

Mix levels of analysis with care; genres not at all.

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From my reading, Tangherlini's chapter gets most things right. However, the chapter has two fundamental problems. When I was writing my book (Rubin 1995), I was lucky to be able to gain the ear of Albert Lord. One of the first things he said to me, once we got past the idea that memory was not all rote memorization, was, "Don't mix your genres." I repeated that mantra throughout my studies and it helped me prevent many potential blunders; I even used it as a quote to start a chapter. The first problem is mixing non-rhythmic, more literate genres with the rhythmic, more oral genres that I selected. As the chapter notes, expertise makes a difference; but expertise is different in different genres.

The chapter notes correctly that I describe and try to explain from the viewpoint of a cognitive psychologist only three genres, all of them rhythmic. I focused on these genres because the role of writing or other recording devices was minimal and usually viewed as a problem when it was used by the people I studied and by my most reliable sources. I wanted to look at the role of memory in the transmission of oral traditions and needed to minimize external memory aids, such as writing, that could act as prostheses to hide the limitations of memory. I could not see how memory shaped the oral traditions if the traditions were not kept in memory. It turned out that in the three traditions I studied, as in many other oral traditions, there was little formal training. The expert singer sang and the novice singer listened. The novice then practiced without an audience, and then perhaps with one or sang the song back to the expert, who might say something like, "That is not the way I have heard it." The expert did not know the rules of the genre in a form that could be summarized and presented to a novice; the rules were simply followed. Lord (1960) makes this point repeatedly for his singers. I made it in my book and elsewhere (Rubin 1988).

Psychologists contrast contingency learning with rule-bound learning (for a discussion of differences in expertise see Rubin, Wallace, and Houston 1993). When rules are taught explicitly, learning is much more efficient and subject to conscious reflection of the kind that the chapter claims Arlo Guthrie uses. If one knows what a page of music looks like, and can have a discussion about poetic devices and music theory that benefits from a millennium of human intellectual activity, then one approaches music differently and one's expertise includes much that is not included in the expertise of someone who learns by observation and trial and error. The two kinds of learning lead people to produce different kinds of songs in different ways. In a study of the very early beginnings of expertise, we had extremely literate undergraduates learn five similar oral tradition ballads by simply listening to them without any formal teaching of the ballad form (Rubin, Wallace, and Houston 1993). They learned each successive ballad better over the course of the five ballads, following more of the regularities of the form and content. At the end, they composed a new ballad that was supposed to be indistinguishable from the five ballads they had learned and then to try to state the explicit rules that the ballads followed. They

stated, but did not follow, some rules including, "the protagonist dies." They followed, but did not state, rules such as "ballads have no explicit settings," "ballads are composed of mostly one and two syllable words," "nouns in ballads are concrete and easy to visualize, rather than abstract." Their literate college training led them to notice explicitly one kind of regularity; their observational learning led them to produce another.

How did Arlo Guthrie get to the point where he sang Alice's Restaurant? It is doubtful from the information in "The Official Oughtabiography of Arlo Guthrie" (<http://www.arlo.net/bio.shtml>) that it was from pure observation without any more active teaching. The line from Alice's Restaurant quoted in Tangherlini's chapter, "So we'll wait for it to come around on the guitar, here and sing it when it does" is probably not a spontaneous creation of an oral tradition singer of tales. Guthrie registered his ownership of the entire talking blues monologue, including that line, in addition to the sung portion of Alice's Restaurant (<http://www.arlo.net/resources/lyrics/alices.shtml>).

Moreover, the performance from which the recording of the monologue came would hardly be the time to make up such a long monologue without prior practice.. Arlo Guthrie wanted to involve a large audience at Newport, including me, and so he did have to keep playing until it came around or he would have violated genre and performance expectations in a way he could not, even if he had the ability to skip ahead. Imagine the jarring effect on the audience if he had jumped from where he was playing to the note he needed to start singing instead of continuing to play until it came around. Even a classical musician "craftsman" soloist, who could start reading the score at any note, returns to the beginning of a movement if a string breaks. The next time Arlo Guthrie wants the audience to join in, he adds the accurate, humorous, and copyrighted, "We're just waitin' for it to come around is what we're doing." Such are the talking blues. I will let pass why the audience laughed, but there are more possible reasons than the one given in the chapter.

Arlo Guthrie was, and is, a master of the genres in which he works and these depend, in part, on text that is documented and revised by use of external memory aids, such as audio recordings, and written notation. Musical notation, recording, and writing can play a major role in some genres. Perhaps Tangherlini's figure of levels of expertise may be correct for these genres: I remind the reader of Lord's cautionary "Please don't mix genres." I have every reason to assume that the British singer with whom I spoke, who was brought to Duke University for a British-American festival, was an expert and a "craftsman" and that what he said was the actual truth. Yet he maintained that it was necessary for him to wait until the chorus came around in order to sing the words. When Bruce Kapferer asked one of his expert performers to inform him about a demon and the singer had to sing to the part where the demon was mentioned, we had no reason to doubt him (Rubin 1995, 190). In these oral traditions, experts need a running start. As such, the figure in the chapter will need a different size box for "cue-item discriminability very important" for different genres.

