LETTERS TO THE EDITOR

Comment on "Issues related to the prediction of craniofacial growth"

To the Editor:

In the January, 1981, issue of the AMERICAN JOURNAL OF ORTHODONTICS James Todd and Leonard Mark have set forth a mathematical model for craniofacial growth prediction in their article, "Issues Related to the Prediction of Craniofacial Growth." Analyzing the whole curving profile of the cranium, from mandibular notch to occiput, they suggest that change over time may be summarized by a particular systematic adjustment of distances measured out to the profile from a constructed center. The adjustment, which corresponds to the hydrostatics of fluid-filled spheres, has been shown earlier to be highly concordant with the subjective perception of maturation.

In their article, we are shown that the adjustment accounts to some extent for real growth of actual human profiles as well. In my opinion, flaws and omissions in their procedures quite drain the findings of any import for prediction. From the several aspects of their argument, I will review three whose common thread is *missing information.*

The geometric transformation model

> In selection of a coordinate system to represent a given change, the goal is to find a coordinate system that is *privileged*—privileged in the sense that it reveals geometric relations which are preserved over that specific change . . . .
>
> The optimal coordinate system for depicting any change is one which allows us to see the geometric relations that remain invariant under the transformation in question . . .. Since our model says that growth is radial (Equation 2:$\theta' = \theta$), a polar coordinate system should be preferred to a rectangular coordinate system, as the former represents this geometric property more clearly.
> Todd and Mark, pp. 67, 79)

Any biologic shape change may be viewed in two different ways. We may choose to observe points succeeding each other in the course of the growth process. Whether we follow "anatomic" points, landmarks serving identical functions over time, or "material" points marked by implants, in either version we have represented growth as a mapping, a geometric function taking points to points over time. In the other view of shape change, we distort space (that is, the coordinate grid) and carry the embedded anatomic or material points passively along. A transformation grid is the assertion of an identity between two transformations, one of each type. The model, then, presents a deformation of coordinates such that points related by the biologic growth function (homologous points) have the same coordinates in the two coordinate systems. In this case, and this case only, the description of shape change reduces to the description of distortion, so that biologic data may be analyzed as if they were geometric. Landmarks serve as samples of these maps, enabling us to keep our place as we inspect differentials of growth over the form. D'Arcy Thompson's

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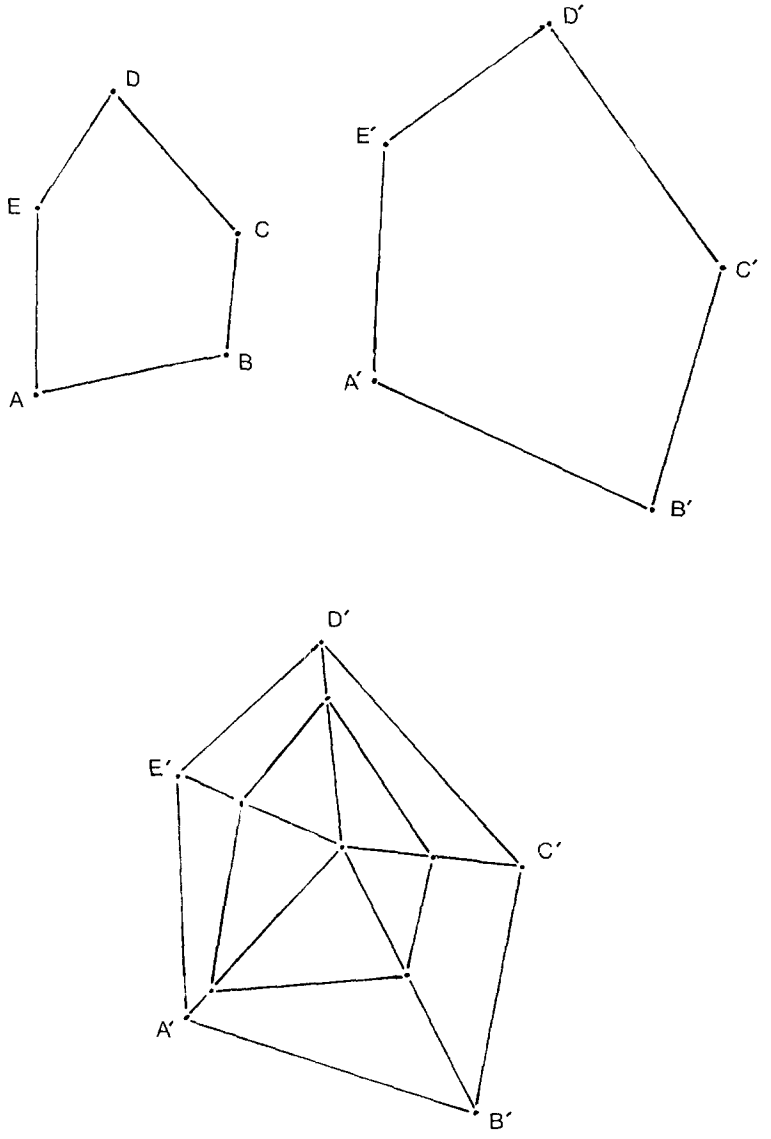

