An Opportunity for Empiricism

Howard Gardner
Multiple Intelligences: The Theory in Practice
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Review by
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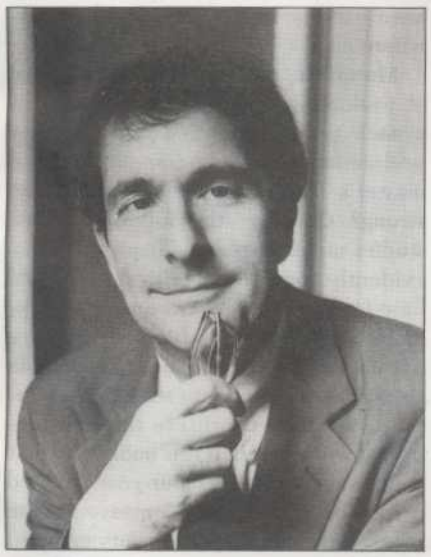
Multiple Intelligences: The Theory in Practice is a distillation of work in educational assessments and interventions stimulated by Gardner's (1983) theory of multiple intelligences (MI), which was first presented in his popular *Frames of Mind: The Theory of Multiple Intelligences*. Succinctly, Gardner has proposed that conventional general intelligence tests are narrow and one-dimensional whereas, in reality, there are seven "intelligences" (i.e., musical, bodily–kinesthetic, logical–mathematical, spatial, linguistic, interpersonal, and intrapersonal intelligences). These intelligences are construed as literally independent from one another, both phenotypically and neurologically, and should be, but are not, equally valued from a sociocultural point of view. We agree with Gardner that the differential validity of ability dimensions beyond general intelligence is underappreciated in educational settings (Humphreys, Lubinski, & Yao, 1993). We also applaud him for reminding us of the importance of establishing differential educational–vocational counseling expectations on the basis of individual differences in abilities and interests, a perspective with a long and celebrated history in applied psychology (Brayfield, 1950; Brewer, 1942; Williamson, 1939, 1965). It is a message that has been lost in today's school reform. Yet we feel obliged also to note that these intelligences, while adding nuances to the psychology of intellectual behavior, are not new. They possess strong linkages to traditional psycho-


Howard Gardner (©1994 by Jerry Bauer)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metric conceptualizations of human abilities, and nearly all have been identified in the classic psychometric literature (Carroll, 1993, p. 641; Messick, 1992). Moreover, their applied utility, in educational (Snow & Lohman, 1989) and vocational (Lubinski & Dawis, 1992) contexts, has been stressed by those who find scientific merit in conventional instruments. Nonetheless, they remain underused.

These are some general comments on Gardner's theory and philosophy. Yet MI theory is not the main thrust of this book, although it is the conceptual foundation for the work presented. Discussed here, in more detail, is the need to assess each of these intelligences in an intelligence-fair way by using portfolios and project ratings, for example, in contrast to out-of-context standardized assessments. This book also attempts to capture how MI theory is and can be translated into practice through a collection of papers and chapters not necessarily written with this book in mind or solely by Gardner himself. Some chapters, for instance, include updates on Project Spectrum (a project aimed at assessing abilities in an intelligence-fair way) and the Key School in Indianapolis (a K–6 elementary school based on MI theory). For the remainder of this review, therefore, we have decided to focus on the conceptual and empirical bases for Gardner's recommendations for school practice.

We will examine in particular his assessment recommendations inasmuch as Gardner has noted that, "while our educational work has ranged from curriculum development to teacher education, our primary point of leverage has been the creation of new forms of assessment" which "allow individuals to demonstrate their strengths and their understandings in ways that are comfortable for them yet subject to public accountability" (p. xv). These assessments have attempted to "get away altogether from tests and correlations among tests, and look instead at more naturalistic sources of information about how peoples around the world develop skills important to their way of life" (p. 7). This certainly sounds interesting and important. Yet a logical–mathematical mind might be moved to ask: What is the empirical support for Gardner's proposals? From our perspective, the answer to this question is important to examine. After all, we may all agree, irrespective of our frame of mind, that our educational system needs improvement but, as the history of medicine reveals, implementing changes without empirical documentation of their efficacy (simply because a problem exists) can make things worse.

