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Applying the Theory of Successful Intelligence to Education

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Applying the Theory of Successful Intelligence to Education—The Good, the Bad, and the Ogre

Commentary on Sternberg et al. (2008)

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ABSTRACT—Sternberg, Grigorenko, and Zhang (2008) have presented a creative model for the application of research on intelligence and personality to education. There are many positive aspects of their effort. These include the coordination of individual differences in cognition and personality and their extension of the concept of intelligence beyond skills that can be tested in the normal testing paradigm. Although various parts of their approach have appeared elsewhere, the development of a single package containing all these ideas is a worthwhile improvement in thinking about education. They cite numerous studies in support of their efforts. However, a close examination of some of the major studies show that this evidence, though not inconsistent with their claims, is not as strong as their review indicates. If one applies conventional psychometric criteria, simpler explanations than those they propose could be found. Their model of thinking styles is probably too complex for many practical applications. However, a good case can be made that the appropriate criterion is "How useful will this approach be in education?" By this criterion, the total package is impressive and should be seriously considered by educators.

Sternberg, Grigorenko, and Zhang (2008, this issue; henceforth, Sternberg et al.) describe how Sternberg's theory of successful intelligence can be applied in educational settings. I heartily applaud the many efforts that Sternberg and his colleagues, both here and elsewhere, have made to apply theories of individual differences in cognitive capacity to education. I would like to be a cheerleader for them. Alas, "cheerleader" is not one of the categories in Sternberg et al.'s many classifications of intellectual styles. Also, I cannot do a back flip. But I can fit in to their classification somewhere.

Sternberg et al. describe three classes of intelligence—analytic, practical, and creative—along with three classes of styles—legislative, executive, and judicial. They also offer ingenious, worthwhile programs building educational programs around these classes. In their own terms, they have exercised creative intelligence in legislative mode. I shall try to apply analytic intelligence in judicial mode.

Judicial analysis can be nasty. Therefore, let me call attention to my title, which merges the title of one good movie *The Good, the Bad, and the Ugly* with the theme of a very different movie, *Shrek.* There is substantial good and (unfortunately) some bad in the Sternberg et al. effort. As for ugly? In *Shrek*, the beautiful princess turns into a female ogre. She then looks pretty good to Shrek, who is also an (amiable) ogre. A lot depends on your perspective.

THE GOOD

When I was in high school many, many years ago, I was required to take Latin. The Latin teacher had a simple theory of learning, which he generously shared with his students:

"You are here to learn. And what do I mean by learn? Memorize, retain, and be prepared to recite." (The author's unloved but unforgotten Latin teacher, circa 1947).

Sternberg et al. do not hold to this theory of education. Neither do many modern educational psychologists. Understanding, not reciting, is said to be the goal of education (Bransford, Brown, & Cocking, 1999). Modern educators also assume that, in order to impart understanding, the teacher needs to know what the student already understands. In science education, diSessa (1993) and Minstrell (2001) have argued that a teacher should first try to find out both what students think about a topic before instruction and, possibly even more important, how they think about it. For instance, some of the best arguments for the principles behind Einstein's theory of relativity are based on logical contradiction: if A then B, then if ~B has been observed, ~A must be the case. This is not an acceptable form of reasoning for many people. A good teacher will find what arguments his/her students will accept at the outset of instruction. Sternberg et al. want the teacher to find out what form of thinking students excel at and to teach accordingly. In fact, they go further. They argue that personality traits should also be considered.

Such an endeavor presumes the following: (a) that Sternberg's theory of successful intelligence and model of self-government accurately describe individuals, (b) that appropriate techniques are available to respond to people after they have been evaluated, and (c) that it is pragmatically possible to adjust instruction to the need of individual students. Sternberg et al. only attack the first two of these issues, which they claim to have solved. In practice, constraint (c) could prove to be limiting, but this is not a scientific issue. I deal only with (a) and (b), and first look at the good in Sternberg et al.'s proposals.

To borrow from Greek mythology, I propose that there are four Graces in Sternberg et al.'s work. The first Grace is Sternberg et al.'s extension of the concept of intelligence beyond the capabilities evaluated by conventional intelligence tests. Sternberg et al. acknowledge that present intelligence tests (and the chastely named surrogates that correlate highly with avowed

intelligence tests, such as the SAT) do a nontrivial job of sorting people out in both academic and industrial personnel selection (cf. Gottfredson, 1997; Hunt, 1995). The validity coefficients are around .5. This leaves plenty of room for improvement.

