yes	Yes (1P only)	No
Estimates error	Yes	Yes	No
Accommodates missing data	Yes	Yes	No
Implements tests of independence & double cancellation (tests data consistency)	Yes	No	No
Persons and items scaled on common metric	Yes	Yes	No
All statistics reported for both persons and items in same analysis	Yes	No	No
Provides multiples tests of construct validity	Yes	No	No
Provides multiple supports for creation of keyforms	Yes	No	No
Parameter Separation Reliability Coefficient	Yes	No	No
Approximation of Cronbach's alpha	Yes	Yes[1]	Yes
Error and reliability reported in both modeled and fit-adjusted forms	Yes	No	No
Infit Mean Square	Yes	No	No
Stdized Infit	Yes	No	No
Outfit Mean Square	Yes	No	No
Stdized Outfit	Yes	No	No
Pt. Biserial Correlations	Yes	No	No
Bias displacement estimates	Yes	No	No
Plots variable maps	Yes	No	No
Data displayed in Guttman scalogram format	Yes	No	No
Individual expected vs observed responses and residuals detailed in plots and tables	Yes	No	No
Infit-Outfit plots	Yes	No	No
Infit/Outfit-Measure plots	Yes	No	No
Raw score-Measure plots	Yes	No	No
Principal Components factor analysis of residuals	Yes	No	No
Dichotomous models	Yes	Yes	No
Rating Scale models	Yes	Yes	No
Partial Credit models	Yes	Yes	No
Multifaceted models	No	No	No
2P models	No	Yes	No
3P models	No	Yes	No
Samejima's Graded model	No	Yes	No
Bock's Nominal model	No	Yes	No
Thissen/Steinberg multiple choice model	No	Yes	No
Tests assumption of normal population distribution	No	Yes	No
Combines different models in single analysis	Yes	Yes	No

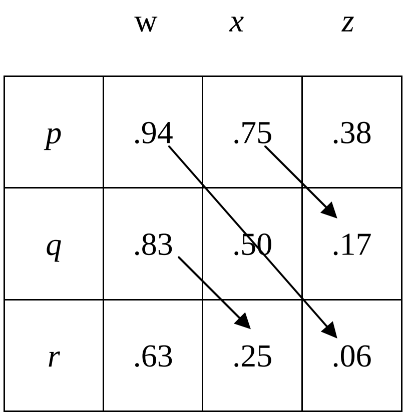
Table 13. Comparison of various features of typical PCM, IRT, and Summated Ratings analyses. (Many features of PCM and IRT can be applied to raw scores by competent users of generic statistical packages, such as SAS, SPSS, SYSTAT, etc.)

[1] Marginal reliability (Thissen 1991, p. 4-11).



	w	*x*	*y*	*z*
p	.94	.75	.75	.38
q	.83	.50	.50	.17
r	.63	.25	.25	.06

INDEPENDENCE



	w	*x*	*z*
p	.94	.75	.38
q	.83	.50	.17
r	.63	.25	.06

DOUBLE CANCELLATION

Figure 1. Necessary and sufficient conditions for interval scaling and measurement invariance.

