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## Empirical Investigations of the Hedonic and Emotional Effects of Musical Structure

Vorbemerkung der Herausgeber:

*Der folgende Text wurde bereits in dem Sammelband »Emotions and Art – Problems, Approaches, Explorations« (hg.von L.Ya.Dorfman, D.A.Leontiev, V.M.Petrov & V.A.Sozinov, Perm 1992) veröffentlicht, hier jedoch um ein Postscriptum ergänzt. Wegen der nach wie vor gegebenen Aktualität der Problematik sowie der Schwierigkeit, den in Rußland publizierten Band hier zu bestellen, haben sich die Herausgeber zu einem Nachdruck des Textes entschieden.*

### Introduction

It is often overlooked that one of the most valuable contributions that psychology, its practitioners, and certain unique aspects of their training can make to the analysis and understanding of art (broadly defined) is epistemological and methodological, by which we mean primarily a rigorous, objective, empirical scrutiny of speculative thought. In fact, this is precisely what one of the 19th-century founders of modern psycho-aesthetics, Gustav Fechner, set out to do in his pioneering work (on versions of a Holbein painting). And this is also the path that Daniel E. Berlyne, arguably the pivotal recent proponent of a »new psychology of art«, recommended by the example of the design of his experiments and in his influential book *Aesthetics and Psychobiology* (1971).

On the basis of such considerations, perhaps the principal assumption of our paper is that careful, *empirical* tests of theoretical statements, “manifestoes”, and assorted other pronouncements made by art theorists, historians, critics, and artists themselves can bring much-needed rigor and discipline to

a field sometimes prone to arbitrary speculation and dominated by a doctrinaire authority. This assumption seems especially true when the claims emanating from the artworld concern the nature and degree of emotional, perceptual, cognitive, and social – in short, psychological – impact on listeners, viewers, readers, or theatergoers. And such an assumption would presumably be even more valid when the empirical tests examine the highly general (not to say extravagant) claims about the “human experience” regarding, say, a piece of music, and not the more restricted, humble claims about the response of the composer and a miniscule clique of colleagues and followers.

A variety of problems, using different methodological approaches, have been examined since 1975 in the psychoaesthetics laboratory at the University of California, San Diego, which are relevant in the light of the above considerations. The projects have included an examination of the role of melodic complexity in human physiological arousal and aggressive behavior (e.g., Konečni, 1982; Konečni, Crozier, & Doob, 1975); the interaction of melodic complexity with epistemic, attentional, and memory factors (Konečni & Sargent-Pollock, 1976); the effect of various rhythmic structures – in conjunction with social stressors – on task performance (Flath-Becker & Konečni, 1984); the influence of the Type A/Type B personality syndrome and attention on music processing (Konečni & Gotlieb, 1987); the effects of arousal and the quality of affect on preference for Renaissance and 20th-Century paintings (Konečni & Sargent-Pollock, 1977); the manner in which people use music and visual art to optimize mood (Breckler, Allen & Konečni, 1985); the memory and emotional factors in portraiture (Konečni, 1991a); and the importance of negative emotions in the Stanislavskian and Brechtian systems of acting technique (Konečni, 1991b).

Such experimental efforts, in a wide variety of artistic domains, at UCSD and at least 50-60 other laboratories in USA and literally throughout the world, have elucidated many aspects of the highly complex and diverse points of contact between psychological factors, technical aspects of art, and the speculative theories abounding in the artworld. In this paper, however, we wish to focus on a single research strategy, one that seems to us particularly suitable for empirically evaluating the truthfulness of such speculative claims.

## A Research Strategy for Evaluating the Artworld Experts' Claims

One of the chief advantages of this strategy is its directness and simplicity. The starting point of this type of psycho-aesthetic investigation is to choose an explicit or implicit *important* claim made by a recognized expert about the purpose, impact, or effect (cognitive, perceptual, emotional) of a work of art or some of its components and translate it into a testable form.

The original version of a work is altered in several ways, such that the substantive aspects of the alteration procedures reflect the main lines of the arguments expressed by the expert(s). The original and such systematically altered versions of the work are then presented to research participants in within- or between-subject experimental designs (depending on the topic and feasibility). The research participants may vary from reasonably educated lay people to connoisseurs and experts; their choice is guided by the generality of the original claim.

The experimental subjects are asked to rate the original and the »doctored« versions on various dimensions, always keeping in mind the main elements of the experts' claim(s), as well as psychological meaningfulness and theoretical interest. The rating scales may include the versions' respective pleasingness, interestingness, emotional impact, structural integrity, meaningfulness, stylistic purity, originality, the desire to own a reproduction of the work, ease of it being memorized and recalled, and so on. The statistical analysis of the results allows that the accuracy of the expert's various claims about the work of art be systematically evaluated.

