Shinichi Suzuki and Musical Talent:
An Analysis of His Claims

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Abstract

The Suzuki Method is a popular and influential method of music education for strings. Central to this study is the Method's premise that musical talent is not inborn, but rather cultivated through one's environment. A critical reading of treatises by other influential violin pedagogues revealed that Suzuki's premise was revolutionary. An analysis of Suzuki's claims regarding musical talent showed that some of his claims are valid and some are unsubstantiated by current research.

Suzuki's argument that musical talent is not genetically inherited through a comparison to bird-song is flawed. In particular, important differences between bird song and human music suggest they are non-commensurate. From a story about children raised by wolves, Suzuki argued that human ability is a direct result of the environment and not an inborn predisposition. This story proved to be a fabrication, and while one's environment clearly has an effect on development it is likely not as strong as Suzuki maintains. Suzuki argued that just as being right-handed or left-handed is a result of repetitive use of one hand, so too any skill can be trained through repetition. Though current research has failed to identify the cause of handedness, it has shown that, as Suzuki maintained, dexterity in the non-dominant hand is best trained through repetitive use of that hand, and in an environment with strong motivation to do so. Suzuki claimed that all children learn to speak as a result of their environment. Therefore, if we teach music in the same manner, all children should achieve equal mastery. This is only partially correct. Some children do not learn to speak even when the proper environment is in place. However, speech acquisition and music learning show a number of similarities, supporting Suzuki's idea of using speech acquisition as a model for music education. Suzuki's claim that tone-deafness is not an inborn condition, but rather is caused by a deficient musical environment has not been disproven. Indeed, research has provided support for Suzuki's claim that intensive remedial training can rectify deficient musical perception. Through an examination of Suzuki's foundational claims, this dissertation serves as a foundation for future Suzuki research.

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Introduction

In the early 1930s, Shinichi Suzuki (1898-1998), one of the most influential music pedagogues of the twentieth century, began to develop a unique method to teach music. Originally called "Talent Education," this system is now globally referred to as the "Suzuki Method." Suzuki's method is founded on the idea that musical talent is not inborn, but rather cultivated through one's environment and effort. This dissertation analyzes Suzuki's claims regarding musical talent and assesses their validity. That is, whether recent research has affirmed or contradicted Suzuki's claims.

As recognition of the method's efficacy and influence has grown, so too has the need for research on Suzuki's ideas on music education. Suzuki (2012) suggests that the success of his pupils is proof of his method's efficaciousness. In his book *Nurtured by Love*, Suzuki discusses the substantial musical careers of his original pupils, most notably Toshiya Eto, who became a world-renowned violinist and professor at the Curtis Institute, and Koji Toyoda, who became concertmaster of the Berlin Radio Symphony and a professor of violin at the Berlin Hochschule.

In addition to training world-class virtuosi, Suzuki (2012) further asserts the success of his method by discussing the application of his theories to teach a blind boy, Teiichi Tanaka, to play the violin. After only a short time studying with Suzuki, Tanaka was able to play a Seitz concerto. Additionally, Suzuki cited the case of Hiroko Chan as evidence of the depth of his method. Chan, a child considered slow at everything, trained with Suzuki and eventually became the first woman member of the Berlin Radio Symphony. Perhaps the most extreme reported example of the method's capacity is found in an interview of Suzuki by Susan Grilli (1991). In the interview, Suzuki claimed he was able to teach a mentally disabled boy to play one of the most difficult pieces in the violin repertoire, the Tchaikovsky Violin Concerto, by the time he was 18 years old.

Suzuki's global influence can be traced back to 1964 when Suzuki took a group of ten students, ranging from age five to thirteen, on a tour of the United States. These students performed for a standing-room-only audience at the national convention of the Music Educators National Conference in Philadelphia. Evelyn Hermann (1981) noted that over 5,000 North American teachers watched in awe as Suzuki made them question long-held beliefs in string education. Suzuki attracted a large amount of attention during this tour and a revolution in North American music education began to take root.

The Suzuki Method's influence was furthered by the recognition of some of the most prominent music pedagogues of the 20th century. Alfred Garson (2001) noted that Pablo Casals, Dorothy Delay, Ivan Galamian, Joseph Gingold, Arthur Grumiaux, William Primrose, and Janos Starker all praised the Suzuki Method.

Today, there is a Suzuki Association of the Americas (SAA) that consists of "a coalition of teachers, parents, educators, and others who are interested in making music education available to all children" (See http://www.suzukiassociation.org/about/). In 2014, the Suzuki Association of the Americas (SAA) reported a membership totaling 8,364, an increase of 13% over the previous 5 years. Over 100 "Every Child Can" courses, the prerequisite for any SAA-approved teacher training, were offered. In 2012, 2,312 teachers enrolled in 541 teacher training courses, an 8% increase in enrolment from 2011. In 2013, there were 64 Suzuki Summer Institutes modelled after Suzuki's summer program in Matsumoto. Institutes took place in 34 states of the United States and six Canadian provinces. The demand for Suzuki teachers continues to grow as well. The on-line teacher locator service provided by the SAA, averages 3,700 searches per month ("Organizational News," 2014).

While these statistics show that the Suzuki Method has become popular, it should be noted that they only refer to the SAA. Suzuki's influence is actually far greater. There are many more Suzuki teachers who are either not members of the SAA, or teachers who do not call themselves "Suzuki teachers," but incorporate aspects of the Suzuki method into their teaching. For example, the now highly popular *El Sistema* program that has emerged from Venezuela was highly influenced by the Suzuki Method. This influence was facilitated through visits to Venezuela by Hiroko Driver Lipman and William Starr, two prominent Suzuki teachers (Brasch, 2014).

Considering the reported success and influence of the Suzuki Method, it is surprising that there is little research on the method itself. The majority of publications regarding the Suzuki Method are historical, anecdotal or biographical, or are discussions of specific pedagogical points. There is a small body of literature in which the Suzuki Method is the subject of critical research. However, as Robert Dawley (1979) noted:

> Many of the conclusions reached by both Suzuki advocates and detractors were based upon personal experience, misinformation or lack of information, conjecture, personal bias, and/or little or no controlled research (p. 62).

Over 30 years later, the state of Suzuki research is largely the same.

Nancy Mitchell and Elizabeth Guerriero (2014) compiled a bibliography of Suzuki Method research. From 1966-2013 they found only 74 studies. Of these 74 studies, 52 (70.3%) are dissertations (undergraduate, masters, and doctoral) rather than articles published in journals. This suggests that Suzuki Method research has not become a major topic in the field of music education but rather is of interest to occasional researchers. Furthermore, of the 74 studies, Mitchell and Guerriero only classify 13 studies as being empirical (they do not list which studies, only the quantity). Lastly, of these 74 studies, none of the studies attempt to determine whether the foundational claims behind the Suzuki Method are valid.

This dissertation seeks to begin to fill the void of research on the Suzuki Method. As the Suzuki Method is founded on the idea that musical talent is not inborn, investigating the validity of Suzuki's claims regarding musical talent is arguably the best place to begin research into the method. I have thus taken on this task in this dissertation.

In the first three chapters, I provide the reader with background information. The first chapter presents a brief biography of Suzuki and history of the method, with particular attention paid to the method's rise in popularity in North America. The second chapter provides a systematic description of the method through a critical reading of Suzuki's most influential text, *Nurtured by Love* (2012). This analysis is essential because Suzuki did not leave a systematic description of the method, but rather presented his ideas by means of anecdotes. In the third chapter I provide a description of Suzuki's goals in creating the method. Suzuki was not interested in training professional musicians, but rather in creating "noble people" through the study of music. Like the method itself, Suzuki discusses his goals in a non-systematic fashion. Through a critical reading of *Nurtured by Love,* I attempt to determine what Suzuki means when he uses the term "noble." *Nurtured by Love* is used as the basis for these two chapters as this is the book with which Suzuki intended to spread his ideas globally (see chapter 2).

In chapter 4 I summarize Suzuki's five sub-claims to his foundational claim that musical talent is not inborn. These sub-claims are as follow:

1. Musical talent is not a result of genetic inheritance.
2. One's environment, and not inborn predisposition, determines one's abilities.
3. Any skill can be trained with repetition.

4. If one replicates the way in which children learn to speak, one can train any skill to all children successfully.

5. A lack of musical talent, often referred to as tone-deafness, is not an inborn condition, but rather the result of a poor musical environment.

Additionally, through the reading of influential treatises by four prominent violin pedagogues, I consider whether Suzuki was revolutionary in his claims.

The fifth chapter assesses Suzuki's claim that musical talent is not the result of genetic inheritance. Suzuki argued this claim through a comparison of human music-learning to bird song-learning. I investigate the possibility that bird song-learning does not involve inherited elements. I also investigate whether it is valid to compare bird song-learning to human music-learning.

In the sixth chapter I assess Suzuki's claim that it is one's environment that creates ability rather than inborn predisposition. Suzuki argued this claim through a story of two children raised by wolves. I first determine whether the story was a fabrication, and whether there is reason to believe that the two children, who were thought to have been raised by wolves, developed exceptionally as a result of their environment.

In the seventh chapter I discuss Suzuki's claim that any activity can be trained through repetition. In particular, I explore Suzuki's argument that handedness, whether one is right- or left-handed, is the result of repetitive use of the hand or a result of an inborn preference. I further explore whether ambidexterity training is possible and, as Suzuki advocates, beneficial.

In the eighth and ninth chapters I examine Suzuki's mother-tongue argument; namely, that any child can learn any skill if they are taught in the same way by which they acquire speech abilities in their native language. Chapter eight focuses on Suzuki's claim that every child develops normal speech abilities if the correct environment is in place. Thereupon, this chapter tries to identify correlates between the Suzuki Method and elements of speech acquisition. In chapter nine, I evaluate evidence that music learning and speech learning are similar, and whether skill in one domain can be generalized to the other. In this way, I assess Suzuki's idea that speech acquisition can be replicated in music education.

In the remaining chapters I determine the validity of Suzuki's claim that one cannot be born with a lack of musical talent; that is, what is commonly referred to as tone-deafness. In chapter 10 I begin my investigation of this claim through an analysis of an influential study of tone-deafness by Hans Kalmus and Dennis Butler Fry (1980). This article was selected for special consideration as it is the first large-scale empirical study of tone-deafness. Furthermore, the article sets the foundation for much of the more recent research on tone-deafness.

In chapter 11, I compare Kalmus and Fry's test for tone-deafness with a more recent test, the Montreal Battery for the Evaluation of Amusia (MBEA) (Peretz, et al., 2003). In particular, I focus on how Suzuki's description of tone-deafness is similar to the underlying definitions of tone-deafness in these two tests. I further assess each test's strengths and weaknesses in diagnosing tone-deafness as Suzuki understood it.

In chapter 12, I present a critical review of case studies that employ the MBEA. I evaluate evidence presented in these case studies that suggests that Suzuki was incorrect. That is, I determine whether the evidence supports the conclusion that tone-deafness is an inborn condition. I then investigate, through earlier studies in which interventions were utilized for those deemed tone-deaf, whether there is evidence that Suzuki was correct in his belief that tone-deafness is actually the result of a musically deficient environment.

Through these 12 chapters, I seek to determine the validity of Suzuki's claims regarding musical talent. By doing so, I aim to reach a better understanding of the theory behind Suzuki's method of music education, and, hence, an important aspect of the theory's validity. I also hope this dissertation serves as a basis for further research into the Suzuki Method.

Chapter 1

A Brief History of the Suzuki Method

Shinichi Suzuki developed his method in a small town in the mountains of Japan. It is therefore surprising that the method became influential and achieved recognition on a global scale. In this chapter I trace the history of the Suzuki method and how it came to influence the global music education community. In tracing the history of the method, I summarize information presented by Suzuki's biographers, Masaaki Honda (1984) and Evelyn Hermann (1981). I also utilize autobiographical content found in Suzuki's most influential work, *Nurtured by Love* (2012). Particular attention is paid to the development of the Suzuki Method in North America.

Suzuki's Parents

Suzuki's mother, Ryo Fujie, was born into a wealthy samurai family that provided her with a thorough education. She attended a singing school and studied Japanese songs, samisen playing (a three-stringed Japanese instrument), and various other Japanese arts. At 18 years of age, she married Masakichi Suzuki.

Masakichi Suzuki, also a descendent of a samurai family, was the son of a skilled samisen maker. At the age of 10, Masakichi was sent to study at an English school, and at 14 he began to build samisens for his father. When he was 28, Masakichi returned to school to study to become an English teacher, and while there he was introduced to the violin. Though not unheard of in Japan, the violin was quite rare, particularly in Nagoya where Masakichi lived. While Hermann and Shinichi Suzuki claim this violin belonged to an instructor at Masakichi's school, Honda maintains that the violin was owned by a fellow student, Tetsukichi Amari. Nevertheless, all three agree that Masakichi borrowed the violin to examine its construction in order to build one for himself. In 1888, Masakichi built the first Japanese-produced violin.

In 1889, Masakichi took his violin to Tokyo to show it to Shuji Izawa, the head of the Tokyo Music School at Ueno, and Professor Dietrich, a visiting German violinist. After receiving favourable feedback from the two, Masakichi took his violin to Mr. Shirai, the president of Kyokido of Ginza, a branch of Yamaha. He was given a contract to build 10 violins per month. Demand for Masakichi's violins increased so he decided to build a violin factory. By 1910 the factory was the largest in the world, producing 65,800 violins per year. By 1918, the factory employed 1,200 workers and manufactured 500 violins daily.

Suzuki's Musical Training

Shinichi Suzuki, born in 1898, was the third son in a family that would eventually total seven sons and five daughters. Though he grew up working in his father's factory, Shinichi never attempted to play the violin himself. As a teenager, he attended a commercial school, ultimately planning to work in a managerial role at his father's factory. Upon graduating from the commercial school, Shinichi was given a phonograph player. At 18 years of age, Shinichi bought his first record, a recording of Mischa Elman playing Schubert's 'Ave Maria,' arranged by Wilhelmj. Moved by what he heard, Shinichi attempted to teach himself to play the violin. He had little success with the Schubert piece so he purchased another recording by Elman, this time a Haydn Minuet. With the minuet, he had more success. Though Shinichi's violin playing had just begun, his brother, Umeo, had already been studying for some time and violin playing was already well established in the Suzuki family home.

In 1918, Suzuki fell ill with capillary bronchitis and was prescribed an extended period of rest away from home. He spent three months recovering in Okitsu where he befriended Ichiro Yanagida and his family. Suzuki then returned to his family home in Nagoya where he planned to continue his recovery before returning to work at his father's factory. In June of 1919, Yanagida invited Suzuki to join him on an expedition to the Kurile Islands. The expedition included Yanagida's childhood friend, Marquis Yoshichika Tokugawa, as well as numerous researchers from the Tokugawa Biology Research Center. Also joining the expedition was Nobuko Koda, of the Tokyo Music School. Koda's brother, a Lieutenant in the Navy, was famed for his explorations of the northern sea. Koda hoped the expedition would take her to places her renowned brother had travelled. As the only two lay people on board the expedition, Koda and Suzuki spent a fair amount of time together, often playing music.

After the trip to the Kuriles, Koda arranged for Suzuki to study with her sister Ko Ando in Tokyo. Ando, a violin professor at the Tokyo Music School, had studied abroad with Joseph Joachim from 1892 to 1903. Ando then returned to Japan and became one of the pre-eminent violin pedagogues in the country. During his studies in Tokyo, Suzuki lived in Tokugawa's home.

After two years of study with Ando, Tokugawa invited Suzuki to join him on a trip around the world. Tempted to go, but reluctant to end his violin studies, Suzuki agreed to join Tokugawa as far as Berlin, where he would remain to study. While the Japanese presence in the German music scene was small, there were at least three other Japanese musicians studying in Germany at the time, namely, Shigeru Nobutoki, Kenzo Sato, and Tamezo Narita.

For eight years Suzuki remained in Berlin, studying with Karl Klingler.[1] Klingler, like Ando, was also a student of Joachim. He held a position in Joachim's string quartet, and later served as first violinist of the Klingler Quartet. He also took over Joachim's position as violin professor at the Staatliche Hochschule für Musik Berlin. Carl Flesch (1924) credits Karl Klingler with revolutionizing violin pedagogy by including interpretive elements in his pedagogical writings. According to Flesch, previous pedagogues wrote only on topics related to technical problems. Klingler was the first to write on artistry. Perhaps this "breaking the spell," (p. 3) as Flesch refers to it, served as a starting point for the development of Suzuki's theory. The belief that musical ideas or artistry could be taught rather than being something inborn for a select talented few, was a concept Suzuki took further than Klingler. Suzuki later claimed that all cultural aptitude was created through environmental influence and not through inborn talent.

Suzuki Marries and Returns to Japan

While attending a concert, Suzuki met Waltraud Prange. Suzuki became a frequent visitor at the Prange home, and was often joined by his brother, Fumiyo, who was studying cello in Leipzig. Waltraud and her brother and sister were also trained musicians and Shinichi and Fumiyo would often play chamber music with them. Because Fumiyo was in Leipzig, Suzuki decided to commute weekly to Leipzig where he studied composition with Georg Schumann. At the same time, Suzuki and Waltraud began to discuss marriage. Waltraud's extended family objected to her marrying a Japanese man, and Suzuki's own father was also hesitant about the arrangement. Suzuki's family sent his brother Umeo to meet Waltraud and her family and assess the relationship. Despite reservations on both sides, on February 8, 1928, Suzuki and Waltraud

[1] Mark O' Connor has questioned whether Suzuki studied with Klingler. Alice Schoenfeld, a known pupil of Klingler has confirmed that Suzuki in fact studied with Klingler. For more about this debate see Cooper, 2014.

wed. They left the church ceremony to a violin rendition of Schubert's "Ave Maria," the piece that had inspired Suzuki to study the violin.

Shortly following his wedding, Suzuki received news from his father that his mother was ill. Suzuki and Waltraud thus left Berlin for Japan. Upon arrival, Suzuki began to perform as a soloist. In 1929 he formed a quartet together with his brothers Akira, Fumiyo and Kikuo. The four brothers performed extensively throughout Japan and played weekly on the radio. In 1931, Suzuki co-founded the Teikoku Ongaku Gakko (The Imperial Music School). Though 'Teikoku' ('Imperial') was part of the school's name, it was not a state-run institution. The school included faculty in voice, cello, theory, and violin. Most notably, Alexander Mogilevsky was a member of the violin faculty. Mogilevsky had come from Moscow to Japan in 1927. While running the Teikoku Ongaku Gakko, Suzuki also taught at the Kunitachi Music Academy.

The Suzuki Method Begins to Develop

It was while teaching in Tokyo that Suzuki began to accept young students and to develop a foundation for the Suzuki Method. When four-year-old Toshiya Eto arrived at Suzuki's studio with his father, Suzuki was hesitant. Though uncertain about teaching such a young student, Suzuki agreed to consider accepting Toshiya into his studio. After ruminating over how to teach such a young child the violin, Suzuki concluded that children easily learn their native language with little instruction. He concluded that the method by which children learn their native tongue was a perfect method of education. He thus attempted to replicate the process by which children acquire speaking abilities in his method of music education:

> Children freely speak Japanese, I realized, because they are, in effect, given the opportunity to do so. There is the fact of training and education behind their capacity to speak. Therein lay a proper educational method. Every child, without question, is developing

> appropriately. This was precisely that perfect educational method I was seeking (2012 p. 7).

The idea of replicating the process of language acquisition in children in order to teach them other skills became the foundation for what Suzuki termed “Talent Education” and what is now commonly referred to as the “Suzuki Method.”

Suzuki during World War II

Shortly before World War II, due to strained relations with Western Europe, Western culture lost popularity in Japan. Thus operating the Teikoku Ongaku Gakko became increasingly difficult. In 1941, in the midst of World War II, the school closed. Also a result of the war, Suzuki’s father converted his violin factory to build pontoons for sea planes. The factory experienced a shortage of wood, and Suzuki agreed to move to Kiso-Fukushima to facilitate the acquisition of more lumber.

Following the war, Suzuki received news that a young student of his, Koji Toyoda, lost both of his parents as a result of bombings in Tokyo. Toyoda and his parents had moved to Tokyo to study with Suzuki and Suzuki thus felt some responsibility for the family’s tragic loss. Suzuki found Toyoda living with an uncle who operated a sake bar. Suzuki and the uncle agreed that this was not a suitable environment for a young boy, and Toyoda moved to the Suzuki family home in Kiso-Fukushima. Toyoda became a part of the family and remained with them until he left for Paris to study at the Paris Conservatoire. Toyoda became one of Suzuki’s most renowned students. He served as concertmaster of the Berlin Radio Symphony from 1962-1979. Following Suzuki’s death, Toyoda became president of the Talent Education Research Institute, Suzuki's school in Matsumoto. Toyoda remained president until 2008, and is currently teaching there.

Suzuki Moves to Matsumoto and the Method Gains Popularity

In 1945, Kuniji Kajikura, a young journalist from Matsumoto, arrived in Kiso-Fukushima. He approached Suzuki with an offer to join the faculty at a new music school in development in Matsumoto. Among those developing the school was Tamiki Mori, a voice teacher who had taught with Suzuki at the Teikoku Onagaku Gakko. Suzuki agreed to join the faculty of the newly formed school, but only on condition that he would teach young beginner students rather than older advanced students. By the end of the year Suzuki and Waltraud, together with the rest of the Suzuki family, had relocated to Matsumoto.

In 1946 Suzuki published his first book, *Young Children's Talent Education and Its Method*, and his influence on music education began to spread across Japan. (This work was translated by Kyoko Selden in 1996). Suzuki received numerous speaking invitations throughout the country and gradually Talent Education chapters were formed. Honda (1984) suggests that the method became popular in Japan because it provided a sense of hope to those devastated by the Second World War. On October 26, 1950, Talent Education became a recognized corporation of the Japanese Ministry of Education. Shortly thereafter, ten volumes of the *Suzuki Violin School* were published by Zen-On Music Company in Japan.

By 1955, there were over 65 Talent Education chapters in Japan, teaching over 4,000 students. It was at this point that Suzuki held the first Annual Talent Education Concert, at the Tokyo Gymnasium. 1,200 children performed at the concert whose program included a Vivaldi

Concerto, and the Bach Concerto for Two Violins, all performed from memory. Particularly impressive were the following observations:

> There was not a poor left-hand position or bow arm visible in the entire group. Intonation was good and pleasing tone was modulated expressively. In short, this was not just mass playing of 1,200 children from five to thirteen years of age – it was *good violin playing* (Hermann, 1981 p. 38).

The Suzuki Method Arrives in North America

Kenji Mochizuki, a member of the staff at the Japanese consulate in New York, obtained a video of this concert. Mochizuki was a violinist himself and had encountered Suzuki's teaching while still in Japan. Mochizuki had also been a student at Oberlin College where he had played in the College Community String Festival Orchestra. Mochizuki passed on a copy of the video to Clifford Cook, a violin professor at Oberlin and the director of the orchestra at Oberlin in which Mochizuki had played. Mochizuki hoped that Cook would show the film at a meeting of the Ohio String Teachers Association, scheduled for May of 1958. Cook watched the video with John Kendall, a professor at Muskingum College in Ohio, and Robert Klotman, the chairman of the American String Teachers Association. Impressed with the level of playing, but still skeptical, Kendall decided to visit Suzuki in Japan to observe his teaching. (Kendall, n. d.)

In the summer of 1959, with a grant from the Bok and Presser Foundations, Kendall travelled to Japan to study with Suzuki. Following his trip, Kendall was interviewed by the foreign correspondent for *Time Magazine*. In 1960, Kendall published *Listen and Play*, an English language version of the first two volumes of the *Suzuki Violin School*. (It should be noted that while Kendall is credited as the one who first brought the Suzuki Method to the Western Hemisphere, he was not the first American to study with Suzuki. The first American was actually Jacqueline Corina (Hermann, 1981)).

Several other North American teachers followed Kendall to study with Suzuki in Japan. In 1963 Clifford Cook, the aforementioned Oberlin professor, went to observe Suzuki's teaching. Many American teachers, including Alex Zimmerman, president of the Music Educators National Conference, attended the Fifth International Conference of the International Society for Music Education which was held in Tokyo. As part of the conference, 500 Suzuki students gave a performance.

Following this conference, Robert Klotman, John Kendall, and Clifford Cook made arrangements to bring a group of 10 Suzuki students to tour the United States. The students, aged five to thirteen, and were not chosen for their playing abilities, but for their availability for the two weeks chosen for the tour. The tour group stopped first at the University of Washington in Seattle and then traveled to Chicago. They also performed at the New England Conservatory, the Juilliard School, and the United Nations. The final concert of the tour, on March 15, 1964, was performed in front of 5,000 music educators who had gathered together at the Sheraton Hotel in Philadelphia for the Music Educators National Conference. An article about the concert was published in the March 25th issue of *Newsweek Magazine*. Consequently, the popularity of the Suzuki Method began to spread.

The Suzuki Method Gains Popularity in North America

Workshops aimed at spreading the Suzuki Method began to develop. Most notably, in the summer of 1966, Donald Shelter held a workshop at the Eastman School of Music. The Eastman School provided 50 scholarships, one for a teacher from every state of the United States, to attend. With assistance from the New York State Council of the Arts, Shelter launched "Project SUPER," an acronym for Suzuki, Penfield, Eastman, and Rochester. Project SUPER aimed to determine whether the success of the Suzuki Method in Japan could be duplicated in the United States.

In 1967, a group of 55 teachers from Canada and the United States went to Japan to observe Suzuki's teaching. The group also formed Talent Education U.S.A., which later became the Suzuki Association of the Americas. With so many teachers travelling to work with Suzuki in Japan, the 1967 tour included a dedication of the new Talent Education building in Matsumoto which included facilities to host all of the visiting teachers.

By 1982, the Suzuki Method had developed worldwide recognition, and the International Suzuki Association (ISA) was formed. William Starr, Evelyn Hermann, Doris Preucil, and Yvonne Tai represented the United States on the board of the ISA, and Betty Parker-Jervis and Dorothy Jones represented Canada. Jones and Parker-Jervis were both instrumental in bringing the Suzuki Method to Canada. Dorothy Jones contributed significantly to the Suzuki Method when she developed a Suzuki Method-based early childhood music program in London, Ontario. Though both Jones and Parker-Jervis were among the early Suzuki teachers in Canada, neither was the first.

The Suzuki Method in Canada

The first Canadian to receive training from Suzuki in Japan was Jean Cousineau in 1965. He subsequently developed his own method which used the pedagogical and philosophical foundations of the Suzuki Method, but substituted different repertoire. Also in 1965, Ted McLearon became the first Canadian to attend a Suzuki workshop in North America, held at Oberlin College. However, the most important event in 1965 for Canadian Suzuki history was the founding of a Suzuki program in Edmonton called the Society for Talent Education. Thomas

Rolston, though not a Suzuki teacher himself, was a leading promoter of the method. He thus founded the Society for Talent Education and invited several Japanese teachers to join the faculty. By 1974, the program had taught approximately 600 students, including Rolston's daughter, famed cellist Shauna Rolston (Garson, 2001).

In 1966 Alfred Garson gave a demonstration of Suzuki's teaching at McGill University. Garson had encountered Suzuki's teaching through his own teacher, Joseph Szigeti, and he attended teacher training at Project SUPER. Following the establishment of a Suzuki program at McGill, other programs sprouted up in nearly every major Canadian city. Marian Schreiber and Jerold Gerbrecht founded a school in Vancouver; Georgina Ritter, Kyoko Kawakami, and Allison Sloan developed a program in Calgary; and Doreen Breckman founded a program in Winnipeg. Hamilton, Guelph, London, Kingston, and Quebec City also saw the emergence of Suzuki programs. In Toronto, the first program was developed at Seneca College in 1974. (Garson, 2001). (The program at Seneca College is no longer affiliated with the college but has continued as the North York Suzuki School.)

The Current State of the Suzuki Method in North America

The Suzuki Method continues to grow in popularity. In 2014, the Suzuki Association of the Americas (SAA) reported a membership totaling 8,364, an increase of 13% over the previous 5 years. Over 100 "Every Child Can" courses, the prerequisite for any SAA-approved teacher training, were offered. In 2012, 2,312 teachers enrolled in 541 teacher training courses, an 8% increase in enrolment from 2011. In 2013, there were 64 Suzuki Summer Institutes modelled after Suzuki's summer program in Matsumoto. Institutes took place in 34 states of the United States and six Canadian provinces. The demand for Suzuki teachers continues to grow as well.

The on-line teacher locator service provided by the SAA, averages 3,700 searches per month ("Organizational News," 2014). In the 50 years since the Suzuki Method came to North America, it has developed into one of the most influential music education methods available.

Conclusions

Despite beginning violin study relatively late in his life, Suzuki became an accomplished violinist and pedagogue. His desire to create an innovative approach to violin pedagogy is likely the result of his studies with Karl Klingler. Suzuki's method was developed while teaching young children in Tokyo in the 1930s. The method was further developed in Matsumoto and gained recognition across Japan after World War II. The method reached North America in the 1960s, where it has become an increasingly popular method of music education. In the following two chapters, in order to provide the reader with an understanding of the method, I describe the method and its goals as set out in Suzuki's most influential text *Nurtured by Love*.

Chapter 2

The Suzuki Method

Before undertaking an examination of the foundational claims of the Suzuki Method, a determination of what the method involves is warranted. William Starr, in his book *The Suzuki Violinist,* suggests that Suzuki did not leave "a detailed exposition of his way of teaching because he preferred to work with his teachers in person" (1976, see preface). This makes the task of determining what the Suzuki Method involves difficult. Additionally, Suzuki hosted annual conferences to present new teaching ideas, which suggests that his method was constantly evolving. This further complicates the task of determining exactly what the Suzuki Method involves. Nevertheless, Starr also notes that Suzuki believed that while certain aspects of his method were changeable, there were other aspects that were established and unvarying. It is these aspects that appear consistently in Suzuki's writings, and it is these aspects that warrant critical analysis and empirical research. The aspects of the method that were changing constantly, I believe, were those specific to small points of violin pedagogy.

In addition to William Starr's book, several other books by American Suzuki teachers attempt to systematically present the Suzuki Method (for example, Behrend, 1998; Kreitman, 1998; Landers, 1984). However, these books differ in how they explain the method and what aspects of the method they consider most important. Furthermore, these books are explanations of the Suzuki Method as taught by the authors, and not necessarily a guide to basic principles that are uniformly accepted by Suzuki teachers.

***Nurtured by Love* as a Guide**

The description of the Suzuki Method presented below will be formulated by conducting a critical reading of Suzuki's book *Nurtured by Love* (*Ai ni ikeru* is the Japanese title), originally published in 1966. *Nurtured by Love* will serve as the basis because, while it was not Suzuki's first book, (his first book was *Young Children's Talent Education and its Method,* see Honda, 1984) it is the book Suzuki intended to use to spread his ideas on a global scale (W. Suzuki, 1987). Furthermore, *Nurtured by Love* is recommended reading for anyone taking Suzuki teacher training courses approved by the Suzuki Association of the Americas:

> All teacher training participants are asked to prepare the repertoire to be studied to the point of fluent performance. Memorization is not required but is recommended. Additionally, participants should be conversant with the ideas presented in Dr. Suzuki's *Nurtured by Love* ("Teacher Training," 2014).

Similarly, the European Suzuki Association requires that teachers read *Nurtured by Love* before taking any teacher training courses:

> Preparation
> Candidates must -
> a. Familiarize themselves with the first four books of the Suzuki Repertoire.
> b. Listen daily to the Suzuki recordings.
> c. Read and ponder the implications of 'Nurtured by Love' by Shinichi Suzuki and understand how to translate this into their role as a Suzuki Teacher.
> d. Study all necessary books, music and recordings (European Suzuki Association, 2013).

Thus, *Nurtured by Love* is clearly the text that Suzuki teachers view as being their primary guide for teaching.

In her autobiography, *My Life with Suzuki,* Suzuki's wife Waltraud, discusses her task of translating *Nurtured by Love.* She took on this task in order to make Suzuki's ideas available outside of Japan. However, Evelyn Hermann (1981) noted that Waltraud Suzuki had difficulty translating the work:

> When the American teachers learned of the book they immediately clamored for an English translation for the American parents. Several people were asked, but all said it was too difficult to translate into English because one had to be able to read thousands of Japanese and Chinese characters. In many cases, a character was not just a word, but a whole thought or idea. Realizing the importance of this book to people outside Japan, Mrs. Suzuki was determined to make it available in English. With the help of Mrs. Masako Kobayashi, Mrs. Suzuki was able to understand the Japanese. Then she translated it into English, with the help of Mrs. D. Guyver Britton, whose husband was connected with the British Embassy in Tokyo. Waltraud had to look up almost every Japanese character in the dictionary, and then verify the meaning with Mrs. Kobayashi to be certain she had the correct translation. Once she understood the Japanese, she then had to translate it into English. (Since her mother tongue was German, she was working with two foreign languages.) (p. 51)

Given the above circumstances, it stands to reason, that Waltraud Suzuki's translation may not be accurate. As such, the International Suzuki Association commissioned a new translation by Kyoko and Lili Selden (Suzuki, 2012). Kyoko Selden was an ideal translator as she also translated a number of Suzuki's other texts. Thus, she is not only knowledgeable about the required languages, but also about the ideas contained in the text. Because this translation is likely more accurate, it is the one which I have, for the most part, selected to use.

One of the difficulties one encounters with *Nurtured by Love* is that it is not written in a systematic fashion. Suzuki does not provide a detailed step by step process of how to teach; rather the book is written as a series of stories and thoughts. It is left to the reader to determine what exactly the method is and how to put the method into practice.

I believe Suzuki did this intentionally as he believed that education does not work only by instruction, that is telling someone what to do, but by inculcating what is to be learned into the students' daily existence. As Suzuki writes:

> Nine years should be adequate for fostering in every child, at minimum, the splendor of having acquired at least one notable ability. This ability does not even have to be in an academic subject. If, for example, education designed to foster a mindset and behavior based on “kindness to others” was carried out in everyday life at school, amidst friends, and at home, what a pleasant society Japan could create. However, the educational system today merely teaches children that they should “be kind to others.” It therefore produces intellectuals who full well know that they should “be kind to others,” but who are, in reality, fostered to become nothing more than unfortunate egoists. The current state of society is borne of this sort of education (2012 p. 111).

Suzuki’s ideal type of education may be possible in a school or classroom where a teacher can teach by demonstration or teach a certain concept at a specific moment when that concept is applicable. However, in a book, this is more difficult. The author of a book can do little but write how to teach. Suzuki, I believe, attempted to solve this problem by writing specific stories that led to his thinking, or stories about specific students when a certain strategy was used at a specific time. In this way the reader sees the method in practice rather than just being told what to do. The reader is thus inculcated with the method rather than instructed in the method and Suzuki clearly sees this as a superior approach to education.

Suzuki’s approach to writing an education manual in this manner is interesting and whether it is effective in training teachers is a question that warrants further study. To the scholar or researcher, however, it is problematic. If one seeks to test or critically analyze the method presented in *Nurtured by Love* one must first read the book carefully and decipher what his method is.

In this chapter I describe the Suzuki Method as presented in *Nurtured by Love.* It is important to note that this systematic presentation will differ from others available such as those by Starr (1976), Behrend (1998), Kreitman (1998), and Ray Landers (1995). This is because I have used only *Nurtured by Love* as a source. I have additionally provided key words with references to outside research to assist those who wish to research the method further.

Ten Principles of the Method

1. **Start at Birth.** Suzuki believed in beginning a child's education from birth. His students often began study at the age of five or six, or even earlier. In fact, Suzuki believed that education should really begin in infancy. (In education research, the education of children from birth to age eight is referred to as "early childhood education." See Bowman, 1993) Suzuki compares the education of a child to that of a warbler learning to sing. According to Suzuki, warblers are taken from the nest while they are still fledglings and training begins right away. If the warbler begins training too late, its singing ability does not develop. Furthermore, Suzuki believed that training has a greater effect when done with young children:

> Without training, intuition does not develop. People only *think* that intuition is inborn. If intuition unexpectedly reveals itself, however, it is because unconscious training has been amassed somewhere along the way. To cultivate intuition for a skill—the only way to achieve this is to train in earnest. Certainly, the way intuition behaves differs between someone who has trained from early childhood and someone who has not (p. 67).

This concept, that there is a time in one's life in which development may be more affected by stimuli than at other times, is known as the "critical period" in developmental psychology research (see Colombo, 1982 for a review).

While Suzuki argued for early childhood education, he does not suggest that one who has not started their education until later in life should not begin. Suzuki himself did not begin studying the violin until he had completed commercial school in his late teens. Rather Suzuki believed that one who experienced no training or poor training should do everything in their power to correct it:

> I count myself among those humans who were improperly nurtured. Many people have been. From my youth onward, I have continually strived to rehabilitate my flawed self, and thereby overcome my failings. The result is the person I am today (p. 142).

2. **Creating the Environment.** An infant, for obvious reasons, is not able to begin formal violin training. However, the correct environment is to be set up as early as possible. This is done at first, by regularly playing recordings of master musicians for the infant. Later in the student's education, the parent continues the environmental stimulation by having the student listen daily to the piece he or she is working on, as well as pieces the student will be working on in the near future. Additionally, the parent is instructed to continue to play other recordings by master performers.

Suzuki writes of the great success of this aspect of his teaching in the following anecdote:

> Just before me was a baby nestled in Mrs. Kiuchi's arms. On questioning I learned that the infant was named Hiromi, and that she was five months old.
> At the time, Hiromi's older sister, Atsumi, age six, was daily practicing Vivaldi's *Concerto in A minor,* as well as listening to a recording of the concerto… I pulled out my violin, and when everyone had quieted down I began playing a Bach minuet. I kept my eyes glued to Hiromi the entire time I played.
> The five-month-old, who already knew the sound of the violin, responded with alertly shining eyes to this piece she was hearing for the first time ever. Several phrases into the piece, however, I segued without break into the Vivaldi A-minor, which her older sister, Atsumi, was practicing every day. In that moment, before I had even finished playing the first measure of the Vivaldi an amazing thing occurred.
> Hiromi's expression suddenly changed; she flashed a momentary smile, and then, truly joyfully, she turned around to look at her mother holding her.
> She seemed to say, "Oh, it's that piece I know!"…

> Five months after birth, she knew the Vivaldi concerto and its melody inside out. In effect, young children adapt to their environment, unconsciously absorbing what they see and hear as they grow, and in this manner forming their personalities—I sensed that I was observing a strikingly vivid example of this process. It is an almost terrifying fact, one that is emphatically not limited to spoken language or music.
> Four years passed since then. It was a concert in Matsumoto. On stage were 150 young children beautifully playing in unison on their pint-sized violins. The piece was Vivaldi's *Concerto in A minor.*
> "Who is that four- or five-year-old," I was irresistibly compelled to ask the instructor leading the class, "playing at the center of the front row?" The child was playing with so much enthusiasm that I had opened my mouth before I knew it. With excellent posture, and with overflowing joy expressed in every fiber of her being, she was playing the concerto with great verve.
> "That's Kiuchi Hiromi from Ueda city."
> "Oh, so that's her…that child…No wonder, I see, no wonder." (p. 12)

Suzuki's idea that an infant learns things from its environment, and this knowledge cannot be demonstrated until later in life, is known as "latent learning" in psychology research (see Campanella and Royee-Collier, 2005).

In addition to listening, parents create a better environment by demonstration. For example, if a student has a bad habit, Suzuki felt scolding the child was ineffective. He believed instead that the parent should increase their efforts to demonstrate to the child the correct behavior. This form of teaching by demonstration Suzuki felt was more effective. It is for this reason that Suzuki encouraged students and parents to associate with fine people, meaning people who exhibit traits which parents wish their children to emulate. The idea that one learns a behavior from observing another person performing it, is known as "modeling" in psychology research (see Walker and Gersham, 2003).

3. **Parent Training.** Suzuki insisted that the parents of his students learn to play the violin before he would begin instruction with the child. He would have the parent learn at least the first

piece in the Suzuki repertoire, “Variations on Twinkle Twinkle Little Star.” This was done for two reasons. First, the parent would serve as a home teacher. In order for the parent to guide the child to play with proper posture and to develop good practising habits, Suzuki felt the parent must have first-hand experience. Having a home teacher is particularly important because Suzuki felt that children should have the violin around all the time and practice for many short periods. This is not possible if one must always go to a teacher in order to play. However, if the parent serves as a teacher at home, these short periods can be utilized many times a day.

The second reason Suzuki had the parent learn to play the instrument was that it helps develop motivation. Suzuki felt that you must “First foster the heart, then help the child acquire the ability” (2012, p. 125). That is, Suzuki felt that one must create in the child a desire to play. This is why Suzuki had the parent play on a small-size violin, the same size the child would play on. This way the child, in an effort to emulate the parent, would come to take the violin away and say, “I want to play too” (p. 123).

4. **Observation.** Before Suzuki would begin to teach a student, that student would attend someone else’s lessons and observe. Similar to the way in which the child develops a desire to learn by watching the parent play the violin, the desire to play is also created by having the child watch other children play. If the other children are having fun, the new student will want to join in.

> Thus what is necessary is to lead the child to wish spontaneously, “I want to try that too!” So, at home, have the child listen to a recording of beginner pieces every day, and at the lesson, place the child among other children who are playing the violin—the point is to create that kind of environment for the child…. As a natural result, the child begins to wish to take the instrument from his mother so that he, too, can “have fun:”

> "I already know the melody. Other children around me are already playing. I want to play (have fun), too." Such a desire gradually germinates in him (p. 123).

This idea, that a child will watch other children and want to do what they are doing, is referred to as "conformity" in psychology research (Cialdini and Goldstein, 2004). Furthermore, by having watched the parent at home and the other children in lessons, the child will already know much of what to do when it is the child's turn to take the lesson.

5. **Perseverance and patience.** According to Suzuki, the training of young children takes tremendous patience. Things must be repeated many times and must be done regularly with few results. Suzuki compares the teaching of young children to the teaching of a parakeet to speak. The first word a parakeet learns must be repeated 3,000 times daily. However, with time the learning gets easier. The next word the parakeet speaks may take only 200 times. The next word the parakeet learns will take even fewer times. Suzuki argues that this process is the same with children. The early stages of learning may take many repetitions. But as the child advances, the learning of new concepts, technique, or repertoire will come more easily. Suzuki writes that learning the first piece of the method's repertoire, "Variations on Twinkle, Twinkle, Little Star," can require a long period of time. However, as the child progresses the learning of new pieces and skills moves faster. Later on, students can learn a difficult piece, such as a Vivaldi concerto, in only one day. Suzuki believed that this is because "ability begets ability" (p. 11). Thus, Suzuki suggests that learning through his method is exponential rather than additive. The idea that one might learn musical concepts or pieces with fewer repetitions after having learned many concepts or pieces already is referred to as "generalization" in psychology research (Wright and Zhang, 2009). By learning more musical concepts or pieces, one can utilize the similarities

between old concepts and pieces and new concepts and pieces, and thus reduce the amount of new material to be learned. Therefore, the new concept or piece will take fewer repetitions to learn.

6. **Motion Exercises.** Before the student began using an instrument Suzuki had the students play games in which physical motions necessary to play the violin were mimicked. (For a longer explanation of Suzuki's motion exercises see Garson, 1973). Suzuki also used these games as warm-up exercises before rehearsals and concerts. The usefulness of practicing motions necessary to play instruments without the instruments at hand has not been studied extensively. In the few articles in which this concept has been researched, it is usually referred to as playing "air-instruments" (Godoy et al., 2006).

7. **Repetition of Skills.** Repetition is perhaps the most discussed aspect of the Suzuki Method in *Nurtured by Love* (Ebin, in press). Suzuki advised, "If you have learned to do something, be sure to repeat it thoroughly" (p. 47). Suzuki felt that repeating actions makes the ability to do these actions part of oneself. Through repetition *Kan* is developed. *Kan*, which Selden translates loosely as "intuition," (p. 64) is essential to mastering a skill. As Suzuki writes, "I have discovered on multiple occasions that if we repeat something until it is absorbed and becomes habitual, what we first thought impossible proves itself possible, and formerly closed paths begin to open up" (p. 118). The number of repetitions necessary varies; some may need 500 times and some 5,000. However, this variation in required repetition, according to Suzuki, is dependent on one's prior experiences in the skill or related skills, and not on an innate predisposition.

When discussing repetition, Suzuki presents a two stage process. First, one must be able to execute the skill and then one must repeat the skill many times so that it becomes part of oneself or natural. This is similar to Schneider and Shiffrin's (1977) often cited concepts of automatic and controlled processing. Schneider and Shiffrin define automatic processing as a "learned sequence of elements in long-term memory that is initiated by appropriate inputs and then proceeds automatically—without subject control, without stressing the capacity limitations of the system, and without necessarily demanding attention" (p. 1). Controlled processing is defined as "a temporary activation of a sequence of elements that can be set up quickly and easily but requires attention, is capacity-limited (usually serial in nature), and is controlled by the subject" (p. 1). Changing a skill from being a controlled process to an automatic process, Schneider and Shiffrin state, "requires an appreciable amount of consistent training to develop fully" (p.2). An automatic process is so ingrained in oneself that, "once learned, an automatic process is difficult to suppress, to modify, or to ignore" (p. 3).

8. **Repetition of Pieces**. (Many Suzuki teachers refer to this concept as "review," see Kreitman, 1998 pp. 81-90) Many non-Suzuki teachers assign a piece to a student, and once that piece is learned, many more follow in succession. Suzuki felt this practice was not effective. Rather he suggests that once a piece is learned, it must be repeated again and again. Suzuki believed that by doing so the piece is mastered, or as he writes, "polished." (Suzuki, 1983 p. 44 and 75. I have used Waltraud's translation here because the term "polished piece" has become part of the vernacular used at Suzuki institutes and schools. This term does not appear in the Selden translation.) Over the course of time, the student will be able to perform the needed actions to play the piece even when one is not consciously focusing on those actions. Suzuki points out

that Japanese people "…all have the ability to handle a variety of tasks while speaking Japanese without mishap" (p. 128). The ability to perform an action while one is not focusing on that action is often referred to as "automaticity" in psychology research (Palmeri, 1997).

To test whether students could play their pieces automatically, Suzuki would play games with them while they played their pieces. For example, he would have the students play and he would ask them unrelated questions, such as "how many legs do you have?" (p. 128). If the students could answer without stopping or hesitating in their playing, Suzuki knew the skill of playing the piece had been inculcated. As students get more advanced, so do the games. If students can successfully play these games it means their repertoire is at the point where they can use it to further develop their playing. When children continue to play pieces even after they succeed in these games Suzuki writes, "Their tone gradually improves, their movements grow ever more fluid and dynamic, and their performances become more and more musical. Ability, in other words, develops" (p.127). Testing someone's ability to perform a task with minimal attentional resources by having them simultaneously perform another task, as Suzuki does by having students play his games while playing their pieces, is known in psychology research as "Dual-Task Methodology" (Vorberg and Wing, 1996).

