

Toward a New Comparative Musicology: Some Comments on the Paper by Savage and Brown

Victor Grauer

PATRICK Savage and Steven Brown present a welcome and, in my view, convincing, argument for the revival of what was once regarded as an essential component of world music research, but, over the last 50 years or so, has fallen into disrepute and disfavor: comparative studies. Their aim is, wisely, “not to dwell on comparative musicology’s troubled past, but instead to point toward a bright future by applying new methodologies and paradigms to some of its unanswered questions” (149). To that end, they re-examine “five major themes”: (1) classification, (2) cultural evolution, (3) human history, (4) universals, and (5) biological evolution.

The authors go well beyond what one might expect from a general overview to present in-depth analyses of problems and possibilities associated with each of their five themes. While space does not permit a detailed breakdown, I shall list some items I find especially welcome, followed by a brief discussion of certain reservations and/or points of disagreement. Before I begin I want to make it clear that I am, for the most part, enthusiastically in agreement with almost all the arguments and analyses presented here, as well as the drift of the essay as a whole.

SOME HIGHLIGHTS

I shall begin with a brief synopsis of some points I found especially useful and/or insightful:

- The distinction drawn between the *phenetic* classification of surface similarities and the far more ambitious and problematic *phylogenetic* trees intended to reflect evolutionary and/or historical developments.
- A discussion of problems that can arise when comparisons are drawn between broadly defined and rather vague groupings such as “Old Europe” or “Arctic Asia,” as opposed to the more precise groupings recommended here, such as “Basque,” “Slovak,” “Chukchi” or “Ainu.”
- A welcome critique of the “one culture = one music” model characteristic of much comparative research of the past, including certain aspects of Alan Lomax’s Cantometric research. As they argue, “the idea that a culture’s musical repertoire can be captured by a single representative song or style” can be especially misleading when considering all but the most culturally homogeneous societies (157).
- A discussion of the difference between comparisons based on “acoustic” elements such

as scales, melody types (or even stylistic features such as vocal quality, blend, loudness, rhythmic coordination, etc.), vs. “non-acoustic” aspects of the musical picture, notably the various social functions and psychological effects of different musical genres, all too often ignored in comparative studies of the past.

- An especially welcome argument for the value of analytic techniques involving *quantification*, an important aspect of scientific research too frequently ignored by ethnomusicologists.
- A timely reminder that “outdated Spencerian notions of progressive evolution” too often assumed by comparativists in the past have been supplanted in recent years by more methodologically sound advances in historical linguistics and evolutionary biology that can and should be adapted by the ethnomusicologists of today; at the same time, the authors wisely remind us that “music evolution does not necessarily mirror the patterns of languages, genes, or any other system” (165).
- A crucial distinction between the evolution of purely musical variants, as treated, for example, in Charles Seeger’s essay on the ballad “Barbara Allen,” and “the social forces that determine which musical variants get transmitted to future generations and which ones die out” (167).
- A discussion, especially dear to my heart, of the role music can play as a tool in reconstructing human history, “including patterns of migration and interaction that have occurred from recent times all the way back to the migration of humans out of Africa tens of thousands of years ago” (171).
- A reminder that the study of musical universals “cannot be based exclusively on cognitive psychology, child development, neuroimaging findings, evolutionary arguments, or comparisons between human and animal behavior” unless such studies are based on “cross-cultural analyses of music and musical behavior” (174–75); since almost all such studies have drawn their samples from individuals with a very narrow range of cultural backgrounds and musical interests, their value in the determination of universally valid findings is questionable.
- A review and critical analysis of some of the most widespread theories of musical origins, from Condillac and Rousseau to Mithen, Patel, and Brown himself, with special attention given to the very interesting possibility that music and speech may have developed in tandem; as I see it, Brown’s notion of *musilanguage* is of special importance as it has the potential to reconcile the clear evolutionary advantage of language, as an adaptation important for human survival, with the apparent evolutionary irrelevance of music, famously dismissed by Steven Pinker as “auditory cheesecake.” If the early development of language depended on certain musical capacities, once widespread but now more specialized, then musical aptitude could be seen as a kind of “vestigial organ,” an adaptation that at one time made an evolutionary difference, even if it no longer does.

- An all-important caution against “the assumption that contemporary tribal cultures, such as hunter-gatherer cultures, represent the ancestral state of human behavior and thus can serve as models of ancient humans” (179): as I have argued many times, all hunter-gatherer societies are not alike, nor is their music necessarily similar, either note-wise or style-wise. (To my surprise, Savage and Brown include *me* in their list of offenders, an accusation I vociferously deny, as explained below.)