The driving force behind Gardner's agenda to change the way intelligences are assessed stems from his concerns, which are not clearly specified, about traditional "decontextualized assessments," such as the Standard Achievement Test (SAT) and American College Test (ACT) exams. He says that they should be abandoned (p. 184). He recommends instead contextualized assessments, which are conducted in more natural settings—where learning and creativity actually happen. Contextualized assessments are predicated on the idea that, to truly assess intelligence and its many manifestations, we must do so while a person is operating on a meaningful task in "the real world." Why, we feel compelled to ask, do we need to engage in a zero-sum game? Is there not room in psychology for both forms of assessment? And should both approaches not be subjected to empirical scrutiny to ascertain their comparative usefulness and incremental validity? Moreover, there are concerns about contextualized assessment that are not addressed in this book.

The first involves the process of construct validation itself. Scientific justification for the context of assessment, as well as its medium, is obtained from the breadth and depth of a measuring instrument's network of correlates and forecasting capabilities. They document its construct validity. The context of assessment, like an instrument's public relations appeal or its face validity, is not a central component to the construct validation process. Gardner is skeptical of assessments like reversed digit span, because people are not often asked to perform such tasks in meaningful life situations (p. 241). But in reality, most psychologists are no more intrinsically interested in digit span than a physician is intrinsically interested in oral temperature. What these scientific practitioners are interested in are the correlates and causes of individual differences assessed by these measures, because this network enables them to generate many more valid inferences than if they were ignorant of their client's status on these dimensions. From a scientific point of view, there is nothing inherently preferable about whether assessment occurs in one context versus another (and this pertains to the medium of assessment as well). If, however, a context is efficient, easy to adopt by other scientific practitioners, and generates a more impressive network of external real-world relationships than a more elaborate, expensive, and time-consuming system, then the former is certainly preferable.

The importance of real-world criteria is, as a matter of fact, a recurring theme throughout Gardner's book. It is curious, therefore, that Schmidt and Hunter's (1981) work on the validity generalization of "decontextualized" instruments is not cited with admiration. For over two decades, these investigators have compiled thousands of ability–performance validity coefficients on over 12,000 jobs (over the full range of occupational prestige, which they have classified into five categories based on their nature and complexity). Through their extensive meta-analyses of literally hundreds of thousands of workers, using general intelligence and other conventional psychometric factors as predictors, they find that, in intellectually demanding occupations, nearly half of the variance in performance criteria is accounted for by the general intelligence factor of traditional instruments.[1] Furthermore, the general factor accounts for substantively significant variance across all job classes (cf. Schmidt, Ones, & Hunter, 1992, and references therein). Surely these are real-world criteria.[2]

Given their stinging criticism of the validity of psychometric measures, we were surprised by the lack of concern for the many technical reliability–validity problems one encounters with ratings (Landy & Farr, 1980) and portfolio assessments, particularly when done on a national level. We were left wondering whether Gardner fully appreciates these issues and what he anticipates the reliability is of his proposed assessment scheme. Without reliability, we cannot have validity in the real world or anywhere else.

