11

GAGNE IS OMITTING TROUBLESOME INFORMATION SO AS TO PRESENT MORE CONVINCING ACCUSATIONS

HIS ACCUSATIONS ALONG WITH MY OWN EXPLORATION OF THE EVIDENCE FOR INNATE TALENT

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Abstract

The accusations in Gagné's (this volume) chapter are addressed and discussed. More importantly, this chapter describes the detailed differences between our two theoretical frameworks. Our theoretical framework is based on deliberate practice and focus on evidence that practice can dramatically influence the development of children, adolescents, and adults when the individuals engage in the designed practice for hours on a daily basis for months, years, and decades. In contrast, Gagné emphasizes the role of innate genetic differences and informal exercise. Once we accept the overwhelming evidence for genetic control of individual differences in height and body size, our empirical review has not uncovered rigorous scientific evidence that meets our standards and demonstrates conclusively that innate talent (gifts) are necessary for attaining expert performance. Based on the same standards, Gagné's anecdotal evidence from laypersons is rejected. The chapter concludes with an outline for how giftedness can be studied with the same rigorous scientific methods used for studying expert performance.

Keywords: *deliberate practice, expert, expertise, prodigy, common sense, exceptional ability, skill acquisition, anecdotes, music, ice hockey*

I have always been an advocate for scientific discussion. I believe in seeking out scientists who disagree with me to learn about evidence that I was not aware of. I have had published exchanges with Thomas Bouchard (Ericsson, Krampe, & Heizmann, 1993), Howard Gardner (Ericsson & Charness, 1994, 1995; Gardner, 1995), and Robert Sternberg (Ericsson, 1996; Sternberg, 1996) about research findings. More recently, I accepted two opportunities for such a public discussion between researchers. I was invited to write one of two target papers (Ericsson, 2007a) for a special issue of the *International Journal of Sport Psychology* on "Nature versus Nurture in Sport." The contents of the target papers were commented on by eight papers by the most recognized scientists on this issue, and I had the opportunity to write a reply (Ericsson, 2007b) and comment on the other target paper, written by Klissouras, Geladas, and Koskolou (2007) with the title "Nature Prevails over Nurture." I also accepted an invitation to write a target paper for a special issue of *High Ability Studies* on deliberate practice with Roy Roring and Katy Nandagopal, which was reprinted in this volume. This paper was commented on in a series of published articles by numerous famous researchers on giftedness and talent (including a commentary by Gagné). We were also given the opportunity to write a reply to all the commentators, which was included in the special issue (Ericsson, Roring, & Nandagopal, 2007b). In his contribution to the *International Handbook of Giftedness*, Gagné (2009a) wrote a chapter where he accused me and Michael Howe of misconduct. In the chapter for the same handbook (Ericsson et al., 2009), we were asked to respond to some of his charges and did so in less than five pages. Our attempt to give short answers was only partly successful because many of the original charges were seemingly accepted by Gagné (2009b) in his rejoinder. However, in his rejoinder, Gagné (2009b), particularly in his chapter for the current volume (Gagne, this volume), is increasing the severity of the charges of scholarly misconduct. Gagné (2009a, 2009b, this volume) is the first scientist to accuse me of scholarly misconduct, and I have been publishing papers in psychology for over 40 years without any prior problems.

I am grateful to the editor, Scott Barry Kaufman, for providing this opportunity to describe our ideas underlying our theoretical framework to show that our work has provided a comprehensive attempt to understand the development of superior performance in domains of expertise. I have also made an effort to interpret Gagné's (2009a, 2009b, this volume) conceptions so we can successfully resolve the miscommunication of our ideas. Before I do that, I would like to comment on Gagné's accusations at a general level. The charge of intentionally omitting "troublesome" information in order to present a more convincing argument was based primarily on our article (Ericsson et al., 2007a) and our short rebuttal of Gagné in the *International Handbook of Giftedness* (Ericsson et al., 2009). In the target article (Ericsson et al., 2007a), we cited a review by MacArthur and North (2005) concluding that "individual differences in attained elite performance in sports *cannot*, at least currently, be explained by differential genetic endowment" (p. XXX). To Gagné (this volume, p. XXX), our focus on this current assessment showed that we "totally ignored that review's incontrovertible evidence on the hereditary bases of natural physical abilities." What Gagné does not mention is that the research on twins' physical abilities discussed in MacArthur and North's (2005) paper had been previously reviewed by me on six pages in a chapter (Ericsson, 1990), on three pages as part of our original paper in *Psychological Review* (Ericsson, Krampe, & Tesch-Römer, 1993, pp. 394–396), in six pages in a reply (Ericsson, 2003b, pp. 393–398) to comments on my target chapter (Ericsson, 2003a), on six pages in the target article for the special issue on the *International Journal of Sport Psychology* (Ericsson, 2007a, pp. 20–25), and on four pages in my response to the commentaries (Ericsson,

AQ: Update later.

2007b, pp. 111–114). In spite of the fact that four of these articles were explicitly cited in two of the papers (Ericsson et al., 2007b, 2009), Gagné claims that this research evidence was "totally ignored." It would be reasonable to believe that Gagné did not intentionally ignore this printed discussion, and that he never read any articles beyond the two that he cited in his chapter. The explanation for his omission is given by Gagné in his postscript comments on Ericsson et al. (2009), where he writes, "I confess that I have not read the first author's [Ericsson's] papers cited as their rebuttal. But it is easy to imagine what these papers contain: not a single study in support of physical natural abilities, and everything he could find as apparent support for the Anitnat position" (Gagné, 2009b, p. 199). I will defer to the reader to decide if Gagné is justified to accuse me of intentionally ignoring findings from studies that I have discussed in depth in the cited papers. It is Gagné who intentionally ignored reading my discussion of these findings and intentionally omitted evidence to make a more convincing accusation. Later in this chapter I will return to a more detailed discussion of this evidence on heritability of physical abilities.

Anyone who has read Gagné's chapter is likely to be struck by his proposal to divide the research community studying talent development in two dichotomous groups, where one group consists of the good guys (Gagné), and the other group, which includes Michael Howe and me, is engaging in scholarly misconduct to mislead researchers. This development is particularly disappointing to me because after the publication of the *Cambridge Handbook of Expertise and Expert Performance* (Ericsson, Charness, Feltovich, & Hoffman, 2006), there has been an explosion of interest among academic researchers, expert performers and their coaches, and the general public in how they can learn from the study of expert performers. In fact, in the last three years there have been a number of books published that have tried to describe the evidence and implications of our findings on expert performance and its structure and acquisition, such as *Talent Is Overrated: What Really Separates World-Class Performers from Everybody Else* by Colvin (2008), *Outliers: The Story of Success* by Gladwell (2008), *The Talent Code: Greatness Isn't Born. It's Grown. Here's How* by Coyle (2009), *Bounce: Mozart, Federer, Picasso, Beckham, and the Science of Success* by Syed (2010), *The Genius in All of Us: New Insights into Genetics, Talent, and IQ* by Shenk (2010), *Moonwalking with Einstein: The Art and Science of Remembering Everything* by Foer (2011), *Guitar Zero: The New Musician and the Science of Learning* by Marcus (2012), and *Quiet: The Power of Introverts in a World That Can't Stop Talking* by Cain (2012). Most of these books reached the *New York Times* bestseller list, and a few reached the top of some lists. I see a new excitement among teachers and coaches about working with researchers studying expert performance and the belief that collaborative research will increase scientific knowledge and improve training and practice methods for aspiring expert performers. It was hard for me to find efforts, similar to those proposed by Gagné, to divide the community of researchers, teachers, and performers into supporters (pro) and adversaries (anti) on this issue. In the recent past and today, we have such divides on issues such as the role of government, abortion, and the death penalty. A cursory search on Google suggested that one would have to go far back in history to find similar divides in science, such as the controversy over evolution and vitalism. In both of these historical cases, researchers were attacked for searching for explanations for phenomena that did not require the creations of God. In the current chapter, Gagné (this volume) is accusing my colleagues and me of rejecting the existence of innate talent. He argues that the evidence for natural abilities is evidenced by experiences by laypeople. Gagné (this volume) describes our work on alternative accounts of the "emergence" of high levels of ability as "systematic factual misrepresentations" (Gagné, this volume, p. XXX) along with a "refusal to acknowledge testimonial information" (p. XXX).

