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Facts are stubborn things ☆

Phillip L. Ackerman *

Georgia Institute of Technology, USA

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ABSTRACT

Space limitations do not allow me to fully address Ericsson's comments. Instead, I limit my discussion to five of the most salient issues upon which there are significant differences in the evaluation of the existing theory, methodological issues, and data. These relate to Ericsson's use of the construct "innate talent;" his misapplication of Ackerman's (1987, 1988) theory of individual differences during skill acquisition; inadequate attention to selection of tests and consideration of Brunswik Symmetry; oversights and misinterpretations in evaluating the results from Masunaga and Horn (2001); and differences in interpretations of several other studies. In the final analysis, although there has not been a definitive longitudinal study of deliberate practice with random selection/assignment and a control group, there is ample evidence from over 100 years of research supporting the conclusion that abilities are significantly related to individual differences in the attainment of expert performance.

1. Introduction

1.1. "Innate talent"

As suggested in my initial article (Ackerman, in press), use of the term "innate talent" is a pernicious straw-man argument in any discussion of intellectual abilities and learning, skill acquisition and expertise (see Ferguson, 1954, 1956). If Ericsson (in press) fails to account for the fact that in children and adults, intellectual abilities — even though abilities may be partially determined by genetic factors — are the result of learning, transfer, and motivated engagement, there cannot be any useful scientific discussion based on his conceptualization of intellect as only "innate talent." Adolescents who have been raised in severely impoverished circumstances will have substantial intellectual deficits that will, for all intents and purposes, limit their future intellectual development, and in turn, limit their development of expertise in domains that require such intellectual abilities (see discussion by Ferguson, 1954).

2. Misunderstanding theory of individual differences during skill acquisition

Ericsson (in press) devotes a significant amount of text to discussing Ackerman's (1987) theory (though most of his discussion is about a theory explicated in Ackerman, 1988), but there is one major error in his interpretation vis á vis expert performance, and one significant oversight. The error of interpretation is that he equates learning relatively simple closed-ended tasks[1] (e.g., laboratory perceptual–motor tasks, simple addition or subtraction tasks) with the kinds of expertise in music, chess, and sports. Indeed, he says that "Expert performers in music, chess, and sports are constantly learning new things." (Ericsson, in press, p. 4). If that is indeed the case, and there is no inherent reason to doubt his assertion, such tasks are *not* closed-ended tasks, but rather substantially open-ended, and therefore do not resolve into sole dependence on psychomotor abilities. Thus, Ackerman's (1987, 1988) theory would clearly predict the involvement of a variety of cognitive abilities, especially those that match the same content-base of the target tasks (e.g., verbal ability for

☆ John Adams (1770). The entire phrase is "Facts are stubborn things; and whatever may be our wishes, our inclinations, or the dictates of our passions, they cannot alter the state of facts and evidence…"

* School of Psychology, Georgia Institute of Technology, 654 Cherry St., MC 0170, Atlanta, GA 30332-0170, USA.
E-mail address: phillip.ackerman@psych.gatech.edu.

[1] There is no discussion of closed-ended and open-ended tasks described by Ericsson (in press) in Ackerman (1987). There is such a discussion of this distinction in Kanfer and Ackerman (1989).

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verbal tasks, spatial ability for spatial tasks, etc.). The oversight by Ericsson is that in numerous studies subsequent to the 1987 explication of the theory, we have shown significant and substantial ability correlations with the acquisition of new knowledge (e.g., see Ackerman, 1992; Ackerman & Beier, 2006; Ackerman & Kanfer, 1993; Beier & Ackerman, 2005). Although these studies do not meet the criterion for "expert" performance, they clearly show that at intermediate levels of knowledge and skill acquisition, and for the skilled performance of open-ended tasks (e.g., air traffic control), individual differences in carefully selected ability measures are related to individual differences in performance even after practice.

