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Raymond MacDonald, Gunter Kreutz, and Laura Mitchell

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Cognitive Performance After Listening to Music: A Review of the Mozart Effect

E. Glenn Schellenberg

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Abstract and Keywords

This chapter reviews studies that examined the effects of music listening on cognitive performance. It focuses on performance after listening to music. The arousal and mood hypothesis offers an explanation of the Mozart effect that has nothing to do with Mozart or with spatial abilities. Rather, it proposes that Mozart's music is simply one example of a stimulus that can change how people feel, which, in turn, influences how they perform on tests of cognitive abilities. In other words, the hypothesis offers a simple and sensible explanation of the effect when it is evident. There does not appear to be a specific link between music listening and cognitive abilities, and certainly not between listening to Mozart and spatial abilities. Hence, the direct benefits of listening to music on cognition are more of a fantasy than a reality. On the other hand, it is clear that music can change listeners' emotional states, which, in turn, may impact on their cognitive performance, and the fact that the link is mediated by arousal and mood does not make it less meaningful.

Keywords: music listening, cognitive ability, Mozart, emotional state, cognition, arousal, mood

Introduction

The goal of the present chapter is to review studies that examined effects of music listening on cognitive performance. My focus is on performance *after* listening to music, in contrast to Hallam (Chapter 32, this volume) who focuses on *background* music, or listening to music while doing something else. Despite the difference in focus our conclusions are similar. Music influences how a listener feels, and feelings influence a wide range of behaviours including *cognitive performance* (i.e. thinking, reasoning, problem solving, creativity, and mental flexibility). It is also important to clarify that the present focus is on music *listening* rather than music *lessons*, a topic reviewed by Costa-Giomi (Chapter 23, this volume). Although both types of experience involve exposure to music, music listening is ubiquitous and typically a *passive* activity, with obvious exceptions such as dancing. By contrast, only a small proportion of people take music lessons for significant durations of time, and lessons involve *active* participation for years on end in order for skilled levels of performance to emerge. As I and others have argued (Schellenberg 2001, 2003, 2005, 2006; Rauscher and Hinton 2006), there is no reason to believe that non-musical byproducts of music listening would be similar to those that might be accrued from years of intensive music training.

Research on associations between music listening and cognitive performance occurred within a social and cultural context. Indeed, social and contextual factors played a role in the design of the studies that were conducted, the way the results were reported and interpreted, and the public's and the media's response and interest in the topic. Because others (Campbell 2000; Bangerter and Heath 2004; Dowd 2008) have provided insightful commentaries on these sorts of issues, the present chapter focuses solely on the history of the relevant research rather than the social-cultural context in which this literature developed. My review also excludes studies of rodents (Rauscher et al. 1998; Aoun et al. 2005; Chikahisa et al. 2006; Amagdei et al. 2010) and studies of brain-activation patterns in humans (e.g. Sarnthein et al. 1997; Bodner et al. 2001; Jaušovec and Habe 2003; Jaušovec et al. 2006; Suda et al. 2008) simply because these areas are beyond my realm of expertise.

If music listening does indeed have benefits that generalize across a wide range of tests of cognitive performance, the ramifications for health and wellbeing would be profound. In principle, music listening could serve as a means of reducing cognitive deficits in many different groups of people, including the elderly, patients with Alzheimer's disease or dementia, and atypically developing populations such as individuals with autism, Down syndrome, or Williams syndrome. Music listening could also be used as a tool to enhance academic achievement in particular, as well as the acquisition of knowledge more generally. By contrast, if links between music listening **(p. 325)** and cognition prove to be limited to some specific aspects of cognitive functioning such as spatial abilities, the ramifications would be similarly specific. For example, music listening could be incorporated into occupations that rely heavily on spatial reasoning, such as architecture or navigation, and music-listening therapies could be tailored for groups with noted deficits in spatial abilities (e.g. Williams syndrome). In short, documenting the links between music listening and subsequent cognitive performance is important for health and wellbeing broadly construed. But what is the evidence for such links, and if they exist, might the associations be mediated by other variables that are known to influence cognitive performance?

