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This paper is dedicated to Professor John F. Dewey in grateful recognition of his seminal contributions to the theory of orogeny in terms of plate tectonics.

Plate Tectonics and Orogenic Research after 25 Years: A Tethyan Perspective

A.M. CELÂL ŞENGÖR

ABSTRACT

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Orogeny, the process by which the earth's prominent mountain ranges are constructed, has been a central topic of interest in the earth sciences since at least the end of the 18th century. The recognition that strains and displacements of very considerable magnitude occur along all of the three dimensions within an orogenic belt during its evolution has grown gradually during the last two centuries. Emphasis on primary vertical movements dominated the ideas on the nature of orogeny during the first half of the 19th century, whereas compression and consequent uplift across mountain belts were believed to be the main cause for their origin during the subsequent one hundred years or so that were spent under the dominance of the fixist contraction theory. Mobilist tectonicians realised that continental drift in places also required motion along the trend of orogenic belts, but this view did not gain general acceptance. Recognition of significant strike-slip motion parallel or subparallel with mountain ranges evolved independently and mostly within the fixist camp. By the 1960's presence of important motions both along and across mountain belts had become common knowledge, but no theoretical basis existed to account for them all.

Plate tectonics was first described by J.T. Wilson in 1965 and was applied to orogeny a year later also by Wilson. He made it clear that orogeny resulted from convergent plate motion, but important sideways motion along orogenic belts was also believed to be an integral part of orogenic processes. Wilson's interpretation has clarified the uncertainties concerning the nature of orogeny by associating it with a definite process (or group of processes), namely plate convergence, for which there is a good uniformitarian basis. In this paper *I therefore define orogeny as a collective term for convergent plate margin processes*.

The application of plate tectonic concepts to orogenic research has caused a great progress in our understanding of how the earth's mountain ranges and its continental crust have developed. In this paper I consider orogenic belts first in *cross-section*, than in *map view*, and finally in *time*, mainly on examples from the *Tethysides*.

Investigation of orogenic belts along cross-sections reveals that there are a large number of *types of orogenic belts*. These are subdivided into four main orders (*transpressional, subduction-controlled, obduction-controlled* and *collision-controlled orogens*) consisting of two superfamilies, eight families, and twenty genera. Beneath the generic level, the distinguishing characteristics are emphemeral and different species of orogenic belts do not form fundamentally different structural entities. That is why the types of orogenic belts are discussed in this paper on the generic level.

Transpressional orogenic belts are distinct in being influenced by their bounding transform faults that generate peculiar structural and thermal features. *Subduction-controlled orogenic belts* are probably governed by convergence rate, slab dip down to 100 km, and the motion of the overriding plate with respect to an asthenospheric reference frame and the movement of the subduction hinge (trench-roll) in the same reference system. The latter is a function of the buoyancy gradient perpendicular to trench and thus generally of the age gradient (either generation or, if present, rejuvenation age gradient) of the underriding plate. Variations in these parameters create a tremendous variety of subduction-controlled orogenic belts, the end-members of which are *extensional* (Mariana-type), *neutral* (Sumatra-type), and *compressional* (Andean-type) arcs. *Obduction-controlled orogenic belts* frequently arise from the attempted subduction of continental crust under oceanic lithosphere. Many of the large, pre-collisionally obducted ophiolite nappes consist of ocean floor formed above subduction zones. *Collision-controlled orogenic belts* form a very large group, but only two superfamilies are distinguished. Those with overriding continental nappes as the highest tectonic unit are called continental-override-type collisional orogens (COB) and generally stem from the closure of small oceans (< 1000 km width) and those without are called non-continental-override-type collisional orogenic belts (NCOB) and

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commonly form from the obliteration of large oceans (> 1000 km width). COB's and NCOB's are here referred to as 'Alpine-type' and 'Himalayan-type' collisional orogen superfamilies, respectively, after their best-known representatives.

Cross-sectional investigations of orogenic belts show that cross-sectional area during orogeny is not conserved. When we look at orogenic belts in map view, we find that an absolute minimum of 60% of them (by length) display significant strike-parallel motion that leads also to non-conservation of cross-sectional area during orogeny. I conclude that rigorous balanced cross-sections across entire orogens are impossibly difficult to draw and that their construction would require a complete and detailed knowledge of nearly the entire evolution of the orogen.

Large orogenic belts are commonly made up of orogenic collages of microcontinents, island arcs, and accretionary complexes. Such *primary orogenic collage components* may be extensively disrupted to form *secondary orogenic collage components*. The recently-developed 'terrane analysis' is compared with the early concepts of Alpine nappes and is found to be a retrogressive step because of its disclaim of most genetic connotations and because it confuses primary orogenic collage components with secondary ones.

The temporal aspects of orogeny have been debated for over 200 years in terms of continuity vs. world-wide synchronous episodicity. Plate tectonics seems to have provided a consensus for continuous orogeny consisting in any one place of numerous local episodes of deformation.

The tremendous complexity of orogenic processes and the multifarious nature of environments that produce orogenic belts (all located along convergent plate boundaries) render the continuing employment of the term *geosyncline* unnecessary and misleading. I recommend that this term be dropped from our current scientific nomenclature.

I conclude that there are as yet no shortcuts to establishing the kinematics of continental deformation (comparable with the plate tectonics of the oceans) except by the traditional methods of geology, such as geological field mapping aided by relevant geophysical methods. Plate tectonics has given us a new framework in which we can investigate orogeny, but it has not made the job of orogenic geologists any easier. The peculiarities of the continents are such that the simple and elegant rules of plate tectonics are smeared to gross distortions and in places even vanish completely. Plate tectonics has taught us where the limits of our knowledge may lie and thus has enabled us to set realistic goals in orogenic research for the future.