3. Selected tests and Brunswik Symmetry

As described earlier, another key issue pertains to when Ericsson uses the absence of significant correlations to state: "My current conclusion is that these studies have *not yet* established the fact that the attainable level of domain-specific performance is predictable from scores from tests of general cognitive ability." (Ericsson, p. 20). There are multiple inherent problems here. First, the theory he cites (Ackerman, 1987) states that if initial performance of a task is beyond the abilities of some individuals, then those abilities will remain important predictors of performance even after extensive practice and expertise development (*as long as those individuals remain in the sample being examined — which never occurs in Ericsson's studies*). Second, Ackerman's theory explicitly states that abilities other than general intelligence (e.g., perceptual speed and psychomotor abilities) may be substantial predictors of expert performance. *None* of the studies cited by Ericsson included such measures, and indeed perceptual speed and psychomotor abilities are *not* general cognitive abilities. Thus, examining only IQ-type measures is essentially stacking-the-deck toward a "null result," and is not at all informative about predicting individual differences in performance among individuals who have engaged in deliberate practice.

Fundamentally though, the problem for Ericsson's approach is a failure to appreciate what Wittmann and Süß (1999) have called Brunswik Symmetry. When predictors and criterion measures are matched in terms of both breadth and depth, one can expect a maximization of criterion-related validity. Just administering convenient tests (e.g., Raven, Wonderlic, etc.) when they are not appropriate for the sample or the task under investigation, either in terms of reliability or validity — also when coupled with inadequate sample sizes, virtually guarantees a failure to reject the null hypothesis. A failure to reject the null hypothesis has no *probative* value when statistical power is low, or when measures administered are not appropriate to the sample or the research questions being posed.

4. Masunaga and Horn (2001)

Ericsson cites the Masunaga and Horn (2001) study as particularly supportive of his notion that intellectual abilities are not related to individual differences in expertise. Although the article presents an interesting analysis of attempts to predict expert performance in GO, Ericsson's claim that it supports his views is flawed in several important ways. First, like so many of the studies that Ericsson claims support his view, the study was not a random selection of participants (instead, they were all recruited from an association of GO players). Second, there are no data provided by the authors that describes their cognitive abilities with reference to the general population. Thus, it is entirely possible that these participants were all well above average on such abilities, just as it is possible that they weren't. Thus, one cannot use this study to argue that the participants weren't already substantially restricted in range-of-talent to begin with. Third, Ericsson neglected to mention that *every one* of the cognitive abilities assessed (see Table 6, Masunaga & Horn, 2001) was significantly and substantially correlated with measures of GO-embedded tasks. The *only* variable not significantly correlated with the cognitive ability measures was GO rank. The reasons for this may be obscure, but one cannot reasonably argue, as Ericsson does, that cognitive abilities are unrelated to individual differences in task performance for this particular investigation.

5. Other studies

Other studies cited by Ericsson as supportive of his arguments are frequently not at all supportive. He bases his argument about the unimportance of intelligence, with evidence and case studies about IQ and eminent scientists, Nobel Prize-winning scientists, architects, chess masters, and so on, yet in each group, these are clearly made up of individuals who have significantly higher IQ than the average population (either by direct measurement of scores above IQ = 120, or by virtue of these individuals surviving successive selection into colleges and graduate schools). In an example to counter my assertion that Ericsson had not conducted or reported a study that showed development of expert performance among subjects with intellectual deficits, Ericsson reports a study on expert taxi drivers, but then he notes that mean IQ in this group was 97.7 and a *sd* of 6.3, meaning that even someone with an IQ of 70 would be 4.40 *sd* below the group mean (a highly unlikely result [$p = .0000055$]). Such results simply reinforce my point that until Ericsson shows cognitive expertise development in a randomly selected group of subjects, including those with moderate mental retardation, there is no reason to believe that such development can be accomplished. In the absence of such data, over 100 years of intelligence and educational research results suggest that Ericsson's claims should be taken with great reluctance, because they may have highly disappointing results for parents and others who have great hopes, but no data on which to base such hopes.

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