

## Birdwhistell, Hall, Lomax and the “Origins” of Visual Anthropology: A Commentary<sup>1</sup>

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These comments were prepared for discussion in response to papers presented by John Bishop, Martha Davies, Alison Jablonko, and Malcolm Collier, at the Origins of Visual Anthropology Conference, Göttingen, Germany June 21-25, 2001. I call for an appreciative but rigorous, critical analysis of these three pioneering figures in the anthropology of human movement and visual aspects of human communication. I maintain that their theoretical and methodological contributions require historical contextualization, taking into account earlier paradigms, as well as subsequent developments and alternative approaches. The paper makes a number of suggestions for developing a more satisfying historical framework and raises topics for further discussion.

Anthropological interest in visual aspects of human communication and body movement considerably predates the work of Ray Birdwhistell, Edward T. Hall and Alan Lomax in the 1960s and 70s. I confess, therefore, to being somewhat surprised that their contributions—and theirs alone—although certainly pioneering, would be identified as constituting the “origins” of work on body movement within visual anthropology. At the same time, although the authors of the papers for this conference session (Bishop 2001, Collier 2001, Davies 2001, Jablonko 2001) provide us with appreciative descriptions of the three approaches, along with interesting historical detail regarding the professional alliances and politics of the time, largely absent from the discussion so far is any rigorous critical analysis that contextualizes their innovations *vis a vis* earlier theoretical paradigms or alternative and subsequent developments. Without such contextualizations we are unable accurately to situate Birdwhistell’s ‘kinesics’, Hall’s ‘proxemics’ and Lomax’s ‘choreometrics’ theoretically and methodologically within the historical development of the field. Nor can we understand why their import for contemporary students is largely historical, despite the excitement surrounding these approaches when they were produced.

For example, of paramount importance to more recent alternative approaches has been the paradigmatic shift away from the positivist conception of science with its objectivism and empiricism that was employed by Birdwhistell, Chapple, Lomax, Sheflen, Arensburg and others in the informal Philadelphia-New York network that Davis describes. This paradigm shift entails a move away from an observationist view of ‘behavior’ to a conception of body movement as ‘dynamically embodied action’. This important turn to an *agent-centered* perspective is well encapsulated in the preferred use of the term ‘action’ over ‘behavior’ central to Williams’s semasiological approach, for example (1991: 244-276 and Ardener 1973).

Approaches within socio-cultural anthropology (but not psychology) today no longer define human movement as “physical behavior,” “motor habits” or even as “non-verbal” but as “culturally and semantically laden *actions* couched in indigenous models of organization and meaning” (Williams 1982: 15). In retrospect, we can see that the linguistically and semiotically inspired approaches to the study of human movement (including dances) that were developed by Kaeppeler in 1967 and Williams in 1975 have proved more fruitful to later generations than kinesics, proxemics or choreometrics.

These alternative perspectives define embodied persons as primarily *meaning-making* creatures, a move consistent with general shifts towards symbolic and semantic concerns in socio-cultural anthropology generally, influenced by the 'linguistic turn' in philosophy and social theory (see Crick 1975, Rorty 1992). Grounded in different ontological and epistemological positions, the newer point of departure is an entirely different definition of what it means to be human. In contrast to earlier behaviorist and observationist views, bodily actions, while not necessarily part of an actor's focal awareness, are *not* separated from the cognitive capacities of a language-using, symbol manipulating human mind. In semasiology, for example, a non-Cartesian concept of person is coupled with a new-realist, post-positivist philosophy of science. As a result 'vocal signs' and 'action signs' become the primary means by which humans exercise their agency in dynamically embodied shared cultural practices of all kinds (see Farnell 1999, 2000).