The origin

The publication of a brief (one-page) article in *Nature* almost 20 years ago (Rauscher et al. 1993) was the impetus for widespread interest in the possibility that simply listening to music has cognitive benefits. The participants in the original study were undergraduates at the University of California—Irvine who completed one of three spatial tasks after 10 minutes of: (1) listening to a Mozart sonata (K. 448), (2) listening to relaxation instructions, or (3) sitting in silence. Each participant was tested three times in a single visit to the laboratory: once in each of the three listening conditions and once with each of the three spatial tasks. Because performance was better on the spatial tasks after listening to Mozart than in the other two conditions, this result became known as the *Mozart effect*.

The brevity of the article precluded inclusion of several important methodological details that were required to judge its merit and interpret the findings. For example, the reader needed to assume that the six possible orders of the three listening conditions were counterbalanced with the six possible orders of the three spatial tests. Because the authors had a sample of 36 undergraduates, this would mean that there was a single participant in each cell. Without this assumption, testing order would have been confounded with the different listening conditions (or spatial tests), and the results would be meaningless. Rather, performance could have improved over time due to practice effects or to increased comfort in the testing environment. Conversely, performance may have declined due to fatigue or boredom. To complicate matters further, the main statistical result was reported incorrectly in the article (i.e. the degrees of freedom do not correspond to the analysis the authors said they conducted).

The article stated clearly, however, that participants who scored high (or low) on one of the spatial tests also tended to score high (or low) on the other two tests, which provided evidence that the three tests were measuring a single construct (i.e. spatial ability or general intelligence). Indeed, the authors considered the three tasks to be identical in order to analyse differences between listening conditions with a repeated-measure analysis. Each participant had a single spatial score in each listening condition, but each score came from a different task. Thus, examination of differences among listening conditions made no sense unless the authors assumed that the three different tests were measuring the same thing.

In a second paper from the same research team (Rauscher et al. 1995), the authors replicated and extended the original effect. In contrast to the first study, which involved repeated testing in a single test session, participants were tested daily for 5 consecutive days. On the first day, they completed a test of spatial abilities. Performance on this test was used to divide the sample into three groups with equivalent abilities. On the second day, participants completed the same spatial test (with different items) after 10 minutes of listening to a Mozart sonata (K. 448), sitting in silence, or listening to a minimalist piece of music composed by Philip Glass. Performance for the Mozart group was significantly better than it was for the other two groups. Subsequent days were similar except that participants who heard the minimalist piece on the second day heard **(p. 326)** something unique each day (e.g. a narrated story or a piece of dance music). Group differences were no longer evident on these days, probably because performance on the spatial task reached a plateau after repeated testing.

On the fifth day of testing, the researchers compared performance on a test of short-term memory after participants listened to the Mozart sonata or sat in silence. The two groups did not differ. Because the Mozart effect did *not* extend to short-term memory, the null finding was used to justify claims of a special link between listening to Mozart and spatial abilities. It is also possible, however, that participants simply became bored or frustrated after 5 days of repeated testing, which masked group differences that may have been evident on the first day. Group dynamics may also have changed over time as participants became acquainted with other members of their group.

The theory

Gordon Shaw (2000, p. 163), a physicist and the second author of both reports, discussed briefly the review process at *Nature* after the authors submitted the original article. He noted that they were required to remove any mention of the theory that motivated their experiment in order to make their manuscript acceptable for publication. This comment was particularly revealing because the theory—called the *trion model* (Leng and Shaw 1991)—set the stage for subsequent research and scholarly debates about how the Mozart effect could be explained and whether it was even replicable. The trion model posited that neuronal activation patterns in the cortex are similar when listening to Mozart (or other complex music, with 'complexity' poorly defined) as when doing a task that requires spatial abilities. Rauscher et al. (1995) also speculated that Mozart's precociousness as a composer was a consequence of relatively well-developed activation patterns early in life, which allowed him to compose complex music. In psychological terms, however, the model was without empirical support (e.g. Waterhouse 2006) because it described similar brain-activation patterns arising from different activities that have nothing in common (i.e. passive listening to music, performing tasks that require spatial skills). Thus, although the basic finding that listening to music enhances cognitive performance was provocative and newsworthy, it is not surprising that *Nature* refused to publish unsubstantiated and dubious speculation about the source of the effect.

The three spatial tasks used by Rauscher et al. (1993) in the original study were subtests from *The Stanford-Binet Scale of Intelligence* (Thorndike et al. 1986), a widely used measure of intelligence. One was the *Paper-Folding-and-Cutting* (PF&C) test; the others were *Matrices* and *Pattern Analysis.* Because the PF&C test has been used widely in subsequent research, and because differences among tasks became an important issue in the literature, an example of a test item is provided in Figure 22.1. Each item on the test visually depicts a rectangular piece of paper being folded one or more times in a series of folding manipulations. One or more sections are cut out of the final folded piece. The participant's task is to determine how the paper will look when it is unfolded by choosing one of several options (usually from five alternatives).

