Summary and discussion of reports and papers

WILTON MARION KROGMAN, Ph.D.
Philadelphia, Pennsylvania

Dr. Brodie, Ladies and Fellow Alumni:

When Dr. Brodie asked me if I would be with you these days I said, "yes I shall come here to learn," and that I have done most abundantly. I warned him in advance what I knew would be true, and now I tell you that never has one man received so much and given so little. I have on hand a notebook with thirty-four pages of comments, question marks, exclamation points, asterisks, and "the hell you say!" written along the margin. But I think that I have about 3400 pages of notes tucked away in the laminae of my cerebral cortex and I shall thumb through them in the years ahead of me.

I wish to take exception, however, to a remark that was made a little earlier this afternoon, namely, that somehow or other the theme of these few days has centered far too much upon that of variability. I don't feel that way. I have recorded my major reaction that what I have been hearing in these past days has been the concept of *the whole face* in the entire organism. Now, if one is encompassing such a tremendous scheme then obviously the concept of variability becomes relative rather than absolute. I think we have a feeling of the rather evanescent nature of some of our findings only because we are now on the threshold of our learning, and we find that our researches are integrating into a larger complex. I do not find that alarming at all. Indeed, I find it rather comforting because I have learned from studies of human genetics that there are forty thousands pairs of genes characteristic of *Homo* sapiens, and that probably not one of those genes has a single function; all of the multitudinous structures of the human body (including the facial area) are also based upon multigene complexes. I, for one, as a human biologist, must react with a sort of an awed wonder that there are not *more* variations or *more* anamolies merely on the basis of random recombinations of genes. So I urge you that the problem of variability or of variation does not mean that we are going to run wild with your concepts of stability. It means that we are going from the infinite of possibilities into the finite of that which we actually find, and observe. I am coming back to that theme in a moment because I think it has something very real to do with challenges that are now coming forth as to the validity of the accuracy of the several techniques that are being employed.

I want to come back to the concept of the whole face in the entire organism. Like all of you I had work in gross anatomy and in physiology and, as with the majority of you, it occurred many, many years earlier. I am overjoyed to see the evidence of the application of the newer techniques to the elucidation of problems that most all of us have envisioned as being part of the complex of the whole. I listened with a great deal of interest, and certainly with a broadening of my own horizon, to the muscle studies, especial-

*Presented at the Second Reunion Meeting of the Department of Graduate Orthodontia, University of Illinois, Chicago, Illinois, March 1951.

ly the physiological bases elaborated so beautifully by Dr. Pruzansky, and then integrated into a web of thought by the physio-morphological balance pictures that we were getting, and have been getting, from Dr. Brodie. It seems to me that here is the happy blending of the theoretical aspects of muscle physiology with the practical aspects of the functional interpretation of that physiology when trying to ascertain a balance, and — most important of all to you men as orthodontists — when trying to assess what's happening to that balance when by virtue of orthodontic therapy there are inevitable disturbances.

I was very much pleased by all the papers, but this is all I am going to have to say on individual studies, because the remainder of my time I want to devote far more to the broader aspects of problem-raising. I was greatly intrigued by Dr. Engel's bone studies, which are pretty well on a theoretical basis. But you know what theory is. Theory is that which you haven't yet learned to put into practice; therefore, those studies which seek to interpret for us the ground substances of bone, the context of bone which forms its malleable portion, that portion which must yield to the stresses and strains that are imposed by orthodontic appliances, have but pushed back the horizon of our thinking concerning bone-structure merely to its ultimate cellular structure. It is not theory, then, in so far as the time will come, I am sure, when we will implement it into the thinking of just exactly what is happening to bone not only during growth (that's very important), but during the equally important readjustments that must be occasioned by orthodontic therapy.

Certainly I could not let pass the studies that Dr. Ricketts is carrying on of the temporomandibular joint. I studied years — well I didn't study — I *noted* years ago the T-M joints of the anthropoids when I was studying the growth of those Primates; there was that little residual perception or awareness in my mind that there must be something related to the forward and downward growth of the mask or the snout in those beasts, correlated with the temporomandibular structure which in essence is found also in Man. Yet there are certain structural modifications in keeping with the ruggedness of the structure in these giant anthropoids as compared to Man. And here — and it has been my privilege to hear Dr. Ricketts also at Bailey's Harbor. I find that the technique is now at hand where we shall unlock the secrets of the action of that all important adjustive center of movement during growth. Again I repeat, because that's what I'm interested in, but also for your interest during the period of adjustment that must go on when you are moving segments, the understanding of T-M joint function is basic to both growth analyst and orthodontic synthesist.

