SYMPOSIUM NO. 6: MOTOR CONTROL OF SPEECH GESTURES

(see vol. II, p. 315-371)

Moderator: James Lubker

Panelists: R.A.W. Bladon, R.G. Daniloff, Hajime Hirose, Peter F. MacNeilage, and Joseph Perkell

Chairperson: Leigh Lisker

JAMES LUBKER's INTRODUCTION

In preparing my introductory comments for this symposium I have made two assumptions: first, I am assuming that those of you in attendance are interested in speech production/motor control theory and have therefore taken the time to at least glance through the papers for this symposium as they were published in volume II; and secondly, I am assuming the goals of phonetics to be as described by Björn Lindblom in his plenary lecture (p. 3-18, this volume).

Acceptance of the first of these assumptions implies that I need not spend much time in summary of the papers in this symposium; they are there for the reading. Rather, I will take as my goal to provide a common framework for those papers and the points of view expressed in them, in order to allow the discussion of current and important issues in production/motor control theory.

Since acceptance of the second assumption will dictate the nature of the framework and issues which we will develop for discussion, it is perhaps wise for me to be somewhat more explicit about it. In the summary (vol. I, p. 3-4) Lindblom states: "Phoneticians accordingly construe their task of speech sound specification as a physiologically and psychologically realistic modeling of the entire chain of speech behavior." And he then goes on to pose the questions of (1) why it should not be possible for "phoneticians to extend their inquiry into the sounds of human speech to ever deeper physiological and psychological levels using speech as a window to the brain and mind of the learner, talker and listener?", and (2) "Why we should not expect more complete, theoretical models and computer simulations to be proposed for speech production, speech understanding and speech development that match the present quantitative theory of speech acoustics in rigor and explanatory adequacy?".

Indeed, the very title of this symposium, The Motor Control

of Speech Gestures, suggests research and theory devoted to an attempt to elucidate the rules and systems "at ever deeper physiological and psychological levels", by which man generates speech, and to do so with as much precision and scientific rigor as possible. Motor control research and theory must be integral to the goals stated by Lindblom, that is, to the development of explanans principles in phonetic and linguistic theory. Thus, the acceptance of those goals is my second assumption for this symposium.

There remains, however, much room for discussion since the search for precise and valid explanans principles for the generation of human speech is currently faced with several crucial issues, which are well illustrated by the papers presented in this symposium. Those issues can be discussed within three very broad and highly interrelated areas of theory and research.

In the first place, many questions in motor control/production research have quite naturally dealt with the form and function of the system or systems which operate to produce a speech acoustic signal. That is, a major effort in motor control research has been the attempt to discover the rules which explain and predict the transformations at the several interfaces in the chain of language generation and perception. Armed with such rules we would indeed have "a window to the brain". And since that is precisely where language resides, knowledge of these rule systems would provide us with a strong tool for the elucidation of certain aspects of language theory. Efforts to discover the rules have not, thus far at least, resulted in a Motor Control version of the Acoustic Theory of Speech Production, but as Lindblom suggests, there is no reason to believe that we will not one day have such a theory. Every paper in this symposium deals via proposed models, specific data or both with the form and content of such rule systems and it would thus seem obvious that this should be a fruitful area for discussion.

A second broad area of theory and research in the motor control of speech gestures is the precise form or nature of the units which serve as input to the motor control systems. In the papers of this symposium a number of possibilities are suggested: Abbs uses a matrix of phonetic features; in an updated version of their paper Daniloff and Tatham also suggest such a matrix. Bladon

considers several possibilities including features, phonemes and phonological syllables; Gay and Turvey seem to be viewing the input as phonemic; Perkell agrees that studies of motor control mechanisms are closely related to the nature of the "fundamental units underlying the programming of speech production", but he does not speculate in this paper as to what those units might be. Although the papers of Folkins, Hirose, and Sussman are concerned with specific experimentation with the functioning of the motor control systems, irrespective of the input unit, the nature of that unit would clearly seem to be a second broad area for useful discussion.

Finally, let me propose a third general area for discussion; an area which is so related and intertwined with the preceding two as to be virtually inseparable from them. It concerns more the form of attack upon the problems of the preceding two areas.