The issue of the specific task is closely related to the authors' description of the proposed link between listening to music composed by Mozart and cognitive abilities, and the way this hypothesized link changed over time. In the original paper (Rauscher et al. 1993), the link was said to be between listening to 'complex music' and 'abstract reasoning,' with spatial abilities being just one aspect of abstract reasoning. All three tests they administered were measures of spatial abilities and abstract reasoning, and, as noted, the three tests were correlated and appeared to measure the same construct. In the second paper (Rauscher et al. 1995), the link was narrowed considerably, involving listening to Mozart and 'spatial-temporal' abilities. Even though the authors still claimed that all three tests from the original study were spatial-temporal tasks, they also noted **(p. 327)**

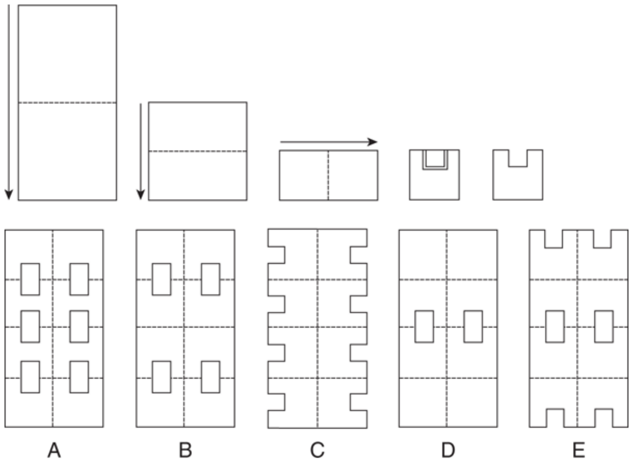

Fig. 22.1 An example of an item from the Paper-Folding-and-Cutting test. In the upper row, a rectangular piece paper is folded three times: in half going downward, in half again going downward, and in half from left to right. A section is then cut out of the folded paper, and the final folded and cut piece of paper is illustrated on the right. In the lower row, five alternatives are provided. The task is to choose the one that represents what the folded and cut piece of paper will look like when it is unfolded. The correct answer is B.

that the one of the three tasks—PF&C—'best fit our concept of spatial-temporal pattern development' (Rauscher et al. 1995, p. 45).

A few years later (Rauscher and Shaw 1998; Rauscher 1999), the same authors claimed that *only* the PF&C task was a measure of spatial-temporal ability; the other two tests from the original study were now considered to measure other (undefined) types of spatial abilities. Narrowing the nature of the association and the appropriate task helped the authors to explain why other researchers could not replicate the effect: They had used the wrong task. To support this notion, Rauscher and Shaw (1998) re-analysed the data from the original study and claimed that the Mozart effect was evident only for the PF&C task. As before, when they compared performance across listening conditions, they found a Mozart effect. When they compared performance across tasks, they found a task effect (i.e. better performance on PF&C than on the other two tasks). Both of these results may have been consistent with the actual data, but once again, the statistical analyses were reported incorrectly (see also Fudin and Lembessis 2004), which does not instill much confidence in the reader. Specifically, the authors reported results from an analysis (i.e. a 3 × 3 repeated-measures ANOVA) that was impossible to conduct with their data set, and the degrees of freedom they reported did not correspond to any legitimate analysis that could have been conducted.

Moreover, the claim of a Mozart effect for one task but not for others was a claim of an interaction between listening condition and task. A test of the interaction could not have been conducted with the original data because of the experimental design. Rather, such a test would require: (1) all participants to be tested nine times (three tasks for each of the three conditions; both variables manipulated within-participants); (2) each participant to be tested three times in the same **(p. 328)** listening condition (a between-groups variable), once with each task (a within-participants variable); or (3) each participant to be tested with the same task (a between-groups variable) in each of the three conditions (a within-participants variable). Even then, testing order would need to be counter-balanced so that response patterns could be attributed unequivocally to the different listening conditions, the different tasks, and/or an interaction between listening condition and task. Another possibility would be to test each participant a single time in one of nine different groups, with both listening condition and task manipulated as between-group variables. Because of substantial pre-existing individual differences in cognitive abilities, this last option would likely require a very large sample size in order for significant effects to emerge.