Now I want to take up some problems. Not that I know anything about them particularly, but because they are problems that have been resided in my own thinking; problems that I perhaps have come to from a different angle. I have come to those problems as a growth student who for many years has been far more intrigued with, shall we say, the fundamental principles of growth and development rather than the elaboration of those principles into a framework of thought that looks to their use in therapy; although you know I am greatly interested in that, too. But I must present to you immediately that I find limitations in my thinking in the latter direction because, not being a therapist, I am not aware of all of the final residual problems that exist in the adjustment between growth and treatment. I can thus go so far in my thinking, and you must meet me half way, to round out the picture.

May I break the train of thought for a moment? Sunday night we talked about cooperation. I don't think that I have ever seen it more beautifully exemplified than within the confines of this group. I don't mean the co-operation that says, in effect, "I'll measure this bone and you'll measure that bone," and that sort of thing. I mean cooperation not so much in matters of technique as in matters of spirit. Of course, I knew it would be that way, because when I was at the University of Chicago there was never any thought that I might go to another institution to spend every Friday noon in learning; it was not *another* institution. They were both institutions of higher learning, and we were men brought together in a common purpose. I have been listening in that same spirit of the very friendly cooperation between the University of Illinois and Northwestern University; again the complete abrogation of all institutional lines with the concentration of men and skill and of knowledge upon a mutual problem. If we can't have that in research and in science then God help us! We'll have it nowheres else within the confines of our work and our thinking. We must establish and keep that way of doing things.

Now, let's consider the problem of "pattern." Sometimes that word pattern has bothered me very much! I must tell you this in all fairness that I think I misunderstood its use earlier when I read that a pattern was established at an early age and that it was set as though it were poured in a concrete mold from that time on. It bothered me because if that were true, then it seems to me that thinking itself must be similarly set. If you say the pattern is immutable then you have surrendered the right to speculate about that pattern. So I have come to the idea of pattern in terms of this: we physical anthropologists for centuries, more or less, have been engaged in the business of measuring — measuring for the purpose of establishing types. It is true that we did not begin with the purpose of establishing gross types. The physical anthropologist concerned himself first with the establishment of cranial types which led him to the consideration of a racial typology. But the philosophy behind it was that he used his measurements to cast them in mold whereby he might say that this is a dolichocranic skull or a brachycranic; or this is a Nordic or an Alpine. The concept of type, therefore, was very similar to the cephalometric concept of pattern, in so far as both used a framework of dimensionality and proportionality to give rise to an idea of relative stability. So that began to make me feel better, so to speak, and I felt that I did not have to surrender any plasticity of thinking in the presence of a type or pattern presumed to be fixed.

I then went a step farther and I began to think in these terms, viz., we have an anthropometric type which is largely quantitative by the very limitations that it is established upon mass studies taken one at a time with never the possibility of recapturing those individuals in successive moments of time; whereas the cephalometrist came to the concept of the whole in so far as he was interested in the broad outline, the delineation of the broader outline — or I give you a better word, the configuration. It was this configuration, rather than the component parts thereof, that established the pattern and it was therefore an *individual pattern.* That took me very logically on to the final resolution of these two different ways of looking at it, that the physical anthropologist in the unity of his measurements, and by the word unity there I mean oneness, was limited to an area, or a regional concept, whereas in contrast the cephalometrist was concerned chiefly with an entire complex. It is true that we have been seeing studies of regions or areas; we have been seeing the condylar area, or we have been focusing upon an incisor area, but always with reference to the relation of that area or that region to its integration within the entire complex. I find, therefore, a very happy meeting of minds necessitating no compromise of thinking in either direction, that this pattern concept has a complete validity in so far as it applies to the individual through time. It refers then, in my final interpretation and understanding of the meaning, ultimately to the general constellation or complex of morphological characters that are that individual's endowment and which he will have as he goes along, but which he will elaborate by growth as he goes along; in so elaborating by growth there are, and Dr. Brodie spoke of it a few minutes ago, these differential rates and timings of growth, or I might say, timings and rates of growth that within the confines of that pattern will endow it with its ultimate proportionality. It is not, therefore, the unit characters of the pattern that are set; it is the complex which in its unfolding within that individual sets the pattern as an integrated whole. And if we at problem level can focus our thinking within that framework, the pattern hasn't lost stability at all. It has, however, lost its overtones of immutability, which is an entirely different idea.

