ESSAY REVIEW

HOW TO VIEW THE EVOLUTION OF THE TETHYS: AS A SLIDE SHOW OR AS A MOVIE?

A review of *The Evolution of the Tethys in China and Adjacent Regions;* by Huang Jiqing Chen Bingwei. P. 187 (English and Chinese bilingual text. Beijing, 1987 (Geological Publishing House, $30.00).

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INTRODUCTION: WHAT IS TETHYS AND WHAT IS IT NOT?

Ever since it was invented by Eduard Suess (1893, p. 183), the concept of Tethys, an equatorial ocean that had been ancestral to the Alpine-Himalayan mountain ranges, has remained dominant in considerations of the tectonic and paleogeographic evolution of Eurasia. Suess coined the term in 1893 to designate a former *ocean* in a discussion of the permanence of ocean basins. The contractional demise of the Tethys to create a major mountain range by fusing together the former portions of Gondwana-Land and the Angara continent was Suess' example to show the untenability of Dana's view of the permanence of ocean basins and continents. Suess' Tethys concept was thus a *tectonic* one from the very beginning, albeit founded on the previous palaeogeographic concept of the "Central Mediterranean" by his son-in-law Melchior Neumayr (Şengör, 1984, 1990a; see also de Margerie, 1900).

Later when Emile Haug developed the "European" intracontinental model for geosynclines, Tethys became its type example (Haug, 1900, p. 631), despite Suess' well-founded protest (Suess, 1909, p. 722 and note 52). In his influential textbook, Haug further diluted the Tethys concept by using it interchangably for both tectonics and palaeogeography and from the Devonian to the late Cainozoic (for example, Haug, 1911, p. 726)! Most authors after Haug followed his example, creating much confusion, because Tethys thus came to cover nearly all marine areas from the Caribbean through the Appalachians (!) into nearly all parts of Eurasia and North Africa, between the late Proterozoic and the late Cainozoic. As such an all-encompassing term is hardly useful, it was given numerous qualifiers and prefixes connoting age and location, creating an impenetrable thicket of Tethyan jargon from the 1920's onward (Şengör, 1984, p. 3–7).

This unsatisfactory situation underwent an apparent dramatic improvement when Bullard, Everett, and Smith (1965) reconstructed the circum-Atlantic continents to their pre-drift positions by holding them rigid. They found it "noteworthy that the reconstruction shows large gaps in the Caribbean and the Mediterranean, which is just where they

would be expected in view of the considerable Mesozoic and Tertiary deformation in these regions" (Bullard, Everett, and Smith, 1965, p. 49–50). Earlier, but less rigorous, Pangaean reconstructions (for example, Carey, 1958; Wilson, 1963) had already identified the eastward gaping enormous embayment with Suess' Tethys, and the first plate tectonics-based Pangaea reconstruction followed suit (Dietz and Holden, 1970), thus confirming Suess' original view of a late Paleozoic to late Cainozoic *ocean,* whose obliteration had created *only* the Alpine-Himalayan system.

Despite this apparent breakthrough in understanding, however, the problem of Tethys was not to be shelved yet! Not only did the plate tectonics-based Tethys concept run into difficulties when confronted with a Permo-Triassic rifting of nearly all the past ocean segments that have since vanished along the Alpide sutures around the Mediterranean Sea, in the Zagros Ranges, in Oman, and in the Himalaya (see Şengör, 1984), but also many, especially paleobiogeographers and stratigraphers, hung onto old habits by calling all Phanerozoic equatorial seas, in the Old World and in the Caribbean, Tethys (for example, see the papers in Adams and Ager, 1967). Tozer's (1989, 1990) recent defense of a paleogeographic Tethys defined as the northern Gondwanian shelf since the Cambrian shows how far and with what conviction some have deviated from Suess' original intent.