For the scientific community at large, the unusual details of the Mozart effect as described by these articles (Rauscher et al. 1993, 1995; Rauscher and Shaw 1998) presented a paradox. On the one hand, the initial report was published in a prestigious, high-impact journal, it received widespread media coverage and public interest, and the main result was provocative yet intuitively appealing to many scientists and lay people. On the other hand, the original report's brevity left many important questions unanswered, the experimental designs and statistical analyses were less than optimal, and the underlying theory was unsubstantiated and shifted from paper to paper. Moreover, the third paper in the series (Rauscher and Shaw 1998) as well as the original description of the trion model (Leng and Shaw 1991) were published in journals with very poor reputations (*Perceptual and Motor Skills*, *Concepts in Neuroscience*, respectively).[1] This particular combination of factors led to a response from the scholarly community that varied from initial disinterest, to dismay and confusion, and, eventually, to hostility and personal animosity between some researchers in the field (e.g. Rauscher

1999; Steele 2000, 2001, 2003, 2006). More generally, the psychological dubiousness of the underlying theory led many to believe that: (1) the effect must be a consequence of psychological mechanisms less mystical than those posited by the trion model, or (2) the effect simply did not exist.

To highlight this paradox further, I calculated the number of papers that have been published on the topic, the year of publication, and the quality of the average publication. First, to document the history of research and media interest in the effect, I conducted an Internet search for published articles using the keyword ‘Mozart effect’. This search uncovered journal articles, magazine articles, unpublished dissertations, editorials, and so on. The results, shown in Figure 22.2, illustrate that interest has been relatively high and consistent for several years (see also Bangerter and Heath 2004), with the number of published articles reaching a notable peak in 1999. In 2009, however, only two articles were published, which suggests that interest may finally have waned. Accordingly, now is likely to be as good a time as any to review what was discovered about music listening and cognitive performance between 1993 and the present day.

A second analysis examined articles from the previous sample that appeared in journals with documented impact factors, which served as an approximate measure of differences in journal quality. Whenever available, I recorded the impact factor averaged over 5 years (2004–2008) of each journal that published one or more articles about the Mozart effect. Figure 22.3 illustrates the association between impact factor and the number of articles published. The association is unambiguously negative. On the one hand, then, the first analysis demonstrated long-term, **(p. 329)**

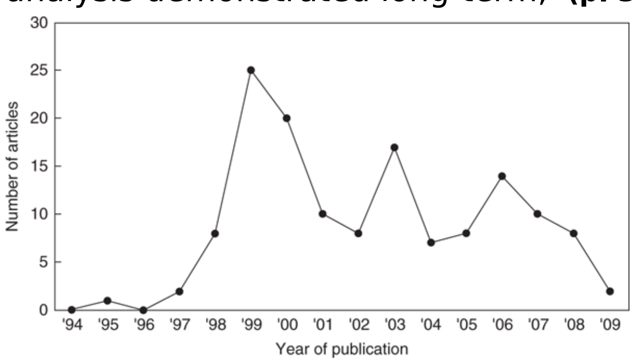

Fig. 22.2 The results from a literature search on the keyword ‘Mozart effect’. The search was conducted in February 2010. The figure illustrates the number of articles published in each year from 1994–2009.

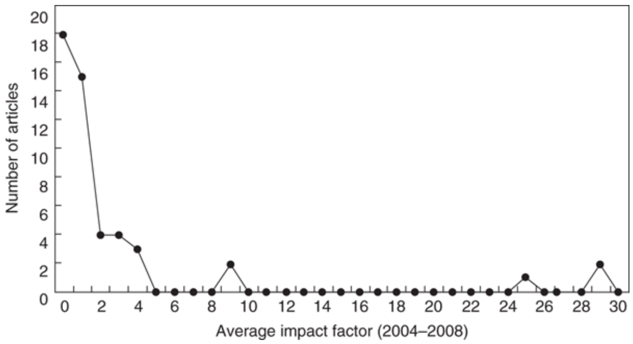

Fig. 22.3 The results from a subset of articles from Figure 22.1 (i.e. those that appeared in journals with available impact factors). The figure illustrates the number of articles published as a function of impact factor (0–1, 1–2, 2–3, and so on).

relatively high and consistent scholarly and media interest in the effect because it was so topical. On the other hand, the second analysis indicated that, with some notable exceptions, relatively few articles were published in good- or medium-quality journals (i.e. with impact factors higher than 1). Indeed, the vast bulk of the research appeared in low-quality scientific journals, presumably because it was conducted without sound theoretical motivation and/or the findings were not particularly edifying.