Now let me go further to another problem. The anthropologist has a pattern of dimensional balance to work with and that is a matter of proportion. The cephalometrist has a pattern of dimensional integration to work with. So you see we have gone another step forward in translation of an almost historic approach to these ideas. The thinking of the anthropologist has been limited, almost to the detriment of his progress, I am sorry to say, by the fact that he has granted dimensions sole priority in his thinking. It is true that by the calculation of numerous indices he has observed changing proportions, but always the proportions have been thought of as the relationship of one dimension to another and he has made a shibboleth, as it were, of the changing indicial value that has become a means rather than an end, the end being the expression of the ultimate attained proportionality. And when, therefore, I can come to the utilization of a series of dimensions either by the Downs or by the Wylie method so that I have an understanding of their relationship to one another in the sum total of the entire configuration, then it seems to me that you men with cephalometry have taken over the crude clay of anthropometry and fashioned it into the figure of meaningfulness with reference to each child as, again I repeat, he moves along his own appointed path in growth.

So I have come full circle in my thinking with you and I am back to the pattern concept with the full realization now that it is not as static as I formerly thought it was, and it has within it the dynamic potentiality of change within the configuration of that pattern — and I'll go along with that completely!

Now why have I talked about some of these things? You may think that we have discussed these concepts enough. Well I don't know, I don't think we have. Anyway, that's the impression I have gained from the literature. I am interested in it right now because with the youngsters that I am working with in Philadelphia I am at the moment rather bound to the consideration of dimensions because up until last September that's all I was able to do with them. I didn't get a roentgenographic cephalometer until then. I have the problem in my own thinking, you see, and in that problem I share with you in thinking out loud, of looking into the possibility that there may be — not necessarily some correlation because that implies consistent meaningfulness — but that there may be some relation between the progress in changing dimensionality and the interpretation of the cephalometric pattern in the same child as we go along. It means, therefore, that with reference to a given youngster I have a couple of arrows in my research quiver and I am going to see whether both of them will penetrate the bull's eye to the same extent when both are fired at the same youngster. That is a problem I will have to cope with in the future.

I want now to go on to the problem of time. "The problem of time!" — I repeat it once again, because it is so fundamental. We're certainly not going to resolve that problem here, now, or in the immediate future, because I think that the burden of almost every paper, either implied or stated, that we have heard in the last few days has something to do with the problem of time; the relationship between adjacent structures or between adjacent systems of structures must be somehow or other genetically linked, and if that linkage be genetic then the time mechanism also must be a matter of, shall we say, a trigger mechanism at the appointed time. Let me give you an example of what I mean with that sort of rather abstract thinking. The first permanent molar erupts in the Anthropoids somewheres about two years of age, and erupts in Man somewheres about six years of age. As far as the dentition is concerned they have the same dental formula that we have (2-1-2 deciduous, 2-1-2-3 permanent). As far as the order of eruption is concerned the only real difference is that very often the M2 may preceed the Pm 1 and Pm 2. There are, therefore, a few differences of eruption order that we recognize. The problem of timing, however, is the important one there, is so far as there is one genetic moment that is characteristic for one animal and another genetic moment that is characteristic for another. It is, therefore, fit and proper, so to speak, that in Man the six year molar should be so timed; if that be so it is also fit and proper that structures associated with it must also be similarly timed. We may, therefore, argue (believe you me, I am way out on a genetic limb right now, but I am out there rather comfortably because the human geneticists don't know enough to shake me off yet) the consideration of the fact that there must be a time-linking that is the fundamental biogenetic rationale behind the whole process. Now no one ever said that if there be a genetic background to this complex that that is cause to throw your hands up in horror and to say, "Well if it's genetic, then genetic it's going to be, and what are we going to do about it?" Did you ever hear of environment? Did you ever hear of the fact that there is the possibility of the impact of the environment so that there may be, as it were, supra- or post-genetic disturbances? So that you may have, *may* have, I repeat, a disturbance in timing or a disturbance in interrelationship that violates or transcends the biogenetic potential that, shall we say, would have gone on had all been well. We heard some cases in point this morning when we were discussing the possibility of an endocrine imbalance that might result in improper timing or the improper amounts of growth to give either growth disharmony or faulty integration. The problem of timing, then, is one which must be clearly understood, in unambiguous terms.