Fig. 1. Any two pentagons *ABCDE*, *A′B′C′D′E′* determine a superposition for which the lines AA′, BB′, CC′, DD′, and *EE′* intersect in a point. To this point corresponds a pair of centers, one for each pentagon, such that angles between pairs of landmarks, measured from the centers, have not changed from one pentagon to the other. These points, which are the only possible pairs of homologous centers for a radial dilation model, have clearly required a *pair* of forms for their computation.

great insight was the notion of drawing out these deformations explicitly as grids. More recently I have put forward some quantitative methods for their analysis and comparison.

The radial growth hypothesis, as it has surfaced from time to time in our literature, declares the existence of a *center* of the head from which all growth proceeds directly outward, so that angles measured at the center between lines to circumferential landmarks are constant. The simplicity of this hypothesis makes it quite easy to test. For any two

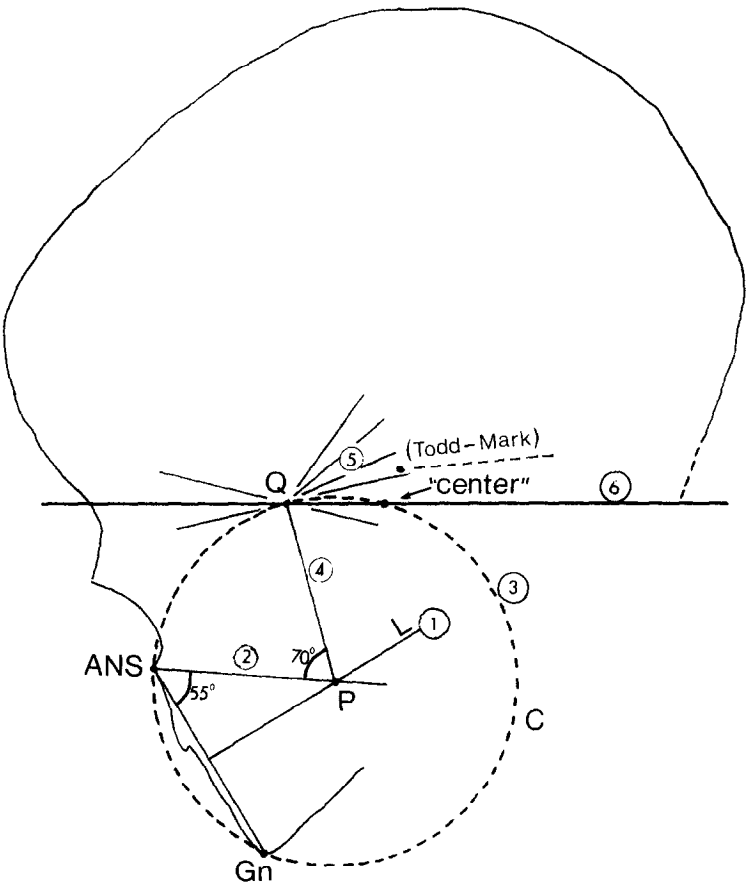

Fig. 2. Todd and Mark's characterization of the polar coordinate system may be re-expressed in a six-step construction exemplified here on the inner profile of their Fig. 4, *A*. The trace was extrapolated beyond their data at far right to complete the construction. The line labeled *Todd-Mark* is the horizontal axis they determined for this figure; the dot just left of the word *Todd* is the point they chose for center. In their coordinate system, ANS is on about the 123-degree radial, Gn on the 155-degree radial. "Horizontal axis" is probably not an appropriate name for the line computed here.

pentagons ABCDE, A′B′C′D′E′ there is a superposition for which lines AA′, BB′, CC′, DD′, and EE′ meet at a point (Fig. 1). For any two configurations of five landmarks, we can compute a superposition in this way. If the radial growth hypothesis is correct, for cephalograms of the same subject at a series of ages any pentagon of landmarks will suffice to determine the center. One would surely then superimpose the forms by registering on the center and orienting along any peripheral landmark, for then every straight line through the center is invariant under growth. An appropriate transformation grid would naturally include these lines as one set of curves. The other set might be taken as circles in one form, yielding the familiar polar coordinate scheme there; but in general the circles do not remain circles after the transformation.