Some of the other issues raised by Gagné are more complex and are due to the fundamental differences in our respective research experience and our theoretical frameworks. If there is something positive that could emerge from this exchange over Gagné's chapter, it is that it has forced me to try to explain my own view in a more general way and to summarize a body of informal evidence, such as anecdotes and interviews, which would not be accepted for publication by the journals in which I typically publish.

Anyone who has read my colleagues and my chapter (Ericsson et al., 2007, this volume) and Gagné's (this volume) chapter may wonder if and how these two chapters can be reconciled and where the specific loci of the scientific disagreements about empirical evidence are. As in many controversies, it seems that Gagné has his theoretical framework that he proposed some time ago, and he is concerned about questions raised regarding the emphasis on natural abilities in his model of talent development. In his chapter, Gagné (this volume, p. XXX) defines natural abilities as "mental or physical abilities (a) whose development is significantly influenced by our genetic endowment, and (b) that act as causal agents in the growth of competencies (knowledge and skills) characteristic of a particular field of human activity." In the end of his chapter he states that he and his colleagues have "have *never* maintained that natural abilities, even with their significant genetic roots, appear suddenly or that they are immutable" (Gagné, this volume, p. XXX). Later in this chapter, I will discuss the anecdotes Gagné presents in support of the evidence for innate talent and natural ability, which are described to illustrate instantaneous and rapid increases in performance—perhaps sudden in the sense of "made or brought about in short time" (Merriam-Webster Word Central, "sudden," 2012). The issues are not concerned with natural abilities, but with the genetic portions that are determined by genetic endowment (innate talent), which cannot be changed by training. Gagné (1991) describes this in a manner that I can agree with: "aptitudes can and do develop, not only through maturation but also as a result of systematic training. However, this malleability remains *within limits fixed by heredity*" (p. 70, italics added). It is the identification of those limits and their role in constraining some individuals from attaining expert performance, that is the central issue.

OUTLINE OF CHAPTER

In my view, the biggest difference between Gagné's and my own theoretical frameworks is that my collaborators and I have tried to develop detailed models of how performance can be changed in response to various forms of extended training and practice. Whereas Gagné (this volume, p. XXX) argues that "[N]atural abilities are not innate; they do develop, especially during childhood, through maturational processes and informal exercise," our work has focused on evidence that practice can dramatically influence the development of children, adolescents, and adults when it is designed and engaged in for several hours on a daily basis for months, years, and decades. In fact, our own work suggests that experience and informal exercise have very limited effects on the development of superior performance (Ericsson, 2006a).

The second and somewhat related difference between us concerns our view of science and the nature of the scientific evidence that can be accepted as valid support for scientific claims. According to Gagné (2009a, p. 155), Howe and his colleagues and my colleagues and I have set out to "disprove the existence of natural abilities (whose giftedness level they usually call *innate talent*)." To disprove the existence of innate talent (gifts) or natural ability is impossible, and no amount of research could prove that conclusive evidence for its existence will never be found. What we, and Michael Howe and his colleagues, did was to examine the evidence that we could find and conclude that the current evidence did not meet our standards for empirical data demonstrating that innate talent (gifts) are necessary for attaining expert performance. To a scientist, this is a very important distinction, which Gagné does not seem to be willing to make or at least accept. The central question for me and my colleagues is whether there is currently *scientific* evidence showing that innate talents (gifts) are necessary for attaining high and expert levels of performance. Gagné (this volume) does not see any problem with the following anecdotal evidence and even points to the general support among laypeople for aptitude and gifts:

> There is no lack of sayings to translate the idea that some individuals possess in one or more domains higher aptitudes than their peers, sometimes much higher ones. When we say of a young person that she is a "born" musician (or "born" athlete, or "born" leader), we are implicitly alluding to the non-learned, biological, or genetic foundations of human abilities. Many more common expressions convey a similar meaning, for instance, "you either have it or you don't," "he's a natural," or "it's a gift from God." (Gagné, this volume, p. XXX)

He then goes on to give a description of the mother of the famous ice-hockey player Mario Lemieux and her memory many years later for her son's *first opportunity to skate*—an account that I will examine in more detail later. He also reports on the amazing performance by Sarah Chang at age 5 or 6 as described by Dorothy DeLay, the famous violin teacher at Juilliard. These two anecdotal accounts by Gagné seem to suggest that they constitute evidence for sudden (Mario Lemieux) and very rapid development of skill—in direct contrast to his claim (Gagné, this volume, p. XXX) that he and his colleagues never argued that natural abilities "appear suddenly."

In science, there has been a long-standing mistrust of lay observations, opinions, and sayings. With respect to the scientific issue of evolution versus creation, only 16% of the American population favored the scientific explanation in a Gallup poll in 2010

(Newport, 2010). In addition, 40% of Americans endorsed the view that "God created humans in their present form about 10,000 years ago" (Newport, 2010). Similarly, children acquire beliefs about living things based on life forces (Inagaki & Hatano, 2004). According to a recent Gallup poll (Moore, 2005), a majority of Americans (71%) believe in at least one form of paranormal phenomena, such as ghosts, haunted houses, witches, astrology, and communication with the dead. No serious scientist would rely on those lay opinions and associated personal experiences as valid evidence for these phenomena.

In the first part of this commentary, I will describe research by my colleagues and me on how expert performance is attained by extensive training in a life-span perspective and whether there is empirical evidence for innate genetic factors that set limits for particular individuals' performance in domains of expertise that cannot be overcome by training. Within this theoretical perspective, there is no theoretical role for natural ability, except to the extent that it may partly reflect genetic differences, which, in turn, reflect aspects that cannot be modified—i.e., it is not generally believed that the genes in the DNA can be changed. It is not even clear to me how one would be able to measure natural abilities of children when training and practice have been initiated in a related domain of expertise. I will also show how the theoretical assumptions of the expert-performance approach (Ericsson, 2006a, 2006b; Ericsson & Smith, 1991; Ericsson & Ward, 2007) led me to not focus on the two studies that Gagné alleged had been intentionally omitted because they would provide "troublesome observations" for my argument. Once my perspective is outlined, I will discuss in the second part the anecdotal evidence offered by Gagné and why it does not convince me.

PART I: EFFECTS OF SUSTAINED DELIBERATE PRACTICE ON THE ACQUISITION OF SUPERIOR PERFORMANCE ACROSS THE LIFE SPAN

At the center of the controversy over innate talent (the genetic portion of natural ability) is the issue about the modifiability of human characteristics resulting from practice during childhood, adolescence, and adulthood. My reading of Gagné's chapter suggests that the two of us have different conceptions of the type, intensity, and duration of practice. He refers to informal exercise as a stimulus for natural ability, and he treats the effects of 1 to 40 hours of practice as conclusive evidence for effects of any amount and type of practice—at least, he cites studies that examined effects of 38 hours (Baltes & Kliegl, 1992) and 1.5 hours of practice (Fox, Hershberger, & Bouchard, 1996) of initially untrained adults in the laboratory as crucial counterevidence to our theoretical framework. In direct contrast to Gagné's conception of practice, our research on expert performance in music, ballet, chess, and sports has focused on the effects of years and decades of sustained daily training that correspond to more than 10,000 hours of deliberate efforts to improve one's performance in a particular domain of expertise. I am inviting the reader to decide if findings of trainability and modifiability from an experiment with adults performing an unfamiliar task that lasts an hour or even 40 hours permits firm inferences about the magnitude and structure of effects of over 10,000 hours of daily training, where the daily training started from a very young age, such as 2–4 years old, and continued uninterrupted until adulthood. For example, who would rely on results from experiments studying training in a language for 2–40 hours to draw valid inferences for mastering a foreign language, which appear to require at least a couple of years of full-time study to attain full mastery? Even more directly to the point, Bill Chase and I (Chase & Ericsson, 1981, 1982; Ericsson, Chase, & Faloon, 1980) examined the effect of training for several hundred hours on a memory task involving immediate recall of a list of digits (digit span). After the first three hours or sessions (c.f. the amount of training in Fox et al.'s 1996 study), we found an increase in memory performance from around 7.5 digit to 8.5 digits (roughly a 15% increase) (Chase & Ericsson, 1981; Ericsson et al., 1980). After 40 hours of training (the amount of training in Baltes and Kliegl's 1992 study), we observed a memory performance of around 15 digits (roughly a 100% increase over starting performance). After over 200 hours of practice, we recorded a memory performance of 82 digits (roughly a 1000% increase over starting performance).