Moreover, the empirical work aimed at evaluating Spectrum, Gardner's approach to assessment, would pass few preliminary screening committees for a master's thesis in psychology. Too strong? Consider the following three studies underscored in Chapter 6. They evidently constitute a critical evidential base for the utility of Gardner's 15 separate competencies, which he feels are useful to assess in the schools. The sample sizes alone preclude meaningful conclusions, yet they constitute the best empirical work found in this book. The first study was based on 13 four-year-olds and used 8 of Spectrum's 15 measures; the second involved 20 participants and used 10 of Spectrum's measures. Although the

intercorrelations of these measures are not provided, Gardner contends that the second study reveals that Spectrum's measures "identify a range of nonoverlapping capabilities in different content areas" (p. 95), because only one pair of scales manifested a significant correlation at $p < .01$. But it is difficult to observe a statistically significant correlation with 20 participants, given that a 95 percent confidence interval for this sample size spans .89 correlational units! Furthermore, no reliability estimates of these measures are offered—but they are needed—because even with meaningful sample sizes these scales could still emerge as relatively independent because of an overabundance of error variance.

Finally, some validity data are provided in the third study, in which 17 of the 20 children in Study 2 (discussed previously) were assessed by the Stanford–Binet. These scores were then correlated with 10 of Spectrum's 15 measures. It is concluded that Spectrum's measures are getting at something unique, simply because the Stanford–Binet only lightly covaries with them and because they generate unique intellectual profiles in relation to four of the Stanford–Binet subscales. Yet the possibility remains that there may be little reliable variance in these measures to appreciably correlate with any external indicator.[3] And, if so, when contrasting Spectrum's measures with well-validated conventional instruments, unique profiles are essentially guaranteed. Such empirical results are further ensured when samples are highly restricted in range. These 17 participants all resided in upper-middle-class homes, and only ≤ 4 had IQs below 100 (cf. p. 95)! Later in the volume, this series of studies is referred to as a "sustained effort to evaluate Spectrum" (p. 223).

There are, however, advantages to conducting evaluations using small samples with multiple measures having dubious psychometric properties, especially if one "begins with the assumption that every child has the potential to develop strength in one of several areas" (p. 89). If, for research purposes, one defines strength in a given area as scoring at or beyond one standard deviation above the mean on an experimental measure (as Gardner does in the studies discussed previously), researchers are almost sure to find that everyone in their sample is "talented" or "at promise" for something. Unreliable measures with low intercorrelations help to protect this supposition from empirical falsification. They also enable researchers interested in multiple intelligences to bypass the positive manifold observed in the full range of talent (the positive intercorrelations cutting across mechanical, quantitative, spatial, and verbal abilities).

In conclusion, like Gardner's (1983) *Frames of Mind*, this volume is more of a literary exercise than an exposition emanating from sound scientific inquiry. This is not only our view but also appears to reflect the consensus of methodologically sophisticated reviewers of Gardner's work (Bouchard, 1984; Carroll, 1993; Messick, 1992; Scarr, 1985; Snow, 1985). Certainly, this volume does nothing to alleviate Sternberg's (1994) concern with respect to Gardner's seven intelligences:

> Curiously, to date, not only are the tests not well underway, but they have not yet been initiated. To my knowledge, there is not even one empirical test of the theory. . . . What is clear is that the anticipated program of research has not been forthcoming, and may never be. (p. 561)

Yet MI theory prompts some other tough questions as well, which are in need of answers. Are we truly willing to agree that all seven intelligences possess comparable social utility? Should more resources be devoted to the development of some intelligences in relation to others? How relevant is this taxonomy to people with IQs below 90 (over 25 percent of the population)? How do we deal with the fact that, if we paid more attention to general intelligence for allocating educational opportunity resources, social mobility would be more fluid (Bereiter, 1976; Humphreys, 1992)? That is, will a shift in emphasis, from conventional ability assessment to portfolio assessment, give us a more or less fluid society? And finally, is Gardner's supposition about human development really accurate? Namely, "My own observations suggest that rarely in life are the fates of individuals determined by what they are unable to do" (p. 205).