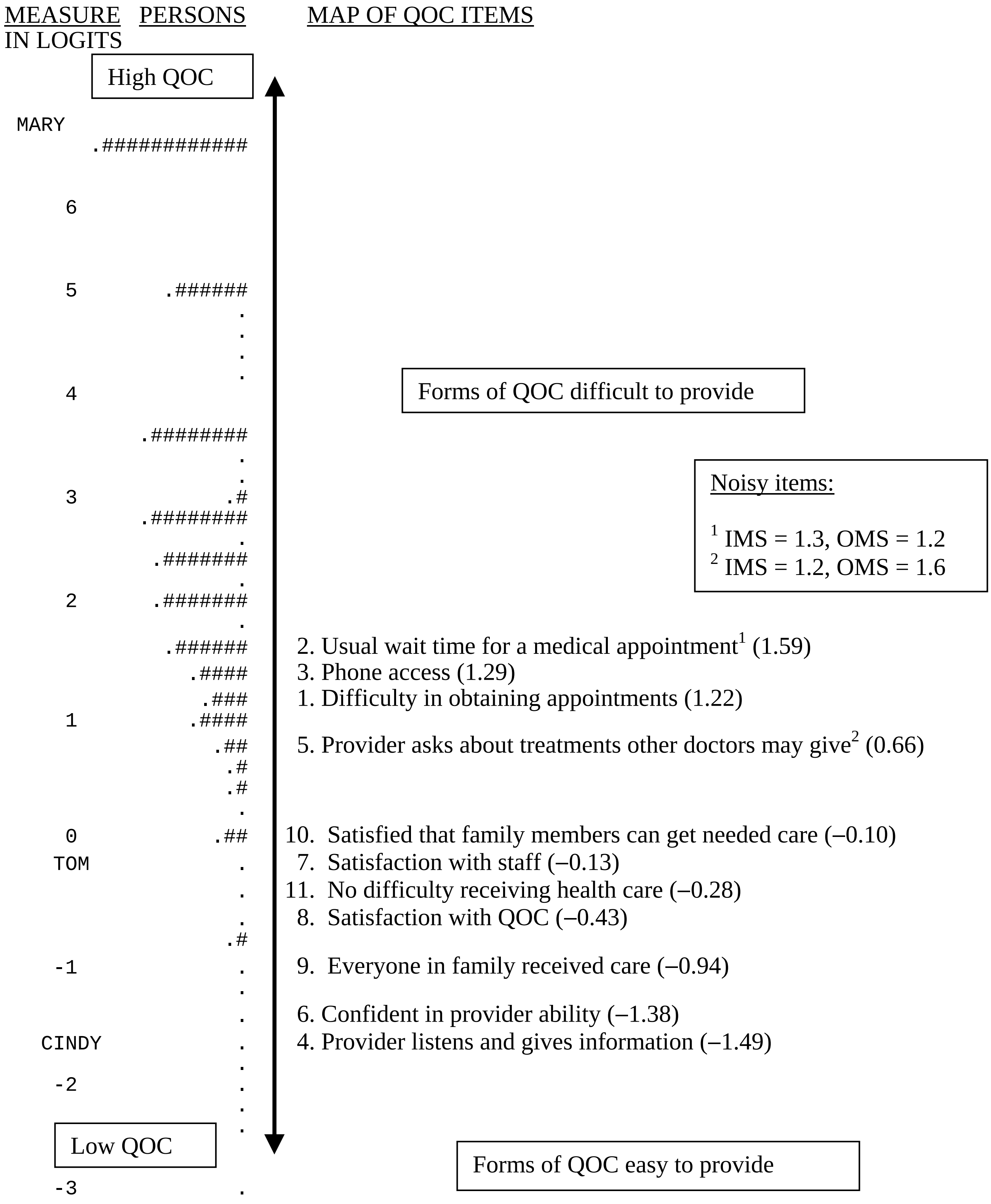


Figure 2. Variable map. Each '#' is 261 cases; each '.' is less than 261 cases.

```
EXPECTED SCORE: MEAN  (":" INDICATES HALF-SCORE POINT)

Less Satisfied  >>>>>>>>>>>>>>>>>>>>>>>>>>>   More Satisfied
-3       -1         1         3         5         7         9
|---------+---------+---------+---------+---------+---------| ITEM
1  1   :    2  : 3  :  4   :       5         :     6        6 [short] APP WAIT TIME(2)
|                                                           |
1          1  :   2  :  3    :   4                          4 [EASY] CONTACT PHONE (3)
1         1   :   2  :  3    :   4                          4 [EASY] TO GET APPT (1)
|                                                           |
|                                                           |
1            1    :     2                                   2 ASKS ABOUT OTHER TRTS (5)
|                                                           |
|                                                           |
1   1   :  2  :  3    :   4                                 4 SATIS FAMILY CAN GET CARE (10)
1   1  :   2  :  3   :    4                                 4 SATIS W/ STAFF (7)
1       1     :    2                                        2 [NO PROBLEMS] OBTAIN CARE (11)
1  1  :   2 :   3   :   4                                   4 SATIS W/ QUALITY OF CARE (8)
|                                                           |
1    1    :     2                                           2 [EVERYONE HAS HAD] CARE (9)
|                                                           |
1  1    :     2                                             2 CONFIDENT IN ABILITY (6)
1 1     :    2                                              2 LISTENS & GIVES INFO (4)
|---------+---------+---------+---------+---------+---------|
-3       -1         1         3         5         7         9

              TOM                 MARY

                    1111 2 2 2   2      1                   3
1      2   3  1622368006 012 432 1     16                   3
7      2 1166521598031389003231091    410                   8
0  4125114965967068458498481128442   2477                   3  PERSONS
           T      S      M       S      T
```

Figure 3. Plot of expected responses (Tom’s expected responses are underlined, Mary’s are circled).

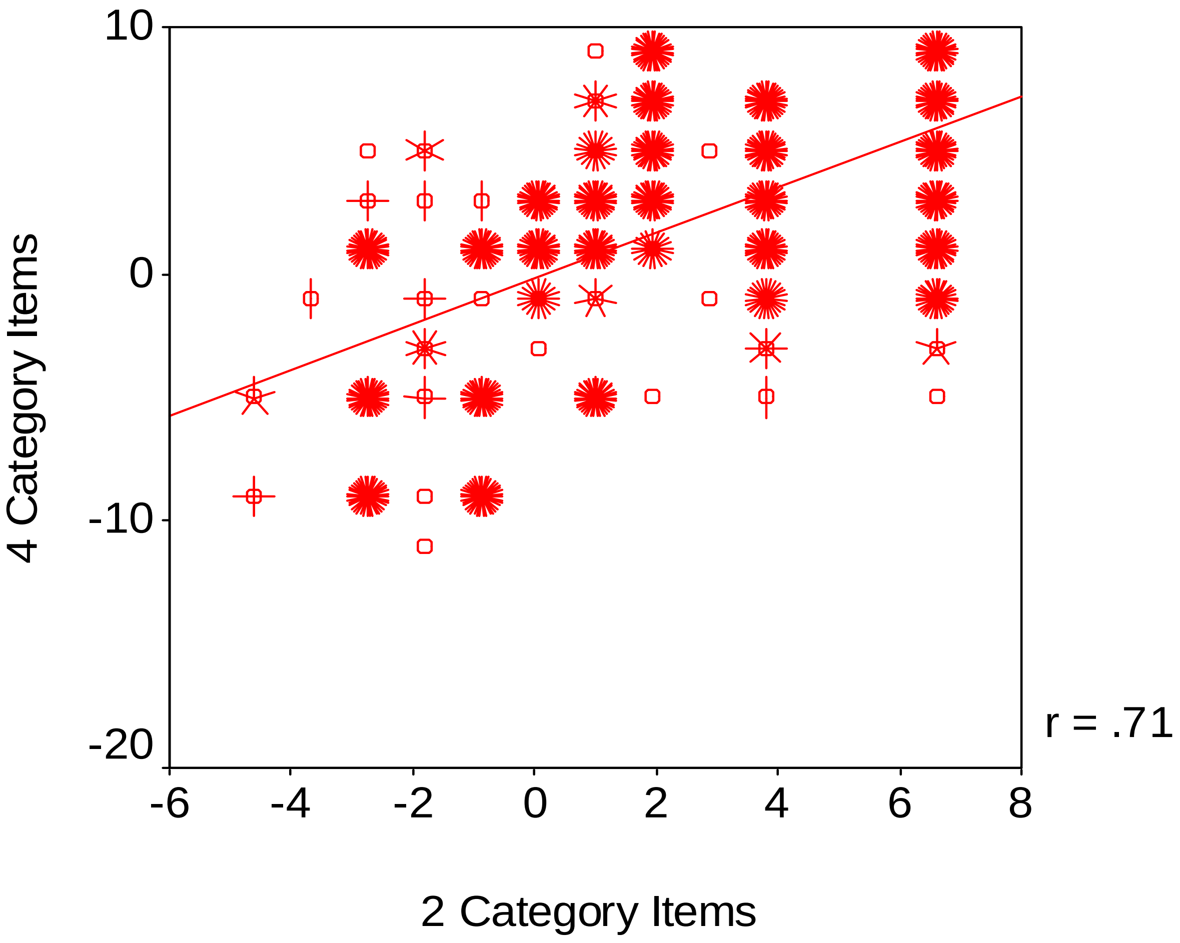


Figure 4. QOC person measures, 2 category vs. 4-category items (r=.71, unanchored).