One could reasonably argue that every adult has developed a natural ability for recalling a sequence of digits with all their experience with remembering a sequence of digits while engaging in activities such as dialing a phone number or copying one's bank account number on a withdrawal slip to get cash in a bank. Their natural ability has developed as they have matured and gained informal exercise. The range of this natural ability typically extends from 5 to 9 digits. With training, four college students were able to develop digit spans of over 20 digits. The two students who practiced for the longest time (Chase & Ericsson, 1982; Ericsson, 1985; Ericsson & Kintsch, 1995; Richman, Staszewski, & Simon, 1995) eventually attained an immediate memory for rapidly presented random digits, where their memory was improved by over 1000% (roughly corresponding to an effect size of 70—where average effect sizes for laboratory experiments are .5, which corresponds to less than one hundredth in magnitude). Even more importantly than the increases in performance, the cognitive processes mediating the performance after three sessions differed dramatically from the cognitive processes and the acquired skill mediating the performance after 40 sessions, which in turn differed from the performance and the complex acquired skill after over 200 sessions (Chase & Ericsson, 1981, 1982). If the memory performance after two hours of training (c.f. the amount of training by Fox et al., 1996) were to show evidence of heritability, it would be possible to increase the performance by over 1000% using different acquired mechanisms, and thus the heritability estimates for the natural ability-based performance after limited training have not been shown to be predictive of performance after extensive training. As these findings of dramatic improvements in memory performance with training were replicated (Ericsson, 1985, 2003c; Wilding & Valentine, 1997, 2006), the qualitative differences between the structure of the attained performance after different types and amounts of practice (Ericsson et al., 1993) were accepted by researchers of memory. In the article reproduced in this volume, Roy Roring, Katy Nandagopal, and I summarized the evidence from a wide range of studies of expertise for the modifiability of virtually all studied aspects of human performance, with one blatant exception, namely height and body size (see Ericsson, 1990, for a review of the strong and compelling evidence for the genetic influences).

SEEKING CONSENSUS ON SOME FORMS OF INNATE ABILITY AND TALENT

Anybody reading Gagné's chapter might be surprised to hear that I have acknowledged evidence for innate talent from the late 1980s (Ericsson, 1990). The most compelling evidence for innate talent (genetic portion of natural ability) should meet the following criteria. First, this evidence should refer to genetic correlates of reliable, objective, individual differences in attributes that differentiate the elite performers from less accomplished performers. Second, it should not be possible to change (improve) these attributes by any known form of training. The attribute may develop during childhood and adolescence, but this development should not interact with engagement in systematic training activities. Finally, we should ideally understand the nature of the genetic mechanisms that influence the growth of the associated tissues and physiological systems. A review of an extensive body of research (Ericsson, 1990) could not uncover any evidence that height and body size (length of skeletal elements of the body) could be influenced (enhanced) by training. The development of height may be influenced by diseases and malnutrition, but when the disease was cured and if and when malnutrition was remedied, there was evidence for catch-up growth. Identical twins in industrial countries have very similar height, considerably more similar than fraternal twins. More recent research has been able to explain most of the environmental factors that lead to increased height over the last couple of generations in industrial countries (Cole, 2000). Research shows how bones can be influenced by training, but it is only the width of bones and not the length of bones that can be influenced. For example, the thickness of the bones of the playing arm of tennis players is much greater than the thickness of the other arm (Haapasalo et al., 2000). Even though all the genes that control the development of height have not yet been fully identified, recent progress has been substantial (Visscher, 2008; Yang et al., 2010) and satisfies my stringent criteria.

Once it is established that one type of attribute, such as height or body size, is primarily determined by individual differences in genes, then it is a candidate for being a marker for innate talent—if particular heights and body sizes can be shown to be predictive of elite athletic status. The evidence that height and body size are related to performance in sports is overwhelming. The probability that an average male would have the height and body size required for playing in the National Basketball Association (NBA) is less than 1:20, and the average size of NBA players has increased steadily during the last decades (Norton & Olds, 2001). Tall athletes are favored in a wide range of sports, such as freestyle swimming for males and virtually every sport demanding strength and power. In direct contrast, short individuals have a great advantage in gymnastics, especially for females, and diving. One of the problems with the effects of height and body size is that they are not known when many children start engaging in a sport—in that sense it is really best described as a potential. Some coaches would request information about the parents' height and body size, which might be the best predictor of a child's adult height and body size. Furthermore, many adolescents may discover during development that their height is not well suited for the current sport, and they may turn to another sport where their height or body size is not a disadvantage for their pursuit of elite athlete status.

The evidence on the innate talent associated with height and body size provides a very important reference point for evidence on innate ability and talent. First, it shows that innate influences exist and that it is possible to accumulate valid evidence of innate effects on achieving elite athletic status. The relation between elite performance and height differs. In sports, extremely short or tall individuals are typically favored. However, in ballet the average height is favored, and extremely short or tall individuals are even deliberately excluded by teachers and ballet company managers (Blank, Gaynor, & Wozny, 2011). Secondly, in those domains where height and body size are related to elite status, it will be essential to partial out the associated effects before one can claim that other characteristics provide innate talent. Gagné (this volume, p. XXX) provides a very typical example by his citation of Dr. Kathryn North (the same North as in MacArthur & North, 2005), who said, "[T]the discovery could explain why 'some people train for ages but remain eighty-pound weaklings, while others develop muscles very quickly'" (Entine, 2000, pp. 254–255). Obviously, physical size must be a confounding factor in the strength development prospects of an 80-pound individual with presumably below-average height. If all claims for innate talent (the genetic portion of natural ability) could be explained by innate influences on height and body size, then Gagné would have no reason to disagree with our framework.

IN SEARCH OF MODIFIABILITY THROUGH TRAINING BY ADULTS

With respect to mental abilities and human characteristics other than height and body size, there are very complex interactions between genetic factors, environmental stimulation, and behavioral activities that result in the acquisition of superior performance during childhood and adolescence. As I already emphasized, the body is growing, and genetic factors influence its developing size. Furthermore, we know that the size of the body has an important influence on performance in sports and ballet. Studies of the acquisition of expert performance make a marked distinction between everyday activities, such as playful interactions, and engagement in practice activities that are focused on improving some aspect of performance by gradual repetition and refinement in response to feedback (Ericsson, 2006b). For those children and adolescents who have engaged in focused training for several years, it is difficult to distinguish maturation and its genetic factors from the effect of training. There are, however, some anatomical characteristics that have been linked to engagement in deliberate practice activities during certain critical developmental periods. For example, during the later part of childhood, when the children's bones and cartilage in joints are not yet calcified, if ballet dancers engage in stretching they can increase the turnout of their feet (angle between their feet in normal standing position), and if the baseball pitchers stretch back with their throwing arm they can increase their adult ability to do so (Ericsson &

Lehmann, 1996; Ericsson et al., 2009). Furthermore, physiological changes induced by deliberate practice are not limited to cartilage and bones. Recent research reviewed by Ericsson et al. (2009) has shown how skilled string players display neurophysiological differences in the cerebral cortex associated with the representation of their left hand, and that these differences are correlated with the age at which the subjects had started playing the musical instrument (Elbert et al., 1995). Musical training is also associated with increased gray matter volume in the primary motor, somatosensory, and auditory cortex (Gaser & Schlaug, 2003; Bermudez & Zatorre, 2005). The adaptations have been found to be related to differences in music experience and the start of music training. Expert musicians show higher levels of relevant excitation and mapping to a larger cortical area if they started training early (Pantev, Engelien, Candia, & Elbert, 2001; Pantev, Ross, Fujioka, Schulte, & Schulz, 2003; Penhune, Watanabe, & Savion-Lemieux, 2005; Trainor, Shahin, & Roberts, 2003). Most interestingly, there is now intriguing evidence from studies of expert musicians that have successfully related the amount of practice during particular periods of childhood and adolescence to the degree of myelination of different areas of the brain. Fredrik Ullén and his colleagues (Bengtsson et al., 2005) were able to identify several regions where the level of myelination was related to the weekly amount of practice at particular age ranges. Very recently, investigators (Hyde et al., 2009) have conducted experiments with 5- to 6-year-old children and random control groups to assess the effects of a half-hour of keyboard training per week for over a year. They were able to identify numerous changes in the structure of the brain related to acquired music skills in the training group, along with differences between the trained group and the control group. These findings highlight the importance of differences in early exposure to music training that cannot easily be explained by innate differences in the genes in their respective DNA or natural abilities that develop by maturation and informal activities.