Extensions beyond a single factor theory of intelligence (*g*) are not new. In the Cattell–Horn model of intelligence, two major dimensions are recognized: fluid intelligence (Gf), which corresponds closely to Sternberg et al.'s concept of analytic intelligence, and crystallized intelligence (Gc), which refers to the ability to solve problems by applying previously acquired knowledge. Cattell (1971) said that a complete theory of Gc would have to encompass special tests for different occupations and cultures. In other words, Gc is conceptually identical to what Sternberg et al. (and Sternberg himself, in many other publications) call "practical intelligence." Pragmatically, though, the testing community has developed Gc tests aimed at a sort of lowest common denominator of what you can expect a young American or European adult to know. Sternberg et al. have renamed and utilized Cattell's concept the way I think he meant it, by developing the various specialized tests he said would be needed.

This is not a backhanded compliment or an accusation that SGZ cynically amplified on Cattell's ideas (without credit). The distinction between Gf and Gc is hardly a new one. Juan Huarte de San Juan (1575/1991) distinguished between problem solving by imagining what could be or by remembering what might happen about 400 years before Cattell did, and Juan Huarte did not get proper credit from Cattell! Such is fame. Sternberg et al. and their colleagues go beyond observing the need for tests of general reasoning and locally specialized tests of practical/crystallized intelligence to producing the tests. That is a major advance.

Very much the same can be said about Sternberg et al.'s emphasis on "creative intelligence." The idea that creativity tests should augment intelligence tests is not new. Torrance

and his colleagues have shown the utility of doing so in both academic and nonacademic settings (Plucker, 1999). There is nothing wrong with building on the ideas of others. Repackaging those ideas into a comprehensive, easily understood set of notions is far from silly. And this is what Sternberg et al. have done. They have placed some very good ideas, previously sold separately, into an integrated set of concepts that can easily be understood and, with more difficulty, implemented in educational settings. That is important.

The second Grace is the way that Sternberg et al. go beyond naming analytic, creative, and practical intelligence. They break them down into identifiable component processes that can hopefully both be tested and trained. There has been a great deal of discussion about the need to train a general ability to think. Sternberg et al. offer some specific guidance.

The third Grace almost slips in. However it may be the most important Grace of all.

In discussing the brand-new Kaleidoscope project for college admissions (at Tufts University, where Sternberg is currently Dean), Sternberg et al. describe how applicants write brief stories, apparently at their own pace, as part of the admissions process. This is an extremely important expansion of the concept of intelligence. Binet and the thousand or so psychologists and psychometricians who followed him have shown, to their credit, that it is possible to evaluate important cognitive talents within a relatively brief testing session. This was an important advance in personnel screening, but somehow these pragmatically useful tests have taken over our thinking about intelligence.

Although there are public statements to the contrary researchers in the field of practical intelligence seem to accept Boring's (1923) dictum that intelligence is what the tests test. The conventional test paradigm cannot evaluate skills involving extended attention, the ability to take multiple perspectives on a problem, priority setting, or, as Sternberg et al. have realized, most of

creative thought. (For an elaboration on this point, see the discussion of the nature of intelligence in Hunt & Carlson, 2007a.) It may be that conventional testing can identify individuals who have these skills on at least a probabilistic basis. However, it would be far better to make direct evaluations of the skills that "drop-in-from-the-sky" testing misses. Hopefully the results from the Kaleidoscope project will be a step in this direction.

We come now to the fourth Grace: the way that Sternberg et al. break down the distinction between personality and intelligence. This is good. To predict complex behavior, one must consider both a person's cognitive power and how that power is likely to be displayed (or "can do" and "will do"). I, and others, have pointed out that intelligence and one conventional aspect of personality, conscientiousness, are essentially additive predictors of performance in the workplace (Hunt, 1995; Schmidt & Hunter, 1998). Skills in self-management and willingness to delay gratification are important predictors of academic success (Duckworth & Seligman, 2005). To predict success in virtually all fields, we need to know the interplay between ability, personality, and interests (Ackerman & Beier, 2001).