The second problem in Tangherlini's chapter is more serious. When I started studying oral traditions, I was a young cognitive psychologist with a good working knowledge of the brain as well as of behavior. This knowledge made its way into the organization of my book. The topics of narrative (or theme), language in the form of poetics, and visual imagery are in separate

chapters, and object and spatial imagery are separated within the visual imagery chapter. Each of the behavioral systems has its own neural system that had been mapped out in terms of anatomy and that we have long known could be damaged separately. As I reviewed in my book, students of oral traditions had made most of these distinctions without help from psychologists or neurologists. However, I found that the distinction between spatial and visual imagery and the idea that narrative could exist without most of what we call language, were novel ideas to many scholars in the humanities. Although most humanists studying oral traditions view visual imagery as a single system, spatial location (the "where system") and object recognition (the "what system") are considered as separate based on behavior studies, on neuropsychological damage studies, and on neuroimaging studies (Rubin 1995, 2006). Similarly, there is good evidence to consider narrative as a mode of thought (Bruner 1986) that need not depend on language, that can be used without language, as in mime and cartoons, and that has a different neural location that can be damaged separately (Rubin 2006; Rubin and Greenberg 2003).

In the book, I downplayed the neural basis of behavior and concentrated on the behavioral level that I felt then and still feel now is the most relevant for students of oral tradition. It is not because researchers viewed the brain as a black box, as the chapter laments; it is a question of determining the most appropriate level of analysis. What I wanted to explain was stability and change in oral traditions. I wanted the clearest theory that could do that. Although I was informed by what was known about the brain, it did not make the theory more precise to try to reduce it to underlying neural mechanisms. To use a concrete example, I knew from anatomy, neuropsychological damage studies, and neuroimaging studies that two behavioral systems important to oral tradition, visual imagery and language, were located in different neural systems. We have known they were separate systems with different properties at the behavioral level since the time of the ancient Greeks. However, labeling the brain locations involved in each system, or the use of any of my other knowledge at the neural level, did not tell me anything important enough to put into the book about how oral tradition is transmitted. Later, I went into more detail about the brain-behavior analysis of the basic systems of episodic memory on which oral traditions draw (Rubin 2006)—but this still did not add anything that would tell me about stability and change in oral traditions. As is often the case, what is known about the brain has no implications for the kind of theoretical distinctions the author wants to make about behavior.

As a counterpoint to Tangherlini's claims, I wish to examine some of the more detailed critiques of my work that his chapter brought forth and in particular show the difficulties of moving from neural to behavioral theories. The chapter contrasts the Standard Theory with the Multiple Trace Theory. The Standard Theory is not so standard as it is presented to be in the chapter and the Multiple Trace Theory is not so different from it. There is general agreement in the field that the hippocampus binds information in many other parts of the brain at the moment of encoding. There is some disagreement as to whether later declarative recall requires the hippocampus. However, this is not a major issue for most behavioral studies of memory and has absolutely no implications, as far as I can tell, for behavioral theories at the level I presented them in my book or that are used in the chapter. The key question here is what we could learn about oral traditions if we knew from the Multiple Trace Theory that cues arriving from sensory, language, and emotion areas of the brain had to pass through the hippocampus before activating networks or associations in other parts of the brain that form a memory. How would that be different from what we would learn if the areas interacted among themselves without involving the

hippocampus, which would be the neural alternative? That is, would this information affect what we know about theories of performance, or in any way restrict the range of possible behaviors in intact human beings? I can think of none, given our current level of knowledge. Even when we wrote papers on the catastrophic effects on memory of brain damage that removed visual memory abilities, there was no need to enter this debate (Greenberg, Eacott, Brechin, and Rubin 2005; Greenberg and Rubin 2003; Rubin and Greenberg 1998). Contrary to what the chapter implies, the key role of multiple cuing and cue-item distinctiveness as developed in my book would rely on information in multiple areas of the brain if the brain were involved in an explanatory role. That is one reason why there were separate sections on narrative, language, visual imagery, and spatial imagery. However, for the book, naming multiple systems of the mind sufficed without specifying in detail the neural basis of each of these systems in the brain. The discussion of Hintzman's instance model (1986) in my book is of a computer model of behavior that is as close to the neural level Multiple Trace Theory as one could get, and I considered it as one possible way to implement my theory on a computer. The model hypothesizes multiple traces, just as the neural model does, and makes predictions about behavior based on the multiple trace—it simply remains silent on where in the nervous system the traces can be found.

Thus, even when Tangherlini's chapter is right about the neural basis of behavior, what it brings from the brain makes no difference in considering how one would describe and explain behavior. What we know about the brain can inform our theories of behavior, but making this connection is not always easy. To put this most simply, one should always try to make use of all levels of analysis, including the cultural, psychological, and neural levels. However, in the case of oral traditions, or of Arlo Guthrie's singing, using the Multiple Trace Theory at the neural level adds no useful information.

What do we know about the relation of brain and behavior that would be applicable to oral traditions and how has it changed in the decade since my book was published? I think it is safe to say nothing basic has been contradicted, but that the rise of structural and functional neuroimaging has offered a great deal of new information. From structural neuroimaging, we now know that the brain changes in relation to expertise; further, we can now measure that change in some detail. From functional imaging, we know which areas of the brain are most active in various tasks. For instance, from my own work on autobiographical memory we know that when people have to judge whether a picture is one they took themselves rather than one they saw in the laboratory, they utilize more areas involved in spatial processing, in self-referential processing, and in recollection, including the hippocampus (Cabeza, Prince, Daselaar, Greenberg, Budde, Dolcos, LaBar, and Rubin 2004). We also know that in the course of recalling an autobiographical memory, the hippocampus is involved early in the search process but becomes less active as the memory is retrieved and visual areas become more active (Daselaar, Rice, Greenberg, Cabeza, LaBar, and Rubin 2008). Neural imaging work is expensive and those who provide the funding are generally more interested in practical problems of health than in oral literature, so the experiments available for review do not investigate oral traditions. Thus, it is hard to recommend any summaries integrating the neural findings directly with work in oral traditions, though I have made some attempts (Rubin 2006).

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