The first five studies of this type at the UCSD psychoaesthetic laboratory (all described in detail in Konečni, 1984) examined: (a) the effects of various orientations of representational and abstract paintings; (b) the comprehension of the meaning of songs (with well-articulated lyrics) from various popular genres; (c) the effects of drastic stylistic alterations in the writing of authors ranging from R.Barthes to (early) S.Beckett to G.Stein; (d) the effects of a rearrangement of the order of movements in Beethoven's quartets and sonatas; and (e) the influence of varying certain features (type of stage, proximity of audience, aspects of blocking) of a theatrical performance.

The results can be summarized quite simply: in sharp contrast to the various experts' pronouncements, very drastic alterations of the works of art produced minimal effects on the responses of the various audiences. Furthermore, this must be viewed in the context of a considerable amount of

the audiences' enjoyment of the works – it was simply that the severely altered versions were enjoyed as much as the original ones.

These results have since been replicated in the same and other artistic domains; also, they foretell our contentions at the conclusion of the present paper. In the remainder of the paper, we focus on our UCSD investigations of the hedonic and emotional effects of tampering with some sacrosanct aspects of *musical structure*.

## Experiments on the Effects of Interfering with Musical Structure

Structure, global and local, is undoubtedly one of the most frequently discussed aspects of works of art. Macro-structure is related to concepts of organization, form, and style, among others, and micro-structure to a whole host of musical elements (e.g., Cook, 1990; Lerdahl & Jackendoff, 1983; Meyer, 1973; Schenker, 1979; Schoenberg, 1984). We now turn to some specific experiments.

*Beethoven's sonatas and quartets.* The first movement of a four-movement classical sonata (from which the quartet can be thought of as being derived) follows the blueprint of exposition, development, and recapitulation, and is typically fast (see, for example, Figure 16 in Cook, 1990, p. 56). The slow movement (often in an ABA form) lies in the middle section of the piece and is expected to hold it together. A minuet or scherzo follows or precedes the slow movement, and the final movement is again fast.

Quoting from the *Britannica Book of Music* (1980, entry on »Criticism«): “If the movements of a great sonata ... are switched around, the result will be musically inferior” (p. 229). And later: “Why does music unfold a particular structure? Why that kind of structure rather than another? The textbooks on form remain silent; yet this is a profound question. It is surely of paramount interest to know why music unrolls in one direction rather than another. Inspired music appears to carry within itself its own blueprint ...” (p. 230). Musicologists have largely agreed and Beethoven is moreover often credited with being the first to switch the structural weight from the first to the last movement of a sonata and treating the whole as a dramatically developed plot (Newman, 1972; Rosen, 1971).

It is because of the unequivocal nature of such statements made by eminent musical authorities about a classical musical form in relation to an undisputed master of both the form and the art of development in general that

we chose Beethoven's works as experimental materials. The two sonatas, Op. 28, the *Pastorale*, and Op. 106, the *Hammerklavier-Sonate*, and the three string quartets (Op. 18, No. 1; Op. 59, No. 3; and Op. 135) span a 25-year period and are universally recognized as sublime achievements.

We made a series of drastic alterations in the order of the movements within the works and presented them to non-music seniors at UCSD. (A particularly »insensitive« rearrangement of the movements was: Last movement, first movement, slow movement, scherzo). The complete details of the studies are presented in Konečni (1984) and the results – which were in sharp contrast to the claims made by the musical authorities – could be summarized as follows: “Only with an extreme modification of the order of the movements [the one described above], and with the subjects forced to compare the original and the modified versions (in the within-subjects design), and only when the original version was heard first, did it emerge as the clearly preferred choice” (p. 85).

It should be noted that (a) *all* the versions were enjoyed a great deal, (b) the research participants who rated the versions were college-educated, young people at an elite university, (c) for many of them classical music already represented the favorite type of music, and (d) many were statistically likely to *become* connoisseurs of classical music.<sup>1</sup>

Commenting on the results of this study, Cook (1990) points out that “Beethoven himself originally concluded Op.130 with the fugal movement now known as the *Grosse Fuge*, later replacing it (at the publisher's instigation) with the present finale« (p.67). Cook relentlessly goes on: »And if it is objected that he must have done this precisely because the original finale was not appropriate ...then other instances can be cited ... In a letter to ... Ferdinand Ries, [Beethoven ] suggested that in order to ensure the publication of his sonata Op. 106 in London, Ries could, if he saw fit, omit the Largo and begin straight away with the Fugue, which is the last movement; or you could use the first movement and then the Adagio, and then for the third movement the Scherzo – and omit entirely No. 4 with the Largo and Allegro risoluto. Or you could take just the first movement and the Scherzo and let them form the whole sonata. I leave it to you to do as you think best.”