Although the 80's witnessed a significant surge of interest in Tethyan geology, emphasized by the organization of a large number of international projects involving most of the countries located along the Tethysides as well as those outside it, there seems still little consensus among the Tethyan workers as to what is and what is not "Tethys." The monograph by the laureate dean of Chinese geologists Professor Huang Jiqing (T.K. Huang) and his co-author Chen Bingwei on *The Evolution of the Tethys in China and Adjacent Regions* therefore begins with the definition of Tethys that Suess gave in the third volume of his classic *Das Antlitz der Erde* (*The Face of the Earth;* Suess, 1901) "in order to show that his definition is not out-of-date, but is still valid in its essentials" (p. 1).[1] Huang and Chen insist that "The idea of some geologists that the Tethys began to take form in the early Paleozoic and thus it comprized the Mongolian and Central Asian geosynclinal regions, should be abandoned,"[2] although they also emphasize that despite Suess' frequent reference to the Tethys as "a broad zone of marine deposits of Mesozoic age" its story really began properly in the Permian, as, they point out, Suess also implied by reference to the essentially Permian Gondwana flora. Huang and Chen are right in thinking that Suess thought that the Tethyan history had already begun in the late Paleozoic, for, in a number of places in the

[1] The page numbers cited by themselves in this review all refer to those in Huang and Chen's book, unless otherwise specified.

[2] This may be why they chose not to cite Suess' 1893 paper, in which the term Tethys was first proposed in a vague association with the ?Cambrian through Mesozoic marine sequences of the Himalaya. See Şengör (1990a).

Antlitz, he explicitly mentioned "Tethyan sediments" even as old as Upper Carboniferous (see Tollmann, 1984, p. 95–96, for quotations and discussion).

This insistence on sticking to Suess' own definition of a tectonic Tethys concept, valid from the late Carboniferous to the late Cainozoic, has more to it than a respect for priority or scholarly pedantry: it is a practical concept that can be defined very precisely now on the basis of the Tethyside suture zones (for example, Şengör, 1986, figs. 2 and 3). Tethys, in our present usage, thus becomes the ocean (or oceans) that vanished along the present Tethyside sutures and that correspond(s) with one or more Pangaean embayment(s) that originated (or remained unclosed) after the late Paleozoic collisions gathered together the major elements of the late Paleozoic Pangaea. The usefulness and practicality of this definition stems from the fact that, whereas sutures may be mapped in the field with relative ease and precision, former water bodies, that is, paleogeographic "seas," are much more difficult to define. In my discussion of Tozer's paper (Şengör, 1990a), I pointed out that even the definition of present-day oceans as *water bodies* is based partly on physical oceanographic criteria that are hard to reconstruct from the fragmentary deposits former oceans have left behind and partly on convention on the basis of geographical convenience. Adherence to Suess' original tectonic definition avoids the well-nigh insurmountable difficulties in circumscribing now vanished water bodies and enables us to give Tethys an unequivocal meaning by relating it to concrete, well-defined objects mappable in the field.

PERMIAN STRATIGRAPHY AND THE DELINEATION OF THE LATE PALEOZOIC TETHYS

Having followed Suess in defining the temporal limits of the Tethys as between the late Paleozoic and the late Cainozoic in their Introduction, Huang and Chen devote the first numbered chapter of their book to "Reclassification of the Chinese Permian and Subdivision of the Permian Tethys," with the object of being able to define with more precision the various paleogeographic realms in "the Tethys." In this, they follow von Gümbel's (1888, p. 624–628) lead in dividing the Permian into three series. The following table (table 1) shows Huang and Chen's classification and its biostratigraphic basis:

This tri-partite subdivision is then correlated with Waterhouse's (1976) global classification. Although Huang and Chen's classification and the rationale behind it do not strike one as unusual, there are nevertheless a few questions that come to mind. First, one wonders what is the significance of their insistence on the definition of the lowest Stage of the Permian on the basis of the *acme zone* of the *Pseudoschwagerina*? In the classical Darvaz and Pamir sections in the southern USSR, Leven (1975) has pointed out that the base of the Asselian was defined by the first appearance of the genera *Schwagerina* and *Occidentoschwagerina,* and its top was defined by the introduction of the *Robustoschwagerina, Acervoschwagerina,* and several new species of pseudofusulinids, paraschwager-

TABLE 1

System	Series	Stages	Index Fossil
Permian	Upper Permian (Lepingian)	Changxingian	*Paleofusulina* zone
		Wujiapingian	*Codonofusiella* zone
	Middle Permian	Maokouan	*Yabeina* zone
			Neoschwagerina zone
	(Yangxinian)	Qixiaian	*Concellina* zone
			Missellina zone
		Upper Longyinian	*Chalaroschwagerina-Pamirina* zone
	Lower Permian (Qiannanian)		*Robustoschwagerina-Sphaeroschwagerina* zone
		Lower Longyinian	*Pseudoschwagerina* acme zone

inids, and pseudoschwagerinids. Thus the question is why choose a less rigorous definition for the Asselian, when a more rigorous one is possible? Secondly, it is not stated why *Lepidolina* is not employed together with *Yabeina,* which would have allowed a finer classification and a better correlation with many east Asian localities, especially since this fusulinacean is so abundantly present in South China, eastern Tibet, Indochina, and Japan (see Şengör and others, 1988, fig. 15). Finally, it is curious that no mention whatever is made of Leven's more recent (1981) tri-partite Permian classification and his correlations in the Tethysides between the Alps and the Pamir in a place where a Permian reclassification in the Tethyan realm is attempted, especially so since both Huang and Chen and Leven (1981) use fusulinaceans as index fossils (except for the Djulfian and Dorashamian, for which Leven uses ammonites), in contrast to Waterhouse's (1976) emphasis on brachiopods. It is also difficult to share Huang and Chen's confidence in employing fusulinaceans in the Upper Permian, where ammonites are clearly a more reliable guide (compare Zhang, 1984, table 1; Yang, Cheng, and Wang, 1986, tables 12-1 to 12-3).

Having established a biostratigraphic scheme for the Permian in China, Huang and Chen proceed to the paleoclimatic record of this Period in China and adjacent regions and divide the Tethys into a northern tropical warm water Tethys and a southern Tethys characterized by a cold- to cool-water environment in the early Permian, but one that also became warm tropical in the middle Permian. Here Huang and Chen depart from Suess' tectonic definition and use the term Tethys interchangeably for paleogeographic and tectonic purposes. The result is utter confusion! Their Permian Northern Tethys thus includes the "Zechstein Sea" in Europe (which had only a narrow connection with the Tethys through the Polish-Dobrudjean Trough; Ziegler, 1988, pl. 8), the Ural "miogeosyncline" (also independent of the Tethys; Vinogradov, 1969, pl. 43; Ronov, Khain, and Seslavinsky, 1984, pl. 47), as well as the water bodies that covered much of Central Asia, the Yangtze, and the southeast China blocks (Huanan in the sense of Hsü and others, 1990),

plus Mongolia and Sikhote Alin! By including Central Asia and Mongolia into the Tethys, Huang and Chen here also go against their own recommendation.

A corresponding Southern Tethys, including Southern Europe, Balkans, Asia Minor, marginal (?northern) Africa, nearly the entire Middle East, the Hindu Kush (east or west? or both?), Central Pamir, Karakorum, Pakistan, Afghanistan, Tibet and marginal (?northern) India, Sinoburmania and Thai-Malaysia is separated from the northern one by a "Tethys Proper" including the Moesian Platform(!), Greater Caucasus, Kopet Dagh, Central Pamir, Sub-Kunlun, Songpan-Garze, Qinling, "West Yunnan Indosinian geosynclines," and Indosinia.