9. **Memorization.** Suzuki had all of his students play from memory. As he writes, "…developing the capacity to memorize is one of the pedagogical areas in which I place the most emphasis. The printed music is nothing but a reference for memorization" (p. 39). Suzuki felt that memorization is a trainable skill to the point that students can eventually memorize something they have heard only once. In Suzuki's school in Japan, a kindergarten was formed in which part of the curriculum was memorizing haiku. In each trimester, the students would

memorize approximately 50 haiku. In the first trimester it is common for students to need more than ten recitations of the haiku to facilitate memorization. In the second trimester, the students can memorize the haiku after three or four recitations, and in the last trimester, the students can memorize the haiku with just one recitation. At the end of each trimester, the students would recite all of the haiku they had learned from memory.

Suzuki writes that it was Daisetsu Suzuki's writing that influenced him to emphasize the ability to memorize. Suzuki quotes Daisetsu Suzuki in *Nurtured by Love* as follows:

> Among the characteristics of human existence is the fact that we experience life… This is because human beings possess such a thing as memory. Memory is tremendously important, for it is the source of human contemplation and creative thought. As long as human beings have memory, experience is possible, and if experience is possible, there will surely be a path for gradual advancement… Memory serves as the basis of experience, and it is because experience exists, one can say, that humans are able to fulfill the reason they are human (p. 120).

What Suzuki means by this is that experiences are only valuable if they are remembered. With memory we can draw on our past experiences because these experiences are remembered and thus build our training. For example, if one has memorized a piece, and that piece is now in one's memory, one can use that piece to help learn others. When the new piece has a passage or technique that is similar to the memorized one, one simply employs the same process in the new piece as was used in the memorized piece. Suzuki thus argues that in order to facilitate generalization, one's memory must be well developed.   The idea that one uses remembered knowledge to understand new situations is much discussed in developmental psychology research. The structures by which people use and organize past knowledge to understand new ideas or situations are called "schemata" in developmental psychology research (Brewer and Treyens, 1981).

As stated above, Suzuki writes that it was Daisetsu Suzuki's book *Zen to wa nani ka,* that influenced him with regard to memorization training. However, I believe his emphasis on memory training also has roots in Tolstoy. Suzuki was greatly influenced by Tolstoy and regularly carried a copy of Tolstoy's diary with him. In Tolstoy's diary there is a section on memory training including how it is achieved (Tolstoy, 1985). Like Suzuki, Tolstoy believed one should learn everything one is studying "off by heart" (p. 14). Similarly Tolstoy stresses repetition of things one has learned just as Suzuki had students constantly repeat pieces they had learned.

10. **Group Class.** In addition to private lessons, Suzuki's students had a regular group class. (Private lessons meaning one-on-one instruction. In Suzuki's 'private' lessons there were frequently several children, parents, and teachers observing; thus they were not really private.) In Suzuki's group classes, the students would play in unison. These classes had two purposes. One was that the children enjoyed these classes the most and thus they served as a motivational tool. Secondly, the children would play along with more advanced students. This is similar to the parent education Suzuki advocated; just as the child desires to emulate the parent, the child later seeks to emulate the more advanced students. Furthermore, by watching the more advanced children, the students hear the more advanced repertoire, and see how the more advanced techniques are executed. As Suzuki writes, "To play alongside more advanced students—how much influence they receive and how well they develop through this experience! This is the true form of education." (p. 124)

This idea is similar to Vygotsky's Zone of Proximal Development which Vygotsky defines as:

> The Distance between the actual developmental level as determined by independent problem solving and the level of potential development as determined through problem solving under adult guidance, or in collaboration with peers (1978 p. 86).

Vygotsky noted, like Suzuki, that by allowing students to observe other students or adults executing more advanced skills they would themselves be able to execute these skills as well. "For example, if a child is having difficulty with a problem in arithmetic and the teacher solves it on the black board, the child may grasp the solution in an instant" (1978 p. 88). Suzuki and Vygotsky differ in that Suzuki felt it was worthwhile for younger, less advanced students to not only see students slightly ahead of them in their musical development, but also students who were well beyond them as well. This Vygotsky did not see as beneficial: "But if the teacher were to solve a problem in higher mathematics, the child would not be able to understand the solution no matter how many times she imitated it" (1978 p. 88). Thus it seems Suzuki would see Vygotsky's Zone of Proximal Development as a much bigger zone, and one where students may not be able to execute skills right away, but at some point in the future. However, Vygotsky and Suzuki agree that: "Human learning presupposes a specific social nature and a process by which children grow into the intellectual life of those around them." (Vygotsky, 1978 p. 88)

Conclusion

Suzuki believed that the method described above could be used to train any child. He believed that every child would develop in accordance with the environment provided and the effort expounded. While many of Suzuki's students went on to become professional performers, it was not Suzuki's goal to train professionals. Rather, the goal of his method was to cultivate noble people. What this means is described in the next chapter.

Chapter 3

The Goals of the Suzuki Method

Suzuki wrote and said in many places that the goal of his method was not to produce professional musicians. Furthermore, Suzuki found the mere question of whether or not a child could "amount to something," (meaning in this case to become a successful professional performer) demonstrates "an unwholesome view of the child as potentially a *usable thing* or worse, a *profitable thing"* (2012 p. 21). Susan Baumann (1994) has highlighted this in her writing about her experience with the Suzuki Method in Japan:

> Upon my arrival in Matsumoto it was pointed out to me that a high percentage of the students who had studied music at the Talent Education Institute from age three or four had actually quit their lessons by the age of fourteen. I was shocked to learn this because most of the fourteen-year olds, and in fact most of the Japanese students over ten years of age, were at my level of ability or well beyond it. I thought about how children of their "talent" in the Western sense would not be allowed to quit in the United States after such miraculous accomplishments. It was mind-boggling to think that Suzuki was producing child prodigies. How could he be happy about producing talented children and drop-outs at the same time? It was a paradox. Yet he was happy, he continually boasted about the fact that only five percent of the Talent Education children go on to be professional musicians (p. 7).

If not to become a professional, then why study music, particularly to such a high level? Suzuki stated, in the preface to Waltraud's translation of *Nurtured by Love*, that through studying with his method, one can turn a "mediocre child into a noble human being" (1983 p. iv). Nobility, truth, virtue, and beauty are all traits Suzuki sought to create in a child who studies with his method. This is a vague goal, often the stated goal of those who teach with little result. However, Suzuki's teaching clearly did produce excellent musicians, so one must ask what Suzuki sought to create and how his method was designed to do it.

Before explaining the specific goals of the Suzuki Method, it is worthwhile to trace the origins of Suzuki's goals. In his late teens, Suzuki spent a great deal of time playing with young children. During this time he concluded:

1. "Young children never deceive themselves."
2. "They believe in others without the slightest hint of doubt."
3. "They know only how to love and know not how to hate."
4. "They love justice and zealously adhere to the rules."
5. "Seeking joy, they live vibrantly and cheerfully."
6. "Unfamiliar with anxiety, they live in a constant state of blissful assurance." (2012 p. 81)

Suzuki further observed that many of these children would "turn out to be adults who harbor suspicion, distrust, injustice, hatred, strife, unhappiness, and darkness" (p. 81). Suzuki blamed this metamorphosis on common practices in education. He thus set out to create a new educational method which creates noble human beings.

Suzuki's definition of a noble person can be divided into several different elements. Fostering these elements of nobility in a person are the ultimate goals of the Suzuki method. These goals are not pure, meaning they are not entirely distinct from one another; rather, they are all related and sometimes similar. Suzuki did not systematically present his goals of the method, but rather his ideas are scattered throughout the text of *Nurtured by Love*. For the sake of better understanding Suzuki's goals as a basis for empirical study, critical analysis of the method, or other research, I have dissected the nobility Suzuki strove to instill in his students into individual goals. While the systematic presentation of the method has been done many times, to my knowledge, this is the first written attempt at a systematic description of Suzuki's goals. As in the previous chapter, I have used Suzuki's text, *Nurtured by Love,* as my source. I have also, as in the previous chapter, indicated key words and citations to other research corresponding to the topics discussed herein as an assistance to those who wish to conduct further research regarding the Suzuki Method.

Eight Goals of the Suzuki Method

1. **One must be productive.** As Suzuki writes in the preface to the earlier edition of *Nurtured by Love,* "Man, I believe, should follow Mother Nature, and bring forth fruit" (1983 p. v). In order for man to be productive, or "bring forth fruit," his ability must be trained. For this reason, "People should make every effort, even though it is difficult, to accumulate and build superior ability." (1983 p. 35) Suzuki felt that teachers must train children, with the assistance of their parents, to develop this superior ability. By "superior ability" Suzuki could mean relative to today's standards. Suzuki's dream was for this ability to be trained in all children, and thus if his dream were to come to fruition he would need to replace the phrase "superior ability" with the phrase "normal ability."

Alternatively, one could define Suzuki's goal by defining "superior ability" relative to the ability common to the generation of the child's parents. Suzuki does not explicitly state that he believes each generation's cultural ability increases. However, he does hint as much when he writes, "Thus be it Einstein, Goethe, or Beethoven had he been born during the Stone Age he would have attained no greater cultural ability than that of the people of that age" (2012 p. 20). Perhaps, then, one could explain Suzuki's concept of "superior ability" to mean an ability developed to an even higher level than what one would expect from the progress of each generation. That is, with each generation one expects people to develop ability beyond what was achieved in the previous generation. Accordingly, by "superior ability" perhaps Suzuki means ability that progresses even more than would happen without utilizing his method but merely by the formation of a new generation.

In psychology research, such an increase in ability from generation to generation has been used to explain the "Flynn Effect." Flynn observed that with each generation IQ scores increased and thus with each generation IQ scores must be re-standardized. While some have suggested theoretical answers to why IQ test scores increase with each generation, these theories remain highly conjectural (Flynn, 1987). One such theory, as stated above, is that with each generation the potential for developing ability increases (Mingroni, 2004).

2. **One must not believe they lack talent.** If one believes some children lack talent and therefore should remain untrained, those children will "spend their days unable to experience a vivid happiness, a soul-satisfying joy" (2012 p. 2). Furthermore, one who has not had their talent trained, and thus does not believe they have it, will believe as Suzuki previously did:

> …I felt my own incompetence piercing me to the marrow of my bones. 'How Pathetic! My talent can only be described as feeble, and yet as I slog away at this day after day. What value can there possibly be in these efforts that will take me nowhere? I just don't have the kind of talent that wells up from within. To give up now may well mean to know myself.' Utterly dejected, I began reasoning thus to myself. This sort of thing can occur to anyone in their youth, to some degree or other, and often more than once. Especially those who pursue artistic paths experience it almost without exception (2012 p. 46).

Suzuki previously believed that because he lacked talent, he could not develop ability and thus he became depressed. He therefore developed a method by which one could train what was previously thought of as talent, and not suffer this type of depression. The emotional state Suzuki describes, which results from continued failure and the belief that one lacks the ability to succeed in learning, is a form of what psychologists term “learned helplessness” (Seligman, 1975).

3. **Memorization.** Memorization is a key element in the Suzuki Method, and training students to memorize with ease is a goal of the method. Students are required to memorize all of their music, and constantly have an entire repertoire in their mind available to play at any moment. As discussed in the previous chapter, Suzuki believed memory can be trained. Suzuki further believed that the skill of memorizing music was transferable to memorization of other material from other subjects. That is, students trained in musical memorization, according to Suzuki, are able to memorize other things as a result of this training. At a rudimentary level, Suzuki believed the skill of memorization is valuable because it helps students succeed in school. As Suzuki writes, “For example, I believe that children who have good grades at school are those whose memory has developed well above the normal range, and that less successful students are simply those who have not formed this ability” (2012 p. 120). On a more complex level, Suzuki felt memory serves as the foundation for contemplation and creative thought and because of memory one can fulfill the reason they are human. He derived this idea from the writings of Diasetsu Suzuki, as he quotes in *Nurtured by Love:*

> This is because human beings possess such a thing as memory. Memory is tremendously important, for it is the source of human contemplation and creative thought. As long as human beings have memory, experience is possible, and if experience is possible, there will surely be a path for gradual advancement . . . Memory serves as the basis of experience, and it is because experience exists, one can say, that humans are able to fulfill the reason they are human. (Quoted in Suzuki, 2012 p. 120)

Lastly, Suzuki believed that memorization of music could help in the treatment of students with physical and mental problems. When writing of a student with infantile paralysis which was later cured, Suzuki claims, “Through violin playing and memorizing the music, her brain and body were stimulated. And it was this activity that made the child mentally and physically sound” (1983 p. 18). Suzuki does not explain how or why memorizing music helped this child.

4. **Self-examination.** In the Suzuki method students constantly review previously learned repertoire so that they can scrutinize their playing and try to improve it. This is done through the assistance of the teacher as well as through listening to recordings of master performers as a model. Students listen to themselves play, and compare their playing to the recording they have already heard. Because the students are playing from memory they need not pay attention to a printed page. Additionally, because the piece has been played many times, students can play it without focusing on what note comes next. This freedom from the printed page and from thinking about what comes next in the piece allows students to concentrate solely on the sound they are producing.[2] Self-examination, Suzuki believed, was not only a skill which helps one improve as a musician, but also is essential to bettering oneself in any regard. As Suzuki writes, “Fortune shines on those who often engage in self-reflection” (2012 p. 115). Social psychologists and educators have used the terms, “self-evaluation” and “reflective practice” to specify this concept (Sedikides and Strube, 1997).

5. **Immediate Action.** In order for self-examination to be profitable, Suzuki argued that students must develop the skill of taking action right away. Suzuki often complains that people merely

[2] Suzuki never advocates using recordings of the students themselves in *Nurtured by Love* as a means of self-examination. This is most likely because of the lack of readily available good quality recording devices at the time. More recently, Suzuki teachers have advocated this technique in order to facilitate students’ self-examination (See Guerriero, 2011).

think about things without putting them into practice. Suzuki believed the habit of immediate action was an essential habit for everyone. He even went as far as to say, “Indeed I would suggest that success or failure in life hinges on this alone” (2012 p. 114). According to Suzuki, people who develop the habit of action as opposed to those who merely think about things are those who will build a “fine society” (2012 p. 115).

Suzuki taught students to develop this habit of immediate action in three ways. The first was by merely insisting on it. When a student stated that they have thought of doing something, Suzuki insisted that the student follow it through right away. This was facilitated by the large memorized repertoire at the student’s disposal. If a student wishes to change an aspect of their technique they can do so by playing all of the pieces they know with this change. This is easier than trying to learn new pieces while incorporating the change.

Secondly, Suzuki helped students pick realistic goals:

> To act with resolve is to live with hope, or to keep in view a lofty mountain. There will be difficulties, but there will not be despair. Nobody can reach the summit in a single bound. And as long as one desires to make the climb, one must approach it step by step. Never make haste. This is a basic principle. One accomplishes nothing if one hurries and falls. Never dawdle either. This too is a basic principle. If one continues, regardless of what anyone else says, to move one foot before the other in silence, and without hurry or rest, one will never fail to reach the goal (2012 p. 57-58).

By having the teacher or sometimes the student pick smaller goals, it is easier for students to develop this habit of action, as they are less likely to fail, and the action required is less demanding. The type of goals Suzuki suggests the student strive for or the teacher assign are often called “S. M. A. R. T. goals” in business research. The acronym stands for specific, measurable, assignable, (although some researchers have substituted “attainable”), realistic, time-related (Doran, 1981).

Thirdly, Suzuki fosters the ability of immediate action by modeling. When Suzuki is confronted with a situation where someone thinks of doing something worthwhile but then does not follow through, Suzuki himself goes out and does it to demonstrate the action and its concomitant skill.

6. **Development of *Kan*.** As mentioned in the previous chapter, *Kan* is a concept Suzuki loosely defines as "intuition," (2012 p. 58) or "sixth sense" (1983 p. 45). Suzuki describes *Kan* as being the "reliability slumbering at the base of rational experiences, and it works in an instant when needed." (1983 p. 54) Suzuki claims that through violin playing, a student can develop *Kan*, and "*Kan* produces *Kan*." (1983 p. 49) Because of *Kan*, according to Suzuki, one is able to accomplish many great achievements (1983 p. 49). (In the 2012 edition of *Nurtured by Love,* many instances of the word *Kan* have been replaced with the word intuition.)

Suzuki provides several examples to explain *Kan.* In one place Suzuki explains *Kan* as the reason why a violinist can tell where violin strings are even with their eyes closed. Even without using their eyes, the violinist can "see" (2012 p. 59) the strings. In another place, Suzuki uses *Kan* to explain how he knows precisely what is problematic in a violin student's technique simply by listening to a recording (2012 p. 68). This may be something as specific as lowering or raising the elbow of the bow arm. Through listening to the recording Suzuki claimed he could "see" (1984 p. 54) the student. *Kan* is additionally used to explain why Suzuki was able to throw a stone accurately over a great distance in a nervous situation because of training from the early years of his life.

From the examples Suzuki provides, *Kan* seems to be a combination of visual, haptic, spatial, and procedural memory. (For a review of these types of memory, see Roediger et al., 2009.) However, Suzuki believed it was because of *Kan* that Nobel laureate physicist Hideki Yukawa was able to conceive his theory of mesons and Einstein his special theory of relativity. Suzuki does not explain how *Kan* functioned in either of these cases. Furthermore, these examples imply that there is more involved to *Kan* than just the aforementioned types of memory. A complete description or definition of what Suzuki means when he uses the word *Kan* may not be possible, and perhaps this is why Waltraud Suzuki, in the original translation, chose not to translate the word from Japanese. However, it seems to me that *Kan* is a form of expertise that makes the performance of skills or creation of ideas easy or second nature.

7. **Search for love, truth, virtue, and goodness.** In his preface to the 1983 edition of *Nurtured by Love,* Suzuki asks, "What is man's ultimate direction in life?" He immediately answers this question by saying, "It is to look for love, truth, virtue, and beauty. That goes for me, for everyone" (p. v). Suzuki never precisely defines what he means by these terms, but he does specify how his method helps students find them. Suzuki claims the aforementioned terms are developed by his method in two ways. The first is that the Suzuki Method does not reject children as being inferior. All children are accepted and treated as if they too can reach a very high musical level, or high level of any skill to which the method is applied.

> All branch chapters of Talent Education throughout the country accept every child without any admissions tests. This is because we operate on the assumption that talent is not inborn, and that every child develops in proportion to her life experience and the efforts she expends. "Let's have children study the violin as a way to acquire a beautiful heart, artful sensibility, and refined abilities. The violin is the medium through which we cultivate their humanity." Teachers of every branch chapter operate on this principle. They collaborate with the parents of their students in a joint effort to foster something precious in each child, who in effect constitutes life itself (2012 p. 23).

Because no child is rejected, the Suzuki Method can be considered a method that loves all children. As Suzuki writes, while discussing a child with infantile paralysis who kept dropping her bow:

> But the great love and persistent endeavor of both the mother and teacher won out. The time came when the child was finally able to hold the bow throughout the entire piece (1983 p. 17).

The second way Suzuki claims that love, truth, virtue, and beauty are found is through the music itself. Suzuki claimed that music "is a language of life that transcends the oral and written word, a living art that should be acknowledged for its mysteriousness, and therein lies its capacity to enthrall" (2012 p. 108). By studying, listening, and playing music, one absorbs the character communicated by the composer, as Suzuki claimed:

> Bach, Mozart, Beethoven… all of these composers are vividly alive within their music, powerfully speaking to our life forces, purifying us, elevating us, and offering us supreme joy and emotional depth (2012 p. 108).

Similarly, Suzuki writes in a reflection upon hearing the Klingler Quartet play the Mozart Clarinet Quintet:

> I am uncertain as to when I started thinking this way, but I consider myself to have been nurtured by Mozart, and through him, to have come to know love, truth, goodness, and beauty that transcend all reasoning (2012 p. 102).

This idea is part of the reasoning behind Suzuki's choice of repertoire for his students. In the Suzuki Method, even the youngest beginners study music of renowned composers rather than playing études or exercises (Suzuki, 1993). In the early volumes of the Suzuki Method students encounter works of J. S. Bach, Handel, Weber, Schumann, Vivaldi, and other notable composers.

8. **Energy and Patience.** Suzuki believed that the ability to continually work on a goal, what Suzuki calls “energy,” (from Suzuki’s examples, it seems “energy” could be defined as persistence) along with patience is essential to develop any skill. As Suzuki often said one must work “without hurry or rest” (2012 p. 58). Suzuki compares the development of ability in children to the planting of a seed. At first it seems that nothing is happening. However, eventually, with proper and constant care the seed germinates and begins to grow. If one lacks the energy for the care a seed requires, one will never see it sprout. Analogously, if the teacher, parent, and student lack the “energy” to repeat things again and again, or lack the patience to wait and see the results, the student will never become an accomplished violinist.

Suzuki claimed that energy and patience were abilities themselves that needed to be trained. As he wrote, “achievement is the product of energy and patience which have to be trained like all other abilities” (1983 p. 44). Suzuki trained students in energy and patience through demonstration. Both the teacher and parent would assist the child in many repetitions of a skill on a daily basis. The teacher and parent would divide the necessary skills into small steps, not moving on to the next step until the student had mastered the current one. Through the reinforcement created by witnessing the results of such work, students were trained in energy and patience. Suzuki believed this type of training in violin playing would transfer to any skill and thereby the students could “follow Mother Nature and bring forth fruit” (1983 p. v). Curiously, while Waltraud Suzuki’s translation makes regular use of the word “energy” when discussing this topic, Selden’s translation does not. Thus in the new translation it is more difficult to see this idea as a goal of the method.

Conclusions

While the Suzuki Method was utilized to produce remarkable musicians, it was not Suzuki's goal to create professional musicians. Rather Suzuki strove to develop noble people through teaching them the violin. Suzuki believed that noble people are productive, do not lament lack of talent, have a developed memory, engage in self-reflection, act on their thoughts, have developed *kan*, strive for love, truth virtue and goodness, and have the patience and persistence to achieve their goals. Through studying the violin using the Suzuki Method, Suzuki argued anyone can develop these traits. The Suzuki Method relies on several foundational claims. In the next chapter, I summarize these claims and investigate their novelty.

Chapter 4

Suzuki's Claims Regarding Musical Talent and their Novelty

Suzuki's Claims Regarding Musical Talent

Suzuki claimed that musical talent is not inborn but rather is the result of one's environment and effort. This claim serves as the foundation for Suzuki's teaching method which he called Talent Education. As he wrote:

> All branches of Talent Education throughout the country accept every child without any admission tests. This is because we operate on the assumption that talent is not inborn, and that every child develops in proportion to her life experience and the efforts she expends (2012 p. 23).

Throughout *Nurtured by Love,* Suzuki's most influential text, Suzuki discusses five sub-claims of his main claim that musical talent is not inborn. These sub-claims are not distinct, but rather overlap with each other and include:

1. Musical talent is not a result of genetic inheritance.

2. One's environment, and not inborn predisposition, determines one's abilities.

3. Any skill can be trained with repetition.

4. If one replicates the way in which children learn to speak one can train children successfully in any skill.

5. A lack of musical talent, often referred to as tone-deafness, is not an inborn condition, but rather the result of a poor musical environment.

These claims will be summarized briefly below and analysed in later chapters. It should be noted that in *Nurtured by Love*, Suzuki does not present these claims in a systematic fashion but rather addresses them sporadically throughout the text.

Musical Talent is Not Inherited

Suzuki argued that musical talent is not a result of genetic inheritance. Suzuki provided support for this argument by drawing a parallel to bird song. Suzuki claimed that birds do not inherit their ability to sing from their parents but rather are trained through external stimuli. Suzuki argued that the same applies for children. That is, children's musical ability is not dependent on genetic predisposition. Rather their musical development is dependent on their environment. As Suzuki wrote:

> The Japanese bushwarbler with its beautiful voice and its masterful warbling—I used to consider those celebrated features hereditary. I believed, in other words, that the offspring of a master bushwarbler inherited the predisposition of its parents. However, that turns out not to be the case. In spring, the warbler trainer enters the mountains to find a wild warbler's nest….. When it has gotten used to its new diet and generally calmed down, he borrows a master bushwarbler from elsewhere…. To raise the young in *a supreme environment*—in other words, this is talent education for warblers…. This illustrates the law of ability formation among living creatures in the natural world. Is it possible that this example relating to the cultivation of young birds proffers a valuable hint for ability development in human beings as well? (pp. 14-15).

(See Chapter 5 for an analysis of this argument.)

Ability Develops as a Result of Adaptation to One's Environment

Suzuki argued that human ability is not inborn; rather it is formed in response to one's environment. Suzuki provided evidence for his argument from the story of Amala and Kamala, two orphans who were supposedly raised by wolves in India.

> Human ability is not innate. The true configuration of human formation—that is to say, the fact that our life forces endeavor to cause us to adapt to our environment, thereby enabling us to acquire ability—is unequivocally known from the following valuable resource, among others. I summarize here from "Girls Fostered by Wolves," an account by Dr. Kida Fumio published in *Child Psychology* (p. 16).

According to the source Suzuki summarizes, the two girls were abandoned by their human parents and raised by wolves. As a result the children developed wolf-like behaviors such as carrying things in their mouths and howling at regular intervals of the night. The effect of the environment, as presented in the story, was so strong that the girls developed wolf-like physiological features as well. For example the shape of their legs was described as wolf-like. Additionally, the girls did not perspire but rather hung out their tongues and panted. As a result of their wolf-like upbringing the girls developed superior abilities such as a keen sense of smell, the ability to see well in the dark, and the ability to run faster on all fours than humans run regularly. The abilities developed by these girls, according to Suzuki, demonstrate that human ability is not developed as a result of inborn predisposition but rather as a result of one's environment. Suzuki thus argues that one need only to create a musically enriched environment to train children to have superior musical ability (See chapter 6 for an analysis of this argument).

Anything Can Be Trained with Repetition

Suzuki argued that one can train a student to develop any skill through repetition. He supports this argument through an examination of the development of handedness, whether one is right- or left- handed. Suzuki claimed that handedness is not the result of an inborn preference but rather the result of repetitive use of a hand. He further argued that if one wanted to train their non-dominant hand to be as capable in a skill as their dominant hand, they need only to repeat the skill in their non-dominant hand many times.

> Your left hand is inferior to your right hand if you have allowed it to stay idle. Even though they both belong to you, your left and right hands reveal a tremendous difference between the one that receives training in the form of everyday use, and the one that does not….

> Look, now, at your right hand. If you train yourself daily the way you have trained your dominant hand, strength will be generated, sensitivity will be fostered, and ability will gradually develop. The ability possessed by your right hand, which surpasses your left, was not created by someone else. You have created it yourself…. How, then, did the extraordinary ability of your right hand come into being? It must have developed through repetition (p. 53).

Just as Suzuki argued one can train their non-dominant hand to become as agile as their dominant hand, Suzuki argued any music related skill can be trained through repetition. (See chapter 7 for an analysis of this claim.)

Replicating Speech Acquisition

Suzuki claimed that all children learn to speak their native language. He therefore hypothesized that if we teach children another skill in the same way in which they learn their native language, they will be equally successful. He thus developed a system of music education which he claimed replicated the way in which children learn to speak.

> Children freely speak Japanese, I realized, because they are, in effect, given the opportunity to do so. There is the fact of training and education behind their capacity to speak. Therein lay a proper educational method. Every child, without question, is developing appropriately. This was precisely that perfect educational method I was seeking (p. 7).

Suzuki thus believed if music is taught in the same way that children acquire speech abilities, any child could be trained successfully. (See chapters 8 and 9 for an analysis of this claim)

Tone-deafness is not Inborn

Just as Suzuki believed that musical talent is not inborn, he also believed that a lack of musical talent, or tone-deafness, is not an inborn condition. Rather Suzuki felt that tone-deafness is a condition that is a result of hearing out of tune music, for example the out of tune singing of a parent.

> I often hear people say, "I'm a tone-deaf parent so…" The claim is that therefore the child is tone deaf and nothing can be done because this is a matter of heredity…. Far from being tone deaf, all infants have marvelous hearing. That is why they unerringly absorb the off key pitches of the lullabies sung to them by their tone deaf parents (p. 16).

Suzuki further believed that the condition of tone-deafness could be rectified by having children listen to music which is in tune. When the amount of music the child hears in tune surpasses the amount of music which the child heard out of tune, Suzuki claimed the child's tone-deafness will be rectified.

> Then, what can be done to address tone deafness? What I discovered was that although I could not teach tone deaf children to adjust their overly high-pitched *fa*, I could help them learn how to produce anew the correct pitch of *fa*. I decided that with a child who has become tone deaf from hearing the wrong *fa* 5,000 times, I would have her listen to the correct *fa* 6,000 times, then 7,000 times. At first nothing changed, but as she heard the correct *fa* 3,000 times, 4,000, 5,000, and 6,000 accumulated times, her ability to reproduce the correct *fa*, which she absorbed from hearing it 6,000 times, began to overpower her ability to produce the wrong pitch she had learned from hearing it 5,000 times. In short, the newly cultivated, correct function had taken root. (p. 116)

(For an analysis of Suzuki claims regarding tone deafness see chapters 10 through 12).

Suzuki's Caveat

While Suzuki did not believe in inborn musical talent, he did provide one caveat to his theory. Suzuki believed that one element of superior ability is inborn. Suzuki believed that this element is not specific to one skill but applicable to all learned skills. Regarding this caveat to his theory, Suzuki wrote in *Nurtured by Love*:

> When it comes to hereditary human superiority or inferiority, observed differences stem from nothing other than the variety in the quality of ability acquisition and formation; in other words, the sensitivity and speed with which humans adapt to their environments. Accordingly, being born with superior natal traits means one possesses outstanding sensitivity and speed for adapting to the environment (p. 19-20).

It is unclear what exactly Suzuki means by this, as he does not explain more than what is quoted above. This idea is mentioned only twice (see the above quote, and the quote below) in *Nurtured by Love* and is not featured prominently in his other writings. Furthermore, Suzuki does not provide any examples of observations of this inborn speed and sensitivity, or any information regarding how this varying speed and sensitivity might impact pedagogy.

Dorothy Jones (personal communication, 2014), an influential Suzuki teacher and developer of the Suzuki Early Childhood Education program, has suggested that Suzuki did not discuss this topic at length as he was more focused on practical teaching rather than theoretical ideas. That is, because one cannot change how one was born, Suzuki did not concern himself with the idea. Rather he focused on what he was able to train.

Gilda Barston (personal communication, 2014), the Chief Executive Officer of the International Suzuki Association, has suggested that Suzuki meant that all children are able to acquire the same level of mastery of any musical instrument, but some take more time than others. Barston claimed that Suzuki did not feel that one student can be born with superior musical aptitude than another and therefore reach a higher level of musicianship, only that one student can develop mastery of an instrument with a shorter amount of time.

It should also be noted that Suzuki was perhaps hesitant to discuss the idea of inborn speed and sensitivity to the environment as then teachers might attribute fast progress or slow progress of a student to this idea. Suzuki cautioned against this as one might erroneously assume this inborn speed was the cause when actually it was early exposure to a particular skill.

> Admittedly, what one person can do after 500 repetitions may require 5,000 repetitions for someone else to accomplish to a similar degree. Seeing the difference in the rate of acquisition between such people, others often discuss the presence or absence of innate intuition, skillfulness or clumsiness. However, they may err in their judgment unless they trace back to the days of each individual's birth, research both of their personal histories, and consider how their present forms of ability have evolved this brings us back to the argument that what is important is to place the children in a good environment from birth and let them train efficaciously; but one must not forget either that those who do not succeed after 500 times can yet develop a desired skill after 5,000 times (p. 67).

Nevertheless, it is clear that Suzuki felt that there was one small component of ability development which is inborn, that of speed and sensitivity of adaption to one's environment.

The Novelty of Suzuki's Claim

It is worthwhile to consider how revolutionary Suzuki was in his claim that there is no musical talent with which one can be born. It is conceivable that another prominent violin pedagogue would argue that a good teacher can create a master out of any student as this argument would confirm the superiority of their teaching. To investigate this possibility I explore the treatises of four influential violin pedagogues of the 20th century: Leopold Auer, Samuel Applebaum, Ivan Galamian, and Carl Flesch.

Leopold Auer and Musical Talent

Leopold Auer (1845-1930) began studying the violin at age eight, with Ridley Kohne at the Budapest Conservatory. He later studied under Jakob Dont and Joseph Joachim. On the recommendation of Anton Rubinstein, Auer was appointed as a violin professor at the St.

Petersburg Conservatory in 1868, succeeding Henryk Wieniawski. In St. Petersburg, Auer taught many students including Efrem Zimbalist, Mischa Elman, and perhaps most notably, Jascha Heifetz. With world class violinists arising from his studio, it is not surprising that Auer's approach to pedagogy became popular. In 1917 Auer fled Russia and its growing instability. After a brief stay in Norway, Auer arrived in New York in 1918. In 1921 he published a treatise on violin playing, entitled *Violin Playing as I Teach It.* Auer's conclusions regarding musical talent are in contrast to those of Suzuki. While Suzuki believed all children could become master players, Auer claimed:

> One of the great mistakes lies in the failure of so large a majority of those who decide to devote themselves to music—to learning some string instrument, the violin for example—to ascertain at the very outset whether nature has adequately supplied them with the necessary tools for what they have in mind (p. x).

Auer periodically references the idea of musical talent in his text and attributes almost every aspect of violin playing, at least in its rudimentary form, to inborn ability. Of the abilities that one might consider cognitive, Auer makes specific mention of inborn ability in what he calls hearing, rhythm, and style. With regard to hearing, Auer writes:

> A keen sense of hearing is, above all, one of the qualities which a musician needs. One who does not possess it in the highest degree, is wasting his time when he centres his ambition on a musical career. Of course one may perfect one's musical hearing if the faculty exists in even a rudimentary form—though the student will have to be quick to improve it by exact attention to the advice given him, and by unremitting watchfulness while he is at work—but there must be a certain amount of auditory sensibility to begin with (p. x).

Similarly, with regard to rhythm, Auer writes:

> But besides an adequate physical equipment, one of the qualifications most important to the musician is a sense of rhythm. Together with the sense of hearing it is a sine qua non for everyone who wishes successfully to devote himself to music (p. xi).

Lastly, when writing about style, Auer claims:

> Imitation may be the sincerest form of flattery but any student who flatters a master in this way is doing so at the expense of his own individual development. His jackdaw mannerisms may in the course of time become second nature, but they will still be mannerisms—whereas in the case of the violinist in whom they are inborn they are not necessarily mannerisms, but a veritable part of his stylistic outfit (p. 82).

Aspects of violin playing which one might consider physical such as favourable conformation and technique, Auer also argues are a result of inborn factors. Auer believed that some people are naturally more adept at handling the violin than others because of the physical make-up of their bodies. He further argued that some peoples' hands simply cannot be made to play the violin.

> Then there is the very important question of the physical conformation of the hand, of the muscles, of the arm, of the wrist, and of the elasticity and power which the fingers possess. There are hands that absolutely refuse to conform to technical requirements indispensable to mastery of the instrument. Many aspiring students have hands, for instance, the fingers of which are too fat. (I have known students, however, who, despite this handicap, have through intelligent and assiduous practice managed to acquire a perfect intonation.) There are hands with fingers which are too flaccid, bending, which refuse to work at the very moment when they should be firmest. There are hands the fingers of which are so short that they can scarcely move with the limits of the first position where the distance between the intervals is greatest, and where they cannot possibly stretch octaves and tenths. And there are also weak fingers whose weakness inherently is so great that the very endeavour to strengthen them by exercise only increases their flaccidity (p. x-xi).

The last line of this particular quote is noteworthy. Auer argues in this particular case the repetition of strengthening exercises only increases the flaccidity of some peoples' fingers. Thus it seems Auer is in disagreement with Suzuki's claim that anything can be trained with repetition.

With regard to technique, Auer believed that the mastery of any aspect of violin technique is due, at least in part, to a violinist's natural predisposition. He even mentions natural predisposition when discussing *pizzicato*, the simplest of all violin techniques.

> Beyond all question, in order to be able to produce the pizzicato under consideration in a satisfactory way—and this holds good for every branch of violin technique—nature should have properly equipped one for the task. Thus, in this connection, the quality of the skin covering the fingertips plays an important part in the production of the pizzicato in question (p. 56-57).

Though Auer claims that every branch of violin technique relies on a student's natural ability, he only explicitly writes this with regard to pizzicato (as mentioned above), tone production, the trill, and double harmonics. When discussing tone, Auer wrote:

> Natural instinct, physical predisposition, the construction of the muscles of the hands and of the bow-arm—each plays a determining part in the ultimate effect (p. 18).

With regard to the trill Auer claimed:

> There are some fingers which, blessed by a happy natural gift experience not the slightest difficulty in carrying out a prolonged trill with perfect equality of movement. But there are other less fortunate fingers, which in spite of long and assiduous practice, never gain more than a mediocre mastery of the trill (p. 50-51).

And, with regard to double harmonics[3] Auer wrote:

> There is but little special material available for the methodical study of double harmonics, the principal reason being, of course, because they are so rarely used. Besides, they require a hand and fingers naturally adapted to produce them, and they also presuppose seasoned nerves, able to endure the torture of notes frequently missed during practice. Yet even with all the natural aptitude and favourable physical prerequisites imaginable, there always remains a certain amount of risk in playing double harmonics in public (p. 60).

[3] A double harmonic is created when the violinist stops two strings with two different fingers. The remaining two fingers are then touched lightly on the string in such a way that it divides the string length into a ratio with which a harmonic tone is produced. The resulting sound is whistle like. This technique is very difficult and be found in the works of violin virtuoso composers such as Paganini.

Despite Auer's belief that all elements of violin mastery involve inborn or natural components, he does concede that there are instances when a violinist can overcome certain natural weaknesses and achieve mastery of the violin.

> Thus, Willhelmj, a great violinist, and one who shone by reason of his physical strength, had neither a good trill nor a good staccato. But to make up for it, he could draw from his Stradivarius the biggest and most powerful of tones, as I well remember, for I heard him in Russia between forty and forty-five years ago (p. 51).

Samuel Applebaum and Musical Talent

Samuel Applebaum (1904-1986) was a student of Leopold Auer at the Juilliard School. He later took a position at the Manhattan School of Music, where he was a violin professor for 35 years. Applebaum's influence can be found in almost every sphere of string music education as he authored and edited over 400 volumes of method books and collections of repertoire. Applebaum frequently lectured at conferences of the American String Teachers Association. In 1986, with the assistance of Thomas Lindsay, Applebaum assembled his most frequently asked questions regarding string playing into a treatise titled, *The Art and Science of String Performance.*

Unlike Auer, it is difficult to determine exactly what Applebaum believed regarding talent, as the book is largely about solving problems frequently presented in string playing. The absence of direct discussion on the question of talent in his treatise is perhaps the result of the focus of Applebaum's writings. While Auer was interested in training world-class virtuosi, Applebaum wrote a great deal regarding music in schools. Nevertheless, we can still see that Applebaum differs from Auer and Suzuki in his thoughts on musical talent. Applebaum believed that many of the abilities, which Auer ascribed at least in part to natural gifts, can be trained.

For example regarding the trill, a technique Auer attributed to an inborn predisposition, Applebaum wrote:

> Q. If Velocity is dependent upon a good trill, how do we develop the trill?
> A. A good trill is possible when we have a relaxed hand and when the fingers have been trained to snap back quickly with precision and speed. Strong fingers per se, will not necessarily produce a good trill. A student can have strong fingers and yet lift them in a sluggish manner from the strings. Lifting the fingers from the string speedily will come when one practices exercises that include mordents. The mordent might well be the secret of a good trill because its performance encourages a snappy lifting of the finger from the string. The finger must always leave the string in a curved shape (p. 13).

There is only one case where Applebaum agreed with Auer that there is a technical problem that is not solvable for all students, and that is with the development of staccato. As he wrote:

> Q. Is it possible for every player to develop a VERY RAPID STACCATO?
> A. Perhaps not (p. 110).

Because Applebaum wrote that a particular technique is not possible for all students, it seems he disagreed with Suzuki's claim that anything can be trained with repetition.

While Applebaum does not write on the topic of talent to the same extent as Auer, he does in passing claim that students do have different amounts of talent. For example:

> Q. How long should a pupil remain in first position?
> A. That is difficult to answer because we are not all alike as far as aptitude for the instrument is concerned (p. 34).

Or similarly:

> Q. When do you introduce the development of the thumb technic in the higher positions?
> A. I like to present thumb technic toward the end of the second year of instruction. With talented children, it can be introduced after a few months of study, with even less time for the very gifted child (p. 40).

One might conclude from the above two quotes that Applebaum believed that inborn talent only contributed to speed of learning, or in other words, speed and sensitivity of adaptation to one's environment. Thus, one might think that Applebaum's view of talent is similar to that of Suzuki's caveat. However, at the end of his treatise Applebaum wrote:

> From my experience in working with gifted children from the age of 4 years and older, I have noticed that they play in public without fear. They develop velocity and accuracy at an early age. One manifestation of great talent is the skill and ease with which they memorize. Sometimes students who are talented have only to play a composition a few times before they can play it from memory. They are likely to be a bit careless about changes of bowing and other details, but they can be corrected (p.145).

All of the qualities which Applebaum ascribes to the gifted student, lack of fear for performance, speed and accuracy of playing, and memorization are qualities which Suzuki specifically argued in *Nurtured by Love* are trained and not inborn. Thus it seems Applebaum believed some components of musical talent are inborn beyond speed and sensitivity of adaptation to one's environment and Applebaum is not in agreement with Suzuki that musical talent is not inborn.

Applebaum also hints that interpretation involves an inborn component although he did not write this explicitly. When discussing interpretation Applebaum wrote:

> Q. But aren't talented players instinctively aware of these principles?
> A. To a great extent there is a science to the art of interpretation, the knowledge of which will confirm the performer's instinct (p. 130).

Applebaum wrote "to a great extent there is a science" meaning most aspects of interpretation can be explained and taught, but others cannot. These aspects are left to the "talented performers." This idea is enhanced when Applebaum later writes, "After all principles of phrasing and interpretation are planned, science ends and art begins" (p. 135).

Ivan Galamian and Musical Talent

Ivan Galamian (1903-1981) was born in Tabriz, Persia (Iran) but emigrated as an infant to Moscow. His first and perhaps most influential teacher was Konstantin Mostras, who was a pupil of Leopold Auer. Galamian later continued his studies with Lucien Capet in Paris. In 1918, Galamian emigrated to the United States where he taught at both the Juilliard School in New York and the Curtis Institute in Philadelphia. The list of violinists that emerged from his studio is perhaps more impressive than any other violin pedagogue. Itzhak Perlman, Pinchas Zukerman, James Buswell, Michael Rabin, David Nadien (David Nadien is one of the violinists who recorded the repertoire of the Suzuki Violin School), Sally Thomas, Jaime Laredo, Glenn Dicterow, Arnold Steinhardt, Dorothy Delay, and David Cerone (David Cerone is another violinist who recorded the repertoire of the Suzuki Violin School) are some of the students that emerged from his studio to become world-class performers and teachers. Because of the fame achieved by many of his students, it is not surprising that when Galamian published his treatise, *Principles of Violin Playing and Teaching* in 1962, it became influential.

Galamian had the reputation of being able to teach anyone to master the violin. As Sally Thomas wrote:

> Mr. Galamian seemed to be able to answer the question many pedagogues ask themselves: "What inspires a student to perform better than their observable capabilities?"
> During Ivan Galamian's life time, it was often stated he could teach a chair to play the violin. What was remarkable about Galamian's teaching was the number of students that transcended their original promise (2012 p. vii).

Because of this reputation, one might expect that Galamian's beliefs regarding musical talent would coincide with Suzuki's. However, this is not the case. Galamian believed that both the technical components of violin playing as well as well as musicianship and interpretation are affected by inborn talent. As Galamian wrote:

> A certain qualification has to be made, however, to the above statements, owing to the fact that there are wide differences in talent among students, not only with regard to the technical ability but also with regard to musicianship and imagination (p. 8).

What is interesting about this particular quotation is that Galamian continues to write that it is the more talented students that are more difficult to teach.

> Not every student has the potentiality of creative imagination that can develop to the point where he can become a fine performer in his own right, even when his technical abilities may be unlimited. Broadly speaking, students may be divided into an “active” and a “passive” category. The active students are those who have the innate urge of a creative imagination. They are the truly challenging ones and can be made to grow into genuine artists (p. 8).

What is further interesting is that despite Galamian’s belief that there are differences in talent as far as technical ability is concerned, never does he address these differences. While Auer discussed how different types of hands are better for certain specific aspects of violin technique, Galamian did not. Like Applebaum, Galamian did the opposite. That is, he discussed the different approaches required with those who have different natural equipment. For example, regarding the position of the left hand, Galamian writes:

> By means of these two variables (the position of the elbow and the vertical adjustment of the hand with regard to the neck) any hand can be positioned so that it is comfortable and can function efficiently (p. 17).

Or similarly with regard to vibrato he writes:

> The forgoing discussion would not be complete without calling attention to the fact that the shape of the player’s fingers has an important influence on the several types of vibrato. Those who have narrow and bony finger tips will be well advised to place the fingers on the strings using a flatter angle (p. 41).

However, unlike Applebaum, Galamian never mentions a technique which cannot be executed by everyone. Thus while Galamian clearly disagreed with Suzuki regarding musical talent, he may agree with Suzuki's claim that any skill can be trained with repetition. However, as mentioned above, Galamian believed that technical aspects of violin playing depend on inborn talent, so perhaps he would argue that while every skill can be executed through repetition, some can perform these skills better than others as a result of inborn talent.

While Galamian clearly believed in inborn musical talent, he acknowledged at the end of his treatise, that talent only plays a small role compared to perseverance and amount of practice in the development of violin playing:

> The road to VIOLIN MASTERY is long and arduous, and great application and perseverance are needed to reach the goal. Talent helps ease the way, but in itself cannot be a substitute for the hard work of practicing (p. 93).

Carl Flesch and Musical Talent

Carl Flesch (1873-1944) was born in Moson, Hungary. At the age of 10 he went to Vienna to study with Jakob Gruen, and at 17 left for Paris to attend the Paris Conservatoire. Carl Flesch taught in many places throughout the world including Bucharest, Amsterdam, Philadelphia, Berlin, and London. He taught many notable pupils, including Ida Haendel, Eric Rosenblith, Max Rostal, Henryk Szeryng, and Roman Totenberg. Carl Flesch's influence was widespread as a pedagogue. He is most famous for his scale book titled the *Carl Flesch Scale System* which is the most widely used violin scale book. He also published an influential treatise titled, *The Art of Playing the Violin* (1924).