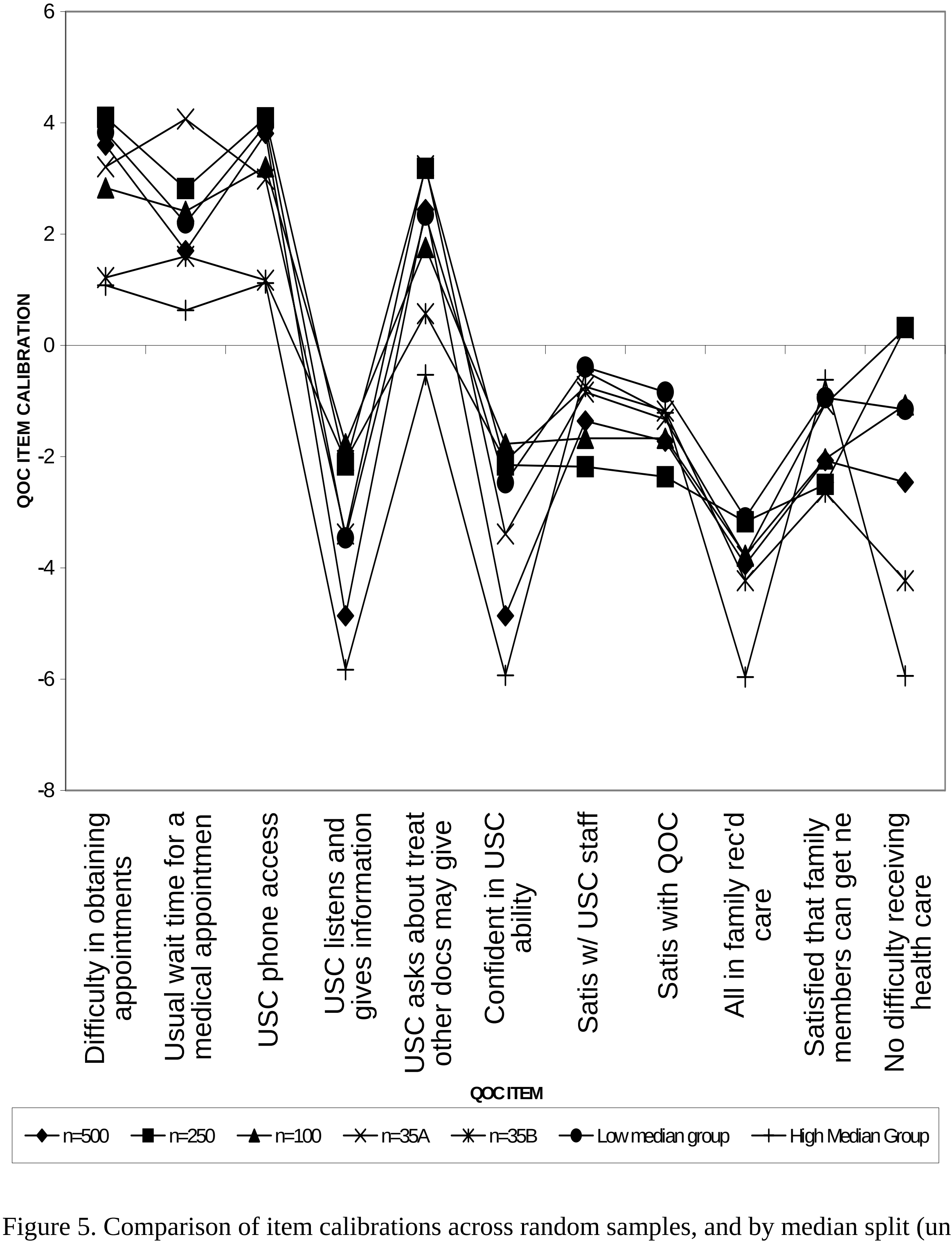


Figure 5. Comparison of item calibrations across random samples, and by median split (unanchored analysis).

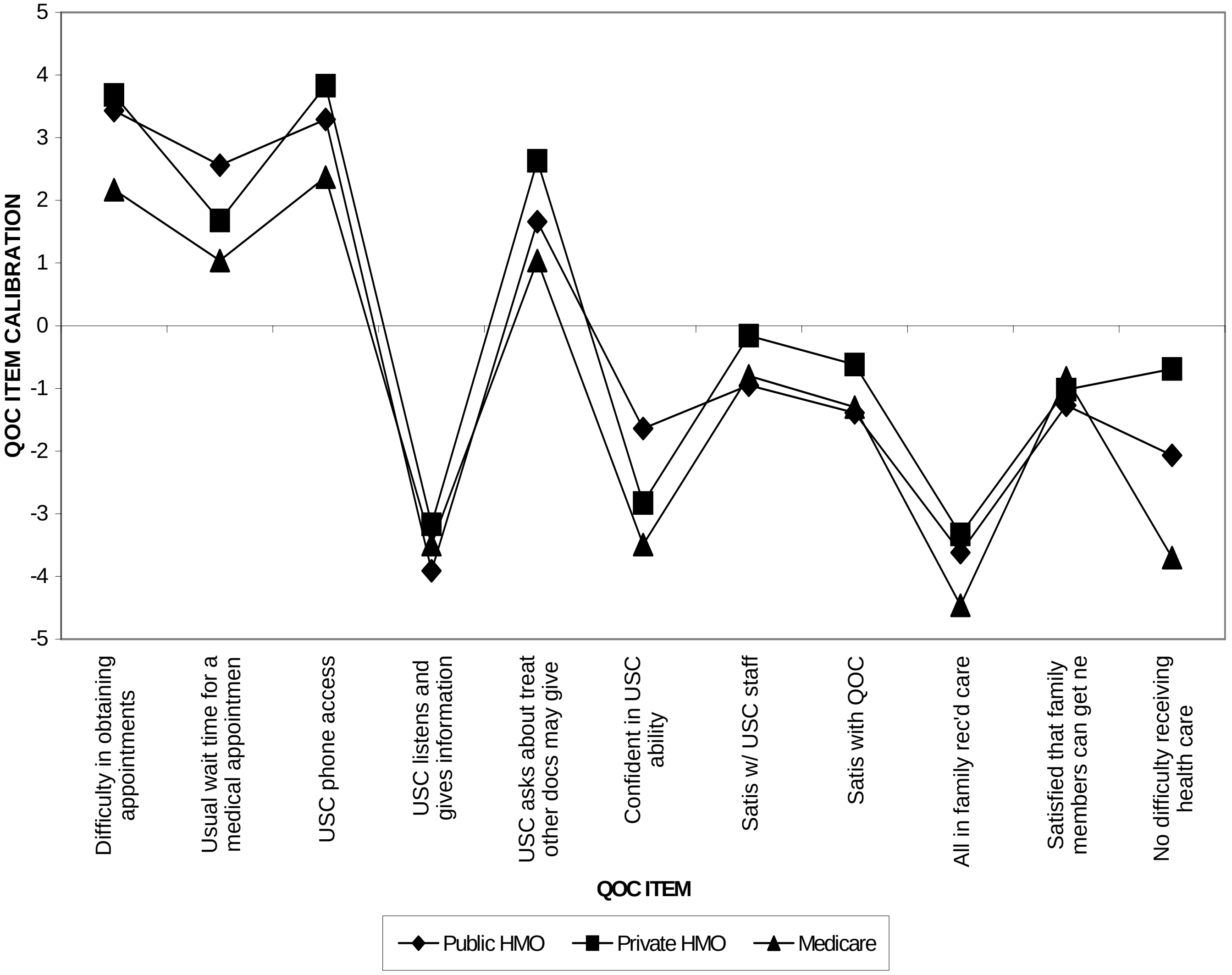


Figure 6. Comparison of QOC item calibrations: Public HMO vs. Private HMO vs. Medicare.
N = 562 Public HMO, N = 3539 Private HMO, N = 190 Medicare

```
        EXPECTED SCORE OGIVE: MEANS
       ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
     6 +                                                             +
       |                                                      6666666|
   5.5 +                                                 55666       +
       |                                           555555  |         |
E    5 +                                    5555555        |         +
X      |                               55555   *           |         |
P  4.5 +                            455        *           |         +
E      |                         444 |         *           |         |
C    4 +                       44    |         *           |         +
T      |                      4 *    |         *           |         |
E  3.5 +                    34  *    |         *           |         +
D      |                   3|   *    |         *           |         |
     3 +                 33 |   *    |         *           |         +
S      |                3*  |   *    |         *           |         |
C  2.5 +              23 *  |   *    |         *           |         +
O      |            22 | *  |   *    |         *           |         |
R    2 +          22   | *  |   *    |         *           |         +
E      |        22 *   | *  |   *    |         *           |         |
   1.5 +    1122   *   | *  |   *    |         *           |         +
       |1111 |     *   | *  |   *    |         *           |         |
     1 +     |     *   | *  |   *    |         *           |         +
       ++-----+-----+-----+-----+-----+-----+-----+-----+-----+-----++
       -4    -3    -2    -1     0     1     2     3     4     5     6
                       PERSON [MINUS] ITEM MEASURE
```

Figure 7. Plot of non-linear ratings against linear QOC measures, for item 2, Usual wait time for a medical appointment