It is interesting to note in passing that different disciplinary perspectives as well as theoretical orientations within a discipline will claim different ancestral lineages. While Davis correctly reminds us that the study of bodily expression in Western thought can be traced back at least as far as Greek and Roman treatises on gesture in oratory, she identifies Charles Darwin's, "The Expression of Emotions in Man and Animals" (1872) as having ushered in "modern scientific study." This may indeed be true of psychological approaches, and of Birdwhistell's kinesics in so far as he worked in clinical contexts, but it cannot be said of the aforementioned alternative approaches within socio-cultural anthropology, i.e. Kaeppler's use of Pikean emic/etics and ethnoscience (1967) or Williams's semasiological theory (1975). It is not the case, therefore, that Darwin's work *remains* a touchstone for anthropologists of human movement. On the contrary, lingering evolutionary, ethological, universalist and psychologistic assumptions continue to be problematic encumbrances for many contemporary investigators:

... investigators still fall prone to the Cartesian dualistic trap of assuming that human actions, being of "the body" are somehow separate from the cognitive capacities of a language-using, symbol manipulating, human mind, and so are more "natural" "primitive," "spontaneous" or even "instinctive," as if these higher faculties don't apply to our actions or our conceptions of those actions. Human body movements, as Mauss (1935) also observed, are necessarily biologically enabled but are everywhere subject to the transformative power of human psycho-social realms of meaning, including language (Farnell 1999: 358).

Recent research by Ingold (1993) and Keller and Keller (1996) illustrates that this is just as true of activities such as tool use as it is of actions that fulfill expressive and communicative functions. Although phylogenetically speaking, we can observe some rudiments of human expression in non-human primate behaviors, it is an observationist fallacy to assume that what looks the same *means* the same in human and non-human domains. Whereas crucial differences between non-human vocal calls and human spoken languages seem clear in this regard, human 'action sign systems' have frequently not been afforded the same distinctions as a direct result of this fallacy. Even Hall's proxemics was prone to a residual evolutionism, although his own data suggest that the rich diversity of culturally defined human spaces make trivial any comparison with notions of programmed responses to 'critical distancing' and 'territoriality' in other animals.

That facial gestures such as ‘smiles’ or ‘grimaces’ could be universally understood was a misconception based on the observationist fallacy. As Davis notes, Birdwhistell objected to the naive universalism that often accompanied such evolutionary assumptions of functional continuity, exemplified by Ekman’s psychological search for universal emotions (e.g. Ekman et. al. 1972). However, Birdwhistell was not the first: Weston La Barre made a strong case for the cultural basis of emotions and gestures as early as 1947 in a paper rich with ethnographic examples. Despite Birdwhistell’s strong support for a relativist position, his discourse is frequently sprinkled with lingering ethological and behaviorist assumptions, as when he talks of smiles in terms of “readiness for aggression” and tension reduction “to avoid hostility and attack” (1970: 34) in the passage cited by Davis. Nor are the differences merely cross-cultural in a simple sense:

All peoples in the world may “smile,” but in English the word refers to much more than an observable movement—a ‘behavior’ of the facial muscles—and it does not have a one-to-one correspondence with a single [meaning]. For example, even within Euro-American culture one can smile not only out of “happiness” or “pleasure” but also out of embarrassment, when at a momentary loss for words, when putting on a “brave face,” or when lying or trying to deceive someone about malicious intent. What is meant by “smiling” when considered as an action as opposed to a ‘behavior’ must be determined by local norms of interaction and specific contexts of use. Ironically perhaps, what makes physical movements of the body ‘actions’ in the human realm is not, in fact, visible. Actions are defined by the varied and complex non-observable conceptual resources that are part of them. Actions, then, in contrast to ‘behaviors’ cannot be understood from observation alone [Farnell 1995; Williams 1991: 212-3] (Farnell 1999: 359).

This last point encapsulates a second important principle for contemporary investigators of human movement, and it raises the important question as to whether socio-cultural investigations into human movement are best served under the rubric of a *visual* anthropology if this is narrowly defined as a commitment to an observationist view and an outdated, positivist conception of objectivity.

### Developing a Historical Framework

If we agree that we might better locate the origins of anthropological interest in body movement prior to the 1960s, how might we construct a deeper history for the field? I suggest that a reasonable (albeit conventional) choice would be the late nineteenth century during the discipline’s formative period. On both sides of the Atlantic at this time, it was the evolutionist search for the origins of language that motivated interest in body movement. For example, the Victorian English anthropologist, E. B. Tylor regarded sign languages and gesture as components of a universal “gesture language,” more primitive than speech or writing, and he expected the elements to be universally recognizable (1865). Tylor believed he was close to discovering the original sign-making faculty in humans that once led to the emergence of spoken language. Meanwhile, in the United States, Tylor’s work provided theoretical support for Mallery’s extensive collection of data on sign languages and gesture. Mallery compared Native American signing systems with deaf sign languages and provided accounts of the use of gesture in classical Greece and Rome, in Naples, and among contemporary Euro-American theatrical actors (Mallery 1881). The first publications of the newly established Bureau of American Ethnology in Washington DC were entirely devoted to accounts of Mallery’s

research on the subject (1980a,b, 1881). On both sides of the Atlantic, however, this focus quickly disappeared once the social evolutionary paradigm and the fascination with “origins” waned. Nevertheless, interest in tool use and gesture has continued to play a significant role in accounts of the evolution of human intelligence (see Gibson and Ingold 1993).

