Intelligence 45 (2014) 112–114

Contents lists available at ScienceDirect

Intelligence

Accounting for expert performance: The devil is in the details

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ARTICLE INFO

Article history:
Received 30 December 2013
Received in revised form 24 January 2014
Accepted 27 January 2014
Available online 24 February 2014

Keywords:
Expert performance
Deliberate practice
Talent
Ability
Intelligence

ABSTRACT

The deliberate practice view has generated a great deal of scientific and popular interest in expert performance. At the same time, empirical evidence now indicates that deliberate practice, while certainly important, is not as important as Ericsson and colleagues have argued it is. In particular, we (Hambrick, Oswald, Altmann, Meinz, Gobet, & Campitelli, 2014-this issue) found that individual differences in accumulated amount of deliberate practice accounted for about one-third of the reliable variance in performance in chess and music, leaving the majority of the reliable variance unexplained and potentially explainable by other factors. Ericsson's (2014-this issue) defense of the deliberate practice view, though vigorous, is undercut by contradictions, oversights, and errors in his arguments and criticisms, several of which we describe here. We reiterate that the task now is to develop and rigorously test falsifiable theories of expert performance that take into account as many potentially relevant constructs as possible.

We credit Anders Ericsson for generating interest in expert performance. Ericsson, Krampe, and Tesch-Römer's (1993) study of musicians has been cited over 4000 times (Google Scholar), and, as Ericsson (2013a) noted, was the "stimulus" for what Malcolm Gladwell (2008) dubbed the "10,000 hour rule." Ericsson has made an important contribution to psychology.

The goal of our study (this issue) was to test Ericsson et al.'s (1993) claim that "individual differences in ultimate performance can *largely be accounted for* by differential amounts of past and current levels of practice" (p. 392, emphasis added). The claim was not supported: amount of deliberate practice accounted for about a third of the reliable variance in performance in music and chess, leaving the majority unexplained. Ericsson (2014-this issue) claims we reject his view on a "common sense basis" (p. 18), but in fact, we reject it on this empirical basis.

Ericsson's (2014-this issue) defense of his view is, in our view, unsuccessful for several reasons. First, he rejects evidence that challenges his view even though he has used the same type of evidence to support his view. Specifically, he criticizes our analysis for ignoring "the effects of forgetting, injuries, and accidents, along with the differential effects of different types of practice at different ages and levels of expert performance" (p. 4), but has never included all of these factors in his *own* published analyses. Most notably, Ericsson et al. (1993) based their conclusion about the great importance of deliberate practice on the relationship between skill level in music and a single variable: self-reported amount of practice alone. Our reanalysis included studies that used Ericsson et al. as the model for measuring and operationally defining deliberate practice—indeed, our reanalysis included studies that Ericsson has praised for rigor and cited as support for his view (e.g., Charness, Tuffiash, Krampe, Reingold, & Vasyukova, 2005). We did no more than follow Ericsson's standards for evidence in selecting studies for our reanalysis.

Second, Ericsson (2014-this issue) contradicts claims he has made in the past. Most notably, Ericsson downplays the

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0160-2896/$ – see front matter
http://dx.doi.org/10.1016/j.intell.2014.01.007

emphasis he has put on deliberate practice, now suggesting it is just one of any number of factors other than innate talent that could affect performance directly. That is, he states that although we criticize him "for attributing too much emphasis to the effects due to deliberate practice" (p. 3), he and his colleagues were explicit that "there might be other types of individual differences than those linked to innate talent" (p. 3). However, in the past, Ericsson has argued that such non-talent factors (e.g., motivation) affect performance indirectly *through* deliberate practice. For example, he wrote, "The theoretical framework of expert performance explains individual differences in attained performance by the factors that influence the engagement in sustained extended deliberate practice, such as motivation" (Ericsson, 2007, p. 4; see Duckworth, Kirby, Tsukayama, Berstein, & Ericsson, 2011). Hypothesizing that certain factors affect individual differences in deliberate practice is not the same as hypothesizing that these factors affect individual differences in performance directly.