“And this is the ‘Hammerklavier’!”, Cook exclaims (p. 67, after Anderson, 1961, pp. 804-805).

This is the type of statement from a great composer that should have found its way to the entry in the *Britannica* cited above. And one wonders

what the response to it would be from a famous music theorist like Heinrich Schenker who »was not in the least interested in explaining how people ordinarily perceive music; what he wanted to do was to demonstrate how music *ought* to be heard« (Cook, p. 21, italics in the original). Would Schenker have to claim that he knows better than the (fickle !) Beethoven the optimal order of the movements in the *Hammerklavier-Sonate*?

*The Goldberg Variations.* The Variations are considered by many authorities (Dowley, 1981; Terry, 1963) to be Bach's most comprehensive keyboard composition. The piece contains an overture, a quodlibet, several dance-type variations, and nine different canons. The performer is called upon to execute a vast array of ornaments and technical manoeuvres (Marshall, 1976).

During the period in which Bach composed the *Goldberg Variations* he is claimed to have been particularly interested in the issues of structure and organization (Wolff, 1976). The aria on which the *Variations* are based contains 32 measures, which are divided into two 16-measure halves. This is duplicated in the overall piece: There are 30 variations between the initial aria and its conclusion at the end of the piece, the aria da capo. Like the aria, the 32 parts of the overall piece are divided into two, with the 16th variation marking the beginning of the second half.

No wonder, then, that the *Goldberg Variations* are universally recognized as the pinnacle of Baroque musical architecture, as a masterpiece of structural design (Gould, 1955; Kirkpatrick, 1984; Robinson, 1973; Terry, 1963).

As if all of the above structural details were not enough, the 30 variations are divided into ten triplets, such that the third variation in each triplet is a canon. And during the piece, the canon interval progresses from the octave to the ninth. According to Kirkpatrick (1954), there is a sense of circular completeness and the variations "follow a geometrical grouping like the beads of a rosary."

Finally, it is important to note that although each variation is commonly thought to have a unique character (e.g., Gould, 1955), many music authorities, including Gould (see also David, 1945; Marshall, 1976) maintain that the impact of the overall piece derives from the contrasting *sequential* nature of the different variations. The piece "unfolds in its grandeur of conception only when the complete series of compositions is heard in succession" (David, 1945). In other words, the very individuality and the contrasting nature of the variations makes a *particular sequence* optimal and "inevitable". They are alleged not to be independent in the sense that they can be

ordered without an aesthetically deleterious effect. This is essential not only as a rationale for our studies of the *Goldberg Variations*, but also because of Batt's (1987) criticism of these studies (described in the next section).

We (Godlieb & Konečni, 1985) carried out three largescale experiments with the *Goldberg Variations*. Because of the debate which rages in the musicological and performance circles over the importance of "authenticity" in the performance of the *Variations* regarding the choice of instrument (harp-sichord vs. piano), style (Classical vs. Romantic), and even, in the case of recordings, "continuous" vs. "spliced" performances (see Godlieb & Konečni, 1985, for details), one of these experiments (using, incidentally, eight different recordings) was directed to these issues and is thus not directly relevant here. The remaining two studies, however, involved drastic alterations of the structural elements of the *Variations*.

In the first of these, three different versions of the *Variations* were presented to research participants in a between-subjects design:<sup>2</sup> The original recording (Version 1);<sup>3</sup> a version in which the 30 variations were randomly scrambled, but the aria and the aria da capo retained their respective positions (Version 2); and a version identical to Version 2 except that the aria was placed immediately after the variation No. 14 (17th position overall) and the aria da capo after the variation No. 15 (30th position overall). Version 3 was thus an even more drastic interference with Bach's structure than Version 2.

On *only one* (warm/cold) out of 15 musicologically and psychologically relevant rating scales was a significant effect: ( $p < .05$ ) found: The original version was judged to be warmer than the other two versions. These results are quite striking: The general, educated audience simply does not perceive (or care for) the inherent structure of the piece that the music authorities are so effusive about (and into which Bach put so much care).