I have been implying that motor control rules of some kind are necessary in order to move from abstract linguistic concepts such as the phoneme or syllable to the concrete data obtained in speech production experimentation. These two sets of units, the abstract concepts of linguistics and the hard data of production research have never been very well matched and if they are to be used together in attempts to explain speech and language generation then transformation rules would, in fact, seem necessary. Fowler et al (1978) have called such efforts "Translation Theories" and they contend that virtually all production research to date may be classed as one or another type of translation theory. Fowler et al also suggest that all abstract linguistic units possess three properties: they are descrete, static, and context-free; while all units of production are dynamic, continuous and context-adjusted. A clear mis-match! Most of us would agree with Liberman and Studdert-Kennedy (1978) that translation from discrete, static and context-free to dynamic, continuous and context-adjusted requires a "drastic restructuring" of segments, whatever the original input segments might be. Thus, the many attempts to provide theories which explain and solve the non-isomorphism between the abstract linguistic units and the concrete production units. In the course of that work much effort has been expended toward attempts to find physical/physiological correlates of the abstract linguistic units.... to eliminate the non-isomorphism.

To date this research has been notorious for its lack of success and physical/physiological correlates of abstract linguistic units are conspicuous largely via their absence. Such repeated failures have caused some researchers to become disenchanted with the particular research strategy entailed in translation theories. They contend that when experimental data are shown repeatedly to be at variance with theoretical constructs it is only natural to begin to question the legality of the constructs. Carried on, such an argument raises the question: should production/motor control theorists develop their own units and concepts which are based on actual experimental observations of motor control mechanisms in general and which are unbiased by notions and abstract concepts borrowed from linguistic theory? Moll, Zimmerman and Smith (1977) have presented perhaps the most explicit and extreme version of this view and they suggest that: "Such an approach might lead us to the identification of units of programming based on the physiological parameters of movement, muscle contractions and neural activity, units which might or might not correspond to any construct previously defined."

Although such a view may be compelling, it can lead to a small feeling of scientific schizophrenia in those of us who have for so long followed the "translation theory road". The notion of sets of transformation rules between such interfaces as the output of a phonological component and the neurophysiological structures of the speech producing mechanism seems such a reasonable notion. The linguistic concept of "phoneme", for example, is indeed an abstract one.... unseen and unseeable. But so also are many of the concepts of the physicist unseen and unseeable. Further, Fromkin and others both previously, and here at this Congress, have discussed persuasively the psychological reality of linguistic units as demonstrated by, for example, speech errors. Nevertheless, the arguments proposed for not allowing ourselves to be prejudiced by the use of preconceived and abstract linguistic notions may also be persuasive and there may thus be some benefit in discussion of this issue.

In any case, we see two quite differing points of view concerning the theoretical and experimental approach to the general problem areas of input units and motor control rules and systems. And, there is yet a third point of view. Berstein's Action Theory

(1967) was originally proposed as a general theory of coordinated movement. Turvey (1977) and his associates (e.g., Turvey et al, 1978; Fowler et al, 1978) have applied this theory to the generation of speech and language. The action theory point of view also argues against the use of translation theories in speech production/motor control research, but does not agree that such research should be conducted without reference to linguistic units. These investigators' use of action theory and their development of such concepts as "coordinate structures" in speech motor control represent an attempt to avoid translation theories while at the same time not rejecting out of hand the use of all traditional linguistic concepts.

And so, the problems regarding our experimental approach to the nature of the input units and the motor control rules and systems which act upon those units would seem to be: (1) Should production/motor control theorists continue to search for translation rules which mediate between abstract linguistic units and concrete production units, or (2) Should production/motor control theorists attempt to ask questions about fine motor behavior in general in an attempt to elucidate speech and language generation and in the process create new or substantiate old input units, or (3) Should production/motor control theorists follow the entirely new course proposed by Action Theory and its claim of understanding linguistic organization via experimental study of the lower, "basic" properties of speech acts without the use of translation rules? I should add, since there was some misunderstanding at the symposium, that I have here only stated these as experimental approaches worthy of discussion and I have not aligned myself with any of them in this paper.