Some of my orthodontic students at the University of Pennsylvania have been rather concerned of late that there is a discrepancy between what I say in lectures and what they read in the literature. It all centers about the interpretation of timing, because I have used a hyphenated word, non-synchrony, and have stated that there is non-synchrony in the growth of the several planes of dimensions of the face. The assumption that has been made is that non-synchrony is of such a gross nature as to imply that it is the causative factor behind most cases of growth deviation, disharmonies or imbalances. It is not that at all. It is an attempt to give a logical explanation, and to verbalize about the fact that the actual amounts of growth in the several dimensions of the face differ postnatally. That is a fact. My millimetric rule tells me that, and it also tells me that if one dimension is going to grow more postnatally than another, that the rate of growth — assuming the total span allotted to each is about the same — has to be more in one and less in the other. And if, therefore, I pursue it still further, and if I handle my data on a chronological basis, I find that there is a time-linking in this sense that the rate in measurement A is a little bit faster at X age and the rate in measurement at B is a little faster at Y age. That is what I mean when I use the term "non-synchrony."

I am open to challenge on another approach. Dr. Brodie and I discussed this at Bailey's Harbor. The way in which data are handled is apt sometimes to "load" the findings. If, therefore, I handle the data (and I have done that in my mass data on the measurement of the living face), on the basis of Hellman's dental stages, or the refinements thereof that are dictated by recent advances, then obviously the inflections of the growth curves, as I plot them, will show changes that are consonant with the way that I set up the data to begin with. That makes sense. I defend it up to a point by saying that if that be true, that in itself is important because we are interested in the changes in dimensionality as they are related to either the developing or the erupting dentition. But I won't sit back and be content with that, and I'll tell you that I have now a two years run-through on 600 children, both on the basis of chronological age and dental age, and am now scanning all my data on skeletal age; so we'll have three sets of curves based upon one chronological scheme and two biological schema. If then we find the same impulses, the same moments of, shall we say, acceleration, or advance, or slow, or whatever you want, in the three sets of data we may at least suspect correlative growth. If we do not find that there is the timing in the way these things are behaving, regardless of how they are set up on a statistical framework, then at least we have to follow the direction that our data are leading us and say that there are time-linked moments in this dimension or in that dimension when there is a slower or a faster relative rate. Now please believe me I am not going back to this stuff of spring-up, fill-up, spring-up, fill-up, and all that sort of business. However, I am convinced that there are major phases in which there is an acceleration in a rate and then a plateauing and then an acceleration. Right now I won't advance my thinking much beyond early infancy for the first sweep of the curve and then the plateau and then so-called circumpubertal acceleration. I was glad a few moments ago to hear Dr. Brodie refer to the fact that when (and I can't quote him directly) adolescent growth came in he had a spontaneous recovery in the case in which there was a tongue too large for its supporting structures. The very fact that one is able to say something like this, that the tongue next to the brain is nearest its adult size at birth, is indicative in and of itself of a disparate rate of development between adjacent structures, and also structures that we have always been wont to think as functionally interrelated. Timing, therefore, really becomes in and of the essence of our thinking on these problems of growth.