In addition, between the latter half of the 19<sup>th</sup> century and the 1960s there was considerable interest from a number of scholars in the subject of dancing. Drid Williams has examined this literature in her book *Ten Lectures on Theories of the Dance* (1991, 2<sup>nd</sup> edition 2004). She shows how explanatory paradigms of the time provided a wide variety of interpretations and explanations of dances, dancing and the dance [in general] in terms of emotional, psychologistic, biological, intellectualist, literary, religious, and quasi-religious theories, as well as functional explanations. Lomax’s choreometrics joins this literature as a somewhat extreme and late example of a functionalist approach at a time when it had been largely abandoned as a viable paradigm within mainstream socio-cultural anthropology (Williams 1991: 19-117). Laden with unexamined presuppositions and pseudo-theories that presuppose various cause-effect relations for which there is little or no evidence whatsoever, Williams cautions us that these approaches provide important examples of how *not* to handle the subject of dancing.

As we might expect, in typical contrast to the universalist theories of “gesture” espoused by the evolutionists, Franz Boas stressed instead the learned, culture-specific nature of body movement. He recognized that artistic form and cultural patterning were present not only in Native American dances, but also in the complex hand gestures and other body movements that accompanied song, oratory, and the performance of oral literature (Boas 1890, 1927, 1972 [1944]; see also Kaeppeler 1978: 33; Williams 1991: 88-89). In the first volume of the *Journal of American Folklore*, which appeared in 1888, Boas published an article, “On Certain Songs and Dances of the Kwakiutl of British Columbia” and his interest in Kwakiutl dances continued throughout his life.

Despite this abiding interest, Boas chose to exclude “gesture-language” from his influential introduction to the *Handbook of American Indian Languages* (1911). Aligning body movement with “musical means of communication,” he limited his consideration to “communication by groups of sounds produced by the articulating organs [of mouth and tongue]” (1911: 10). Boas thus inadvertently set the pattern for the exclusion of body movement from future research in American linguistic anthropology. For example, although Boas’s student Edward Sapir recognized that manual gestures interplay constantly with speech in communicative situations, the linguistic and social significance of what he referred to as an “elaborate and secret code” was left unexplored (Sapir 1949: 556). Likewise, Benjamin Lee Whorf (1956) made programmatic suggestions about spatialized metaphors in speech and gesture but his statements appear to have gone unnoticed (but see Farnell 1996).

It occurs to me that Birdwhistell, Hall and Lomax have perhaps been identified for discussion here, in part, at least, because they all used *filmed* data as the basis for their analyses of body movement and communication. This raises the issue of whether visual anthropology has been defined more by its methods of documentation (i.e. film, video, photographs) than the ethnographic phenomena it purports to

investigate. In any case, as Davis points out, Birdwhistell, Hall and Lomax were not the first to generate filmed data for such analysis.

There are further interesting points made in the paper by Jay Ruby that Davis cites, which are relevant to the loosely historical narrative I seem to be building here. It was in 1930, when Boas was 70 years old, that he returned to the site of his earlier studies among the Kwakiutl with a motion picture camera and wax cylinder sound recording machine. He used the camera to generate data in natural settings (as opposed to a laboratory) in order to study gestures, dances, games, and methods of manufacturing as manifestations of culture (Ruby 1980: 1). Boas asked his daughter Franziska to study the Kwakiutl dance footage and she did so, supplementing her observations with ethnographic material from her father's earlier work (Boas 1897). Part of this material was published as a discussion following an article by Franz Boas in a volume entitled, *The Function of Dance in Human Society* edited by Franziska (Boas 1944).