Whatever the style of diagram—polar, Cartesian, or otherwise—one cannot have a transformation grid without first having a biologic homology map. For Todd and Mark's analysis, there is insufficient information about homology to corroborate any model of distortion. In fact, they have located only *two* landmarks on each form, ANS and Gn, so that no

explicit construction of centers of homology is possible. Instead, the authors compute a "center" and "horizontal" for each form separately and then superimpose as if the radial hypothesis were automatically true. This procedure has three basic flaws:

1. The center is arbitrary. Todd and Mark state, of the inner profiles in Figures 4a-4t: "These profile tracings were oriented with respect to a sheet of polar graph paper by the following procedure: The younger profile was placed over the polar graph paper so that (1) ANS and Gn were placed on the 125-degree and 160-degree radial coordinates, respectively, and (2) the profile was roughly centered around the vertical axis so that the points on the facial mask and cranium that intersected the horizontal axis did so at equal distances from the center of the coordinate system. The younger profile was never moved after its position was fixed."

To check this protocol against their figures, I needed to convert the characterization from their text into an explicit construction. The following six steps, corresponding to the circled features of Fig. 2, can be proved by a series of geometric theorems to yield the coordinate system of which Todd and Mark speak:

1. Erect the perpendicular bisector L of the segment from Gn to ANS.
2. Find the intersection with L of a line through ANS, making an angle of 55 degrees with the line ANS-Gn. Call this intersection P.
3. Draw the circle C about P through Gn and ANS. The center of Todd and Mark's polar coordinate system must lie on C.
4. Find the point Q which is 70 degrees clockwise from ANS on C. This point is necessarily upon the horizontal through the center.
5. Consider all the lines through Q, whatever their direction.
6. If the data are amenable, one and only one line through Q cuts the profile at two points (one near nasion, one near inion) equidistant from the *other* intersection of that line with C. Todd and Mark deem this particular line horizontal; that other intersection, the one that is not Q, is declared to be the center of the head.

Thus the two landmarks are transformed using fully *three* arbitrary constants: the angles of 55 degrees and 70 degrees and the ratio of 1:1 between the distances center-to-nasion and center-to-occiput. In this whole procedure there is no information to be seen. The landmarks ANS and Gn are, for present purposes, an arbitrary pair not easily located with reliability. The other three points of their construction ("top of head" and the intersections of horizontal axis with the profile at front and back) are functions of the constants in the construction, round numbers not derived from measurable anatomic trends or relationships. The sequence of steps looks, and is, strange and produces a "center" devoid of meaning.

The authors have failed to carry out their own instructions. On their figures I located ANS and Gn according to the usual cephalometric characteristics. I discovered that if the coordinate center is supposed to be at the cross of the straight lines on the figures, then assertion 1 of the text is false. The central angle of ANS from the vertical ranges from 122 degrees to 124 degrees and is never the 125 degrees stated by the text; the radial line on which Gn appears, which may be as far clockwise as 152 degrees or as far counterclockwise as 158 degrees, never attains its nominal setting of 160 degrees. In fact, the "center" as they define it is usually not computable from their data. Fig. 4, *A* (copied in my Fig. 2) is typical. The circle C of step 4 is so eccentric to the head that no lines through Q are appropriately bisected by the profile within the limits traced. I had to extend the occipital bone downward (by the entirely hypothetical dashed segment at far right) to complete the diagram.