To analyze a piece (by examining the score, by listening to it *for* the underlying structure) is clearly different from "merely" listening to it (or merely "hearing" it, as some would say). Where musicologists and music theorists step on thin ice is when they imply more or less explicitly that great enjoyment is impossible without what they consider to be a successful or learned analysis. After all, the subjects enjoyed all three versions, but about *equally*.<sup>4</sup>

In the third study, the focus was on the much-praised triplet organization of the *Goldberg Variations*. Two triplets (Variations 1-3 and 16-18) were isolated from the piece (again Gould's 1955 recording) and presented to sub-



jects in a within-subjects design in both the original version and a random order (1, 3, 2 and 18, 17, 16). Each of the four triplets was rated on eight scales chosen from the set of 15 used in Study 2.<sup>5</sup>

There were no significant differences on any of the scales. The subjects enjoyed the triplets equally whether the canon was in the first (18, 17, 16), second (1,3,2), or its “natural”, third position (the two original versions).

Jointly, the results of Studies 2 and 3 question the significance of both global and local structure for the subjects’ considerable enjoyment of this superb example of Baroque musical architecture.

*First Movement of Mozart’s KV 550.* In an article in an influential journal, the musicologist Robert Batt challenged the suitability of the *Goldberg Variations* as material on which to test the importance of structure: “Such works are usually based on a self-contained, and even pre-existing theme; to the extent that each variation has the same form, harmonic, and melodic structure, and is in the same key, each variation is also self sufficient” (Batt, 1987, p. 210). In short, Batt claimed that the *Variations* could be altered without negative impact to the natural progression because of the variations’ independence.

In view of the opinions we cited in the previous section, Batt’s claims strike us as being either contrary to the musicological mainstream, or, more significantly, as an armchair musicologist’s retreat in the face of experimental evidence. Put another way, Batt’s “new look” at the structure of the *Goldberg Variations* is a “major concession, an *empirically* forced distancing from musicologists with a penchant for the fatalistic and architectural metaphors, the tenor of which is to exalt the ‘inevitable’ and ‘inexorable’ structure of the *Goldberg Variations*” (Konečni, 1987, p.217).

Be that as it may, Batt suggested a different vehicle for testing the importance of structure, the First Movement of Mozart’s Symphony in G Minor, KV 550. As he put it, “[not] only could the order of the exposition, development, and recapitulation be altered, but the order of the four sections within the exposition and, separately, within the recapitulation could be altered” (p. 212). Batt then proceeded to specify the bar numbers of the nine sections of the movement (see Batt, 1987, p. 212 and Karno & Konečni 1992, Table 1). In short, Batt seemed to suggest that using this movement as experimental material would be an acid test of the importance of structure.<sup>6</sup>

We (Karno & Konečni 1992) developed four altered versions of the movement. Whereas the original consists of Exposition (theme 1, transition, theme 2, closing), Development, Recapitulation (theme 1, transition, theme

2, closing), in Version 3, for example, the order was Recapitulation (Theme 1), Exposition (theme1, closing), Development, Recapitulation (transition, closing), Exposition (transition), Recapitulation (theme 2), Exposition (theme 2).

In the first study, the original and the four altered versions were presented to non-music undergraduates at UCSD on a within-subject basis (all subjects heard all five versions, with a third of the subjects randomly assigned to one of three different orders in which the five versions were played to them. The subjects rated each version's pleasingness and interestingness and indicated the degree of their desire to own a recording. They also rated each version in terms of "best overall structure".

The results were clear: There were no indications whatsoever of a consistent preference for the original version. Instead, a significant primacy effect was obtained. In each of the three orders the version heard first was favored on the pleasingness, interestingness, and the-desire-to-own measures (this included favoring the original in one of the orders). Other than this primacy-mediated effect, the closest the original came to being rated the highest was in terms of the "best overall structure", but even there the chi-square was associated with only a  $p$  of .12.

In the second study, the subjects were undergraduate music majors at UCSD. Their written comments, not surprisingly, indicated a greater degree of familiarity with the music than was the case in the first study. A within-subjects design was again used and the subjects randomly assigned to one of two orders of presentation of the five versions (in one of the orders the original was heard first).

The results replicated those of the first study in that there were no main effects of the Version and Order factors. This time, however, there were also no significant interactions and thus no primacy effects. Furthermore, the rankings of "best overall structure" favored the original even less than in the first study. In fact, Version 3 (a random arrangement of the nine sections that was described earlier) came close to having its »structure« rated the highest overall.

## Discussion

There was thus no empirical support in the studies we described for the notion that either global or local structure – as conventionally defined – influ-

ences the ratings of various musical masterpieces on an array of hedonic, emotion-related, and cognitive judgment scales. Even when structure was judged directly, the original versions of these specially and conservatively selected works fared no better than the “doctored” (or “butchered”) ones.