Arousal and mood

An alternative explanation of the Mozart effect (Thompson et al. 2001; see also Steele et al. 2000) proposes that it is mediated by the listener's emotional state, specifically arousal levels and moods, **(p. 330)** which can be modified by music listening. For example, when undergraduates are tested on a computer game while listening to music, they perform better when listening to the music they prefer (Mozart K. 448 or the Red Hot Chili Peppers; Cassity et al. 2007). Even low-level measures of attention reveal better performance while listening to a pleasant stimulus (Mozart K. 448) compared to an unfamiliar-sounding stimulus (the Mozart sonata played backward) or silence, an effect that appears to be a consequence of differences in arousal and mood (Ho et al. 2007).

In other words, music can change how listeners feel (Jones et al. 2006), and their feelings, in turn, influence their cognitive performance (Jones and Estell 2007; Jones et al. 2006). In stark contrast to the trion model, both parts of the hypothesized association are well documented in the literature. For example, in an extensive review article, Juslin and Västfjäll

(2008) concluded that ‘people use music to change emotions, to release emotions, to match their current emotion, to enjoy or comfort themselves, and to relieve stress’ (p. 559; see also Sloboda 1992; Laukka 2007). In other words, there is no doubt about the link between music listening and its ability to influence listeners’ emotional state.

The link between emotional state and cognitive performance is similarly unequivocal. For example, in another review article, Isen and Labroo (2003) concluded that ‘positive affect promotes cognitive flexibility and thus facilitates problem solving and decision making in many situations’ (p. 365). Conversely, negative emotional states such as anxiety (Cassady 2004) and boredom (O’Hanlon, 1981) often have detrimental influences on cognitive performance. Positive and negative moods, respectively, appear to facilitate and inhibit the formation of associations between stimuli and pre-existing mental representations (Storbeck and Clore 2005), whereas negative moods increase the amount of subjective effort required for a task (Gendolla et al. 2001). Positive and negative moods are also associated with reliable differences in physiological responses such as blood pressure (Gendolla and Krüsken 2001; Gendolla et al. 2001).

In a series of studies that I conducted with my colleagues (Nantais and Schellenberg 1999; Thompson et al. 2001; Husain et al. 2002; Schellenberg and Hallam 2005; Schellenberg et al. 2007), our goals were to replicate the Mozart effect and to determine the boundary conditions under which it would be evident. As we will see, all of the evidence proved to be consistent with the arousal and mood hypothesis. The initial study (Nantais and Schellenberg 1999) involved two experiments. In the first one, undergraduates came to the laboratory individually on two different days. On both visits, they completed one of two versions of the PF&C task that were equally difficult. The PF&C test was preceded by 10 minutes of listening to music or sitting in silence. Both experiences involved wearing headphones while sitting in a sound-attenuating booth facing a computer monitor. The order of the two listening conditions and the order of the two versions of the PF&C task were counterbalanced. Performance was better after listening to music than after sitting in silence, regardless of whether the music was composed by Mozart or Schubert. In other words, we replicated the Mozart effect but we also found a *Schubert effect* that was equivalent in magnitude.

In the second experiment, we contrasted PF&C performance after listening to Mozart or a narrated story written by Stephen King. Listening to a story represented a better control condition than sitting in silence because, like the music, the story was an auditory stimulus that changed over time, and it was presented at the same amplitude as the music. Moreover, the story was deemed to be approximately as interesting and pleasant for undergraduates as listening to Mozart would be. As expected, we found no difference in PF&C performance between the two listening conditions. When we asked these participants which listening experience they preferred, approximately half preferred Mozart whereas the other half preferred the story. We then analysed the data as a function of preference and listening condition. A significant interaction between preference and condition revealed that performance was higher among the preferred conditions **(p. 331)** (i.e. the Mozart condition for those who preferred Mozart, the story condition for those who preferred the story) compared to the non-preferred conditions. In other words, participants who preferred Mozart showed a Mozart effect whereas listeners who preferred the story showed a *Stephen King effect*. Although we had no direct measures of arousal or mood, it seems fairly safe to assume that arousal levels and moods would be better after the preferred compared to the non-preferred listening experience.