To say that Gardner has had an impact on the educational community would be an understatement. In the decade between publication of this volume and *Frames of Mind*, literally tens of thousands of pages of text have appeared (see Appendix C for a small fraction of published references), which frame and reframe the educational implications of Gardner's model. It has provided a fruitful challenge to our thinking and operations in our educational system. But to say that Gardner's ideas have not adequately met meaningful scientific criteria, over the same period, would be too generous. Based on our reading of the present volume and published work in the most prestigious scientific outlets for educational research (e.g., Gardner & Hatch, 1989), we, like others, find little empirical support for or against the unique features of Gardner's ideas. Before MI theory can be taken seriously by the scientific community and policy makers, Gardner's (1983) bold theoretical skeleton is in need of empirical flesh. Only after this is obtained will we be in a position to ascertain whether his framework ambulates scientifically with policy implications for our educational system. We hope that, if the next decade generates half as much empirical inquiry as the past decade has generated literary prose in response to Gardner's seven intelligences, we will be in an excellent position for substantively appraising the educational usefulness of Gardner's message. To be sure, this volume is authored by one of the great literary psychologists of our generation. But, as its author is fully aware, literary skill is only one important dimension of the human intellectual repertoire; and often for establishing the verisimilitude of novel scientific theories (even in the social sciences), it is frequently not the most important.

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Footnotes

[1] It is possible that we have an appreciable understanding of the relationship between abilities and performance for most educational–vocational domains. To the extent that nonintellectual factors are relevant to performance, and they undoubtedly are (e.g., energy, health, interest, motivation, personality, chance factors, and so on), an important question becomes: How much of individual differences in performance is attributable to ability versus nonability factors? The familiar ".50 barrier" in applied psychology may not be a barrier at all, but rather the asymptotic limit of what the ability domain has to offer; the remaining criterion variance might be accounted for by a host of disparate, nonability variables each adding a small, but unique, increment of validity.

[2] The error of omission that Gardner commits by not citing and substantively appraising Hunter and Schmidt's work is not a small one, given the agenda of this volume. He purports to sample widely from psychology and multidisciplinary contexts to marshal support for his theoretical ideas. Hunter and Schmidt's work on validity generalization (VG) has appeared frequently in high visibility outlets and, over the last 10 years, VG has been one of the most frequently discussed topics in the *Annual Review of Psychology* (cf. Lubinski & Dawis, 1992; & Schmidt et al., 1992, and references therein). There is a rule in philosophy of science called the Total Evidence Rule. It was given to us by Carnap (1950), and it holds that when evaluating a substantive hypothesis or theoretical conjecture investigators are required to assimilate and weigh all available evidence that speaks to the scientific significance of the concept under analysis. For anyone interested in school and work performance, it behooves them to amass the findings on VG. Gardner's comment that IQ tests and other indices of the general factor are fine for predicting school performance "but are only an indifferent predictor of performance in a profession after formal schooling" (p. 14) is dated. This was believed to be true years ago, but as the quality of criterion assessments improved and sample sizes became more respectable, the forecasting efficiency of the general factor in industrial and vocational settings unequivocally revealed itself. In the words of Paul E. Meehl (1990), "Almost all human performance (work competence) dispositions, if carefully studied, are saturated to some extent with the general intelligence factor g, which for psychodynamic and ideological reasons has been somewhat neglected in recent years but is due for a comeback (Betz, 1986)" (p. 124). Yet Gardner's analysis motivates him to conclude that, "a focus on testing for an allegedly general ability is no longer tenable" (p. 242).

[3] This is the same point McNemar (1964) made in his *American Psychologist* classic, "Lost: Our Intelligence? Why?," with respect to early attempts at measuring creativity. Some builders of these early measures argued that they were getting at something unique, because their scales correlated so lightly with general intelligence. What they failed to demonstrate, however, was whether their new assessment tools tapped meaningful psychological phenomena beyond general intelligence—it turns out that they did not.

A Response on Four Fronts

The following is an invited response by Howard Gardner to David Lubinski and Camilla P. Benbow's review of his book Multiple Intelligences: The Theory in Practice.

I appreciate the opportunity to respond to the review by Lubinski and Benbow. I cannot say "review of my book," because their essay constitutes an attempt by the authors to grind their own axes, or, to twist the metaphor, to execute a hatchet job. I reply on four fronts.