There should be no disagreements between Gagné and me about the crucial role of genes in influencing the development of children, adolescents, and adults. The disagreements between us concern the role of individual differences in genes that would constrain some individuals from reaching expert levels of performance. In the section above, I accepted the evidence for genetic control of individual differences in height and body size, and would readily include facial features, color of eyes, skin, and hair. The crucial question to me is once these genetic factors are controlled, would there be additional individual genes that set individual differences in the limits on how far performance can be improved? In the earlier section we also discussed how training can influence the biochemical environment, which in turn activates genes in DNA, which in turn generate biochemical processes that modify the structure of muscles, arteries, and the heart (see Ericsson, 2007a, 2007b, for a detailed account). Scientific evidence for innate talent would require identification of particular genes or combinations of genes that would be shown to be necessary for attaining expert performance with any known type of practice activity. In the last decades there has been a frantic search for such unique genes, but, surprisingly to many, this search for individual genes has been unsuccessful so far. This was the theoretical context in which we (Ericsson et al., 2009) cited a review where MacArthur and North (2005) concluded that "individual differences in attained elite performance in sports *cannot*, at least currently, be explained by differential genetic endowment" (p. 37).

To give further background to my discussion of research on heritabilities of physical performance in Ericsson (2003b, 2007a, 2007b), I will try to summarize the lengthy discussion in a few paragraphs.

The most relevant section in Ericsson (2007a, pp. 20–21) starts like this: "Many scientists, such as Abernethy, Farrow, and Berry (2003) and Janelle and Hillman (2003), argue that demonstrated genetic influences on the development of physical characteristics and mental abilities under typical everyday conditions imply that genetic differences (innate talents) have to similarly influence the acquisition of elite athletic performance. Although behavior geneticists have expressed support for such an extrapolation from observed heritabilities of everyday abilities to those of expert performance (Bouchard & Lykken, 1999), they also acknowledge that any generalization of heritability estimates depends directly on the similarity of environmental conditions of everyday activities to those of expert training." I discussed the evidence for how basic characteristics, such as aerobic capacity, can be changed through training and that the measurement of maximal capacity is problematic. This type of testing requires extreme effort on the part of the tested individual and thus is really an indirect test of motivation. I also showed that the research findings on twins are much more varied across studies. In at least one case, when researchers controlled for differences in body size, there remained no reliable heritable variance. I also raised the issue of motivation to start, increase, and sustain deliberate practice to attain and maintain physiological adaptations (Ericsson, 2007a, pp. 23–24): "My colleagues and I (Ericsson et al., 1993) recognized early on the difference between genetically determined capacities (innate talent) and the ability to engage in deliberate practice: 'we reject an important role for innate ability. It is quite plausible, however, that heritable individual differences might influence processes related to motivation and the original enjoyment of the activities in the domain and, even more important, affect the inevitable differences in the proclivity to engage in hard work (deliberate practice)' (p. 399). How could one demonstrate that nearly all healthy individuals are capable of acquiring the required physiological adaptations, if they are not willing and sufficiently motivated to engage in the intense physical exercise required to produce such adaptations?"

Ericsson (2007a) then goes on to discuss animal research where rats and horses forced to exercise by the use of punishment and other incentives displayed profound physiological changes that occurred as a result of enforced training.

The central issue for our theoretical framework is the degree to which heritabilities measured after shorter and less intense training can be generalized to the heritabilities after intense training extended for years and decades. Genetic researchers emphasize that a given heritability estimates the amount of variability related to genes in a given population and for a given type of environment. In an influential review, Charmantier and Garant (2005) show that heritabilities are higher for animals growing up in favorable environments as compared to stressful environments, such as those with unpredictable food sources and adverse temperature and weather conditions, where organisms have to adapt and learn to survive. An analogous argument could be made that heritabilities estimated for human abilities in typical natural environments with play and recreation may not generalize to heritabilities estimated in environments where daily deliberate practice is engaged in for several hours on a daily basis, typically seven days a week, every week for years. Even the researchers that Gagné cites raise these questions as the most important ones for implications of the laboratory research. In his recent review of genomic effects on trainability, Claude Bouchard (2012, p. 350) writes in the final paragraph: "One key question is whether the response pattern in a given individual is specific to the given exercise mode and regimen. Another issue is that of the duration of the exercise intervention or the training programme. Would the fitness outcomes be different if the exercise programme lasted for years instead of months?" It would thus appear that Gagné (this volume) and his supporter(s), who believe that these issues are already settled, are in the minority.

BUILDING NEW STRUCTURES THAT MEDIATE THE ACQUISITION OF EXPERT PERFORMANCE

This chapter has so far focused on reviews of evidence on modifiability by training and has primarily been a review of other investigators' research findings. My colleagues and my own research have relied on a different approach. We have tried to study the acquisition and structure of acquired superior performance. My work on memory training with Bill Chase provides a nice example of the radical change in thinking about the acquisition of expert performance. In the 1970s, the main theory of skill acquisition (Fitts & Posner, 1967) argued that, with training, individuals were able to automate their performance, so that with sufficient experience the performance was effortless. Hence, training did not modify general abilities, such as the capacity of short-term memory (STM) and working memory, and thus the effects of training were limited to automation of fixed procedures and forming larger chunks in long-term memory (LTM). In fact, when I met Bill Chase as a postdoctoral fellow at Carnegie Mellon University in 1977, his research examined different measures of individual differences in STM. I approached Bill with some early studies showing that immediate memory for lists of digits (digit span)—the primary measure of the fixed capacity of STM—could be improved by training (Chase & Ericsson, 1981). He was intrigued by whether digit span could be improved from a value in the normal range of 5–9 digits to a trained performance well outside of that range, such as 15 digits; if so, then clearly the capacity of STM is not fixed and would not describe an invariant capacity limit of the human information processing system (Newell & Simon, 1972). Bill Chase and I (Chase & Ericsson, 1981, 1982) conducted a series of training studies showing that with hundreds of hours of training, two participants were able to gradually attain a digit span of over 80 digits—an effect size that is 100 times greater than the normal effect size in psychology studies. More important than the demonstrated improvement in memory, our work examined the changes to the cognitive processes during the acquisition of the increased memory performance by collecting verbal reports from the two participants and then tested hypotheses by carefully designed experiments. These findings were published in *Science* (Ericsson et al., 1980).

Chase and Ericsson (1981, 1982) proposed that individuals could increase the storage of accessible information in LTM by acquiring a memory skill (Skilled Memory Theory), which permitted individuals to increase dramatically their memory for particular types of information without relying on an expansion of the fixed capacity STM. These findings were replicated several times in experimental training studies and were found to be consistent with a large body of studies of individuals with demonstrated exceptional memory (Ericsson, 1985, 2003c; Wilding & Valentine, 1997, 2006). These ideas were generalized to long-term working memory (LTWM; Ericsson & Kintsch, 1995; Ericsson, Patel, & Kintsch, 2000), which proposes that experts can acquire expanded functional working memory in a wide range of domains of expertise, such as chess, sports, medicine, and music, and skilled activities, such as text comprehension.

The crucial findings are that the acquired skill is not attained by simply automating and building large chunks, but involves building new mental structures and mechanisms that are not constrained by fixed general capacity limits set by STM and working memory. These improvements reflect large differences in performance within a given individual rather than differences between individuals, whose different performance may correspond to different information in their DNA. Given that people's DNA cannot be changed by training, these improvements are accomplished with the same DNA, and thus the difference in performance before and after training cannot be explained by innate talent or unique genes. Research shows that the improvements of performance require engagement in deliberate practice on a regular basis for weeks, months, and years, and in many domains it is estimated that expert performers have accumulated over 10,000 hours of solitary practice (Ericsson, 2006b).

Over historical time there has been a remarkable increase in the level of performance in sports, as shown by this quote from our target article (Ericsson et al., 2007a, p. 42):

> Some of the most striking improvements in the level of performance over historical time are found in sports, where today's world records may be as much as 50% superior to those a century ago (Schulz & Curnow, 1988). The elite performance for events such as the marathon and swimming have changed so much that the gold medal winners of the early Olympic Games would barely meet the standards for amateur athletes, such as entry in the Boston Marathon, and admittance to high school teams in swimming. In these sports there have been large increases in the intensity and daily duration of practice (for a review, see Ericsson et al., 1993; also see Ericsson, 1990, 2003a, b). In technical domains, such as diving, figure skating, and gymnastics, the difficulty levels of routines have changed and many of the available triple and quadruple jumps in figure skating were not even considered a few decades ago. The change in technical standards becomes even more apparent if one considers that after the IVth Olympic Games in 1908, officials almost prohibited the double somersault in dives because they believed that these dives were dangerous and no human would ever be able to control them. Today triple somersault dives are standard among elite divers.