Combining intelligence and personality is consistent with cutting-edge modern research. Neuroscientific studies of decision making and reasoning have shown that one of the important aspects of reasoning is managing differing inputs from the reasoning system and the motivational-reward system. See Posner and Rothbert (2007) for some interesting examples. The battle between your desired weight and chocolate cake is just one of the many battles in the war between the anterior and posterior cingulate cortex (and their allies). Some people perform better than tests of purely cognitive characteristics would predict, and some perform worse. This phenomenon is important in both industrial and academic settings. Outside of the testing paradigm, cognition and personality are part of the same package.

That finishes my description of the Graces. I now turn to the Furies.

THE BAD

Sternberg et al. offer an interesting metaphor. They describe the legislative personality, who likes to develop broad plans and ideas but has less interest in their execution. In their words, the legislative mind may not see the trees for the forest.

Sternberg et al. are in full legislative mode. They have flown us over a forest of experiments and shown us a beautiful canopy of ideas. If we walk slowly through the forest, we find that some of the trees (experiments) holding up that canopy are not as healthy as they appeared to be from the air. It would not be appropriate, or even possible, for me to critique all of the studies that Sternberg et al. cite. Here I take a fairly close look at three of their cognitive studies and their model of personality. Think of them as four trees that the Furies landed on.

Here is the tree the first Fury landed on. Sternberg et al. use a study in which high school students were enrolled in a special preparatory course at Yale University to illustrate how the theory of successful intelligence can be applied to teaching. The students were first sorted into three groups based on whether they were strong in analytic, creative, or practical intelligence. They were then assigned to instructional groups that emphasized, in Sternberg et al.'s words, "memory, analytical, creative, or practical instruction." According to the theory, the students who were given instruction appropriate to their strength should do better than students who had a mismatch between their strength and the style of instruction. Sternberg et al. claimed that this is exactly what happened.

Brody (2003) has published a careful critique of this study. He concluded that the test results were most economically described by a model of general intelligence, and that the study was marred by dropping a fairly large number of participants.

Sternberg (2003) published a reply to Brody in which he failed to challenge any of Brody's criticism. Instead he said that he and his group had prepared new techniques for measuring creative and practical intelligence, including asking examinees to comment on video presentations. In 2003, Sternberg was correct in saying that work on the theory of successful intelligence was just beginning, and that it was reasonable to have to refine measures. Still, it bothers me that Sternberg et al. still cite this study 5 years later as strong evidence for their theory, without a hint of a response to Brody's cogent criticisms.

The tree the second Fury landed on contains the measures that are supposed to be used to evaluate Sternberg et al.'s model of intelligence. Some of the "new measures" that Sternberg (2003) referred to were incorporated into the battery used in the RAINBOW Study (Sternberg & The Rainbow Project Collaborators, 2006), which Sternberg et al. cite as strong evidence for their model. They correctly say that measures of analytic, practical, and creative intelligence did predict college grades over and above the prediction that could be achieved with the SAT. But they gloss over some problems.

The RAINBOW study was a heroic and, inevitably, only partially successful attempt to evaluate applications of the theory of successful intelligence in tertiary education institutions that ranged from community colleges in Orange County, CA, to Yale University. Participation across schools was uneven: The school with the highest participation was Brigham Young University (N = 139), and the school with the lowest participation was Florida State University (N = 7). By design, no participant took all tests, something that would have been virtually impossible to arrange. The analysis of this statistician's nightmare was, in my opinion, just about the best it could have been. Sternberg et al.'s reporting of it is accurate, as far as they could go. There are some further facts about these results that concern me.

The SAT is, in Sternberg et al.'s terms, a test of analytic intelligence. Therefore, one might expect the analytic test in the successful intelligence test battery to be redundant to the SAT. It appears to have been. One could also suppose that the addition of tests of creative and practical intelligence would add to the prediction of GPA beyond the prediction that can be obtained using the SAT alone. This did happen, but it happened in an unusual way. The added prediction was achieved by the common variance in practical intelligence test measures and one of the measures of creativity: production of oral stories. As Brady argued, the common variance in practical intelligence measures may well be a representation of *g*, a point also made by Gottfredson (2003) in her critique of research on practical intelligence. This point is reinforced by the fact that the practical intelligence factor had correlations of .54 and .57 with the SAT-V and SAT-M scores, respectively (Sternberg & The Rainbow Project Collaborators, 2006, Table 8).