The second study (Thompson et al. 2001) had the same basic design but it tested specifically the arousal and mood hypothesis. Each participant was tested individually on the PF&C task on two different days, once after listening to music and once after sitting in silence. The music was Mozart (K. 448) for half of the participants and Albinoni's Adagio for the other half, with order of the two PF&C tasks and the two listening conditions counterbalanced. We also measured arousal and mood before and after the listening experience. In contrast to the Mozart sonata, which was relatively fast and in a major key, the Albinoni Adagio was slow and minor. It is well established that fast-tempo and major-mode music tends to make listeners feel happy, whereas slow-tempo and minor-mode music makes listeners feel sad (e.g. Hunter and Schellenberg 2010). Our prediction was that we would once again find evidence of a Mozart effect but *not* of an Albinoni effect, and that this difference between groups would be a consequence of differential changes in arousal and mood. The data were consistent with this prediction. For the Mozart group, arousal levels and mood improved after listening to music but not after sitting in silence. For the Albinoni group, there were no differences between the music and silence conditions. Moreover, compared to sitting in silence, performance on the PF&C task was enhanced after listening to Mozart but not to Albinoni. Finally, the observed Mozart effect disappeared when changes in either arousal or mood were held constant in the statistical analyses.

The null finding for the Albinoni piece could be a consequence of specific associations between the piece and personal experiences, such as a recent death in the family (the Albinoni piece is played commonly at funerals) or previously learned associations between this particular piece and sadness. To address this concern, in the next experiment (Husain et al. 2002), the stimuli consisted of different versions of the *same* music (Mozart K. 448) recorded as a MIDI file. The file was manipulated to create four versions that varied only in tempo and mode (fast–major, fast–minor, slow–major, slow–minor). As noted, these two musical characteristics have clear associations with emotion. Each participant was tested individually on the PF&C task only once (a between-subjects design) after listening to one of the four versions, and arousal and mood were measured before and after the test session. PF&C performance was better after listening to the fast compared to the slow-tempo versions of the sonata, and after the major compared to the minor versions. Tempo influenced arousal levels (faster = higher arousal), whereas mode influenced mood (major = more positive). Again, individual differences in arousal and mood accounted for the bulk of the variance in performance on the PF&C task.

In the next study (Schellenberg et al. 2007), we tested non-spatial abilities. Participants were tested twice on different days after listening to Mozart (K. 448) or to Albinoni's Adagio. After both listening experiences they completed one of two tests: a measure of processing speed or a test of working memory. Order of the two tests and the two musical pieces was counterbalanced. We also measured arousal and mood before and after the test session. At the first test session, we found that mood improved after listening to Mozart but not to Albinoni, whereas arousal levels *decreased* for both groups. Because the arousal and mood measures were inconsistent, we were not surprised to discover that there were no group differences on either cognitive test. At the second session, however, arousal levels *and* mood both improved after listening to Mozart but became worse after listening to Albinoni. In line with the arousal and mood hypothesis, performance on one of the cognitive tests (processing speed) was enhanced after listening to Mozart **(p. 332)** compared to Albinoni. In other words, we replicated the Mozart effect with a non-spatial task. There was no difference on the test of working memory, however, which implies that tests of some cognitive abilities may be relatively impervious to effects of arousal and mood. Some previous evidence also suggests that performance on tests of working memory may be relatively immune to the Mozart effect (Rauscher et al. 1995; Steele et al. 1997).

If the arousal and mood perspective on the Mozart effect is correct, then the most appropriate music to influence how listeners feel, and how they perform subsequently on a cognitive task, should depend critically on the particular sample of listeners. In line with this view, rock musicians show specific brain activation patterns (i.e. lower P3 amplitudes) when listening to rock music, whereas classical musicians show the same patterns when listening to classical music (Caldwell and Riby 2007). The next experiment (Schellenberg and Hallam 2005) tested directly whether the specific music that leads to better cognitive performance depends on the particular sample. It was conducted in collaboration with the BBC, which issued a call to schools in the UK to participate in a study on the Mozart effect. Approximately 200 schools responded and over 8000 10- and 11-year-olds participated. Each school divided students into three groups at random. Each group was assigned to a different room at the school. At precisely the same time, students listened to music by Mozart (not K. 448) on BBC Radio 3 in one room, popular songs (including 'Country House' by the UK band Blur) on BBC Radio 1 in another room, or to a discussion about the experiment on BBC Radio 5 in a third room. Afterwards, they completed two tests of spatial abilities. We expected that emotional states would be enhanced for these children after listening to pop music compared to music by Mozart or a scientific discussion, and, consequently, that cognitive performance would show a similar pattern. Spatial-task performance was consistent with this prediction for one of the two tasks, the one that proved to be more difficult. In short, the results revealed a *Blur effect* for school-age children. They also suggested that effects of emotional state on cognitive performance might be more likely as the difficulty of the task increases.