The intention of this subdivision was likely the separation of an oceanic "axial" Tethys from its northern and southern continental margins (for example, on p. 19 the "Northern Tethys" is described as "the southern marginal part of Eurasia"), but in their confusion of paleogeography with tectonics Huang and Chen include such entirely platformal areas as Moesia and Kopet Dagh into the "Tethys Proper." This confusion is evident even in their own inconsistent use of their own subdivisions. Already on page 11, for instance, the "Southern" and the "Northern" Tethys are considered "two parts" of the Tethys Proper, whereas on pages 26 and 27, they are once more considered distinct from the Tethys Proper!

Temporally Huang and Chen divide the Tethys into an exclusively Paleozoic Paleotethys, a Mesozoic Mesotethys, and a Cainozoic Neotethys "unlike the previous authorities" (p. 1), because "in stratigraphic terminology we used (*sic!*) to apply Paleo- for Paleozoic and Neo- for Cenozoic" (p. 63). I have discussed at some length elsewhere that such a strictly temporal division was used by many authors before and that it created a most unnatural and confusing nomenclature when applied to tectonic entities such as oceans and continents, whose lifespans do not necessarily coincide with era boundaries (Şengör, 1984, p. 7; 1986, p. 178, footnote). I show below to what further confusion it leads in Huang and Chen's monograph.

PALEOGEOGRAPHIC AND TECTONIC EVOLUTION OF THE TETHYS

After having defined the subdivisions of the Permian Tethys, Huang and Chen go on to use the paleoclimatic evidence in their "Southern Tethys" indicating a middle Permian warming to infer a wholesale northerly drift of the entire Gondwana-Land to eliminate completely what they call Paleotethys! Not only do they thus disregard the end of the Gondwanian glaciation toward the end of the early Permian[3] (for example, Ziegler and others, 1979; Crowell, 1983), but they also ignore the

[3] Later in the book (chapter VII, section 7, entitled "Importance of Glacial Deposits and Permian Floras for Interpretation of the Evolution of the Tethys") Huang and Chen do consider whether the global warming in the middle Permian may not have been the cause of the change in the paleoclimatic record and decide that it may not, because the mixing of Gondwana and Cathaysia floras in some localities in northern Gondwana-Land implies, according to Huang and Chen, a wholesale northerly drift of Gondwana-Land anyway!

evidence for a late Permian rifting of the Cimmerian Continent from Gondwana-Land (see Şengör and others, 1988; Şengör, 1990b, for a review of the evidence).

A "Variscan orogeny" is thought to have resulted from the collision of Gondwana-Land with Eurasia along "the major Palaeotethyan suture" which Huang and Chen draw along a line passing through Longmuco (centered on about 80°25′E, 34°37′N: Tsaggar tso on Hedin's maps: Hedin, 1966, sheet NI-44; for synonymies see Farquhar, Jarring, and Norin, 1967) and Yushu (97°E, 33°N), although neither Norin (1946) nor the more recent Franco-Chinese expedition (Matte and others, 1990) found any evidence for a late Paleozoic orogenic deformation near Longmuco.[4] Huang and Chen also point out that "late Variscan folding has not yet been found along the Sub-Kunlun zone," but they ascribe this to lack of geological mapping or to Triassic cover hiding the evidence! (p. 21.)

Huang and Chen base their "Variscan orogeny" on unconformities between Middle and Upper Permian in Yushu, Batang (99°10′E, 30°N) and Derong (99°20′E, 28°40′N), that is along the Jinsha Jiang suture, although unconformities between the Lower and Middle Permian and between the Upper Permian and the various levels of the Triassic are indicated to be present also. The movements were accompanied by granodiorite intrusions and eruptions of andesite, the entire process being described as "Pacific-type" (p. 19), but Huang and Chen still think that these were all manifestations of a collision, although in the late Triassic they describe Indosinian "andesitic volcanic island arcs" from the same region (p. 37). They do not mention, however, the thick Lower Triassic andesites and basalts from the same areas (for example, Bally and others, 1980, table 4), which would have destroyed the neat—but unreal—Variscan/Indosinian distinction they maintain. In fact Chang, Pan, and Sun (1989) have argued for continuous late Paleozoic to late Triassic subduction along the Jinsha Jiang suture on the basis of a continuous Permian to Triassic flysch deposition, mélange formation, and arc magmatism. I believe that the data, even those described by Huang and Chen, are more comfortably interpreted in terms of the continuous Permo-Triassic subduction model.