A.M. Celâl ŞENGÖR, born in 1955 in Istanbul, studied at the State University of New York at Albany, USA, completing a PhD dissertation in 1982 on the structural evolution of the Albula Pass, eastern Switzerland. Since then he has been a member of the Faculty of Mines of the Istanbul Technical University (İ.T.Ü.). His main research interest is the tectonics of mountain belts, especially of the Alpine–Himalayan system. He is currently the chairman of the Tethys Panel of Working Group 2 of the International Lithosphere Project. He is an Associate Editor of *Tectonics* and the *Geological Society of America Bulletin* and is an editorial advisory board member of the *Journal of Structural Geology*, *Tectonophysics*, *Eclogae Geologicae Helvetiae*, *Earth Evolution Sciences* and the *Bulletin of the Technical University of Istanbul*. In 1989, he held a Royal Society Visiting Research Fellowship at the Department of Earth Sciences of the University of Oxford. Şengör received a President's Award from the Geological Society of London (1984), the Science Award of the Turkish National Research Council for Science and Technology (1986), and a D.Sc. (*honoris causa*) from the University of Neuchâtel (1988). He was also elected a Korrespondent of the Geologische Bundesanstalt in Vienna (1989). His present address is İ.T.Ü. Maden Fakültesi, Jeoloji Bölümü, 80626 Ayazağa, Istanbul, Turkey.

FOREWORD

This paper was originally prepared for publication as a part of the Proceedings of the 25th Anniversary Symposium of Plate Tectonics held in April 1987 at the Texas A&M University. A first typescript was completed in Autumn 1987 and two amendments were made subsequently as publication was delayed. Because the Proceedings showed no sign of getting close to publication, I have finally decided to retract my typescript for publication elsewhere, lest it became totally redundant. This explains why the text reflects my perception of the state of the art as of end-1987. My later amendments consisted of very inadequate incorporation of a part of what I considered essential, but had I written the text anew now, in a few of the subjects covered my emphasis would have been slightly, but not essentially, different. A few important changes concerning the interpretation of particular examples, I indicated in new footnotes. The last amendment was done in Oxford, where I held a Royal Society Visiting Research Fellowship, and it was typed by Sally Thompson.

INTRODUCTION

Importance of orogenic research

"... to group the folded ranges together in natural units of a still more comprehensive character, and to explain by means of a single, simple expression as large a part as possible of the terrestrial folding [1]—such is the task which now awaits the geologist. The *plan of the trend-lines*, written by nature on the face of the earth—this it is which he has to determine" (Suess, 1908, p.3) [2]. This is what Eduard Suess, the great Austrian geologist and the founder of modern tectonics, identified as the main task of contemporary geology at the beginning of our century. Now, more than three quarters of a century later, we are in a position to claim with some confidence that we have finally found that "single, simple expression ... to explain ... the terrestrial folding": that single, simple expression is the theory of plate tectonics. Models based on it have more successfully accounted for, in an order of decreasing success, the placement on the face of the earth, the evolution, and the internal structure of orogenic belts than any other theory proposed so far. We now know that orogenic belts form along plate boundaries. We also

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[1] The term *folding* is here used by Suess in the sense of *orogeny*. Orogeny did not become a popular term until the end of the first decade of the 20th century, despite earlier common usage by such influential French and American writers as de Lapparent (1893, p. 1508ff) and Dana (1894, pp. 345, 380ff), respectively. *Folding* (with its equivalents *Faltung, plissement*, and *skladtchatost* in German, French and Russian, respectively) was widely employed instead and this usage, along with orogeny and orogenesis, lasted well into the 20th century. In *Grundfragen der Vergleichenden Tektonik* (Basic Problems of Comparative Tectonics), Stille devoted an entire section to the explanation of the concept of *folding* which he defined as "orogenic compression of regions on earth" (Stille, 1924, p. 244; see also Grabau, 1940, "mountain chains arise by folding" of the strata deposited in a geosyncline, p. 48, also fig. 14). This double sense of the word *folding*, i.e. both for folding sensu stricto and for orogeny, along with the terms *folded belts* or *foldbelts* for orogenic belts originated towards the middle of the 19th century, when all shortening in mountain belts was ascribed to folding. It gradually faded away during the first decade of the second half of this century, when it was realized that structures other than folds probably accounted for much more of orogenic shortening (e.g. Longwell, 1945). However, even now one occasionally encounters the terms foldbelt or folded belt for orogenic belts (e.g. Bally et al., 1979; Bally, 1981). This usage ought to be discontinued, though, for not all 'foldbelts' are orogenic belts (e.g. the Yakima 'foldbelt' in Washington State, USA).

[2] Throughout this paper references to Suess' *Das Antlitz der Erde* are to the authorized English edition, *The Face of the Earth*. The original dates of publication of its four volumes and those of their English editions (in parentheses) are as follows: I, 1883–1885 (1904); II, 1888 (1906); III/1, 1901 (1908); III/2, 1909 (1909). In the English edition, the original volumes III/1 and III/2 were issued as volumes III and IV, respectively.

know that they grow during subductive removal of ocean floors and subsequent continental collision, although in most cases we are not quite sure to what extent components of motion other than convergent (e.g. transcurrent or even divergent) may also have been involved. Finally, we believe that the internal structures of orogenic belts largely result from relative plate motion, but as yet we seem to be far away from having established a one-to-one correspondence between plate motions and the origin of structures within orogenic belts (e.g. Dewey, 1975; Beck, 1984).