In this treatise, Flesch, like Suzuki, complained that teachers often assume student abilities to be natural or inborn, rather than something that is trainable. For example, regarding vibrato he wrote:

> The teaching body has been content to call *vibrato* "natural and "impossible to learn," confusing personal need with the mechanical prerequisites for its creation. How *does* the beginner commence to vibrate? Does it come to him suddenly, like a divine revelation? Not at all. The process is far more prosaic (Vol. 1 p. 37).

Similarly regarding the trill Flesch wrote:

> In reality, however, *vicious* cramped conditions of certain parts of the hand very often prevent the development of that benign tremulant movement which is known as the trill. Their cure is often hindered by prejudices on the part of teachers, who also call an aptitude for the trill a "natural" gift, hence one which cannot be acquired. Although this is correct insofar that many violinists, especially Gipsy musicians, are possessors of a naturally good trill, yet it in no wise justifies the deduction that it is generally impossible to acquire or improve it (Vol. 1 p. 45).

Flesch believed, like Suzuki, that many of the skills which many teachers believe to be natural are trainable. He therefore might agree with Suzuki that any skill can be trained with repetition. Nevertheless Flesch still believed in a concept of inborn talent. Flesch used the term "psychic" to describe inborn cognitive qualities that contribute to the successful development of a musician:

> We already have had numerous opportunities, in Part One of this work, to stress the fact that many violinistic qualities are regarded as "inborn" gifts, i. e., not capable of being acquired. In reality it is possible, however, by adequate instruction, to acquire all those violinistic characteristics which are not rooted in the psychic field. The technique of bowing and fingering, as well as an unobjectionable tone production, are within reach of every industrious and carefully guided pupil. They can be created, so to say, out of sheer space. Other qualities, again, must be available at least in germ form, if they are later to be developed to perfection. Above all, it is those signs of character already mentioned as being common to the artist, which even the most finished teacher cannot conjure up (Vol. 2 p. 83).

While Flesch believed that certain aspects of violin playing were dependent on talent, he qualifies his belief by arguing that without the guidance of a careful teacher, the musician will not develop.

> Yet even those more elevated qualities which alone permit a performance to become an act of art—style, rhythm, power of expression—and which from the start may be available to a certain degree as that something which we call talent, can only attain their full development when, by the prescient care of the teacher, they have been freed from their slag (Vol. 2 p. 3-4).

However, he nevertheless believed that should a student demonstrate a lack of talent the student should be discouraged from pursuing a career in music.

> When this natural disposition, in the case of young pupils, can be recognized in time, the honest teacher must spare no efforts to convince the psychically unfitted student to take up some other profession (Vol. 2 p. 68).

What is particularly interesting about Flesch's approach to talent is that he is the only pedagogue of those discussed to suggest that musical talent is not only inborn but also genetically inherited. He is thus the only pedagogue to contradict Suzuki's aforementioned sub-claim that musical talent is not inherited. Flesch argued that musical talent is a result of genetic inheritance by noting that some races appear more successful in music than others:

> The original qualities of our ego are the heritage of a long line of forbears. The laws regulating the inheritance of intellectual qualities are still shrouded in darkness. We cannot—in so far as they may be considered *racial peculiarities*—dismiss them without mention, the more so, and in particular, seeing that violinistic talent is one of their outstanding characteristics…Among all races the *Jewish* race probably can point to the greatest number of violinistic talents…In England owing to the presence of a larger number of foreign teachers, violin playing during the past few decades has experienced a considerable revival, in spite of the fact that the average Englishman, in general, does not possess the sensitive nervous organization essential for violin playing (Vol. 2 pp. 71-72).

Of further interest, is that Flesch is the only pedagogue discussed that mentions the concept of talent not just for playing but for teaching as well.

> In addition, the teacher does not always possess that amount of original talent and those acquired abilities which are a prerequisite for valid criticism (Vol. 2 p. 5).

Conclusions

Suzuki not only believed that musical talent is not inborn, he founded an entire institution of music education based on this claim. He additionally made five sub-claims related to this foundational principle, namely musical talent is not the result of genetic inheritance, it is the environment and not inborn predisposition that determines ability development, anything can be trained with repetition, if one replicates the process of speech acquisition any skill can be developed, and lack of musical talent, or tone-deafness, is not inborn.

Four of the most influential violin pedagogues, however, disagree. All four of the aforementioned violin pedagogues believed to some extent that violin mastery is dependent on some form of inborn talent. While these four pedagogues cannot account for the beliefs of every violin pedagogue of the time, they are perhaps, because of their influence, the best representatives. Therefore one can conclude that Suzuki was revolutionary in posing his theory that musical talent is not inborn.

However, regarding Suzuki's sub-claims there is more variability. Auer and Applebaum disagree that any skill can be trained with repetition, while Galamian and Flesch may agree with Suzuki. Flesch is the only pedagogue who disagreed with Suzuki regarding genetic inheritance. However, the other pedagogues do not address the topic so it may be that they would disagree as well. None of the pedagogues address the issue of environmental factors and none of the pedagogues draw parallels between music education and speech acquisition. Thus in these regards, Suzuki was revolutionary. Also, none of the pedagogues address the issue of tone-deafness. This is particularly interesting as Flesch and Auer make specific mention of discouraging those they deem to be not talented from further study. This may, however, be a result of their accepting only students of the highest caliber. Thus they were only interested in finding those that were abnormal in that they were more able than the normal population as opposed to eliminating those who were less able than the normal population. In contrast Suzuki believed all children could master the violin and thus he needed to address the issue of those that believe they are inferior due to an inborn lack of talent.

As demonstrated by this chapter, Suzuki's claims regarding musical talent were novel. In the remaining chapters I investigate their validity.

Chapter 5

Musical Ability is not Inherited: Bird Song

The Suzuki Method is based on the idea that musical talent is not inborn but rather cultivated through one's environment and effort. As Suzuki wrote:

> All branches of Talent Education throughout the country accept every child without any admission tests. This is because we operate on the assumption that talent is not inborn, and that every child develops in proportion to her life experience and the efforts she expends. (2012 p. 23)

As stated in the previous chapter, Suzuki makes five sub-claims related to this foundational principle of the method. In this chapter I address the first of these sub-claims, namely that musical talent is not genetically inherited.

Suzuki's Bird Song Argument

Suzuki argues that what is observed as musical talent is not a result of genetic inheritance. Suzuki provides evidence for this argument by drawing a parallel to the way in which birds learn to sing. Suzuki argued that songbirds do not inherit their ability to sing from their parents but rather are trained through environmental stimuli. He further argues that humans are the same with regard to music. In *Nurtured by Love*, in a section titled "Ability is not Innate," Suzuki wrote:

> The Japanese bushwarbler with its beautiful voice and its masterful warbling—I used to consider those celebrated features hereditary. I believed, in other words, that the offspring of a master bushwarbler inherited the predisposition of its parents. However, that turns out not to be the case.
>
> In spring, the warbler trainer enters the mountains to find a wild bushwarbler's nest. Catching and bringing home a young bird still being cared for by its parents, the trainer coaxes the fledgling to acclimate to being fed by humans. Timing the moment when it has gotten used to its new diet and generally has calmed down, he

> borrows a master bushwarbler from elsewhere and exposes the young bird on a daily basis to the master's beautiful warbling. This period lasts approximately one month.
>
> The above process of cultivating wild young birds, with the aim of turning them into master warblers, is a method practiced from long ago in Japan. To raise the young in a supreme environment—in other words, this is Talent Education for warblers. The young bird to be trained in this manner is called *tsukego*, or "attached child," perhaps meaning a fledgling attached to a teacher bird.
>
> The *tsukego* receives careful education in a variety of forms after that initial period. However, what is vital above and beyond all else is to attach it to a good teacher in that first month. For indeed, the very future of the young bird is determined by the superiority or inferiority of its teacher's vocal quality and melodic phrasing. In other words, there is no innate superiority or inferiority. What is there is the young bushwarbler's life force. The operation of that life force has a marvelous power through which the bird strives to adapt to its environment, enabling it to undergo physiological changes that allow it to foster the ability to utter beautiful sounds, acquire a masterful delivery, and, by the time it is full-fledged, warble with the same beautiful voice and turns of phrase as those of its teacher.
>
> Based on empirical evidence, however, it has been said from long ago that the attempt always fails when the apprentice bird has already been exposed to the singing of wild bushwarblers. This illustrates the law of ability formation among living creatures in the natural world. Is it possible that this example relating to the cultivation of young birds proffers a valuable hint for ability development in human beings as well? At least for me, the belief that this might indeed be the case has provided tremendous strength and confidence in my approach to nurturing children (2012 p. 14-15).

Two questions arise regarding the validity of using the example of a warbler to support Suzuki's argument. The first is whether Suzuki is correct in what he writes about the way warblers learn to sing. The second is whether it is valid to make a comparison between the way a bird learns to sing and how humans acquire musical abilities.

Bird Song Learning

The Japanese Bush warbler

The Japanese Bush warbler (*Cettia diphone* or Uguiso in Japanese) is a bird commonly found throughout Japan. The bird is frequently cited in Japanese folklore, art, and literature and is often associated with the start of spring. Because of the bird's beautiful song (for a sonogram of four song types of the Japanese Bush warbler see Hamao et al. 2008: p. 273) it became known as the Japanese Nightingale. This is why in Waltraud Suzuki's translation of *Nurtured by Love,* she uses the word Nightingale rather than Warbler (Suzuki, 1983 p. 9). However, it is no longer referred to as the Japanese Nightingale because the bird does not sing at night and is not closely related to the nightingale commonly found in Europe (Brazil, 1991: p. 223-224).

There is little research on the song of the Japanese Bush warbler. In particular, there is no research on changes in Japanese Bush warbler songs during their lifetime. As far as I know, research on the song of this particular species is limited to studies that use the species' song as a means to distinguish it from other similar species of birds (see for example Hamao et al. 2008). Therefore, in order to investigate Suzuki's claims, this chapter will discuss bird song as a whole, drawing on research of several species.

The Critical Period

The study of how birds learn birdsong has a long history dating back at least 200 years (Barrington, 1773). In the past half century, there has been a tremendous growth of research largely due to technological advances. Many cite the invention of the sound spectrograph, first used to study birdsong by Thorpe (1954), as the reason for this growth in research. In more recent times, better and more portable recording equipment, coupled with cheaper and more readily available sound analysis software, has enabled researchers to study birdsong with greater ease and at a fraction of the cost (Catchpole and Slater, 2008: pp. 2-3). This research has shown that Suzuki is partially correct in his two assumptions. Suzuki is partially correct in his assumption that birds have a critical period of their life in which they are sensitive to song learning. He is also partially correct in his claim that birds learn their song by means of an external auditory stimulus such as a tutor bird.

Research on a number of species of birds has shown that birds memorize their songs at an early age and for a set amount of time. That is, after a certain critical amount of time, birds no longer learn to sing. For most species, this time is within the first few months of hatching. This has been shown to be true for the Zebra finch (*Taeniopygia guttata*), the White-crowned sparrow (*Zonotrichia leucophrys*), and the Song sparrow (*Melospiza melodia*). With many birds, there is a second sensitive period shortly before they are one year old. This second sensitive period has been observed with the Chaffinch (*Fringilla coelebs*), the Canary (*Serinus canaria*), the Starling (*Sturnus vulgaris*), and the Nightingale (*Luscinia megarhynchos*) (for a chart of the sensitive time periods of these birds see Hultsch and Todt, 2004 p. 85). The sensitive time periods for the learning of song correspond to when adults of that species are vocally active. It thus serves as a mechanism to ensure that birds learn the songs of their own species, rather than the songs of other birds (Hultsh and Todt, 2004). Thus it seems Suzuki was partially correct. Birds do have a sensitive time period in which they learn to sing. Suzuki's statement simply needs a slight adjustment. The learning period for most birds is longer than one month, and frequently there are two periods rather than one.

The Problem of Open-ended Learners

However, there is a problem with this conclusion. There are some birds who have been observed to be open-ended learners. That is, some birds have been observed to memorize and produce new songs as adults. For example, nightingales have been observed to be open-ended learners, (Todt and Geberzahn, 2002), as have canaries (Nottebohm et al. 1986). Most open-ended learning occurs when birds listen to previously unheard songs of their own species. There are however, a small number of birds who will learn the songs of other species, for example the Brown thrasher (*Toxostoma rufum*). There are also a small number of birds who create new songs later in life by combining and rearranging fragments of previously learned songs, for example the Sedge warbler (*Acrocephalus schoenobaenus*) (Brenowitz and Beecher, 2005).

One might think that open-ended learners prove to be problematic for Suzuki's idea that birds only learn to sing early in life. However, open-ended learning has been observed only in birds raised in wild environments or non-isolation environments. Under such conditions, the birds have always developed the ability to sing in their early life. It is likely that birds who have not been trained to sing at all during their sensitive period (if for example they were raised in isolation) may not be able to develop later singing abilities, and thus may not be able to learn songs later in life. Implying that while open-ended learning birds are able to learn and produce new songs as adults, they still learn the skill of singing early in life. This conclusion has not been proven or refuted, but nevertheless seems likely. Thus, while Suzuki's claim needs a little adjusting, he is on a basic level correct. That is, birds do have a sensitive period in which they learn to sing, and many birds learn the majority of their repertoire during an early sensitive period. There are some birds who can learn or create additional songs later in life, but only when they have learned to sing while they were young.

Learning through Environmental Stimuli

In regards to Suzuki's claim that birds learn to sing by means of an auditory model, there is a large amount of relevant research. In many studies, birds have been raised in isolation in a laboratory with no exposure to other birds. While these birds still develop song, their song is considerably different from both their wild counterparts and other birds raised in laboratories but not in isolation. For example, the chaffinch (*Fringilla coelebs*), when raised in isolation, produces a song that is similar in duration and frequency to that of the wild chaffinch. However, while the wild chaffinch produces a song with clear distinct phrases in which syllables are structured and nearly identical and are followed by an elaborate phrase ending, the isolated chaffinch sings a song composed of syllables that are not clearly distinguishable and the song simply "peters out at the end" (Slater 1989, p. 21) (See Slater, 1989 p. 21 for sonograms of the song of a wild chaffinch and a chaffinch raised in isolation). Thus, it is clear that raising birds in an environment in which they hear bird song is crucial to the development of their species' song.

Just as raising birds in isolation provides evidence that certain aspects of bird song must be learned from an outside source, studies where birds have been deafened have yielded similar conclusions. For example, Nottebohm (1968) found that chaffinches who were deafened as juveniles did not learn to sing the species' song, while birds deafened as adults who had already developed their song continued to sing normally, as if no change had taken place. Similarly, Marler and Sherman (1983) found that in some cases deafened birds have been observed to produce songs that were more screech-like rather than song-like. If a bird's ability to sing was inborn, one would expect deafened birds to sing just as non-deafened birds do. This suggests that birds must actually hear birdsong in order to sing their natural song.

Additional evidence that birds learn song from outside sources rather than being born with the ability to sing their species' song comes from research regarding the number of repetitions of a song a bird must hear before being able to produce it. Most studies in which birds are trained to sing expose the birds to thousands of repetitions of the song, as it has been widely observed that the more repetitions a bird hears of a song, the greater chance the bird will then be able to produce it. However, the number of repetitions a bird actually requires seems to be drastically fewer than typical laboratory practice (Peters et al. 1992). In the most extreme case, nightingales (*Luscinia megarhynchos*), who were tutored with recordings at approximately 5 weeks of age, have been shown to produce near perfect copies of songs which they hear only fifteen times. This study also found that if the nightingales hear ten or fewer repetitions, they do not learn the song accurately (Hultsch and Todt, 1992). Because increased numbers of hearings of a song have been demonstrated to increase the accuracy of nightingale singing, there is further evidence that birds learn their song from an outside source rather than being born with the ability to sing. If birds were born with the ability to sing, the number of repetitions the bird hears would not affect the bird's ability to sing a song accurately.

Based on research of birds raised in isolation, birds that are deafened, and the number of repetitions required for a bird to learn a song, it would seem that Suzuki was correct. That is, it appears that birds do in fact need to learn their species' songs from an outside source, and they are not just born with the ability to sing their species' song. However, some research poses problems with this conclusion.

The Problem of Species Specific Song

The first issue to complicate the idea that birds are not born with their song, is that birds of a particular species tend to learn the song of their own species and not the song of other species. Studies in laboratories have shown that birds, when presented with a tutor bird of another species will learn the song of the tutor. However, when birds are presented with recordings of various species of birds, they pick out the song of their species and only learn that song (Soha, 2004). Birds can identify the song of their species though the use of auditory cues. For example, the White-crowned sparrow's (*Zonotrichia leucophrys*) song always begins with one or two distinctive whistles. When one creates a recording of the White-crowned sparrow's song, but edits out the opening whistle, another White-crowned sparrow will not learn this song. Similarly, if one adds the opening whistle to the alarm call of a ground squirrel, the white-crowned sparrow has been observed to learn this vocal combination. These effects were demonstrated in birds who were tutored at about two weeks of age. Each of these birds heard 100 repetitions of the songs daily for 60 days in both the autumn and the following spring (Marler and Soha, 2000). The aforementioned research, which demonstrates that birds have inborn knowledge of their species song, suggests that at least part of the bird song learning process is inborn, perhaps a result of genetic inheritance, rather than learned.

The Problem of Social Interaction

Another issue which forces one to question whether birds learn their song only from outside sources is the way in which birds learn to sing through social interaction. While birds raised in isolation do not learn to sing normally, some studies have questioned whether or not this is a result of an absence of an auditory model, or simply an absence of social interaction.

European starlings, (*Sturnus vulgaris*) when raised in isolation, do not develop normal song. However, when the starlings are raised in pairs with no exposure to adult song, they still develop a close approximation to adult song (Chaiken et al., 1997). The starlings even developed normal song when raised with a companion who was devocalized (Chaiken, 2000). Thus it seems that at least some birds do have an innate ability to develop their song and simply require social interaction to produce the song.

A similar effect has been observed with the Brown-headed cowbird (*Molothrus ater*). There are two subspecies of the Brown-headed cowbird which live in two different geographic regions and have two distinct songs. When young male Brown-headed cowbirds are raised in pairs with a female of the other subspecies, they do not learn their own species' song, but rather the song of the female's subspecies. This is of particular interest because the females do not sing (King and West, 1983). It is thought that the females train the males through visual displays, such as a wing stroke, which elicit particular vocal responses in the males, innate socio-sexual responses that are reinforcing stimuli (West and King. 1988). While the males still need training, the sounds they produce must have an inborn component. It would be highly unlikely that the cowbirds produce the song that elicits a response in females purely by trial and error with random sequences of sounds. It must be that the birds have some sort of inborn knowledge that guides them to likely choices. Plausibly this would be related to inherited physiological or neurological features of a species. (This is similar to the idea of generative grammar theorized by Chomsky. Chomsky observed that children learn grammar despite few instances of stimuli which suggests that inborn neurological features guide children to make certain choices. See Chapter 8).

It should be noted that the case of the starlings and the cowbirds are unusual. There are few other birds that have been observed learning proper adult song without some sort of auditory model. However, the cases of the starlings and the cowbirds suggest that one should use caution before making the assumption that all other birds learn simply by hearing vocal models. Similarly a bird's ability to select his own species' song out of the multitude of sounds around him (even when the bird is not able to see the bird producing the sounds, for example if a recording is used) also suggests that part of birdsong learning is inborn. The most likely scenario for most birds, it seems, is that both inborn tendencies and external auditory modeling are involved in the process by which a bird learns to sing. As Catchpole and Slater write, "Most people who study behavior now accept that behavior patterns cannot just be labeled as innate or learned but arise through an intricate interplay between the two. Studies of bird song have done more than any other studies to foster this realization." (2008: p. 50).

It seems, regarding whether Suzuki was correct in what he says about bird song, the answer is that he was only partially correct. He is correct in assuming that birds do have a sensitive period in which they learn to sing. He is also correct in assuming that many birds do need some sort of auditory model to learn to sing. However, it seems likely that Suzuki was incorrect in assuming that birds learn to sing purely through environmental stimuli rather than some sort of inborn ability. We can thus conclude that Suzuki's use of the Japanese Bush warbler as an example to prove that musical ability is not inborn is faulty.

The Comparison of Bird Song to Human Music

While Suzuki's example of the bird song is faulty, its fallacy does not necessarily disprove his original idea that human musical ability is not inborn but is created by environmental stimuli. In order to disprove Suzuki's argument, one would need to show that comparing the way birds learn to sing is comparable to the way humans learn music. This is a matter of much debate which remains unsettled.

Human musical ability is a topic that has fascinated scholars for a long time. However, it is nearly impossible to empirically investigate certain aspects of human musicality. This is because it is nearly impossible to truly control many aspects of musical exposure in young children in an effective or ethical way. In contrast, birds can easily be raised in isolation, or with specific other birds. Additionally, birds can be played only certain recordings, and purposefully denied others. They can be raised in soundproof chambers so that they do not hear any sounds from the outside world. Lastly, they go from a stage of total non-ability to one of full ability in only a year, unlike humans who take many years for both speech and music acquisition. The ease with which one can experiment with birds rather than humans provides a strong incentive for us to confirm the similarity between bird song acquisition and the acquisition of human music abilities. One can run many experiments with birds in the same time and with the same resources as one experiment would take with humans. Thus if the similarity were valid, we could learn more about human music learning in a shorter amount of time and with fewer resources if we used birds.

Hultsch and Todt (2004) review several reasons one might think to compare bird song to human speech. All of the arguments they use to compare bird song to human speech can also be made to compare bird song to human music. Hultsch and Todt note that both bird song and human speech involve learning. Both rely on auditory perception, memorization, and imitation of sound patterns. In both bird song and human speech, perception precedes production (with the few exceptions already mentioned). In both cases, early stages of life are the best times to start learning. Lastly, birds and humans seem to have a similar process to memorize auditory material known in the research as "chunking." That is, birds and humans divide series of sounds into short sections in order to memorize them.

However, there are many who point to a number of problems in using bird song to understand human music. For example, McDermott and Hauser (2005) claim that using bird song is unhelpful in understanding human music. McDermott and Hauser argue that bird song serves a biological purpose while human music does not. Bird song is used almost exclusively as a part of selecting mates and marking territories, which serve as means to ensure the survival of the species. By contrast, human music does not often serve any specific functional purpose that directly affects natural selection but rather is performed or listened to as a pastime. Furthermore, in the overwhelming majority of species of birds only the males sing and not the females. This is also different from human music. McDermott and Hauser also note that if one were to cut out song in birds, many species would be destroyed. If one were to cut out music from the lives of humans, they could still go on living. This has been shown to be the case with people with congenital amusia, who live otherwise normal lives except that they have difficulty in producing or perceiving music (see Chapters 10 through 12 for a discussion of whether or not congenital amusia is truly congenital). Lastly, McDermott and Hauser note that from an evolutionary perspective, none of the closest ape relatives to humans has music.

One might suggest that the bimanual drumming of apes should be considered music, however this is merely an example of apes using one element of human music. Furthermore, most scholars would not consider this drumming to be "musical," and hence in scholarly literature it is referred to as a "signal" rather than a "song" or other term suggesting music (Acardi et al. 2004 and Schaller, 1963). Furthermore, apes have not been shown to synchronize to a beat, an essential skill in human music. Interestingly, other than humans, only birds have demonstrated this skill (Patel et al., 2009).

While bird song may sound very similar to human music to the human ear, McDermott and Hauser, and those that agree with their position, argue that this is largely a coincidence of nature and not because the two abilities are comparable in a meaningful way.

Conclusions

We have seen that Suzuki's argument that bird song is learned and not inborn is at least somewhat false and we should therefore discredit it. Thus, Suzuki's use of bird song learning as evidence that musical talent is not genetically inherited is not valid. However, we should not necessarily use this fallacy to abandon Suzuki's argument that human musical ability is not genetically inherited. There is strong reason to believe that the way bird song is developed and the way human musical abilities are developed are not comparable, and the idea that humans hear bird song as similar to music may simply be a fluke of nature. In the next chapter I investigate Suzuki's claim that it is the environment and not inborn predisposition that determines one's ability development.

Chapter 6

The Influence of Environment: The Wolf Children

As a sub-claim to Suzuki's foundational principle that music talent is not inborn, Suzuki argued that human ability development is the result of environmental factors and not inborn traits. To argue this point, Suzuki uses the story of two children raised by wolves. These children, according to the story, developed wolf-like abilities such as the ability to see in the dark and to run on all fours faster than humans can run regularly. Suzuki argued that the environment, that of being raised by wolves, not inborn traits caused the girls to develop wolf-like traits. He then further argues that just as the wolf-children developed wolf-like abilities as a result of their environment, humans develop musical ability as a result of their musical environment. Thus, if one creates a superior musical environment, superior musical ability will result.

Suzuki became aware of the story of the wolf-children through reading an essay by Fujimo Kida, published in a periodical called *Jido Shinri (Child Psychology).* (Suzuki, 2012 p. 16, a more complete translation of the Kida text can be found in Suzuki, 1990 p. 15-19) In *Nurtured by Love,* Suzuki tells the story as follows: A priest from India, Father Singh, discovered two children living in a wolf den, northwest of Calcutta. Singh was a priest in India who ran an orphanage. Singh kept a diary detailing his discovery and care of the two girls whom he named Amala and Kamala. In the diary, the girls are described as having many wolf-like characteristics. They ran on all fours, grabbed things with their mouths, preferred raw meat, did not perspire but hung their tongues out and panted, had bent legs that would not stretch out straight, and howled at regular intervals of the night. Amala died shortly after coming to Singh's orphanage. Kamala lived to the age of 16 in Singh's care, during which time he attempted to teach her to behave like a human. However, he was largely unsuccessful.

Doubts Raised Regarding the Wolf-Children

To a modern reader, this story seems ridiculous; however, this story was accepted by many scholars in the academic community. Suzuki noted that professors from both the University of Denver and Yale University (2012, p. 16) considered this research valuable, although Suzuki does not name the professors. The professor Suzuki refers to, from the University of Denver, is likely the anthropologist Robert Zingg. Robert Zingg wrote a book titled *Wolf Children and Feral Man*, the first half of which is a publication of the diary kept by Singh while working with the wolf-children (Singh and Zingg, 1939). The Yale professor Suzuki speaks of is likely psychologist and pediatrician Arnold Gesell, author of the book *Wolf Child and Human Child* (Gesell, 1941). Additionally, in an article from 1951, Wayne Dennis points out three other significant psychological textbooks which accept the story of Amala and Kamala (Anastasi, 1949; Ruch & Leeper, 1948; Sargent, 1950).

Doubts about the story of Amala and Kamala began to surface shortly after the story became known to the public, in the late 1920s. One point of contention for critics concerned the origin of the girls. From earlier correspondence with Singh, it appeared the girls were brought to him at the orphanage (Benzaquen, 2001). Only later did Singh claim that he was actually present when the girls were removed from the wolf den (Dennis, 1943). Other doubts surfaced because of a report from Sarbadhicari, who was a physician who attended the girls. In his report, Sarbadhicari makes claims similar to Singh's regarding the mental state of the girls (Quoted in Kellog, 1934). However, the abnormal physical state of the girls, as claimed by Singh, is absent.

If the girls had abnormally shaped legs, did not perspire, and had other wolf-like physical features one would expect these traits to be noted in the physician's report. Further suspicions were raised because of the large number of similar wolf-children stories stemming from India. As Wayne Dennis writes, "Of the 14 reports of wolf-children, 12 have come from India, where the myth of a child reared by animals is very widespread" (Dennis, 1941).

In 2007, Serge Aroles, in his book *L'Enigme des enfants-loup,* investigated the case of Amala and Kamala using previously ignored sources. In this book he claims the diary of Father Singh was not written while caring for the girls but actually six years after Kamala died. Aroles also concluded that the photographs appearing in the diary, which were taken by Singh, were taken after the death of the two girls and thus must be pictures of different children. Furthermore, according to several testimonies, Father Singh used to beat Kamala in order to get her to act wolf-like in front of visitors. Lastly, there is evidence that Father Singh invented the story of the orphan girls for financial gain, and in actuality the two girls were simply abandoned children (Aroles, 2007, see p. 103-122).

With the benefit of hindsight, it is easy to see the implausible nature of the wolf-children story. However, decades ago, the story was accepted as valid. This was largely because Singh was viewed as a trustworthy man. Furthermore, the diary left by Singh was so detailed, consistent, and thorough that many assumed it to be true. As Robert Zingg wrote, "A three year check of Rev. Singh's diary account of the rescue and lives of the wolf-children of Midnapore, leads the writer [Zingg] to believe that it is authentic" (Zingg, 1941 p.435).

The Fabrication of the Wolf Children Story and Suzuki's Claim

The false nature of the story of Amala and Kamala leaves the reader of *Nurtured by Love* with a problem. One cannot fault Suzuki for citing this story; many scholars believed it. However, the reader is left with the question of whether the story's fabricated nature invalidates Suzuki's claim. This is further problematic because of the early use of the wolf-children story in *Nurtured by Love*, where it appears on the sixteenth page (the tenth page in the 1983 edition). Placing a false story so close to the beginning of the book may make many readers cast aside the whole book as nonsense. This is particularly problematic because *Nurtured by Love* is the standard text for the Suzuki Method (see chapter 2).

Bruno Bettelheim (1959) wrote an article in which he compares the descriptions of Amala and Kamala with autistic children treated at the Orthogenic School of the University of Chicago. In the article he discusses how some of the children at the school displayed wild-like behavior similar to that of wolves. He therefore concluded that Amala and Kamala behaved as they did, not because they were raised by wolves, but because they had the same form of autism as the children at the Orthogenic School. While compiling case histories of the children at the Orthogenic School who exhibited wild animal-like behavior, Bettelheim concluded that the children who exhibited such behavior came from severely abusive and neglectful environments.

Bettelheim notes that Amala and Kamala also suffered maltreatment. He notes that in Singh's diary, Singh wrote that Amala and Kamala, upon discovery, were kept in a small pen. Singh left the girls in this pen for several days. When Singh returned to the girls, he discovered that the caretakers, with whom he had left them, had deserted them. The girls were left trapped in the small enclosure without food and water. Singh wrote that when he arrived the girls were emaciated. The girls were then transported in narrow confinement for seven or eight days. The maltreatment was so extensive that Singh noted himself, “they were so weak and emaciated that they could not move about” (Singh & Zingg, 1939 p. 12). Just like the wild autistic children of the Orthogenic School, Amala and Kamala perhaps behaved wildly as a result of the maltreatment they suffered. As Bettelheim concluded his article:

> To Sum up: Study of the so-called feral children, and comparison of them with known and well-observed wild autistic children, suggests strongly that their behavior is due in large part, if not entirely, to extreme emotional isolation combined with experiences which they interpreted as threatening them with utter destruction. It seems to be the result of some persons’-usually their parents’-inhumanity and not the result as was assumed, of animals’-particularly wolves’-humanity. To put it differently, feral children seem to be produced not when wolves behave like mothers but when mothers behave like non-humans (1959 p. 467).

More recent research has corroborated Bettelheim’s conclusion that maltreatment can result in wild behavior in those diagnosed with autism. In a 1994 case study, Edwin H. Cook and colleagues present a case of a child diagnosed with autistic disorder who suffered physical abuse from a worker in his residential school. When the child’s parents removed him from the school they noticed many changes in his behavior.

> They reported that he often became angry when he talked about being hit; he became agitated and appeared frightened. He reported fears of returning to the school especially of having to go back to the school dormitory. He awoke frequently at 3 or 4 A. M. and became frightened of the dark. He developed increased rocking and had crying spells during which he would lie on the floor for up to an hour and not allow his parents to touch him although previously he was able to be comforted by them (pp. 1292-1293).

The state of the child described here bears similarity to the behaviours used to describe the wolf-children, particularly, waking in the night, lying on the floor, and an inability to be comforted by human contact.

In a 2011 study, Mehtar and Mukaddes describe 69 children with autistic spectrum disorder. Of these 69 children, 18 experienced abuse or witnessed abuse which led to post traumatic stress disorder. Of these 18 children:

17 showed aggressiveness and anger bursts (94.4%)

17 showed distractibility (94.4%)

17 had sleep disturbances (94.4%)

16 showed agitation and restlessness (88.9%)

16 regressed in social interaction (88.9%)

14 had an increased activity level (77.8%)

13 had appetite disturbances (72.3%)

13 experienced self-care skills loss (72.3%)

These characteristics show resemblance to the descriptions of the state of the wolf-children. Based on the findings of Mehtar and Mukaddes as well as the case study of Cook and colleagues, it seems child abuse and neglect have the potential to change autistic children into wolf-like children.

Abuse, neglect, and child maltreatment have harmful effects not only on autistic children, but on non-autistic children as well. Child maltreatment has been shown to have adverse effects on children's physical, cognitive, emotional, and social development. Maltreated children tend to be aggressive and wild. Maltreated children are more likely to perform poorly academically and commit crimes. They tend to experience depression and other emotional problems and are more likely to commit suicide. Studies have shown that maltreated children are more likely to experience sleep problems, eating disorders, drug abuse, and physical aggression when they become adults. The effect of maltreatment on children is often severe and long lasting.

(English, 1998) This research indicates that maltreatment affects not only autistic children, but non-autistic children as well.

However, from the results of Mehtar and Mukaddes, one might hypothesize that the effect of maltreatment on children with autism disorder may be greater than that of non-autistic children. This idea is supported by a 2009 study in which Kumagami and Matsuura analyzed juvenile court cases in several family courts in Japan for prevalence of pervasive developmental disorder. Pervasive developmental disorder (PDD) is the term used to describe five disorders, of which, autism is the most common. Kumagami and Matsuura found that of the 28 juveniles with PDD in their study, 21.4% had experienced recurrent physical abuse and 25% of them had experienced recurrent emotional abuse. Non-PDD juveniles had a rate of only 11.8% for physical abuse, and 10.7% of emotional abuse. Additionally, 17.8% of juveniles with PDD suffered neglect by the parents in contrast with only 8.6% of non-PDD juveniles. Despite the small sample size, Kumagami and Matsuura's observations suggest that not only does abuse and neglect play a role in developing criminal behavior in children, but also it may be a more significant cause for children with PDD. As Kumagami and Matsuura point out, "Over half of the cases [of juveniles with PDD] involved adverse environmental factors" (p. 983).

Kumagami and Matsuura present a possible problem for Suzuki's example of the wolf-children. From the reports of Bettelheim, Mehtar and Mukaddes, and Cook one could conclude that the wolf-children may have been the way they were because of their environment. However, from Kumagami and Matsuura, one might conclude that environmental factors were more likely to have an effect on autistic children than on non-autistic children. If the tendency of the environment to affect these two groups of children differs, it is problematic to consider what happened with one group to explain what happened with the other. Therefore, if we assume

Bettelheim is correct, and the wolf-children were autistic, and they behaved wolf-like as a result of their environment, the story of the wolf-children may not be transferable to our understanding of the way that normal children develop. Thus, the example of the wolf-children that Suzuki uses to demonstrate how all children are affected by their environment would not be a valid example for all children--only children who are autistic.

The Effect of the Environment: Other Examples

The case of the wolf-children presents a major problem for Suzuki's arguments in *Nurtured by Love.* In *Nurtured by Love*, Suzuki argues that it is the environment that shapes a person and not inborn or inherited factors. There are other examples Suzuki provides of the environment shaping one's character. However, they too prove to be problematic.

For instance, Suzuki discusses a boy who is brought by his father, Mr. Y (Mr. Y is called Mr. X in the 1983 edition), to play the violin for him, and the boy shows all of the same mannerisms and playing style as Mr. Y. Suzuki explains that it is the child's environment, that of living and observing Mr. Y constantly, that has made the boy develop in this way. In another case, Suzuki claims he rectified Koji Toyoda's undesirable behaviors through environmental influences. Suzuki claimed Toyoda developed undesirable behaviours by spending time in a sake bar with his uncle. Rather than using scolding, Suzuki had everyone in the house model desirable behavior. Without any instruction, only by instituting the correct environment Suzuki claims he changed Koji's undesirable behavior. These two examples presented in *Nurtured by Love* are further examples of environments causing human development. However, in both the cases other factors that may affect human development cannot be ruled out. In the case of Mr. Y. and his son, clearly one cannot rule out genetic inheritance as a factor since the two people involved are closely related. In the case of Koji Toyoda, one might argue that his behaviour changed purely as a result of maturation rather the environment.

Conclusions

The story of the wolf-children is the only case presented by Suzuki where it is clear that it is the environment that has had a drastic effect on child development. Since the story has elements that were likely fabrications, Suzuki is left with little evidence that human development is determined to such an extreme extent by environmental factors. However, Bruno Bettelheim and subsequent later research has shown that while the story of the wolf-children was not one of children being raised by wolves, the children described may have been the way they were, at least in part, because of environmental factors such as physical and emotional abuse or neglect. Research has shown that abuse and neglect, both environmental factors, can have a significant effect on a child's development. However, research has also provided evidence that environmental factors may affect autistic children more than normal children. And thus the effect of the environment on Amala and Kamala cannot be generalized to normal children. While Suzuki is correct in that the environment affects human development, there is no reason to suspect this effect is as strong as Suzuki maintains. In the next chapter, I evaluate Suzuki's claim that anything can be trained with repetition.

Chapter 7

Any Skill Can be Mastered with Repetition: Handedness

As a sub-claim to the foundational claim of the Suzuki Method that musical talent is not inborn, Suzuki argues that any skill can be trained through repetition. As he wrote:

> Through concerted repetition, we are able to master and turn into ability whatever we wish to learn. (2012 p. 118)

One argument Suzuki utilizes to support this claim is the development of handedness, whether one is right- or left-handed. Suzuki argues that one is not born right-handed or left-handed instead handedness is trained through repetition. That is, one is right-handed or left-handed as a result of repetitive use of one hand. Furthermore, Suzuki claimed one can be trained to be ambidextrous if their non-dominant hand was made to perform the same tasks as the dominant hand with many repetitions:

> Who is it that develops your ability? Ability does not develop innately. It is up to you. Each one of us is responsible for developing our own selves. Don't lament your lack of ability without trying to cultivate that ability. Your left hand is far inferior to your right hand if you have allowed it to stay idle. Even though they both belong to you, your left and right hands reveal a tremendous difference between the one that receives training in the form of everyday use, and the one that does not. This applies as well to your ability as a human being. It is folly to consider a talent innately flawed when you have done nothing to develop it. Look, now, at your right hand. If you train yourself daily the way you have trained your dominant hand, strength will be generated, sensitivity will be fostered, and ability will gradually develop. The ability possessed by your right hand, which surpasses your left, was not created by someone else. You have created it yourself. When you were born, neither hand was superior or inferior. Why then do they function so differently? No ability is ever demonstrated innately. Indeed, ability is created only when you make efforts to develop it. Your right hand, for one, knows that you yourself are the creator of your abilities. How, then, did the

> extraordinary ability of your right hand come into being? It must have developed through repetition. To become an exceptional human being requires the same approach. If you neglect a talent you have acquired, it will not improve. Repeat what you have achieved to ensure that you can still succeed at it, try it once again with even better results, and further repeat it with yet more impressive results . . . It is only by reinforcing your skills that you will see continual improvement (pp. 53-54).

Handedness is a topic which Suzuki discusses in many of his writings. In addition to arguing that handedness is something which is trained, Suzuki also argues that the teaching of children to be ambidextrous is beneficial. He argues that ambidextrous people have an easier time with tasks such as manual labour and playing the piano:

> I think raising children to be ambidextrous would be best. It is preferable to be able to use both hands freely. I know one lady who is ambidextrous. When she does needlework she sews from the right until she tires and then sews from the left. She can even cut radishes into long, thin, pieces freely from either left or right. If parents would raise their children to be ambidextrous it would be very handy and efficient…. Even for practicing the piano I think that what is necessary is to train the left hand for an extended number of hours so that both left and right hands can play with the same power (1981, p. 50-51).

Furthermore, developing each hand to its best ability conforms to Suzuki's general goal for life of developing one's ability to the fullest (see chapter 3).

What is of particular interest with this argument, is that Suzuki acknowledges that most people will not agree with his claim:

> Do not rely on common superstitions. The reason I have spoken so much about left-handed and right-handed is because I think that by looking at something from a new angle, one can have the excitement of discovery and of grasping a new truth. (1981 p. 51)

In this chapter, I will investigate Suzuki's claims regarding handedness. Using earlier research I will look at similar claims made by scholars before Suzuki. I will then, using more contemporary research, evaluate his claim that handedness is merely caused by repetition and is not an inborn trait. Lastly, I will determine whether there is evidence that it is in beneficial to be ambidextrous as Suzuki maintains.

Early Handedness Research

Suzuki was not the first to preach the ability to teach ambidexterity or the advantages of doing so. In early 20th-century England, the topic of ambidexterity was much discussed. At that time there were two prevailing theories on the development of handedness. One theory, posited by James Sawyer (1900), was that right-handedness had its origins in battle. Because the heart, the organ most necessary to protect, is on the left side, it followed that warriors would hold their shields with their left hand. It then follows that the sword would be held with the right hand, resulting in the right hand becoming more agile and skillful. It was believed that this development of the right hand was then passed down to future generations through "imitation and by the hereditary transmission of an acquired peculiarity" (p. 1302). (It is interesting to note that Sawyer does not use Darwin's theory of natural selection, which Darwin posited some 40 years earlier, as an explanation for the transmission of right-handedness. This is particularly striking since Sawyer suggests that right-handedness was developed as a means of success in battle, or in other words survival.)

The other prevailing theory of that time was that handedness was a result of the unbalanced weight of the body's contents. A physician named Sir John Struthers, while dissecting the human body, found that the contents on the right side of the chest and abdomen were heavier than those on the left. Because of this difference in weight, he theorized that the right leg, which carried more weight, would gradually become stronger than the left leg. The superior strength of the right leg that developed as an infant begins to spend more time erect, it was believed, resulted in preferential use of the right hand as well (Smith, 1900).

Both of these explanations imply that handedness is not entirely inborn. E. Noble Smith (1900) added that education is also a driving force in handedness:

> Most careful observers of children agree with Sir John Struthers that the very young are often inclined to use the left hand as much as the right, but the teaching of mothers and nurses is always to encourage them to prefer the right. (p. 580)

Because it was believed that handedness developed over time, it was postulated that ambidexterity could be trained. Many argued that this training should take place in the public schools. In Philadelphia, Professor J. Liberty Tadd attempted to do this (Smith, 1900). Tadd's teaching philosophy shows remarkable similarity to the Suzuki Method although he focused on the visual arts rather than music (Tadd, 1899).[4] Tadd, and his English contemporaries, believed that ambidexterity provided advantages in manual labor, sports, and music. They also believed that ambidexterity enhanced brain function, and that its training led to greater academic success (Smith, 1900). It was further believed that ambidexterity training could be used to treat hemicrania and hemiplegia (Sawyer, 1900) and would prevent the development of deformed spines (Smith, 1900).

In 1935, Wayne Dennis published a study utilizing a set of twins. The twins were raised in a carefully planned manner such that the caregivers would not influence the development of handedness. Dennis found that in spite of this, both infants developed right-handedness. This would suggest that handedness is not socially transmitted. Dennis also noted that while both

[4] In an annual report to the Board of Education in Philadelphia, Tadd noted that his work had become very influential in Berlin. He stated that his writings had been translated into German, and that the president of the Institute of Elementary Education in Berlin said that his work was changing the methods of Education in Germany (Baker, 1988). It is conceivable that Suzuki became aware of Tadd's work and writings during his eight year stay in Berlin.

children mostly preferred their right hands, for certain activities they had a preference for the left hand. These activities were not the same for both infants, one infant preferred its left hand for some activities while the other infant preferred its left hand for different activities. Since these children were raised in the same manner, one designed to prevent social transmission of handedness, it seems that handedness is not developed socially or by the environment, but rather is something inborn or a result of genetic inheritance, albeit seemingly subject to individual differences (Dennis, 1935). Dennis's study provides evidence against Suzuki's claim that handedness is a result of the environment. However, it should be noted that Dennis's study was limited to only two infants. Furthermore, it is difficult to believe that it is possible or ethical to raise children in an environment that would be entirely uninfluenced by caregivers' handedness.

Current Research

Current research is still inconclusive regarding the causes of handedness, and whether or not it is inborn or a result of training. In a frequently cited article from 1981, George F. Michel argues that handedness is the result of the shape of the uterus while the fetus is developing. Because of the shape of the uterus, the movement of the fetus's head and one hand is restricted. This results in asymmetrical development of the cerebral hemispheres. This in turn causes the child to become right-handed or left-handed depending on which side is restricted and which side has freedom of movement. Michel's theory supports Suzuki's argument that handedness is not something innate to the person, but rather is something that is developed as a result of the environment: in this case, the environment the fetus experiences in utero. This study also provides a defense for Suzuki's claim against Dennis's findings. Perhaps, the infants in Dennis's study developed the way they did because of the shape of the uterus in which they grew. So while handedness was supposedly not socially transmitted in Dennis's study, it was still the result of the environment, the environment the fetus experienced in the womb. However, Michel's theory does not eliminate the possibility that the training of ambidexterity is possible, provided the necessary brain development has not stopped by the time the child is born.

Another influential theory of handedness was proposed by Marian Annett, in 1985. Annett proposed a genetic model of handedness where handedness is determined by one gene with two alleles. One allele is dominant and selects right-handedness (RS+) and the other is recessive (RS-) and selects for both right-handedness and left-handedness. The three possible combinations are: RS+RS+ strongly right-handed, RS-RS- left-handed, and RS+RS- right handed. Annett argues that the recessive trait has survived because of the cognitive advantages of people who are RS+RS-. This genetic model of handedness argues against Suzuki's theory, with the exception of the proposed disadvantage of being strongly one-sided. However, Annett's claims have failed to be confirmed by more recent studies (Johnston et al., 2009).

Studies with Mice

To explore the cause of handedness, some have turned to studying animals, in particular mice. Mice have an observable front paw preference. That is, given a food-retrieval task, mice will tend to use one paw more frequently than the other. In 1985, Robert L. Collins attempted to breed mice for a right-paw preference and a left-paw preference. He was unsuccessful at doing so, which led him to conclude that paw preference and by extension handedness was not a result of genetic inheritance. However, Collins was able to breed for degree of dominance. Meaning, Collins was able to breed mice specifically who were strongly one-sided, slightly one-sided, or had no preference, but could not breed the mice to prefer specifically the right or left paw. This led Collins to conclude that while right- or left-handedness is not a result of genetic inheritance, being one-sided or two-sided is the result of genetic inheritance.

