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Author response

Music lessons and intelligence: Reply to commentaries

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This article is an author response to Bialystok's (2011) and Hargreaves and Aksentijevic's (2011) commentaries on Schellenberg (2011).

The commentaries by Bialystok (2011) and Hargreaves and Aksentijevic (2011) on my article (Schellenberg, 2011) raise interesting questions and concerns about the association between music lessons and intelligence. The authors are critical of my use of a high-functioning sample (Hargreaves and Aksentijevic, 2011) and the inclusion of children who were relatively unengaged in music lessons (i.e., quitting after 1 or 2 years; Bialystok, 2011). These criticisms imply a lack of statistical power due to a restricted range or noise, respectively, yet the between-group difference in full-scale IQ (FSIQ) was larger (10 points, 2/3 of an *SD*, $r = .39$) than that observed in my previous studies (Schellenberg, 2004, 2006).

Hargreaves and Aksentijevic (2011) suggest that the mediation hypothesis – executive function (EF) accounting for the link between music lessons and IQ – is more of a 'straw man' than a serious possibility. On the contrary, the hypothesis is well grounded in the literature, including reported associations between music training and EF (Bialystok & DePape, 2009; Bugos, Perlstein, McCrae, Brophy, & Bedenbaugh, 2007) and between EF and IQ (Ardila, Pineda, & Rosselli, 2000; Salthouse, 2005; Salthouse, Atkinson, & Berish, 2003), and claims that EF is the missing link between music training and IQ (Hannon & Trainor, 2007; Schellenberg & Peretz, 2008). Indeed, the empirical and theoretical foundation of the mediation hypothesis is considerably more solid than Hargreaves and Aksentijevic's (2011) hypothesis that high-IQ children take music lessons because music engages mechanisms subserved by the left and right cerebral hemispheres.

Hargreaves and Aksentijevic (2011) also question the construct validity of EF, both because of its overlap with IQ and because different tests of EF measure distinct aspects of cognition that are independent or weakly correlated. The results of the target article, along with other findings (Salthouse, 2005; Salthouse *et al.*, 2003), lend credence to such skepticism. The focus on individual differences in EF (for reviews see Banich,

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2009; Zelazo, Carlson, & Kesek, 2008) may stem, in part, from the relative immunity of EF to the controversies surrounding IQ tests (e.g., Neisser *et al.*, 1996).

Bialystok (2011) takes issue with three of my five EF measures. She considers the Sun–Moon Stroop Test unsuitable for children in the age range of my sample (9–12 years), but the data indicate otherwise. Specifically, a one-sample *t*-test of interference scores confirmed that children had difficulty in the incongruent (sun = 'moon') condition compared to the congruent (sun = 'sun') condition, $t(105) = 11.65, p < .0001$. Moreover, interference scores ranged from $-.47$ to .20, reflecting marked individual differences that were correlated positively with IQ and with scores on the Wisconsin Card Sorting Test. Bialystok (2011) also questions phonological fluency as a legitimate test of EF. Note, however, that Luo, Luk, and Bialystok (2010) used the same test in conjunction with a semantic fluency test, precisely because the semantic test tapped verbal representations and the phonological test measured EF.

I agree with Bialystok (2011) that only the backward condition of the Digit Span Test is a true measure of working memory and hence the purest measure of EF. Nevertheless, musically trained and untrained children performed similarly in this condition, $p > .1$. Had I reported performance in the backward condition only, there would have been no between-group differences on *any* measure of EF. Although the two groups also performed similarly in the forward condition, $p > .05$, a small but significant difference was evident for the aggregate (presumably more stable) measure. To avoid obscuring differences in EF between groups, I reported the aggregate measure. In any event, my set of EF measures was both valid and comprehensive.

Direction of causation is undoubtedly the most provocative issue. Both commentaries concur with my position: the association between music lessons and intelligence is complex and difficult to delineate clearly, and a consequence of nature *and* nurture. Whereas I accord primacy to inherited intellectual and personality tendencies, Bialystok (2011) argues that music lessons alter brain structure and function, with favourable consequences for EF and/or intelligence. It should be noted that alterations in brain structure and function arise from altered input of various kinds (e.g., athletic activities, blindness) without necessarily affecting EF or intelligence.

To support her position, Bialystok (2011) embraces the current neuroscientific obsession with plasticity, which she considers 'enlightened'. She also marshals the results from a single true (but artificial) experiment with random assignment that allowed for inferences of causation from music lessons to intellectual functioning (Schellenberg, 2004). Even within the modest time frame of that experiment (9 months), children assigned to music lessons dropped out at more than twice the rate of those assigned to drama lessons, which implies that factors such as conscientiousness must be considered when evaluating outcomes.

By contrast, my view is that the most parsimonious interpretation of the available literature is that children from well-educated and affluent families, who have intellectual as well as social advantages, are more likely than other children to take music lessons. These lessons may compound their initial advantages, much like the 'Matthew effects' (i.e., the rich getting richer) in reading and academic achievement (Stanovich, 1986).

As someone who took piano lessons from the age of 5–16, I have no doubt that countless hours of practicing, learning, and performing changed me in a multitude of ways. As a scientist, however, I am reluctant to link my non-musical achievements to those lessons. In any case, the decision about whether a child takes music lessons is anything but random, in line with concerns expressed by Hargreaves and Aksentijevic (2011). Rather, taking lessons is associated with a range of social and intellectual factors, including musical aptitude (e.g., Orsmond & Miller, 1999). For example, children who take music lessons have highly educated parents (Schellenberg, 2006, 2011, in press), and educational achievement is tightly and reciprocally linked with IQ (Ceci & Gilstrap, 2000; Scarr, 1992). Quasi-experiments or correlational studies that account for these sorts of extraneous variables tell us that the partial association between music lessons and IQ is significant without telling us anything about causation.

Regardless, the association between intelligence and out-of-school activities is unlikely to be limited to music lessons (Hargreaves and Aksentijevic, 2011). Rather, it extends to other activities that are cognitively challenging such as recreational reading (Winner, 1982). Music instruction is still unique, however, as a demanding out-of-school activity that is enjoyed by a sizeable minority of children, unlike, say, chess or math lessons.

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