



Comment on John Roeder's article

Stephen Smoliar



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[1] As a computer scientist I have to confess that it is often difficult for me to stifle a choke whenever I confront the word “semiotics.” While I have the greatest respect for the heritage of the discipline, I also feel it is important to recognize that its pioneers were restricted by the intellectual equivalents of stone axes. When they worked with symbols and codes, the only tools they had were their own pencils and papers. That they were able to achieve as much as they did is admirable, but it more than a little tragic to think that there are still those who are blind to the limitations of those tools.

[2] It did not take long for the computer pioneers to recognize that their machines could manipulate symbols just as easily as numbers. (Turing was certainly well aware of this, although I am not sure I would credit him as being the first to have the insight.) Even when John von Neumann wrote his first computer program (documented by Donald E. Knuth in Volume 2, Number 4 of *Computing Surveys*) he realized that it was easier to work with symbols than “raw” binary code. The need for computer programs which would process such symbolic codes was a very early insight which was quickly followed by the recognition that any such program would have to be based on models of the syntax and semantics of those codes.

[3] One might argue that work on machine translation of *natural* languages got off to a false start due to *ignorance* of semiotics—particularly the insight that there was more to language than syntax and semantics. Nevertheless, we eventually dug ourselves out of that hole. What is more important today is that we now have very powerful systems at our disposal which allow us to manipulate symbol structures; and I find it a bit difficult to wade through much of the arcane language of the semiotic theorists when it seems to me as if it could all be articulated so much easier in LISP. (This is not to say that LISP solves all problems. Rather, it often helps us to formulate clearer questions, even when answering those questions turns out to be very difficult.)

[4] Having gotten all that off my chest, I would like to provide my own take on what “Toward a Semiotic Evaluation of Music Analysis” is really all about. From where I sit (one hand on my Macintosh keyboard and the other at the piano), the key problem of music analysis is one of *description*; and the key problem of description is that it cannot be readily reduced to a simple exchange of symbolic codes. Put in the bluntest and most obvious way, a description of a music experience is not the same thing as a description of an algorithm. When I need to describe an algorithm to my computer, I know that I have to respect certain *a priori* conventions assumed by the machine as to how I use my symbols to communicate. When I need to

describe a music experience, I cannot always assume that those conventions exist, let alone identify what they are.

[5] The task of description, as it applies to music analysis, is founded on two agents: the agent giving the description and the agent receiving it. Those two agents are rarely “of the same mind,” so to speak. (They certainly do not have identical mental states, no matter what particular philosophy of mental state you happen to subscribe to.) Therefore, description is not so much a matter of the describing agent passing a code to the receiving agent as it is a matter of the two agents mutually negotiating towards a point where they have some confidence that they are both talking about the same thing.

[6] Within a community of experts (such as those who haunt meetings of the Society for Music Theory, for example), negotiation can often be minimized. Music theorists constitute what Donald Schoen calls a “community of practice,” within which *some a priori* conventions of communication can be assumed. However, since most music theorists are interested in talking about new things, it is often the case that novelty goes beyond the limits of those conventions; so even within a well-defined community of practice, the need for negotiated communication is always present.

[7] From this point of view, Roeder offers a key insight. However, it is more an insight about negotiation than about codes. Mathematics, graphical representations, and narrative are not codes which mediate our analyses. Rather, they are our negotiating tools: different resources upon which we can draw when, in undertaking the task of description, we have to confront the problem of whether or not the receiving agent is really “getting it.” Furthermore, if that receiving agent is not giving us any feedback (which is what happens when we are writing a paper rather than engaging in face-to-face dialog), then we might do well to consider how we may effect a *synthesis* which brings all three of those tools to bear.

[8] I am currently reviewing David Lewin’s new book, *Musical Form and Transformation* for *Computer Music Journal*; and I am trying to argue that Lewin has succeeded in such a synthesis in the four analyses in this book. I mention this because I think one of the most important moments in Lewin’s *Music Perception* paper, “Music Theory, Phenomenology, and Modes of Perception,” is when he introduces the concept of “music behavior.” The descriptive act of music analysis is yet another instance of that behavior. As such, we should think about it in terms of its behavioral implications rather than trying to reduce it to an exchange of codes.

Stephen Smoliar
National University of Singapore
Institute of Systems Science
Heng Mui Keng Terrace
Kent Ridge, SINGAPORE 1027
smoliar@iss.nus.sg

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