Much earlier than this, in *Primitive Art* (1927), Boas articulated a theory of dance as emotional and symbolic expression; a component of his theory of rhythm in art and culture. Ruby (1980) suggests that Boas was trying to overcome the prejudice of some scholars that the dance and other arts of body movement were not fit subjects for scientific investigation since they were so "emotional" in content. Although Boas certainly saw dances as emotional and aesthetic outlets for the dancers, his interest was not in the individual so much as the social—of the dance as an expression of culture. For Boas, Ruby suggests, body movement of any kind was a means of signifying one's cultural identity, and, as such, should be amenable to ethnographic description and analysis (Ruby 1980).

Historical evidence suggests that Boas was interested in using the films and sound recordings for a study of rhythm but that he could find no suitable method of analysis. Two letters written in the field to Ruth Benedict are particularly revealing and relevant here: on November 13, 1930, he wrote "Julia [his field assistant] danced last night with the crowd and has her first formal dancing lesson tonight ... the dance problem is difficult. I hope that the films will give us adequate material for making a real study." On November 24, 1930 he wrote, "I already have a good deal of materials for this style-motor question." On the same day, Boas wrote to his son, Ernst, "Julia is learning the dance, but I believe it is too difficult to learn quickly. At any rate, *through the criticism she receives I learn what it is all about.*" (Rohner 1969: 293-4 cited in Ruby 1980—italics added). Clearly, Boas understood two criteria that today we take for granted as necessary to good field research in the anthropology of human movement. First, that learning the action sign system under investigation from local skilled practitioners is essential, and just as important as learning the spoken language of a community. Second, that critical remarks from such practitioners provide important means for understanding such things as indigenous concepts of the body, space and time, as well as criteria for adequate performance.

Since Boas had gathered written descriptive data on Kwakiutl dances since 1888, it is pertinent to ask why he thought the new filmed data could provide him with "adequate material for making a real study." Ruby notes that Franziska Boas provides us with a tantalizing possibility. In a personal communication she suggested that Boas filmed because he had heard of Laban's work and "wanted to know

whether Laban Notation [sic] was being expanded for wider use than just for [Western theatrical] dance, but I did not know enough about it to make use of it myself. His pattern was to investigate any new channels that might be fruitful. He very probably would have used Laban Notation had he lived later into the 1940's." If her conjecture is accurate, Boas was not only among the earliest researchers to use a camera with a view to using filmed data for detailed analysis, he also recognized the analytic possibilities that a transcription system like Labanotation offered the anthropological study of body movement.<sup>2</sup>

Boas also fought against the popular misuse of race as an explanatory device for human social differences. He was a fervent opponent of racial explanations of behavior and sought to establish the primacy of culture over race as a means of understanding social behavior. This interest took on a particular urgency in the 1930s when racism in America and Nazism in Europe were powerful forces. When Nazi social scientists began to publish their alleged "scientific" explanations for the racial inferiority of non-Aryans, Boas had an additional reason for advocating the primacy of culture for understanding human differences. As Davis describes, Boas combined his need to dispute the racists with his interest in gesture and motor habits in the work he directed by one of his last students, David Efron (1942).

Boas had also encouraged earlier students to pay attention to body movement. While in the field, Margaret Mead wrote to Boas on March 29, 1938 saying, "When I said I was going to Bali, you said: 'If I were going to Bali I would study gesture'" (Mead 1977: 212 cited in Ruby 1980). However, by the time Mead and Bateson returned from the field Boas was apparently too frail to see or discuss their work with them. Whether Boas, Mead and Efron, ever spent time discussing their mutual interests is unknown, but clearly Columbia University was a place where ideas about how to study body movement were circulating in the 1930s as well as in the 1960s.

Boas's students contributed to a functionalist view of human movement systems. Mead (1959[1928]), for example, regarded the dances of Samoan adolescents as a vehicle for psychological adjustment; for Benedict (1934) the function of the entire Kwakiutl Winter Ceremonial (a series of religious rites) was to rehabilitate the individual back into secular society. In addition to the essay by her father, Franziska Boas's edited collection contained essays on the functions of dance in Haiti, Bali, and "primitive" African communities. Unfortunately, actual body movement remains epiphenomenal in such descriptions because ritual actions and dancing are described in terms of adaptive responses either to the social, the psychological or the physical environment (Williams 1991: 119). Similar descriptions also appear in the work of many British functionalist anthropologists (e.g. Firth 1965 [1936]; Malinowski 1922; Radcliffe-Brown 1964 [1913]). Lomax's brand of functionalism differs from this in the sense that he did at least attempt to deal with the movement itself. The problem was that he removed arbitrary fragments of movement from the very social and linguistic contexts that gave it meaning.