Plate tectonics has not only solved Suess' problem, but also has brought to daylight, and/or suggested novel avenues of attack for, a number of other problems connected with orogeny. A very major part of the research endeavour in the earth sciences is today concentrated on problems directly or indirectly related to orogeny, largely because much of the geological history has consisted of a procession of life cycles of oceans, as Wilson (1968) suggested. Every one of such cycles inevitably culminates in orogeny and the resulting orogenic belt is commonly its only surviving—albeit very incomplete—record. Also, it has now become clear, largely through plate tectonics, that much of the continental crust, the main chronicler of earth history, is made and further speciated within orogenic belts (e.g. Taylor, 1967; Dewey and Burke, 1973; Dewey and Windley, 1981; Allègre and Jaupart, 1985; Burke and Şengör, 1986; Dewey, 1977; also see McKenzie, 1984; Harley, 1987). Thus, orogeny not only (albeit imperfectly) chronicles a major part of the earth history, but it also generates much of the material on which to record it. In a way, it is not only the observer and the scrivener, but also the paper-maker.

This prominent rôle of orogenic belts in providing fundamental information about the history of the processes governing the evolution of our planet was first noted by the famous Swiss naturalist and mountaineer Horace-Bénedict de Saussure (Fig. 1) possibly inspired by his countryman J.J. Scheuchzer's (and Scheuchzer's brother's) earlier observations published in the *Beschreibung der Natur-Geschichten des Schweizerlandes. 3. Teil: Schweizer Bergreisen* (Description of Natural-Histories of Switzerland, 3rd Part: Swiss Mountain Journeys, 1708) and by the statement of his fellow citizen J.A. de Luc "that it is in the mountains and nowhere else should one study natural history" (de Luc, 1778, p.127). In the preliminary discourse of his *Voyages dans les Alpes* (Travels in the Alps), de Saussure observed that "it is above all through the study of mountains the progress of a Theory of the Earth can be accelerated. Plains are uniform; it is impossible in them to inspect a section of the earth and its different beds except by excavations effected either by water or by the hand of man... High mountains, on the contrary, infinitely various in their material and their form, present to the light of day natural sections of great extent, in which one can observe with the utmost

Fig. 1. The Swiss naturalist and mountaineer Horace-Bénedict de Saussure who initiated the tradition of research on orogenic belts.

precision, and embrace in a moment, the order, the attitude, the direction, the thickness, and even the nature of the beds of which they are composed, and of the fractures (*fissures* = cracks) which traverse them. *It is in vain, however, that mountains offer opportunities for such observations, if the student does not know how to look on those great objects as a whole and in their more general relations* (de Saussure, 1779, p.II–IV, italics mine). [3]

Both of de Saussure's points, viz., (1) that we must emphasize the study of mountains (i.e. orogenic belts) if we wish to learn how the earth evolved and how it works, and (2) that if we are to understand the mountains, we need to look at them "as a whole and in their more general relations" seem as valid today as when he first made them. This dominant importance of orogenic research in the history of geology and its relevance to all other branches of geology was emphasized by Hölder (1960), who commenced his scholarly survey of the history of earth science with a section entitled "The Origin of Mountains" (see especially his justification on p.15 of his book). Even in our day of rapidly developing technology, when sophisticated geophysical and geochemical techniques are continuously enlarging the scope of research on the earth's lithosphere (e.g. Allègre, 1982, 1987; Oliver, 1982), the value of being able to "observe with the utmost precision, and embrace in a moment, the order, the attitude, the direction, the thickness, and even the nature of the beds of which ... (mountain belts) are composed, and of the fractures which traverse them" remains the same, because most of our new techniques depend for their success on a detailed knowledge of the geological structure in a given region as revealed mostly by surface observations. For instance, seismic reflexion techniques yield the best results where reflexions can be tied to well-mapped surface structures (e.g. Barazangi, 1984, p.44). It is clear that the more information surface geology can provide, the more efficiently can we employ new technology. Orogenic belts, owing to better outcrop conditions they generally offer, in cases even down to the upper mantle, are indeed the prime places where surface geology commonly provides the largest amount of information.

When de Saussure referred to the "infinitely various ... material" of mountain ranges he unwittingly emphasized another critical aspect of orogenic belts: the fact that they contain—albeit in an incomplete and probably highly selective sample—the diverse remnants of vanished oceanic realms swept into them during past episodes of subduction, as first realised by F.E. Suess, when he wrote: "the characteristics of orogens are best developed in 'continental margin ranges'. Where they are juxtaposed against a foreign foreland, (their) tectonics and palaeogeography indicate drift over great distances" (Suess, 1937, p.VI), and later set into a plate tectonic framework by Wilson (1968) and Hsü (1971, 1972). If the plate motion rates of the last 200 Ma, the approximate age of the oldest ocean floor, are representative, our planet must have entirely renewed its oceanic surface at least twenty times during its four-billion-year history. This means that whatever remains of the 90% of the total oceanic surface that has ever existed, now must be found in orogenic belts. [4] Therefore, a history of the oceans can be written only on the basis of the record pre-

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[3] This and the next quotation from de Saussure were taken from de Margerie (1946) after a cross-check with the original. De Margerie states (p.xcix, footnote) that the quotations were translated into English by D.W. Freshfield. I altered Freshfield's translation in only a few places, where I thought his choice of English words were not the most appropriate.

[4] The percentage of the now-vanished portion of the total oceanic surface that has ever existed actually must be higher, because plate motion rates in the Archaean may have been nearly six times as fast as those that characterized the last 200 Ma (see Dewey and Windley, 1981). Howell (1989) using post-Palaeozoic average rates of plate motion requiring an about 5 cm/yr full spreading rate, calculates that the oceanic lithosphere must have been renewed 34 times in the history of our planet.

served in orogenic belts. That is why it is of great importance to know what has been, and what gets, preserved in them.