There were three measures of creativity: evaluations of both the examinees' written and oral stories, and an evaluation of examinees' titles for cartoons. The added predictivity associated with creativity was entirely due to the oral stories measures. Written stories added nothing. This was not because the measures of written stories were redundant to those of oral stories, for the correlation between written and oral stories was only .07 (Sternberg & The Rainbow Project Collaborators, 2006, Table 7).

What we have here is a case in which the pattern of correlations was interesting but was not quite what one would expect. Replicating the RAINBOW study exactly would be a very difficult administrative task. However, I do not think that the results provide a strong platform for a theoretical structure. What is needed are replications of some of the key findings that also

include the negative findings. Then it would be appropriate to proceed to application of the theory. But replication is needed.

Sternberg et al. claim that the successful intelligence measures reduce the proportion of variance in test scores explained by (more properly, "associated with") ethnic status. Because this is a socially important issue, it warrants special attention. The evidence they have is weaker than their statements suggest.

The basis for their claim is that the omega squared measure across ethnic groups, as shown in Figure 2 of the Sternberg et al. article, is lower for ethnic groups than are SAT measures. However Sternberg et al. do not break down ethnic scores by group. In particular, they do not distinguish between Asians who, in this study and in general, have higher SAT scores than Whites (Sternberg & The Rainbow Project Collaborators, 2006, Table 4), and the other groups, who have lower test scores. This is important because Asians (77) and Latinos (89) constitute the majority of the minorities in the sample, as compared with Blacks (47) and Native Americans (11; Sternberg & The Rainbow Project Collaborators, 2006, Table 1). Thus, when we look at effects on minorities, we are looking largely at comparisons of Whites to Asians and Latinos, two very different minority groups.

By far the biggest "ethnic relevant" omega squared for SAT is in the SAT verbal score. Interpretation of this is clouded by an interaction between location and type of institution and enrollment. Of the 77 Asians who participated, 32 were enrolled in community colleges in Orange County, CA, or in California State University, San Bernardino (CSUB). Of the rest, 36 were enrolled either at a specialized engineering school in the Eastern U.S. or at Yale. In terms of student characteristics, the Asian students presented a mixed bag. (I also point out that the general practice of referring to "Asians," who constitute about half of humanity, lumps together

quite disparate groups.) At the least, any comparisons of "minorities vs. Whites" should have treated Asians separately, and in this case a comparison of Whites and Asians would be confused by the heterogeneous makeup of the Asian group.

The situation is similar with respect to Latinos. Of the 89 who participated, 58 were enrolled in community colleges in Orange County (28) or at CSUB (30). The Southern California area, including Orange and San Bernardino counties, is an initial target area for immigrants from Latin America and Asia. It may well be that for a substantial number of students, ethnic status is confounded with speaking English as a second language. It is hardly surprising that this will affect verbal measures. My belief in this conclusion is strengthened by Sternberg et al.'s statement that "Latino American students benefit the most from the reduction of group differences."

The largest omega-squared value in the Sternberg Triarchic Abilities Test is for the oral stories measure. The measure is trivially different from the figure for the SAT math test (.03 vs. .04). But the creative oral stories test is the only single measure from the Sternberg Triarchic Abilities Test to add predictive power to SAT scores (see above). If we stick to the statistical evidence, Sternberg et al. are justified in saying "We can reduce the variance associated with ethnic status providing that you let us put in a lot of tests that do not predict criterion performance." I do not think that is the message they want to give.

At one point, Sternberg et al. say something that is accurate but that I think would have best been left unsaid. They say " …differences can also be seen for Native American students relative to European American students. Indeed, their median was higher for the creative tests. However, the very small sample size suggests that any conclusions about Native American performance should be made tentatively."

There were 793 students in the RAINBOW study. How large do you think the Native American group should be to justify the above statement and the qualifier? To frustrate people who read ahead, I give the actual size of the Native American sample in a footnote.[1]

I point this out because I believe that when reporting results that can have social consequences, it is important that the science be very good. See Hunt and Carlson, (2007a, 2007b) for elaboration. This caution applies just as much to studies claiming to have reduced ethnic differences as it does to studies claiming to have uncovered such differences.