It seems to me that this symposium offers a reasonable forum for the discussion of these very important issues.

Here, then, are three very broad and interrelated areas of research and theory from which we might profitably draw questions for discussion: (1) the nature of the programming units; (2) the form and structure of the system or systems which act upon those units; and (3) what the best theoretical approach might be to discover what those units and systems are.

Each of the papers in this symposium takes up issues in one or more of these broad areas and it may now be appropriate to

consider some of their specific points of view.

For example, one topic which may be of general interest to all of the papers and which may involve each of the three areas discussed above is: What is the nature and the relative roles of feedback mechanisms versus central programming/simulation loops in motor control systems?

In that framework Abbs presents a model which stresses that not only is afferent feedback required in speech control, but it must take place at a variety of sites, including rather low level ones, in order to account for speakers' ability to compensate rapidly to unanticipated disturbances in ongoing speech. While he does not reject out of hand the possibility of a pre-adjustment, or efferent copy, system he argues that afferent control capability is the prime factor in accounting for rapid adjustments to dynamic unanticipated loads.

Perkell, on the other hand, argues that both orosensory feedback and central programming with internal feedback play important roles in motor control. Specifically, he implies a major role for central programming and internal feedback (feedback entirely internal to the central nervous system) "for the moment-to-moment (context-dependent) programming of rapid movement sequences".

Gay and Turvey present still a third possibility in the form of data which they interpret as being negative to the existence of an open-loop control system and positive to the function of the coordinate structures of Action Theory. Their principle argument against any closed loop system, "internal" or otherwise, is that "while an error signal can index how near the collective action of a number of muscles is to the desired consequence, it does not prescribe in any straightforward way how the individual muscles are said to be adjusted to give a closer approximation to the referent."

Several of the papers present data which are relevant to these theoretical observations. For example, in one experiment Folkins provides an indication of the variability, and thus the trade-off in muscle function, for jaw elevation, thereby supporting MacNeilage's (1970) earlier views on the variability of muscle activity for the attainment of particular vocal tract targets. Additionally Folkins shows that the medial pterygoid muscle contracts in a similar manner with or without a bite block in place thus

suggesting that "unnecessary" jaw closing activity is not eliminated either in the equations of constraint proposed by Action Theory or in the central movement plan of a simulation loop.

Data supportive of intermediate stages of feedback control as well as different patterns of control, which tends to support the model proposed by Abbs are presented by Hirose in his study of electromyographic activity and movement of the soft palate.

Sussman's elegant single-motor unit work demonstrates evidence for cellular level reorganization of muscle function in jaw elevation in response to a "behavioral and biomechanical aspect of the encoding program for speech.

These and additional experimental data provided by Folkins, by Hirose and by Sussman must be considered in the theoretical interpretations provided by Abbs, by Gay and Turvey and by Perkell. Perhaps in doing so, and in discussing additional data, we can make some progress in the question of the nature and relative roles of feedback and central programming. Unfortunately it must be noted, in retrospect, that such a discussion was difficult for the panel to initiate, largely due to the fact that several of the authors were unable to attend the congress. Specifically, Abbs, Folkins, Gay, Turvey and Sussman were not present on the panel. Sussman was ably represented by Peter MacNeilage but it was not possible to get the viewpoints of the others in the form of direct discussion.

Nevertheless, with all of these issues, ranging from the relative merits of translation theory versus action theory versus (for want of a better term) exclusively neurophysiologically based theory to the issues of the relative importance of feedback versus central programming, I think that without any more preambling on my part we have more than enough conflict with which to begin a discussion of the motor control of speech gestures.

References

Bernstein, N. (1967): The coordination and regulation of movements, London: Pergamon Press.

Fowler, C.A., P. Rubin, R.E. Remez, and M.T. Turvey (1978): "Implications for speech production of a general theory of action", in Language production, B. Butterworth (ed.), New York: Academic Press.

Liberman, A.M. and M. Studdert-Kennedy (1978): "Phonetic perception", in Handbook of sensory physiology, vol. III, 'Perception', R. Held, H. Leibowitz, and H.-L. Teuber (eds.), Heidelberg: Springer-Verlag.