These qualitative changes in performance suggest that there are also qualitative changes in the mechanisms mediating the performance that are the result of years and decades of deliberate practice. Based on the theoretical assumption that expert performance is mediated by the acquisition and construction of new mechanisms, it is not obvious and, in fact, even unlikely that natural abilities, such as those measured by performance on IQ tests, will constrain the level of attainable performance. Gagné asserts that whatever performance on IQ tests measure, such as working memory, is correlated to the level of attained performance in a domain of expertise because "natural abilities or aptitudes serve as the 'raw materials' or constituent elements of talents" (Gagné, this volume, p. XXX). Gagné (1995) describes the relation even more clearly: "natural abilities are the constituent elements of systematically developed ones. Indeed, the reasoning of a mathematician is just an application of general reasoning abilities to the particular subject matter of numbers; the interpersonal qualities of a salesperson of general social abilities to the particular context of sales." In contrast, we propose that the mechanisms that mediate high levels of skilled performance in domains of expertise, such as chess, Go, and music, are acquired (constructed) to support performance in the particular domain and thus not made with the same mechanisms (elements) responsible for performance on IQ tests. Both hypotheses predict that individuals with lower levels of skill, such as children playing chess, will be forced to rely on general skills and abilities because they have no recourse to acquired (constructed) skilled mechanism. For low-level individuals, researchers have found correlations between chess-playing skill and performance on IQ tests (Ericsson & Charness, 1994; Ericsson et al., 2007a). Most interestingly, however, the two hypotheses yield different predictions for skilled individuals. According to the expert-performance hypothesis (Ericsson & Charness, 1994), skilled individuals would have acquired mechanisms to generate their superior performance and thus would not rely on any general natural abilities. In support of this hypothesis, studies of highly skilled players have not found significant correlations between chess performance and tests for general ability (Djakow, Petrowski, & Rudik, 1927; Doll & Mayr, 1987). Some of the grandmaster chess players in Doll and Mayr's (1987) study even had IQ scores below the normative mean of 100. In a recent study, Bilalič, McLeod, and Gobet (2007) found a correlation between IQ and chess skill in young chess players, even when they controlled for practice. Consistent with Ericsson and Charness' (1994) hypothesis, they found that when they identified the best players there were no significant correlations, and even a suggestion of a negative relation. Similarly, Masunaga and Horn (2001) found no differences between the highest-level expert Go players and lower-level experts in their performance on cognitive ability measures. Furthermore, 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. In a very recent study, Meinz and Hambrick (2010) reported a significant correlation between measures of working memory and sight-reading performance, but this study included data on individuals with lower levels of sight-reading in their statistical analysis, thus confounding the two hypotheses. Finally, Ruthsatz, Detterman, Griscom, and Cirullo (2007) examined the correlation between performance on IQ tests and music performance, and they found a significant correlation among musicians with lower levels of music skill, but not among the highly skilled musicians. Only future research on the structure and acquisition of expert performance will allow us to develop a general theory of both general natural abilities and expert performance.

THE ISSUE OF AGING AND THE EVIDENCE FOR BIOLOGICAL DIFFERENCES

In the renewed attack, Gagné (this volume) insists that my co-authors and I "totally ignored" the work of Kliegl, Smith, and Baltes (1990) and the comment by Baltes (1998) on Howe, Davidson, and Sloboda's (1998a) target article. I had thought that we had explained why the research referred to by Baltes (1998) was not relevant to talent and excellence in our reply published several years ago (Ericsson et al., 2009, p. 148):

> Gagné incorrectly assumes that these articles prove *innate* capacity differences among different adults. However, the individual differences referred to the quote from Baltes (1998) in his chapter (Gagné, this volume) concerned *effects of age (the performance of 66 to 80 year olds versus the performance of 20 to 30 year olds)*. That study essentially examined a *within-subjects variable*—we all have been 20 years old and will eventually be over 60 years old. The same differences between age groups would be expected if participants' performance on this memory task was tested first when they were 20–30 years old and then a second time at 60–80 years of age. Given that the genetic make-up (DNA) does not change with age these performance differences due to age cannot be explained by innate talent or inter-individual differences in genes.

Baltes (1998, p. 408) does claim that Howe et al. (1998a) did "ignore" individual differences variables and more specifically that "the correlation between the skill trained in this testing-the-limits experiment and a multivariate measure of intelligence was larger at the end of training." Whereas Baltes (1998) is clear that he is referring to studies of age-related individual differences, Gagné seems to believe that he is referring to more general results regarding individual differences in talent. Baltes (1998, p. 408, italics added) even anticipates this ambiguity when he acknowledges the difference between aging and individual differences in innate talent when he writes: "Howe et al. are likely to argue that research into age-associated individual differences of asymptotes in performance potential *is not a direct analogue to the cases of talent and excellence* that they have pursued." A more careful reading of Baltes' (1998) comment shows that he wants Howe et al. (1998a) to recognize that testing the limits would be a good paradigm to use in future studies of individual differences related to talent. When Howe, Davidson, and Sloboda (1998b) responded to Baltes (1998) in their reply to commentators, their response concerned primarily the merits of the testing-the-limits approach, and there are no disagreements about the empirical results on effects of aging reported by Kliegl et al. (1990). The existence of age-related differences in performance before and after training is obviously due to biological factors if we compared groups of 3-year-olds to 20-year-olds and no more controversial if we compared 20-year-olds to 80-year-olds. Many age-related changes are similar to changes in height and body-size—they will take place for a developing individual independent of training and are controlled by biological processes.

In a recent study, my collaborators and I studied the performance of monozygotic and dizygotic twins on a wide range of laboratory tasks while the participants verbalized their thoughts aloud (Nandagopal, Roring, Ericsson, & Taylor, 2010). We were able to show that cognitive strategies were related to superior performance on several of the tasks. Most interestingly, we also found that the cognitive strategies of monozygotic twins were more similar than for dizygotic twins and that this relation could not be explained by variables measuring scholastic and cognitive abilities. This raises an important issue and challenge for future research. It is not appropriate to simply give people opportunities to train and practice, but researchers need to monitor the thought processes of participants during learning and performance (Fox, Ericsson, & Best, 2011). Researchers need to study the strategies and the factors leading participants to adopt strategies to fully explain performance differences.

PART II: COMMON SENSE, ANECDOTES, AND "EXPERT" OPINIONS VERSUS SCIENTIFIC EVIDENCE

In a commentary by Howard Gardner (1995)—the famous proponent of multiple intelligences—on Neil Charness' and my argument for the acquired nature of expert performance (Ericsson & Charness, 1994), he argued for innate talent based on "logic and common sense" (Gardner, 1995, p. 803). Gardner (1995) suggested that the experimental studies might never be able to capture the difference between "the technical virtuoso pianist from the brilliantly expressive one who wins a competition" and that only by studying world-class performers will we be able to understand their unique talents. Without studying only the best performers in each domain of expertise, we will understand only the skills mediating "impressive but somewhat less outstanding" (p. 803) individuals. In his famous book *Creating Minds,* Gardner (1993) illustrated how researchers can learn from the very best individuals by reviewing information on their lives based on interviews with them, their teachers, and their associates.

Consistent with Gardner's (1995) focus on the most outstanding performers, Gagné (this volume) directs the reader to anecdotes of one outstanding athlete and a world-famous musician, along with a case description of Sarah Chang's music development, as his most compelling evidence for innate talent.