Collins's research supports Suzuki's ideas. Suzuki argues in *Nurtured by Love* that ability is not inherited but rather developed as a result of environmental stimuli. However, Suzuki acknowledges one caveat to his theory, noting that one can be born with different speeds and sensitivity to adapting to one's environment. Suzuki claims that this is the one developmental factor which is inborn. (See Suzuki, 2012 p. 20) This idea is consistent with Collins. Collins showed that he could breed mice for one- or two-sidedness but not for a specific side. One could speculate that the mice bred for one-sidedness were bred for having a greater speed of adjusting to the environment. That is, these mice were stimulated to use one paw rather than the other and more quickly developed a preference for that paw. In contrast, the mice that were two-sided did not respond as quickly to environmental stimuli, and thus did not develop a strong preference for one paw or the other.

In a 1993 study, Pascal Barneoud and Hendrik Van der Loos were successful at breeding mice for paw preference. That is, they were able to breed mice that preferred specifically the right paw or left paw. However, they were only able to do so when paw preference was coupled with an asymmetry of the whisker pad. That is, when the whisker pad was shaped asymmetrically, the mouse showed a preference for the paw on the larger side of the whisker pad. Barneoud and Van der Loos end their article with two possible conclusions. They suggest that either paw preference is a result genetic inheritance, and the gene for paw preference is linked in some way with the gene that determines the shape of the whisker pad, or paw dominance in mice is a result of the shape of the whisker pad. That is, the whisker pad acts as a stimulus to prefer a paw. However, if the other paw were trained intentionally using some other stimulus it would still develop as the preferred paw. This hypothesis is quite plausible, but was not tested. Thus, depending on which conclusion is correct, Barneoud and Van der Loos would either affirm Suzuki's idea of handedness being the result of training or reject it.

Ambidexterity Training

There is no conclusive answer to the question of whether environmental or inherited factors determine handedness. However, a search of the internet suggests that the general population believes that ambidexterity can be trained regardless of whether one was born with a one-sided hand dominance. There are many articles written for a general audience on training oneself to become ambidextrous. For example, *WikiHow* has an article titled "How to Be Ambidextrous" (Viren et al., 2014) and similarly the website *Instructables* has an article titled "Train Yourself to be Ambidextrous" (Rain, 2014). These articles and other similar ones all advocate constant repetitive use. They argue, as Suzuki did, that through constant training and repetitions, one can train the weaker hand to be as agile as the dominant one.

There is empirical research regarding the training of dexterity in the hands of patients who have suffered strokes or other brain injuries. Many studies have demonstrated ways in which patients have both recovered use of their dominant hand and developed dexterity in their previously non-dominant hand. One such study is that of Sabine Schneider and colleagues (2010). Their study has two important conclusions relevant to Suzuki's work. First, they concluded that the important factor in training a hand for fine motor skills is the hand's repetitive use, rather than the restriction of the other hand. This suggests that the training of ambidexterity is possible, and not just switching the dominance of the hands. Second, Schneider and colleagues have found that motivation is an important factor in developing skill. They found music-supported training to be more efficient than functional motor training: specifically, when patients experienced some sort of pleasure through training, they are trained more efficiently. This supports Suzuki's idea of inculcating the child with the desire to be trained, before beginning training. (See chapter 2)

In a 2005 study, Judith Palac and David Sogin also found that repetitive use could increase hand dexterity. In their study, Palac and Sogin tested right-handed Suzuki students who played string instruments for dexterity in their left hand using a task unrelated to string playing. They found that the more musical experience the string students had, the greater the dexterity they had in their non-preferred hand. This not only suggests that training dexterity in the non-preferred hand is possible to a significant extent, but it also shows that the training of one ability in the non-preferred hand leads to greater ability in other activities. This supports Suzuki's idea of "ability begets ability" (Suzuki, 2012 p. 11, see chapter 2). However, Palac and Sogin's study lacked a control group. So while their study found a significant difference in non-preferred hand dexterity, it is difficult to rule out other factors that might have resulted in this dexterity. Perhaps, non-preferred hand dexterity simply improves with age, and is not the result of training. Thus before one can conclusively determine the extent to which string playing increases hand dexterity, one must create a study where the participants are matched with controls to rule out other factors.

Ambidexterity Training May Not Be Beneficial

In addition to claiming that it is possible to train ambidexterity, Suzuki also argues that it is beneficial. However, recent research suggests that ambidexterity may not actually be advantageous. In a study by Lesley Rogers and colleagues (2004), it is argued that lateralized function in the brain is beneficial. Rogers and colleagues bred chicks such that some chicks would be one-eye dominant while others would be bred to use both eyes equally. One might hypothesize that having equal ability in both sides would be advantageous, particularly when considering an aspect of a chick's life such as fleeing from predators or finding food. However, the study found that chicks with lateralized vision function, while they had the disadvantage of being one-sided, had the advantage of being able to do two different tasks simultaneously better than the chicks which were not one-sided. The lateralized chicks in the study were able to look for food and monitor their surroundings for predators at the same time. Rogers and colleagues argue that this is perhaps why vertebrates have evolved to be lateralized in function, even though one might speculate, as Suzuki did, that being equal on both sides would be preferable.

In human studies, ambidexterity has also, in contrast to Suzuki's theory, shown to be a disadvantage. Many studies have found that children with mixed-handedness have academic problems. For example, Alina Rodriguez and colleagues (2010) found that children with mixed-handedness were more likely to develop language difficulties and ADHD. Similarly, David Johnston and colleagues (2009) found that children who showed no hand preference were at a significant cognitive disadvantage.

Most relevant to Suzuki's claim is a study by Shiro Mori and colleagues (2007). This study tested right-handed, left-handed, and mixed-handed preschool children in a bi-manual tapping task designed to investigate auditory-hand coordination. Children were instructed to tap two plastic switches one with their left hand and one with their right hand along with a metronome. Participants performed this task eight times. First they tapped both hands together to four different metronome speeds: 1.0 Hz, 1.5 Hz, 2.0 Hz, and 3.0 Hz then participants were asked to tap using alternate hands, first one hand than the other, to the same four metronome speeds. Mori and colleagues found that right-handed children performed the best, followed by left-handed children. Performing the worst were children determined to be mixed-handed. This study is relevant to Suzuki's claims as one would assume that children who struggle with bi-manual control combined with an auditory stimulus would also struggle with piano playing. This contradicts Suzuki, and many other proponents of ambidexterity, who claim one of ambidexterity's advantages is ease in piano playing.

The chick study by Rogers and colleagues is consistent with the study of pre-schoolers by Mori and colleagues. Rogers and colleagues found that chicks without lateralized eye-function had more difficulty finding food and monitoring for predators, doing two tasks at once. Similarly, it was found that children whose hand function was not lateralized had trouble doing two tasks at once: tapping the switches at the appropriate times meanwhile attending to the auditory stimulus. It seems from the above research that Suzuki and other promoters of ambidexterity were incorrect. That is, ambidexterity does not give the person an advantage but rather a disadvantage.

However, there is a fundamental problem in comparing the aforementioned research and Suzuki's claims. The research discussed above does not take the cause of handedness into consideration. This is likely because, as mentioned earlier, the cause of handedness is not fully understood. If we assume Suzuki's claim to be true, that is, handedness is trained through repetitive use or stimulation, we can explain the above research not in terms of handedness but in terms of stimulation. The mixed-handed children in the research, according to the Suzuki, would most likely be those children who have had little stimulation for use of their hands. One might then posit that these children were more likely to not have had as much stimulation in other areas of their experience as well. According to Suzuki's theories, these children would have cognitive disadvantages and other learning problems. These children have not had stimulating environments and therefore are not truly mixed-handed but rather no–handed. That is, they are not ambidextrous by virtue of superior function in both hands; rather, they are ambidextrous in that they have underdeveloped function in both hands. As Johnston and colleagues acknowledge:

> Although hand preference may develop in the first year of life, the rate of development is variable. The children with mixed-handedness may therefore be developmentally delayed in relation to their hand preference and cognitive ability, and this has little to do with laterality per se (Johnston et al., 2009 p. 297).

Modern research has shown a correlation of dexterity and academic achievement. This gives further support to the idea that the ambidexterity in the aforementioned studies is actually a lack of ability rather than an enhanced additional ability. For example, in a 2011 study, Pieter Vuikk and colleagues assessed children with learning disabilities using the *Movement Assessment Battery for Children* (MABC). The study found significant correlations between total MABC scores and spelling and mathematics abilities. Similarly, Jose Morales and colleagues, in a large study involving 487 children, showed that results of a Tower of Cubes test, a test designed to test advanced motor skills, correlated significantly with scores on tests for linguistic and mathematical ability. This led them to conclude that advanced motor skills are associated with better academic performance. This link of motor skills with academic achievement may be explained by a child's being raised in a particular environment. The child with good motor skills is likely to have been raised in a stimulating environment and therefore is more likely to be advanced in academic achievement. Similarly, a child who was not stimulated, is likely not to have advanced dexterity and also not likely to succeed academically. Such research suggests that we should not completely dismiss Suzuki's advocacy of ambidexterity training.

In order to truly test whether or not the ambidexterity Suzuki advocates is beneficial, one would need to perform a longitudinal study in which children are trained through repetitive use of their weaker hand and then assessed. To my knowledge, this type of study has never been done, and might well be ethically dubious.

Conclusions

With regard to Suzuki's claims concerning handedness, we are still unable to draw clear conclusions. Current research has not reached a conclusion regarding the cause of handedness. It is therefore impossible to say whether Suzuki is correct in claiming that repetitive use of the dominant hand is the cause. It is nonetheless clear that training dexterity in the hands, and in particular the non-dominant hand is possible. This is best done through repetitive stimulation of that hand in an environment in which there is strong motivation to do so, as Suzuki advocates. Whether all normal children can attain true ambidexterity is unknown, but it is clear that some certainly have attained competence with their weaker hand. Furthermore, training the non-dominant hand in one skill has been demonstrated to improve another skill in the non-dominant hand. All the same, there is evidence that ambidexterity, contrary to Suzuki's claim, is not necessarily beneficial. However, further research is still needed regarding whether trained ambidexterity, as opposed to ambidexterity resulting from a lack of stimulation, is beneficial. In the next two chapters, I discuss Suzuki's mother-tongue argument. That is, if you replicate the way in which children learn to speak their native language, you can teach them any skill to the same level of mastery with which they speak their native language.

Chapter 8

The Mother Tongue Argument

Suzuki claimed that all children learn to speak their native language with ease. As such, if we replicate the process by which they learn their native language, we can teach all children to do anything, including music, with ease. Early in Suzuki's teaching career, he was asked to accept a very young pupil. While pondering how he would approach such a task he had the following epiphany:

> Children freely speak Japanese, I realized, because they are, in effect, given the opportunity to do so. There is the fact of training and education behind their capacity to speak. Therein lay a proper educational method. Every child, without question, is developing appropriately. This was precisely that perfect educational method I was seeking (2012 p. 7).

In the following two chapters, I explore the validity of Suzuki's mother tongue argument. In this chapter I examine Suzuki's claim that "every" child develops the capacity to speak through the "training and education" which Suzuki claims are in place. I then determine in what ways the Suzuki Method recreates the process of speech acquisition in order to teach music. In the next chapter, I examine whether speech abilities can be generalized to music abilities and vice versa. By undertaking these investigations, I determine the extent to which Suzuki's mother tongue argument is valid. It is important to note that the mother-tongue argument, after the foundational principle that talent is not inborn, was the one that Suzuki felt most important. In addition to Suzuki's aforementioned epiphany serving as the original impetus for developing the Suzuki Method, Suzuki often referred to his method as the "mother tongue teaching method." (p. 71)

The Meaning of "Every Child"

Suzuki claims that all children learn to speak, but it is clear that some children do not acquire verbalization skills. Research indicates that a small percentage of children do not learn to speak due to hearing loss, physical damage to their speaking apparatus, acquired brain damage, autistic disorder, various mental deficiencies, and a variety of other problems (Bishop, 2006). Therefore, Suzuki must be wrong. However, the assumption that Suzuki's use of "every child" was literal may be an erroneous supposition.

It is fair to assume that when Suzuki writes "every child," he is colloquially referring to the overwhelming majority of children. It is probable that, because the percentage of children with the above conditions is so small, Suzuki discounted them. However, accepting this assumption opens the possibility that Suzuki, while discounting those with disabilities, was also discounting those born with extraordinary abilities. While this would appear to be a logical theory, Suzuki does not seem to exclude those born with extraordinary ability, as he wrote:

> There is no innate talent called genius; genius in an honorific term given to human beings who have been fostered to achieve magnificence (p. 28).

Suzuki did not believe that children were born with talent, even those appearing to be particularly gifted. As such, it is evident that when Suzuki refers to "every child," he is including noticeably gifted children, but likely excluding those with evident impairments. Consequently, while Suzuki's argument that "every child is developing appropriately" (p. 7) appears to be unsound, understanding "every child" as explained above, resolves the irregularities in this statement.

Evidence of Environmental Factors in Language Impairment

Suzuki claims that children learn to speak as a result of education and training, or in other words a proper environment. Thus, a question arises regarding his mother tongue argument, namely, can children have a language impairment that is unrelated to improper training? Though Suzuki claims that every child learns language as a result of education and training, psychologists and educators have documented cases of children who have experienced unique difficulties in developing normal speech and language skills. This is a condition referred to as specific language impairment (SLI). The term SLI is applied to someone with a spoken language impairment that cannot be explained as a result of brain damage, hearing loss, damage to the speaking apparatus, autistic spectrum disorder, or more general learning difficulties (Bishop 2006). If we assume that Suzuki's assessment of speech education is accurate, then we must assume that those diagnosed with SLI did not develop appropriate spoken language skills because they did not receive the proper education and training, or in other words the proper environment in which speech learning occurs.

Certain studies suggest that SLI is a result of environmental factors. For example, Fundudis and colleagues (1979) found that children diagnosed with SLI generally come from a lower socio-economic status. Additionally, Bishop (1997) found significantly higher rates of SLI in children who are among the younger children of larger families. The researcher hypothesized that the younger children in a large family do not get as much parental interaction at the early stage of life as their older siblings experienced, and as a result they do not develop language skills as well as their older siblings. Likewise, Tomblin and colleagues (1991) found that parents of children diagnosed with SLI had significantly fewer years of formal education than parents of children with normal language abilities. From these studies one might conclude that Suzuki is correct; language learning occurs when there is a normal environment.

Evidence that Language Impairment is Inborn

However, other studies suggest that heritable traits play a role in the development of SLI. Tallal and colleagues (2001) tested the family members of children diagnosed with SLI and matched controls for language impairment and found significantly higher rates of language impairment in families of children diagnosed with SLI than with controls. Their study found 27.3% of mothers of children diagnosed with SLI had language impairment, compared to 11.5% of controls. Similarly, 31.8% of fathers of children with SLI showed language impairment, compared to 3.9% of controls. Lastly, 31.1% of siblings of children with SLI showed language impairment compared to 6.1% of controls. A family aggregation study such as this one (for a review of other similar studies see Stromswold, 2000) serves as evidence that there is a heritable cause for SLI. However, because families have shared environments, it is possible that the environment is still a factor. As such, family aggregation can only be used as an argument for a genetic cause when grouped with other types of studies.

One of these types of studies is those that examine both identical, or monozygotic twins (MZ), and fraternal, or dizygotic twins (DZ). MZ twins are genetically more similar; therefore if SLI is a genetic disorder one should see a higher rate of SLI in MZ twins than with DZ twins, when one twin has already been diagnosed with SLI. Three studies have found significantly greater concordance rates for SLI between MZ twins than DZ twins. Concordance is the term used in genetics research to describe the probability that two individuals of a defined group, such as twins, will exhibit a certain characteristic, given one of them has the characteristic. Lewis and

Thompson (1992) found a concordance rate for SLI of 0.86 for MZ twins compared to 0.48 for DZ twins. Similarly, Bishop and colleagues (1995) found a concordance rate of 0.70 for MZ twins and 0.46 for DZ twins. Lastly, Tomblin and Buckwalter (1998) found a rate of 0.96 for MZ twins and 0.69 for DZ twins. If one assumes that the difference in environments is the same for both MZ and DZ twins, than it appears that SLI has a genetic, rather than environmental, cause. (However, this may not be a valid assumption. To my knowledge, no empirical study on whether parents raise DZ twins in a more similar manner than MZ twins has been completed.)

Reconciling Evidence for Environmental Causes with Evidence for Inborn Causes for Language Impairment

In 2006, Bishop and colleagues designed a new type of SLI twin study which, instead of looking for rates of concordance between twins of SLI, looked for rates of concordance of different observed aspects of SLI between twins. Bishop and colleagues tested a sample of 173 pairs of six-year-old twins in two commonly observed deficits in SLI. One of the deficits was in phonological short-term memory; the other was the ability to add endings to verbs in order to change their tense. To test phonological short-term memory, the participants were asked to repeat meaningless words of increasing number of syllables. To test the ability to add appropriate endings to verbs, the participants were given two standard tasks in which they had to describe a picture or series of pictures. The pictures were designed to elicit certain verb tenses. The participants' answers were then evaluated for correct or incorrect use of verb tenses. Bishop and colleagues found significantly greater rates of correspondence on both tasks for MZ twins compared to DZ twins, suggesting that both deficits are hereditary. However, the two deficits were only weakly correlated with one and other, suggesting different genes are involved in the two types of deficits. This suggests that while SLI seems heritable, what we observe as SLI may be caused by different factors and different genes, depending on the individual.

Another type of study that warrants investigation are those that deal with family trees. That is, members of several generations of a family are tested for SLI such that one can attempt to trace the inheritance. The KE family in England has attracted much attention by those who study SLI. This family is unique in that there is a family tree of three generations that has been studied (see Vargha-Khadem et al., 1995 for a diagram of the KE family tree). Approximately 50% of the third generation of the KE family is diagnosed with SLI. What is of further interest is that each individual in the KE family who exhibits SLI has a mutation of the FOXP2 gene. The FOXP2 gene is a gene which regulates other genes responsible for the development of areas of the brain necessary for speech development (Fisher, 2005). For example, brain imaging studies of people with the FOXP2 mutation show abnormalities in Broca's area, a part of the brain often associated with language (Vargha-Khadem et al., 2005). While the KE family has demonstrated a link between the FOXP2 gene and SLI, most people diagnosed with SLI do not exhibit a mutation in the FOXP2 gene (Bishop, 2006). This suggests that while genetic inheritance is clearly a cause of SLI in the KE family, it is not always relevant to those diagnosed with SLI.

The twin study of Bishop and colleagues, studies of the KE family, and the contrasting findings of studies arguing for environmental causes versus genetic causes for SLI suggest that language development is a highly complicated process. Involved in the process are both environmental and genetic factors, and likewise the development of a language disorder has both environmental and genetic factors.

The Validity of Suzuki's Claim

As such, it would seem that Suzuki was only partially correct. When normal language education and training is in place, children typically develop normal speaking abilities. When this training and education are not in place, normal speech may not develop. However, there is also strong evidence that there are genetic influences on language development. And thus, sometimes children do not develop normal speech abilities even when raised in a normal environment. Therefore, Suzuki's theory is flawed.

However, perhaps, Suzuki would discount those with SLI. Perhaps when he writes "every" child, as stated earlier he is using a colloquialism to mean most children. If this is the case, than research on SLI may be irrelevant to the examination of Suzuki's mother tongue argument. Furthermore, Suzuki was trying to recreate how normal children develop speech abilities in his method of music education. Thus two questions remain: to what extent is normal speech acquisition innate and does the Suzuki Method actually mimic speech acquisition.

Theories of Speech Acquisition

The extent to which speech acquisition is innate and the extent to which speech acquisition is a result of environmental stimuli is a debate which dates back centuries. For example, Sanskrit grammarians argued over whether the meanings of words are recognized because of a God-given ability, or if it is developed through conventions passed on from previous generations (Matilal, 1990). Speech abilities must be, at least to a certain extent, innate as humans have the ability to speak and animals do not. Thus the question of innateness is really a question of whether humans have an innate capacity for speech which is different from any other learned and uniquely human skill.

Current research is largely based on the spectrum between the two opposing views of Skinner and Chomsky. Skinner believed that speech abilities are developed through operant conditioning. That is, the babbling of a child gradually begins to resemble an approximation to words spoken by the child's verbal community. When the child creates an approximation of a word, the child is rewarded by responses from people in the community. As the child progresses, the child is rewarded only when closer approximations to words are uttered. In this way complex verbal behaviour is established. As Skinner writes:

> Any operant, verbal or otherwise, acquires strength and continues to be maintained in strength when responses are frequently followed by the event called "reinforcement." The process of "operant conditioning" is most conspicuous when verbal behavior is first acquired. The parent sets up a repertoire of responses in the child by reinforcing many instances of a response. Obviously, a response must appear at least once before it is strengthened by reinforcement. It does not follow, however, that all the complex forms of adult behavior are in the child's unconditioned vocal repertoire. The parent need not wait for the emergence of the final form. Responses of great intricacy can be constructed in the behavior of any organism through a procedure illustrated by the following demonstration experiment… [Skinner describes an experiment in which pigeons are trained to walk in a figure eight. He does this by first rewarding any motion, then only rewarding motion in a particular direction, and then withholding reward unless walking in one direction is followed by turning in the other.] In teaching the young child to talk, the formal specifications upon which reinforcement is contingent are at first greatly relaxed. Any response which vaguely resembles the standard behavior of the community is reinforced. When these begin to appear frequently, a closer approximation is insisted upon. In this manner very complex verbal forms may be reached (1957 pp. 29-30).

Through the comparison of speech acquisition to pigeons learning to walk in a figure-eight it is clear that Skinner believes, as Suzuki does, that language acquisition is not innate, at least not more so than any other skill.

Chomsky (1959) opposes this idea. Chomsky, and those who have continued in the same line of theorizing, argue that there are certain aspects of speech acquisition that are innate. They believe this to be the case because of the ease with which children learn to speak like adults, despite a seeming lack of stimuli available for the child:

> The discovery of the richness of the implicit knowledge of language immediately raised the question of acquisition. How can it be that every child succeeds in acquiring such a rich system so early in life, in an apparently unintentional manner, without the need of an explicit teaching? More importantly, the precise study of fragments of adult knowledge of language quickly underscored the existence of "poverty of stimulus" situations: the adult knowledge of language is largely undetermined by the linguistic data normally available to the child, which would be consistent with innumerable generalizations over and above the ones that speakers unerringly converge to… In fact, no normative, pedagogic or (non-theory based) descriptive grammar ever reports such facts, which are automatically and unconsciously assumed to hold not only in one's native language, but also in the adult acquisition of a second language. So the underlying principle, whatever its ultimate nature, appears to be part of the inner background of every speaker (Chomsky, 2002 p. 5-8).

What is particularly interesting about the two theorists mentioned above is that Suzuki's ideas align more with those of Skinner. However, Suzuki had the same observation as Chomsky. That is, both Chomsky and Suzuki observed the apparent ease, despite lack of direct instruction, with which children learn language. Chomsky uses this observation as an argument that speech acquisition has innate components. Suzuki uses this observation as a means to attempt to find better pedagogical practices which resemble the practices in place which facilitate speech acquisition.

Comparing Speech Acquisition and the Suzuki Method

Despite much being written on the subject, speech acquisition, in particular the apparent ease with which children learn to speak and the failure of artificial intelligence systems to learn language, continues to baffle researchers (Kuhl, 2004). One key problem is that it is nearly impossible and ethically dubious to control the environments of children to isolate aspects of speech acquisition. This is particularly difficult as there is evidence that human fetuses can hear in the third trimester of pregnancy (Hauser & McDermott, 2003). Furthermore, while some animals exhibit speech-like behaviors, the similarity may not be enough for any form of valid comparison. Thus using animals to study speech acquisition may not be possible (see chapter 5).

Nevertheless, recent research, while it cannot explain speech acquisition in total, has begun to validate certain elements of speech acquisition. In 2004, Kuhl reviewed research regarding elements of speech acquisition. Interestingly, three of the elements of speech acquisition which have been verified empirically are present in the Suzuki Method theoretically. That is, Suzuki has included them in writing about his method. Whether the method is successful at replicating these elements in practice is a matter for further research.

The first element of speech acquisition which Kuhl addresses, which can also be found in Suzuki's writing, is that of neural commitment. Infants are born with the ability to distinguish many sounds, sounds that adults cannot distinguish. As infants are exposed to their native language, neuropsychological and brain imaging research has shown a commitment of their brain's neural networks to the patterns of their native language. This neural commitment facilitates the learning of patterns that conform to the native language while interfering with patterns that do not conform. This explains why children learn to speak a language with relative ease, while adults struggle to learn second languages.

While empirical evidence to support neural commitment is recent, and Suzuki was not aware of the term "neural commitment," he nonetheless observed the phenomenon. He thus advocated beginning instruction from birth, and advocated the playing of recordings for the child such that they would develop a sensitivity for fine musical artistry. (See Chapter 3; Suzuki, 2012 see pp. 11, 29, and 55) He further speculated that because of early experience one develops intuition or *Kan*, and this intuition helps one develop skills later in life. As Suzuki writes:

> Admittedly, what one person can do after 500 repetitions may require 5,000 repetitions for someone else to accomplish to a similar degree. Seeing the difference in the rate of acquisition between such people, others often discuss the presence or absence of innate intuition, skillfulness or clumsiness. However, they may err in their judgment unless they trace back to the days of each individual's birth, research both of their personal histories, and consider how their present forms of ability have evolved (2012 p. 67).

One could argue that what Suzuki is describing is neural commitment.

In addition to neural commitment, Kuhl also addresses the notion of statistical learning. That is, the sensitivity of learning of sounds "through the computation of information about the distributional frequency with which certain items occur in relation to others, or probabilistic information in sequences of stimuli, such as the odds (transitional probabilities) that one unit will follow another in a given language." (2004 p. 831) Research has shown that infants rely on distributional frequencies, that is, how often particular sounds occur in their environment, in becoming sensitive to the sounds that occur in their native language and losing sensitivity for sounds that do not occur in their native language. (Kuhl et al., 1992)

Similarly, Saffran and colleagues (1996) have shown that infants rely on transitional probabilities in order to learn words. That is, in any language certain phonemes are more likely to follow one phoneme than another. By being sensitive to the transitional probabilities, infants are guided to where new words begin. For example, in English, the probability that 'ty' will follow 'pre' is higher than the probability that 'bay' will follow 'ty.' Using this knowledge infants are able to identify 'pretty' as a potential word rather than 'tybay' when they hear 'pretty baby,' even before they understand what it means (Kuhl, 2004).

Suzuki argued that statistical learning, like that discussed above, is at fault for tone deafness:

> In the case of tone deafness, many children produce overly high-pitched semi-tones; for example, with the note *fa* in the first four notes of the major scale, do, re, mi, *fa*. These children have already been nurtured to hear and produce a slightly high-pitched *fa*. I have come to realize that nothing can be done about this past that has already been nurtured. In other words, this out-of-tune pitch cannot be rectified. It is literally impossible to correct the *fa* of a person who has mastered it at a slightly higher pitch. Then, what can be done to address tone deafness? What I discovered was that although I could not teach tone deaf children to adjust their overly high-pitched *fa*, I could help them learn how to produce anew the correct pitch of *fa*. I decided that with a child who has become tone deaf from hearing the wrong *fa* 5,000 times, I would have her listen to the correct *fa* 6,000 times, then 7,000 times. At first nothing changed, but as she heard the correct *fa* 3,000 times, 4,000, 5,000, and 6,000 accumulated times, her ability to reproduce the correct *fa*, which she absorbed from hearing it 6,000 times, began to overpower her ability to produce the wrong pitch she had learned from hearing it 5,000 times. In short, the newly cultivated, correct function had taken root (Suzuki, 2012 p. 116).

Suzuki notes that it is the high frequency of hearing the “wrong” *fa* which led the child to use this note. The solution to the problem was not one of instruction, but rather changing the distribution of occurrences such that the child would prefer the new correct *fa*.

The last element of speech acquisition which Kuhl addresses, which is embedded in the Suzuki Method, is that of social interaction. Suzuki emphasized social interaction in his method through the training of parents and through group classes (see chapter 2). Kuhl notes that social interaction improves infants’ abilities in tasks of phonemic contrast, and increases the quantity and quality of infant utterances. In 2003, Kuhl and colleagues explored the effects of short term exposure of a foreign language to infants. In their first study they compared the sensitivity of nine-month old American infants to a phonemic contrast found in Mandarin and not English. Half of the infants were exposed to 12 sessions in which Mandarin speakers read books in

Mandarin and talked about toys that they showed the infants. The other half were not exposed to Mandarin. Kuhl and colleagues found that after the twelve sessions, the infants were significantly more sensitive to the Mandarin phonemic contrast than the infants in the control group. To test whether such an effect depends on live interaction, a new group of infants saw the same Mandarin speakers from the experiment on a screen or heard them over a speaker. These infants showed no difference in their ability to discriminate the phonemic contrast from the infants in the control group. The infants who experienced the live Mandarin speakers were significantly better at the discrimination task than both the control group and the group that was exposed to recorded Mandarin speakers. Thus, it seems exposure to the sounds of language is not enough to influence speech acquisition, but rather there may be a need for a live social tutor.

Similarly, Goldstein and colleagues (2003) showed that social interaction has an effect on the quantity and quality of utterances of infants. In their study mother-infant interactions were manipulated after a short period of normal interaction. Half of the mothers were instructed to respond immediately to infant vocalizations by smiling, moving closer, and touching their infants. The other half of mothers were instructed to give the same response to their infants, however, not after vocalizations, but rather at controlled times which corresponded to the timing of responses of mothers in the other group. Goldstein and colleagues found that infants in the first group vocalized significantly more than infants in the other group and their vocalizations more closely resembled adult speech.

In addition to the three elements of speech acquisition discussed above, there are several other observable elements of speech acquisition that are present in the Suzuki Method. For example, in both speech acquisition and the Suzuki Method perception precedes production. That is, children hear language before they can speak, and in the Suzuki Method, children listen to recordings of their repertoire before they can play. Additionally, in the Suzuki Method and in speech acquisition production, precedes reading. That is, children learn to speak before they can read, and in the Suzuki Method, children learn to play before they learn to read music.[5] Lastly, in speech and in the Suzuki Method children use a common repertoire. That is, children in a common verbal community learn to speak the same language, and children who study using the Suzuki Method all play the same repertoire of their instrumental community.

It is clear that Suzuki used his observations of speech ability acquisition to recreate the process in which children learn language in order to teach music. Furthermore, many theoretical elements of the Suzuki Method have an empirically verified presence in speech acquisition. Whether Suzuki actually recreated the process by which children learn to speak and whether this process can be effective in teaching music are still matters of debate. However, Barbara Barber (1993), in an article regarding the differences between Suzuki teaching and traditional teaching, notes that the aforementioned elements of speech acquisition are what set the Suzuki Method apart from other teaching methods. Thus, while the Suzuki Method may not actually replicate the speech acquisition process, its attempt to do so is perhaps its defining characteristic.

Conclusions

Suzuki's mother tongue argument has several flaws. First, it is clear that not all children learn to speak. Secondly, there are cases of speech impairment which seem to have non-environmental causes. That is, children are, in rare occasions, exposed to a normal speech learning environment and yet fail acquire speech abilities. However, if we assume Suzuki is

[5] This aspect of the method is not mentioned in chapter 2 as Suzuki never writes about this idea in *Nurtured by Love.* However, it is an often cited idea in Suzuki related-literature. Delayed reading is also one of the most controversial and criticized aspects of the Suzuki Method. See Behrend, 1998.

using a colloquialism to mean the majority of children this is not problematic. The third flaw of the mother tongue argument is the lack of evidence that speech acquisition does not have innate components as Chomsky argues. Nevertheless, the Suzuki Method does attempt to replicate empirically verified aspects of speech acquisition, and furthermore the attempted replication of speech acquisition is what sets the Suzuki Method apart from other methods. The question still remains however, whether the Suzuki Method actually replicates the process by which children acquire speech abilities or if it is purely a theoretical attempt.   This is a matter of for further research. The first question one must ask in this regard is how similar are the domains of speech and music such that learning can be generalized between the two domains. This question is addressed in the next chapter.

Chapter 9

Speech Acquisition and Music Learning

As discussed in the previous chapter, Suzuki believed if one replicates the way in which children acquire speech abilities one can teach anyone, not just those deemed to be musically talented, to play music at a high level (Suzuki, 2012). The previous chapter found several flaws with Suzuki's mother tongue argument, namely, not all children learn to speak and many theorists believe that some components of speech may be innate and are not developed by the environment. However, the previous chapter found correlates between empirically verified elements of speech acquisition to elements of the Suzuki Method. These correlates provide evidence that Suzuki's method replicates the process by which children learn to speak. In this chapter, I evaluate this idea further by looking for similarities in speech acquisition and music learning and whether there is evidence that skill in one domain can be generalized to the other. In other words, this chapter seeks to determine whether there is reason to use speech acquisition as a model for music education as Suzuki advocates.

The Similarity of Music and Speech

Spoken language and music are similar in that they involve one person creating organized sounds that are meant to be perceived by either the same person or another person, or both. In both music and speech, discrete elements (tones in music and phonemes in speech) which have little meaning or effect on the listener by themselves, are combined in numerous ways to create structures of complex meaning or effect. However, this similarity is not enough of a basis on which to validate Suzuki's idea that teaching music in the same way that a child learns to speak will yield superior results.

Suzuki's theory that music and spoken language are similar was not unprecedented. This idea was considered by scholars for centuries. Plato, Vincenzo Galilei, Jean-Jacques Rousseau, and Ludwig Wittgenstein all wrote about the relationship between music and language (Patel, 2010). Charles Darwin even went as far to suggest that the original form of human communication was a hybrid between music and modern speech (Darwin, 1871). Ideas about the similarity between music and spoken language range from the simple observation of sonic similarities to highly speculative ideas. Aniruddh Patel (2010), for example, has suggested that language and music are similar in that they define us as being human. Patel argues that the phenomena we categorize as music and language are universal to every human culture, but other phenomena that one might think are fundamental to human life, such as counting or visual arts, are not.

One can argue that music and spoken language are comparable in that, as George List (1963) writes, it is not always easy to distinguish speech from song, a form of music. As List notes, some cultures do not even make a distinction between the two genres. List uses examples from several cultures such as Sprechstimme, Palau Intonational Recitation, and the speech used by tobacco auctioneers as forms "intermediate to speech and song" (p. 9). He proposes a two-dimensional classification system. One dimension is the amount of variability in pitches used. The second dimension is the amount of structure presented by those pitches. List's model uses these dimensions to serve as a means of classification for where in the song-speech spectrum an intermediate form lies. List notes, however, that as sound analysis techniques improve more complex models regarding the classification of speech-song genres can be developed. Lastly, he writes that using pitch as a basis of classification is not the only, and not necessarily the best criteria for classification. However, for the purposes of this chapter it suffices to conclude that song and speech must be similar because of the difficulty of distinguishing one from the other. If one considers song a form of music then it follows that speech and music are not always distinguishable. Furthermore, an inability to distinguish the two domains suggests that the two are processed similarly and thus the acquisition of abilities in the two domains are likely similar.

However, one might argue that while forms intermediate to speech and song draw on elements from both music and speech, these elements are processed, produced, and perceived in different ways. (Some argue this is the case because brain damage has been observed to cause the loss of speech abilities without causing damage to musical abilities (aphasia), and vice versa (amusia) (Peretz and Coltheart, 2003). Furthermore, while Suzuki wished that his educational philosophy be applied to other subjects, he was concerned with violin pedagogy. While creating a clear boundary between speech and song may be challenging, distinguishing violin playing from speech is less difficult. Thus in order to evaluate Suzuki's mother tongue argument, I largely consider the similarity of speech acquisition with non-verbal music learning (instrumental music or vocal music without lyrics). I divide speech and music into several elements. For each element I evaluate whether there is evidence that the acquisition of skill in one domain can be generalized to skill in the other domain.

Pitch

Pitch is perhaps the most observable musical element that is also used in speech. While humans are capable of speaking in a monotone, they seldom do so. When one speaks in a normal conversational manner, they use differences in pitch to communicate an array of things. These variations of pitch are not random, but rather are highly structured in order to communicate linguistic, attitudinal, and emotional information. For example, consider the phrase “It’s cold outside.” This statement could be interpreted as a question, “It’s cold outside?” or a statement, “It’s cold outside.” The way in which the listener determines whether or not this phrase is a question or a statement is by the pitches used by the speaker. When the phrase is spoken as a question, there is an upward pitch glide at the end of the phrase (this is colloquially referred to as “up speak”). Using pitch to indicate whether an ambiguous phrase is a question or a statement is only one of the many ways we use variable pitch in speech (for an analysis of how English speakers use variation of pitch in their speech see Cruttenden, 1997.)

As stated above, some researchers theorize that the presence of people with amusia without aphasia, that is, brain damage that has affected musical abilities but not speech capabilities, is evidence that music and speech stem from distinct cognitive processing systems. However, a case study by Karen Nicholson and colleagues (2003) questions this notion. This case study investigated the speech perception abilities of a man, referred to as K. B., who suffered a right-hemisphere stroke and subsequently lost his musical abilities. The study used 12 ambiguous phrases, phrases that could potentially be interpreted as either a statement or a question, such as, “He speaks French.”, or “He speaks French?” Each phrase in this study was presented twice, differing only in the pitches used on the last syllable or word. The phrases were constructed so that they were acoustically identical until the final syllable or word. The waveform of the final syllable or word was edited such that the timing, amplitude, and perceived intensity were the same. The only remaining cue the participant could use to determine whether the phrase was a question or a statement was pitch (see Patel et al., 2008 for a more thorough description of the stimuli used and other studies which use similar methodology). 12 control participants all performed consistently on this task with a mean of 94.8% correct responses and the lowest score was 87%. In contrast, K. B. scored significantly lower than the control participants (more than three standard deviations below the mean). Because disability in pitch perception in one domain coincided with a pitch perception disability in the other, one suspects that ability in both domains would coincide as well. Research by Daniele Schön and colleagues (2004) has shown this to be the case.

Schön and colleagues tested a group of musicians and a group of non-musicians for pitch perception abilities in both language and music. For music, the participants were presented with familiar melodies, but with the last note of the melody sometimes altered by raising it either a semitone (100 cents) or 1/5th of a tone (40 cents). For language, a recording of spoken phrases was used, but in some of the recordings the fundamental frequency of the last spoken phoneme was raised by either 35% (520 cents) or 12% (196 cents). In both the language and music tasks participants were asked to push one of two buttons depending on whether they determined what they were hearing was normal or abnormal.

In both the musical and speech perception tasks, when there was a greater alteration of the usual tone or the fundamental frequency of the last syllable, both groups had little trouble identifying the abnormality. In the music task, when the alteration of the final fundamental frequency was increased only by 1/5th of a tone, the musicians were significantly better at discerning the abnormality. Most importantly, the musicians also significantly outperformed the non-musicians in the speech perception tests. These results suggest that training in one area leads to an increased ability in the other area.

However, this study neglects the issue of whether people with better pitch perception are more likely to become musicians. To address this problem, Sylvain Moreno and colleagues (2009) designed a longitudinal study with 32 eight-year-old children who had never received musical training. They randomly divided the students such that half were given music lessons while the other half were given painting lessons, instruction the researchers deemed equally interesting to the students. The music training the children received was a combination of Orff, Kodály, and Wuytack methodologies, and involved imitation and improvisation of rhythms, imitation and improvisation of melodies (whether this was vocal or instrumental is not specified in the article), as well as ear training that included recognition of different timbres, chord progressions, and form. After six months of training, the children were given the same tests used in Schön and colleagues' study. Moreno and colleagues found similar results to Schön and colleagues. Both the musically trained children and the children trained in painting responded equally well to the stimuli that had larger differences from the usual fundamental frequencies. However, the musically trained children significantly outperformed the painting students on both the music task and the speech perception task when smaller alterations were used. Moreno and colleagues also measured event-related potentials (ERPs) of the children, while performing the tasks. Before training, the ERPs of the two groups showed no significant differences. However, the amplitude of the ERPs, (specifically the N300 of the midline electrodes, Fz, Cz, and Pz) during both the music task and the speech perception task showed significant differences from pre-intervention testing for the musically trained students but not for the students who studied painting.

The above study provides evidence to support Suzuki's claim that the acquisition of music and speech abilities are similar. To further assess the validity of Suzuki's claim, it would be advantageous to design a study similar to that of Moreno and colleagues, but instead of using music training, substitute instruction in a speech-discipline involving the use of pitch, such as public speaking or drama. For example, Michael McCallion (1988 see pp. 94-111) created a series of exercises to assist actors and public speakers in using pitch to speak effectively. If these exercises were employed over the course of a similar time period as the music training in Moreno and colleagues' study, perhaps one would observe a similar effect. That is, those trained in public speaking would improve their pitch perception in both speech and music. If the domains of speech and music are similar and the exercises created by McCallion are affective, then this should be the result. While this type of study is warranted, it has never been done. There is reason, however, to believe that a study like the one suggested above would yield different results than Moreno and colleagues' original study because of the differences in pitch use in speech and music. In speech, pitch generally involves only larger intervals (Xu and Xu, 2005). Furthermore, while pitch is highly structured in music, meaning each pitch is heard in a context of what precedes and follows it, pitch in language bears less of an exact relation to what precedes or follows it (Patel, 2010). Additionally, pitches in music are often heard in the context of other pitches that are played simultaneously. Lastly, there is a difference between speech and music in the proportion of syllables and tones in which the pitch changes continuously or is repeated. (See Cruttenden, 1997 for examples of notated spoken English as a means of comparison.) Therefore, a study which explores the effect of speech training on pitch perception in music would help to ascertain whether one can generalize the abilities of music and speech.

The only research, to my knowledge, which explores the generalization of pitch perception in speech to music is that which deals with absolute pitch and tonal languages. Absolute pitch (colloquially called perfect pitch) is the term used to describe the ability to accurately label a pitch without a reference pitch. That is, one hears a pitch in isolation and knows what it is. Tonal languages refer to those languages in which the same sound produced at different pitches has different lexical meaning. Diana Deutsch and colleagues (2009) tested a group of 203 music students from the Thornton School of Music (USC) for absolute pitch. They divided the participants into four groups: those that were very fluent in a tonal language, those that were fairly fluent in a tonal language, those that were not fluent in a tonal language but were of East Asian or Vietnamese decent, and Caucasians who were not fluent in a tonal language (fluency was based on self-report). The last two groups were utilized as a means to control for genetic influence. That is, by including a group that is ethnically the same as the first group but with no fluency in tonal languages one can determine if absolute pitch is a genetically based ability rather than one facilitated by tonal language learning.

To test for absolute pitch, Deutsch and colleagues presented participants with a series of 36 pitches and participants were instructed to write down the names of the pitches. The pitches were presented in three blocks of twelve with a short break between each block. The pitches used as stimuli spanned three octaves, from C (131Hz) to B (988Hz). Each pitch heard was more than an octave away from the previous pitch in order to minimize the ability to use the previous pitch as a reference for the next pitch.

Deutsch and colleagues found that all of the participants who reported speaking tonal languages very fluently scored between 90% and 100% correct on the task. Furthermore, the group of participants who spoke a tonal language very fluently scored significantly higher than all of the other groups. The group of participants who spoke a tonal language fairly fluently scored significantly higher on the task than the two groups that did not speak a tonal language. A regression analysis showed that tonal language fluency was a significant predictor of scores on the task. Early onset of musical training was also found to be a predictor of scores, however this was not found to interact with the language effect. One might think that these results could be explained by the country in which the participants' music education took place. For this reason Deutsch and colleagues divided the very fluent tonal language speakers into three groups, those that were born in the United States, those that arrived before age nine, and those that arrived after age nine. Deutsch and colleagues found no significant differences in performance on the task between these groups suggesting that the country in which music education took place was not a factor.

The results of Deutsch and colleagues demonstrate that fluency with a tonal language is correlated with absolute pitch. This supports the hypothesis that learning a tonal language facilitates the acquisition of absolute pitch. This is an example of experience in speech and speech perception affecting absolute pitch, a skill considered to be advantageous to musicians. However, it should be noted that while absolute pitch seems like an ability that would be advantageous to musicians, most professional musicians do not have this ability. (Deutsch et al, 2009) Thus the classification of absolute pitch as a musical ability warrants some hesitation.

Pitch perception disability coincided when people were tested in both domains. Similarly, pitch perception ability coincided as well. Furthermore, from the longitudinal study, there is evidence that training in music leads to better pitch perception in speech. Lastly, people who speak tonal languages were more likely to have absolute pitch, and thus pitch perception ability acquired from speech was generalized to the domain of music. Thus, with regard to pitch, it seems Suzuki was justified in creating a method of music education based on speech acquisition.

Rhythm

In addition to pitch, rhythm also plays an important role in speech perception. For example, the role of rhythm in poetry is a concept that has been studied since the time of the ancient Greeks. However, poetry, while we may classify it as linguistic, has some musical elements. List (1963) might classify poetry as an "intermediate form of speech and song." The notion that rhythm plays a role in regular conversational speech is a more contentious idea, and research on this idea only dates back to the 1940s.

Kenneth Pike (1945) was, perhaps, the first scholar to introduce the idea that spoken language has rhythm. In his seminal work, *The Intonation of American English*, he proposes that languages fall into two distinct categories: stress-timed or syllable-timed. In stress-timed languages, for example English, Pike proposed that the time intervals between each stress in a phrase remain relatively consistent. Similarly, in a syllable-timed language, such as Spanish, the time between each syllable onset remains constant. The idea that all spoken languages fall into one of these two categories is appealing to those looking for similarities between music and speech. If spoken language functions this way, the onset of syllables in syllable-timed languages and the onsets of stresses in stress-timed languages would resemble beats in music. This way of dividing spoken languages is of further interest because the division has no bearing on the historical development of the languages. For example, languages with diverse etymological histories, such as English, Arabic, and Thai, are all classified as stress-timed languages.

However, Pike's theory has a major flaw. Empirical studies have failed to confirm Pike's idea of isochrony, equal rhythmic division of time in speech, with regard to both stress-timed and syllable-timed languages. Rebecca M. Dauer (1983), for example, showed that the time interval between stresses in English grows as the number of syllables increases. Additionally, Mary Beckman (1982) showed that morae, a Japanese system for word division similar to syllables, are not of equal duration in Japanese as was previously believed. Finally, Peter Roach (1982) found that based on the duration of intervals between stresses, English, Russian, and Arabic, languages formerly classified as stress-timed, could not be distinguished from French, Teluga, and Yoruba, languages formerly classified as syllable timed.

The absence of isochrony in speech presents what seems like an insurmountable problem when comparing rhythmic elements of music and speech. Isochrony is a recurrent aspect of rhythm in music. Predictability of beats in music facilitates playing together in groups and dancing. Isochrony also facilitates certain musical effects such as syncopation. Lastly, because there is a foundation of isochrony in music, violating that isochrony can be used as an expressive effect as in a ritardando, accelerando, or rubato.