Huang and Chen seek to support their interpretation of a late Permian collision between Gondwana-Land and Eurasia on the basis of the occurrence of mixed Cathaysia and Gondwana floras on undisputed Gondwana-Land territory (for example, southeastern Turkey, Saudi Arabia, Kashmir), arguing that this indicates a rapid northward drift of the entire Gondwana-Land, which culminated in its collision with Eurasia in the Permian. Even if one does not take into account the difficulties

[4] Matte and others (1990) also draw the Paleo-Tethyan suture (of late Triassic or early Jurassic age) through Longmuco, although it must be drawn farther north, since the clearly Gondwanian Tashliq-köl section of Norin (80°40′E, 34°43′N) lies at least 6′ north of it, and it remains unclear where the Eastern Loqzung Mountains (along about 35°N) must be assigned, the oldest rocks exposed there being of Cretaceous age (Norin, 1946).

associated with paleofloral identification, Huang and Chen's argument is still based on a very simplistic interpretation of the Permian phytogeography, not recognizing the dominant climatic controls that changed drastically during the course of the Permian Period. Ziegler (1990) recently showed how misleading it is to correlate floral realms, which he analyzed in terms of climatically defined biomes, with continental assemblies on a one-to-one basis. This mistake regrettably is made very often not only in China but along the entire Tethysides and has long confused paleotectonic interpretations in such places as the Caucasian realm (see Belov, Gatinsky, and Mossakovsky, 1986).

In the three chapters following the Permian evolution Huang and Chen discuss, successively, the "Triassic palaeogeography and Indosinian tectonism" (chapter IV), the "Jurassic-Cretaceous palaeogeography and plate motions in the Tethys Proper" (chapter V), and the "Tertiary palaeogeography and plate motions in the Tethys Proper" (chapter VI). In these chapters the "Tethys Proper" refers to the Neo-Tethys of common usage, that is, the ocean that closed along the Alpide sutures, including the Indus-Yarlung suture. Huang and Chen let Neo-Tethys open in the late Triassic, for this opening is viewed as a counterpart of the Indosinian orogeny in the north. Huang and Chen also let a narrow ocean reopen and close back in the Triassic north of Neo-Tethys, along the main Paleotethyan suture zone, to be able to explain the Indosinian tectonism in northern and eastern Tibet!

The Banggong Co-Nu Jiang ocean is believed by Huang and Chen to have opened in the early to middle Jurassic, because this is the age of the ophiolites that are preserved along its suture. Others (for example, Chang, Pan, and Sun, 1989), however, noted a differentiation between the northern and the southern margins of this former ocean already in the Triassic, in better agreement with its along-strike continuations in the Rushan-Pshart (Central Pamir) and Waser (Central Afghanistan) zones to the west (see Şengör and others, 1988) and in Burma to the east (Mitchell, 1989).

Interestingly, Huang and Chen do not recognize the division of the north Tibetan Qangtang block into two segments along the Chasa suture (see Şengör and others, 1988), even though the Permian palaeoclimatic data as displayed in their figure 22 clearly show the absence of glacial deposits of Permian age in the eastern Qangtang block.