NATURAL GIFTS COMING FROM THE SKY

Gagné (this volume, p. XXX) quotes a description by the mother of Quebecer Mario Lemieux—one of hockey's most celebrated players. Gagné claims that the description below refers to her memory of "the first time her son stepped onto an ice rink":

> When he disappeared into the crowd of boys with his stick and his puck, he took all eyes with him. They all said, "Look at this little guy, look at him go, look at him skate!" Pierrette says.… "That," Pierrette insists emphatically, "came from the sky. It's a natural gift. No one in the family gave it to him." (Brender, 1997, p. 14)

The description of the skating episode seems to me to be an example of one "of their imaginary Pronat straw men: *sudden appearance*" (Gagné, this volume, p. XXX). Nonetheless, I will try to address the evidence value of this description. A little search on the Internet uncovered evidence that raises questions. In a biography, Christopher and Stout (2002) describe Mario as the third son in the hockey-crazed family. Mario started playing hockey at a very young age:

> By the time Mario put on skates at the age of three, he already knew the basic rules of the game. He and his brothers pretended that the Lemieux family's basement was a hockey rink and would slide about in the stocking feet, using wooden kitchen spoons as hockey sticks and a bottle cap as a puck. (pp. 2–3)

Further evidence for the unusual environment of the Lemieux family: Mario's parents even created ice on the floor INSIDE their house so they could skate in their house: "…Pierrette [Mario's mother] and Jean-Guy [Mario's father] even carried snow into the house and packed it down on the front hallway, through a door into the dining room, then through the living room and back into the hallway. They left the door open so it would stay cold and the boys could skate through the house" (Cristopher & Stout, 2002, p. 4). Further considerations are that Mario's mother might on occasion exaggerate Mario's accomplishments. In a published interview (Hockey Central, 2012), Mario's mother insisted that the time "Mario scored a goal and an assist that day against players 9 and 10 years old" he was only 6 years old, whereas his father claimed that he was 7 or 8 years old. In fact, a superficial search of the Internet uncovered sources claiming facts that are directly inconsistent with Pierrette's cited description, namely:

> Mario was taken for his first skating lesson when he was three years old. Like thousands of children before him he had to learn how to skate and to play hockey from scratch. During his first years on skates Mario skated under the watchful eye of coach Fernand Fichaud. *One year after his first hesitant steps* onto the ice when he still was only four years old, Mario showed Fichaud such a spectacular move that a dozen years later the moment still sparkles as bright as a diamond in the memory of the coach." (Angelfire, 2012, italics and underlining added)

This latter account would suggest that someone interested in "innate talent" might want to objectively document and account for Mario's spectacular move at age 4.

NEVER SEEN OR HEARD ANYTHING QUITE LIKE IT

Gagné (this volume, p. XXX) quotes Dorothy DeLay, a renowned professor at New York's Juilliard School of Music, as she recalled her first encounter with the young violin prodigy, Sarah Chang: "I think she was six, or perhaps five, and she played the Mendelssohn concerto with real emotional involvement, and I said to myself, 'I have never seen or heard anything quite like it in my entire life' (Lang, 1994, p. 123). Is professor DeLay an unreliable witness? How can one explain such extreme precocity without invoking some form of natural talent?" (Gagné, this volume, p. XXX).

Let us take Dorothy DeLay's word for the fact that she had "never heard anything quite like it in [her] my entire life." The question then becomes what kind of experiences that DeLay had had during her career prior to first hearing Sarah Chang. The music students that DeLay worked with at Juilliard were in their late teens to early 20s (Sand, 2000). She did work with younger musicians on Saturdays in her home in Rockland County, outside of New York City, as part of Juilliard's Pre-College Division program. Very few of them were below 8, especially before the time she first heard Sarah Chang. As we will show later in more detail, Sarah Chang did not play on a regular violin, but a quarter-sized violin, which would have made the experience quite unusual regardless of Sarah's remarkable music skill. This leads to the question of why and how Dorothy DeLay even came to listen to young Sarah playing music. How many other young children had Dorothy DeLay already listened to when she first heard Sarah? Without knowing the answer to that question, it is difficult to assess how special Sarah Chang's performance would have been compared to other children with similar training history.

In her book, Sand (2000) mentions Yehudi Menuhin, who gave his sensational debut as music prodigy in Oakland in the 1920s. Around six decades later, Sarah Chang's performance at age 8 with the renowned New York Philharmonic Orchestra created a public sensation, which suggests that she was doing something different from what the music audience had recently seen and heard before. Most strikingly, Sarah differed from the majority of other performers by her sex (most child prodigies have been boys; Goldsmith, 1987) and her young age (even Midori was 11 and thus substantially older at her debut; Wikipedia, "Midori Gotō," 2012). Yoshihara (2007) argues that there were several Suzuki-trained musicians of Asian descent who were breaking through in the late 1980s. The Asian background of Sarah and her female gender added to the experience. In an interview, Sarah "recalled that in her first part of her career when she was hailed as a child prodigy, her mother used to dress her up in red and pink and put bows on her dresses and hair" (Yoshihara, 2007, p. 122). After the freshness of the new experience of a young child playing difficult music pieces, there was a reduced interest in young music prodigies (Niles, 2009). In sum, there are several other factors that made Sarah Chang's performance in the late 1980s and early 1990s capture the attention of audiences (including Dorothy DeLay). Without comparing Sarah's performance to other children with similar starting ages, parental encouragement, and training resources, we cannot establish that Sarah's music performance was outside the range of these other children.

THE PRODIGIOUS DEVELOPMENT OF SARAH CHANG'S MUSIC PERFORMANCE

In his chapter, Gagné (this volume) makes Sarah Chang's rapid development as music performer the cornerstone of the evidence of innate talent that cannot be accounted for by environmental circumstances. He starts with a quote from the Pittsburgh Symphony Orchestra's artist page, which he downloaded in 2007:

> Born in Philadelphia to Korean parents, Sarah Chang began her violin studies at age 4 and promptly enrolled in the Juilliard School of Music, where she studied with the late Dorothy DeLay. Within a year she had already performed with several orchestras in the Philadelphia area. Her early auditions, at age 8, for Zubin Mehta and Riccardo Muti led to immediate engagements with the New York Philharmonic and the Philadelphia Orchestra. (Gagné, this volume, p. XXX)

Starting with the violin at age 4 and then performing with the New York Philharmonic Orchestra at age 8 implies that her public performance was accomplished with around four years of practice. We cannot, however, analyze the quality of her performances because there are no publically available recordings of them. The earliest publically available recording is Sarah Chang's first record *Debut*, which is still available for purchase today. This recording was released when Sarah was 10 years old, and thus should reflect an even higher performance than when she was only 8 years old. Hence, when I address the five questions given to me by Gagné, I will focus on the recordings as the target performance and then discuss what we know about Sarah's training during around six years preceding the performance on the *Debut* album.

The biography cited by Gagné (2007, this volume) surprisingly omits some critical information for judging the development of Sarah's unique achievement. Perhaps the most surprising omission is that Sarah Chang did not play on a regular violin at her auditions, at her New York Philharmonic Orchestra debut, and at the recording studio at age 10. She was playing on "a quarter-sized fiddle" (Godell, 1995) developed to allow young children to play the violin with the Suzuki method. Gagné disparagingly contrasts Sarah Chang with other children who learn with the Suzuki method but omits mentioning that she was trained with the Suzuki method in the beginning (Yoshihara, 2007)—in fact, Sarah Chang says that she started with the Suzuki method, supervised by her father, in an interview with Laurie Niles (2009).

Listeners to Sarah Chang's violin playing were provided with a very unusual experience, namely hearing classic music played on a quarter-sized fiddle. In his review of Sarah Chang's debut album, Godell (1995) states: "For this recording, Chang plays on a quarter-sized fiddle that sounds wiry, thin and nasal in the upper register, weak and boxy at the bottom end." When someone like Sarah Chang keeps playing on a quarter-sized violin at the relatively mature ages of 8 and 10 years old, it will be rather difficult to compare her performance to those of other children and adults playing with regular violins. Consequently, Gagné's third question—"Do her 8-year-old achievements compare favorably with those of most 'expert' *adult* violinists studied by Ericsson?"—cannot be answered even with respect to her studio recording at age 10.