Initial papers on orogeny–plate tectonics relationships

As a result of the abundance of information orogenic belts generally disclose, the harbingers of a very large number of our present concepts were developed by orogenic geologists in pre-plate tectonics days (see, for example, White et al., 1970; Davis et al., 1974; Şengör, 1977, 1982a, b; Thenius, 1980), but these people could not weave these into a coherent theory of earth behaviour in any way resembling plate tectonics, because they had very limited information from the ocean basins. None of them could have foreseen that the key they were searching lay on the ocean floor and probably few had much sympathy with Argand's prophetic words uttered in 1919: "Geology is a science of the past; the future is geophysics" (Thalmann, 1943, p.158). They therefore could not have understood the present behaviour of the planet *as a whole*, which deprived them of a uniformitarian basis on which to interpret their observations.

The development of plate tectonics in the sixties on the basis of observations made on ocean floors provided orogenic geologists for the first time with a sound uniformitarian framework. Wilson (1965) first formulated the three kinds of plate boundaries and made clear that orogeny dominantly resulted from activity along the convergent kind.

This was immediately applied by Wilson himself to the evolution of the Appalachian /Caledonian system (Wilson, 1966a) and by Gansser (1966) to the Himalaya (with no reference to Wilson, 1965). Later papers by Wilson himself (1966b, c, 1967, 1968), Gass (1968), Dewey (1969a, b), Hamilton (1969a, b, 1970), Laubscher (1969), Mitchell and Reading (1969, 1971), Thayer (1969), Atwater (1970), Bird and Dewey (1970), Coney (1970, 1971), Dercourt (1970), Dewey and Bird (1970a, b, 1971), Dewey and Horsfield (1970), Dickinson (1970, 1971, 1973), Ernst (1970), McKenzie (1970a), Moores (1970), Oxburgh and Turcotte (1970, 1971), Coleman (1971), Hsü (1971), Karig (1971), Matsuda and Uyeda (1971), Burke and Dewey (1972), Miyashiro (1972), Wilson and Burke (1972), Khain (1973), and Roeder (1973) elaborated on the relationships between orogeny and the plate tectonic theory. A most significant geophysical paper, later much used in developing models of orogeny, is McKenzie (1969).

Previous reviews

In 1959 [5] the Dutch geophysicist Vening Meinesz reviewed the problem of orogeny mainly from a geophysical viewpoint and made the important observation that contraction could not be the cause of mountain-building, because shortening and extension on earth occur coevally forming orogenic belts and rift valleys, respectively. Vening Meinesz ascribed orogeny to convection currents in the Earth's mantle and developed a hypothesis of earth evolution surprisingly similar to Hess', but he invoked continental drift to explain the origin of continents and oceans only in the remote past of the earth, before a rigid shell had developed.

In 1960, when the birth-pangs of plate tectonics already were being felt in certain quarters, Billings published a detailed review entitled *Diastrophism and Mountain-Building*. In this thoughtful paper he concluded: "A successful theory of diastrophism and mountain-building must explain among other things (1) the horizontal compression that is essential to form belts of folded and thrust-faulted strata; (2) extensive vertical movements, with or without high angle faulting and unrelated to folding (these two conclusions were also stressed by Vening Meinesz); (3) extensive

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[5] Vening Meinesz's review was published originally in 1958 in Dutch. I here cite a more easily accessible German translation.

strike-slip faults" (Billings, 1960, p.394). As it becomes apparent in the following paragraphs, these conclusions of a leading geophysicist and one of the classical structural geologists show, in retrospect, that the earth-science community in the beginning of the sixties had become intellectually 'ripe' to 'receive' plate tectonics. The last comprehensive 'pre-plate tectonics' reviews of orogeny are contained in Wunderlich (1966), Mišik (1968) and Kent et al. (1969).

Since the 'initial papers' dealing with orogeny and plate tectonics, a vast number of publications appeared dealing directly or indirectly with orogeny–plate tectonics relationships. Perhaps the richest post-plate tectonics collection of regional data on Mesozoic–Cenozoic orogenic belts is represented by Spencer (1974). Another good source of information is the 'Rodgers Volume' of the *American Journal of Science* (Ostrom and Orville, 1975), dealing with a broader age spectrum, but a smaller number of orogenic belts.

In 1976 Smith reviewed the state of the art mainly from a geologist's perspective. In 1978 a Penrose Conference convened in Ascona, Switzerland, to discuss the problems pertaining to the geophysics and structure of folded belts (Bally et al., 1979). In 1980, Aubouin provided a shorter review. More up-to-date reviews are represented by the contributions to the 1981 Zurich symposium on *Mountain Building Processes* (Hsü, 1982a), to the *Geological Society of America Memoir 158* (Hatcher et al., 1983), and to volumes 10 (Rast and Delany, 1983) and 19 (Leitch and Scheibner, 1987) of the *Geodynamics Series*. For recent descriptions of a number of little-known orogenic belts, see Schaer and Rodgers (1987). For reviews of Precambrian orogeny in general, two recent compendia (Kröner, 1981; Kröner and Greiling, 1984) offer a wide spectrum of data and interpretations, although more recent regional summaries dealing with groups of, or individual, Precambrian orogenic belts have also been published. The most recent assessment of Archaean orogeny in particular is provided by de Wit and Ashwal (1986).

Dennis (1982) and Schwab (1982) contain collections of reprints of some of the historically significant contributions to the study of orogeny. Also see the rich quotations in Hölder (1960).

Purpose and scope of this review

The purpose of the present review is to show where we now stand with respect to orogeny–plate tectonics relationships by considering the developments of the last 25 years. My emphasis is mostly on the progress since Smith's (1976) review. I limit the present review topically to the more classically 'geological' aspects of the problem—mainly because this is my area of expertise—and regionally to the Tethyside orogenic complex (Şengör, 1987a), because it offers the largest variety of orogenic structures, whose mutual relationships in space and time can be examined with greater facility than in any other major orogenic system in the world owing to advantages of youth, near-equatorial location, abundance of fossils and outcrops, and a long history of research. For certain other aspects of orogeny, the reader is referred to the following recent compendia and reviews. For orogenic magmatism Gill (1981), Thorpe (1982), Aramaki and Kushiro (1983), Roddick (1983), Harris et al. (1986), England and Thompson (1986), and Spiegelman and McKenzie (1987). For regional metamorphism England and Thompson (1984), Thompson and England (1984), Day (1987), Oxburgh et al. (1987), and Daly et al. (1989). For HP/LT metamorphism Evans and Brown (1986), Daly et al. (1989), and Okay (1989a). For a general geophysical review Meissner (1986).