I have written a key phrase that I now want to talk about: the theme of "plane determination of relative constance" - plane determination, p-l-a-n-e. I was struck by Lande's paper yesterday in which he very beautifully demonstrated the relative constancy of the profile; demonstrated it by virtue of the fact that the method of superimposition guaranteed, as it were, that that moiety would be held in a constant relationship throughout the progress of growth because the basis of superposition was the S - N kept constant by the reference to the Frankfort Horizontal. I had to ask him about that afterward to straighten it out in my mind, and we agreed upon the principle of subtending a given area to focus upon its relative growth-movement. We agreed that in all methods of superposition that the end points that are constantly used in matters of dimensionality and proportionality were not in and of themselves perfectly stable; therefore, if stability be assumed in this profile dimension or direction, or plane, call it what you will, then the changing configuration has to be understood in terms of the area held relatively constant. I noted particularly the movement at gonion. I know full well that the mandible in growing doesn't grow all the way back as it would seem in facial contoural superimposition. This has nothing to do, please understand, with the findings, per se; it has to do with the problem of interpretation of the data within the framework within which you're working. Dr. Brodie has seen an advance copy of a paper that I hope will appear shortly in the A. J. O., in which I have brought together no less than twenty planes of superposition used in craniometry and cephalometry. Many of them were devised for a specific purpose, that is, devised to bring out the cranial differences in races, a difference in contour, a difference in proportionality; or used in the dynamics of growth; used for the purpose of demonstrating the all around constancy of the pattern even while concentrating upon the growth fluctuation that is occurring as one region after another is adjusting during growth. So this business of planes is an exceedingly intriguing one. It's a challenge to big thinking in the sense that a shift in the end points of the plane or a shift upon the point of superimposition will result in a relative shift of the point that formerly was assumed to be stable. No one showed that more aptly than Todd when he was playing around with the nasion parallel. You may see that in Broadbent's 1931 publication; that when nasion was superimposed upon nasion, porion was drifting down and backward, but when porion was superimposed upon porion, then nasion was going forward and slightly downward. Which was moving? The answer is that *both* were, but for the sake and in the interests of a relative stability it was necessary to assume that stablity on the basis of the superimposition which was the basic premise upon which interpretation was made. And I might add this, that of all the planes that I have played around with I think that coming to the S-N plane is one of the happiest choices that could have been made. It has, as it were a functional synchrony between the cranium and the face, and it is an area that is quite stable.

Now I am going on to another problem. I said I wasn't going to mention any individual papers and yet I have to come back to one because I thought that it was an exceedingly important contribution to the thinking of interrelationship; that was Dr. Steadman's demonstration of the apparent independence of overbite and overjet as exemplifying a non-synchronous, or better, non-relational situation. That suggested to me a very important idea, and yet it's not a new idea by a long shot. It was an important thing to think in terms of these two measurements, one of vertical height and the other of horizontal length or of depth as we may want to call it. Dr. Steadman's paper has contributed, in its analysis, that height and length are not related in a cause-and-effect relationship, but rather both may be symptomatic of the same generalized growth deviation or disturbance that is characterized by time failure, not in one but in two non-linked planes. So I return therefore to the possibility that there may be some genetic non-linkage and there may be failure in a time relationship, but I don't know exactly how to apply it any further. But that represents the type of what my former chief at the University of Chicago, Dr. Fay-Cooper Cole, used to speak of as "negative evidence," which is every bit as important as positive evidence, for here is a case where the negative evidence freed us from the shackles of thinking that the two might be related morphogenetically. We now know apparently that they are not and we can go ahead from there. The problem now comes to us not to focus upon them as related elements but simply as non-related elements: how is each related to the facial complex, and through the entire facial complex related to one another? I toss that in Steadman's lap as the next phase in his problem.

I now approach the problem of the normal. I don't know what the normal is. I am six feet, five inches tall. I am out at 99.2% in the range of stature distribution for American males, but don't call me abnormal! I am within the normal range of stature for *Homo sapiens.* Sure, I am at one end of it — almost all alone, too. There are only eight out of a thousand to keep me company out there. I hit the problem of the "normal" in Philadelphia and I hit it with a vengeance; or shall I say that I was hit with it with a vengeance because some of my colleagues in orthodontics said, "Bill, in this growth study of yours you are going at it all wrong." You see I started 600 children between 6½ and 12½, (now the age span is 6½ to 14½, since we've moved into our third increment). I am taking measurements, and x-rays, and so forth and so on, on all these children on an annual basis and we are developing what we are pleased to call "norms of changing dimensionality" in the face, and in the whole body as far as that's concerned. But you may be amazed when the orthodontist who is associated with me in this study revealed the fact that 54.1 percent of the children in that sample had a malocclusion. When I reported that to some of my colleagues it almost administered in their eyes a *coup de grace* to our program. One said, "Bill, you should have had 600 children who were certified as having normal occlusions." I said, "certified by whom — where can you get two orthodontists who will agree on 600 children or will agree on the same child two years apart?" But that's an over-statement, anyway you get the idea. When I reported this to the School Board they thought I was crazy because the school dentist had said there were only 8 percent malocclusions in the public school population and the medics said only 2 percent. Now, obviously, the problem here is who is setting up the standard of the norm, who is identifying what is normal and how infinite are the details that are going into the constellation of what comprises the norm? I think you get one aspect of the problem I am driving at.