While the existence of isochrony in speech has been empirically falsified using research that focuses on speech production, research that utilizes perception complicates the matter. In 1977, Ilse Lehiste proposed that periodicity in perceived speech may be greater than periodicity in spoken language. She tested this idea by asking listeners to identify the longest and shortest inter-stress interval (ISI) in a spoken phrase of four ISIs. The participants then did the same task using a non-speech analog where stresses were replaced by clicks and the speech by noise. The participants performed far better in the non-speech analog. That is, in the spoken phrase they failed to identify which ISIs were the longest and shortest implying that they perceived the ISIs as being more equal in the spoken phrase than in the non-speech analog. Lehiste thus concluded that while spoken language is not isochronous, perhaps we nevertheless perceive it as such.

This idea was further supported by Andrew Donovan and C. J. Darwin (1979). Donovan and Darwin asked participants in their study to listen to English sentences and then replicate the timing of the stress pattern of the sentence by tapping. Participants also did the same task in response to a sequence of noises that shared the same timing as the stresses in the spoken phrase. It was found that participants tapped with much less variability, in other words closer to an isochronous manner, when imitating the spoken phrase rather than the sequence of noises. This is further evidence that people perceive speech as isochronous.

Patel (2010) however, has pointed out that the above studies may be flawed in their design. It could be that the observed periodicity of the tapping is affected by the complexity of the stimuli. The spoken phrases contain more information and thus are more taxing on one's memory. Perhaps it is easier to remember the nuances of timing in the sequence of noises compared to the spoken phrases, and therefore, the timing for the imitation of the spoken phrases is more periodic. Thus, more research which controls for this factor is warranted before making a conclusion regarding the perceived isochrony of spoken language.

Perceptual studies have provided evidence that rhythm in speech and music may be similar. Research investigating the rhythmic abilities of those with language impairments further supports this idea. In 2011, Martina Huss and colleagues conducted a study which was based on the theory that musical rhythm and the rhythm of speech are similar. Huss and colleagues hypothesized that because rhythm in the two domains is similar, at least in terms of perception, people with language disabilities would exhibit problems in the perception of musical rhythm. To test this hypothesis they tested whether dyslexic children would show problems in rhythmic perception. It should be noted, that dyslexia is usually associated with reading difficulties, and thus one might argue is not relevant to the current discussion which focuses on speech. However, recent research has found that children with dyslexia also have difficulties with both speech production and perception (Snowling & Stackhouse, 2006).

Huss and colleagues tested 64 children aged eight to thirteen. 33 of these children were diagnosed with dyslexia. 16 were aged-matched control participants, and the remaining 15 were reading level matched control participants. That is, they were children who were younger in age than the dyslexic children but had the same reading level. The participants were presented with 36 trials of melodies which were played at a rate of 120 beats per minute. The melodies were three measures long in 4/4 or 3/4 time, and contained half notes, quarter notes, and eighth notes. All of the melodies contained only a single repeated pitch, and one beat in each measure was intensified by 5 db. This beat was different for each trial, but the same beat for each measure was intensified within one trial. Each melody was played twice. In half of the trials, the melody was played identically both times, and in half of the trials one note was lengthened by adding either 100 or 166 milliseconds. Participants were asked to determine whether the two melodies they heard were the same or different. It should be noted that the paradigm used by Huss and colleagues was designed to test the perception of musical metre. I am not convinced that the methodology does this, as there is more involved in the perception of musical metre than the presence of an accented note indicating a grouping of notes. However, I do believe that the methodology of Huss and colleagues is one which tests rhythmic perception abilities, at least in one aspect, that of a note that is played too long.

Huss and colleagues found the children with dyslexia performed significantly worse on every variation of the rhythmic task presented above than children without dyslexia who were the same age. However, the dyslexic children performed equivalently to the children who were at the same reading level on the task. This is particularly interesting because Huss and colleagues, when selecting control participants, matched them with the dyslexic children for non-verbal IQ. This rules out the possibility that the dyslexic students would perform significantly worse on any task than the age-control participants. Because language disability predicted poor rhythmic perception there is evidence that rhythm abilities in the two domains are similar.

In a study based on the same theory that musical rhythm and speech rhythm abilities are similar, Kathleen Corriveau and Usha Goswami (2009) tested the rhythmic entrainment abilities of children with Specific Language Impairment (SLI). Corriveau and Goswami use the term rhythmic entrainment to refer to the ability to tap a steady beat along with a metronome, and to continue that steady beat when the metronome is turned off. In their study, Corriveau and Goswami tested 63 children. 21 of these children were diagnosed with SLI and were approximately 10 years of age, 21 were age-matched controls, and 21 were language ability matched controls who were approximately eight years of age. In the task, children were presented with a metronome and asked to tap along with the metronome. They were further asked to continue their tapping when the metronome stopped. The children alternated tapping with the metronome for 30 seconds and without it for 30 seconds for three minutes. The task was repeated three times, each time with a different metronome speed: 1.5, 2, and 2.5 Hz.

Corriveau and Goswami found that the children with SLI performed less well on the tapping tasks with the metronome than both control groups and this effect was observed to be greater at the slower speeds. When the metronome was turned off, all of the children performed poorly, and thus little effect was found. Only in the slowest speed was the effect found to be significant. Nevertheless, children with SLI showed a deficiency in rhythmic entrainment. Interestingly, this ability to synchronize with a beat, while long thought to be only a human ability, has been observed in other animals, albeit, only those with advanced vocal learning (Patel et al., 2009). This may be further evidence that musical rhythm and language rhythm abilities have similarities; however, whether one can compare the vocal learning of animals to that of humans is debatable (See chapter 5).

While studies that deal with speech production present a problem to those who wish to compare musical rhythm with rhythm in speech, studies of perception and studies of those with language disorders suggest a closer link than one might expect. Therefore, there is reason to believe that Suzuki was right to compare speech learning to music learning at least with regard to rhythm.

Timbre

Rhythm and pitch, one could argue, are an essential part of music perception, but a less essential part of speech perception. One can speak in a monotone, with little variation in timing, and yet still convey complex information. However, if one were to eliminate all timbral contrasts in speech, one would not be understood. If one cannot tell the difference between various vowels and consonants, one will have trouble communicating. This difference, one would think, makes timbre a promising area for speech-music comparison. However, while there is much research on timbre in speech, there is very little research which explores how timbre in speech relates to timbre in music.

One place to look for speech-music timbral similarities is in the widespread practice of using speech cues or vocables to teach musical concepts. The practice of using speech cues to teach music is one that is used by many musical communities throughout the world (Patel, 2010). For example, Suzuki teachers regularly use words to teach the rhythms found in the Twinkle variations which are the first pieces studied by Suzuki violin students (Ebin & Rahn, 2011). There is to my knowledge only one study which seeks to compare the timbre of the speech cues to the music which they seek to facilitate. Patel and Iverson (2003), have explored the vocables used by the Tabla drummers of Northen India. They recorded six professional Tabla players in speaking the vocables and playing the corresponding sounds on their drums. The main aim of the study was to explore whether the vocables of the Tabla drummers are associated with their corresponding drum sounds by convention, or if there exist timbral similarities between the two sounds.

Patel and Iverson found that there are similarities between the sounds of the vocables and the sounds produced by the drum hits. For example, for the vocable "Dha," Patel and Iverson found that both the vocable and the drum hit produced a two-stage acoustic structure with the first stage showing a dominance of low frequency energy. (For spectrograms and waveforms of the two sounds see Patel, 2010 p. 65-66). Furthermore, by contrasting four pairs of similar vocables such as "Tin" and "Tun," Patel and Iverson found similar differences between the vocables and the drum sounds. "Tun" has a lower overall spectral centroid than "Tin" in both the vocable and the drum sound.

The findings of Patel and Iverson demonstrate that people are able through speech to reproduce timbral differences found in music. This suggests that the perception of timbre between the two domains has similarities. An interesting way to further this connection would be to study whether people with language disorders show impaired perception of timbre in music. For example, would people with language disorders be less able to tell the difference between two instruments of similar sounds, or between two types of drum hits on the same drum? This type of question, I believe, has promise for future research in the similarities between speech and music, but to my knowledge has not been done. However, one must acknowledge that the vocables employed by tabla drummers may not be purely speech but rather an intermediate form of speech and song. Nevertheless, a comparison of the pedagogical methods of tabla drummers and Suzuki teachers may prove interesting as both use spoken language as a basis for educational practices.

Emotional Expression

Another place to look for similarities between speech and music is in the way each domain communicates emotions. Research on speech perception has demonstrated that listeners use non-verbal cues of a speaker to decipher the emotional state of the speaker. For example, Klaus Scherer and colleagues (2001) recorded German actors reading nonsense sentences made up of meaningless syllables. The actors spoke the sentences in five different ways, one which was neutral, and the others in ways which expressed anger, fear, joy or sadness. Scherer and colleagues then took their stimuli to nine cities throughout the world and had listeners determine the emotional state of the speaker. They found that listeners from other countries were able to determine the emotional state of the speaker, despite not being able to understand the text which was said. The idea that music elicits emotions through nonverbal cues is one that has been long established. For example, 2000 years ago Plato wrote in *The Republic* that certain musical modes elicit certain emotions. If we consider this idea combined with the findings of Scherer and colleagues, it leads to the question of whether the way music conveys emotion and the way speakers convey emotion in speech are similar.

In a comprehensive review of 104 speech studies and 41 studies of music performance, Patrik Juslin and Petri Laukka (2003) sought to answer this question. In their review, Juslin and Laukka found that listeners were fairly accurate in deciphering the emotions portrayed by both speakers and musical performers when the choices of emotion were limited to five categories: happiness, sadness, anger, fear, and tenderness. Juslin and Laukka then explored whether the music and the speech which portrayed these emotions had acoustic similarities. They found considerable similarities between the two domains. For example, for anger, Juslin and Laukka found that both speakers and musicians used: fast tempo, high intensity/sound level, much intensity variability, much high-frequency energy, high pitch levels, much pitch variability, rising pitch contours, fast voice onset/tone attacks, and micro-structural irregularity. (For a chart of similar cues used in music and speech to convey the emotions explored, see table 11 of Juslin and Laukka (2003)). Juslin and Laukka further found that in both speech and language the cues are used probabilistically and continuously, such that the cues are not reliable by themselves but must be used in combination with others.

The use of sonic cues to communicate emotional meaning in music and language, has been shown to be similar. Thus there is further reason to suspect that Suzuki's idea that the two can be learned in the same way is correct. As with timbre, research which investigated whether people with language impairments would decipher emotional content in music differently from people without language impairment is warranted. One would suspect given the above research that responses from those with language impairment would be more random as they would not be able to perceive the sonic cues which those without language impairment use to determine the emotional meaning of music. Should this be the case, there would be further evidence in support of Suzuki's theory.

An Overlap of Cognitive Resources

Comparing various elements of speech and music has yielded evidence that Suzuki is correct; there are many similarities between speech and music and thus it is likely that they are learned in similar ways. There is further evidence for this idea from studies which seek to find an overlap of cognitive resources in the perception and production of both domains.

Stefan Koelsch and colleagues (2005) found an overlap of cognitive resources used in both domains through the use of an event-related potential (ERP) study. In their study, participants read words of a sentence while a chord progression played. Only one word was presented on the screen while each chord was played. The sentences were five words in length and were in German. Three different types of sentences were utilized. In one type the sentence was syntactically correct with a high cloze probability, meaning the last word was a word one would expect (for example, "He drinks the cool beer."). In the second type the sentence was syntactically correct, however the final word had a low cloze probability, meaning the final word, while it was possible, would not be what one would expect (for example, "He sees the cool beer."). The last type of sentence was one that finished with a high cloze probability but was syntactically incorrect. In this study, this type of sentence was created by a gender disagreement between the last noun and both the prenominal adjective and the definite article. Each of these three types of sentences was then presented with two types of chord progressions. One progression was a typical five chord progression found in tonal music ending on the tonic chord. In the second type, the progression would finish with an unexpected chord, an unprepared Neapolitan sixth chord. In summary, each of the three sentence types was presented twice, one time with each of the two types of chord progressions.

In linguistics research that utilize ERPs, sentences that violate syntax through gender violations have been shown to elicit a left anterior negativity (LAN) and sentences that conclude with a low cloze probability have been shown to elicit an N400 (a negative-going deflection that peaks approximately 400 ms after the onset of the stimulus). If language and music are processed by distinct cognitive systems one would expect that these responses to the stimuli would remain unchanged. However, Koelsch and colleagues found this not to be the case. When sentences contained a gender violation, the amplitude of the LAN was significantly reduced when accompanied by the irregular chord compared to when it was accompanied by the tonic chord. This suggests that language and music share cognitive resources. This evidence is particularly strong because the language stimuli were presented visually and the music stimuli were auditory, suggesting that the overlap in cognitive resources goes beyond the resources required for processing sounds. If one performed a similar experiment that used speech perception rather than reading one would suspect an even greater effect.

One might argue that it was not the unexpected conclusion of the chord progression that affected the LAN elicited by the gender violation, but rather an acoustic anomaly. That is, any deviant sound might have the same effect. To rule out this possibility, Koelsch and colleagues ran a second study using the same three types of sentences, but the auditory stimuli were replaced with a series of tones. To mimic the normal chord progression, the same tone was repeated five times. To mimic the irregular chord progression, a different pitch was used for the last tone. In this experiment, no difference in the LAN was observed.

In contrast to the LAN caused by the gender violation, the N400 elicited by the low cloze probability remained unchanged whether the sentence was presented with an irregular chord progression or one that concluded on the tonic chord. This appears to be evidence that music processing and the processing of semantics in language do not overlap. This is what one might expect given the lack of or at least diminished semantic content of music compared to speech. (This question will be addressed further below.)

In a behavioral study, Evelina Fedorenko and colleagues (2009) also found that speech perception and music perception share cognitive resources. Fedorenko and colleagues had 60 undergraduate students, naïve to the purpose of the study, listen to sung sentences. The sentences either contained a subject-extracted relative clause (such as "The clerk that liked the boss had a desk by the window") or an object-extracted relative clause (such as "The clerk that the boss liked had a desk by the window"). The sentences were sung to a composed melody with one syllable per quarter note. The melodies each contained 12 notes, all quarter-notes except the last note which was a half-note, sung at 120 beats per minute. Sometimes this melody contained only notes in the key of the melody and sometimes the melody contained a pitch that was out of the key of the melody. This out-of-key-note always occurred on the sixth note of the melody, and always on the last word of the relative clause (the out of key note would fall on the word "boss," and "liked" in the example sentences above). Following the singing of each sentence, the participant had to answer a yes or no question about the sentence, for example, who did what to whom.

Previous research has shown that subject-extracted relative clauses are easier to process than object-extracted clauses. In the musical domain, previous research has shown that melodies that contain an unexpected note are more cognitively taxing than melodies with only notes in the key. Thus Fedorenko and colleagues argue if music and language utilize different cognitive resources, one should see a similar deficit in processing object-extracted clauses in both types of melodies. However, if music and language share cognitive resources, one should observe a stronger deficiency in the processing of object-extracted clauses in the melodies which contain an out-of-key note, as both domains would be competing for resources. One possible objection to this design is that any auditory anomaly in the melody of the sung sentence might produce a similar result. For this reason, Fedorenko and colleagues also included a melody similar to the melody in which an out-of-key note was used, except instead of changing the note in the melody, they increased the intensity of the note by 10 dB.

Fedorenko and colleagues found, as expected, that in all three types of melodies used, participants had significantly more trouble processing object-extracted clauses, as evidenced by more incorrect replies to questions about the content of the sentence. What is interesting, however, is that the observed deficiency in processing object-extracted clauses in the melody which contained an out-of-key pitch was far lower than the other two types of melodies. When the melodies contained only pitches in the key of the melody, or contained one note that was louder than the others, there was no observable difference in the processing abilities of the participants. This suggests that speech and music perception share cognitive resources.

Further evidence for shared cognitive resources between the two domains comes from neuroimaging studies. For example, Steven Brown and colleagues (2006), using positron emission tomography (PET), found that people use many of the same areas of the brain for speaking sentences as they do for singing wordless melodies. The participants of this study were five men and five women who were all strongly right-handed and had no history of psychiatric or neurological illness. The participants underwent PET scans while completing two tasks, one which required them to produce sentences and one which required them to produce wordless melodies. For the sentence task, the participants were presented with the first half of a sentence and asked to complete the sentence. For example, the participant would be presented with a phrase such as, "August was the best month for them to take the Spanish course in Peru because…" and might then generate the completion, "Peru was a great place to be that time of year, and the weather was just fine" (p. 2794). For the melody generation task, the participant would hear a four-measure antecedent phrase ending on a note of the dominant chord, and was asked to improvise and sing a consequent phrase using the syllable /da/.

Comparisons of the PET scans from the two different tasks revealed activations of nearly identical brain areas. These included: the primary motor cortex, supplementary motor area, Broca's area, anterior insula, primary and secondary auditory cortices, temporal pole, basal ganglia, ventral thalamus, and posterior cerebellum. While there was a slight difference in the lateralization for the two tasks, with the sentence task favoring the left hemisphere and the melodic task favoring the right, many of the activations for both tasks were bilateral with significant overlap.

Brown and colleagues' study is strong evidence that music and language are processed and produced using similar areas of the brain, and therefore it is likely that there are strong similarities in the way the two domains are learned. Brown and colleagues' study design is particularly noteworthy because it utilizes both perception and production; however, this design necessitates the use of participants who have had at least some musical training. This is problematic because the musician's brain and the non-musician's brain shows differences (Schlaug, 2001), and therefore one may not be able to generalize the results of the study to the general population. To minimize this problem, Brown and colleagues claim to use "amateur" musicians as their participants. However, these participants were all university music education students, and therefore referring to them as amateurs may be a matter of debate. Surprisingly, these participants only had a mean of 5.3 years of formal music training, a number lower than what one might expect. So perhaps there is more reason to generalize the results to the population at large.

Acquired Amusia and Aphasia

Behavioral, ERP, and neuroimaging studies have shown significant overlap in the cognitive resources used in speech and music processing, thus there is strong reason to accept Suzuki's theory that music and speech are learned in similar ways. However, there is still the problem of acquired amusia without acquired aphasia and vice versa. That is, some people after brain damage have been observed to lose their musical abilities without losing their speech abilities (Peretz et al., 1994), and some brain-injured people have been observed to lose their speech abilities (Tzortzis et al., 2000) without losing their musical ones. This suggests that music and speech are processed by different cognitive systems. However, an examination of relevant research casts doubt on the conclusion that acquired aphasia and amusia can exist without the other.

There are several case studies of amusia without aphasia, for example, the aforementioned case of K. B., and the cases of C. N. and G. L. (Peretz et al., 1994). C. N. and G. L. were both patients with bilateral lesions of the superior temporal cortex that resulted in an observed loss of ability in music. Both C. N. and G. L. could not recognize familiar melodies taken from their own collection of commercial recordings, well-known nursery rhymes, or well-known folk songs when they were presented on a synthesizer without lyrics. They also performed significantly worse on a battery of tests (Peretz, 1990) used to measure the ability to discriminate variations in musical sequences and to measure short-term musical memory. However, both C. N. and G. L. were able to recognize familiar lyrics both when they were spoken and when they read them. One might use these case studies, as evidence of unique cognitive systems for language and music.

However, both C. N. and G. L. performed poorly in speech recognition tasks that involved non-linguistic cues. C. N. could not distinguish between a declarative, imperative, and interrogative tone in semantically neutral sentences such as, "You are going to bed" (p. 1291). This is similar to the inability of K. B. to distinguish if a phrase was a question or a statement. G. L. could not distinguish the emotion conveyed on a recording of actors speaking nonsense phrases who were instructed to convey one of five emotions with each phrase. Most commonly G. L. confused sadness for joy. As mentioned above, speakers convey emotion or intent in a variety of ways, often utilizing elements one might consider musical such as pitch. Thus, while C.N. and G. L. may recognize spoken and written lyrics, they struggle in understanding prosody and emotion, key components in speech perception. Therefore, to say that their speech perception abilities are intact is misleading.

Of further concern with all case studies of acquired amusia without aphasia is the difference of experience in each domain of the participants. In the case studies of amusia without aphasia, the participants are all non-musicians, thus their experience with language will far exceed their experience with music. It could be that the brain damage is affecting both domains; however, because the participant uses language with great frequency one would need to utilize more difficult language tasks than those typically encountered in these case studies. Interestingly, a case study of aphasia without amusia provides an argument for this assumption.

Catherine Tzortzis and colleagues (2000) report a case study of M. M. who was a 74 year old professional pianist and composer. While M. M. was suffering from progressive aphasia with severe anomia, his musical abilities were left intact. What is remarkable about the case of M. M. was that while he had difficulty naming sounds and pictures of objects from nature, such as animals, he had minimal difficulty doing these tasks with musical instruments. That is, when played a recording of an animal, such as a dog barking, M. M. had trouble naming what made the noise. Similarly, when presented with a picture of an animal such as a dog, M. M. had difficulty saying what it was. However, when presented with the sound or a picture of musical instruments, M. M. had far less difficulty. Tzortzis and colleagues theorise that M. M.'s ability to name musical instruments was spared because of his increased familiarity with musical instruments and the increased frequency with which he encounters musical sounds and instruments. One might then argue that people who suffer brain damage who are not so experienced in music might keep their speech which they are more experienced with, but lose their musical abilities with which they have less experience. Thus, the effect observed where people lose ability in the domain of music but not language may not be the result of unique cognitive systems. In order to test this hypothesis one would have to study musicians who suffer brain damage which results in amusia. To my knowledge such a case study has not been completed.

The idea that amusia and aphasia exist independently and therefore cognitive systems for each domain are unique is further complicated by two studies by Patel and colleagues (2008). In the first study, 12 Broca's aphasics as well as matched controls were given two speech comprehension tasks and a musical task. In the first speech comprehension task participants were instructed to indicate whether or not a sentence contained a syntactic error, such as, "The sailors call for the captain and *demands* a fine bottle of rum." In the second speech comprehension task, the participants had to determine if a sentence contained a semantic error such as, "Anne scratched her name with her *tomato* on the wooden door." For the musical task, participants were played a chord sequence and asked to determine if the sequence contained an out-of-key chord.

Aphasics performed significantly worse on all three tasks than controls. However, one might question whether the poorer performance on the music task is a result of the musical abilities and language abilities sharing cognitive resources, or did some of the aphasics have extra lesions in areas that affected music in addition to the lesions that affected their language abilities. To resolve this issue, Patel and colleagues looked for a correlation between the two types of tasks. When all of the participants in the study were included in the correlation, which was calculated using a multiple regression analysis, Patel and colleagues found that performance on the music task was a significant predictor of performance on the syntactical language task. This suggests that at least part of the processing of music and speech is shared by both domains.

In the second study, Patel and colleagues found that Broca's aphasics are not affected by harmonic priming. Harmonic priming is a well-established phenomenon in which one chord can act as a context for another and thereby influence a person's music perception. A common way in which harmonic priming is studied is through the playing of two chords. The second chord can either be in-tune or out-of-tune by lowering one of the notes by .35 semitones (35 cents). Participants are instructed to determine whether the second chord is in-tune or out-of-tune. The participant performs this task with two variations. In the first variation the second chord is harmonically close to the first chord. In the second variation, the second chord is harmonically distant from the first. Distance is determined by how far the two chords are apart on the circle of fifths. The task is easy and most people correctly assign a label to the chords. What is interesting is that when the chords are harmonically close, reaction times are significantly faster.

When Patel and colleagues used this task to test nine Broca's aphasics and matched controls, they found that all participants answered correctly, however, control participants answered significantly faster when the chords were harmonically close, showing evidence of harmonic priming. The aphasics showed no difference in their response times whether the chords were harmonically distant or close. Thus, even though aphasics sometimes appear to be musical, it seems that at least some aspects of their music perception are changed when they suffer brain trauma that causes them to lose their speech abilities. From all of the aforementioned research it seems on the surface that amusia and aphasia can occur independently, thereby suggesting that music and speech are processed by unique cognitive systems, but upon closer examination, there is evidence to the contrary.

Melodic Intonation Therapy

Further evidence to support Suzuki's idea that music learning and language learning are similar comes from a type of speech therapy called Melodic Intonation Therapy (MIT). MIT is a type of speech therapy originally designed to assist aphasics in relearning to speak. There are perhaps just as many variations in MIT as the number of therapists who use it (Schlaug et. al, 2010). However, the basic concept remains the same. That is, an aphasic is taught to speak phrases by singing them while tapping. In its original form, developed by Nancy Helm-Estabrooks and colleagues (1989), the therapy involved the therapist singing a phrase using two notes that are a minor third apart. For each stressed syllable the therapist uses the higher note. Additionally, while singing the phrase the patient's left hand is tapped with each syllable in an isochronous manner. Thus, both pitch and rhythm are utilized in the singing. The therapy begins with simple two to three syllable phrases, and progresses to phrases of five or more syllables. The therapy also begins with the therapist and patient singing together, gradually moving to the therapist singing and the patient imitating, and finally the patient performs the phrase as a response to a probe question. Additionally, throughout the therapy, the therapist and patient begin by singing, switching to a sprechstimme, and finally ending with regular speech (For a more detailed description of the therapy see Norton et al., 2009).

MIT has been shown to be effective treatment for aphasia. That is, it has been shown to teach patients to speak not only phrases that they are trained with, but also phrases which have not been trained. This suggests that MIT retrains speech, not just the speaking of certain phrases. Furthermore, recent studies have shown that MIT may be even more effective than other methods of therapy (see Wilson et al., 2006, and Schlaug et al., 2008). However, because speech therapy as part of recovery from brain injury is often designed uniquely for each patient, and these studies only use a small number of patients, more research is required before making a judgement of relative efficacy (Schlaug et al., 2010). Nevertheless the effective use of singing to retrain people to speak suggests a similarity in the way in which ability in the two domains, music and language, is acquired.

What is further interesting is the theory behind MIT. As discussed earlier, Brown and colleagues' PET study showed many areas of overlap in active brain regions during speech and singing. However, those areas that did not overlap were most frequently analogous areas in the other hemisphere. That is, when speech activated a part of the left hemisphere of the brain not utilized by the singing, the same part of the brain but, in the right hemisphere, was activated during singing. It is theorised that by using MIT, one can train these analogous areas of the right hemisphere to those damaged in the left for speech.

Gottfried Schlaug and colleagues have shown using fMRI that post therapy, patients do show more activation in the right temporal, premotor, and posterior inferior frontal regions. Similarly, Schlaug and colleagues have shown using diffusion tensor imaging that the right arcuate fasciculus (AF) shows significant development post-MIT treatment. The AF is a fiber tract which connects the superior temporal lobe with the posterior inferior frontal gyrus, and has been shown to play a critical role in auditory-motor mapping. It is therefore important to speech development. The AF is normally less developed in the right hemisphere as it is in the left, for right-handed people. The development of the right AF after MIT therapy is evidence that using music allows the training of speech in right hemisphere analogs of left hemisphere regions typically activated in speech. Because through therapy one can train right hemisphere regions thought to be unique for music for speech, there is evidence of similarity in the way each domain is cognitively processed. It therefore supports Suzuki's idea that language and music are learned in a similar way. What would support this claim even further would be the development of a therapy which trains amusics using speech to develop their musical abilities. However, to my knowledge this type of study has never been done.

Semantic Content

We have thus far seen a large amount of evidence that music and speech share cognitive resources. Skills in one domain can be generalized to the other, and those with deficits in one domain have been observed with deficits in the other. This provides evidence that Suzuki was indeed correct, that behind the process by which children learn to speak lies a method that can be applied to music education. However, one of the main reasons humans use speech is to communicate semantic content, and there is no evidence that the learning of semantic meaning, perhaps one of the most fundamental elements of speech acquisition, has an analog in music learning. In fact there is evidence to the contrary.

If Suzuki is correct that speech acquisition and music learning are similar, one would argue that learning music once one already spoke their native language would be analogous to learning a second language. Thus, one would expect that those with a music education would show greater ease at learning a second language. Robert Slevc and Akira Miyake (2006) tested this hypothesis with 50 adult native Japanese speakers who were speaking English as a second language. All of the participants arrived in the United States after age 11 and had resided in the United States for at least six months prior to the study. Participants were given a series of standardized tasks to test receptive phonology, productive phonology, syntax, vocabulary, and phonological short-term melody in English. Participants also completed the Wing Measures of Musical Talents as well as a singing task. Slevc and Miyake found that receptive phonology, productive phonology, and syntax scores all correlated significantly with scores on the music tasks. To test whether this relationship persisted when controlling for other relevant factors (such as age of arrival in the U. S., length of time in the U. S., amount of language use and exposure, and phonological short-term memory) Slevc and Miyake conducted a five-step hierarchical regression analysis. The inclusion of musical ability as the last step of the analysis accounted for variance in receptive and production phonology but not for syntax or lexical knowledge.

Thus one might argue that Slevc and Miyake have demonstrated that ability in the domain of music is a predictor of the acoustical aspects of speech but not for the semantic aspects. This would lead one to believe that training in music would improve one's learning of a second language and perhaps vice versa. However, it should be noted that Slevc and Miyake did not consider amount of musical training or musical experience. They only considered current musical ability. According to Suzuki, who believes one's musical ability is caused by environmental exposure and effort on the part of the individual, musical ability would be another way to measure the amount of musical training and exposure. Nevertheless, it seems the domain of semantics may not have a correlate in music training and since semantics is such a fundamental aspect of speech acquisition, this is a major flaw in Suzuki's theory. This idea may also account for the observation that speech is unaffected in cases of acquired amusia. That is speech appears intact because the semantic aspects of speech are maintained, however upon closer look, prosodic aspects of speech perception are lost.

Bias of the Research

It should further be noted that the research regarding music-speech correlates has an overall flaw that may lead one to support Suzuki's theory in a biased manner. As stated above, the idea that music and speech are similar goes back many centuries. Thus when researchers seek to investigate this similarity they look for correlates between the two. An experiment may be deemed successful if it shows results that show a similarity between the two domains. These experiments are therefore more likely to be presented or published. Experiments that fail to show a correlation may be deemed unsuccessful and thus remain unknown. This bias may cause one conducting a literature review to be biased in favor of demonstrating a link between the two domains rather than showing differences. Thus all of the research presented above must be taken with some hesitation.

Conclusions

We have seen that Suzuki's theory that music can be taught in the same way that children acquire speech abilities has supporting evidence. Research on pitch in the two domains has shown that ability in one domain can be generalized to the other. Research on rhythm has shown that a lack of ability in musical rhythm was present in those with speech disorders, again suggesting a link. With regard to timbre, we have seen that one can use speech to replicate musical timbres, and this speech is used as a pedagogical tool for teaching music. We have also seen that music and speech convey emotion in similar ways, and those that are lacking musical ability as a result of brain damage are also lacking in the ability to determine the emotion present in speech. Behavioral, neuroimaging, and ERP studies have all confirmed shared cognitive resources between speech and music, and research with aphasia and amusia has failed to show distinct cognitive systems for the two domains. Lastly, for people who lose speech abilities, music has proven to be a tool to help them regain those abilities. However, we have not seen that non-verbal music has a correlate with speech with regard to semantics. Musical ability does not aid in the learning of semantic content in second language acquisition. Semantic contact is a fundamental aspect of speech, thus Suzuki's argument has a flaw. Furthermore, the motivation of researchers to find a connection between speech and music may bias research, and the evidence that shows a lack of a link between the two domains may be underreported.

Nevertheless, the aspects of speech acquisition which have been shown to have correlates in music are acquired by children in a seemingly miraculous manner as Suzuki observed. Perhaps if one considers how children acquire these abilities, and disregard how they acquire semantic understanding, therein lies promise for a better method of music education.

Chapter 10

Musical Talent Is Not Inborn: The Problem of Tone-Deafness

For more than a century, it has been a common belief that one can be born "tone-deaf." (Allen, 1878). "Tone-deafness" is a colloquial term applied to a condition also referred to as "note deafness," "melody deafness," "tune deafness," and "dysmelodia" in past research. In more recent research the condition is known as "congenital amusia" (Peretz et al., 2008). The above terms refer to a condition, thought to be inborn, in which a person possesses normal cognitive function in all other areas, but has an unexplained difficulty with music perception and production. It has been suggested that just as one can be born colourblind, one can be born with a disability in music perception (Kalmus, 1952).

Suzuki (2012) believed that talent for music is not inborn. He likewise believed that tone-deafness, or a deficiency in musical talent, is not inborn. In contrast to much scholarly research Suzuki believed that what we observe as tone-deafness is actually the result of a poor musical environment and not an inborn condition:

> Based on the rule of ability formation in living beings that we have observed in the example of bush warblers,[6] let's look at one aspect of ability formation in human children. I often hear people say, "I'm a tone-deaf parent, so . . . " The claim is that therefore the child is tone deaf [7] and nothing can be done because this is a matter of heredity. However, I would suggest that just as there is no such thing as an innately tone deaf warbler, there does not exist an innately tone deaf human child. Far from being tone deaf, all infants have marvelous hearing. That is why they unerringly absorb the off-key pitches of the lullabies sung to them by their tone deaf parents. The process is exactly identical to that of Osaka children, who without exception master the delicate melody of Osaka dialect.[8] It is thus clear that

[6] Suzuki argues that Japanese Bush warblers are not born with their distinctive song, but rather are trained through environmental stimuli. He argues that people are the same with musical ability. See chapter 7 for a discussion.
[7] Sometimes tone-deaf is printed in *Nurtured by Love* with a hyphen and sometimes without. When quoting Suzuki and others, in this and the following chapter, I have maintained the original text.
[8] Umegaki (1962), describes differences between the dialects of Osaka and Tokyo. As discussed in Chapters 8 and 9, above, Suzuki claims that his method of teaching music is based on the way in which children acquire speaking

> recording a horribly out-of-tune song and playing it daily for a group of babies will render every single one of them tone deaf. In other words, if we wished to do so, we could turn all children throughout the world tone deaf. Given that such a plan could successfully be carried out, there assuredly cannot be such a thing as inborn musical talent. (p. 15)

Suzuki further believed that tone-deafness can be changed by having the tone-deaf child listen to music which is in tune. When the amount of music the child has heard in tune surpasses the amount that child heard out of tune, Suzuki believed the child's tone-deafness would be rectified.

> Then, what can be done to address tone deafness? What I discovered was that although I could not teach tone deaf children to adjust their overly high-pitched *fa*, I could help them learn how to produce anew the correct pitch of *fa*. I decided that with a child who has become tone deaf from hearing the wrong *fa* 5,000 times, I would have her listen to the correct *fa* 6,000 times, then 7,000 times. At first nothing changed, but as she heard the correct *fa* 3,000 times, 4,000, 5,000, and 6,000 accumulated times, her ability to reproduce the correct *fa*, which she absorbed from hearing it 6,000 times, began to overpower her ability to produce the wrong pitch she had learned from hearing it 5,000 times. In short, the newly cultivated, correct function had taken root. (p. 116)

A large amount of research has been published regarding tone-deafness. Much of this research claims that tone-deafness is an inborn condition. Should the research be conclusive, it would pose a problem for a basis of the Suzuki Method, namely, that lack of musical talent is not inborn. Furthermore, if one concludes that people can be born with an extreme deficiency in musical ability, one might also hypothesize that people can be born with an unusually high aptitude for music.

In the following three chapters, I analyze tone-deafness research. I determine the extent to which this research has affirmed the existence of an inborn condition, and whether or not this

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abilities. According to Suzuki, all children acquire speaking abilities, and if we replicate the process by which they acquire speech we can teach all children anything, including music.

research disproves Suzuki's claim that lack of musical talent is not inborn. In the present chapter I evaluate an influential paper by Hans Kalmus and Dennis Butler Fry (1980) and in the two following chapters I evaluate more recent research.

Kalmus and Fry (1980) "On tune deafness (dysmelodia): frequency, development, genetics, and musical background"

Kalmus and Fry's (1980) study on tone-deafness, which they call "tune deafness"[9] (p. 369) is the first that attempts to define tune deafness quantitatively using a psychometric test. Kalmus and Fry define tune deafness as a "deficiency in melodic aptitude" (p. 369). Through the use of the word "aptitude" rather than "ability" it seems they believe the condition is inborn rather than developed. Furthermore, Kalmus and Fry suggest the term "dysmelodia" as a replacement for "tune deafness" as a parallel to dyslexia which is thought to be an inborn condition (e.g., Galaburda & Kemper, 1979 and Habib, 2000). Lastly, at the end of the article, Kalmus and Fry theorize that tune deafness is a result of "an autosomal dominant trait with imperfect penetrance" (p. 382).

Kalmus and Fry's paper is widely cited. (*Google Scholar* lists 130 publications which cite this paper.) Because it is the first attempt to define tune deafness quantitatively, it has become influential. Of particular interest is the test they designed to measure tune deafness and administered to over 4,000 participants including children from different cultural backgrounds, adults of different cultural backgrounds, adults with low IQ scores, adults from the faculty of a university, and members of more than one generation of particular families. By testing a variety of groups, Kalmus and Fry address many of the aspects of tune deafness on which subsequent

[9] I will, in accordance with Kalmus and Fry, refer to the theorized condition of tone-deafness as tune deafness in this chapter.

research has focused. By contrast, earlier studies included few participants and few kinds of participants. Additionally, Kalmus and Fry report results of correlations between their test and two subtests from Carl Seashore's tests of musical talent. Lastly, and most importantly, Kalmus and Fry include a pair of experiments which they claim demonstrate that the percentage of tune deaf individuals in the general adult population is approximately 4.2%. This claim has been repeated in over 97 publications (*Google Scholar*) and is still cited (e. g. Wen et al., 2014; Hatada et al., 2014; Tan et al., 2014).

The Distorted Tunes Test

Kalmus and Fry term the test they designed to measure tune deafness the "Distorted Tunes Test" (DTT). The DTT originally comprised recordings of the beginnings of 25 well-known English melodies played on a piano. Each melody was used in the test twice, once correctly and once incorrectly. The melodies that were played incorrectly contained notes that were moved up or down by a semitone or whole tone. The resultant 50 melodies were played in a random order and participants were instructed to indicate whether each melody was played correctly or incorrectly.

There are two types of errors possible on the DTT. One can mistakenly deem a correct melody incorrect (Kalmus and Fry refer to the total number of these false negative errors as the *f-score)* or an incorrect melody correct (the number of these false-positive errors is called the *m-score).* For most of the data analysis in the paper, Kalmus and Fry only use the *m-score.* They deem the *m-score* "more important" than the *f-score* and claim that it is "good enough" (p. 371) to determine whether someone is tune deaf. They found that the false negatives were not highly correlated with the false positives and thus claim the "nine faultless tunes simply serve as controls" (p. 371) By "controls," Kalmus and Fry seem to mean that including both correct and incorrect melodies would prevent participants from merely guessing that all the melodies were faulty.

On the basis of this initial test, Kalmus and Fry concluded that 24 of the 50 melodies did not significantly contribute to identifying a contrast between tune deaf and normal adults. They thus shortened the test by removing these 24 melodies. The remaining 26 melodies included 9 which were correctly played and 17 which were incorrect. (For a list of the melodies employed in the second version of the DTT see pp. 370 and 371.)

As discussed below, 55 participants were given both the original and shortened versions of the test, and in general, the number of incorrect responses, specifically the false-positives (the *m-score*), did not substantially change. Kalmus and Fry thus continued their research with the shorter test and argued that data from both versions of the test can be pooled.

The Stimuli

The stimuli of the DTT contained 26 melodies, nine of which were played correctly, and 17 of which were played incorrectly. The melodies that were played incorrectly contained notes that were moved up or down by a semitone or a whole tone.   Kalmus and Fry do not provide scores of their stimuli. Rather they list which melodies they used and how many notes they used in each melody. They additionally provide numbers corresponding to the notes that changed for the incorrect melodies, and list whether each change was up or down and a semitone or whole tone. Because they do not provide scores and the melodies used sometimes appear in various sources with variations, it is not possible to determine with complete certainty what the actual stimuli were. Nonetheless, as a guide to the following discussion, Appendix 1 includes notations of correct and incorrect versions of the 17 melodies they employed as the stimuli.

Of the 17 melodies, 12 preserved the contour of the melody. That is, the changed notes did not change whether the pitches of the melody went up, down, or stayed the same, from the previous note to the changed note or from the changed note to the next note (example 1).

Example 1: King Wenceslas: a) Correct Version b) Incorrect Version

a)

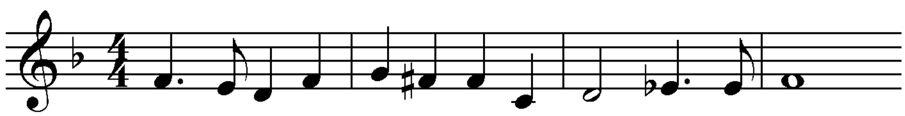
b)

In five of the melodies (Annie Laurie, Jolly Good Fellow, All Thro' the Night, Auld Lang Syne, and Clementine) the changed note altered the contour of the melody. For example, in All Thro' the Night (Example 2) the second and third quarter notes in the second measure descend a semitone in the correct version, and stay the same in the incorrect version.

Example 2: All Thro' the Night: a) Correct Version, b) Incorrect Version

a)

b)



However, in three of the melodies where the contour changed (Jolly Good Fellow, Auld Lang Syne, and Clementine), this contour violation occurred between phrases and would perhaps be less apparent. For instance, in the fourth measure of Clementine (example 3) the half note and the following dotted-eighth note have the same pitch. In the incorrect version, the half note descends a whole tone to the subsequent dotted-eighth note. However, the phrase ends with the half-note and a new phrase begins on the dotted eighth note, perhaps making the contour violation less prominent.

Example 3: Clementine: a) Correct Version, b) Incorrect Version

a)

b)

In English speech, important linguistic information is contained in the pitch relationships between adjacent syllables but not the absolute pitches used. In other words, in perceiving English speech, listeners rely on the contour of pitches used and not exact pitch movements (Patel, 2010, See p. 195-196). In contrast, changes in Kalmus and Fry's stimuli involve not only the alterations of contour just discussed for 5 of the melodies, but also intervallic changes in each of the 17 melodies. A contour change in a melody is more similar to the changes one perceives in speech than an intervallic change in which contour is preserved. Thus, a tune deaf participant may be able to determine when an incorrect note changes the melodic contour by relying on a linguistic capacity rather than a uniquely musical ability.

Because the DTT includes few melodies which contain contour violations, one can eliminate the possibility that participants use solely a linguistic capacity when they score well on the test. Even if participants were able to utilize contour changes to determine whether a melody was played incorrectly, this would only allow them to correctly identify five out of the 17 melodies that were played incorrectly. They would still score poorly and would still be considered tune deaf. Thus, because of the nature of the stimuli, one can assume that the DTT actually tests music perception.

The nature of the stimuli of the DTT is also remarkable as anecdotal evidence suggests that tune deaf people sing in such a way that their melodies resemble the melody they are trying to sing in contour but not in actual intervals. For example, Suzuki wrote (see above) that tone-deaf people sing with a *fa* that is too high. This implies that they sing in the correct direction, but do not use the correct interval. Thus the DTT tests for tune deafness as Suzuki thinks of it.

This version of the otherwise vague notion of tune deafness may be equally valid in perception and production. That is, tune deaf people may recognize the contour of a melody but not identify when a note is played incorrectly if it does not violate the contour. This may explain why in earlier studies Kalmus and Fry could not distinguish tune deaf people from normal people by asking them to name familiar melodies played without text (p. 369). That is, by recognizing the overall contour of a familiar melody even tune deaf people can determine which melody is being played. Interestingly, this idea is in contrast to the case described by the popular rhyme quoted at the beginning of Kalmus and Fry's article:

> There was an old fellow of Sheen
> Whose musical sense was not keen.
> He said: 'It is odd,
> I can never tell God
> Save the weasel
> From pop goes the queen!' (p. 369)

Reliability of the DTT

The retesting of the 55 participants mentioned above demonstrated that the DTT produces reliable results on retesting. Of the 55 participants who were given the DTT, the majority (32 participants) had the same *m-score* both times. 14 participants had lower *m-scores* on the second test, and nine participants had higher *m-scores*. Of the 23 participants whose score changed, 13 changed scores by only one point in either direction. Nine of the ten remaining participants whose scores changed by more than one point scored poorly on the test both times. This is expected, as those that score poorly would likely have more variability in their scores as they may be guessing more often than those with better scores.

Only one participant had a large change in score between the tests; this participant's score was in the normal range for the first but not the second test (Kalmus and Fry define an *m-score* less than or equal to two as normal and greater than or equal to three as tune deaf). This participant achieved an *m-score* of two on the first test and five on the second test. This participant was the only one out of 55 participants whose designation switched from normal to tune deaf upon retest. There was also only one participant who switched from tune deaf to normal upon retesting. This participant had an *m-score* of three on the first test and two on the second.

The reliability of results from the DTT is further noteworthy because for 26 of the 55 participants, there was a 24-30 year break between the two tests. The remaining 29 participants were retested after a period of a few weeks or months, and showed only a slightly higher correlation between their scores on the two tests (r=0.889 for the shorter gap, and r=0.816 for the longer gap). Nonetheless, the correlations were not perfect. On retesting, 41.8% of participants had *m-scores* that changed by one point or more, and 18.2% of participants had *m-scores* that changed by at least two points.

Correlation with Other Tests

Kalmus and Fry tested 989 grammar school children on the original 50 item DTT and on three other tests. In other experiments reported by Kalmus and Fry they only use the *m-score*, however, to test for correlations with other tests Kalmus and Fry utilized the function 3m-f (Kalmus and Fry do not explain how they arrived at this formulation). To test the hypothesis that tune deafness is a consequence of defective pitch discrimination, Kalmus and Fry tested the children with Carl Seashore's (1946) Test for Pitch Discrimination. In the pitch discrimination test, participants listen to pairs of tones that differ from B4 (493.9 Hz, notated on the third line of treble-clef staff) by 7 cents to 60 cents above or below to determine whether they are the same or different. A high correlation of scores on the DTT and the pitch discrimination test would suggest that tune deafness is a consequence of poor pitch discrimination.

In order to investigate the hypothesis that "the material defect underlying tune deafness is a deficiency in 'Gestalt perception' probably localized in the cortex," (p. 375) Kalmus and Fry utilized Seashore's Test of Tonal Memory. In the Test of Tonal Memory, participants listen to a melody consisting of three to five notes played twice. Participants are tasked with identifying by number which of the three to five notes was changed in the second hearing.

Lastly, Kalmus and Fry administered a Number Memory test in order to investigate a correlation between scores on the DTT and non-musical memory. In the Number Memory test, participants heard a string of numerals twice. The second reading contained a numeral that was changed. Participants were tasked with determining which numeral in the second reading was unique to both readings.