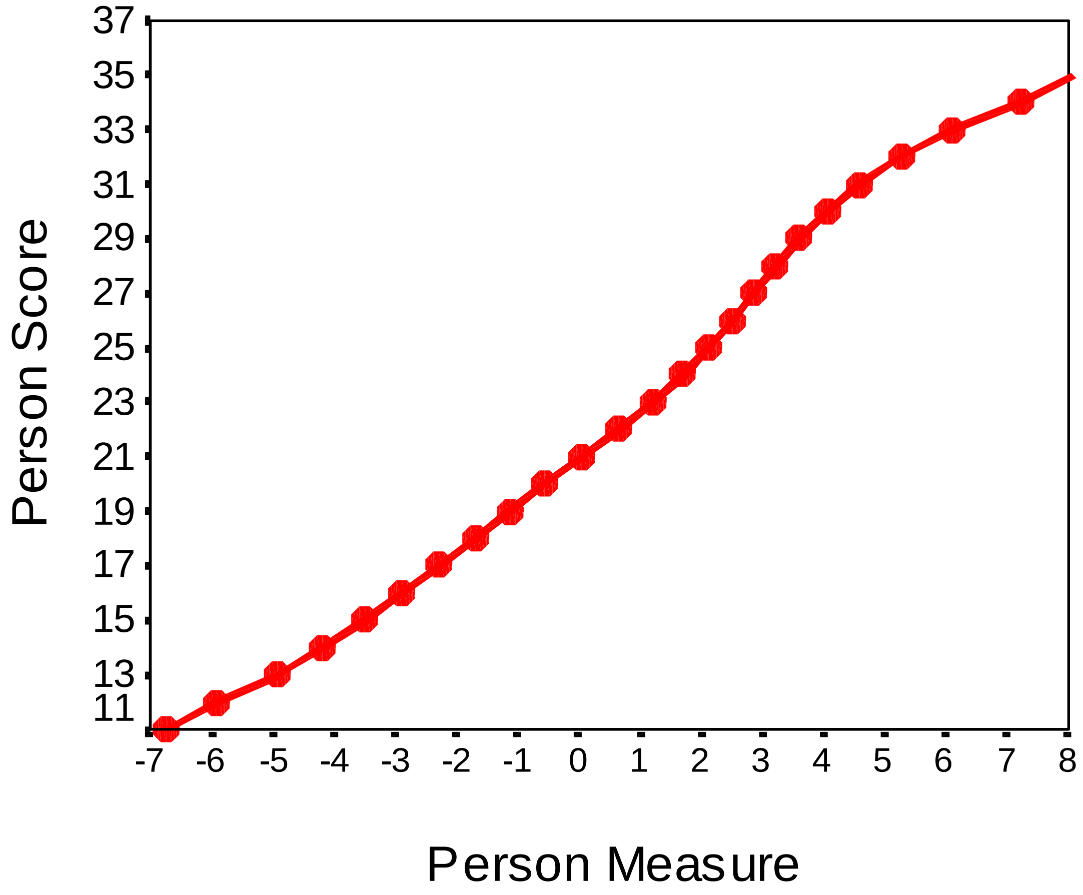


Figure 8. Plot of scores against linear QOC measures.

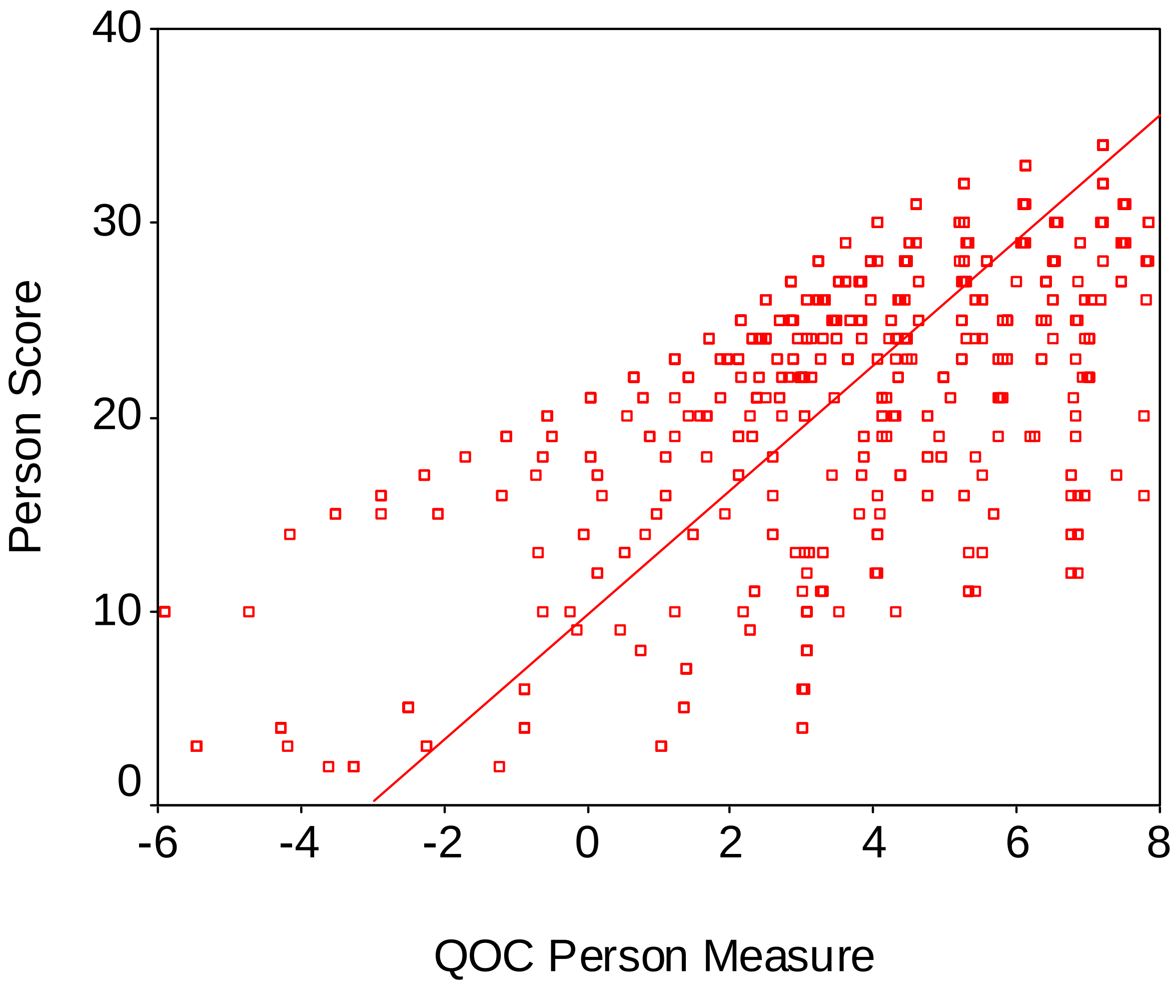


Figure 9. Plot of person scores against linear QOC measures (actual, r=.79).