MacNeilage, P.F. (1970): "Motor control of serial ordering of speech, Psych.Rev., 77, 182-196.

Moll, K.L., G.N. Zimmerman, and A. Smith (1976): "The study of speech production as a human neuromotor system", in Dynamic aspects of speech production, M. Sawashima and F.S. Cooper (eds.), 107-127, Tokyo: University of Tokyo Press.

Turvey, M.T. (1977): "Preliminaries to a theory of action with reference to vision", in Perceiving, acting and knowing: Toward an ecological psychology, R. Shaw and J. Bransford (eds.), Hillsdale, New Jersey: Erlbaum Press.

Turvey, M.T., R. Shaw, and W. Mace (1978): "Issues in the theory of action: degrees of freedom, coordinative structures and coalitions", in Attention and performance, VII, M. Requin (ed.), Hillsdale, New Jersey: Erlbaum Press.

COMMENTS FROM THE PANELISTS

Two panelists had comments to make on the nature of the programming units. MacNeilage pointed out the potential of single motor unit research as a means for defining the nature of such units, although he also made clear that at present he and his colleagues are not attempting to posit "any straightforward relationship between these data and such concepts as the phoneme or distinctive feature". Bladon spoke somewhat more extensively on this issue. Specifically, Bladon called for the recognition of "a plurality of articulatorily relevant units", including features, phonemes and phonological syllables. He provided examples in support of each of these and then went on to say, "moreover coarticulation needs to be sensitive at times to other properties than phonologists have proposed, including a strength hierachy, including even rule-order in rapid speech forms, and including also phonetic system size (perhaps implying some sort of articulatory distance measure)". He then noted that the existence of counter-examples against all of these units might "lead into the question of perhaps whether an interesting possibility would be that different types of units might be made use of for different motor control functions".

Two panelists also took up the question of the form and function of motor control rule systems. Hirose directed his comments to these systems by pointing out that his overall aim was to "investigate the temporal organization of the speech production process", via investigations of the "relationship between the pattern of motor control signals...and the dynamic characteristics of the speech organs which act in response to the control signals". In summarizing the EMG and movement data from velopharyngeal function in Japanese presented in his paper (vol. II, p. 351-357) Hirose noted that both the EMG activity and the resultant velar movement for nasals varies predictably depending upon the class of nasal sound being produced. He states: "It can be assumed that the EMG activity for moraic /N/ is characterized by a step-like suppression and the velar movement can be regarded as a smoothed response of the second order system to it. For the initial /m/, the velar movement can be taken as a ballistic impulse response like movement. For the geminate /Nm/ there must be a positive control which can inhibit extreme lowering of the velum in spite of the longer duration of nasalization." Thus, Hirose stressed the importance of studying the relationship between EMG activity and structural movement as one method for evaluating potential motor control rules and systems. Daniloff and Tatham, on the other hand, investigated EMG activity in the production of English bilabial stops. In a reinterpretation of the original data, Daniloff reached the following conclusions, among others: First, there is "definitely an impression from the data of multiple articulatory solutions (there is no one muscle nor any one articulator that needs to move in exactly the same way from trial to trial to get a given acoustic end) and,thus, you need to know the biomechanics of an articulator in order to interpret the EMG". Secondly, and related to the first point, "coarticulation, which you expect to be extreme in a stop consonant-vowel syllable, may be optional or there may be ways to solve the coarticulation using different muscles from repetition to repetition". Finally, Daniloff stressed the close relationships which they noted between temporal characteristics of their EMG data and the resultant labial productions. Thus, in agreement with Hirose, Daniloff provided examples of the use of relationships between EMG activity and output behavior of the structures.