From the trenches of musicologists, music theorists, musicians, and others, there are several possible lines of attack on these conclusions and we shall examine them in turn.

Perhaps the most (apparently) damaging line of attack is logical/statistical – somewhat of a paradox considering the underpinnings of our approach. The criticism would be that not much can be said on the basis of a series of null results. However, there have actually been quite a few significant results in the studies described. Viewed jointly, these results are important because they specify in some detail under what precise conditions the structure *does* exert an effect on hedonic, emotional, and perceptual judgments (and this is not limited to music). Factors such as primacy (in our studies), recency (see Cook, 1987, p. 200), the degree and type of structural intervention (the Beethoven sonatas study), and the necessity for the subjects to listen to both the original and altered versions before being able to respond especially positively to the original version should be seen by musicologists in a constructive way: In statistical parlance, structure<sup>7</sup> appears to exert an influence on hedonic processes not as a “main effect” (as commonly thought), but through interactions with a variety of other – musical and nonmusical – factors.

Another defense against the “null-effects” attack is based on a principle well-established in the philosophy of science regarding inductive procedures: “[W]hen a substantial number of studies using different methods and domains of inquiry ... but all asking the same basic questions – all obtain a negative answer, this cumulative evidence begins to count” (Konečni, 1984, p. 89). This is especially true when the experimental materials are chosen and the experiments designed to maximize the finding of the effect (of structure) if it, in fact, exists. We thought we did this by following Batt’s (1987) recommendation about working with Mozart’s KV 550.

A different possible line of musicological attack against our conclusions is that hedonic response to still finer aspects of microstructure ought to be examined. This reasoning, when carried to its logical extreme, becomes an easy target for a *reductio ad absurdum* counterattack, because at some point structure becomes *texture*. In fact, even raising this argument represents a major retreat from conventional musicological thinking.

A further apparently serious challenge to our negative conclusions regarding the lack of hedonic effects of musical structure is concerned with the issue of who should and should not be a subject in our type of experiment. We addressed this issue briefly earlier in this paper and at length elsewhere (e.g., Konečni, 1984). The challenge consists of an escalating series of demands for musical sophistication of the subjects (the level deemed essential typically not being based on *a priori* theoretical grounds, but rather decided upon in an *ad hoc* manner, depending on which particular study a particular musicologist wishes to criticize at a particular time). College-educated, wellrounded, intelligent consumers of classical music (in terms of their listening and music-purchasing preferences) just will not do for some musicologists, even if they are music majors and play an instrument. What one must have, says Batt (1987, p. 212, in recommending KV 550) are “only...subjects familiar with Classical period music...[which] relies heavily on the listeners’ hearing of the subtleties of its structure, on the listeners’ ability to read the code of the music”.

But that is just the beginning. There is the question of other factors, such as the mental and emotional faculties (inborn or learned, usually left unspecified, but we suspect the former are implied): Simply listening to music, says Cook (1990, p. 15) when describing Hanslick’s views in the latter’s influential *The Beautiful in Music* “has nothing to do with beauty because it does not involve an imaginative awareness ... Aesthetic value in music ... means experiencing a piece of music as a kind of beautiful object through ‘the voluntary and pure act of contemplation which alone is the true and artistic method of listening’.”<sup>8</sup> This view is echoed in ideas of “active perception”, “conscious participation”, “a transference of concepts from sphere to sphere” (all Schenker’s concepts), in the exclusionary pronouncements based on extreme demands for technical expertise and talent by authorities such as Adorno, Dahlhaus, Cone, and many others, and also reflected in some contemporary general theories of aesthetics (e.g., Scruton, 1979). Cook (1990, p. 21) cites Schenker, “the uncorrupted instincts of which [the dilettante] is so proud have no value whatever for art itself as long as they [are] unable to move on the same level as the artistic instincts of the masters ... ” Cook continues that Schenker’s “conviction [was] that virtually nobody knew how to listen properly to the master-works of the past” (p. 21).