To address Gagné's first question about whether Sarah's achievements provided "verifiable" facts on her extremely rapid progress, we need information about Sarah's daily activities prior to her achievement at age 10. There is very little published information about her development, especially before her performance at age 8 with the New York Philharmonic Orchestra and the release of her album, *Debut*, at age 10. Akin to other published discussions of prodigies, the interactions between child and parents is not documented by third parties and scientists. When prodigies give their successful public performances, it is difficult to assess retrospectively their prior training activities during their childhood. In numerous cases there have been allegations that music prodigies as younger children have been forced by their parents to practice by threat of physical punishment and by other more subtle means. Sand (2000) argues that Hayden, Mozart, and Beethoven were abused as children in an attempt to achieve financial gain and fame for their fathers, and that violin virtuoso Paganini was forced to practice 10 hours a day by his father. Sand (2000) also describes the famous pianist prodigy Ruth Slenczynska, who at age 32 wrote a biography (*Forbidden Childhood*) after a nervous breakdown. She claimed that she had been forced to practice nine hours a day supervised by her domineering father, who punished her for every mistake. More recently, the famous pianist Lang Lang told of an event where his father did not think that Lang Lang had practiced hard enough, so he told Lang Lang to commit suicide by jumping from the 11th-floor balcony to his death (Kendall, 2010). Similarly, Michael Jackson, the famous "pop music" singer, claimed that he had a deep fear of this father, who he remembered as holding a belt in his hands during rehearsals and practices (Bashir, 2003). There is no reason to believe that all music prodigies were punished, threatened, or maltreated by their parents, but we cannot know because successful prodigies and their parents have no incentive to report any repulsive details of a difficult childhood. Instead, there would be a joint incentive to tell accounts emphasizing the joy and passion of the child to play and limiting practice to short durations. The use of physical punishment to force children to practice is believed to continue, and Sand (2000) gives an example of a music student at Juilliard who had unusual marks on her arms suggesting that someone (most likely her mother) had repeatedly pinched her to force her to continue attentive practice. Similar accounts of parental abuse in sports have been given by Andre Agassi (2009) when he, as a child, had to train with a tennis-ball cannon.

What do we know about Sarah Chang's musical development and her opportunities for practice and instruction? To me it is relevant that Sarah's father was a violinist, that her mother was a composer, and that there were no other children in the family until Sarah was around 7–8 years old. According to Sand (2000), Sarah was immersed in music from birth, with her parents actively engaged in learning to make and listen to music. Sarah is reported to have started playing around with piano to reproduce melodies at age 2 (Sand, 2000) and was given instruction by her mother in piano playing at age 3. At her 4th birthday Sarah got a quarter-sized violin, and her father started training her on a daily basis using the Suzuki method (Niles, 2009; Yoshihara, 2007). Sarah's mother said that 8-year-old Sarah was concurrently practicing 2–3 hours per day and even more during the summers at the Aspen Institute (Sand, 2000). In interviews, Sarah reports that she spent four hours a day at age 18 (Masur, 1998) and 4–5 hours a day at around age 30 (Niles, 2009) practicing. So Gagné's question is whether Sarah's achievements at age 8 and 10 provided hard evidence for "extremely rapid progress" in support of innate talent. As far as I know, we don't know how many children were given a comparable environment with two professional musicians as parents and a start with music at age 2–3 *and* who had similar help with practice *and* engaged in daily amounts of practice (we don't know the exact amount). I would argue that without a comparison group exposed to similar environmental opportunities, we cannot infer that Sarah Chang offers evidence of unique innate gifts—at least not based on scientific evidence. It is interesting to note that two of the four outstanding successful musicians of Asian descent interviewed by Yoshihara (2007) were violinists, and both had started playing the violin with the Suzuki method at age 3, but neither had professionally trained musicians as parents.

Gagné's second question raises the question of whether Sarah Chang's performances at age 8 (and 10) qualify as "expert" performance. I have already mentioned that Sarah played with a quarter-sized violin, which makes it very hard to compare her performances to those played with a regular violin. The most useful source of interviews would ask Sarah Chang to compare her earlier performances at age 8 and 10 to her adult performances of the same pieces. In an interview, Niles (2009) asked her about a recent recording of a violin concerto by Max Christian Friedrich Bruch, and she responded:

> I learned the Bruch when I was five. It means I played the notes, and I had enough emotion in me then to play it well—at least well enough to get into Juilliard; it was my audition piece. Mozart three and the Bruch were my two audition pieces for Juilliard. And then I put it away, I didn't touch it. I didn't touch it for another 10 years, and then I started performing it in public when I was 17 or so. . . . So again, I put it away for about 10 years. I was probably 18 or 19 before I even went on stage with the Brahms. From that point on, with every concert, that's when the learning actually begins. That's when you realize how very little you do actually know about the piece! (She laughs).

It would have been interesting to have Sarah's recordings released at age 10 rated along with tape-recorded performances of other adult students at music academies under blind conditions, but Sarah played them with her quarter-sized violin and the others with regular violins. I conclude that it is not possible to answer Gagné's first and second questions based on objective evidence.

Gagné's fourth question concerns whether Sarah Chang's achievements provide a clear exception to the 10-year rule, where Simon and Chase (1973) argued that 10 years of engagement is necessary to reach an international level of adult performance. Based on the earlier comments, it is difficult to evaluate Sarah's performance on the quarter-sized violin, but her performances at age 8 and 10 are unlikely to have reached an international level for adult violin performance.

Gagné's (2007, this volume) fifth and last question to me states: "[d]oes Ms. Chang's early progress exceed by 'galactic' units the learning pace of those thousands of young violin learners who 'screech' their way through years of Suzuki classes?" It is somewhat noteworthy that Gagné did not mention that Sarah was trained with the Suzuki method by her farther, as stated in her interview (Niles, 2009) and in the book by Yoshihara (2007). If Gagné's statement is merely that Sarah Chang exceeded the performance of children who trained less in less supportive learning and developmental environments, then there is no disagreement. But if the claim is that Sarah Chang was born with some innate talent that was a necessary prerequisite for her development, the evidence does not meet scientific standards. There are three lines of evidence that raise issues about the innate talent claim for Sarah Chang.

First, Sarah's training schedule is not distinguishable from other expert violinists as she trains and matures into an adult music performer. When she is asked in an interview (Niles, 2009) how she maintains her high level of technique, she answers: "Basics, every day. There's no other secret, really. I wish there were! (She laughs) Scales, arpeggios, thirds, octaves—the regular stuff you would hate as a student, you need to do it every day." Her practice schedule as described in interviews (Masur, 1998; Niles, 2009) is remarkably similar to those described for the expert musicians studied by Ericsson et al. (1993; Ericsson, 2002).

Second, Sarah seems to attribute her achievements to her focused practice. In her interview with Masur (1998), she emphasizes the importance of focus and concentration: "I think if you're going to practice, if you're going to put the time and the effort into it, then you really should just go into your music room and start practicing. Even if it's not for 2 or 3 hours at a time, even if it's just 30 minutes, you should just do it and make sure that your time is well spent." Sarah's belief in the critical role of practice seems to generalize to other domains as well. In her interview with Masur (1998), they started talking about Sarah playing tennis with her younger brother and she was asked, "Why aren't you a great tennis player?" and her answer was: "Oh, because I haven't given it my all. Definitely. I mean I started tennis for fun."

Finally, there are several reasonable hypotheses about unique environmental conditions that could explain the development of Sarah Chang from a music prodigy to a world-class violinist that do not depend on her innate talent or inborn advantage. The vast majority of music prodigies do not develop into successful adult professional musicians (Bamberger, 1982). Consistent with Bamberger's (1982) hypothesis that development of an early ability to generate musical expression, my own favorite account focuses on Sarah's early learning environment, where her parents stimulated her experience and generation of music. Sarah's father and mother created a unique environment focusing on the experience of music, and one of the outstanding characteristics of Sarah's first music performances was that they gave expression to music experiences rather than a display of mere technical proficiency. There are other unique factors in Sarah's development. A frequently mentioned factor was Sarah's public performances at a very young age, which removed the difficult transition to public performances during her teenage years (Sand, 2000). A related factor was Sarah's enjoyment of performing. In her interview with Jones (2000), she said: "I love being onstage, so that is the ultimate for me …having a live audience in front of you. There's nothing that comes even close to that; it's something very special that you can't even put into words," Chang says. "It's a completely different level of an adrenaline rush that you get than anything else."

Scientific research on early activities involving children and their parents may help improve the development and facilitation of many characteristics that seem to differentiate successful from less successful development of performers into adult expert performers. In my search for new studies on talented children and adolescents, I discovered a recent dissertation by Jeongmin Park (2009), who designed a questionnaire based on Gagné's (2003) Differentiated Model of Giftedness and Talent (DMGT) that 136 teenage and young talented music students from Sydney, Australia, and Seoul, South Korea, and 88 of their parents filled out. She found that the Korean music students attained a higher level of technical proficiency, which could be explained by their much higher level of accumulated practice (roughly 100% more hours of practice) and the cultural environment's support for practice and training. Rather than supporting the assumptions of DMGT, she concluded that after a remarkably careful review of published research on giftedness and an analysis of the questionnaire data from her dissertation, the importance of innate talent is not supported. Park (2009) did find strong support for "the role of parents, the importance of practice, and various environmental factors which can influence each music student, *rather than focus too much on early musical signs to identify the musically gifted*. As much of the research cited indicates such early signs are unreliable" (p. 304, italics added).