Two of the panelists presented views concerning the best theoretical approach to motor control research. MacNeilage stated that one of the reasons underlying his interest in single motor unit work "derived from a relative disenchantment with attempts to define the underlying abstract units of the speech production process on the basis of experimental studies of speech production". He thus wanted to provide some data about the rather high level stage of the motor unit, which he believes "defines the way the

central nervous system must encode its information", before ultimately returning to the "larger questions" of underlying units. Bladon, on the other hand, expressed concern that "the limited predictive capacity of each of these linguistic constructs (features, phonemes, and phonological syllables) have led various people to be critical". Specifically, Bladon cited both MacNeilage and Lubker in statements relevant to the lack of correspondence between production data and theoretical linguistic constructs. He suggested that "large numbers of linguistic constructs have been shown to have some relevance to the control of coarticulation and if they have come to very little effect in their operation, can you really expect all data to be supportive of any one construct?" Bladon answered his own question in the negative and expressed considerable unease at the "nihilistic" views of Moll, Zimmerman and Smith (1977) cited above in the introductory comments. In the subsequent panel discussion, MacNeilage extended his views somewhat by stating: "I think the basic state of affairs is that we have a linguistic message that we are trying to implement by a motor control system and the implementation of that message must obviously be related to the nature of that message and therefore we need to continue to struggle with the problem of what the underlying abstract forms are." And further, speaking directly to the issues raised by Bladon, he stated: "When I say that I think the theory is relatively unsuccessful, what I mean is that there is no simple set of rules that can account for the observed coarticulatory behavior. I think our problem is that we just simply have too many divergent pieces of data and we do not have a clear-cut relationship between those data and the underlying concepts like the syllable. So, we have these kinds of anomalies and we have these fairly spectacular cross-language differences in exactly how speakers handle coarticulatory events, and I would stick with my characterization that the theories have been relatively unsuccessful." In return to MacNeilages comments, Bladon agreed that there was no simple set of rules but did not think "that we should therefore conclude that a complex set of rules is a non-successful one". It would thus seem that both Bladon and MacNeilage were concerned with some form of "translation theory" approach to motor control systems in spite of some differences regarding the nature of the translation theory. Indeed, this seemed to be true in the case of

all of the present panel members. The paper by Gay and Turvey was supportive of Action Theory but since neither of them were present that view was not taken up at this point in the discussion.

Finally, Perkell provided a consideration of the relative roles of feedback and central programming mechanisms in motor control systems and in doing so pointed out that it is necessary that we "understand the way feedback works if we are ever going to come close to understanding the physiological/neurophysiological correlates of linguistic units". Perkell suggested three forms of feedback which might be important to speech motor control: (i) "oral-sensory feedback utilized over relatively long time spans in conjunction with auditory feedback to establish and maintain a subconscious knowledge of certain vocal tract states which produce sound outputs that have distinctive and relatively stable acoustic properties"; (ii) "peripheral feedback used to inform the control mechanism about changes in the frame of reference which must be taken into account in making adjustments in motor programs". Perkell discussed this second point in detail in his paper (vol. II, p. 358-364). In the present discussion he added the notion that "when a motor program is constructed and executed, it is probably accompanied by a set of expectations on the outcome of the program and feedback is likely used to compare the actual with the expected result. If a large enough mismatch is detected then adjustments have to be made in subsequent programs."; (iii) "Feedback could be used on a moment-to-moment basis in the partial control of the individual's articulatory movements or in the coordination of more or less simultaneously occuring movements of different articulators." In discussing this last form of feedback control Perkell brought in the work of Folkins and Abbs (1975) which suggests that the "peripheral reflex pathways are programmed to make on-line or moment-to-moment adjustments in commands to the articulators". He also discussed the work on head-eye coordination in monkeys which has been shown to be controlled by reflex pathways involving the vestibular apparatus. This, in turn, led him to the question: "is there anything like the vestibular apparatus for vocal tract movement coordination? In other words, in what ways might the neural organization for speech production be specialized for moment-to-moment use of peripheral feedback?" Perkell warned that in seeking answers to such questions we must

be very cautious since the experimental conditions in feedback research might cause subjects to use mechanisms which are available but not used for ordinary "ongoing overlearned speech activities". Perkell concluded by suggesting that "a great deal of movement control for ongoing adult speech production is probably accomplished through pre-programming. We use motor patterns which are stored in some kind of incomplete form and elaborated in part during pauses and in part on a moment-to-moment basis. The control mechanism could use what the motor control theorists like to call 'efferent copies' or a knowledge of ongoing motor commands which could be used to compensate for self-generated changes without having to resort to peripheral feedback. In order to account for natural variations in articulatory movement (e.g. motor equivalence) some moment-to-moment feedback function seems to be necessary. Now, this feedback function could include peripheral feedback and it probably includes feedback mechanisms contained entirely within the central nervous system (cf. the discussion by Hirose, below). The use of internal feedback in place of peripheral feedback might be part of learning how to speak and there is most likely a fluctuating use of various forms of feedback depending on the demands of the situation."