The RAINBOW study was an important study. It unveiled many leads that should be followed up. At present, though, sweeping generalizations based on it are not appropriate.

The third Fury landed on a tree planted in the current Sternberg et al. article. Sternberg et al. report a large ($N > 4{,}000$) study of an application of the theory of successful intelligence in the 4th grade. They present considerable detail, because no account of it appears elsewhere. On the one hand, it is not fair to expect Sternberg et al. to report in the detail that would be presented in a research study. On the other hand, this means that they (perhaps inevitably) omit some details that make all the difference in the world when you evaluate the study. I will raise some questions. They should not be interpreted as attacks on the study, because I do not know the answers to these questions. My point is that you cannot regard the study as strong support for the theory until these questions receive a satisfactory answer.

According to Sternberg et al., the design involved contrasts between schools that taught using the theory of successful intelligence methods, those that taught using a critical thinking curriculum, and those that taught using a "memory" curriculum. As my old Latin teacher is now long since departed, I wondered where in 21st century American schools you would get any

superintendent to say that his or her district used a "memory" method of instruction. This thought lead to more serious concerns.

Who designed the two comparison curricula? How were the teachers trained to use each set of materials? If the successful intelligence group was involved in the design and training for the two control groups, one has to be concerned that they may, quite unconsciously, have designed stalking horses. How were the teachers assigned to groups? Random assignments of teaches to conditions is extremely hard to achieve in the schools, not in the least because individual teachers often have considerable freedom in determining the style of instruction in their classroom. If the assignment resembled anything that looks like "volunteers against the rest," we immediately have a problem in interpretation.

The results (their Figure 1) are reported in terms of gain scores, without error bars to indicate variability. Initial scores are not reported. Although the psychometric techniques used for analysis were probably sufficient to counter a statistical objection to the use of gain scores, knowing the initial values is important for nonstatistical reasons. In practically all learning situations, a fixed amount of training will lead to diminishing returns as initial level of learning increases. Indeed, as a practical matter, there may be more interest in moving students' grades from C to B than from B to A. If all we are given is gain scores, we have no way of assessing the extent to which this is an issue.

The results appear to have been concentrated in tests of practical and creative ability. But, from what little description is given of the curriculum, it appears that only the students in the successful intelligence group went through practical and creative exercises. The fact that you learn what you practice is well known.

Again, I am not saying that this study is invalid. I am saying that publishing sketchy results of a complicated study that is not fully described in publicly available sources is not a good idea, because readers have no way of evaluating the argument.

My fourth Fury did not land in a tree—it landed on a bramble bush: Sternberg et al.'s discussions of personality and intellectual style. Although I applaud their overall attempt to unite personality and intelligence, I wonder about the particulars.

The first thing that struck me is that we already have a model of personality, the Big Five model. Would it not be better to work from that model, for which there is a great deal of data, than to start with a completely new model?

My desire for greater connection with existing theory is amplified by the complexity of their proposed theory of mental self-government. There are 96 combinations of function, form, level, scope, and leaning in the theory of mental self-government. I do not see how a theory with 96 facets would be of any help to a practicing teacher or superintendent. Even if the classroom contained 60 students, that would only be 2/3 of a student per cell!

Sternberg et al. do not deal adequately with the distinction between personal and situational influences. Are some people legislative, executive, or judicial in all things? Or does it depend on the situation? If so, what are the circumstances, and how does a teacher adapt to intrapersonal variation? These are very difficult issues. Sternberg et al. seem somewhat glib when they assume a trait-based answer.

There seems to be a good deal of social desirability built into Sternberg et al.'s model. Is it also built into the questionnaires used to characterize people? If so, I have a concern. I think they may undervalue the utility of an authoritarian, take-charge attitude in many situations. To be fair, this is a view that they share with many social scientists and even with academics in general.

Which leads me to my final evaluative comment on specific issues. Here the Graces unite to scare off a Fury.