A reader with even the slightest sympathy for the normative experimental approach can draw obvious conclusions from this. The only appropriate experimental subjects are the masters themselves (and Schenker). The nec-

essary-type-of-subject argument by musicologists as well as analogous experts in other artistic domains is rather disingenuous because their positions without experimental evidence – are often couched (implicitly or explicitly) in highly general terms. Such claims are clearly meant to apply not just to a couple of grandmasters, or a handful of experts, or even to a few thousand connoisseurs, but are phrased in terms of the “human response” (perceptual, cognitive, emotional, culturally-determined, etc.), “human neuropsychological make-up”, “hemispheric dominance”, or what have you (see Gordon, 1981, and Konečni, 1984, 1991a, for a fuller discussion and examples of this point).<sup>10</sup>

The fact of the matter is that the degree of musical training has not been found to play a very important role in many experiments in our and other laboratories. This is by no means limited to research on the role of structure such as that described in this article. For example, using first- and second-year music majors as subjects, Cook (1987) found that the “influence of tonal closure over listener’s responses is restricted to a maximum time scale, possibly on the order of 1 *min*” ( p. 203, italics added). Yet large-scale tonal closure is one of the absolute fundamentals of Western music of the tonal (18th and 19th) centuries. And as Cook points out (tongue-in-cheek, we suspect), one minute is a period “much shorter than the duration of most tonal compositions” (1987, p. 197).

In other experiments, Cook examined a very elementary, but important, issue – the listeners’ ability to detect repetition. The matter is of considerable interest because of “Schoenberg’s frequent statements regarding the importance of repetition in clarifying formal structure” (1990, p. 45). In one of the studies, the stimulus material was the first movement of Webern’s Symphony Op. 21: “Despite the fact that they had a relatively high degree of musical training [2nd and 3rd-year music students at Cambridge University ], only half of these listeners observed the *literal repeat* of the exposition; the others heard the entire passage as being through-composed” (Cook, 1990, p. 45, italics added).

In other experiments challenging serialist assumptions underlying their compositional devices, Deutsch (1984) demonstrated – with audiences of a very broad range of technical expertise – that interval class cannot be treated as a perceptual invariant; yet, Schoenberg included octave separation as an example of “equivalence” (along with transposition, inversion, etc.) .

Also, in her other work, Deutsch showed that perceptual configurations

based on pitch proximity may entirely override the effects of spatial separation. Thus, "spatial separation [forcefully urged, for example, by Berlioz] by no means guarantees that music will be perceived in accordance with the positioning of the instruments" (1984).

It is clear, then, that many music-theoretical assumptions and doctrines – for example, regarding the effects of global and local structure, large-scale tonal closure, equivalences, and spatial separation – are simply wrong for a broad range of audiences, at least under certain conditions. This is particularly so if one is primarily referring to the *listeners' aesthetic response* – hedonic, emotional, perceptual, cognitive. So the defensive strategy of some musicologists, notably that of Cook – paradoxically one of the perhaps most empirically-minded among them – has been to make a sharp distinction between the music-theoretical *analysis* and the listeners' *perception* (broadly defined). For instance, consider Bailey's (1983) statement that Webern's Symphony represents "two quite different pieces – a visual, intellectual piece and an aural, immediate piece, one for the analyst and another for the listener" (p. 195). Cook (1990, p. 58) explicitly agrees and adds that »while this may be an extreme case, it is an extreme case of a general phenomenon« (p. 59). And elsewhere he puts it even more plainly: "[Music] theory...is more usefully regarded as a means of understanding [compositional] organization than as a means of making empirically verifiable predictions regarding the effects of music upon listeners" (1987, p. 197).

Yet when Schoenberg says that repetition is important in "clarifying formal structure" (Cook, 1990, p. 45), for whom precisely is it being clarified? Could it be that even Schoenberg, that abstract analyst *extraordinaire*, had the listener somewhere in the back of his mind (and was wrong in his assumptions)? In fairness to Cook, he is quite willing to acknowledge the existence of musicological excesses far greater than those of Schoenberg (see 1987, pp. 203-205).

In our opinion, however, Cook and other musicologists have retreated too far and have drawn *too* sharp a line between the analytic and the perceptual, between the spatial in music organization and the temporal in the listener's mind, between the role of structure, tonal closure, and so on as compositional, indeed pedagogical (Cook, 1987, p. 204) devices and their having an actual, measurable effect on the listeners' enjoyment. This is because we feel, and the data support us, that the hedonic and emotional effects of musical structure are complex, higher-order effects: Structure acts in conjunction with other formal and contextual factors. What is, therefore,

desirable is to study – empirically, parametrically, and collaboratively – the precise conditions under which listeners respond to structure, tonal closure, and so on, and to what degree.