Scope

A review should convey to the reader the intention and scope of the book. *Multiple Intelligences: The Theory in Practice* is not an extension of the theoretical or empirical work summarized in *Frames of Mind: The Theory of Multiple Intelligences,* but rather a report on various efforts to reform precollegiate education along lines suggested by multiple intelligences (MI) theory. Included in the collection are answers to frequently posed questions about the theory, descriptions of several programs implemented at levels ranging from kindergarten to high school and encompassing participants running the gamut from the arts to study skills, as well as essays about schools in the future.

One section, covering about a sixth of the book, is directed toward assessment, and even that section stresses the importance of going beyond assessment per se. The key chapter, "Assessment in Context: The Alternative to Standardized Testing," discusses a number of the issues that also concern Lubinski and Benbow.

Accuracy

My attention was caught by Lubinski and Benbow's remark that, on page 184, I recommend the abandonment of Standard Achievement Test (SAT) and American College Test (ACT) exams. I quote my actual words:

> In my view, there is little need and little advantage to be gained by continuing to require the Scholastic Aptitude Test (I have fewer reservations about the achievement tests).

> Most colleges are not selective enough to warrant such an instrument, and those that are have sufficient additional sources of information about their candidates. (p. 184)

I go on to point out,

> there are risks and expenses involved in a shift to a wider and more flexible set of instruments but to my mind these are worth taking. The very fact that some schools have already taken them—and these are among the very best schools—shows that my vision is not utopian. (p. 186)

Such misleading paraphrases lead me to suggest that readers consult the book itself, where they will find that a "zero-sum game" occurs in the minds of Lubinski and Benbow rather than in the words of Howard Gardner.

Substance

In the aforementioned chapter, and in other passages (as in the one just cited), I consider some of the issues involved in a shift to performance-based assessments. Lubinski and Benbow show scant awareness that the current short-answer examinations, emphasized far more in the United States than in other industrialized societies, have had quite destructive influences. Such tests all too often constrain what goes on in American classrooms; and, as dramatized by the content of and the response to *The Bell Curve*, they can also inflict pain on larger society as well. The continued reliance on subtests like Reverse Digit Span does send out an unhelpful signal; we should be looking at abilities that truly matter (like understanding the relations among numbers) rather than abilities that might correlate with abilities that we truly value. If we want to have schools in which youngsters learn what is worth knowing, and strive to use their minds well, and if we want a society in which individuals are valued for what they can actually do, then an exploration of new forms of assessment becomes not an option, but an imperative.

I am of course aware of the work of Hunter, Schmidt, Bishop, and others, which I review in a forthcoming textbook on intelligence. That work is interesting but controversial and limited (see forthcoming critiques by Richard Murnane, Frank Levy, Earl Hunt, and others). In a sense the Hunter–Schmidt line of research bypasses performance at both ends: The correlations are typically between test scores and supervisers' ratings. A more genuine performance-based instrument would look at a sample of work in, say, an assessment center, and then examine actual productivity on the job.

What of efforts to assess multiple intelligences themselves, along the lines suggested by standard psychometrics? Such efforts certainly can be conducted; indeed, *Multiple Intelligences* cites a number of individuals who have developed MI instruments, and I know of other more recent efforts. I have personally been leary of these efforts, for I feel that they run counter to the contextualized bases of the theory (so I have often argued "intelligences should be assessed directly and not through the lens of a standard test" [p. X]); putative "MI tests" are as likely as not to repeat the sins of traditional testing to which I have already alluded. The few empirical demonstrations reported on in the book are just that: efforts to show that one can make rough-and-ready performance-based assessments of the intellectual strengths of young children and that the results of these surveys are consistent with the major claims of the theory. They are not introduced as ersatz national exams but rather as useful classroom assessments.