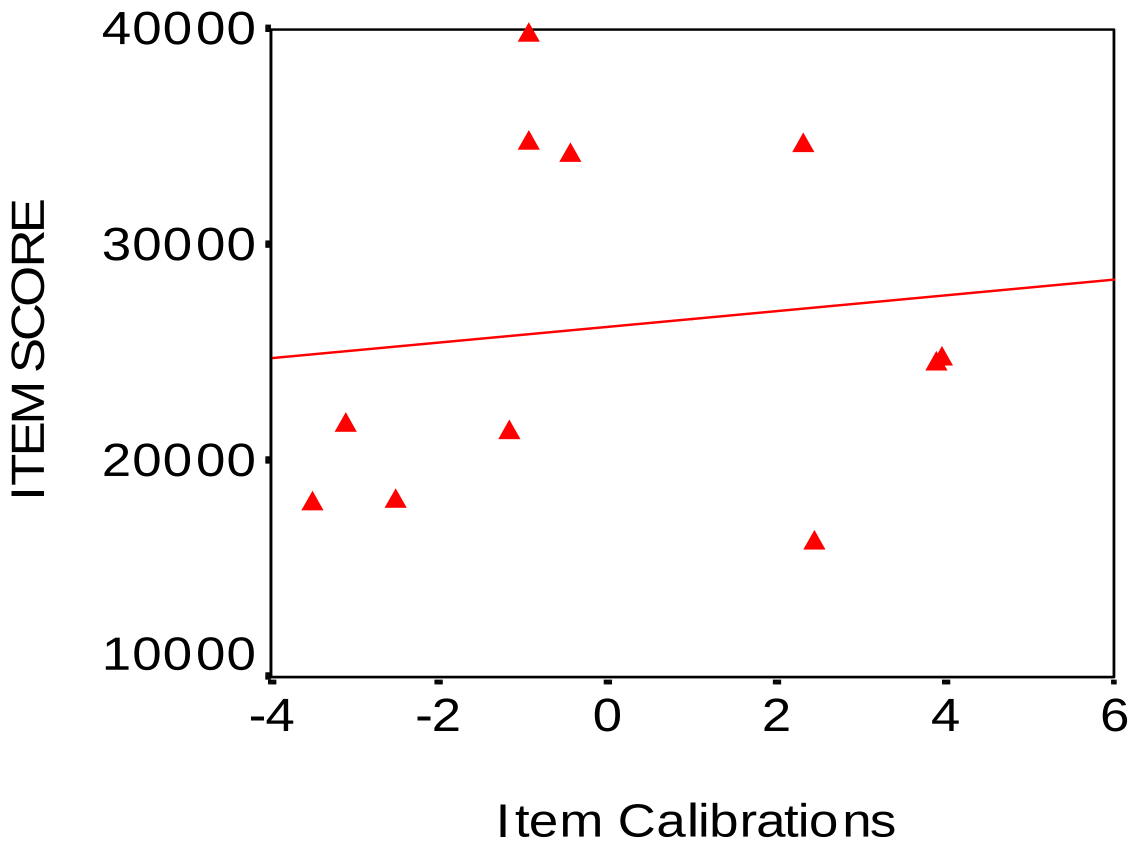


Figure 10. Plot of item scores against linear QOC item calibrations (R=.79).

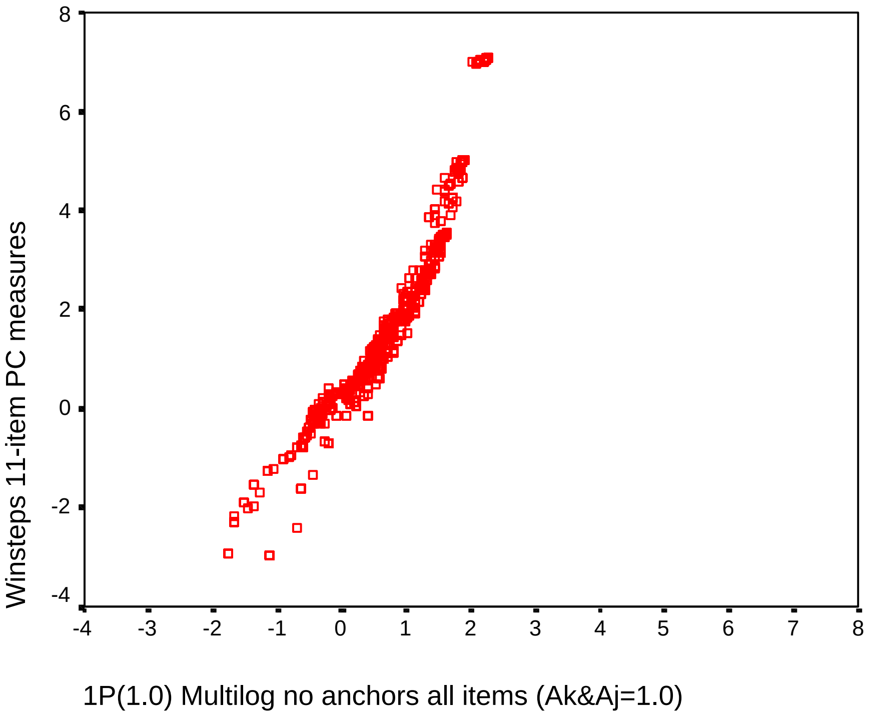


Figure 11. WINSTEPS PCM vs. MULTILOG 1PL Samejima Graded Response Model measures

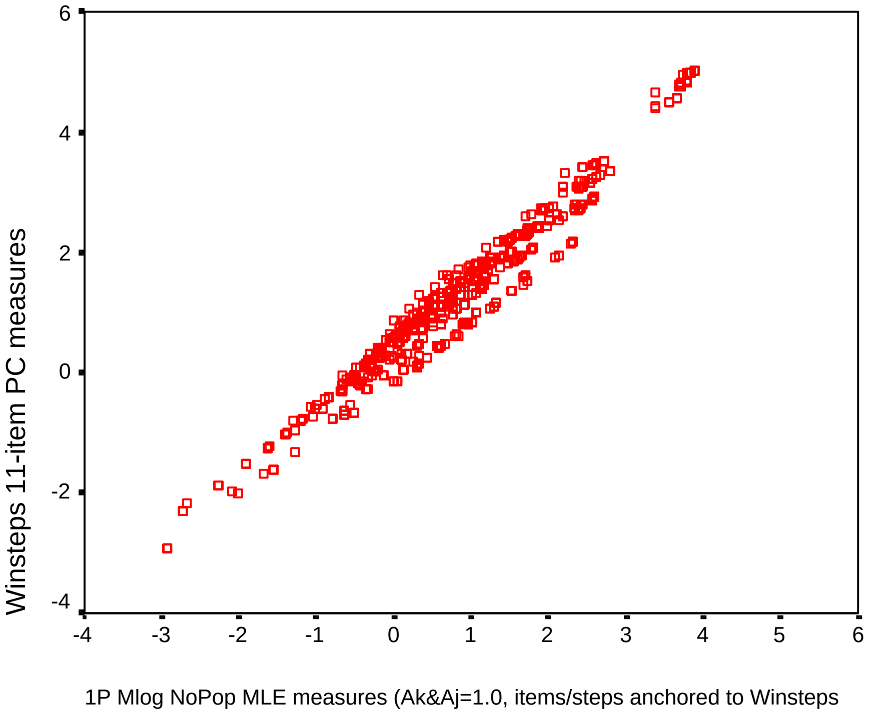


Figure 12. WINSTEPS PCM vs. MULTILOG 1PL Samejima Graded Response MEPS QOC measures, with the MULTILOG items anchored at the WINSTEPS values