I should also remark here in passing that non-plate tectonic views on orogeny, although no longer significant, are nevertheless not extinct. For recent versions of a contracting earth model that has not been used in the earth sciences for more than two decades now, except for such insignificant exceptions as Meyerhoff and Meyerhoff (1977, pp.361–365) see Jeffreys (1976) and Lyttleton (1982). The papers in Carey (1983) provide a good overview of the expanding earth hypothesis and its relation to orogeny (for an excellent comprehensive refutation of fast earth-expansion models see Weijermars, 1986). The Soviet version of the oscillation theory finds its most recent expression in Beloussov (1980 and 1989). A brief review of the most prominent Chinese tectonic schools is given in the introductory chapter in Ren et al. (1987), but for a full exposition of Chen Guoda's Diwa theory, see Chen (1989). These various non-plate tectonic interpretations will not be considered any further in this paper.

Before I begin dealing with orogeny–plate tectonics relationships, I present, in the next two sections, a simple working definition of *orogeny* and a brief review of ideas on the nature of this process emphasizing how our thinking evolved along a line of a growing appreciation of the *multi-dimensionality* of orogenic mobilism and the kinds of biases geologists have brought into plate tectonics.

OROGENY—A DEFINITION

Very much like Suess (1916, p.432) and Popper (1966, pp.9–21; 1983, pp.233, 261–277, esp. footnote on p.275) I too am not keen on definitions. A rigorous pursuit of definitions would lead to an infinite regress. As Popper says "in science, we take care that the statements we make should never *depend* upon the meaning of our terms. Even where the terms are defined, we never try to derive any information from the definition, or to base an argument on it. This is why our terms make so little trouble. We do not overburden them. We try to attach to them as little weight as possible. We do not take their 'meaning' too seriously. We are always conscious that our terms are a little vague..." (Popper, 1966, p.19). With the above limitations, definitions are only useful as shorthand symbols or working tools for complex concepts. It is a shorthand symbol that I propose to give in the following.

It has long been a popular opinion that Gilbert (1890, p.3) was the one "who coined the noun, orogeny" (Dennis, 1980, p.570; see also Hsü, 1950, p.7 and 1973, p.81). However, the noun *orogeny*, defined as mountain-building through a tectonic process had been in use in continental Europe before Gilbert. For example Boué (1874, p.262) wrote: "It therefore follows that one is justified in assuming a certain sequence of faulting directions as well as mountain-chain construction in a group of countries or even for a whole continent in certain geological periods.... Now without the discovery or recognition of this first theoretical rule of orogeny (in the original German text of Boué as *Orogenie*) one can only find oneself in a confusion of mountain-trends exactly like the geologists of 50 years ago, and one could derive no causal discovery out of it." Here Boué clearly suggested a structural cause for mountain genesis, which he termed *orogeny*.

Gilbert, too, stated that "the process of mountain formation is *orogeny*" (Gilbert, 1890, p.340) and clearly implied, as Boué (1874) had done, a *structural process* that generated mountainous relief as a by-product, although Gilbert (1890) made no reference to previous usage (*orogeny* from classical Greek *oros* = mountain, *genna* = birth). De Lapparent (1893, p.1508ff) indicated that orogeny was a *study of mountains*, by which he explicitly meant a study of the *dislocations* of the earth's crust, therefore stressing the *structural* rather than the morphological aspect. Haug (1907, p.14) defined it as a 'phase' during which the reliefs of the earth's crust were formed and also explicitly indicated that it was a structural process whose record was seen in 'folded regions'. Following Stille (1919, p.171), I emphasise that Gilbert—wittingly or unwittingly following what seems to have been a general view in Europe—defined orogeny as a structural process whose by-product was the mountainous relief, so that statements such as "Gilbert (in defining orogas Meyerhoff and Meyerhoff (1977, pp.361–365) see Jeffreys (1976) and Lyttleton (1982). The papers in Carey (1983) provide a good overview of the expanding earth hypothesis and its relation to orogeny (for an excellent comprehensive refutation of fast earth-expansion models see Weijermars, 1986). The Soviet version of the oscillation theory finds its most recent expression in Beloussov (1980 and 1989). A brief review of the most prominent Chinese tectonic schools is given in the introductory chapter in Ren et al. (1987), but for a full exposition of Chen Guoda's Diwa theory, see Chen (1989). These various non-plate tectonic interpretations will not be considered any further in this paper.

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OROGENY—A DEFINITION

Very much like Suess (1916, p.432) and Popper (1966, pp.9–21; 1983, pp.233, 261–277, esp. footnote on p.275) I too am not keen on definitions. A rigorous pursuit of definitions would lead to an infinite regress. As Popper says "in science, we take care that the statements we make should never *depend* upon the meaning of our terms. Even where the terms are defined, we never try to derive any information from the definition, or to base an argument on it. This is why our terms make so little trouble. We do not overburden them. We try to attach to them as little weight as possible. We do not take their 'meaning' too seriously. We are always conscious that our terms are a little vague..." (Popper, 1966, p.19). With the above limitations, definitions are only useful as shorthand symbols or working tools for complex concepts. It is a shorthand symbol that I propose to give in the following.