In sum, during the last 30 years I have been told of potential exceptions that might be evidence for innate talent. I have tracked down dozens of cases and always found that either the alleged performance evidence could not be substantiated by any written report or was simply a report from memory, often written down years or even decades after the original event. In those cases where there seemed to be sufficient evidence, such as in the case of Mozart, I found that alternative accounts based on practice, instruction, and training were just as plausible (Lehmann & Ericsson, 1998). In the case of individuals with superior memory performance, with or without alleged innate talents, we brought the individuals into our laboratory and collected verbal reports on their thought processes and designed experiments to test hypotheses about various mechanisms. Based on a series of studied individuals (Ericsson, Delaney, Weaver, & Mahadevan, 2004; Ericsson & Polson, 1988a, 1988b; Hu & Ericsson, 2012; Hu, Ericsson, Yang, & Lu, 2009), we have demonstrated a methodology that can assess the structure of the mediating mechanisms and that can predict the kind of experimental manipulations that would interfere with the superior performance and reduce it toward the level of control participants. My challenge to scientists who want to demonstrate evidence for innate talent is to design similar series of experimental studies that can assess the specificity and generalizability of the hypothesized innate mechanisms mediating the superior performance.

WHEN THERE IS NO SCIENTIFIC EVIDENCE, ASK THE "EXPERTS"

Gagné verified our claim about MacArthur and North's (2005) conclusions about unique genes in the DNA of elite performers: "individual differences in attained elite performance in sports *cannot*, at least currently, be explained by differential genetic endowment" (Ericsson et al., 2007, p. 37). Gagné's response (this volume) was to start searching for statements in that article and other sources that express the opinions of researchers and authors about the role of genetics in determining the highest level of attainable performance. For example, MacArthur and North (2005, p. 331) write: "Most of us could never achieve elite athlete status, however hard we trained." I do not know the two authors personally nor the individuals that they refer to as "us," but I will assume that it is possible to agree on the fact that none of them has yet achieved elite athlete status and that they are past the prime age of athletic achievement. I know of no examples of individuals who started training after the prime age of athletic achievement who were able to reach elite athletic status in Olympic sports. If one is to interpret the statement as a statement of the future, then I would agree that it would be unlikely that any one of them would be able to do so by any type of training. The scientific issue that my colleagues and I are interested in is whether these individuals hypothetically could have reached elite athlete status if their developmental history had been different. What would have happened if they had been brought up by very encouraging parents, who had guided and encouraged them to develop some athletic ability and who had invested in getting the best available coaches and training environments?

It is much easier today than when I started my research with Bill Chase over 35 years ago to point to examples of children who were given such opportunities, kept engaging in intensive training and deliberate practice, and eventually reached elite athletic status, such as Tiger Woods (Woods, 1997) and Andre Agassi (Agassi, 2009). Given that new training methods and better training environments are continuously developed, it is unlikely that we can ever identify the optimal developmental experience for a given individual and thus experimentally evaluate the conditions described by MacArthur and North (2005, p. 331) as "however hard we trained."

If we accepted Gagné's argument that we should interview "experts" who lack scientific evidence and are even willing to agree to that lack of knowledge, then there is no need for future research because we already have our answer. In a similar vein, real scientists do not accede to authority, unless the authority can establish their claims with reproducible scientific evidence.

CONCLUDING COMMENTS

In this chapter I have tried to share my view of how we will be able to accumulate knowledge about the structure and acquisition of expert performance. My commitment is completely to science and scientific evidence. As I indicated in the introduction, I have a long history of interacting with researchers with different theoretical frameworks where they explicitly search for genes that can explain individual differences in physical and mental attributes and performance in different domains of expertise. Scientists like me must be allowed to review evidence and try to reproduce results, but eventually they will accept whatever future research findings may show convincingly. The current evidence for giftedness does not meet my understanding of scientific standards, and it is puzzling to me how teachers, some researchers, and children still embrace its existence and its importance. I remain, however, willing to accept that the teachers' and parents' beliefs that a child is talented (devoid of any objective evidence for innate talent) may be associated with superior development of performance. Ever since the paper with Ralf Krampe and Clemens Tesch-Römer (Ericsson et al., 1993), I have seen evidence in support of "perceived talent," which is likely to influence their parents' efforts to support the child's needs for travel to teachers, public performances, and competitions; the need to have time for practice (relieved of other family responsibilities); and even the need to move permanently to a different country to provide access to the best teachers in the world. The teachers' and parents' efforts and their expressed confidence in the child's eventual success are also likely to build up the child's self-confidence, which may be particularly important during periods in adolescence and early adulthood when the child experiences no performance improvements due to sickness or other circumstances (Bloom, 1985).

I would like to restate our wish, published as the last sentences of our earlier chapter (Ericsson et al., 2009, pp. 149–150), where we initially responded to Gagné's (2009a) attacks:

> We would hope that all readers are committed to the long and cumbersome path of collecting and analyzing reproducible scientific evidence on high ability rather than breezing through the literature looking for supporting opinions of popular authors without adjoining scientific evidence or failing to read scientific articles carefully. Remember that arguments based on cited beliefs of other people do not constitute reproducible scientific evidence! Our final wish is that scientists refrain from ad hominem attacks and debate topics with the normal tools of the scientific method.

Hopefully, this is the last time I need to make this request. I am very excited about the potential for collaboration between researchers and expert performers and their coaches and teachers. I am worried that Gagné's proposal for dividing individuals into two dichotomous groups with opposing beliefs will distract from a shared commitment to increased collaboration toward a translational science of expert performance (Ericsson & Williams, 2007). In translational science, there is a dialogue around the training of expert performers so that important issues in training will be captured by laboratory tasks. Research on the laboratory tasks using refined measures of performance along with "think-aloud" protocols and other methods of process tracing will uncover the structure of the superior performance. These findings will then be translated into changes in training, where its effectiveness can be evaluated by analysis of performance during competitions and work-related activities. The current excitement about deliberate practice is based on observed changes in target performance. As long as we can anchor the research activity to the palpable performance, everyone involved will be able to assess the value of the ideas and recommended changes. If and when genetic researchers are able to extract information from whole-genome scans, which can reliably and accurately predict an athlete's adult performance, this will be truly exciting, but a very recent report cautions us that this will not happen anytime soon (Roberts et al., in press). I will even be excited when DNA scans are able to predict the adult height and body size of a child, and be able to do so better than conventional methods based on the parents' and the child's current heights. At that time, researchers, aspiring expert performers, and their coaches and teachers will be able to successfully use genetic information, physiological information, and cognitive information to support the quest for expert performance, as well as our understanding of the development of the structure of superior performance and how it can be enhanced by deliberate practice. I also think that parents of children who they feel are born with a definite gift, and researchers, should be encouraged to initiate collaborations to carefully document the engagement in music-related activities and the development of performance along with testing under standardized conditions. This would give researchers the detailed evidence on the emergence of precocious performance, and it would provide the parents with clear evidence that they helped the child develop without any inappropriate pressure to practice.

I want to end my chapter with an example of how collaborations between researchers and expert performers and their coaches and teachers might look. During my work on this chapter, I was intrigued with something that Dorothy DeLay said in an interview with Sand (2000, p. 63): "What fascinates me …is watching somebody come in here and stand in front of that music stand and suddenly discover that he can do something that he didn't think he could do." DeLay then went on to describe an example of one of her students, who claimed that he could not play a given piece as fast as Itzhak Perlman (the world-famous violinist). DeLay and the student measured how fast Perlman played the piece from a recording, and then the student played the piece slowly several times and then gradually increased the speed of the metronome. After working on the piece for 45 minutes, DeLay noted: "I had the metronome in my hand and finally I said, 'Do you want to know how fast you were going?' And I showed him, and he was right on the speed where we had clocked Itzhak" (Sand, 2000, p. 64). If we had had a recording of this interaction or other similarly interesting practice sessions, and also had earlier data on the students and their teachers and opportunities to obtain additional data, such as retrospective reports, we might have been able to dramatically increase our knowledge about how performance at the highest levels can be improved by acquired skills combined with deliberate practice. This knowledge could permit expert performers and their teachers to work with researchers to gain insights into one of the remaining major research frontiers on acquisition of more advanced skills and developing new ideas, which in turn could help any motivated individual to find a path toward improving his or her performance.

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