In addition to these relatively formal comments there was also some more informal discussion among the panel members, some of which has already been alluded to in the above section on theoretical approaches to questions in motor control. During this discussion Perkell pointed out that coarticulation is observed in terms of structural movement and that "we don't see the movements of features". He further observed that structural movement, using the example of the mandible, is set by goals specified as a function of time and influenced by the movement and positions of other structures such as the lips, tongue body, tongue tip and even the larynx. All of these requirements on the mandibular movement must be summed so that they "produce a set of motor goals for the mandible which is really vertical position as a function of time". Further, what seems to apply "almost universally" for such conditions is some form of "look-ahead" mechanism which checks for future goals and intervening requirements, thus allowing smooth movement from goal to goal. Perkell then notes that recent data (see discussion below by McAllister) suggests that in rounded

vowel-nonlabial consonant-rounded vowel utterances there is a trough, or reduction, in EMG activity that would not be predicted by a look-ahead mechanism. He then called for some discussion of such look-ahead mechanisms and the possibility of word or syllable boundaries to help us "nail down" such data. In response to this, Daniloff suggested that juncture which exceeds some given length of time may result in suppression of activity in certain articulators and movements towards more neutral positions. Bladon noted that although the mass of data seems in favor of articulatory spread of features such as rounding across syllable and word boundaries there may well be cases in which speakers are simply using different strategies and where boundaries "have come to be influential". However, he does feel that the weight of the evidence is to the opposite and that coarticulation does spread across such boundaries.

DISCUSSION

Since space does not permit the inclusion of all points made during the open floor discussion, only those points most relevant to the main issues raised by the panel will be taken up. Additionally, priority is given to those who were motivated enough to comply with the Congress Organizers' request to supply written summaries of their questions.

Löfqvist provided an extensive discussion of Action Theory. He pointed out that not much experimental work had yet been done within that framework but that theoretical considerations are equally important and that theoretical arguments and issues should be sorted out before starting experimental work. He said that "one of the main problems in motor control, emphasized by the Russion physiologist Bernstein, is that of reducing the number of degrees of freedom to be directly controlled". He also suggested two problems which any explanatory theory of motor control must deal with: "Movements should be made to reach a given goal irrespective of varying initial position", and "Movements should be carried out in the face of unexpected perturbations or changes in the environment." Löfqvist emphasized that both of these movement conditions must be carried out "without any lengthy search procedure". Action Theory accounts for such movement phenomena via the concept of coordinative structures, which can be "regarded as a functional grouping of muscles constrained to act as a unit.

Specified relationships between a group of muscles, expressed by equations of constraint, make the group self-regulatory." He suggested, in closing, that "the perspective of coordinative structures would lead you to predict that invariance will not be found in the individual muscles. Rather, it should be searched for in the dynamic relationships between muscles, or groups of muscles, over time.

In response to Löfqvist's comments, Lindblom asked how Action Theory accounted for the ability of the motor system to adapt to an almost infinite number of new situations while goals remain constant. Lindblom further called for the panel to clarify the term "pre-programming" which he took to mean, in general, "some kind of adaptive, creative control strategy derived on-line and involving foresight". Specifically, Lindblom called for discussion of a possible mechanism to account for such control. Hirose answered Lindblom's second question by reference to a cerebro-cerebellar loop which has been proposed by Allen and Tsukahara (1974). These authors describe a specific neurophysiologic system, the cerebro-cerebellar communication system, "the function of which is largely anticipatory, based on learning and previous experience and on preliminary, highly digested sensory information that some of the association areas receive."..."In other words, in central monitoring of efference, a copy of the motor commands sent to the muscle is monitored centrally and thus it should not wait for proprioceptive comparison." Bladon also offered some comments on Lövqvist's view of Action Theory and in doing so extended Lindblom's question concerning it. Bladon first stated that he felt that the concept of coordinative structures was quite promising. Nevertheless, he felt that there was a major problem which both Löfqvist and Lindblom had alluded to, and that was, "how do you actually investigate this, how do you test this theory, how do you compare it with what you have already?" Bladon suggested that since it has been stated that coordinative gestures involving speech are agents of coordinative structures, then perhaps experimental proof of the existence of such coordinative gestures would provide the sought after evidence. In reviewing that evidence with which he is familiar Bladon was unable to provide any direct support for such coordinative gestures and feels that the question of experimental proof for Action Theory remains an unaswered and important one.