It has long been a popular opinion that Gilbert (1890, p.3) was the one "who coined the noun, orogeny" (Dennis, 1980, p.570; see also Hsü, 1950, p.7 and 1973, p.81). However, the noun *orogeny*, defined as mountain-building through a tectonic process had been in use in continental Europe before Gilbert. For example Boué (1874, p.262) wrote: "It therefore follows that one is justified in assuming a certain sequence of faulting directions as well as mountain-chain construction in a group of countries or even for a whole continent in certain geological periods.... Now without the discovery or recognition of this first theoretical rule of orogeny (in the original German text of Boué as *Orogenie*) one can only find oneself in a confusion of mountain-trends exactly like the geologists of 50 years ago, and one could derive no causal discovery out of it." Here Boué clearly suggested a structural cause for mountain genesis, which he termed *orogeny*.

Gilbert, too, stated that "the process of mountain formation is *orogeny*" (Gilbert, 1890, p.340) and clearly implied, as Boué (1874) had done, a *structural process* that generated mountainous relief as a by-product, although Gilbert (1890) made no reference to previous usage (*orogeny* from classical Greek *oros* = mountain, *genna* = birth). De Lapparent (1893, p.1508ff) indicated that orogeny was a *study of mountains*, by which he explicitly meant a study of the *dislocations* of the earth's crust, therefore stressing the *structural* rather than the morphological aspect. Haug (1907, p.14) defined it as a 'phase' during which the reliefs of the earth's crust were formed and also explicitly indicated that it was a structural process whose record was seen in 'folded regions'. Following Stille (1919, p.171), I emphasise that Gilbert—wittingly or unwittingly following what seems to have been a general view in Europe—defined orogeny as a structural process whose by-product was the mountainous relief, so that statements such as "Gilbert (in defining orogeny) was more concerned with physiography than with structure" (Dennis, 1980, p.570) do not quite reflect the truth.

Similarly misleading is Bucher's (1933, p.402) statement: "Confusion concerning the use of (orogeny) resulted when the study of the tectonics of mountains was separated from that of their physiography, each being cultivated by different groups of men. The result is that structural geologists have developed their own definition of 'orogeny' based exclusively on criteria observable in the stratigraphic and structural record of the past." The *one* group implied in Bucher's quotation, cultivating, in the past, the physiography and tectonics of mountains at once never existed. It is a figment of Bucher's imagination and a result of the myth that orogenic studies developed gradually from physiography to structure. This has never been the case, because the structural aspects of rocks were first studied by miners. An unfortunate terminological confusion was indeed created by these people, by calling groups of strata 'mountain series' or simply 'mountains' (e.g. Füchsel's *series montana* was translated into German by Keferstein (1840) as *Formation*; Werner's *Urgebirge, Übergangsgebirge, Flötzgebirge* had nothing to do with 'mountains' (Gebirge = mountain), but were the primitive *rocks*, transition *rocks*, and floetz (flat-lying) *rocks* (e.g. Phillips, 1855, p.8; see also Naumann, 1858, p.866, footnote). For this reason regional *stratigraphic* studies used to be called 'orographies' until the middle of the 19th century (e.g. Keferstein, 1840, pp.223ff). Thus, a geologist's study of *orography* used to be one of rocks and their structure and not necessarily of mountains. Even Thurmann, in whose work the term orography was used to imply real mountains, bitterly complained about the geographical classification of mountains. He wrote: "... it is difficult to agree on a nomenclature convenient for everyone at once. Also, the classifications (of mountains) so far offered by physical geography are purely artificial and could hardly give us a proper idea on mountains: they are mostly inapplicable to geology save for some utility in hydrographic studies" (Thurmann, 1852, p.77).

Therefore, people studying the *physiography* of mountains and those studying their *internal structure* have never been one. In fact, when these two groups of men finally came into contact towards the end of the 18th century, they found that they had developed entirely different conceptions of earth behaviour. The 'physiographers' had mainly studied the effects of running water and developed a uniformitarian school in which the patient and tranquil effects of streams accomplished gigantic work in a long time. As Davies (1969) pointed out, this school had its greatest champion in Hutton. The 'structural geologists', on the other hand, as descendants of miners, have always had the *finished products* of diastrophism before their eyes: faults, angular unconformities, and terribly contorted folds. Unlike the fluvial geomorphologists, they knew nothing in the modern world that they could have compared with what they saw. They could not have conceived that a fault with hundreds of metres of throw could have accumulated it slowly, especially not if it was also abruptly truncated by an unconformity on top. The descendants of the miners thus came to develop a catastrophist view of the world and following their great teacher Werner (who himself was not particularly interested in catastrophic mountain-building) were anxious to show that they could find their neat system of (non-mountainous) mountains the world round. In the following brief synopsis of the evolution of thought on orogeny, I shall come back to this extremely important schism between the two schools which still persists.

In summary I wish to underline that both the term 'orogeny' and its conceptual predecessors have always implied the origin of *structures* (and *not the mountainous relief*) as seen by the geologist on outcrop (see also Stille, 1950, p.92). That is perhaps why the term orogeny has never been applied to volcano-building by accumulation of volcanic material, which, otherwise, is a perfectly respectable way of 'mountain-making' (see esp. Andrée, 1914, pp. 1–4; Andrée, on p.5, calls orogeny '*eigentliche Gebirgsbildung*', i.e. 'real mountain building' as opposed to other, especially magmatic ways of mountain-making).

The clearest definition of orogeny—and one readily usable by the field geologist—was given by Stille (1919, p.195), according to which *orogeny is an episodic process that changes the 'fabric' of rocks*, i.e. produces structural changes visible to the eye such as faults, folds, thrusts etc. Its clearest evidence is angular discordances, as the 'angularity' of the discordance is a result of the rocks' underlying it having a different 'fabric' (i.e. folded, faulted, cleaved etc.) from those newly laid down on it.