Despite these sentiments, and notwithstanding Cook's openmindedness (as well as the existence of truly interdisciplinary journals such as *Music Perception*), it is probably premature to be too optimistic and conciliatory regarding the rapprochement of music theory and experimental psycho-aesthetics. After all, the experimental results presented in this paper can be summarized as follows: Given their analytical doctrines, it is easy to destroy a piece of music by structural alteration by the standards of conventional music theorists; in contrast, from the perspective of our samples of listeners' enjoyment, a great work is immune to quite radical interventions. As the music theorist Thomas Clifton put it (quoted in Cook, 1990, p. 59), »for the listener, musical grammar and syntax amount to no more than wax in his ears«.

## Postscript

This post-scriptum has a substantive and a polemical part.

In strong support of the substantive arguments in our article, Tillman and Bigand (Université de Bourgogne, Dijon; 1994) have recently shown by means of a somewhat different experimental procedure that global musical structure can be drastically altered with no detriment to the hedonic impact on average listeners. Short compositions by Bach, Mozart and Schönberg were broken into brief segments (average duration = 6 sec.). Non-music students heard all three pieces (within-subjects factor), but with the segments of each piece played either in the original sequence or in the inverse sequence (between-subject factor; the splicing noise was electronically removed). This procedure thus totally destroyed the structure of each piece as a unitary whole, but since the material *within* the segments was left intact, the "surface characteristics and local structures" of the pieces were not altered in the modified versions. In terms of 29 semantic-differential bipolar scales, ranging in content from the hedonic to coherence and expressivity, it was found that this radical alteration of global structure resulted in essentially no decrease in the pieces' aesthetic appeal.

By itself, this result would have been hard to believe, but as a part of the now large body of evidence on the weak effects of musical structure (as tra-

ditionally defined) on musical perception and enjoyment, it makes a great deal of sense.

In a recent polemical article, Swain (1994) claims that the lack of empirical support for various cherished music-theory principles (including some of the findings reviewed in our paper) is not really bothersome after all. Commendably, he rejects the notion that music theory should »go its own way«, abstractly and axiomatically, intentionally oblivious of research. But, unfortunately, he does not feel forced to squeeze through Cook's well-constructed and reasonable escape hatch – that many theoretical principles are simply, and importantly, useful pedagogical tools for music students, and analytical and organizational devices for composers. Instead, he suggests that the constantly mounting negative results in studies of musical perception are important and relevant, but differentially so, for various “musical communities” that have different competencies and play different roles.

The gist of the “musical communities” argument is identical (without being acknowledged as such by Swain) to the type-of-subject issue that I thought I had preemptively dealt with at length already in the 1984 paper. It was further gone over in the Batt-Konečni exchange in 1987, and was summarized in the present article (middle of p. 129, comments regarding Cook and Schenker). Essentially, like Batt, Swain is compelled by empirical findings to scale down music-theoretical claims from grandiose universals to rules that are perhaps applicable to the activity of sometimes very tiny »communities« that are perhaps influential from some points of view. Moreover, once the right experimental participants have been identified, they, says Swain, have to be asked the right questions. Cook should not have cunningly asked his subjects how much they liked the pieces (modified so as not to end in the “correct” key), but rather, “[D]oes the composition end in the same key in which it began, and does this increase or decrease the sense of closure?” (Swain, 1994, p. 314).

This totally misses the point of Cook's and similar studies. The relevant question is not whether a person *can* notice that a piece ends in a music-theoretically “wrong” key when asked to pay attention to this, but whether the lack of closure inexorably exerts a negative effect on the hedonic judgment of the piece. In other words, does tonal closure have causal power with regard to pleasure? My bet is that it does not, except for very short pieces, and except for the composer as subject (a musical community of one) – and also possibly for scholars who “know” (and have ceased to listen to) the music, which would include people who believe that the true experi-



ence of music is possible only with the score in one's lap. (Anecdotally, many composers vehemently reject this type of music experience even for themselves). Yet, to his credit, it is not that Swain haughtily rejects the importance of compositions causing pleasure to listeners, far from it: "What is the point of the pedagogy, after all? It teaches composers how to make good effects on listeners ..." (Swain, 1994, p. 318).

Swain is undoubtedly correct in saying that miniscule, but highly gifted and influential groups exist in music (and most other fields) and that what they can perceive and do may be very important regardless of normative results. It is a further truism that at least some of these people's perceptions and inventions can be taught to some others. But, how does the average music theorist reconcile, for example, Cook's results that advanced music students at Cambridge University cannot detect repetition (p. 130 of the present article) – quite apart from whether repetition, even when consciously detected, would be hedonically relevant – with Schönberg's sweeping theoretical claims? Swain's idea of »communities« with variable and partially overlapping memberships can, of course, easily accommodate this – too easily: Schönberg thought of repetition as clarifying formal structure; Schönberg is undoubtedly an influential composer; therefore, repetition is important and should be taught as a tool to students – at least, that is, until serialism breathes its last.