Sternberg et al. somewhat obliquely raise a very important issue. Students have certain expectations about how teachers are supposed to act. Sometimes students want to engage with teachers in a quest for education. In that case, and if the students see the give-and-take of inquiry oriented instruction as an appropriate manner of adult–child interaction, Sternberg et al.'s stress on liberal, interactive teaching is appropriate. Although educators might not like this, in many cases students see preparation for the examination as the most important goal of teaching. I think that this is particularly likely to be the case if the students interpret a situation as one in which the goal is to be certified rather than to be intellectually enriched or if the course is clearly required but the rationale behind the requirement is not clear. (How many middle and high school students have ever seen an adult actually using trigonometry?) When students see the educational situation this way, the wise educator either adopts my Latin teacher's approach to instruction (with a few more jokes than he ever offered) or spends some time explaining to the students why they are there at all. This can be difficult.

This concludes the forays of the Graces and the Furies. How to sum up support for the theory of successful Intelligence? I return to the forest metaphor.

Suppose you wanted to know if there was significant animal biomass in a forest. If you saw a moose, you would know there was. If you saw a mouse, the question would be in abeyance until you saw another mouse, and another, and another. (And, in fact, the biomass of the mice in a real forest does exceed that of the moose.) I do not know of any one study that Sternberg and his colleagues have done that convinces me that the theory of successful intelligence would be a useful framework for educational reform. In spite of claims to the contrary, there is no moose.

However, there are an awful lot of mice, studies that are consistent with the theory of successful intelligence, even though these studies are also consistent with other, conceptually simpler theories (even *g*). So how do we balance the good and the bad?

THE OGRE

Shrek and his princess show that ugliness is in the eye of the beholder. So it is with the theory of successful intelligence.

Models of human intelligence are used for three purposes. The first is to summarize the data from a large body of empirical data. The second is to place individual differences in a causal web of biological and environmental variables. The third is to guide personnel classification, education, and training. What sort of theory you want depends on which of these aims you are pursuing.

As Brody and Gottfredson pointed out, if you apply statistical criteria for data summarization of many of Sternberg's studies, there is little advance beyond *g*. I would amplify that by referring to an expanded Gc–Gf model (Carroll, 1993), with a dash of Torrance's work on creativity thrown in. With respect to reductionism, no effort has been made to connect the variables in the theory to biological or environmental causes. I doubt that such an effort would be worthwhile, as the best models for this purpose are models whose dimensions of intelligence are closely linked to biology (e.g. Johnson & Bouchard, 2005).

Things change markedly if we judge the model by how well it provides guidance in solving some real-world problem—in this case, various aspects of education. I have referred to the theory of successful intelligence as a repackaging. Education and training are fields in which packaging is extremely important. Building a good package is a nontrivial exercise in scientific creativity.

The theory of successful intelligence provides a unified program for classifying and educating students. Competing psychometric theories of intelligence provide only classification, with no guidance to the educator as to what to do next. Gardner's highly publicized theory of multiple intelligences is very weak on classification, which is an essential first step (Visser, Aston, & Vernon, 2006). The only model I know of that provides as complete a package for education as the Sternberg et al. model does is J.P. Das's model (planning, attention, simultaneous, and successive or PASS; (Das, Naglieri, & Kirby, 1994). This model does not have the strong tie to conventional psychometric theory that the theory of successful intelligence does, nor has it been applied over such a wide range of situations as the successful intelligence model has.

The tools for classification and education provided by Sternberg et al. and their colleagues are probably not quite as good as they claim (see **THE BAD** section earlier in this article), but enough tantalizing results have been obtained to show that there is something there. The model is not inconsistent with the data, although it may not be the most economic description in mathematical terms. There are still real issues of practicality. Given the constraints on educators, can we afford to match instructional styles to student styles? How do the gains balance out against the costs? These questions must be answered. That is for the future.

For the present, there are different ways to look at the program Sternberg et al. have put forth. If you look at it solely from the viewpoint of summarization of data, the evidence has just a tinge of ugliness. It's oversold. If you look at it from the viewpoint of reductionism, it's irrelevant. If you look on it as the use of a theory of intelligence in educational settings, the criteria change—it is an interesting and worthwhile effort. It should be and will be applied in many future studies.

My advice to educational psychologists? Check it out. There may well be something there that you can use, even if you do not buy the whole package.

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[1]With respect to creativity, Native Americans were superior to Whites on just one measure, oral stories. The statistics were $d = .50$, $N = 2$. In practical measures, they were superior to Whites on the College Life questionnaire, $d = .20$, $N = 3$. See Table 15 in Sternberg and The Rainbow Project Collaborators (2006).