Third, Ericsson (2014-this issue) does not mention findings that contradict his view. For example, he notes Horn and Masunaga (2006) found no significant correlations between Go ranking and scores on intelligence tests, but fails to note that measures of Go-related performance *did* correlate significantly with scores on intelligence tests ($p < .01$ for 50 of 56 rs; see Masunaga & Horn, 2001). Similarly, he fails to note the central result of Meinz and Hambrick's (2010) study of sight-reading — the finding that there was no interaction between deliberate practice and working memory capacity, indicating that working memory capacity positively predicted performance even at high levels of deliberate practice. This finding is inconsistent with Ericsson's hypothesis that mechanisms acquired through deliberate practice enable circumvention of basic cognitive capacities. Describing the results of another study of sight-reading, Ericsson (2013b) claimed, "Kopiez and Lee (2006) found that for musicians with lower sight-reading skill there was a correlation with their working memory. For musicians with a higher level of sight-reading skill there was no significant relation between their performance and their working memory" (p. 236). In this case, Ericsson's error is one of commission: Kopiez and Lee reported no such finding.

Finally, Ericsson (2014-this issue) criticizes others' research based on what turn out to be material errors in his descriptions of that research. For example, he writes, "It is surprising that Hambrick et al. (2014-this issue) did not cite Howard's (2012) data for evidence of an elite chess player, who had *never* studied chess" (p. 14). If Howard had claimed he found evidence for an elite chess player who had never studied chess, one might wonder whether we did not report this finding because it seems implausible and would cast doubt on the validity of Howard's study, which we included in our re-analysis. However, Howard (2012) made no such claim. Moreover, ratings of individual players in Howard's sample cannot be determined from Howard's published report. As another example, Ericsson makes two errors in describing a study of chess by two of us. First, he writes, "Data was collected from 104 respondents, but Campitelli and Gobet's (2008) [sic] only analyzed 90 participants and did not describe the objective reasons for discarding 14 of the collected questionnaires" (p. 14). If this claim were true, one would be well advised to dismiss the results of that study. However, this claim is not true. Campitelli and Gobet did not discard collected questionnaires; rather, as they clearly explained in their article, there were missing data: "Not all players answered all questions, with the result that the number of data points varies across our measures" (p. 448). Second, Ericsson writes, "It would be nice to have Gobet and Campitelli (2007) conduct a re-analysis that would identify the amount of practice required prior to first attaining the rating of master" (p. 14). If Gobet and Campitelli had not performed this analysis, then they would have had no basis for their conclusion that, contrary to Ericsson's view, there is a large amount of variability in the amount of deliberate practice players need to achieve a given level of skill in chess. However, as they report in a major section of their article (pp. 165–166), Gobet and Campitelli performed *exactly* this analysis and found that amount of deliberate practice required to first attain the rating of master ranged from 728 to 16,120 h. Thus, one player reached the master level *twenty-two times* faster than another player.

Ericsson also calls attention to our reporting of Gobet and Campitelli's (2007) results. He writes, "Surprisingly, Hambrick et al. (2014-this issue) reports the lowest value for a chess master as 832 h instead of the 728 h as reported by Gobet and Campitelli (2007, p. 166) without providing an explanation for the difference" (p. 14), and "Surprisingly, Hambrick et al. (2014-this issue) reports the highest value for a chess master as 24,284 h instead of the 16,120 h reported by Gobet and Campitelli (2007, p. 166) without providing an explanation for the difference" (p. 14). The explanation is that the different numbers reflect different measures. The range of 832 to 24,284 h reported in Hambrick et al. (2014-this issue) is for accumulated amount of deliberate practice, the focus of our study. The range of 728 to 16,120 h reported in Gobet and Campitelli (2007) is for hours to master status. So, in several cases, what Ericsson characterizes as problems with studies that contradict his view are not problems.

The deliberate practice view has had a major impact on the trajectory of research on expert performance. But now we have empirical evidence that deliberate practice, while important, is not as important as Ericsson has argued it is—evidence that it does *not* largely account for individual differences in performance. The question now is what else matters.

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