The essay concludes with an eloquent defense of comparative musicology from its present-day detractors, turned off for many years now by “a host of methodological and ideological problems” in the work of their predecessors (182). Savage and Brown patiently field criticisms centering on comparative musicology’s presumed complexity, its alleged foregrounding of the purely “acoustic” at the expense of sociocultural context, and its past tendencies to accept what are now regarded as politically insensitive assumptions pertaining to race, “unilinear progressive evolution,” social Darwinism, outdated and insensitive notions of the “primitive,” and, one could add, a pervasive blindness with respect to issues relating to the role of women and gender. The authors argue, convincingly, that none of these issues is inherent in comparative studies per se, and that an updated, more methodologically sophisticated and politically sensitive comparative musicology can indeed rise from the ashes of its now outdated ancestor.

SOME RESERVATIONS

Time now for some reservations, quibbles, nitpicks, what have you:

- I cannot completely agree with the opinion offered here that *phenetic* classification must necessarily precede classification based on *phylogenetic* relationships. When we attempt to classify purely on the basis of surface relations we are, in a sense, flying blind, and consequently all too easily misguided by our chosen methodology. For example, a phenetic comparison of repertoires based solely on scale structures might well produce a classification totally at odds with one based on vocal style. Classification completely separated from evolutionary/historical considerations may thus turn out to be either pointless or misleading. As has often been noted, it is only in the light of a coherent theory that meaningful observation is possible to begin with. Thus, as in all other sciences, observation and theory should ideally go hand in hand.
- Under the heading “Sample size,” the authors recommend, as a corrective to the flawed “one culture = one music” model, a simple increase in the size of each cultural sample: “the greater the within-culture diversity, the greater the sample size needed to provide a reasonable picture of overall patterns of musical style” (157). While a large sample size is always desirable, I do not see sample size alone as an effective means of dealing with the problem of within-culture diversity, because regardless of how large one’s sample might be, one will still need analytic tools to separate each “sub-style” from all the others. They are on firmer ground when, a bit later, under the heading

“Quantification,” they outline a statistical approach based on “clusters of stylistic similarity, either within or between cultures” (162). The awareness that stylistic heterogeneities may well reflect historical developments that cut across the traditional cultural boundaries led me to develop the approach reflected in my early paper “Some Song Style Clusters” (1965), which focused on the global distribution of clusters of distinctive features rather than broad-based comparisons among presumably static and homogeneous “cultures.” As I see it, the two approaches complement one another.

- Under the heading “Scope of comparison,” an approach they refer to as “remote comparison,” i.e., the comparison of “small numbers of songs from very distant regions,” is singled out as a weakness of old-school comparative musicology, due at least in part to the danger that “particular songs that satisfied preconceptions of musical similarity” could be “cherry picked” at the expense of songs exhibiting important dissimilarities (158). In my view, the relative strengths and weaknesses of this mode of research are more profitably subject to discussion and debate than outright dismissal. What should always be of primary concern is the question of whether or not any given approach is appropriate to the nature of the hypothesis being tested. If one wants to argue that, say, Flamenco and Hungarian Gypsy music are stylistically equivalent, then the cherry picking of similar songs from both repertoires would certainly be misleading, since the chosen examples might not be representative. If, on the other hand, one were testing the hypothesis that pockets of a very specific style of drone vocalization survive among minority groups in many different societies worldwide, then it is not necessary that the selected “cherries” be representative of the society as a whole—the presence of even one might well be sufficient, assuming that it has been adequately documented.
- Over and above the issue of cherry picking, I must also question the authors’ tendency to dismiss remote comparison altogether, as though there were something inherently illegitimate about this approach. While it is certainly true that remote comparisons based on fanciful, poorly supported assumptions can be identified as one of the major weaknesses of old school comparativism, especially as manifested in the notorious *Kulturkreis* model, we now have so much more and better evidence, both musical and non-musical, from so many legitimate sources, that there is far less danger of ungrounded speculation. As I see it, despite some overly speculative “sins of the fathers,” remote comparison lies at the heart of the comparative method, and remains the arena in which some of its most exciting discoveries are to be found.
- Under the heading “quantification,” the authors make a special point of distinguishing between within-culture diversity and between-culture diversity, but such a distinction implies that we have some absolute basis for determining diversity per se, and we do not. Diversity can never be assessed independently of the standard used to determine it. Thus I must take exception to the findings of Rzeszutek et al. (2012), as cited here, when they claim for music generally a higher level of within-culture than between-

culture diversity, a conclusion based solely on a study of indigenous songs from Taiwan and the Philippines. For one thing, the results of a study of only two culture areas can hardly be extrapolated to apply to the world in general. For another, the music of Taiwan represents, as I see it, a special case in which between-culture differences among the various indigenous groups are unusually difficult to determine. More fundamentally, my own experience in research of this kind tells me that levels of within-culture diversity will be much greater when based on scales and melody types, for example, than the sort of stylistic features on which *Cantometrics* is based. Indeed, *Cantometrics* was very consciously designed to focus on those features most likely to favor between-culture diversity over within-culture diversity, a response to more traditional, notation-based methods that failed to reflect between-culture differences easily perceived by even the most unsophisticated listener.