The results of correlations were not surprising. Scores on the DTT correlated most highly with those of the Test of Tonal Memory. They correlated slightly less with scores on the Test of Pitch Discrimination, and only slightly with scores on the Number Memory Test. None of the three additional tests contributed much to the diagnosis of tune deafness, and thus Kalmus and Fry did not apply them to later studies. Nevertheless, the higher correlations found between the DTT and the Seashore tests provide evidence that poor performance on the DTT is a result of a deficiency in specifically musical ability rather than a non-musical cause.

Cultural Influences and the DTT

Kalmus and Fry administered the DTT to groups of non-English participants. Non-English participants, as expected, had higher *m-scores* than the English participants. The best scoring group was the German participants (N=93). 45.2% of the German participants made no errors and 24.7% of the German participants had an *m-score* of three or higher, Kalmus and

Fry's criterion for tune deafness. In the Dutch group (N=23), 30.4% of participants made no errors, and 43.5% had an *m-score* of three or higher. These groups were followed by the Italian group, 'Other Europeans,' and Brazilians. The poorest scoring group was the Indian and Pakistani group (N=17). Three participants from the group would be considered normal by Kalmus and Fry's three error-criterion, but 82.4% of the sample would be deemed tune deaf.

The ability of some Europeans to achieve perfect *m-scores* or near perfect *m-scores* without being familiar with the English melodies utilized by the DTT suggests that participants were relying on their knowledge of Western tonal music. Lack of such knowledge would also explain why the Indian and Pakistani group performed worse than any other group, as according to Kalmus and Fry, the participants in this group had the least familiarity with Western tonal music. As well, if the 'Other European' participants were speakers of a non-Germanic language, the relatively successful results of the German and Dutch participants might have been a consequence of differences in the idiomatic coordination of musical accents and linguistic stress among various languages (for prosodic differences among Germanic, Romance, and tone languages, see van der Hulst et al., 2010).

To further test their hypothesis concerning knowledge of Western tonal music, Kalmus and Fry created another version of the DTT that utilized 25 German songs played twice in place of the 26 English songs. Both the English and German DTTs were administered to a group of 41 English adults and 51 German adults. As expected, each group performed better when their nationality's melodies were utilized. Nonetheless, a high correlation was observed between individual participant's scores on both tests (r=0.807). In particular, out of a total of 1,025 possible *m-errors* in the German test administered to the English participants (25 incorrect melodies x 41 English participants), only 121 were observed (11.96%). Similarly, out of a total of 867 possible *m-errors* in the British test for the German participants (17 incorrect melodies x 51 participants) only 102 were missed by the Germans (11.8%). From these results Kalmus and Fry conclude, “Clearly judgements were mostly based on knowledge of structure of western melodies” (p. 378).

Children and the DTT

Kalmus and Fry administered the DTT to 1,541 English children between the ages of five and 18. They found that 40% of children under the age of nine would be classified as tune deaf using the adult three-error criterion. This number gradually decreases to about 4% when the children reach sixteen years of age. There were no significant differences in scores between boys and girls except between ages nine and 12, when girls tended to score slightly better. Kalmus and Fry thus suggest that a child with an *m-score* of fewer than three should be considered normal. However, a child, under the age of 16, with an *m-score* of three or higher should not be considered tune deaf without further consideration.

Kalmus and Fry also administered the DTT to 48 Dutch children “in Holland” (p. 373)—they do not specify a particular region or regions--and 208 children from orthodox Jewish schools in North London. The Dutch children and the orthodox Jewish children scored significantly worse on the DTT than the English children in the study just discussed. Kalmus and Fry hypothesize that their poor scores were a result of a lack of familiarity with the stimuli.

One aspect of the results of the orthodox Jewish children that warrants further investigation is the sudden improvement from age 16 to age 17. From age 12 to age 16 approximately 70% of the orthodox Jewish children had *m-scores* higher than two. However, at age 17 the percentage of orthodox Jewish children with an *m-score* higher than two decreased to approximately 25%. This is in contrast to the English children, whose percentage of participants with an *m-score* higher than two gradually decreased. Investigation into the music curricula of the orthodox Jewish schools, and the extent to which orthodox Jewish children have been active in secular culture at various ages would perhaps provide an explanation.

The 4.2% Claim

On the basis of two experiments Kalmus and Fry claim that 4.2% of the adult population is tune deaf. In the first experiment, they tested two groups on the 26 items of the DTT. One group comprised 130 adults who were deemed musical by a "panel of the British Broadcasting Corporation" (p. 372) and the other group comprised 20 adults "who considered themselves to be tune deaf, or were so considered by others" (p. 372). All of the musical group made two or fewer errors and the majority made no errors at all. In contrast, the tune deaf group had a wider spread of scores, from 0-15, and only a minority, 25%, had fewer than three errors (see Table 1).

Table 1: Number of participants with each *m-score**

m-score	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**15**
Musical Group (n=130)	114	14	2	0	0	0	0	0	0	0	0
Tune Deaf Group (n=20)	1	1	3	0	1	3	3	3	4	0	1

* Adapted from Table 3 of Kalmus & Fry, 1980

By comparing the distribution of *m-scores* for each group, Kalmus and Fry conclude that "it is reasonable to consider any British person who scores three or more as tune deaf and anybody scoring two or less as normal" (p. 372).

Kalmus and Fry do not provide information regarding how the BBC panel determined people were musical. We also do not know who was on the panel. Regarding the tune deaf group, 25% of the participants scored within the normal range, thus perhaps one should not rely on self-report to determine whether someone is tune deaf. Nevertheless, the musical group scored significantly higher on the DTT demonstrating that the two populations are distinct. This is the case whether one excludes or includes the five participants in the tune deaf group who misidentified themselves and scored in the normal range.

There was one outlier in the tune deaf group who had an *m-score* of 15. The probability of achieving such an *m-score* by random guessing is 0.001. It is possible that the participant reversed the directions of the test and thus actually should be assigned an *m-score* of two. Whether one includes the outlier, excludes the outlier, or reverses the outlier's score, there is still a significant difference ($p<0.0001$) between the *m-scores* of the tune deaf group and the musical group (see Table 2).

Table 2: Means and Standard Deviations of *m-scores* of a Musical Group and Tune Deaf Group on the DTT.

Group	**Mean *m-score***	**Standard deviation**
Musical Group (n=130)	0.14	0.39
Tune Deaf Group (n=20)	5.60	3.35
Tune Deaf Group without misidentified participants (n=15)	7.00	2.56
Tune Deaf Group without outlier (n=19)	5.11	2.58
Tune Deaf Group with outlier reversed (n=20)	4.95	2.60

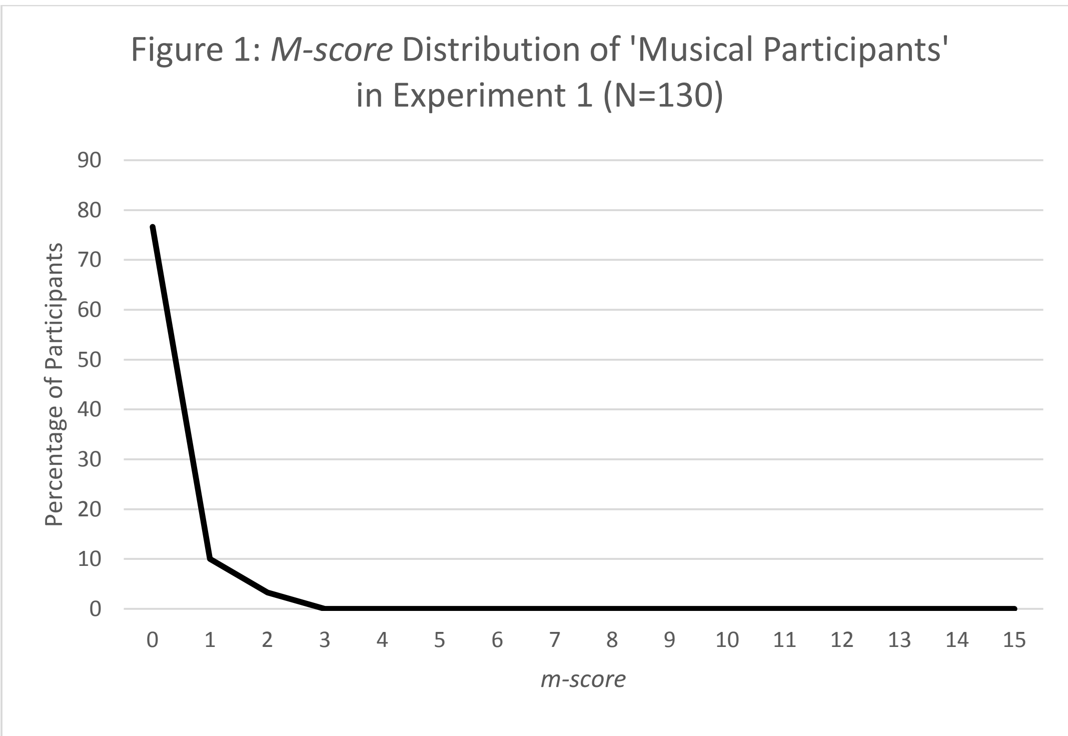
In the second experiment, Kalmus and Fry administered the DTT to a group of 604 adults who were not selected for musical ability or tune deafness. 4.2% of the participants in the second experiment had an *m-score* of 3 or higher on the 26 items of the DTT (See Table 3).

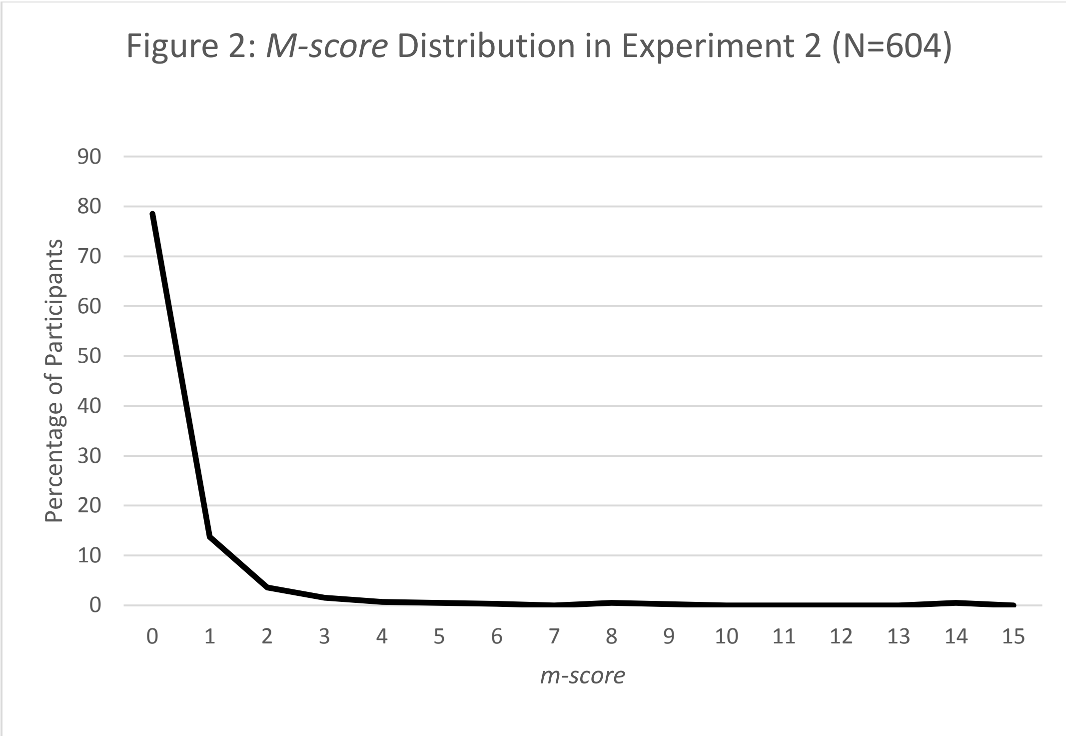
Table 3: Distribution of *m-Scores* for 604 adults not selected for musical ability*

m-score	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**14**
Number of participants	474	83	22	9	4	3	2	0	3	1	3
Percent of sample	78.5	13.7	3.6	1.5	0.7	0.5	0.3	0	0.5	0.2	0.5

* Adapted from Table 4 of Kalmus & Fry, 1980.

The distribution of scores on both experiments are similar (See figures 1 and 2, for the distribution of scores of tune deaf participants see figure 3 below). Both are J-shaped distributions with the majority of participants scoring perfectly.





Comparing the means of *m-scores* from both experiments, from the 'musical' group in both experiments, and the tune deaf group in both experiments, yields no significant differences (at the 0.05 level) (see Table 4).

Table 4: Comparison of Score of 'Musical' and Tune Deaf Participants in Experiments 1 and 2

	Experiment 1	**Experiment 2**	**Probability**
'Musical' Participants	N=130 Mean= 0.14 SD=0.39	N=579 Mean=0.22 SD= 0.5	P=0.09
Tune Deaf Participants	N=20 Mean= 5.6 SD=3.35	N=25 Mean= 5.8 SD=3.61	P=0.85

Thus there is no reason to conclude that, with regard to the DTT, there is a difference between 'musical' participants in the experiments and no reason to conclude there is a difference between tune deaf participants in the experiments. Conversely, there is a clear reason to conclude that there is a difference between the 'musical' and tune deaf participants in the experiments.

As stated above, Kalmus and Fry observed that the tune deaf group in experiment one tended to have an *m-score* of three or more. They therefore conclude that any English adult with an *m-score* of three or higher should be deemed tune deaf. Because 4.2% of the participants in the second experiment had an *m-score* of three or higher, Kalmus and Fry conclude, “One may reasonably consider 4.2% as an estimate for both sexes of the frequency of tune deafness among English adults” (p. 373).

Problems with the 4.2% Claim: The Three-Error Criterion

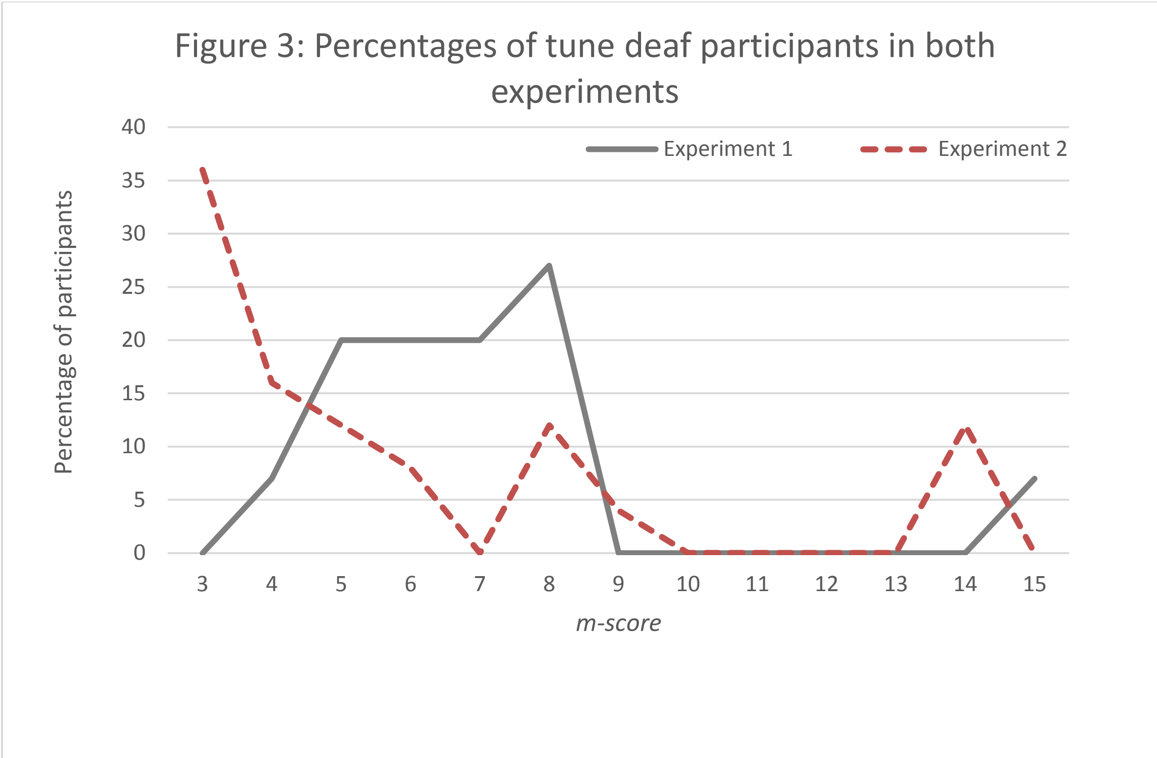
One problem with Kalmus and Fry’s claim that approximately 4.2% of the population is tune deaf is the arbitrary nature of their three-error criterion. Other than the results of the participants selected by the BBC panel for the first experiment there is no empirical reason to use a three-error criterion instead of four, five, or six (Henry & McAuley, 2010). No matter what criterion one uses, there is always a significant difference ($p<.0001$) between the mean *m-scores* of normal participants and tune deaf participants in both experiments. There is also no significant difference between the mean *m-scores* of tune deaf participants between the two experiments, provided one eliminates the scores of the participants who misidentified themselves in experiment one. (It should be noted that as one increases the number of errors required to identify a participant as tune deaf, the number of misidentified participants in experiment one increases). There is, however, a significant difference ($p=0.03$) between the mean scores of ‘musical’ participants in the two experiments if one uses a criterion of less than four errors to define ‘musical.’ (See Table 5).

Table 5: Mean *m-scores* of 'Musical' Participants in Experiments One and Two When 'Musical' is Defined as $M < 4$

	Mean *m-score*	**Standard Deviation**
'Musical' Participants in Experiment 1 (N=130)	0.14	0.39
'Musical Participants in Experiment 2 (N=588)	0.26	0.60

Furthermore, if one further increases the number of errors as the criterion of distinction, the difference between the two resultant 'musical' groups of the two experiments increases. Perhaps then one can say the three-error criterion chosen by Kalmus and Fry is not arbitrary, in that a higher criterion produces two groups of 'musical' participants that have significantly different *m-scores*. However, because in experiment one, only 'musical' participants, as determined by the BBC, and self-identified tune deaf participants, are utilized, perhaps experiment one does not include those that would be at the low end of the normal group but still should not be considered tune deaf. This would depend on how the BBC determined one to be 'musical,' information which Kalmus and Fry do not provide. If the means by which the BBC determined participants to be musical was an evaluation similar to the DTT, and the BBC selected only participants who scored above a similar threshold, the three error-criterion would then still be arbitrary.

Additionally problematic, while the mean *m-scores* for the tune deaf participants in the two experiments did not differ significantly, the distribution has one noticeable difference (see Figure 3).



The most apparent difference between the two distributions is the percentage of participants with an *m-score* of three. In experiment one, none of the participants had an *m-score* of three, while in experiment two, more participants (9 participants, 36%) had an *m-score* of three than any other score (above 2). Therefore, perhaps an *m-score* of three should be considered the upper extreme of the 'musical' range. If one uses the four-error criterion to deem a participant tune deaf, the distributions of scores amongst tune deaf participants is more similar. If one uses this criterion one concludes 2.6% of the adult English population is tune deaf. However, as the four-error criterion is also arbitrary, one could use a five error-criterion, thereby decreasing the percentage to 2.3%. The high percentage of participants with an *m-score* of three also suggests that while a clear boundary between normal and tune deaf participants was observed in experiment one, when one considers the general population, as in experiment 2, the boundary is not clear. That is, musical competence is not present in one group and not the other, but rather exists in varying degrees. The idea of a non-distinct boundary is further evident from the large percentage of participants (41.8%) whose score changed upon retesting (see above). That is, if scores change upon retesting, one with an *m-score* that is one point lower than the criterion may change their designation from 'musical' to tune deaf upon retesting.

Problems with the 4.2% Claim: Alternative Explanations

One of the flaws of the DTT is that there is no reason to think one melody or another is more difficult. Perhaps an analysis of Kalmus and Fry's data would show that certain melodies were labeled incorrectly more often than others, or melodies with certain types of errors were labeled incorrectly more frequently. However, Kalmus and Fry do not report such data and assume that the melodies were equally difficult. This is problematic as many of those considered tune deaf performed above chance levels (See Table 6).

Table 6: Probability of Guessing Melodies Incorrectly *

Number of Melodies Guessed Incorrectly	**Probability**
All 17	0.000008
At least 16	0.0001
At least 15	0.001
At least 14	0.006
At least 13	0.025
At least 12	0.072
At least 11	0.166
At least 10	0.315
At most 9	0.500
At most 8	0.315
At most 7	0.166
At most 6	0.072
At most 5	0.025
At most 4	0.006
At most 3	0.001
At most 2	0.999
At most 1	0.0001
None	0.000008

* The probabilities shown here assume that one is equally likely to guess incorrect or correct for each melody.

As shown in table 6, the probability of achieving *m-scores* between three and five (incorrectly guessing on only 3-5 melodies) by guessing is low. This suggests that much of the time participants deemed tune deaf were able to accomplish the task of determining if a melody contained errors. If one assumes each melody is equally difficult to label, one is left with the question of why most of those deemed tune deaf labeled most melodies correctly. Possibly those deemed tune deaf were distracted during the playing of some stimuli. Or perhaps, participants were more familiar with some melodies and less familiar with others. These are two of many possible factors that could explain the results. Because Kalmus and Fry claim the percentage of tune deaf individuals in the population is small, one must be careful to determine whether their errors on the DTT were made as a result of the same underlying condition, or if they were subject to some individual factor or factors that would explain why they were sometimes able to perform the task successfully and sometimes not.

Another possibility is that participants were able to determine whether a melody was played incorrectly if it contained a change in contour (see above). Perhaps tune deaf participants only needed to guess on the 12 melodies in which contour was preserved. If a participant correctly identified the melodies in which contour was violated and then guessed on the remaining melodies, one would expect most tune deaf participants to achieve an *m-score* of five, six, or seven. This is close to the result found in experiment one (see table 1). However, one would expect then that the melodies with contour violations would be more regularly identified and thus they would not serve as a means to distinguish tune deaf participants from normal participants. They would have thus been removed from the test when Kalmus and Fry reduced the test from 50 melodies to 26. One should therefore conclude that this hypothesis is not a valid explanation for why most participants deemed tune deaf were able to achieve better results than those expected if they were guessing. This explanation may however be valid for a small portion of the tune deaf participants.

Problems with the 4.2% Claim: Failure to Replicate the Result

Another problem with Kalmus and Fry's claim is that it has not been replicated by further studies. In Kalmus and Fry's article they report an experiment comparing the results of the DTT of a group of high-grade defectives (IQ = 50-60) and a group of academic and scientific staff of the University College London and Rothamsted Experimental Station. Not surprisingly, as they likely perform poorly on all varieties of tests, the group of high-grade defectives performed poorly. However, the group of presumed "high intelligent academics" (p. 376) contained seven participants out of 61 (11.4%) who had an *m-score* of three or higher. There is no reason to think this group should perform worse than the general population. Kalmus and Fry attribute the high number of academics deemed tune deaf to a sampling bias. However, it is difficult to determine how such a bias might be corrected statistically (for example, by weighting the responses of the 'defective' and 'high intelligent' participants, as Kalmus and Fry seem not to have compiled values on background variables for the larger sample that they regard as representative of the population as a whole).

Similarly, in two of the other experiments discussed above, the 4.2% result was not replicated. In the experiment where German and English participants were given two versions of the DTT, nine of the 41 English participants (22%) had an *m-score* greater than two on the English DTT, and seven out of 51 German participants (13.7%) had an *m-score* greater than two on the German version of the DTT. In the experiment where 55 participants were tested twice using the DTT, 19 out of 55 participants (34.5%) had *m-scores* greater than two in both the first and second set of scores.

Lastly, in 2001, Dennis Drayna and colleagues tested pairs of twins utilizing the DTT. Because their study involved American and English participants and because 20 years had elapsed since the conclusion of Kalmus and Fry's extended study, Drayna and colleagues created an updated version of the DTT. Drayna and colleagues' DTT also contained 26 melodies, however, different melodies were chosen as the melodies would be more familiar to their participants. In contrast to Kalmus and Fry, Drayna and colleagues did not observe a clear point of distinction between tune deaf and normal participants. (However, it should be noted that Drayna and colleagues did not recruit specifically tune deaf participants or musical participants, in any of their experiments, which may account for why they did not observe two distinguishable groups.) Furthermore, 39.6% of the 568 participants would be deemed tune deaf by Kalmus and Fry's three-error criterion. Thus, one should not conclude that Kalmus and Fry's claim that 4.2% of the population is tune deaf is reliable.

Is tune deafness the result of an autosomal dominant trait?

At the end of their article, Kalmus and Fry construct 19 family trees of those the DTT identified as tune deaf (p. 380-381). What is particularly valuable about this study is the number of generations who were tested using the DTT. As discussed in chapter 12, self-report and the report of others are not useful bases on which to determine tune deafness. However, for the majority of people in the family tree diagrams, Kalmus and Fry did not rely solely on self-report or the report of others, for they tested each of these participants using the DTT. Furthermore, in all 19 of the families diagrammed, Kalmus and Fry were able to test family members from at least two generations. In diagrams 1, 2, 5, 9, and 12, Kalmus and Fry present diagrams in which they were able to test three generations. Necessarily, Kalmus and Fry relied to a small extent on self-report and the report of others; however, they based their analyses on actual testing.

Kalmus and Fry found that tune deafness was transmitted by both sexes. That is, when either the father or the mother was tune deaf, they had a tune deaf child or children. If one parent in the family was tune deaf, the trait appeared in their children with a ratio of approximately 1:1 (one normal child for every tune deaf child). Because either parent can pass on the trait, and because it was passed on with a ratio of 1:1, Kalmus and Fry conclude that tune deafness is likely, “an autosomal dominant trait, highly variable in severity and not quite completely penetrant” (379). An autosomal trait is one which is present on the non-sex chromosomes, and dominance means that a single copy, rather than two copies, of the mutation is enough to cause the trait. With autosomal dominant traits, if only one parent has the trait, children have a 50% chance of inheritance. Thus, if tune deafness is an autosomal dominant trait the expected ratio of tune deaf to normal children is 1:1. (If both parents have the trait children have a 75% chance of inheritance and thus the expected ratio of tune deaf children to non-tune deaf children when both parents are tune deaf would be 3:1.) A trait that is not completely penetrant refers to a trait which occurs in most, but not all, of those who have the mutated gene.

Kalmus and Fry acknowledged problems with their conclusion and admitted the existence of a “multiplicity of alternative explanations” (p. 379). One problem is that of families where both parents were found to be tune deaf. In diagrams, 1, 2, 12, and 18, two parents were found to be tune deaf, and four out of nine children were found to be tune deaf. If tune deafness is an autosomal dominant trait, one would expect a higher ratio (3:1) of tune deaf children to non-tune deaf children. Furthermore in diagrams 6, 7, and 8, children were found to be tune deaf when none of the members of the generation above them were found to be tune deaf. Thus there is evidence that the tune deafness of these participants is not the result of a genetic inheritance. Lastly, Kalmus and Fry's diagrams show the family members of individuals found to be tune deaf. Before making a conclusion regarding the genetic nature of tune deafness, one would also need to examine family trees of musical people to demonstrate that their normality is the result of descending from musical parents.

Assortative Mating

Kalmus and Fry tested 67 British couples, which included 11 tune deaf and 56 normal men, and 16 tune deaf and 51 normal women, to investigate the hypothesis that people who are tune deaf seek mates who are tune deaf. Among the 67 couples, 47 had both a normal husband and wife. 13 of the couples had a husband or wife who was tune deaf but not both, and in 7 couples both husband and wife were deemed tune deaf. Kalmus and Fry acknowledge that the percentage of participants in this experiment who are tune deaf far exceeds that of the general population and thus the couples are not random. However, had the 67 men and the 67 women been paired randomly, the probability of obtaining seven or more couples where both partners were tune deaf is less than 0.0025. Thus there is strong evidence of assortative mating.

Is Tune Deafness Inborn?

As stated above, Kalmus and Fry hypothesize that tune deafness is an inborn condition. However, throughout all of their studies they never report the histories of the participants. We are not told if participants had a music education or what their listening habits were. In all of the experiments Kalmus and Fry report, their results could be explained by varying amounts of musical experience and exposure. For example, the experiment on the basis of which Kalmus and Fry conclude that 4.2% of the adult population is tune deaf does not include histories of the individuals, so that one could conclude that 4.2% of the adult population has experienced little musical experience and exposure. This is particularly problematic as Kalmus and Fry themselves conclude that results of the DTT are affected by experience with the stimuli (that is, knowledge of the individual melodies) and experience with Western tonal music.

With regard to the family tree diagrams, Kalmus and Fry have a similar problem. One could explain the patterns found in the families by shared environment and similar activities of a family rather than genetic inheritance (evidence for this idea can be found in the observed assortative mating of tune deaf individuals). That is, if a parent is tune deaf as a result of not having musical experience they may not value music education and thus would not provide their child with musical experiences. Or similarly, if one sibling experiences an environment lacking in music another sibling would be more likely to experience that same lack of musical exposure. Thus, while Kalmus and Fry present valuable data, their studies may actually be studies in musical ability and not musical aptitude. In short, Kalmus and Fry present no compelling evidence that refutes Suzuki's claim that tune deafness is not inborn.

Conclusions

Kalmus and Fry have provided one of the most extensive studies on tune deafness. For this reason their article has become influential. The DTT designed by Kalmus and Fry is unique in that it has been administered to large numbers of people. They have shown through a test that produces consistent results that there is a small percentage of the population that has problems with music perception. Furthermore, by utilizing melodies that contain wrong notes but maintain contour one can assume that the participants deemed musical are in fact relying on cues unique to music and not common to speech perception. There is further evidence that the DTT tests for a lack of musical ability in that scores on the DTT correlated in a valid manner with scores on two other musical tests. Kalmus and Fry have additionally provided evidence that children as a group reach similar musical competence to adults at the age of 16 and that people can utilize their experience with Western tonal music to make judgements about the acceptability of a melody even if they are not familiar with the melody. Lastly, Kalmus and Fry have provided evidence that a lack of musical competence tends to be found in multiple members of the families of those demonstrating a lack of musical competence.

However, Kalmus and Fry's estimate that 4.2% of the adult population is tune deaf is flawed. The three-error criterion they use is arbitrary, and they have not ruled out alternative explanations of why participants deemed tune deaf perform poorly on the DTT. Secondly, their conclusion that tune deafness is an autosomal dominant trait requires further research. They present some data which supports this conclusion, but some of their data refutes it. Lastly, because Kalmus and Fry provide no histories of tune deaf participants in any of their experiments, one cannot conclude that tune deafness is an inborn trait. It could be that what Kalmus and Fry are observing throughout their paper is varying amounts of musical experience and exposure in the population. Because Kalmus and Fry do not provide evidence that tune deafness is an inborn condition, they do not refute Suzuki's claim that tune deafness is a result of poor musical environment.

Chapter 11

An Evaluation of Recent Research on Tone-Deafness: The Montreal Battery for the Evaluation of Amusia

In recent research the supposed inborn condition colloquially referred to as tone-deafness is termed “congenital amusia.” Brain injuries have been observed to result in the loss of perceptual abilities in pitch, rhythm, and metre, a condition referred to as “acquired amusia.” Different cases of brain injury have exhibited losses in perception in each of these elements to varying degrees. Sometimes, for example, brain-injured patients exhibit a loss in pitch perception but no loss in rhythm perception.

Isabelle Peretz and colleagues have coined the term “congenital amusia” as a more accurate term than “tone-deafness.” “Congenital” is used to distinguish this type of amusia from the acquired type, and implies that this type of amusia is inborn. The word “amusia” is utilized as it can be applied to different types of problems in perceiving music. Because acquired amusia has involved different problems in music perception it is theorized that there are different types of congenital amusia as well (Peretz et al., 2008).

As stated in the previous chapter, Suzuki believed that talent for music is not inborn. He likewise believed that tone-deafness, or a deficiency in musical talent, is not inborn. This belief is one of the basic principles underlying Suzuki’s philosophy of music education. Suzuki argued that what we observe as tone-deafness is the result of a poor musical environment. He further argued that one can rectify tone-deafness by having a person deemed tone-deaf listen to more music which is in tune. When the amount of in-tune music one has heard surpasses the amount of out-of-tune music one has heard, Suzuki argued, one’s tone-deafness would be rectified.

In this chapter I evaluate a recent test used to diagnose congenital amusia, the Montreal Battery for the Evaluation of Amusia (MBEA). Additionally, I compare and contrast the underlying definitions of tone-deafness present in the MBEA and Kalmus and Fry's (1980) Distorted Tunes Test (see chapter 10 for more on this test) and how these tests' definitions compare to the way Suzuki describes tone-deafness.

The Montreal Battery for the Evaluation of Amusia

The most widely accepted test for congenital amusia is the Montreal Battery for the Evaluation of Amusia (MBEA). (*Google Scholar* lists 306 publications which cite the MBEA.) This test was developed by Peretz and colleagues to study brain-injured patients who lost some component of their musical ability. The test has more recently been used to evaluate people with unexplained difficulties in music. (For a detailed description of the MBEA see Peretz et al. 2003).

In the MBEA, music perception is divided into six components: scale, contour, interval, rhythm, metre, and memory. Participants are administered six tasks, each of which tests ability in one of these components. Each of the subtests utilizes melodies from a set of 31 composed melodies.[10] (To see and hear samples of the stimuli used in the MBEA see: http://www.brams.umontreal.ca/plab/research. For complete scores of the stimuli of the MBEA see http://www.brams.umontreal.ca/plab/publications/article/57#downloads.)

To test the participant's ability in the perception of scale, the participant is presented with a series of pairs of melodies. Each pair of melodies contains a target melody and a comparison melody. The comparison melody is the same as the target melody in half of the trials and different in the other half. Participants are instructed to determine whether the two melodies are

[10] Peretz and colleagues (2003) write that there are 30 melodies, however, the scale, contour, interval, and rhythm subtests utilize 26 different melodies, while the metre subtest utilizes these 26 melodies and 5 additional melodies; a total of 31 melodies.

the same or different. When the comparison melody is different, it has one note which is different and is not in the key of the melody (See Example 1: The melody is in g-minor, but the comparison melody contains a C-sharp). However, this changed note does not change the contour of the melody. In Example 1, note that the first measure contains upward motion followed by downward motion in both the target melody and the comparison melody.

Example 1: Melody 8 in the Scale Subtest of the MBEA: a) Target Melody b) Comparison Melody

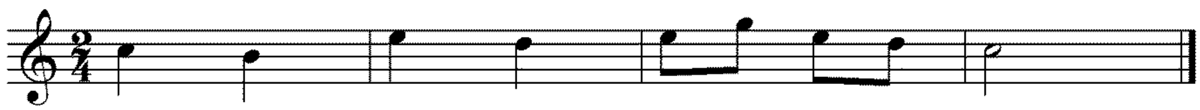
a)

b)

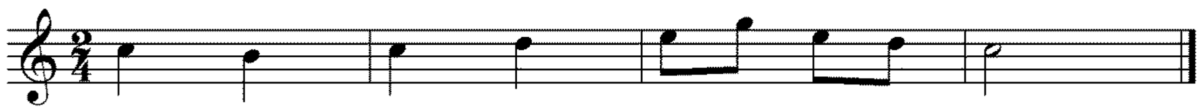
To test contour, the participant is presented with a similar task as that utilized for the scale subtest. However, in this subtest, when the comparison melody is different, a note from the target melody is changed such that the contour of the melody changes. By contour, Peretz and colleagues mean the direction by which notes are approached and left (upwards, downwards, or staying the same). For example, in Example 2, the first note of the second measure in the target melody is approached by upward motion and followed by downward motion. By changing the first quarter note of the second measure to a C in the comparison melody, the contour is changed such that the note is now followed by upward motion rather than downward motion. However, the new note, C, is still in the key of the melody, thus preserving scale.

Example 2: Melody 1 from the Contour Subtest of the MBEA: a) Target Melody b) Comparison Melody.

a)



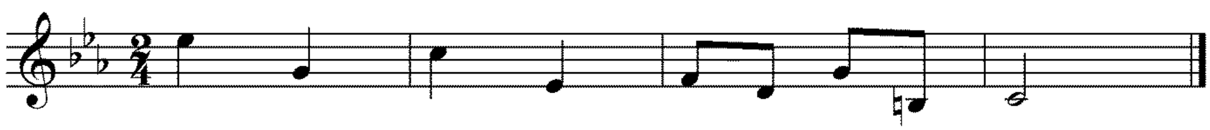
b)



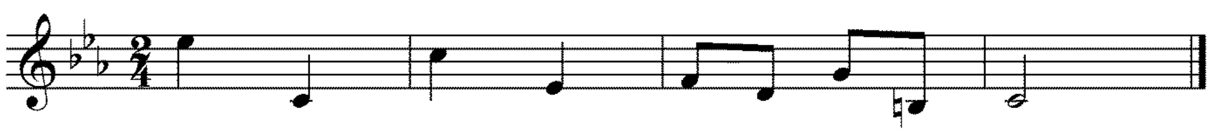
For the interval subtest, the participant is presented with pairs of melodies and again identifies whether they are the same or different. In this subtest, when the comparison melody is different, it differs by moving a note either up or down depending on the contour of the melody. The new note is still approached and followed with the same direction of motion (thereby preserving the contour of the melody), and the new note is still within the same key of the melody (thereby preserving scale). However, the intervals between the changed note and the notes on either side of it, are changed (see measure 1 of Example 3).

Example 3: Melody 2 from the Interval Subtest of the MBEA: a) Target Melody b) Comparison Melody

a)



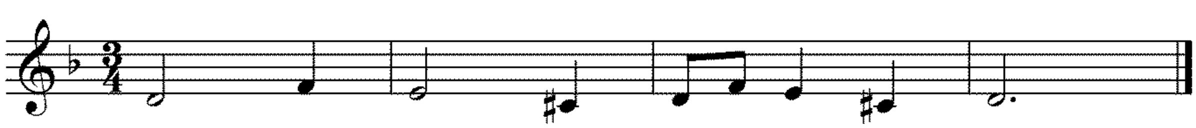
b)



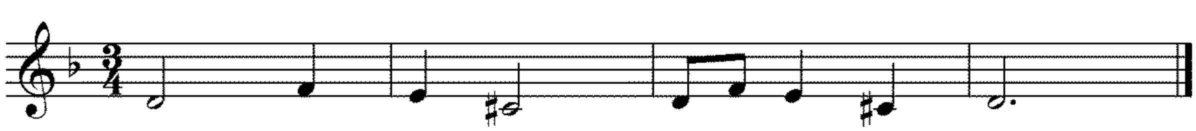
For rhythm, a same-different paradigm is also used. In the rhythm subtest, when the comparison melody is different, it is changed by altering the rhythm of two notes. For example, two quarter notes may be changed to a dotted quarter note and an eighth note. Similarly a half note followed by a quarter note may be changed to a quarter note followed by a half note, as in measure 2 of Example 4. In the comparison melody, the pitches always remain the same as does the metre.

Example 4: Melody 2 from the Rhythm Subtest of the MBEA: a) Target Melody b) Comparison Melody

a)

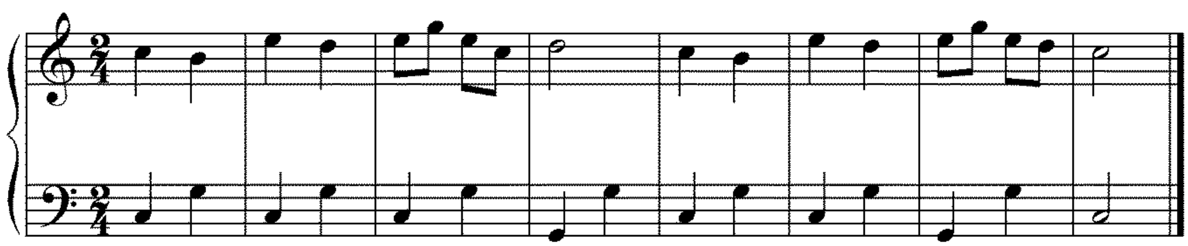


b)

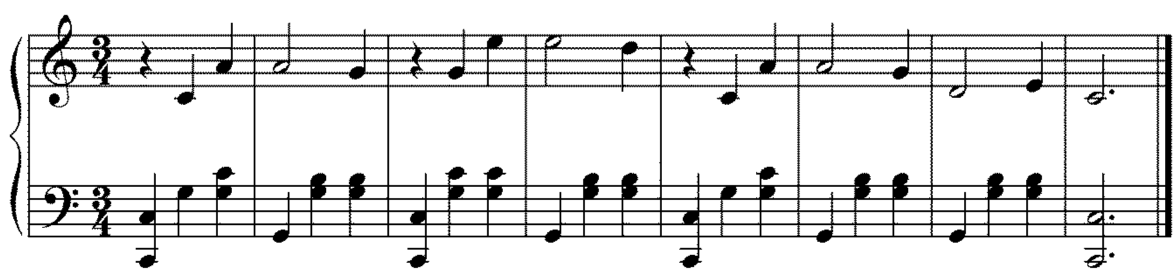


To test metre, participants listen to a series of melodies, half in duple meter and half in triple meter. Participants are asked to classify each melody as a march (duple meter) or a waltz (triple meter). Participants are presented with four practice trials before they begin, and are encouraged to tap along with the melody. The metre task is the only task of the MBEA in which the melodies are eight measures rather than four. It is also the only subtest in which the stimuli are not monophonic. Each melody in the metre task has a chordal accompaniment designed to accentuate the melody's duple or triple metre (See Examples 5 and 6 for examples of a march and waltz from the MBEA).

Example 5: Melody 1 from the Metre Subtest of the MBEA: March



Example 6: Melody 2 from the Metre Subtest of the MBEA: Waltz



Finally, for the memory subtest, participants are played a series of 30 melodies, of which half were used as target melodies in earlier subtests and half are previously unheard melodies. Participants indicate which melodies they have heard before and which are new. Participants are not informed at any point in the previous five subtests that they will be tested for their memory of the melodies later. Peretz and colleagues have not published musical scores for this subtest.

Peretz and colleagues (2003) use a composite score (the average of all six subtests) two standard deviations below the mean of control participants as a cut-off for the diagnosis of amusia. Using a set of norms based on 421 unselected participants provided by Peretz and colleagues, (see: http://www.brams.umontreal.ca/plab/publications/article/57#downloads) this would mean a composite score of less than 22.25 out of 30 would indicate amusia (see Table 1).

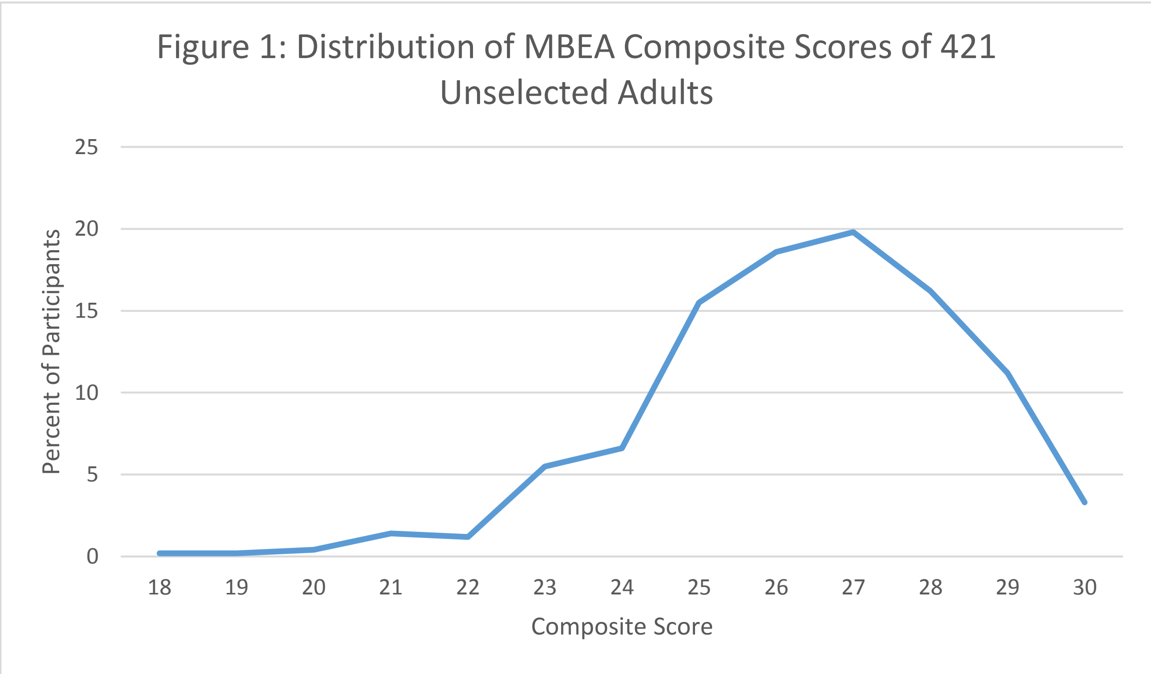
Table 1: Mean Scores of 421 Unselected Adults on the MBEA

Test:	**Scale**	**Interval**	**Contour**	**Rhythm**	**Metre**	**Memory**	**Composite**
Mean	26.34	26.19	25.74	26.69	25.59	27.08	26.27
SD	2.68	2.62	2.85	2.64	4.32	2.51	2.01
Mean-2SDs	20.98	20.95	20.04	21.41	16.95	22.06	22.25

The MBEA, the DTT, and Suzuki's Claims

The MBEA is similar to the DTT utilized by Kalmus and Fry (1980, see chapter 10) in that it can test the musical abilities of a layperson. Creating a test that evaluates the musical ability of someone who is not musically trained is difficult. This is the result of a layperson's lack of the following: vocabulary to describe music, knowledge of musical notation, and training in music performance. The DTT overcomes this problem by asking participants to identify if a melody was played with wrong notes. The MBEA overcomes this problem through the use of the same-different paradigm employed by most of the tasks in the test. The tasks that do not use a same-different paradigm only require the participant to choose one of two answers, an activity a layperson is able to complete. In short, the MBEA and the DTT require no formal musical training.

The MBEA and DTT show some similarity in the distribution of scores found in large samples of unselected adults. (For the distribution of scores on the DTT for 602 unselected adults see chapter 10, figure 2; for the distribution of scores for 421 adults on the MBEA see figure 1; for the distribution of scores for 421 unselected adults for each subtest of the MBEA see Appendix 2)



This graph was created using data provided at:
http://www.brams.umontreal.ca/plab/publications/article/57#downloads

Both the distribution of the MBEA scores and the DTT scores are positively skewed and most of the participants score well. Both distributions are also continuous with no clear boundary to distinguish those that are amusic from that are normal. In other words those deemed amusic or tune deaf represent those that scored lowest on a continuous distribution rather than a distinct group.