The final experiment in this series tested the creative abilities of Japanese 5-year-olds (Schellenberg et al. 2007). Each child was initially given a piece of paper and 18 crayons and asked to draw whatever they wanted (i.e. a *baseline* drawing). They made a second *music* drawing on a different day after one of four musical experiences to which they were assigned randomly. The experiences involved hearing Mozart (K. 448), Albinoni's Adagio, or Japanese children's songs for one hour during lunch, or singing children's songs for 20 minutes after lunch. The dependent measures involved comparisons between baseline and music drawings (one pair for each child) in terms of: (1) time spent drawing, and (2) adults' ratings of the drawings' creativity, energy, and technical proficiency. (The adults did not know which drawing was the baseline or music drawing.) Our prediction was that all outcome measures would differ between the two classical music conditions (Mozart and Albinoni) and the two children's music conditions (listening or singing), with the results favouring creativity in the latter case. In line with these predictions, drawing times increased more from the baseline to the music condition for children who heard or sang familiar children's songs, and their music drawings were judged to be more creative, energetic, and technically proficient. In short, we found a *children's playsong effect* for creativity among 5-year-olds.

Successful and unsuccessful attempts to replicate

Many researchers have attempted simply to replicate the Mozart effect. When the dependent variable was a measure of spatial abilities, some studies succeeded (e.g. Rideout and Laubach 1996; Rideout and Taylor 1997; Wilson and Brown 1997; Rideout et al. 1998; Twomey and Eastgate 2002; Ivanov and Geake 2003; Cacciafesta et al. 2010) but others did not (e.g. Carstens et al. 1995; Steele et al. 1999a,b,c; McCutcheon 2000; McKelvie and Low 2002; Jackson and Tlauka **(p. 333)** 2004; Črnčec et al. 2006b; Hui, 2006) Similarly, when the dependent variable was a measure of performance on a test of some other kind of cognitive ability, some studies reported an effect (e.g. Roth and Smith 2008) but others did not (e.g. Stough et al. 1994; Newman et al. 1995; Steele et al. 1997; Bridgett and Cuevas 2000; Twomey and Eastgate 2002; Gray and Della Sala 2007). In some instances, the effect was evident for some participants (non-musicians: Aheadi et al. 2010; women: Gilleta et al. 2003) but not for others (musicians or men). In addition to the *Schubert*, *Stephen King*, *Blur*, and *children's playsong effects* described above, others have provided evidence of *Bach* (Ivanov and Geake 2003), and *Yanni* (Rideout et al. 1998) *effects*.

A meta-analysis of the Mozart effect from 1999 concluded that the effect was weak or non-existent (Chabris 1999), whereas a second meta-analysis published the very next year concluded that the effect was robust but limited to spatial tasks similar to PF&C (Hetland 2000). One review article concluded that there was 'no strong evidence of a Mozart effect in children' (Črnčec et al, 2006a, p. 581), which is not surprising because children are unlikely to find Mozart's music particularly pleasant. A more recent meta-analysis (Pietschnig et al. 2010) found a small but significant effect on spatial abilities when they examined studies that compared listening to Mozart with a non-musical stimulus (usually silence). When they examined studies that used music other than Mozart, the effect size was similar in magnitude. As in the present chapter, the authors attributed the observed effects to differences in arousal.