The construction bears no information about homology. It makes no sense, in great part because the points upon which it hinges, P and Q, have no relation either to the data or to the hydrostatic model. Nothing can be learned from these coordinate systems about the

biologic relations linking points on the profile to the "center." As ANS is fixed on the 125-degree radial, and Gn on the 160-degree radial, these points have no growth but radial by definition, and no other points are followed. By ignoring landmarks upon and within the curve of the profile, Todd and Mark are free to *declare* that points sharing an azimuth correspond under growth. However, as the data are not consulted in this matter at all, the validity of the graded radial expansion model is begged. For a proper growth analysis, the coordinate systems as wholes must be homologous over the forms. For a pair of polar systems, centers of the grids must correspond. In the absence of all interior data, this cannot be verified even in principle, so that it cannot mean anything to say that one of these polar systems grows into the other. How, then, can a radius dilation function have any meaning? The authors have not accommodated the basic idea of geometric transformation models, that "the transformation in question" (see the extract at the head of this section) is not in the model but in the biologic data.

The explained variance

Were there a single center, in accordance with the radial model, there would be an average rate of dilation of each radius for each interval of biologic age. For a particular form, prediction would proceed in two steps: (1) estimating the center, so that radii can be measured, and then (2) dilating the radii appropriately. Verification of the invariants of the model (angles between landmarks in pairs, measured at the center) would be completed before any estimation of those rates (the dilations of the separate radii) which may then be further constrained by biomechanical or other theory.

Todd and Mark's statistical approach tangles these steps until their validities cannot be discerned, either individually or jointly. With only two landmarks taken, there is no way to examine the radial model; the authors must simply assert it. Likewise, the function representing radial dilation in terms of the azimuth θ is simply asserted, not observed, to be a multiple of $(1 - \cos \theta)$. A test of either of these assertions would require repeated analysis of the data to check whether the assumptions significantly constrain the explanation of variation observed. Such is the standard logic of statistical inference which underlies, for instance, the testing of linear models and analyses of variance by F ratio. The authors have not done this. They report percentages of variance "explained" equal to 75.6 for females and 77.7 for males and declare that these are "impressive" and "closely predict the actual outcome." Unfortunately, those coefficients do not test what the authors are asserting and are useless in the absence of further comparisons.

> After a profile was properly oriented, a graph pen tablet was used to record the polar coordinates of about 150 points along the outer boundaries of the cranium, facial mask, and mandible. These points were not anatomic landmarks but arbitrary points on the facial profile roughly 3 to 5 mm. apart. These data were used to generate a continuum of transformed skull outlines by applying the transformation $[\theta' = \theta, R' = R + b\, a\, R(1 - \cos \theta)]$with successively larger values of b. The resulting family of forms constituted a predicted path of craniofacial growth . . . . This hypothesis was tested by comparing the predicted skull outlines for any individual with a profile of that same individual at an older age.
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> . . . . [We divided] each profile into 5-degree sectors and [computed] a single average for the radial coordinates of every point within each sector. In comparing two profiles, we measured the difference between the averages obtained in homologous sectors as if they were homologous points. The average difference over all possible sectors provided a reasonable estimate of how much the profiles differed from one another. (Todd and Mark, pp. 73-75)

We notice, first, that all information not expressly relevant to their theory has been

discarded—not only landmarks but also, in the course of averaging, the local details of the profile by which landmarks are noted. Todd and Mark do not explain how they arrived at particular values of *b* to use in their predictions. Of the several possibilities, I think the following is the most reasonable. I believe they executed a regression of *older* radius upon *younger* radius and cosine azimuth, separately for each subject, according to conventional least squares:

$$r_{older} = r_{younger} + b \cdot (1 - \cos \theta) \cdot r_{younger} + \text{error}$$

According to the equations in the text, this regression has no constant term. Since cos 0° is unity, it follows that, apart from noise, the center has been fixed with respect to the top of the head throughout growth. Their system is equivalent to registering at the top of the head somewhere and orienting along the "horizontal"; the superposition, fortunately for their figures, does not require attention to the "center" at all.

The fractions of variance reported by the authors are appropriate for testing whether *b* is zero. Now, aside from technical problems in the sampling of θ (as the profile is not closed), a test of *b* is nearly equivalent to a test of the correlation between change in radius and cos θ. Since cos θ is proportional to the height above the center of a point at constant distance from the center, we are, in effect, testing whether increase in radius itself increases as one goes down through the head. But this is easily deduced from the most basic craniofacial lore. The jaw grows faster than the cranium after the age of 4. Then growth away from any "center" near the middle of the head will be noticeably faster below than above, especially since growth directly above the center is constrained by the model to be exactly zero. The presence of the coefficient *b* must be significant, once the brain has reached adult size. The "impressive" variance statistic argues only that size change continues below and therefore fails to test the model in any of its particulars. Such tests would require identification of landmarks severally, followed by checks on the invariance of angles and on specificity of radial dilation rates. (For instance, does the center really stay fixed relative to the top of the head? Apparently not, as the authors disobey their own text by failing to superimpose at the tops in Fig. 4, *E, K, M, N,* and *O*. The horizontal width of the head at the forehead is systematically overestimated. What does this say about the transformation?) Such queries could be spun out for some time and might even lead to an improvement of 0.01 or 0.02 in the variance "explained." But this sort of optimizing is fruitless as long as the model does not confront a biologic homology function. The authors have explained the wrong variance.