However, the distributions of scores of the tests have a notable difference. While the scores of the DTT showed a J-shaped distribution with the majority of participants scoring perfectly, the composite scores of the MBEA showed a distribution more similar to a normal distribution. Nonetheless, the scores still violate normality by a Kolmogorov-Smirnov test. A plurality of participants scored 27 (90%) and only 3.3% of participants scored perfectly. Because few participants scored perfectly and the scores are more variable in the MBEA, it is perhaps a better test for assessing musical ability of the general population as unlike the DTT, it can distinguish different degrees of musical ability in those that score well. (It should be noted that all of the subtests of the MBEA show similar distributions of scores, except for the metre subtest in which a plurality of the participants score perfectly, and the distribution is J-shaped, although the scores decrease less rapidly than the distribution of scores on the DTT. See Appendix 2.)

The DTT and the MBEA are also similar in that they use a similar number of stimuli; Kalmus and Fry uses 26 melodies and the MBEA uses 30 melodies for each subtest. These stimuli are similar in length in that both tests utilize melodies of a few measures. Kalmus and Fry's stimuli have a mean of 16.9 notes (SD=4.4) and the melodies of five subtests of the MBEA have a mean of 10.3 notes (SD=3.2) (The metre subtest uses melodies that are twice as long.) The MBEA utilizes melodies that are four measures in length and Kalmus and Fry use "a few bars" (p. 369) from the beginning of well-known songs. 11 out of the 17 incorrectly played melodies in the DTT are four measures long (see Appendix 1).

There are, however, a number of notable differences between the two tests. Firstly, the DTT utilizes familiar melodies while the MBEA utilizes newly composed melodies. As a result, the DTT is affected by participants' familiarity with the stimuli themselves while the MBEA is not. This makes the MBEA a better tool for determining whether Suzuki's claim is valid as poor performance on the DTT can be explained by lack of exposure to the stimuli and not an inborn deficiency, while this argument cannot be made with the MBEA.

The DTT and the MBEA also differ in the number of errors present in the stimuli and in the type of error. The MBEA subtests only contain one error in each comparison melody which is different from the target melody, while in the DTT the incorrectly played melodies contain a range of two to eight errors (Mean=5.1, SD=1.7). It should be noted, however, that this difference is reduced when one considers adjacent errors. That is, one can argue that two or more adjacent notes changed the same way should be considered one error rather than multiple errors. For example, in King Wenceslas (see Melody 16 in Appendix 1) the last seven notes are all lowered a semitone. Perhaps this should be considered one error and not seven. If one takes this into consideration, the stimuli of the DTT have a range of one to seven errors (Mean=3.1, SD=1.5). Nevertheless, the DTT and the MBEA differ in the number of errors present in the stimuli.

However, the locations of errors in the stimuli are similar. In both tests, the first note of the melody is avoided. In the DTT the earliest error is on the fourth note of a melody. The MBEA tends to place the changed note towards the middle of the melody; however, in four (out of 15) melodies the change happens on the second note, and in two (out of 15) melodies the change happens on the fourth. The change never occurs on the third note. Similarly, both tests mostly avoid the last note of the melody; the MBEA never changes the last note, and the DTT changes the last note in four of the 17 melodies. However, in three of these melodies the change is the same as a change affecting the penultimate note, and thus, as mentioned above, may not be considered an error.

While the location of errors may be similar in both tests, the type of errors present in the two tests are different. Firstly, the DTT does not address rhythm or metre as the MBEA does. Kalmus and Fry limit what they term “tune deafness” or “dysmelodia” (p. 269) to a pitch perception deficiency. Suzuki (2012) does not describe in great detail what deficiencies a person deemed tone-deaf exhibits; rather he describes it as a general lack of musical ability. However, in one place in *Nurtured by Love*, Suzuki does provide more detail, and it seems, like Kalmus and Fry, he limits the condition to pitch perception and not rhythm. As mentioned above (p. 113):

> In the case of tone deafness, many children produce overly high pitched semi-tones; for example the note *fa* in the first four notes of the major scale, *do, re, mi, fa*. These children have already been nurtured to hear and produce a slightly high-pitched *fa* (p. 116).[11]

Thus, while the MBEA’s inclusion of rhythm and metre subtests may make it a better test in that it can test more elements of music perception, it does not give the MBEA an advantage with regard to evaluating what seem to be Suzuki’s claims.

There is also a notable difference between the two tests regarding the size of the difference of affected notes. In the DTT changes are mostly that of one semitone and sometimes two semitones. However, in the MBEA, the pitch changes are larger. For example, in the scale subtest, the smallest change utilized is moving a note three semitones from its position in the target melody, and this change is only employed once. The remaining melodies have changes ranging from four to six semitones (Mean=4.4, SD=1.1).

This also provides evidence that Kalmus and Fry’s definition of tune deafness is more in line with Suzuki’s. In Suzuki’s above quote, he notes that people considered tone-deaf sing and

[11] This might betoken a decrease in the contrast between semitones and whole tones, so that an equiheptatonic tuning is approached, rather than a diatonic tuning.

hear semitones as too large. Thus when they sing and perceive melodies they generally follow the melodic shape but sing notes which are out of tune. The DTT, in utilizing smaller changes of pitch, is thus perhaps a better test for tone deafness as Suzuki illustrates it.

However, the MBEA is advantageous for investigating Suzuki's claims in that it includes tests which do not contain violations of scale, namely the interval and contour subtests. In addition to providing a more complete evaluation of a person's music perception ability, they also help control for amount of exposure to Western music. That is, all of the DTT melodies which contain errors contain notes which are out of the key of the melody and thus violate typical practices of Western music. As discussed in the previous chapter, Kalmus and Fry found evidence that exposure to Western music as a whole affected scores on the DTT. They reached this conclusion because Asian participants who had little experience with Western music performed poorly on the DTT, while European participants, who were not familiar with the stimuli themselves, were still able to successfully complete the task (see chapter 10). In the MBEA, only the scale subtest contains notes that are out of the key of the melody. The interval and contour subtests do not contain notes out of the key of the melody, and thus the MBEA may be able to isolate those with inborn perception deficiencies from those who have had little exposure to Western music. However, testing this hypothesis is warranted before making a final conclusion. Because Suzuki argued that tone-deafness is not inborn but rather the result of a musically deficient environment, a test which can control for exposure to Western music is preferable.

Both the DTT and the MBEA ignore a musical element essential to Suzuki's pedagogy, that of fine distinctions of timbre. A main focus of Suzuki's teaching was developing tone. As Suzuki writes, "I invest considerable effort to foster in students the ability of mind and ear to listen critically to one's own tone" (Quoted in Kuramochi, 2000 p. 84). "Tone" here refers to the quality of sound one produces from the violin. As Fumiyo Kuramochi writes:

> Dr. Suzuki defines 'Shizen-on no genon' as "The original tone of a natural tone, that is, the resonance that lingers after plucking." Regarding 'Shizen-on' or 'Natural tone' he writes, "The tone that rings on when a string is plucked is a beautiful pure tone. Let your child listen often to it, and start practice sessions with attempts to create the same tone color with the bow. I am naming this 'natural tone.'" (p. 84)

The focus on training the perception of tone, Suzuki felt, was a fundamental part of training violinists. Suzuki was not unique in his emphasis on developing tone; however, he was unique in advocating this type of study at the early stages of training. Thus to investigate Suzuki's claims regarding tone deafness, it would be worthwhile to create a test which examined one's ability to discriminate different timbres, or as Suzuki would say, different qualities of tone.

This is further important because Suzuki used tone as a means to train pitch perception. The tone that Suzuki advocated relied on the resonance created by the sympathetic vibrations of violin strings when notes corresponding to harmonics of the open strings are played. Suzuki students are trained (typically right before beginning volume 2 of the *Suzuki Violin School*) to listen for this resonance. Notes on which this sympathetic vibration occurs are often referred to as "ring tones" by North American Suzuki teachers (see Kreitman, 1998 p. 51). Suzuki students are trained to listen for this resonance as in addition to creating a superior tone, it indicates that they have played the note in tune. If the finger is placed slightly sharp or flat the sympathetic vibration will not occur. As Edward Kreitman writes:

> The next step is to take each ringing tone one by one and practice adjusting the position of the finger on the string until you actually see or hear the vibration of the corresponding open string. Care must be taken to ensure that nothing is inadvertently touching the open string. When doing this step, I ask the student to use a short bow stroke that ends on the string. After each bow stroke ends, we listen to hear a

> *ring* from the corresponding open string. This process of *listening and adjusting* is tedious and slows down a student who likes to race through pieces without much regard for proper placement of fingers, consequently playing fast and out of tune. I usually find that I can eliminate the finger tapes[12] on the violins of students who have developed this technique because their sense of playing in tune is based on a combination of listening and a tactile sensitivity to where the fingers' placement creates resonance, rather than on a visual aid. I like to teach this technique of *absolute* pitch[13] at the very beginning of Book 2 because it allows the student to take advantage of the preponderance of ringing notes in the book 2 pieces (pp. 51-52).[14]

Thus in addition to being a fundamental element of music perception, this training in tone perception, Suzuki felt, facilitated training in pitch perception.

Conclusions

The MBEA has become the predominant test used in recent research to diagnose tone-deafness, or congenital amusia. Both the MBEA and the DTT when employed with large samples of unselected adults have shown that musical ability varies in a continuous manner throughout the population. That is, there is no clear cut-off point that distinguishes tone-deaf people from normal people. The MBEA is more advantageous for studying congenital amusia than the DTT as the scores show more variability. The MBEA is also not effected by participants' familiarity with the stimuli and is likely less affected by the amount of exposure to Western music of the participants. However, the DTT is structured such that it tests for tone-deafness more closely to the way in which Suzuki defines it. Both tests however, neglect the perception of timbre, a key element to music training according to Suzuki. In the next chapter I

[12] Kreitman is referring to the common practice of placing narrow tapes on the fingerboards of violins of beginning Suzuki students to serve as an indicator for where to place their fingers.
[13] The term *absolute pitch* usually refers to the ability of some people to determine what a pitch is without the use of a reference pitch. Here however, Kreitman uses the term to mean the ability to determine if a pitch is in tune based on the resonance it creates.
[14] Suzuki does not write much on this topic. However, both Starr (1976) and Garson (2001) trace the practices described above to Suzuki's teaching.

evaluate case studies which employ the MBEA as a means to determine whether there is evidence that congenital amusia is inborn. In other words, using these case studies, I determine whether Suzuki is correct in saying a lack of musical talent is not inborn, but rather the result of deficient musical environment.

Chapter 12

An Evaluation of Recent Research on Tone-Deafness: Empirical Case Studies

As stated in the previous two chapters, Suzuki believed that a lack of musical talent, or tone-deafness is not an inborn condition. Rather Suzuki believed that what we observe as tone-deafness is the result of a musically deficient environment. However, recent research has concluded that there is an inborn condition with which people have a deficiency in music perception. This condition is most often referred to as congenital amusia in recent research. The condition is most often studied using a test titled the Montreal Battery for the Evaluation of Amusia (MBEA) (see chapter 11 for a description of the MBEA). In this chapter I present the findings of recent case studies of congenital amusia. I determine whether there is reason to reject Suzuki's claim that tone-deafness is not inborn. I further determine whether there is evidence that supports Suzuki's claim that what we deem tone-deafness is actually the result of a musically deficient environment.

Case Studies that Utilize the MBEA

The Case of Monica

The first empirical case study which claims to document a case of congenital amusia was published in January of 2002 by Peretz and colleagues. In this case study, a participant named Monica was recruited through a newspaper advertisement soliciting people who considered themselves "musically impaired" (p. 185). From 37 people who responded to the advertisement, Monica was selected as the most obvious case of someone with musical impairment.

Monica is a middle-aged woman who appeared normal in every regard. A quantitative volumetric examination of her brain revealed no "obvious cortical atrophy or pathology" (p. 186) and a thorough audiological exam revealed no abnormalities in her hearing. Lastly, she scored above average in a standard intelligence test. Yet, Monica appeared to have musical impairment. Monica and 57 non-musician control participants were tested using the contour, interval, rhythm, and metre subtests of the MBEA. She was also tested using a variation of the scale subtest. In this variation, which did not employ the same-different paradigm, Monica heard 30 melodies (the same as those used in the MBEA) half of which contained a note out of the key of the melody. She was asked to determine whether the melodies contained a "wrong note" (p. 187). On all of the subtests, except metre, Monica scored at chance levels: specifically, her scores were three standard deviations below the mean of control participants. On the metre subtest, she correctly identified 19 out of 30 melodies, which was in the range of controls (18-30). (The probability of labeling at least 19 melodies by guessing is 0.10.)

Monica's musical disabilities were further explored through additional tests. In one test, she and 33 control participants heard 52 melodies without lyrics of songs that are familiar to most people in Quebec (where Monica lives). (4 controls were matched to Monica's age and education level, and the remaining 29 controls were unselected adults.) For each song, participants were instructed to pick the correct title for each song out of four possible titles. Matched control participants had a mean score of 51 correct responses, and unmatched control participants had a mean of 50. Monica however, correctly identified only 22 melodies. In short, Monica failed to identify titles of well-known songs on the basis of their melodies.

This finding is surprising as it is in contrast to the results of the first test of tune deafness designed by Kalmus and Fry (1980) (see chapter 10). Before creating the DTT, Kalmus and Fry designed a test for tune deafness which asked participants to identify well-known melodies. Kalmus and Fry do not provide data from the results of this test. All they report is "our first test for tune deafness, which required the naming of tunes, proved to be inefficient" (p. 369). However, one assumes that if the test did not serve as a means to identify tune deaf individuals, everyone was successful at the task. Thus one concludes that Monica is highly unusual and her musical impairment is perhaps more severe than the cases studied by Kalmus and Fry.

In another test Monica was presented with a series of five tones. The first, second, third, and fifth tones were the same in pitch. The pitch of the fourth tone was the same as the others or differed from them by one to 11 semitones. Monica was instructed to determine if all of the notes were the same or if one was different. She did not detect a pitch difference when the fourth tone was lower than the others, even if it was 11 semitones below the others. When the fourth tone was higher than the others, she performed better. Monica was able to determine when the fourth tone was higher by 4 semitones 90% of the time. This ability decreased to chance levels for differences of two semitones and one semitone. In English-language prosody, higher pitch corresponds to stress accent, which could explain Monica's better performance on higher pitches

Monica performed slightly better when the task was changed such that she was comparing two tones. She always detected a difference between two tones when they were 4 semitones apart regardless of whether the second tone was higher or lower than the first. As the size of the interval decreased, however, Monica's success in detecting a difference decreased as well. She did not detect a difference between two tones when they differed by a semitone. Monica's poor performance on the two pitch discrimination tasks led Peretz and colleagues to conclude that "the ensemble of musical deficits are cascade effects of a faulty pitch-processing system" (p. 189). However, it should be noted that Monica's poor performance on the rhythm subtest was not further explored. It should also be noted that discriminating differences greater than a semitone might be sufficient for speech perception where larger intervals are utilized but not for the perception of Western music.

In order to determine whether Monica's perceptual disability was unique to the musical domain, Peretz and colleagues administered three speech perception tasks to Monica and the 33 control participants. In the first task, participants were presented with phrases which could be understood as questions or statements: for example, 'He speaks French?' and 'He speaks French.' The phrases were computer edited such that the only cue participants could utilize to determine if phrases were statements or questions was pitch. For questions, the phrase ended with a rise of three to 11 semitones; statements ended with a descent of two to three semitones. Both Monica and the control participants had little trouble in this task. Monica scored 100% and the mean of control participants was 98%. It should be noted that in English-language prosody a descent is a gradual descent. Additionally, since this task was a binary-forced choice task, Monica would only have had to identify questions which had a pitch 3 to 11 semitones higher. This could explain the presence of a music deficit without a speech perception deficit.

In another task, participants were presented with phrases in which a word was accentuated by means of a rise of eight semitones and no other auditory cues to create emphatic stress on that word: for example, 'SING now please,' and 'Sing NOW please.' Participants were asked which word bore the stress. Monica was able to correctly identify the word with the emphasis 77% of the time and the mean for control participants was 87%, with a range of 67-100%.

In the remaining speech task, participants were asked to identify the voices of 33 well-known speakers. Peretz and colleagues do not provide examples of who these speakers are; the only information given is that they are "well-known" (p. 187). The speech excerpts were selected such that participants could not use contextual word cues to identify the speakers, but rather had to rely on auditory cues. Monica correctly identified 30 of the 33 speakers, and control participants had a mean of 30 correct responses (SD=3.1).   Because Monica scored comparably to control participants in all three speech-perception tasks, Peretz and colleagues concluded that Monica's disability was unique to the musical domain and her speech intonation perception is normal.

The Case of 11 Amusics

In a similar study, published in February of 2002, Julie Ayotte and colleagues document the cases of 11 adults claimed to be congenitally amusic. These 11 adults were chosen from a group of 37 people who responded to an advertisement soliciting musically impaired people. 22 of the 37 adults exhibited a pronounced deficiency in musical perception when tested with the MBEA. However, only 11 were both willing to participate and matched the eligibility criteria for the study. The eligibility criteria included:

> (i) a high level of education, preferably university level to exclude, general learning disabilities or retardation; (ii) music lessons during childhood, to ensure exposure to music in a timely fashion; (iii) a history of musical failure that goes back as far as they could remember, to increase the likelihood that the disorder is inborn; and (iv) no previous neurological or psychiatric history to eliminate an obvious neuro-affective cause (p. 239).

The 11 adults were given a standard IQ test, and an audiological exam. With regard to the IQ test, all 11 participants appeared normal. In the audiological exam, two participants exhibited hearing loss that was not congenital, but rather was the result of prolonged exposure to loud noises in one case and chronic ear infections in childhood for the other. Because the audiological exam did not interfere with results of subtests of the IQ test that involved auditory (non-musical) stimuli, Ayotte and colleagues ruled out the possibility that the audiological impairment would affect these two participants' performance on musical tasks.

The 11 participants and 61 non-musician control participants were tested with the MBEA. Additionally, 20 matched control participants were tested. These participants were matched for similarity of age, education level, and musical experience to the 11 amusics. Ayotte and colleagues do not specify how they assessed musical experience. It appears from their article that this matching merely meant that both amusics and controls had some form of music lessons as children.

The results from the MBEA administered to the 11 amusics, 61 unselected controls, and 20 matched controls are presented in Table 1.

Table 1: Scores on the MBEA of 11 Amusics, 61 Controls, and 20 Matched Controls

Participant	Scale	Contour	Interval	Rhythm	Metre	Memory	Composite
A1	*76.7	x50.0	x56.7	x53.3	63.3	x*66.7	x61.1
A2	x60.0	x43.3	x56.7	*73.3	*66.7	x53.3	x58.9
A3	x50.0	x50.0	x50.0	x50.0	56.7	x50.0	x51.1
A4	x50.0	*70.0	x50.0	x53.3	53.3	x46.7	x53.9
A5	x56.7	x53.3	x50.0	x53.3	60.0	x40.0	x52.2
A6	x46.7	x46.7	x53.3	*76.7	*76.7	x53.3	x58.9
A7	x63.3	*80.0	x60.0	*76.7	60.0	*73.3	*68.9
A8	x53.3	x56.7	x53.3	x63.3	*70.0	x50.0	x57.8
A9	x56.7	*66.7	*73.3	*96.7	*73.3	*73.3	*73.3
A10	x53.3	x53.3	x53.3	x63.3	*73.3	*73.3	x61.6
A11	x46.7	*66.7	*73.3	*93.3	*70.0	*80.0	*71.7
Mean	x55.8	x57.9	x57.3	*68.5	65.8	x60.0	x60.9
SD	8.7	11.4	8.5	16.3	7.6	13.6	7.6
Mean of Controls	91.7	90.2	89.3	91.5	81.6	89.5	89.0
SD of Controls	6.8	7.0	7.9	6.8	9.9	7.2	7.6
Mean of Matched Controls	90.0	91.5	88.7	91.7	83.5	92.8	89.7
SD of Matched Controls	7.8	6.4	7.2	8.2	10.3	6.3	7.7

Adapted from Table 1 of Ayotte et al, 2002. Scores more than three standard deviations away from the mean of controls are displayed with an x. Scores not likely achieved by random guessing ($p<0.05$) are displayed with a *. This assumes each participant is equally likely to guess either answer, and the probability of guessing correctly on 20 or more of the 30 trials (at least 66.7%) is less than 5%.

As shown in Table 1, the mean scores on all of the subtests for the 61 unselected adults are much higher than those expected to be achieved by random guessing (a score higher than 66.7%, $p<0.05$). In contrast, the mean scores of all the 11 amusics on each of the tests are below chance levels except for the rhythm subtest which was slightly higher. The composite score for all the 11 amusics on all the tests is more than two standard deviations below the controls, thus confirming the diagnosis of congenital amusia as Peretz and colleagues define it. Ayotte and colleagues further note that several of the amusics' scores satisfy a more "conservative cut off"

(p. 240) of three standard deviations below the mean of unselected controls. While a composite score of two standard deviations below the mean was used to diagnose amusia, Ayotte and colleagues suggest that a subtest score of three standard deviations below the mean of controls indicates "the presence of a deficit" (p. 240) in that particular element of music perception.

The mean scores of the amusics on the scale, contour, interval, and memory subtests were all three standard deviations below the mean of unselected controls. In particular, Ayotte and colleagues note that all 11 amusic participants scored more than three standard deviations below the mean of unselected controls on at least two out of the three subtests which utilize pitch changes (scale, contour, and interval). Thus all amusics had a deficit in perception in at least two out of three types of pitch changes. In contrast, the mean score of the amusic participants on the rhythm and metre subtests were above the three standard deviation cut off.

With regard to amusics scoring above the three standard deviation cut off for the metre subtest, Ayotte and colleagues, argue this is "probably because this task was relatively difficult for a few control subjects as well" (p. 240). This relative difficulty made the mean of unselected controls lower which made scoring within three standard deviations easier for the amusics.

However, it should be noted that five of the amusics scored above chance levels on this subtest (see Table 1). Five participants also scored above chance levels on the rhythm subtest, but for all other tests, fewer than five participants scored above chance levels. Additionally the mean score of amusics on the metre subtest (65.8) was second highest of all of the subtests (the rhythm subtest had a mean score of 68.5) and is just below the score (66.7) that would counter-indicate guessing ($p<0.05$). In short, amusics performed better on the metre subtest than all other subtests, except for rhythm. Thus, the amusics' scoring above the three standard deviations may not be merely the result of controls scoring relatively poorly, but may also be the result of less severe impairment in metre perception in amusics as compared to other subtests.

A possible explanation for the amusics' relative success on the metre subtest is found in the stimuli. The chordal accompaniment of the stimuli of the metre subtest was always in quarter notes, with a bass note as the down beat. The chordal accompaniment for a waltz was always a low note followed by two higher notes, and the accompaniment for a march was always a low note followed by one higher note (see Chapter 11 Example 5). Thus, one could perform well on this task simply by counting the number of notes between each bass note in the accompaniment, and not by perceiving the metre. If some control participants utilized this feature of the accompaniment and not others, it may explain why control participants had more variable scores for this test. Similarly, if some of the amusics relied on this aspect of the stimuli, it may account for their relatively high scores on the subtest.

In the rhythm subtest, the mean score of unselected controls was comparable to all of the other subtests (except for metre as mentioned above); thus, unlike the metre subtest, it was likely comparable to the remaining four subtests in difficulty. The mean score for amusics was both above the three standard deviation cut off and above chance levels. This suggests that rhythmic perception in amusics is less impaired than the other elements of music tested.

However, as shown in Table 1, the amusic participants' subtest scores were variable, and thus thinking of them as a group may be problematic. The rhythm subtest showed the most variability with a range of scores from 50.0 to 96.7. The scores on the rhythm subtest divide into three groups. Six of the 11 amusics, namely, A1, A3, A4, A5, A8, and A10, performed as expected if they were guessing. Three, A2, A6, and A7, performed above chance levels, and above the three standard deviation cut off, but were still in the lower range of controls. Two, A9 and A11, both scored higher than the mean of controls (96.7 and 93.3 respectively). Thus rhythmic perception seems more variable in amusics than other elements of music, with some amusics showing no deficiency compared to controls.

Like the rhythm subtest, the three subtests related to pitch (scale, contour, and interval) showed variability as well. In the scale subtest only one participant scored above chance levels ($p<0.05$) and above the three standard deviation cut off. In the contour subtest, four participants performed above chance levels, and in the interval subtests two participants performed above these criterion values. Only two of the participants performed above chance levels on two of these subtests, and none on all three, indicating that greater ability in one element does not mean greater ability in the others. This is further evidence that amusia varies depending on the individual, and perhaps the musical impairment experienced by amusics is not a result of a single underlying disorder.

Furthermore, with few exceptions the scores on the MBEA were not highly correlated (see Table 2).

Table 2: Correlations (Spearman's Rho) of MBEA Scores for 11 Amusics

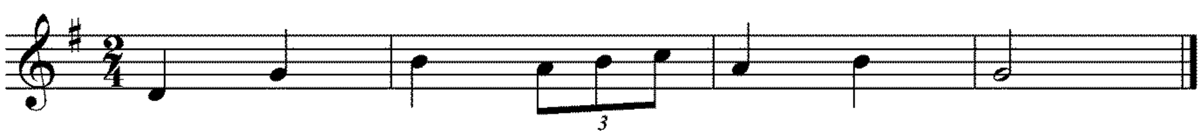
	Scale	**Contour**	**Interval**	**Rhythm**	**Metre**	**Memory**
Scale	1.000	-.016	.282	-.072	-.210	.077
Contour	-.016	1.000	.241	.271	-.244	.239
Interval	.282	.241	1.000	.826**	.464	.866**
Rhythm	-.072	.271	.826**	1.000	.675*	.720*
Metre	-.210	-.244	.464	.675*	1.000	.522
Memory	.077	.239	.866**	.720*	.522	1.000

* indicates significance at $p<0.05$ and ** indicates significance at $p<0.01$

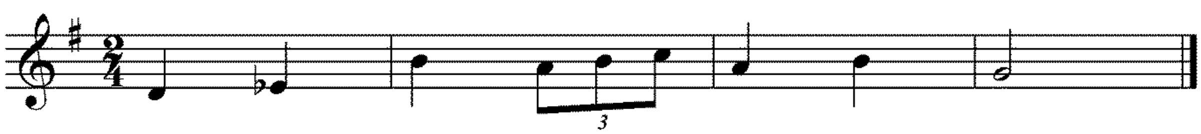
In particular, scores on the three pitch related tasks were not substantially correlated. This is particularly surprising because the scale and interval subtests are similar. The comparison melodies, when they differ from the target melodies for both subtests, always use the same scale degrees. However, in the scale subtest an accidental is added. For instance, in Example 1, both comparison melodies (b and c) have a changed second quarter note in the first measure. In the interval subtest (c) the changed note is an E and in the scale subtest this note is an E-flat (b). This similarity is maintained throughout both subtests. One would thus suspect, due to the subtests' similarity, that participants would score similarly on both tests and thus the scores would correlate. However, this was not the case. Thus it seems, the music perception deficiencies experienced by the amusics are variable and individualized. One should therefore use caution when categorizing amusics as a unified group and avoid regarding even closely related subtests as assessing the same capacity. One should also use caution when using composite scores on the MBEA as the way in which the subtests contribute to the composite score varies.

Example 1: Melody 6 from the Scale Subtest and Melody 26 from the Interval Subtest: a) Target Melody b) Comparison Melody from the Scale Subtest c) Comparison Melody from the Interval Subtest

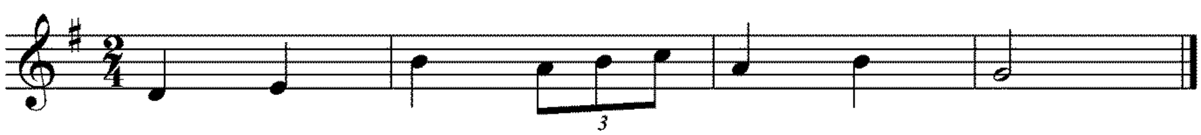
a)



b)



c)



The correlation of the rhythm and metre subtests is not surprising as both involve temporal elements of music; however, the substantial correlation of scores on the interval subtest with rhythm is surprising. Furthermore both interval and rhythm are the only subtests that are substantially correlated with memory. These correlations suggest a surprising commonality in the perception of these two elements that might warrant further research.

After testing the participants with the musical battery, Ayotte and colleagues administered additional tests to the amusics and the 20 matched controls. In one test, participants were presented with 30 familiar folksong melodies and the 30 melodies from the scale subtest of the MBEA. In half of the trials, one note of the melody was raised or lowered by a semitone such that it preserved the contour of the melody but did not belong to the key of the melody. Thus, this test was similar to the DTT except that it used only one error per stimulus. Participants were asked to determine whether the melody contained a "wrong note." The amusics performed close to chance levels on this task and scored significantly lower than controls.

In a second test designed to assess sensitivity to dissonance, amusics and matched controls heard 24 musical excerpts which were "highly consonant and taken from pre-existing classical music" (p. 242). Each excerpt was played twice, once in its original form, and once with all of the pitches of the leading voice shifted up or down by a semitone, thereby creating dissonance with the accompaniment. Participants were asked to rate each excerpt on a ten-point scale of how pleasant they found each excerpt (1 was labeled very unpleasant and 10 was labeled very pleasant). The control participants rated the consonant excerpts with highly pleasant ratings and the dissonant excerpts with highly unpleasant ratings. In contrast, the amusic participants rated all melodies as weakly pleasant. Thus Ayotte and colleagues conclude that the amusics are significantly less sensitive to dissonance than the controls. It should be noted that the difference between the musical stimuli involved a semitone as in the task discussed above. Thus, this may be further evidence that amusics do not perceive a difference between tones that are a semitone apart.

Using the same 24 excerpts, Ayotte and colleagues administered a test which sought to determine whether the amusics could determine the "affective tone" (p. 243) of music. Half of the 24 excerpts were in a major mode and were played with a median tempo of 138 beats per minute; these excerpts were considered "happy" (p. 243). The other excerpts were in a minor mode and were played with a median tempo of 53 beats per minute; these excerpts were considered "sad" (p. 243). Participants were asked to rate, on a 10-point scale, how happy or sad the excerpt was (1 was labeled sad and 10 happy). Control participants and amusic participants rated the excerpts similarly. From this finding, Ayotte and colleagues conclude that the amusics' inability to perceive musical pitches normally "is not the result of poor auditory attention or a deficient affective system in general" (p. 243). However, as discussed above, amusics sometimes do not show a deficit in temporal deficits such as rhythm and metre. It is plausible that the amusic participants relied on temporal cues to rate the melodies as happy or sad.

Ayotte and colleagues administered two of the speech tasks that were used in the case study with Monica to the amusics and matched controls. In the first task, participants were asked to determine if a phrase was a statement or a question, and in the second task participants were asked to identify which word of a phrase contained a stress created by a change in pitch. The amusics and matched controls both performed well on the tasks with no significant difference between the two groups. This was expected as it replicated the results of the case study with Monica. These results provide evidence that congenital amusia is domain specific.

Ayotte and colleagues also presented the same phrases used in the two experiments mentioned above in a new task in which participants heard a phrase twice and had to determine if it was spoken in the same way or differently (meaning did the word on which the stress occur change and were phrases either both questions or statements or one of each). As expected, both amusic and matched controls performed well with no significant difference between the scores of the two groups. However, in a variation of this task, all linguistic information was removed from the recordings, and only the intonation contours of the speech were maintained. When asked if the two resultant intonation contours were the same or different, the control participants performed almost perfectly, while the amusic group performed significantly worse. From this, Ayotte and colleagues conclude that congenital amusia does not inhibit pitch perception in speech but only if lexical information is present. This suggests that congenital amusia is not completely domain specific; rather Ayotte and colleagues deem congenital amusia "music relevant" (p. 250).

However, these results differ from those of a similar study utilizing the same methodology by Aniruddh Patel and colleagues (2008) who found that 30% of amusics had difficulty differentiating between a question and a statement when the same phrase was used. Surprisingly, these same amusics did not have trouble distinguishing intonation contours when the words were eliminated. This further supports the conclusion that amusia manifests in individualized ways. Patel and colleagues, like Ayotte and colleagues, used the MBEA to confirm that participants were amusic. However, Patel and colleagues did not report the scores obtained by amusics on the MBEA. Thus the differing results may be explained by differing degrees of deficits in different elements of perception. The differing results may also be explained by the use of different stimuli; however, the stimuli used by Patel and colleagues involved larger changes in pitch than those used by Ayotte and colleagues on both of the tests. This may explain why the amusics performed better with the intonation contours but it does not explain why amusics were not able to determine if a phrase was a question or a statement. Thus, the results of Patel and colleagues suggest that in at least some cases of observed amusia, the deficit has exhibited speech intonation problems, and thus in some cases the perception deficiency is not domain specific. Further research is warranted, using more subtle and more variable stimuli, to investigate whether all those diagnosed with congenital amusia exhibit a deficiency in speech intonation perception, and this deficiency exists to varying degrees, or sometimes amusics' perception deficiency extends to the speech domain and sometimes not.

To further test the extent of domain specificity of the perception deficits of the amusics, Ayotte and colleagues presented the 11 amusics and matched controls with four more tasks. In one task participants had to name the source of recorded sounds such as recordings of animal noises and transportation vehicles. In a second task, which was also utilized in the Monica case study, participants were asked to identify well-known speakers based on recordings of their voices. In both tasks if a participant could not identify the source of the sound or the name of the speaker, they were presented with a choice of four options which were similar and asked to identify the correct one. Both groups of participants performed well with no significant difference between the groups. The amusics' ability in these tasks provides further evidence that the perception deficits experienced by congenital amusics are limited to the musical domain.

In the remaining two tasks participants were asked to identify common folk songs. In one task participants were played 52 melodies without lyrics of familiar folk songs and asked to identify them. In the other task, participants were read lyrics from 25 familiar folk songs and asked to identify the title of the song. The lyrics were selected in such a way that the text did not provide direct clues concerning the title. As in the previous two tasks, when participants could not identify the song, they were presented with four semantically related options and asked to identify the correct option. For example if the song was a Christmas carol, the participants were presented with four titles of Christmas carols.

In the spoken lyric task there was no significant difference between the performance of amusics and controls. However, in melody recognition there was a significant difference in performance. Control participants were able to identify approximately 39 (75%) of the titles on the basis of their melodies and approximately an additional 10 (77%) of the remaining 13 titles in the multiple choice format, a total of 49 (95%) of titles. In contrast, the amusics only identified approximately 8 (15%) of the titles on the basis of their melodies, and approximately 23 (52%) of the remaining 44 titles in the multiple choice format, a total of 31 (60%) of titles. Guessing would have tended to yield approximately 11 (25%) additional titles, thus the amusics, while they perform worse than the controls, are not always guessing on the multiple choice format, but rather recall the title when it is presented in a multiple choice format significantly often ($p<.01$ for the mean).[15] The contrast of results found in the lyric and melody recognition tasks are particularly interesting as they demonstrate that the amusics' inability to identify folksong melodies was not a result of lack of exposure to the songs, as they were able to recognize lyrics.

One might think that while amusics failed to recognize familiar folk songs, they may be able to relearn them if explicitly directed to do so in the laboratory. To test this hypothesis Ayotte and colleagues tested the participants using three tasks: one with melodic recognition, one with spoken lyric recognition, and one with environmental sound recognition (the animal sound and vehicle noise recognition in the previous experiment). In each task participants listened to

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[15] Ayotte and colleagues do not provide distributions of scores for these tasks. It might be interesting to compare the distributions of scores on this task with MBEA scores to investigate whether the scores continually decrease, or whether there are two distinct distributions. This might provide further insight into how musical ability is distributed in the general population, in particular whether those diagnosed with congenital amusia are at the tail end of a continuous distribution, or there is a clear cut off point with which to make the diagnoses.

20 auditory targets and were asked to memorize each of them. They were then presented with 40 recordings and asked to identify which recordings they had heard before and which were novel. Amusics and controls performed with no significant difference on the spoken lyric condition, and the environmental sounds condition. In the melodic recognition condition, amusics performed significantly worse than the matched controls. Thus, the hypothesis that amusics would be able to relearn the melodies if instructed to do so in the laboratory was rejected.

The last experiment in the study by Ayotte and colleagues was designed to evaluate music production in amusics. In this experiment amusics and matched controls were asked to sing three familiar songs into a microphone, and were asked to tap along in synchrony with a recording. Not surprisingly, amusics performed poorly in both tasks compared to controls. This was true for all of the amusics tested except for A9, who performed normally in the tasks. The ability of an amusic to produce a normal performance suggests that there may be a "non-trivial dissociation between perception and performance" (p. 249). However, Ayotte and colleagues also suggest this observation may be a result of the "crude measures of performance" (p. 249) which they employed. (The performances of the participants was scored by six judges who rated performance on each task on a ten-point scale. Scores between each pair of judges were found to significantly correlate ($p<0.01$).) It should however be noted that, of all the amusic participants, A9 had the highest composite score on the MBEA. A9 also performed above chance levels on five of the six subtests. The anomaly of A9's normal results on a production task may thus be explained by A9 having less severe amusia than the other amusics, or that amusia is not a discrete category.

The Case of AS

There is to my knowledge, only one study in which a child has been diagnosed with congenital amusia. In this study, Marie-Andrée Lebrun and colleagues (2012) present a case of a 10-year old girl (AS) who exhibited congenital amusia. AS was referred to Lebrun and colleagues by her choir director, who in more than 30 years never experienced such difficulty helping a child. AS was born without complications and developed normally. Her performance at school was in the normal range, even for her music class (which makes one question what her music class involved). AS tested normally for her hearing, and her IQ was average. The only cognitive deficiency AS presented was a low score on the Peabody Picture Vocabulary Test, a test designed to evaluate receptive vocabulary ability. In a Peabody Picture Vocabulary Test, a participant hears a word spoken by the tester and the participant must then point to a picture of that word. While AS's score is low, in the 16th percentile, it is still considered in the normal range. (AS lived in Moscow until she was six years old. She then moved to Quebec. In this study she was tested in French, her second language, and this may account for the low score.)

AS, three matched controls, and 20 unselected children were given a child version of the MBEA. The child version of the MBEA is similar to the adult version however it utilizes shorter melodies (a mean of 7.1 notes) and fewer trials. Instead of 30 trials for each subtest, the child version combines scale, contour, and interval into one "melody" (p. 684) subtest where 3 comparison melodies contain a scale error, 4 contain a contour error, 4 contain an interval error, and 10 are the same as the target melodies (a total of 20 trials). This subtest is followed by a rhythm subtest with 20 trials (10 same and 10 different) and a memory subtest with 20 trials (10 melodies heard previously and 10 novel melodies) both of which are the same as the adult version except for the use of shorter melodies. The child version of the MBEA does not have a metre subtest.

AS's scores were at chance levels for all of the subtests of the child version of the MBEA (melody: 7/20, rhythm: 10/20, memory 12/20, composite: 29/60). Her scores were also significantly below both controls (mean score for melody: 17.2/20, rhythm: 19.0/20, memory: 17.4/20, composite: 53.4/60). From the results of the child version of the MBEA Lebrun and colleagues concluded that AS had a severe musical impairment.

AS's pitch perception was further evaluated using a task similar to one administered to Monica. In this task AS and the three matched controls were presented with a series of five tones, and were asked to determine if the fourth tone was different from the others. This fourth tone was either the same, or moved up or down 25 cents or 200 cents. AS scored normally when the 200-cent alteration was utilized, but performed at chance levels when the 25-cent alteration was used. Controls performed similarly when both alterations were used. This result is interesting as it confirms Suzuki's understanding of tone-deafness as a problem in the perception of small differences in pitch rather than larger differences. AS's difficulty distinguishing small changes of pitch, but not larger changes is also consistent with the findings of the case studies discussed above.

This result was examined further using electroencephalography. In this task AS and five matched controls watched a silent movie while the stimuli from the tone-matching experiment were played. The only difference was that instead of being played in groups of five tones, the stimuli were played as one continuous sequence. The participants were instructed to ignore the tones. Lebrun and colleagues focused on the Mismatch Negativity (MMN) which is an event-related potential component that peaks between 100 msec and 250 msec and that is "known to reflect pre-attentive change detection" (p. 686). In control participants, an MMN was observed in both the 200-cent and 25-cent alterations. In AS, an MMN was observed in the 200-cent alteration but not in the 25-cent alteration.

AS and the three matched controls were also given a singing task where they were recorded singing "Frère Jacques" and "Bonne Fête." Lebrun and colleagues report that AS made significantly more pitch errors as compared to controls, and showed more impairment in singing large intervals than small intervals. However, AS did not make more contour errors than controls, thus providing further confirmation of Suzuki's understanding of tone deafness. AS showed slight impairment in rhythm compared to controls but only for "Frère Jacques." For "Bonne Fête" AS performed comparably to controls with regard to rhythm.

Development, Inheritance, and Intervention

Musical Experience

In order to conclude that what is deemed congenital amusia is actually congenital, one would need to eliminate the possibility that the observed deficiency in musical perception is not the result of a lack of musical experience and exposure. In the case of Monica, Peretz and colleagues report that Monica participated in a church choir as a child and a high school band. From this, Peretz and colleagues conclude that Monica's amusia is not the result of a lack of musical experience.

However, there are problems with this conclusion. Firstly, church choirs vary greatly in what they involve from choirs that rehearse regularly and perform often, to a group of children directed by a volunteer parent who may or may not be musically trained. This group might rehearse rarely, and perform even less frequently. Furthermore, the choir may not be a place of instruction but rather just a group that gets together and sings. If one assumes the choir Monica was involved in was closer to the latter than the former group described, this musical experience may have had little or no effect. It is possible that many of the children in the church choir, and perhaps even the director, were singing out of tune.

With regard to Monica's high school band, one can pose similar questions. High school bands vary in level, amount of rehearsal, number of performances, and skill of the teacher. Furthermore, we are not told what instrument Monica played in the band. If Monica played the bass drum, it would have less of an effect on her pitch perception abilities than if she played the flute. Furthermore, Monica reported joining both the choir and the band because of social pressure and not out of a desire to play music. Thus, she may have practiced less, attended with less frequency, and have been less motivated than other students. Therefore, without more details, one cannot conclude that Monica had sufficient musical experience and one cannot rule out lack of musical experience as a cause for Monica's problems with music.

Ayotte and colleagues' study presents problems similar to those found in the case study of Monica. In this study it is reported that all of the participants had "music lessons during childhood, to ensure exposure to music in a timely fashion" (p. 239). As with Monica, no details are given about these music lessons. The participants could have been involved in anything from drum lessons to singing lessons. Also not reported is whether the music lessons were group study or one-on-one instruction. Perhaps even more problematic, it is not reported how often these lessons occurred, whether at-home practice was involved, at what age the lessons began, and how long the participant studied music. Lastly, whether the music lessons involved fixed pitch instruments, (such as the piano or xylophone) or instruments which require the performer to carefully adjust pitch (such as a string instrument) is not reported. If the time of study was not long, and the student did not practice, one could reason that studying a fixed pitch instrument in this fashion would not drastically affect one's pitch perception abilities. Thus, based on the information provided, one cannot rule out the possibility that the 11 amusics' musical impairment was a result of a lack of musical experience.

An additional problem in the studies discussed above is the time gap between the musical experience and the testing for all of the participants. Monica is reported as being in her early forties, yet her reported musical experience ends when she is in high school. Thus there is a gap of at least 20 years between the time of her experience and the time she was evaluated. In Ayotte and colleagues' study, the 11 amusics had a mean age of 57 (SD=8.21) and the youngest participant was 41. The musical experience reported was music lessons in childhood. Again, this created a gap of at least 20 years between the time of the musical experience and the time of testing.

In the case of Monica, she reported that "she does not like to listen to music because it sounds to her like noise and induces stress" (p. 186). In the study of the 11 amusics, seven amusics reported "not appreciating music" (p. 241) and two reported that they found "music unpleasant and tried to avoid it" (p. 241). (Ayotte and colleagues do not report which amusics specifically gave these answers.) Thus, in addition to the time gap between the musical experience and the test of the participants of these studies being large, it was also likely a time in which little music was heard compared to controls. If one trains in any skill, and then does not use that skill for 20 years, one expects ability in that skill to diminish. Thus, even if the amusics had substantial musical experience, the 20-year time gap may account for their poor performance and not an inborn deficiency.

Musical Exposure

In both the case study of Monica and the study of the 11 amusics, most amusic participants reported that some, but not all, of their siblings were amusic. If congenital amusia was a result of a deficient musical environment one might speculate that all siblings would be amusic. The presence of siblings who are not amusic, the researchers argue, suggests that congenital amusia is not a result of a deficient musical environment.

However, this conclusion relies on the assumption that all children within a family are raised in the same environment, which is not necessarily the case. Many young children attend preschool, daycare, or have caregivers. Should any of these factors differ for two children in a family, the environments would differ as well. Perhaps, one child in a family attended a preschool where the teacher played music on a stereo and had the students sing throughout the day, while the other attended preschool where the teacher did not have any music in the classroom. Or perhaps, with one child the mother stayed home in the early years and with the next child the father stayed home in the early years. If the parents have different music listening habits, then two different musical environments would result. These are two examples of many, of how musical environments may be different for two children in the same family. Thus, the musical ability of the siblings of amusics does not rule out an environmental cause of the observed perception deficits.

AS's Musical Experience and Exposure

In contrast to the case studies of Monica and the 11 amusics, we are given more information regarding AS's musical experience and exposure.

> When AS lived in Moscow, she had group music lessons in kindergarten and later, group music lessons in elementary school in

> Quebec. Her mother, who highly values music, used to play Mozart recordings frequently and in different situations (during meals, at bed time, etc.) as AS was growing up. Ever since they moved to Canada, AS and her mother attended numerous classical music concerts. AS attends approximately 10-12 concerts/year. Above all, AS has been actively involved in a choir since September 2008 (about 20 months prior to testing), which requires her to take part in 2-hour practice sessions twice a week and four concerts a year. (p. 2)

Because of this musical exposure and experience, Lebrun and colleagues conclude that AS's lack of musical ability is the result of an inborn condition and not a result of a musically deficient environment.

However, much of the information provided about AS's musical experience raises questions. With regard to the "group music lessons" it is questionable whether this was serious music study, or if occasionally the teacher in the regular classroom led the class in singing. One cannot assume, based on the information provided, that these group music lessons resulted in significant musical experience. After all, AS, a student diagnosed with congenital amusia, received normal marks in her music class.