Contrary to what Lubinski and Benbow say, much current experimental and empirical work bears on the claims of MI theory: As instances I can mention Rosnow, Skleder, Jaeger, and Rind's (in press) work on personal intelligence and Rauscher, Shaw, and Ky's (1993) work on the relation between musical and spatial abilities. But as I indicated, review of such work was not within the scope of *Multiple Intelligences*.

Rhetoric and reality

I have no objection to criticism; I can give it and receive it. Lubinski and Benbow correctly cite individuals who have critiqued MI theory and I can add to their list! However, the argument from authority is a weak one at best, and theory evaluation is not a bean-counting game. *The Wall Street Journal* was able to find 52 psychologists who would endorse much of *The Bell Curve*, but I was certainly not impressed, particularly when I considered how many of the signers have a vested interest in the survival of psychometrics. Although I do not object to criticism, however, I frankly resent the snide tone of the review written by Lubinski and Benbow.

Rhetoric is one thing, reality is another. As I pointed out in my review of *The Bell Curve* (Gardner, 1995), much psychometric work has been carried out with a callous disregard for the uses to which such instruments have typically been put, and most especially, to those uses that represent a disservice to the broader society.

MI theory has garnered unexpectedly wide appeal in part because it offers a more generous—and, I fervently believe—a more accurate view of the human mind. My own turn to the classroom has been motivated by a desire to improve the performances (and hence the lives) of school children. Naturally, I would like to obtain the most convincing evidence for any effects that might be achieved, and I join Lubinski and Benbow in hoping that the best measures can be devised, even if none of us turn out to be the devisers. In the meantime, I am content to let MI theory—and its rivals—be assessed by two criteria: concordance with the accumulating information about the human brain and the human mind and contributions to the educational welfare of school children.

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An Opportunity for "Accuracy"

The following is a rejoinder by David Lubinski and Camilla P. Benbow.

In a section titled Accuracy, Gardner begins, "My attention was caught by Lubinski and Benbow's remark that, on page 184, I recommend abandonment of Standard Achievement Test (SAT) and ACT exams" (p. 938). He goes on to quote his "actual words" (words that communicate a more neutral stance); but these are not the words we had in mind. They come at the end of the same paragraph. "I would like to see leading colleges follow the example of Bates College and Franklin and Marshall College: They should dispense with the requirement of

the Scholastic Aptitude Test and its counterpart instruments" (p. 184).

Gardner then goes on to say, "Such misleading paraphrases lead me to suggest that readers consult the book itself, where they will find that a 'zero-sum game' occurs in the minds of Lubinski and Benbow rather than in the words of Howard Gardner" (p. 939). We agree with Gardner that readers should consult the book itself rather than simply relying on "misleading paraphrases" in our exchange. We are extremely comfortable with readers deciding for themselves. We stand behind our review. ■

On Accuracy

The following is a final response by Howard Gardner.

Lubinski and Benbow are dismissive about "literary" matters, but literacy matters. I did not recommend "abandonment" of the Standard Achievement Test (SAT); as indicated in the quoted passage and later in the response, I favor making the SAT optional (p. 186, line 10). The zero-sum remains just where I originally placed it—in the imaginations of the reviewers. ■

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The Publications and Communications Board has opened nominations for the editorships of the *Journal of Experimental Psychology: Animal Behavior Processes*, the "Personality Processes and Individual Differences" section of the *Journal of Personality and Social Psychology*, the *Journal of Family Psychology*, *Psychological Assessment*, and *Psychology and Aging* for the years 1998–2003. Stewart H. Hulse, PhD; Russell G. Geen, PhD; Ronald F. Levant, EdD; James N. Butcher, PhD; and Timothy A. Salthouse, PhD, respectively, are the incumbent editors.

Candidates should be members of APA and should be available to start receiving manuscripts in early 1997 to prepare for issues published in 1998. Please note that the P&C Board encourages participation by members of underrepresented groups in the publication process and would particularly welcome such nominees.

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