Huang and Chen let the Indus-Yarlung suture close in the late Cretaceous, "no ophiolites having been formed later," to terminate the history of what they call "Mesotethys," and view the later deformations in the Himalaya as intracontinental shortening (so far as I know only Jaeger, Courtillot, and Tapponnier, 1989, would agree with them!). Their Neo-Tethys thus existed not in the Himalaya but farther west along the present Zagros and in Oman.

A major conclusion of Huang and Chen's analysis of the history of Tethys is their recognition of an "Interchange Domain" comprizing

those crustal elements located between the northern and southern main suture zones of the Tethys. They point out themselves that this is nothing more than Stöcklin's "Central Domain" (Stöcklin, 1977) and Şengör's "Cimmerian Continent" (Şengör, 1979), but according to Huang and Chen, the "interchange Domain" never was an independent entity: rather it moved north as an integral part of Gondwana-Land, and, after its collision with Eurasia in the late Permian, it stayed as a part of the latter when the Indus-Yarlung rifting removed the rest of Gondwana-Land at the end of the Triassic! This is a "neat" schema, but one difficult to justify in the face of such abundant and sundry data, showing that rifting was clearly underway—and in such places as Oman already may have created an ocean (Blendinger, 1990)—along the future southern suture zone, while subduction-related tectonism and magmatism was still active along the northern suture zone (for sources of data, see Şengör and others, 1988; Chang, Pan, and Sun, 1989).

In the last two chapters, Huang and Chen discuss some of the problems concerning the extent of the Tethys in China (chapter VIII) and the "American Tethys," the existence of "Pacifica," and the Indo-Pacific Tethys (chapter IX).

In chapter VIII Huang and Chen discuss, among others, the question of the existence of a Western Ordos-Greater Khingan suture postulated by Klimetz (1983) and Şengör (1984). As far as the Western Ordos part of this suture is concerned, most researchers— including its originators—would now agree with their negative answer to this question, although neither Huang and Chen, nor anyone else, has yet provided a satisfactory explanation for the strong, east-vergent deformation of late Jurassic-early Cretaceous age here, known since the explorations of Obruchev (1894) and Teilhard de Chardin and Licent (1924, especially figs. 5 and 5bis), and confirmed and amplified by recent petroleum exploration (see especially the seismic reflection profiles in Yang and Zhang, 1987, showing far-reaching east-vergent nappes!). Huang and Chen's contention that no suture exists along the Greater Khingan (Da Hinggan) is contradicted, however, by the persistence into the early Permian of a Hinggan-Mongol deep sea trough with abundant volcanism of intermediate composition (see Wang, 1985, p. 78), along the present suture of which Upper Paleozoic ultramafics also have been mapped (Anonymous, 1973, map 11; Geological Map of the People's Republic of China, 1976). The Greater Khingan suture thus does exist and connects with the latest Paleozoic suture zone in Inner Mongolia (Wang and Liu, 1986). Whether this should still be considered a Tethyside suture is a question that can only be answered once the late Paleozoic suture distribution in northern Gansu and easternmost Xinjiang is cleared, which is regrettably still not the case.

Huang and Chen's discussions on the "American Tethys" are further examples of confusing paleogeography with tectonics. The equatorial flora and fauna in some of the exotic continental and oceanic slivers in the

North American Cordillera are interpreted as being transported across the Pacific as a result of the partial dispersal of Nur and Ben-Avraham's (1979) "Pacifica." Being faced with the difficulty of accommodating a sizeable Pacifica in the Permian world, Huang and Chen conclude that it must have foundered largely through basification *à la* Beloussov (1962)!

CONCLUSION: HOW TO INTERPRET THE HISTORY OF THE TETHYS

All in all, I am afraid that the book by Huang and Chen is of limited use for furthering our understanding of the Tethyan history. It relates the story of the Tethys in China on the basis of an extremely and arbitrarily selective and inadequate database, gives few particulars of the data that are presented, and, in view of the huge area considered and the enormous amount of available information, cites few references (only 120 references are cited to work by Chinese geologists in the People's Republic, of which 19 are to the authors' own papers).