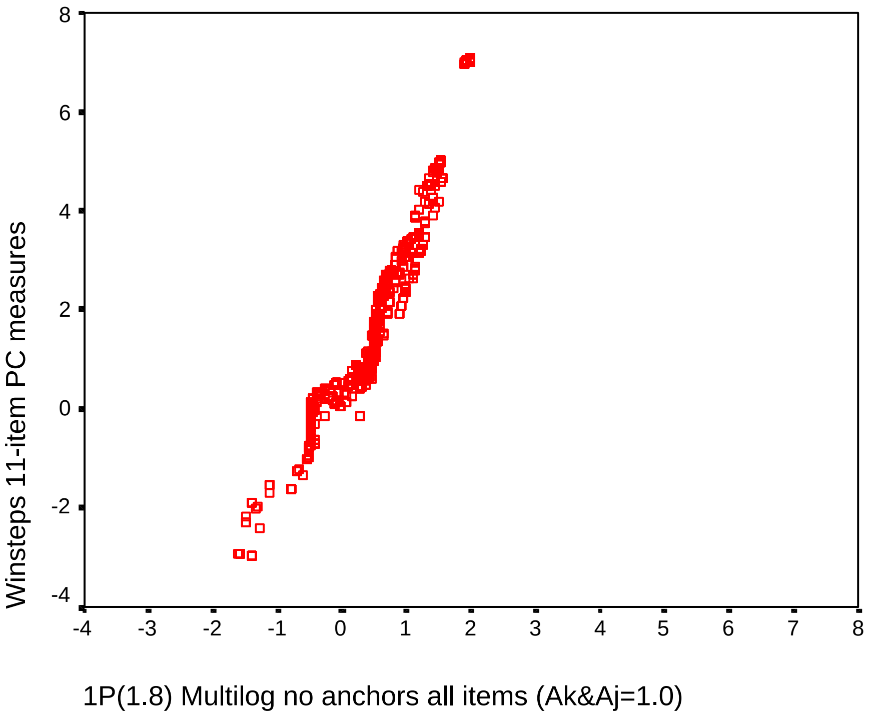


Figure 13. WINSTEPS PCM vs. MULTILOG 1PL MEPS QOC measures, with the MULTILOG 2d item parameter fixed at 1.8

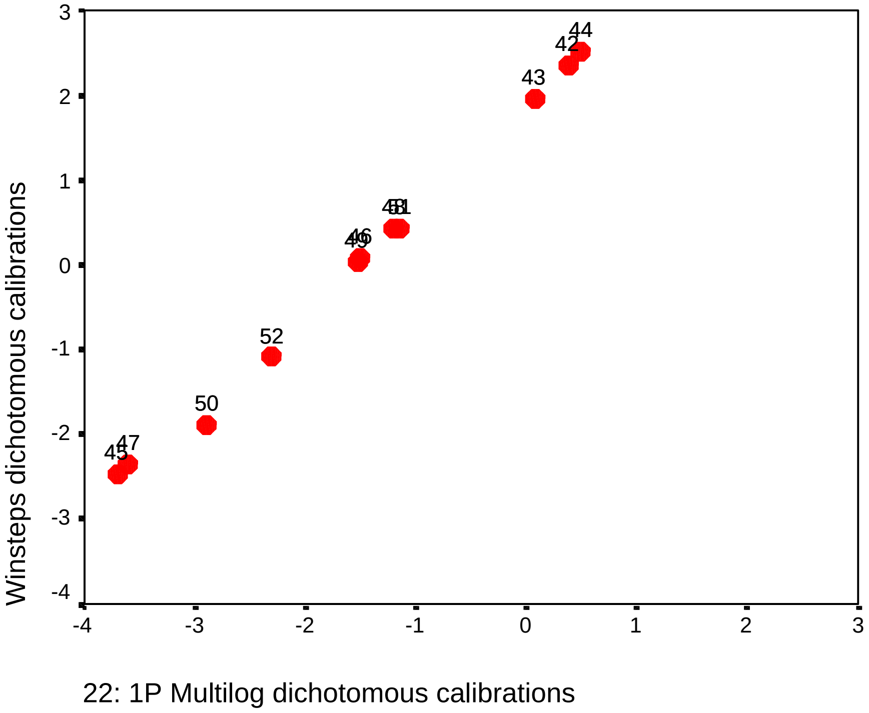


Figure 14. WINSTEPS dichotomous item calibrations vs. MULTILOG 1PL dichotomous calibrations. (The item labeled 42 corresponds to item 1 [AC17], and 52, to item 11 [AC25], in this and the following plots.)

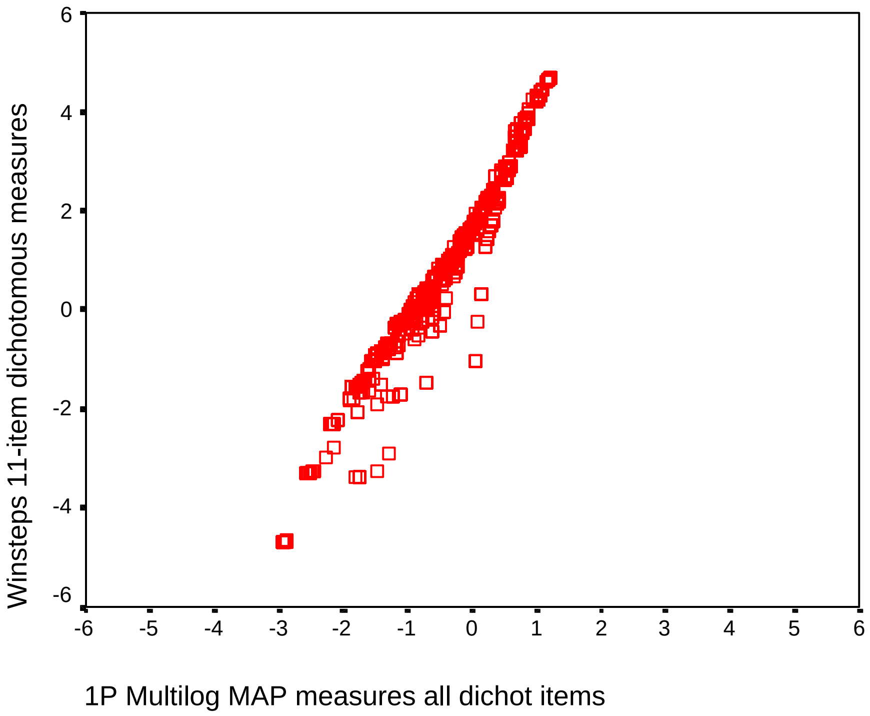


Figure 15. WINSTEPS dichotomous MEPS QOC measures vs. MULTILOG 1PL maximum a posteriori dichotomous measures