Somewhat later in the discussion Port made a comment which was relevant to the Action Theory concept. He argued for a less limited role for timing in coarticulation theory. Specifically, he suggested that "an adequate theory of coarticulatory phenomena should probably also include explanation of examples of inherent durational effects and their compensatory adjustments as an integral part of the system--not as a different theory patched on at the end. It is even possible that by building in this kind of temporal coarticulation at the outset, we will find the entire project more tractable." Port then stated that "the notion of coordinated structure employed in action theory is intended to capture both the temporal and spatial invariants of a phonetic event. Perhaps this is a theoretical notion that could be developed to capture both the temporal aspects of the spatial position of articulators as well as the inherent temporal structure of segments and prosodies."

Turning in another direction, McAllister responded to Perkell's question (see above) concerning the failure of "look-ahead" models to account for the observed "trough" in recently reported EMG data. McAllister showed simultaneous movement and EMG data from labial function during the production of rounded vowel--nonlabial consonant string--rounded vowel utterances. The nonlabial consonant strings consisted of one, four and six consonants. These data clearly showed troughs, or relaxations, in both the EMG activity and in the lip rounding, the most interesting point being that the relaxations occured at the boundary between the offset of the consonant string and the onset of the second vowel. McAllister agreed with Perkell that such data are incompatible with previous descriptions of the look-ahead mechanism, and stated that he is particularly "hard pressed to explain the location of the trough." He suggested that there may be "a critical acoustic boundary" at that point which demands a "neutralization" of rounding.

Ohala suggested that our search for underlying units would perhaps be facilitated by examining cases where coarticulatory behaviors were "clear" rather than "smeared". Specifically, he presented a number of examples of cases, in Swedish and in English, where coarticulatory behavior was time-locked to phonemes.

As a final point in this summary of the discussion from the floor, the comments made by Porter may be appropriate. Porter called for considering production and perception phenomena more closely together rather than as distinct fields of study. He felt that this would aid us in "terms of understanding perception and also in understanding the role of feedback in the control of output". Porter extended his argument via Action Theory by noting that somewhere between "abstract phonetic entities and the more concrete properties of motion and acoustics" there must be an "interface and a common code". That is, a common code to the exclusion of a translation theory. A code that functions both in production and in perception.

Very little summary is required for the above comments. It seems very clear that answers are being sought and that there is a healthy amount of controversy. The seeking and the controversy suggest that researchers in the field of motor control are, indeed, working toward those goals stated by Lindblom in his plenary lecture: that "phoneticians should extend their inquiry into the sounds of human speech to ever deeper physiological and psychological levels using speech as a window to the brain and mind of the learner, talker and listener", and, further, that we should expect "more complete, theoretical models and computer simulations to be proposed for speech production, speech understanding and speech development that match the present quantitative theory of speech acoustics in rigor and explanatory adequacy".

References

Allen, G.I. and N. Tsukuhara (1974): "Cerebrocerebellar communication system", Physiol.Rev. 54, 956-1006.

Folkins, J. and J. Abbs (1975): "Lip and jaw motor control during speech", JSHR 19, 207-220.

Moll, K.L., G.N. Zimmerman, and A. smith (1977): "The study of speech production as a human neuromotor system", in Dynamic aspects of speech production, M. Sawashima and F.S. Cooper (eds.), 107-127, Tokyo: University of Tokyo Press.