Growth analysis: The scientific agenda

> The prediction of craniofacial growth involves four central issues: (1) What frame of reference should be adopted for measuing change? (2) What type of coordinate system should be used? (3) How should the change be described? (4) How can the change be explained biologically? (Todd and Mark, p. 63)

I would place a superordinate question at the head of this list:

> What do we need to know before we can measure change?
> On what data must we insist?

Todd and Mark abstract form quite stringently into a single irregular curve beginning and ending arbitrarily, bearing only two landmark points and with an interior devoid of any information at all. One cannot expect to model growth effectively by analysis of data this sparse. There is simply not enough information at hand. "Growth" of the profile could proceed from any center at all as long as we covary the azimuths appropriately with radii. Since there is nothing in the data permitting a check upon these postulated movements of points, the Todd-Mark framework is geometrically null. To be able to uncover growth

invariants of any sort, we need additional information specifically biologic—the homology map of earlier profiles onto later profiles. In describing this homology, the transformational point of view does not permit one to discard the interior of the profile. Whether or not there is a center, the regulation which coordinates growth throughout the head must be expressed in mutual constraints transmitted through a most inhomogeneous medium of bones, sutures, capsules, and space. The serious study of craniofacial growth by any model more sophisticated than hydrostatics requires data about biologic homologies throughout whole plane sections. The authors do not seem to address this issue.

It is as a consequence of this lapse that the authors settle on four "central issues" that are instead, I feel, flaws of method.

1. There is no need for any a priori "frame of reference" for measuring change. It is points, not coordinates, which bear the homology function we need to analyze. Just as distances and angles are measured independently of coordinates, so may the analysis of change proceed quite independently of the coordinate systems chosen for the images separately.

2. For similar reasons, the issue in prediction is not the identification of a coordinate system but, rather, the extraction of invariants from completed growth data, invariants whose constancy can drive an extrapolation in any coordinate system. Neither a suite of vectors describing movement of a profile out of a center nor a biorthogonal grid detecting directions of maximum and minimum changes of length throughout homologous areas can do more than summarize an observed change. Changes are predictable only when these summaries are regular over persons and over time.

3. Todd and Mark reduce growth to a simple function of azimuth, θ. They can manage this only by restricting themselves to nearly landmark-free profiles, so that there is nothing left in the data to contradict the reduction. But all realistic data sets are too complex to be so reduced. While movements have two degrees of freedom, the general deformation has three. And beyond deformations as objects of study there are more general geometries of form change—slip, plastic flow, creation of coordinate mesh—not yet formalized in any of the branches of mathematics applied. These and other changes cannot be modeled until they are first recorded in the biologic homology map expressed in any suitable coordinate system. All prediction is based in the further analysis of these maps.

I come finally to the authors' fourth question. Todd and Mark see radial growth from a center. That is their theory. Therefore, they measure only a radius function, standardized in a most inappropriate way. This decision, I believe, has rendered biologic explanation impossible. There must be biologic information in the data if it is to confront any biologic explanation. The data by which we study change are the raw maps of deformation, the biologic homology function. Each growth record is a continuum of histories of local deformations throughout the form. A proper description respecting the map must quantify differential growth rates among many points and in many directions independently, based on the behavior of the map within large sets of landmarks and substructures. It is premature to hypothesize particular global regularities of craniofacial growth. Neurocranium, splanchnocranium, and mandible grow according to different regimens and schedules and are subject to different anomalies. The intractable facts have been known since the nineteenth century. Todd and Mark ignore all this literature of differential growth in favor of the one-parameter "revised cardioidal strain."

If we are to develop a technology of growth prediction, it will come from painstaking computation of coordinate systems from the behavior of growing form, from observation and verification of natural invariants and the clinical interventions designed to alter them, and from analysis of differential growth in all the geometric detail of which we are capable.

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