As Bucher (1933, p.403) correctly points out "the real core of Stille's definition refers to that property which alone can be observed impartially in the field, the effect of the disturbance on the visible structure of the rocks." But this definition includes *all* kinds of faulting and most folding (together with their attendant phenomena such as the formation of foliations) and indeed has been used also recently by the adherents of plate tectonics: "... the plate tectonic hypothesis ... depicted a world whose crust is composed of a small number of rigid plates, and in which 'orogenic' deformation takes place at convergent, divergent, and transform plate boundaries" (Monger and Francheteau, 1987, p.ix). It throws strike-slip faults, rift valleys and orogenic belts into the same category and is therefore not particularly useful—because it is too wide—from the viewpoint of plate tectonics.[6] If used in a plate tectonics context, such a definition could separate roughly plate boundary zone phenomena from those of plate interiors (and indeed this is what Monger and Francheteau, 1987, end up saying), but orogeny should not be expanded to embrace all plate boundary processes. Despite its all-inclusive character, Stille's concept remained the most widely used definition of orogeny for more than half a century forming the basis of his phase concept. Both Dennis (1967, pp.112–113; also 1980) and Cebull (1973) reviewed the various definitions proposed in this century and some of the criteria to constrain better definitions, but they both ended up essentially with Stille's formulation.

Cebull's proposed definition was criticised later by King (1974) and Wang (1976), but neither offered anything better. King recommended the 'standard definition' given in the *Glossary of Geology* (Gary et al., 1972), which is simply Stille's definition, and Wang argued that both morphogenic and structural mountain-building must be integral parts of a definition of orogeny—a definition that would leave out a considerable number of the present-day orogenic belts along extensional arcs (see below).

I here propose a plate tectonics-based working definition that is more restrictive than Stille's and that fulfills the task of a shorthand: *orogeny is a collective term for convergent plate margin processes*. This definition is that of a group of processes that result from one phenomenon, which is plate convergence, and is close to Hsü's view of orogeny (Hsü, 1982b, p.3). As Cebull (1973) concluded in his essay, "first, however, it is essential to clarify (agreement being unlikely) what phenomenon is being considered when orogeny is discussed or studied." My definition identifies the processes involved in orogeny and includes both of Stille's conditions implicitly, because of the peculiarities of convergent plate margin phenomena, including metamorphic and magmatic ones, but excludes *purely* extensional or *purely* strike-slip processes. This is also in agreement with the popular concept of orogeny. For example, the

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[6] Neither was it very useful in the framework older theories of orogeny, so that Steinmann (1913, pp. 224) felt compelled to distinguish a 'positive mountain-building' consisting of "mainly uplifting movements regardless of whether they are caused by folding or *en bloc* uplift" from a 'negative mountain-building' "expressed mainly as subsidence and crustal in-throws".

geographer Edgar Ford wrote in his popular geography of Papua New Guinea (Ford, 1973, p.18) that orogenesis meant "mountain building by folding and uplift" clearly implying a component of crustal shortening.

Although restraining bends along transform faults are, strictly speaking, places where two plates converge, my initial inclination was to take them out of the concept of orogeny too, because they commonly lack most earmarks of orogeny such as abundant magmatism and metamorphism. However, my friend Ken Hsü persuaded me otherwise. He reminded me that much of the classical debate between Gilluly (1949, 1950) and Stille (1950) on the continuity vs. episodicity of orogeny had been waged on Southern Californian examples; on basins, whose deformation has been related to the San Andreas fault (see also Hsü, 1973, p.81). What is the plate kinematic difference, he asked me, between the Transverse Ranges connecting as they do the various northwest-striking branches of the San Andreas fault system to their north and south, and the Aleutian subduction zone connecting the Queen Charlotte fault with the Kiska–Komandorsky strike-slip segment? As no kinematic difference between these two cases exists, and as the orogenic status of the Aleutian arc system east of the Kiska island admits of no argument, I also had to count the Transverse Ranges as orogenic, and with them all other major restraining bends along transform faults. This is in full accord with my above-given definition, because any restraining bend on a transform fault is also a convergent plate boundary.

An *orogen* (Kober, 1921, p.21) thus becomes a structure produced by the collective work of convergent margin processes. We shall see in the seventh section below (p.148) that, owing to the advances in our understanding of the structure and evolution of *orogenic zones* (Şengör, 1987b, p.367), the term *orogen* alone is no longer adequate to describe many major *orogenic belts* which are formed as a result of the activity of a large number of convergent plate boundaries in space and in time. In other words, many orogenic zones consist of numerous orogens. For such zones, we shall have to make use of Helwig's (1974) apposite term *orogenic collage*.

I should also remark here that recently Wernicke (1981, 1985) compared broadly localized zones of intense extension such as the Basin and Range province of the western United States with similarly broadly localized zones of intense compression and interpreted the former as extensional analogues of the latter naming them 'extending orogens'. Although I think Wernicke's analogy is appropriate, I fear his terminology may lead to confusion. As I indicated above, my preference is to confine the term orogen to convergent structures, as its originator intended it to be (Kober, 1921, p.21), and to use Krenkel's (1922, p.181) term *taphrogen* (through-building) for broadly localized zones of intense extension. Krenkel defined *taphrogeny* in a very similar way to Wernicke's definition of 'extensional orogeny': "Disintegration into blocks by extension, the ... counterpart of orogeny, is thus called *taphrogeny*." In an associated footnote he added: "Derived from *he taphros* = graben. Grabens are the most conspicuous structural elements of extensional regions. Therefore they might lend their name to designate the totality of all tectonic processes associated with extension."