The idea is too flexible for its own good. Each empirical result defines a new "community" on a post hoc basis. Miniscule communities of transient significance (cliques and music bureaucracies) can be used as excuses for faulty theories instead of the theories being closely re-examined to begin with, especially with regard to generality. Most importantly, Swain's concept presents no guidance for future research. Nor does it have anything to say on the all-important relationship between the detection of a musical phenomenon (with or without specific instructions to research subjects, including "Schönberg") and the hedonic relevance of that phenomenon.

To paraphrase what I said elsewhere (Konečni, 1984, p. 91): Various attributes of a piece of music may interact in complex ways and only certain subsets of the higherorder interactions may produce highly positive effects (in terms of the rated pleasingness, interestingness, etc.) Music theorists, partly because of the inherent limitations of the purely speculative, non-experimental style of inference, and partly because of the prevalent linearity of human thought and language, may fail to see the relationships among attributes of a musical work (e.g., global structure, tonal closure) as highly in-

teractive and context-dependent. They may be correct in providing accurate lists of important unitary attributes, yet fail to predict the complicated ways in which such attributes combine to produce the observed experimental effects. The solution (p. 132 of the present article) – careful parametric studies of even the most sacrosanct music-theoretical principles, using various types of listeners – is one that Swain would readily agree with, judging by the tenor of his article.

## Summary

A research strategy for evaluating the artworld experts' claims was first reviewed. A number of experiments were then described which had been designed by the authors to test the alleged effects of musical structure on the hedonic and emotional responses of various categories of research participants. Some of the pieces examined were Beethoven's sonatas and quartets, Bach's "Goldberg Variations", and Mozart's KV 550. No empirical support was obtained in the various studies for the notion that either global or local structure, as conventionally defined, influenced the subjects' ratings of the masterpieces on an array of hedonic, emotion-related, and cognitive judgment scales. The significant effects that were found were subtle, complicated, and unexpected. Various explanatory possibilities were discussed in detail.

## Footnotes

- 1 With regard to points (c) and (d), it is worth noting that we have been informed by the marketing department of the 24-hour-per-day classical-music station in San Diego, KFSD – which incidentally has the proportionally highest local listening share of any classical-music station in the USA – that 78% of their listening audience is college-educated.
- 2 The subjects differed widely in their musical background, so this and the related variables (e.g., familiarity with baroque music; playing a keyboard instrument) were used as covariates.
- 3 Glenn Gould's first (1955) recording was used. He is considered as a pure "Bachian" player and even though the rendition is on the piano, Gould's playing was thought by many reviewers (Hume, 1955; Said, 1983; Wadsworth, 1980) to be singularly devoid of pianistic effects.
- 4 On a 200 mm pleasingness scale anchored by 0 = not pleasing and 200 = very pleasing, the means were 147, 148 and 136 mm from the negative end for Versions 1, 2 and 3, respectively: Quite positive ratings and miniscule differences.
- 5 Slow/fast; weak/strong; orderly/not orderly; pleasing/not pleasing; interesting/not interesting; beautiful/ugly; crisp-clear/not crisp-clear; wish to own (a cassette)/do not wish to own.
- 6 Commenting on our Beethoven sonatas and *Goldberg Variations* findings, Cook (1987, pp 197-8),

among other things, says: "To a classically trained musician, such findings may be a little disappointing, but they are not particularly surprising. Most theorists in fact recognize that the structural relationships between the movements tend to be weaker than those *within individual movements* (italics added).

- 7 Within-subjects designs in which the research participants hear both the original and altered versions accomplish two distinct things: (1) the subjects' ability to compare the versions and (b) the reduction in statistical error. It is not clear which of the two (or perhaps both) were operative in our studies. If the latter was responsible, this would indicate a weak effect requiring literally hundreds of subjects per condition in a between-subjects design for the effect to manifest itself.
- 8 Hanslick, writing in 1854, would be surprised to learn that he himself was merely hearing and not really listening to music, for some have "associated hearing... with romantic music and listening with 20th-century music" (Cook, 1990, p. 36, footnote 13, referring to a statement by Ortega y Gasset).
- 9 Glenn Gould (e.g., 1987), in various interviews, expressed a rather more favorable opinion of the "instincts of the dilettante".
- 10 Viewed pessimistically, this has to do in part with the sociology of scholarship and the nature of academia: The more sweeping the claim and the greater its generality, the more important it (and its author) must be.

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