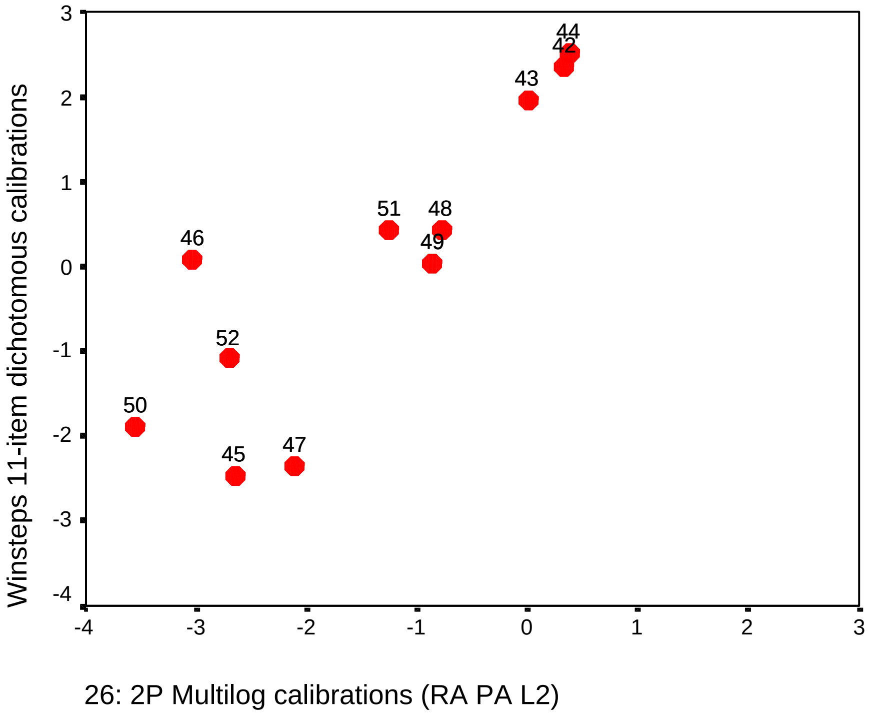


Figure 16. WINSTEPS dichotomous MEPS QOC calibrations vs. MULTILOG 2P calibrations (using marginal maximum likelihood estimation and data input as counts of response patterns).

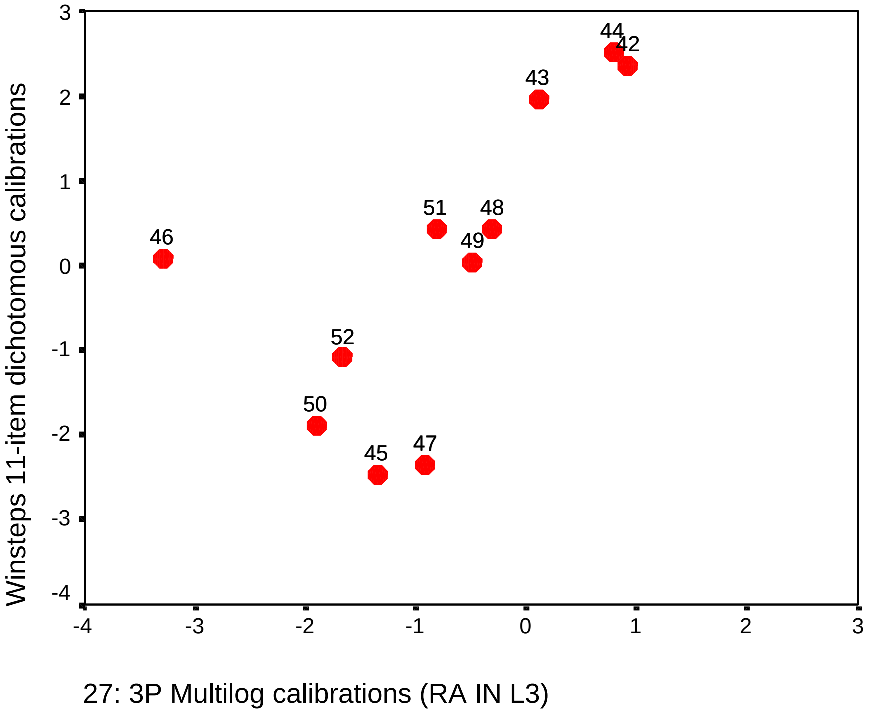


Figure 17. WINSTEPS dichotomous MEPS QOC calibrations vs. MULTILOG 3P calibrations (using marginal maximum likelihood estimation and data input as individual item response vectors).

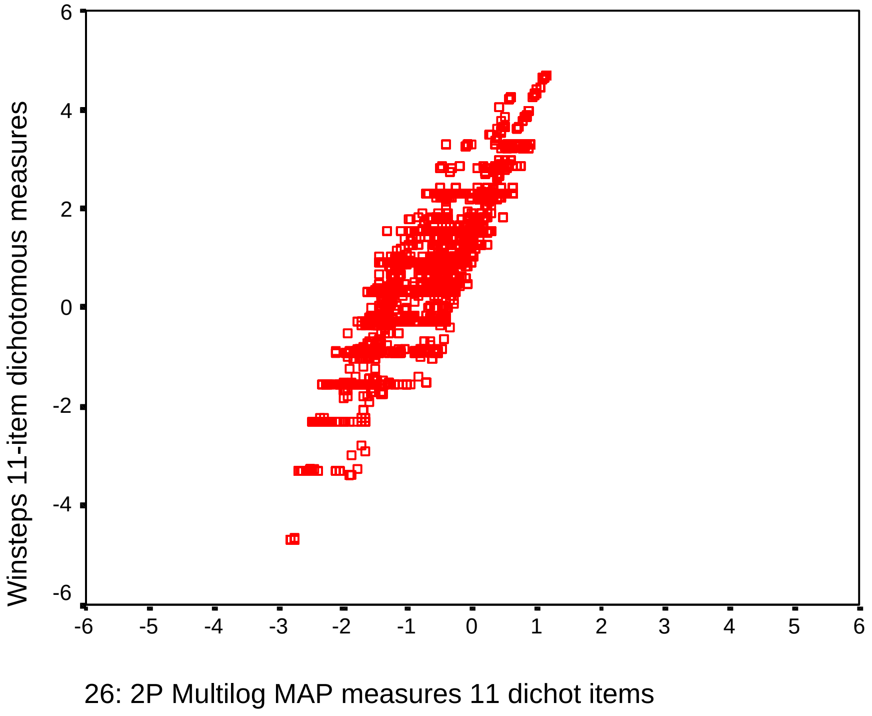


Figure 18. WINSTEPS dichotomous MEPS QOC measures vs. MULTILOG 2PL maximum a posteriori measures

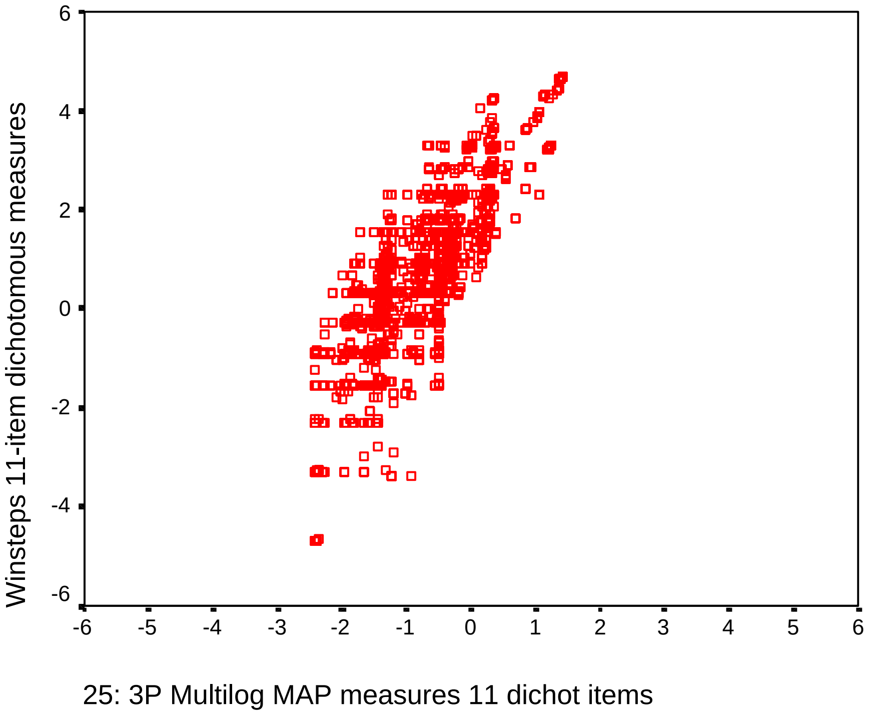


Figure 19. WINSTEPS dichotomous MEPS QOC measures vs. MULTILOG 3PL maximum a posteriori measures