Also suspect is the statement that AS's mother played recordings of Mozart's music regularly. In the past two decades there was a commercial enterprise based on the so-called "Mozart Effect." Due to exaggerated claims based on narrowly focused research, people began to believe that listening to Mozart makes one smarter. This resulted in many parents buying Mozart CDs for their children to listen to. (For more on the "Mozart Effect" see Demorest & Morrison, 2000). This also became a standard answer that a parent might give should they be questioned about their parenting. A parent who truly played recorded music for their child would more likely answer a questionnaire by saying they listened to "music" or "classical music." The answer of "Mozart" leads one to be suspicious that the parent is merely answering what they think is the correct answer, or the answer that would indicate that they are a good parent.

Another cause for suspicion is the attendance of 10-12 classical music concerts per year. This is highly unusual. Many music educators complain that their students do not attend concerts. This is the case even for serious music students who take private lessons and practice many hours per week. Classical music concerts tend to be at night, expensive, require a long attention span, and sitting quietly. All of these factors make taking children to concerts difficult. In contrast, concert series aimed at children and scheduled during the daytime, comprise far fewer than 10 to 12 performances, even in large cities. If it is indeed true that AS attends this many concerts, she is a remarkable music student. However, it makes one question if AS is a remarkable music student because her mother is so dedicated or if AS is actually remarkable in that her mother will answer a questionnaire in such a way that she appears to be a good parent. Of further concern is the nature of the concerts. It is not reported whether AS is going to see a professional symphony, or if she is going to a local violin teacher's studio recital where many of the students play poorly and out of tune.

With regard to AS's seemingly significant musical experience with her choir, there may be a problem of age. AS did not start choir until she was nine years old. Perhaps one must experience music in their environment before a certain age in order to prevent what appears to be congenital amusia. Or, perhaps it takes a critical amount of musical experience, after having little music experience, to eliminate the disability which appears to be congenital amusia. Prior to the study, AS had been in the choir for only 20 months. If AS did not experience a musically rich environment before this time, perhaps she needs more time to resemble the other children in musical ability. It is also possible that the later a child who appears to have congenital amusia begins to experience a musical environment the longer it takes to eliminate the disability. Suzuki (2012) argued that the process of rectifying tone deafness in children takes “six or seven months” (p. 116). While this is a shorter amount of time than AS has been in a choir, Suzuki’s time frame is based on a child working daily and intensively. AS’s choir rehearses twice a week, thus according to Suzuki, it may take longer to rectify the deficiency.

Heritable or Environmental Cause of Congenital Amusia

In both the case study of Monica and the case studies of the 11 amusics, researchers observed that parents of amusics, most often the mother, tended to be reported as amusic. From this they conclude that congenital amusia is likely genetically inherited. However, the observation of amusics having amusical parents could also be used to argue an environmental cause of congenital amusia. (Kalmus and Fry made claims regarding heritability of tone-deafness. For these claims see chapter 10.)

McDonald and Stewart (2008) surveyed 21 adults diagnosed as congenitally amusic (based on their scores on the MBEA) and 21 matched controls. They found that control participants reported listening to three times more music than those diagnosed with congenital amusias. In a similar study, it was found that 88 percent of those diagnosed with congenital amusia rarely sing in private and 100% reported that they rarely sing in public. This study also found that 100% of participants diagnosed with congenital amusia reported that they rarely listen to music (Peretz et al., 2008). Thus a child born into a family with a parent who would be diagnosed with congenital amusia is not likely to experience music in their home environment. Furthermore, the parent who would be diagnosed with congenital amusia is unlikely to sing to their infant child, which is plausibly an important factor in the musical environment a child experiences. Language development varies with the extent to which children interact verbally with parents (Hoff, 2003). Thus, one would suspect musical development would vary with the extent to which parents interact musically with their children. It is therefore not surprising that it was most often the mother who was reported as being amusic in Ayotte and colleagues' study, as it is the mother who is more likely to be the one to sing to, and talk with, their child. Thus the presence of an amusic parent can be used as an argument for an environmental cause of congenital amusia and does not necessarily indicate a genetic condition.

There are several other studies which suggest a genetic cause for the condition deemed congenital amusia. For example, Drayna and colleagues (2001) administered a Distorted Tunes Test (DTT) to many pairs of dizygotic and monozygotic twins. Drayna and colleagues found that performance on the DTT was more highly correlated for the monozygotic twins, who are genetically more similar, than for the dizygotic twins. This suggests that pitch perception is due to genetic differences in auditory function.

The study by Drayna and colleagues presents a challenge for those who claim that what is thought to be congenital amusia is a result of a musically deficient environment. However, the study relies on the assumption that the environment of each twin in a set of twins has the same amount of variability whether the twins are monozygotic or dizygotic. This assumption warrants more research, as it could be that parents are more likely to treat each monozygotic twin in a more similar manner than each dizygotic twin (This question has been raised in research utilizing twins and language impairment, see Bishop et al., 2006)

Secondly, it should be noted that Drayna and colleagues assessed pitch perception ability in a normal population and not specifically those that would be diagnosed with congenital amusia. If congenital amusia just refers to those at the extreme end of a normal distribution one could argue that Drayna and colleagues' findings can be used to draw conclusions about congenital amusia. However, if those that would be diagnosed with congenital amusia are outliers one may not be able to generalize from the cause of pitch perception ability in the normal population to the part of the population that would be diagnosed with congenital amusia. Thus, while Drayna and colleagues provide motivation for further similar studies they have not provided conclusive evidence that congenital amusia is genetically inherited.

In 2007, Peretz and colleagues compiled family trees of families in which there were members who were diagnosed with congenital amusia. They found that 39 percent of first-degree relatives of participants diagnosed with congenital amusia were amusic while only 3 percent of first-degree relatives were amusic for controls. Peretz and colleagues concluded that congenital amusia is likely the result of several genes which influence brain development and thereby cause the deficiency.   However, this study has many of the problems mentioned earlier. Most importantly, Peretz and colleagues did not take into consideration that amusics listen to less music and sing less than non-amusics. Therefore, first-degree relatives of amusics would have a less musically enhanced environment and thereby might be at greater risk for developing a disability with regard to music perception.   Additionally, whereas Peretz and colleagues tested the participants of the study, in the majority of cases they relied on self-report for relatives. Self-report has proven to be an inaccurate measure of amusia.

In 2005, Lola Cuddy and colleagues surveyed a class of 2000 university students and 17 percent of them reported being amusic. A sample of 100 of the 340 self-reported amusics and 100 self-reported normal students were then tested using the MBEA. Cuddy and colleagues found that many of the students who reported being amusic were not found to be so using the battery. Only 10% of the self-reported amusics scored lower than two standard deviations below the mean of the control participants. Over all, the distribution of scores of the two groups (the self-identified amusics and controls) were nearly the same (see Cuddy et al, 2005 Figure 2). Furthermore, self-reports regarding the amount of singing one does, one's experience with music instruction, and one's music-listening habits were better predictors of scores on the MBEA than whether one reported being amusic. Thus, one should not rely merely on self-reports of musical ability in general when determining whether one is amusic. Instead, previous musical experience is of great consequence. Consequently, one should also not accept the family tree diagrams in Peretz's study as accurately diagraming the presence of a genetic disorder within a family.

Brain imaging studies have shown evidence that congenital amusia may have a genetic cause. Two studies, Krista Hyde and colleagues (2007) and Jake Mandell and colleagues (2007) have found that the brains of those diagnosed with congenital amusia according to the MBEA are different from the brains of controls. Hyde and colleagues found that those diagnosed as congenital amusics have less white matter in the right inferior frontal gyrus and more gray matter in the same region. Amusics also have thicker cortex in the right inferior frontal gyrus and right auditory cortex relative to controls. Mandell and colleagues found that those diagnosed with congenital amusia had more gray matter in the left inferior frontal gyrus and the left superior temporal sulcus. These findings are not surprising as they are consistent with studies on brain-injured patients. That is, patients who have had damage to these regions of the brain also performed poorly on the MBEA (See Hyde et al., 2007 p. 13030 for a review of these studies).

Hyde and colleagues argue that the anatomical differences found in the brains of those diagnosed with congenital amusia are likely the result of abnormal cortical development caused by genetically based abnormal neuronal migration. They provide support for this argument by comparing amusia to other similar disorders, such as dyslexia and epilepsy, which are thought to be caused by genetically based abnormal neuronal migration. However, Hyde and colleagues acknowledge that they cannot rule out "the environmental influence on brain structure in terms of lack of same amount of musical exposure in amusics relative to musically intact controls" (p. 13031).

Interventions

There is to my knowledge no current research that utilizes interventions in an attempt to eradicate the musical perception deficits experienced by those who are diagnosed with congenital amusia. Rather current research focuses on whether the observed condition is actually inborn, the domain specificity of the disorder, and how the disorder is manifested in different individuals. There are however, two older studies which sought to create interventions.

In 1969, David Joyner developed an intervention for children diagnosed with congenital amusia, which he called the "monotone problem." Joyner's intervention was founded on the theory that what is thought to be congenital amusia is the result of a faulty vocal instrument. One possible reason Joyner provided for children having a faulty vocal instrument is that they had not sung much and as a result their vocal instruments were underdeveloped. Joyner developed a series of remedial exercises to aid the children in the physical aspects of singing (For a detailed description of similar exercises see Bridgehouse, 1978.) With a group of 72 participants Joyner was able to improve the singing and pitch perception abilities of all but four of the children. Joyner noted that of these children, "all four were problem children at their school because of their general incapacity for any kind of school work" (p. 124).

In 1933, M. Wolner and W. H. Pyle studied students aged seven to nine at three Detroit public schools. From this large sample they took the seven students who showed the most difficulty in pitch perception. None of these students could distinguish a 30 vibration difference on Whipple Tuning Forks (~114 cents or a little larger than a tempered semitone above A = 440 Hz) when the tones were played one after the other. These students were given musical training for 20 minutes a day, five days a week. The training lasted three months. At the end of three months, all seven students were able to perfectly discriminate octaves, fifths, thirds, whole tones, and semitones within a four octave range. After training, each student showed improvement in singing. The students were able to sing melodies with no traces of deficiency in pitch.

Because Wolner and Pyle drew their participants from such a large group, and because they took the seven weakest students, it is probable that at least one of these students would have been diagnosed with congenital amusia by modern researchers. Yet, with the intervention, all of the students became proficient in interval perception. This suggests that when children are given proper musical experience, the condition observed as congenital amusia ceases to exist. Perhaps when children do not experience music at a young age, they need intensive remedial assistance to eliminate what is thought to be congenital amusia, as Suzuki advocated. However, it should be noted that the interventions described above are proactive interventions. This is in contrast to Suzuki's passive method of rectifying tone-deafness by merely having the individual listen to more music which is in tune.

Conclusions

Current research has demonstrated that there are people who are very deficient in music perception. Some of these individuals are able to perceive pitch differences in speech if lexical content remains, and thus the disorder appears to not be specific to the musical domain but rather relevant to both music and speech. The musical deficiency of these individuals manifests in variable ways; that is, there are individual differences in both the kinds and the extents of musical deficiencies manifest by particular persons. Current research has concluded that this music perception deficit is the result of an inborn condition which is likely genetically inherited. However, a critical analysis of this research reveals that there are problems in the way in which these studies have controlled for musical exposure and experience. Furthermore, if one considers older studies in which researchers have attempted interventions for children who have been deemed musically disabled, the interventions are successful and the students become normal in terms of their musical ability.

In view of flaws in recent congenital amusia research, Suzuki's argument that tone deafness is a result of a musically deficient environment has not been disproven. The findings of the research in which interventions were successful suggest that Suzuki was correct in claiming that what is now deemed congenital amusia can be rectified through intensive remedial training. Thus, congenital amusia research does not serve as a disproof to the Suzuki Method's foundational assumption that one cannot be born with a lack of musical talent. Instead, better controlled research on congenital amusia and more modern research utilizing interventions to help those diagnosed with congenital amusia both seem warranted.

Chapter 13

Conclusion

The Suzuki Method has become both a popular and influential method of music education on a global scale. Yet, despite its popularity, the method has not received much attention in music education research. The method was likely developed as a result of Suzuki's studies with Karl Klingler. He began to develop his method in Tokyo in the 1930s and continued in Matsumoto following World War II. The method spread in the 1960s and found increasing popularity within the global music education community. A premise of the Suzuki Method that is central to the present study is the idea that musical talent is not inborn, but rather cultivated through one's environment. Suzuki believed that by using his method every child can be taught to master the violin.

Though Suzuki's method has proved successful in training world-class virtuosi, this was not his initial goal when he developed the method. Rather, Suzuki sought to cultivate his students from ordinary children into noble human beings. Suzuki defined noble people as those who are productive, do not lament lack of talent, have a developed memory, engage in self-reflection, act on their thoughts, have developed *Kan* (intuition), strive for love, truth virtue and goodness, and have the patience and persistence to achieve their goals.

In relation to Suzuki's foundational claim that musical talent is not inborn, Suzuki made five sub-claims:

1. Musical talent is not a result of genetic inheritance.

2. One's environment, and not inborn predisposition, determines one's abilities.

3. Any skill can be trained with repetition.

4. If one replicates the way in which children learn to speak one can train children successfully in any skill.

5. A lack of musical talent, often referred to as tone-deafness, is not an inborn condition, but rather the result of a poor musical environment.

Suzuki provided one caveat to his claim that musical talent is not inborn. He wrote that one can be born with superior speed and sensitivity to adapting to one's environment. Such speed and sensitivity are not domain-specific, but rather can be used in developing any skill. Suzuki did not write extensively on this topic, so it is difficult to determine exactly what he meant. It is likely that Suzuki believed that anyone can develop any skill to an equally high level, however some people develop faster and with more ease than others.

It is plausible that other pedagogues would agree with Suzuki that musical talent is not inborn. A teacher might argue the superiority of their teaching because they can train anyone to master an instrument and not just those deemed talented. This claim was made about Ivan Galamian. However, an examination of treatises by four influential violin pedagogues reveals that Suzuki was revolutionary in his claims.

Suzuki argued his first sub-claim, that musical talent is not the result of genetic inheritance, through a comparison to bird song. Suzuki argued that bird-song learning is not the result of genetic inheritance, and thus human musical talent is also not the result of genetic inheritance. Suzuki was correct in several of the claims he made regarding bird-song learning. Birds tend to have a critical period in early life in which they learn bird song. Furthermore, there is evidence that birds learn their song through external stimuli. However, research on a few species of birds has revealed that some components of bird-song learning are inborn. For example, birds have inborn knowledge of how to identify their species' song. Additionally, some birds learn their song guided by visual displays rather than auditory models. Lastly, some birds do not require tutor birds who are vocally active, but only social interaction to learn their song. As such, Suzuki's bird-song argument is faulty.

However, bird song and human music differ in several ways suggesting that they may be non-commensurate phenomena. The apparent similarity of human music and bird song may simply be a coincidence. Thus, one should not generalize findings from bird-song learning to human-music learning.

From a story about two children raised by wolves, Suzuki provided evidence for his second sub-claim, namely, that human ability is the result of the environment and not an inborn predisposition. This story proved to be a fabrication. The children in question were not actually raised by wolves. It is plausible that the two children displayed unusual behaviors as a result of autism and environmental factors. However, it could be that autistic children are more affected by environmental factors than normal children and, as such, one should not apply findings from one group to the other. Nonetheless, while environmental factors clearly have an effect on child development, they are likely not as influential as Suzuki maintains. Furthermore, other anecdotes Suzuki employs to demonstrate the effect of environmental factors on child development can be explained in other ways.

Suzuki claimed that any skill can be trained with repetition. Suzuki provided an argument for this claim through a comparison to handedness. Suzuki maintained that whether one is right- or left-handed is simply the result of repetitive use of the dominant hand and not inborn predisposition. Suzuki further maintained that one should train children to be ambidextrous as it provides them with numerous benefits.

Recent research has not reached a final conclusion regarding the cause of handedness. However, research has shown that, as Suzuki maintained, dexterity in the non-dominant hand is best trained through repetitive use of the non-dominant hand in an environment in which there is strong motivation to do so. Furthermore, while it is not clear whether ambidexterity can be trained, it is clear that training dexterity in the non-dominant hand for one skill facilitates greater dexterity in the non-dominant hand for other skills.

Some research has shown that ambidexterity is correlated with cognitive deficits, suggesting that Suzuki was incorrect in stating that ambidexterity is advantageous. In particular, ambidextrous children have deficiencies in tapping switches while attending to an auditory stimulus, a skill one would think is essential to playing music. However, a possible explanation is that the ambidexterity of the children in these studies is a result of being raised in a non-stimulating environment. Rather than thinking of these children as two-handed, it may be more accurate to describe them as no-handed. Research that investigates trained ambidexterity is necessary before arriving at a final conclusion regarding Suzuki's claim that ambidexterity training is beneficial.

Suzuki argued that all children learn to master speech in their native language as a result of environmental stimuli. He theorized that if we teach children any skill in the same manner as they acquire speech abilities, they will be equally successful. This argument has several flaws. First, it is clear that not all children learn to speak. Secondly, some cases of speech impairment have non-environmental causes. However, it is reasonable to believe that when Suzuki discusses "all children," he is referring to the majority of children.

Another flaw in this argument is the lack of evidence supporting the idea that, in contrast to Chomsky's view, speech acquisition does not have innate components. Nevertheless, the Suzuki Method does attempt to replicate empirically verified aspects of speech acquisition.

Furthermore, these replicated elements of speech acquisition are what distinguishes the Suzuki Method from other pedagogies.

While Suzuki's mother tongue argument has flaws, research has confirmed similarities between music learning and speech acquisition, providing support for Suzuki's idea that speech acquisition can be used as a model for music education. Research on pitch in the two domains has shown that ability in one domain can be generalized to the other. Research on rhythm has shown that a lack of ability in musical rhythm was present in those with speech disorders, again suggesting commonality. With regard to timbre, researchers have found cases where spoken vocables used in music pedagogy show similarities in timbre to their corresponding instrumental sounds. Researchers have also concluded that music and speech convey emotion in similar ways, and those that are lacking musical ability as a result of brain damage, are also lacking in the ability to identify emotions present in speech. Additionally, behavioral, neuroimaging, and ERP studies have confirmed that speech perception and music perception share cognitive resources; as well, research on aphasia and amusia has failed to show distinct cognitive systems for the two domains. Lastly, for people who lose speech abilities, music has proven to be a significant tool in assisting them to regain those abilities.

The generalization of musical ability and speech ability does not apply to semantic content. That is, there is no correlate to semantic content in speech in non-verbal music. Furthermore, while musical ability is correlated with prosodic abilities in second-language learning, it is not correlated with increased understanding of semantic content. As semantic contact is a fundamental aspect of speech, Suzuki's argument has a flaw. Furthermore, the motivation of researchers to find a connection between speech and music may bias research, and the evidence that shows a lack of a link between the two domains may be underreported.

Consequently, while Suzuki's idea that speech acquisition can serve as a model for music education has much support, one should still treat it with some reservation.

Suzuki argued that a lack of musical talent, colloquially referred to as tone-deafness, is not an inborn condition. However, researchers have concluded that tone-deafness is in fact inborn. Utilizing their diagnostic instrument, the Distorted Tunes Test (DTT), Kalmus and Fry have provided one of the most extensive studies on tone- deafness. They have shown that there is a small percentage of the population that has problems with music perception. Kalmus and Fry provided evidence that children, as a group, reach similar musical competence to adults around the age of 16 and have provided evidence that a lack of musical competence tends to be found in multiple members of families of those demonstrating a lack of musical competence.

All the same, their often cited estimate that 4.2% of the adult population is tone-deaf is flawed. The criterion they used to diagnose tone-deafness is arbitrary, and they have not ruled out alternative explanations as to why participants deemed tone-deaf perform poorly on the DTT. Moreover, their conclusion that tone-deafness is a genetically inherited trait requires further research. They present some data which supports this conclusion, but some of their data refutes it. Lastly, because Kalmus and Fry provide no histories of tone-deaf participants in any of their studies, one cannot conclude that tone-deafness is an inborn trait. It is plausible that what Kalmus and Fry observe throughout their experiments is varying amounts of musical experience and exposure in the population. Indeed, Kalmus and Fry have also shown that people can utilize their experience with Western tonal music to make judgements about the acceptability of a melody even if they are not familiar with the melody. Because Kalmus and Fry do not provide convincing evidence that tone-deafness is an inborn condition, they do not refute Suzuki's claim that tone-deafness is a result of poor musical environment.

In the last decade, the Montreal Battery for the Evaluation of Amusia (MBEA) has become the predominant test used to diagnose tone-deafness. Both the MBEA and the DTT, when employed with large samples of unselected adults have shown that musical ability varies in a continuous manner throughout the population. That is, there is no clear cut-off point that distinguishes tone-deaf people from normal people. The MBEA and the DTT have different advantages with regard to studying tone-deafness. The MBEA is advantageous in that it is not effected by participants' familiarity with the stimuli and is less likely to be affected by the amount of participants' exposure to Western music. However, the DTT tests for tone-deafness more closely to the way in which Suzuki defines it. The DTT is also advantageous because it uses changes of a semitone without changes in contour. This allows the DTT to test for perception abilities unique to music and not shared with speech. However, both tests neglect the perception of timbre which, according to Suzuki, is a key element in music training.

Like older research which utilizes the DTT, recent research utilizing the MBEA has demonstrated that there are people who are deficient in music perception. Some of these individuals are able to perceive pitch differences in speech, but only if lexical content remains. As such, the disorder appears to not be specific to the musical domain, but rather it is relevant to both music and speech. The musical deficiency of these individuals is manifest in various ways; that is, there are individual differences in both the types and the extents of musical deficiencies manifested by particular persons. Current research has concluded that this music perception deficit is the result of an inborn condition which is likely genetically inherited. However, a critical analysis of this research reveals that there are problems in the way in which these studies have controlled for musical exposure and experience. Furthermore, in older studies in which researchers have attempted interventions for children deemed tone-deaf, the interventions were successful and the students became normal with regard to musical perception.

In view of flaws in tone-deafness research, Suzuki's argument that tone-deafness is a result of a musically deficient environment has not been disproven. The findings of the research in which interventions were successful suggest that Suzuki was correct in claiming that what is deemed tone-deafness can be rectified through intensive remedial training. Accordingly, tone-deafness research does not serve as a disproof to the Suzuki Method's foundational assumption that one cannot be born with a lack of musical talent.

An analysis of Suzuki's claims regarding musical talent has revealed that while some of his arguments are valid, some are not. Further research regarding the extent of acceptance by Suzuki teachers of Suzuki's claims is warranted. One might use this research as a means of distinguishing a Suzuki teacher from a non-Suzuki teacher. This would, in turn, be a means to compare the efficacy of Suzuki teaching with non-Suzuki teaching. Likewise, additional research regarding how the acceptance of Suzuki's claims affects the way in which Suzuki teachers teach is crucial. It is possible that a teacher who accepts Suzuki's claim that musical talent is not inborn will be more likely to persist longer in helping a struggling student.

The influence and popularity of the Suzuki Method demands more research into the method itself. The topics just mentioned are only two of the many I hope future research will address.

Appendix 1: Melodies Incorrectly Played on Kalmus and Fry's DTT

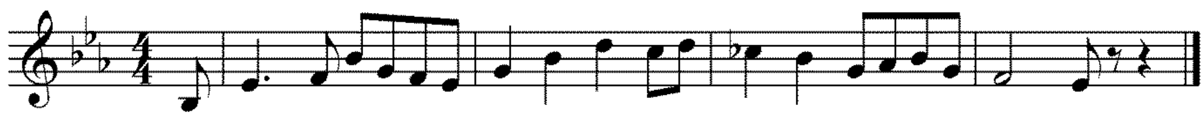
a) **Melody in its correct version b) Melody as it is utilized in the DTT**

The melodies position in the sequence of 26 melodies of the DTT is noted in parenthesis.

1. (2) Minstrel Boy

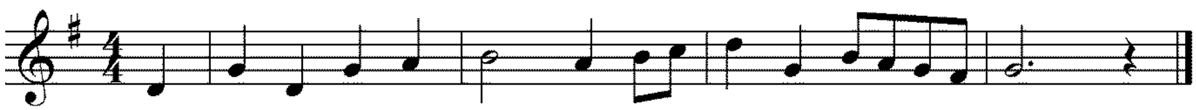
a)

b)

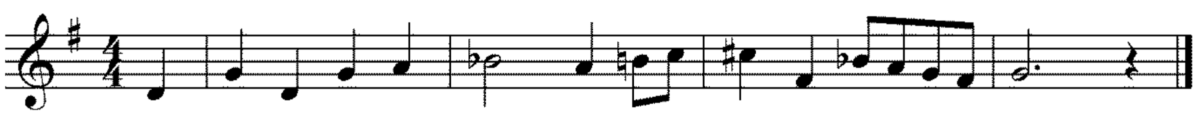


2. (4) Grenadiers

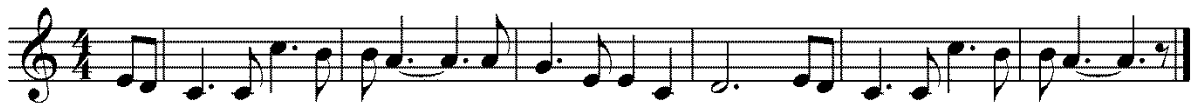
a)



b)



3. (5) Annie Laurie



b)

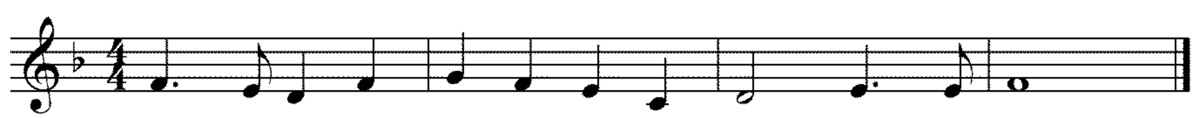
4. (7) Jolly Good Fellow

a)

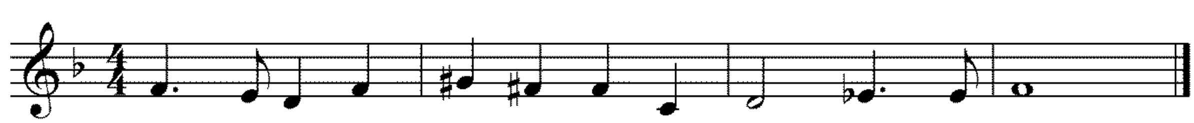
b)

5. (8) All Thro' the Night

a)



b)



6. (9) Tipperary

a)

b)

7. (11) Adeste Fideles

a)

b)

8. (12) Land of Hope and Glory

a)

b)

9. (14) Clementine

a)

b)

10. (16) Daisy, Daisy

a)

b)

11. (17) Deutschland

a)

b)

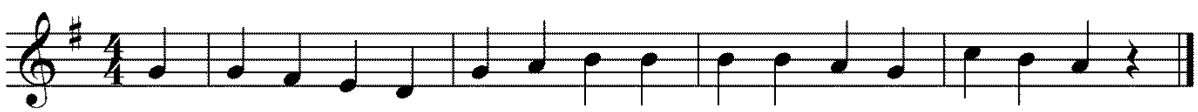
12. (18) Auld Lang Syne

a)

b)

13. (19) Doxology

a)



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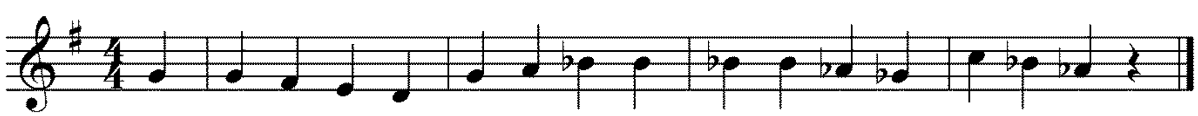
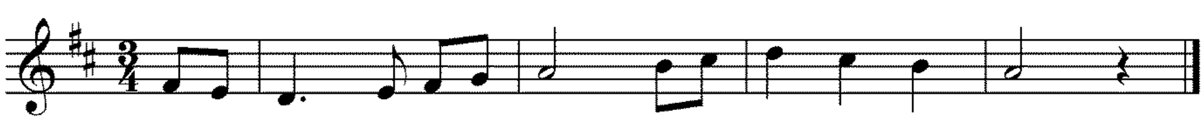
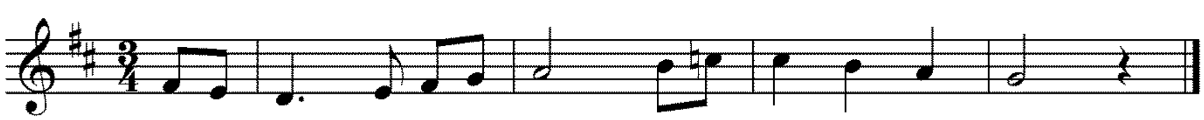


14. (20) First Nowell

a)

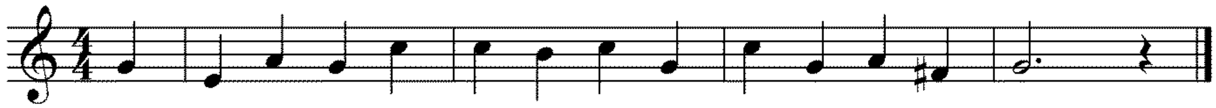


b)



15. (23) O God our Help

a)

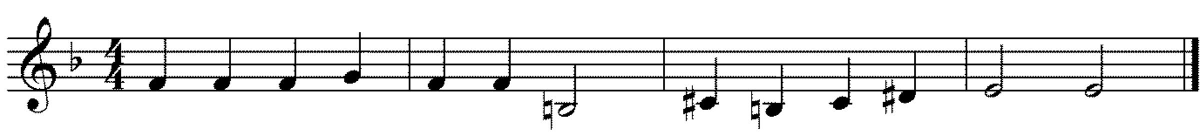


b)

16. (24) King Wenceslas

a)

b)

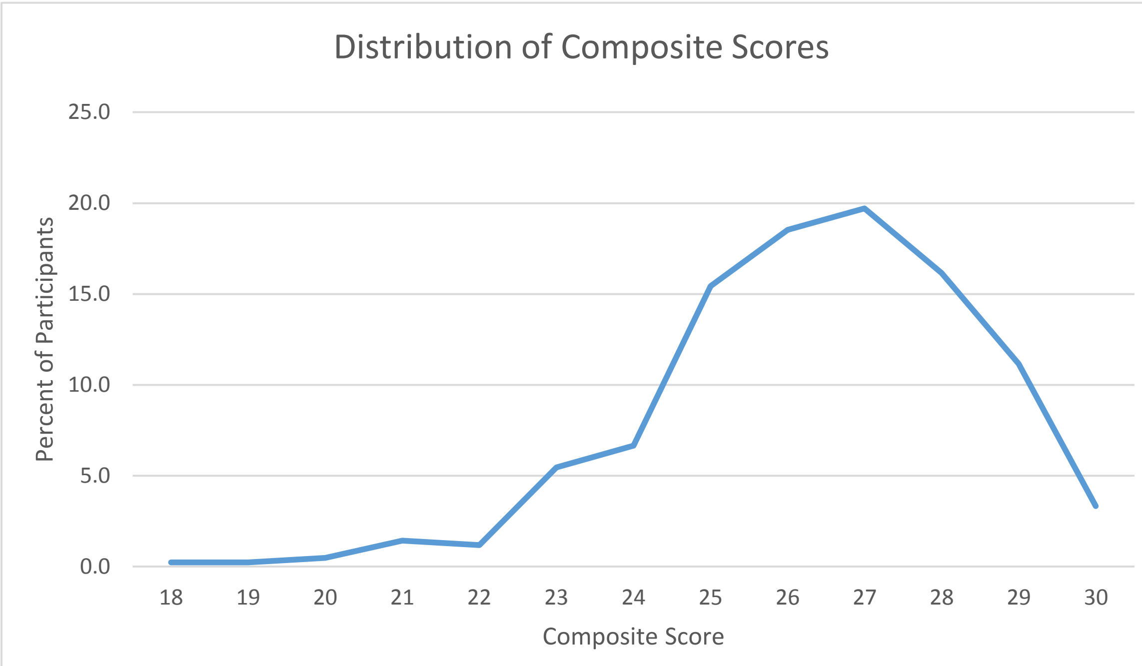


17. (26) Old Folks at Home

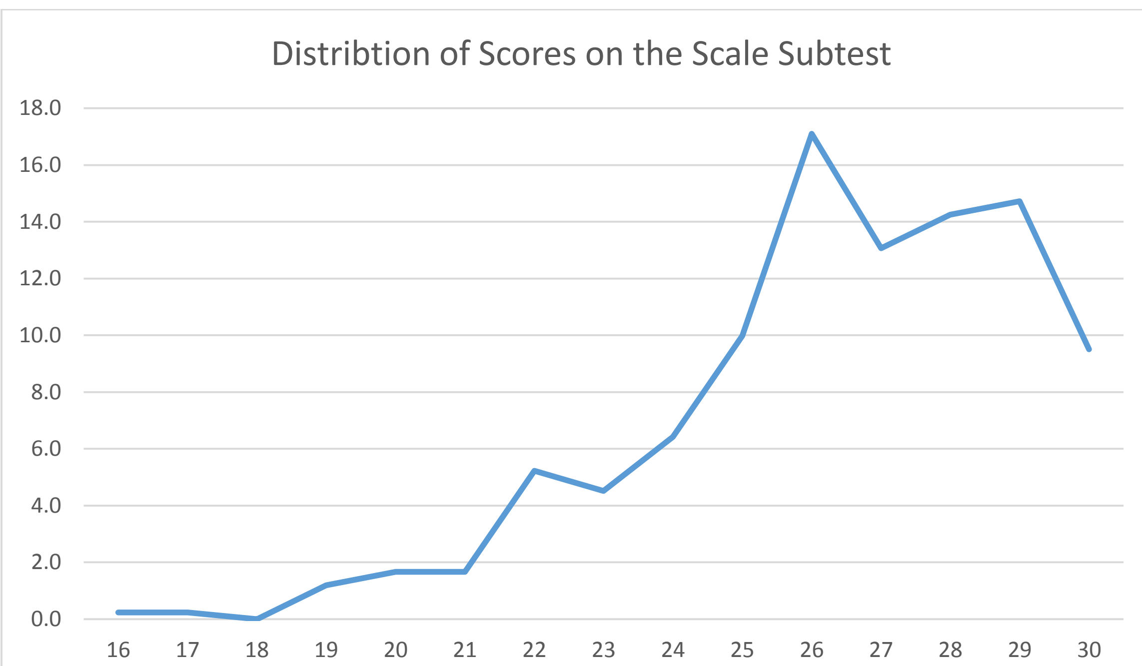
a)

b)

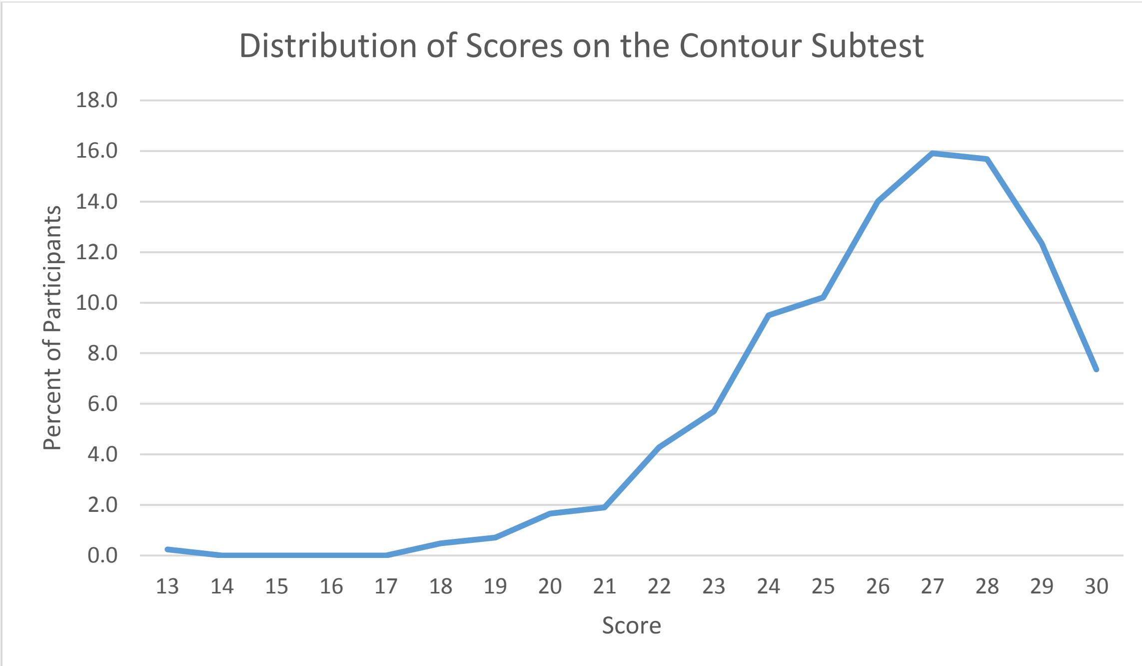
Appendix 2: Distribution of MBEA Scores for 421 Unselected Adults
Data available at: http://www.brams.umontreal.ca/plab/publications/article/57#downloads



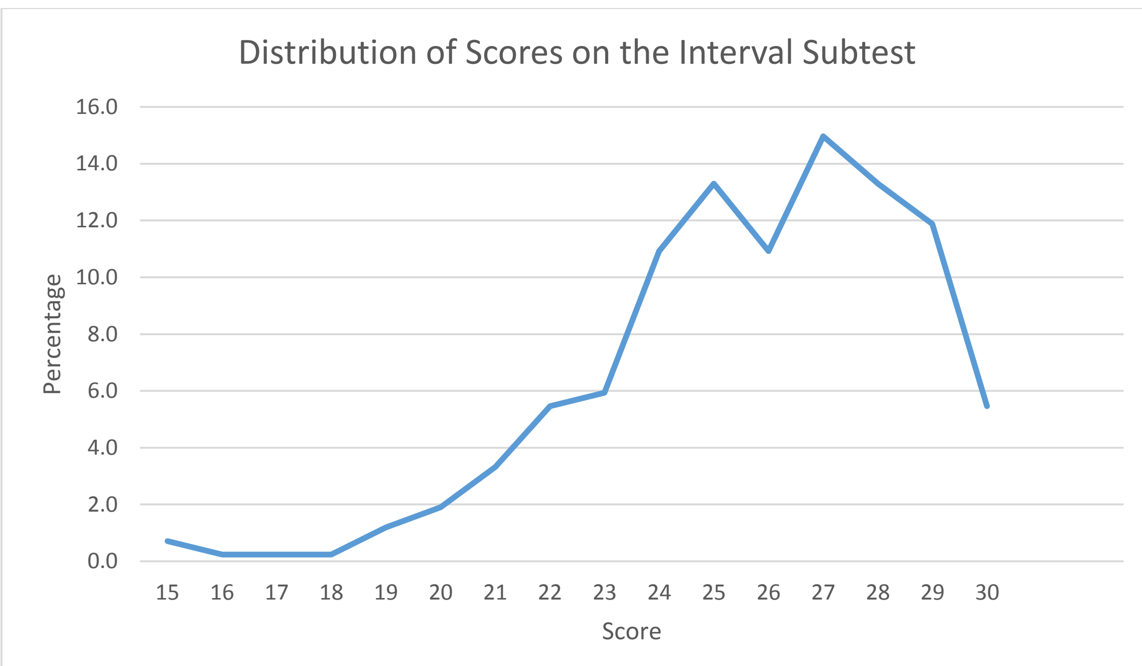
Mean: 26.27 SD: 2.01



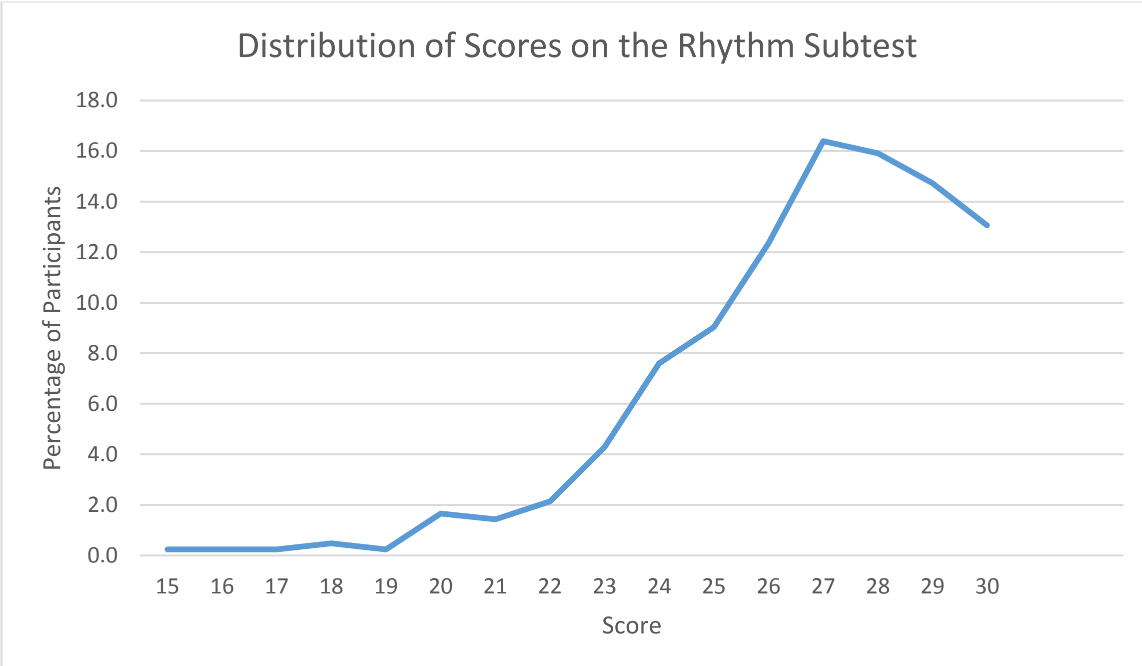
Mean: 26.34 SD: 2.68



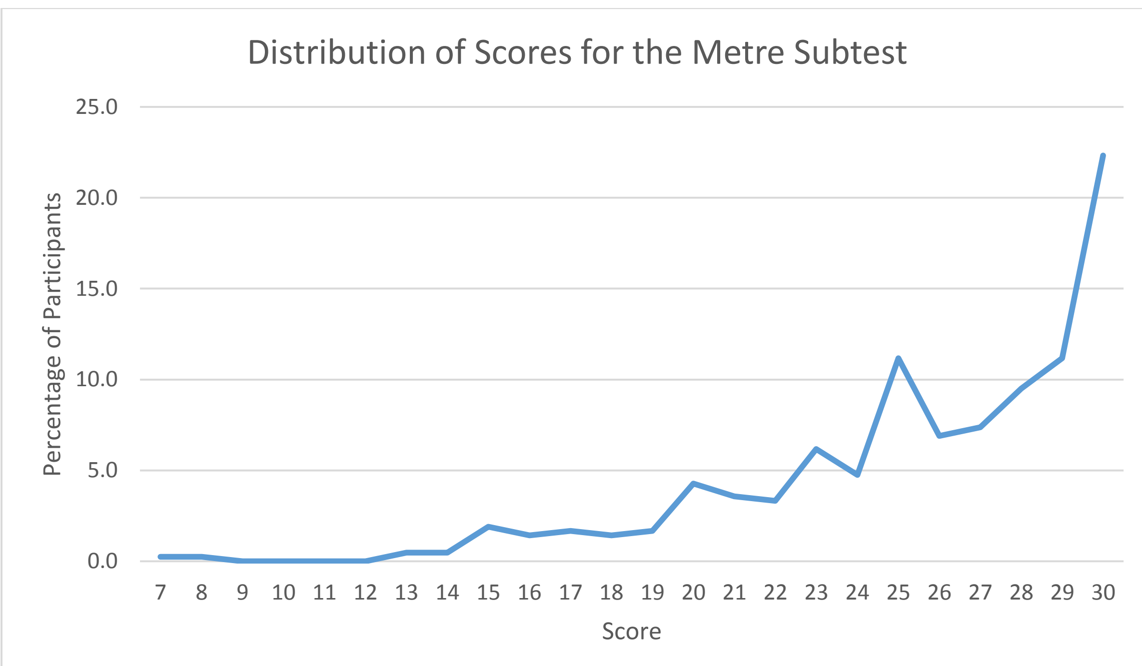
Mean: 26.19 SD: 2.62



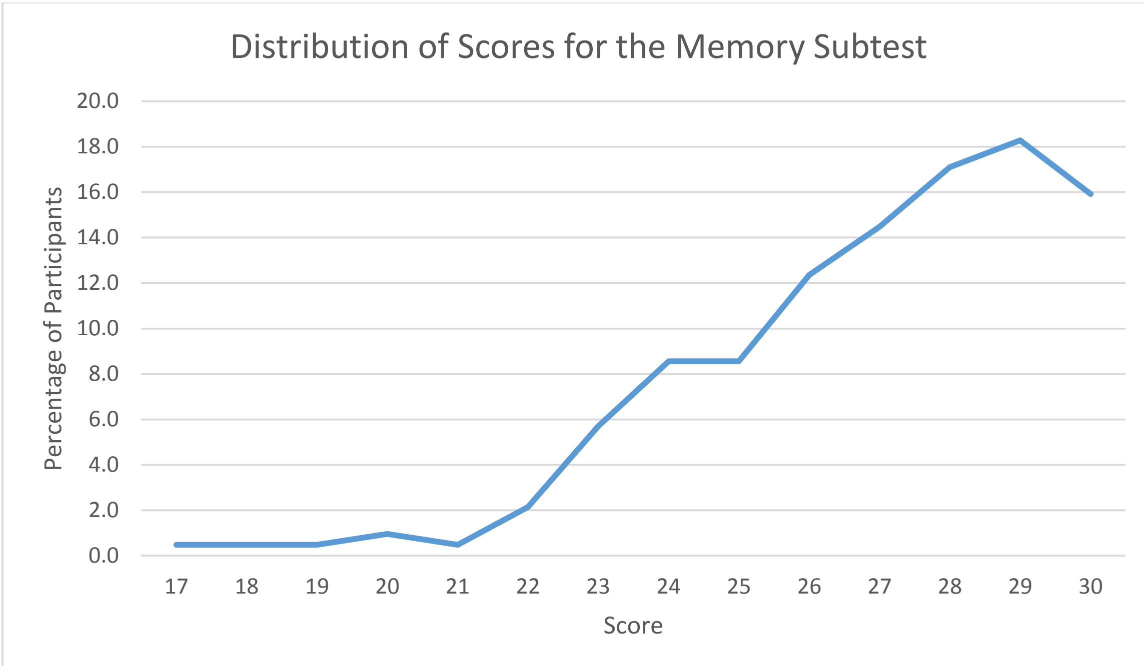
Mean: 25.74 SD: 2.85



Mean: 26.69 SD: 2.64



Mean: 25.59 SD: 4.32



Mean: 27.08 SD: 2.51

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