- Under “Features,” the authors distinguish between “acoustic” and “non-acoustic” musical information in a manner that is meaningful, but also to some extent misleading. Since the performance of music is already a highly symbolic act, a recording of a musical performance can go well beyond the limits of the purely “acoustic,” just as the reading of a literary text goes well beyond the limits of the purely “visual.” While it is of course important to take into account all sorts of contextual ethnographic data not apparent from a recording alone, it is also important to recognize how much of importance *can* be conveyed via a good recording made under appropriate conditions.
- I would now like to complain a bit about what has been left out. As I see it, there can be no really meaningful phylogenetic study in the absence of a baseline, either already established or posited. A phylogeny is, in essence, a tree, and a tree must have roots. While purely *phenetic* classification can be useful, it can also too easily morph into an empty technical exercise. Population genetics has handed us our roots on a silver platter and it would, in my opinion, be a mistake to ignore them. Yet the authors, leery of anything smacking of “monogenesis,” have hardly anything at all to say regarding either roots or the revolution in our understanding of human history precipitated by population genetics. Another quibble is the lack of any reference at all to the fascinating question of archaic cultural survivals, a conspicuous absence, based perhaps on an overly confident reliance on that good old standby, “convergent evolution.” This is not the place for me to sound off on so contentious a topic. All I’ll say for now is that in my opinion it is a mistake to rely too heavily on convergent evolution, to the point that archaic cultural survivals are completely ignored.
- Finally, I want to take this opportunity to defend myself from the accusation implied in the following statement, included under the heading “Origins”:

For example, while there may be truth to Grauer’s (2006) theory that musical similarities between Pygmies and Bushmen reflect ancient musical origins, testing such a theory requires sophisticated phylogenetic models that incorporate

detailed cross-disciplinary information about both past and present diversity wherever possible. It cannot simply be based on assumptions of either biological or cultural “inertia” (Stock 2006; Leroi and Swire 2006). (180)

I leave it to others to decide whether or not my phylogenetic models are sufficiently “sophisticated.” But I must take strong exception to the implication that my writings lack “detailed cross-disciplinary information about both past and present diversity.” Nor can I accept the allegation that my concept of “cultural inertia,” as I have articulated it in various forms over several publications, is in any way based on assumptions. I have in fact gone to a lot of trouble over the years to base my theories on the careful sifting and evaluation of evidence and have very deliberately avoided assumptions of any kind. The commonly held notion that the “hunter-gatherers” of today represent our early ancestors is an assumption. The notion that music is continually subject to change, as is now so widely accepted among ethnomusicologists, is an assumption. My notion of cultural inertia is *not* an assumption, but a testable (and already much tested) hypothesis, based on very thorough, painstaking, evidence-based research carried out over a great many years. That does not mean it has to be so. I could be wrong, which is why I so often use terms such as “hypothetical” and “provisional.” As for the references to critiques of my work by Jonathan Stock and Armand LeRoi, I refer skeptical readers to my detailed response to Stock, in the same publication cited above (Grauer 2006b). I did not respond to LeRoi’s comments in a subsequent volume, as I felt I had already defended my work adequately in the earlier publication.

OH FREUNDE, NICHT DIESE TÖNE!

I do not want to conclude on a negative note (no pun intended, Ludwig), because in fact I very much approve of what my respected colleagues have accomplished overall. As I see it, their insights and thoroughness more than compensate for any of the reservations and/or disagreements I have expressed. It is important to raise questions concerning certain details one finds questionable, but we must also respect the overall scope of what has been expressed here, which is considerable. Savage and Brown make a compelling case for the revival of comparative musicology and I most certainly want to second that motion.

I shall conclude by quoting some of the things in their paper that resonate most strongly with my own views:

[I]t is difficult to celebrate the world’s musical diversity or argue for the need to preserve endangered cultural heritages without placing music cultures in their broader historical and geographic context. (186)

Avoiding the perceived reductionism of comparative analyses may help to avoid an oversimplification of musical complexities, but it also makes it harder to convince the public that music cultures are worthy of study, archiving, funding, or political

recognition. (186)

[C]ontemporary ethnomusicologists have accumulated millions of recordings and associated documentation from all over the world but generally lack theories and methods to synthesize these data. (187)

Much could be gained if ethnomusicologists reincorporated cross-cultural comparison, scientific methodology, and contemporary evolutionary theory into their research program and thereby returned to the big-picture questions of comparative musicology that we have described in this article. (187)

Amen.

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