In any event, simple attempts to replicate the Mozart effect are not informative whether they succeed or not. If the replication is successful without informing us about the underlying mechanisms that are the source of the effect, then we have not learned much. Failures to replicate are even less instructive. First of all, the null hypothesis can never be proven. Secondly, when researchers from independent laboratories have replicated the effect, what does a failure to do so indicate? That all previous findings of a Mozart effect were due to experimenter bias or Type I error? Both of these interpretations are implausible. My own view is that failures to replicate indicate that the researchers did not try hard enough, typically because they relied on the convenience of group testing (e.g. Carstens et al. 1995; Steele et al. 1999a,b,c; McCutcheon 2000; McKelvie and Low 2002; Lints and Gadbois 2003; Črnčec et al. 2006b; Hui 2006). In normal circumstances, listeners often talk to one another when pre-recorded music is heard in group situations (e.g. in bars, at parties), yet these studies required participants *not* to talk, thereby creating an artificial listening context with little or no ecological validity. Requiring a group of people to sit in silence is even more artificial. One can only imagine the smiles, smirks, and rolling eyes as groups of participants were required to sit together in silence doing absolutely nothing. Failures to replicate also have no ramification for the arousal and mood hypothesis, which was formulated to explain *successful* replications of links between exposure to music and cognitive performance. The hypothesis does *not* predict that exposure to music (composed by Mozart or anyone else) at any time in any context will lead to cognitive improvements, or that whenever differences in arousal or mood are observed, differences in cognitive performance will also be evident.

Conclusion

The arousal and mood hypothesis offers an explanation of the Mozart effect that has nothing to do with Mozart or with spatial abilities. Rather, it proposes that Mozart's music is simply one example of a stimulus that can change how people feel, which, in turn, influences how they perform on tests of cognitive abilities. In other words, the hypothesis offers a simple and sensible explanation of the effect when it is evident. As described in the above review, many factors appear to influence whether effects of music listening on cognition will be observed in an experimental setting, including the match between the sample of listeners and the music, whether listeners are tested in groups or individually, the particular test, and the difficulty of the test. Participants' **(p. 334)** performance also depends on whether they are aware of the experimental hypothesis (Lints and Gadbois 2003; Verpaelst and Standing 2007; Standing et al. 2008).

The arousal and mood hypothesis was designed to be very general, extending beyond Mozart and music to any stimulus that affects how the perceiver feels, and beyond spatial abilities to cognitive performance broadly construed. In retrospect, however, the hypothesis was not general enough. Applied research reveals that listening to music has positive effects that extend well beyond cognition. For example, self-selected music can reduce pain perception (Mitchell et al. 2006), particularly when the music is very familiar (Mitchell et al. 2008). In these instances, music's positive effect on emotional state appears to act jointly with its ability to distract the listener's attention away from a negative stimulus. Music (typically lullabies) also promotes weight gain for pre-term infants and decreases length-of-stay in hospital (Standley 2002), perhaps by slowing energy expenditure (i.e. metabolic rate; Lubetsky et al. 2010). After suffering from a stroke, daily listening to music improves mood in addition to facilitating recovery of memory and attention (Särkämö et al. 2008), whereas patient-selected music can reduce the amount of anaesthesia required during surgery (Ayoub et al. 2005). Finally, music listening in older age improves quality of life by helping the elderly cope with pain and confusion (McCaffrey 2008).

In sum, conclusions that can be drawn about the impact of music listening on health and wellbeing are both disappointing as well as promising. On the one hand, there does not appear to be a specific link between music listening and cognitive abilities, and certainly not between listening to Mozart and spatial abilities. Hence, direct benefits of music listening on cognition are more of a fantasy than a reality. On the other hand, it is clear that music can change listeners' emotional state, which, in turn, may impact their cognitive performance, and the fact that the link is mediated by arousal and mood does not make it less meaningful. Moreover, unlike other stimuli that influence how we feel (e.g. candy, cigarettes, drugs, exercise), portable devices with hundreds of digital recordings can accompany listeners almost anywhere, and if the music is played at a reasonable volume, there are no adverse effects. In this sense, music is special because it is an easily transportable but non-toxic stimulus that influences how we feel, and because how we feel affects virtually all aspects of human experience.

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Notes:

([1]) Impact factor is an index of how many times, on average, articles in a journal are cited in a particular year. In 2008, *Perceptual and Motor Skills* and its sister journal, *Psychological Reports* (same publisher, website, policies, and so on) had impact factors of 0.402 and 0.309, respectively. For the same year, *Nature* had an impact factor of 31.434, *Psychological Science* had an impact factor of 4.812, and *Neuroscience Letters* had an impact factor of 2.200. As far as I can tell, *Concepts in Neuroscience* does not have a documented impact factor, it published only five volumes, it has no website, and Gordon Shaw was co-editor.