The unprecedented, seminal, essay of French anthropologist Mauss (first published in 1935) prefigured the interests of Benedict, Mead and others in noting how each society imposes on the individual a rigorously determined use of the body during the training of a child's bodily needs and activities. Mauss's essay clearly illustrated how seemingly "natural" bodily activities were (Durkheimian) social facts that were simultaneously sociological, historical and physio-psychological.

Continuing our loose chronology: in the 1940s and 1950s we see the potential importance to anthropologists of recording and analyzing body movements being demonstrated by the photographic analysis of Balinese character by Mead and Bateson (1942); the contrastive analysis of the gestures of Italian and Southeastern European Jewish immigrants in New York by Efron (1942); La Barre's essay on the cultural basis of emotions and gestures (1947), and a paper on the cross-cultural comparison of "postural habits" by Hewes (1955).

By the 1960s, Goffman's influential micro-sociological studies of social interaction included attention to the agentic management of bodily performances in the presentation of self. Goffman introduced the term "body idiom" to describe the socially constructed knowledge found in conventionalized vocabularies of gestures and postures as well as the corporeal rules important to understanding behavior in public, thereby prefiguring Williams's "action sign systems" (1975) in some ways. However, Goffman did not systematically explore this notion nor was he concerned with providing an explicit theory of the body in society (Goffman 1963, 1969; see Shilling 1993: 74, 85-88).

It was at this point that Birdwhistell emerged as an early pioneer, coining the term 'kinesics' to describe his micro-analytic approach. In addition to being influenced by the work of Bateson (1958 [1936]) and Goffman (1963, 1969), Birdwhistell was inspired by what he viewed as Sapir's anticipation of the interdependence of linguistic and kinesic research (Sapir 1949), and by attempts on the part of others to apply the methods of structural linguistics to non-segmentable aspects of vocalization ('paralinguistics'). He envisioned a discipline that would parallel linguistics but deal with the analysis of visible bodily motion. Using filmed data, he applied a linguistic *model*, attempting to identify movement units based on contrastive analysis in a manner similar to that established by structural linguists for establishing the phonemes and morphemes of a spoken language. Unfortunately, he was without the theoretical means to specify how bodily movements could be made *finite* for analytic purposes. Minus the concepts of 'action sign' and 'action sign system' that would provide suitable units of movement and a concept of structured system (Williams 1975), Birdwhistell's analyses tended to dissolve into micro-analytical minutia from which he seemed unable to emerge.<sup>3</sup>

Davis's report illuminates some reasons for this, given the influence of behavioral psychologists such as Shelden on Birdwhistell's analytic attempts. Behavioral micro-analysis in laboratory and experimental settings asks different questions from those posed by anthropological investigations in ethnographic contexts. Birdwhistell was clearly aware of this, given his criticism of experimental controls and his call for "naturalistic observations" more in line with anthropological principles. However, Davis tells us that Ekman and the experimental psychologists managed to win control of the funding source from Birdwhistell when kinesics couldn't defend having spent four years on the micro-analysis of one film!

Birdwhistell's research, like Goffman's, was limited to interaction contexts, usually in clinical settings and he considered more formalized idioms such as dancing, drama, mime, and religious ritual to be beyond the interests of kinesics (1970: 181). This was unfortunate as it narrowed the scope of the potential field, separating kinesics from much that was of interest to mainstream anthropology.

Whereas 'kinesics' focused on body motion, Hall's 'proxemics' (1959, 1966, 1968) drew attention to the role that spatial relationships play in human affairs, although again, one would want to draw attention to two prior contributions: A. Irving Hallowell's 'Cultural Factors in Spatial Orientation' (1955) and Eingar Haugan's 'The Semantics of Icelandic Orientation' (1957). Hall postulated that there are socially established zones of space surrounding individuals that are generally out of awareness but that influence, and may even determine daily interactions (Hall 1959, 1966). Hall's writings include many thought-provoking ethnographic observations about the uses of spatial relationships in different contexts, including situations of cultural contact. As Collier notes, his applied interests and desire to engage a wider audience frequently led to over-generalizations, but his work was widely known and appreciated, sensitizing many non-specialists to the cultural construction of spatial relationships. His popular books remain 'a good place to start' with beginning students today.