Now let's say for the sake of argument that I had gone out and, if I could in the age-range, had found 600 children who had completely normal occlusions. That would have been for the Philadelphia child population completely misleading and unrepresentative. I would have had an atypical selected sample. To say, therefore, that the data based on such evidence is normal is no more than a statistical fiction. The essence of a true random sample must be its representative homo- or heterogeneity.

Now how are we going to set up the norm? Probably you won't agree with me on this. I know that Dr. Ricketts has raised the question of semantics and maybe my sample isn't big enough yet; maybe through errors in sampling I have gone to parts of Philadelphia where the standards of living are subminimal, that there are dietetic imbalances that have given rise to growth deviations. That I'll test by further sampling. But I am prepared to defend them, my sample, as normal, by saying that at those ages, or between those ages, it apparently is the usual and in that sense a normal thing for this malocclusal frequency to present itself, and I say no more or less than that. In that sense, therefore, the accepted condition, the status quo, becomes the operating normal with which one is going to work. The 1933 White House Conference on Child Growth, when on the subject of child welfare, side stepped the normal so completely that all that they ever ventured to say was, "a normal child is a healthy child." That is all you will find, in four volumes, on a definition of the norm. By implications, therefore, an abnormal child is an unhealthy child and you have in their thinking a clear-cut separation between normal and the lack of it. Although if you go through the index, or if you will comb the volumes very carefully, especially volumes two and four, which I have read very carefully, you will never find what defines the abnormal except that abnormal is the reverse of the normal, and that is as far as we can go on that.

I have sensed the tenor of this group to accept the normal in a statistical sense, and I think that that perhaps is, on the basis of our present knowledge and experience, the safest thing to do. This is also intriguing: we should recognize that variation around the central tendency is more normal than adherence to the central tendency. Theoretically, on the basis of statistics, out of every one hundred that you have there is only one at the mean (the 50%). If you have a sample large enough where mean, median and mode coincide, you have 50 percent on one side and 50 percent on the other. The average, therefore, is a very lonely place. I am sure you have heard the story of the Pullman porter who was asked by a traveller who for the first time was going clear across the country, "What is the customary tip for a three-day trip?" "Well, sir, it's $5.00." So he paid him the $5.00 and the porter said, "Sir, you are the first man who ever came anywhere near the average." I am accepting in my thinking of the normal, plus and minus one standard deviation. That may be a little bit narrow. Let's put it this way, that beyond plus one and minus one in the standard deviation I begin to look for possible significance. I look very carefully, controlling all variables that I have on that individual child. Interestingly enough, Greulich and Pyle in their new atlas of skeletal maturation, do not look for significance until they have achieved plus or minus two standard deviations. That means that in only the lowest 2½ percent and highest 2½ percent are those with which they concern themselves in the sense that there may be a meaningful deviation from the standard or the norm.

The use of ± one S. D. demands common sense. In the Hellman S. D. diagram, for example, facial heights, breadths, and depths are plotted with reference to deviation from mean values. Now how absurd it would be to say that a given configuration is normal if all of the dimensions fell within plus or minus one standard deviation, but with some at minus, some at plus. Obviously, such a pattern would be erratic. That is the illustration that I was going to use, Dr. Downs, to say that the variability of dimensionality, per se, is meaningless; the variability of dimensionality with reference to an integration into the complex is the thing that we want to strive for. I am speaking now in terms of the configuration that you have developed and that deviation of one of them is not accepted as meaningful except in the sense that it may provide a profound disturbance in the entire configuration. A mere statistical, or shall I say reliance upon a mere statistical concept of the normal, is relatively sterile if one follows each one of the variants and does not consider its relationship to the others.

I think I may summarize the past three days by observing that we all follow the dictum to "measure that which is measurable, to make measurable that which cannot yet be measured." In our cephalometric points, dimensions, angles, planes, we are measuring — we are *quantifying*. I am in accord with that, for one aspect of growth is quantitative. Yet, I venture to urge caution, that quantity is not enough, that to follow a dimension or an angle slavishly may be part of the *science of orthodontics*. But in working with individuals — with an absolutely unique facio-dental complex — there is quality, too. This is the *art* of orthodontics. It is here that researcher and practitioner meet to pool knowledge, skills and experience in the service of children.

Graduate School of Medicine,
University of Pennsylvania