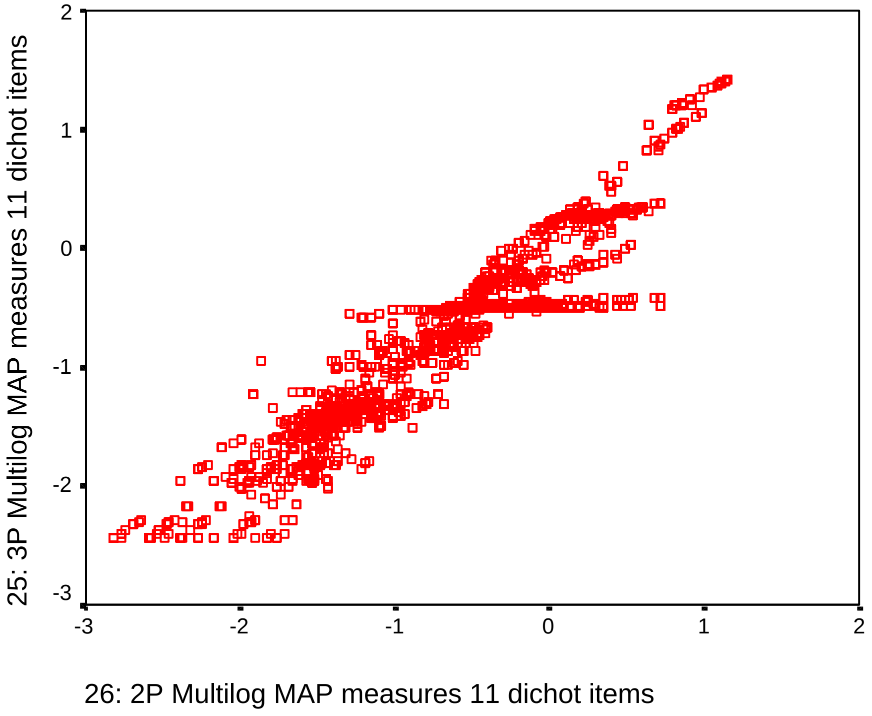


Figure 20. MULTILOG 3PL maximum a posteriori measures vs. MULTILOG 2PL maximum a posteriori measures

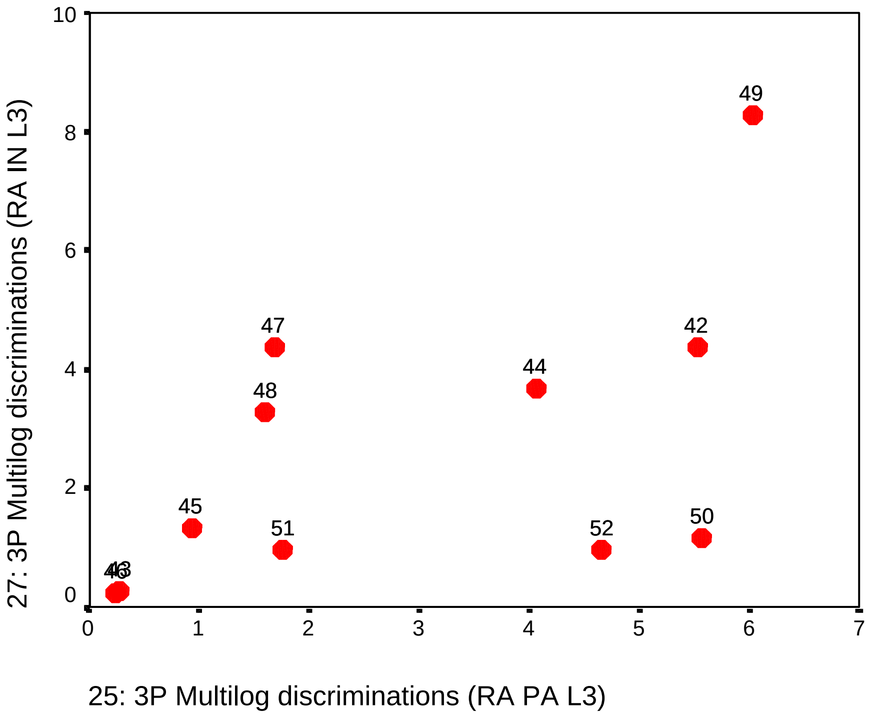


Figure 21. Variation in item discrimination values by type of data input for the MULTILOG 3P model (PA indicates data input as counts of response patterns; IN indicates data input as individual item response vectors).

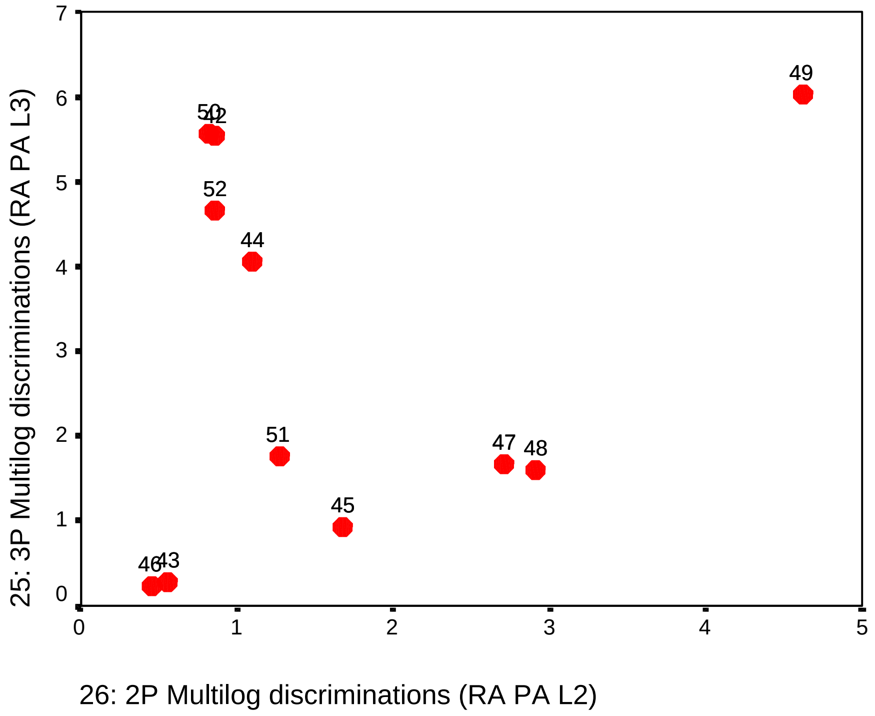


Figure 22. Variation in item discrimination parameter estimates in MULTILOG 3P vs. 2P models, using the same estimation method (marginal maximum likelihood) and the same type of data input (counts of response patterns).