'Kinesics' and 'proxemics' obviously provided important sensitizing constructs in the 1960s and 70s. They raised new questions, suggesting frameworks that could be advanced by later investigators. Problems arise in the two approaches, however, due to a separation of body motion and space. Kinesic motions of the body exist in a spatial vacuum, while proxemic zones of space are empty of the dynamically embodied action that structure their meaning. Today, we recognize that it is dynamically embodied action *within* structured semantic spaces that we wish to account for. In retrospect we can see that this separation was possible because both approaches take an *observationist* rather than an *agentic* perspective on action.

Unfortunately, I find Lomax's 'choreometrics' such a flawed project from its very conception, in so many ways, that it is impossible to locate any redeeming features—certainly nothing that would stimulate useful new directions for visual anthropology, as Jablonko enthusiastically suggests. I was especially disappointed that neither paper on Lomax's work took into account any of the critical reviews it received. Grave objections to this approach have been pointed out in painful detail by many well qualified critics, including Kealiinohomoku (1976, 1979), Hanna (1979) and Williams (1972, especially 1991: 139-150), all of which I strongly recommend.

Unfortunately, choreometrics represents the worst kind of abuse of statistical models. Its primary data are arbitrary fragments of filmed movement torn out of the social contexts that provide them with meaning. We know nothing of the danced events from which the stretches of movement are taken, nor what the movements might mean to the people dancing. On the contrary, we are told the intent is explicitly "not to translate." Completely unjustifiable assumptions follow, such as: "When we find analogous bits occurring with notable frequency in life activity outside the dance, we assume that the bit in the dance and the bit in life stand for each other."

In choreometric explanations, a misguided notion of objectivity assigns the dancers only physical identities, and the dances (whatever they were), are reduced to raw movements bereft of any semantic content or significance whatsoever. Statistical correlations are conflated with causation when motor complexity in one set of work activities connected with agricultural technology is assumed to be a constant factor in danced activities, implying a causal connection. Overblown claims are made that the



resultant movement profiles can capture “the characteristic stances and modes of using energy that underlie *all* social interaction, *all* work, *all* activity in a particular culture” (italics supplied) and that these will map out nothing less than continent sized movement style families. I find the idea of Lomax’s ‘global jukebox’ nothing more than butterfly collecting reminiscent of Victorian obsessions—a Middlemarchian Casaubon indeed!

Instead of allowing ourselves to be seduced by the objectivist pseudo-science of an approach like choreometrics, we need to reflect seriously upon Williams’s point that,

[T]his project has contributed (perhaps more than anyone is aware) to the stultification of further subsidized research on the dance and other structured movement systems. Having failed to produce a viable “measure of dance” or a reliable “theory of dance as the measure of culture,” policy makers and those who exercise control over research monies now seem to believe, owing to these failures, that there are simply too many variables connected, especially with the dance, and that it cannot be studied in a “scientific manner” or in a manner that would make a further contribution to knowledge (1991:141).

One can reasonably suppose that Williams experienced this kind of response to requests for funding in the aftermath of choreometrics during the late 70’s early 80’s.

In closing, let me emphasize that this commentary is in no way intended to be a definitive history of the field. I hope merely to have raised some topics for further discussions about theory, method and historical context that will prove fruitful to future developments within visual anthropology and its engagements with human movement.

#### Notes:

<sup>1</sup> Reprinted from *Visual Anthropology* Vol 16, Number 1, pp. 43-56.

<sup>2</sup> Labanotation, the script for writing body movement, was first used in anthropological contexts by Williams in 1975. It is also used by Kaeppler, myself and others for transcribing movement data of all kinds. It is quite distinct from Laban’s Effort/Shape analysis, a largely ethnocentric classification of dynamic movement qualities that was used in Lomax’s Choreometrics project, although space prevents me from discussing this further here.

<sup>3</sup> Birdwhistell also recognized the need for an adequate transcription system and tried to develop one. Absent the theoretical concepts mentioned and without any understanding of basic conceptual principles for organizing body movement such as those developed by Laban, his attempts at transcription were without systematicity and did not meet the criteria required for a true script. See Williams 1996; Williams and Farnell 1990.

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