On the more positive side, it provides a succint account of *one* Chinese interpretation of the Tethys that would be useful for those who need to make use of the Chinese regional geological literature. I cannot emphasize too strongly, however, that this is not *the* Chinese view, but only one of a fairly large number. Perhaps another positive aspect of the book is that it does not follow the fashion of subdividing the Tethysides in China into innumerable mute "terranes," which inevitably would have rendered the narrative much more opaque. Huang and Chen do not hide their views behind a non-committal nomenclature, thus making argument possible and easy, as this essay attests to. For instance, they draw the main Paleo-Tethyan suture (their "Tethys Proper") in Thailand through Chiangmai (see their fig. 4). This is not how most workers have drawn it, but there seems to be evidence now in favor of placing a Paleo-Tethyan suture through Chiangmai (A.H.G. Mitchell, in preparation.) Had Huang and Chen only identified a "terrane boundary" through Chiangmai—which could have been any sort of fault, or facies boundary, or indeed a suture!—further discussion of such a vague and non-commital statement would have been difficult.

The theoretical basis of the book is a somewhat confused agglomeration of elements of a number of old fixist models (especially Stille's and Beloussov's) and plate tectonics. Both in the evolutionary model outlined and in the terminology employed, internal inconsistencies abound that the authors seem not to have noticed. Observations unfavorable to the authors' model, when mentioned at all, are explained away by ad hoc assumptions.

Huang and Chen's book shares most of the shortcomings listed above with not a small number of recent Tethyan syntheses, but I believe the most serious drawback of this book, which it also shares with many post-plate tectonic Tethyan syntheses, is its conceptual basis. Huang and Chen seem not to have taken on board one of the most fundamental messages of global comparative tectonics (both new and old!), namely

that the tectonics of one region cannot be considered in isolation from others. Their view of Tethys, despite the plate tectonic interpretations they attach to it and despite digressions on the Pacific and the western North America, is fundamentally one that is parochial and, partly as a consequence, static. The "evolution" they relate jumps from one "time-lapse frame" to the next without providing a continuity of process. This results from confusing the record of the history of the Earth with that history itself. Wegmann (1950) once expressed this as the contrast between the continuity demanded by rational thought and the discontinuity of the geological documentation. He also pointed out that although without discontinuity, without "breaks" in sequences to function as markers, there would be no stratigraphy, without continuity there would be no complete picture. "The geologist" he wrote, "can always comprehend only a part of the phenomena and therefore must establish connections between the disjointed parts through hypotheses, if he wishes to obtain a complete picture" (Wegmann, 1950, p. 132). Herein lies the difference between those who think that the record is the history itself and those who believe that the record must be interpolated to reconstruct the history.[5] The first group collects observations and then tries to fit to them segments of diverse models, so long as the accounts of observations they possess remain intact. The second group contemplates observations with the intent of fitting to them all one all-encompassing model. If this cannot be done, the second group will mistrust the observations and will check them, or at least propose that they be checked. The second group emphasizes fitting the record to processes, while the first insists that processes must be fitted to the record. In Şengör (1991) I discussed at some length the rôles of these two groups in the history of tectonics and do not wish to elaborate on it here, but in every aspect, Huang and Chen's book is an excellent example of a product of the first group. The failure of orienting one's mind according to processes in considering historical geology is currently one of the most serious problems that beset studies in Tethyan geology. Unless processes are considered in space and in time in detail, many more books such as the one under review here will be inevitable, books that mistake a slide show for a movie.

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[5] For an excellent discussion on the controversy between those who believe the record should be read literally and those who think that the record should be interpolated by theoretical deductions see Gould's (1979) article on Agassiz. It is an outstanding essay showing the vast philosophical gulf separating not infrequently excellent scientists and underlining that this significant difference in opinion should not be masquerading as one between "good guys" and "bad guys."

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