BRIEF REVIEW OF THE EVOLUTION OF THOUGHT ON THE NATURE OF OROGENY

Spatial aspects of orogeny

Although the *term* orogeny appeared towards the end of the 19th century, the *process(es)* it describes has(have) long been one of the central themes of earth-science research (for more detailed historical reviews see: Vose, 1866; Hölder, 1960; Dennis, 1982; Greene, 1982; Schwab, 1982; Şengör, 1982a, c). Since the days of antiquity, mountain belts have attracted attention (e.g., for the earliest Greek accounts, see Freeman (1948), fragments 1.16 (Orpheus), 3.18 (Epi-

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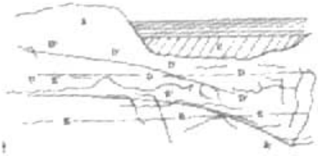
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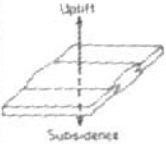

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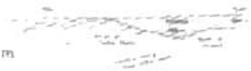
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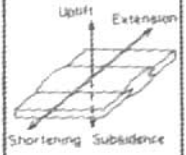

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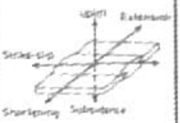

menides), and 4.8 (Hesiod)). However, by the end of the 18th century, the isolated efforts of such men as Strabo, Pliny the Elder, Avicenna (Ibn Sina), Georgius Agricola, Leonardo da Vinci, Steno, Scheuchzer, Moro, Lomonosov, de Luc and finally de Saussure had succeeded, to quote de Margerie (1946, p.xcviii) "only in accumulating facts without any connection, merely satisfying a feeling of curiosity" (see also Hölder, 1960, pp.15–34). De Saussure's famous words of desperation in the last volume of the *Voyages dans les Alpes* have been frequently cited in support of this view: "In my youth, when I had crossed the Alps through only a few passes, I thought I had understood the facts and general relations. I even gave, in 1774, a discourse on the structure of mountains, in which I set out these results. But after repeated wanderings in several parts of the chain have shown me more facts, I recognize that one could almost assert that there is nothing constant in the Alps save their variety" (de Saussure, 1796, p.464). Despite all this, de Saussure did reach three general conclusions concerning the structure of the Alps that seem important in retrospect and were so considered by him also (de Saussure, 1796, pp.464–465).

One of de Saussure's conclusions was that in general the strike of the beds making up the Alps followed the trend of the range. This observation had been made independently already by others in other orogenic belts and employed in formulating the first global theories of orogeny already during de Saussure's lifetime. His second conclusion was that the Alps had an asymmetric morphology, with a steeper southern margin. This observation became the starting point for Suess' theory of the asymmetric structure of mountain belts three quarters of a century later. Finally, de Saussure's third conclusion, that a nearly chaotic disorder characterizes the bowels of the Alps had to await the 20th century, especially plate tectonics, for an explanation in terms of complex three dimensional motions within orogenic belts.

As already hinted in the preceding paragraph, de Saussure's three conclusions, in the order presented above (the ordering is not his), also serve to characterize the three important steps in the development of thought concerning some of the *spatial* aspects of orogeny since his time (Fig. 2).

The first step had already been taken during de Saussure's life-time by Michell (see Mather and Mason, 1939, pp.84–87; also Hölder, 1960, pp.35–37), Pallas (Fig. 2a), and Hutton and later elaborated by von Buch (Fig. 2b), Studer (Fig. 2c), and, to quote John

Fig. 2. The three important steps in our understanding of the spatial aspects of orogeny before plate tectonics and the principal contributors to their development. *I* = one-dimensional view of orogenic evolution: *a* = Peter Simon Pallas; *b* = Leopold Freiherr von Buch; *c* = Bernhard Studer; *d* = Pallas' cross-section across an idealized orogenic belt; *C* = crystalline ('intrusive'); *K* = limestones and schists (from Heim, 1921, p. 3); *e* = James Hall; *f* = Herschel's diagram of a sedimentary basin (from Babbage, 1838, p. 234). *II* = two-dimensional view of orogenic evolution: *g* = Jean-Baptiste-Armand-Louis-Léonce Elie de Beaumont; *h* = Arnold Escher von der Linth; *i* = James Dwight Dana; *j* = Eduard Suess; *k* = Elie de Beaumont's conception of the symmetric structure of the Mont Blanc (after Favre) which led him to the view that all orogenic belts possess a similarly symmetric internal structure; note that Elie de Beaumont's cross-section is nothing more than a 'pinched' version of Pallas' cross-section; *l* = Suess' cross-section of a dominantly asymmetric orogenic belt with back-land (i.e. hinterland), backfolding (retrocharriage), and foreland; *m* = Suess' concept of orogen–foreland relations and the nature of foredeeps; note the incredibly modern outlook of his view of foredeeps, with the foreland sinking under the border of the folded chains along a gently inclined thrust-plane (both *l* and *m* are from Suess, 1924); *n* = Leopold Kober; *o* = Hans Stille; *p* = Kober's idealized symmetric orogenic belt (from Kober, 1931, p. 10). Note the similarity, in principle, to Elie de Beaumont's cross-section. This 'reactionary' concept of orogenic structure, defended by both Kober and Stille, dominated orogenic research well into the sixties. *III* = three-dimensional view of orogenic evolution: *r* = Alfred Lothar Wegener; *s* = Emile Argand; *t* = Franz Eduard Suess; *u* = Argand's cross-section across the Alps from about north central Europe to Africa (from Argand, 1977, fig. 16). Notice the return to Suess' asymmetric orogenic belt concept.

Rodgers (1949, p.1646) in a "quaint" form by the Rogers brothers. All of these people (with the exception of the Rogers brothers) viewed mountain belts as bilaterally symmetric structures with respect to a central axis extending along the trend of the mountain range and along which plutonic (Hutton, 1795, esp. Craig et al., 1978, figs. 23, 24 and 25; later Studer, 1851) or volcanic (Michell, 1760; Pallas, 1777; von Buch, 1820, 1824; later Rogers and Rogers, 1843) processes were believed to have burst open and uplifted the overlying strata. Fig. 2d shows schematically how the proponents of this early view, known as the vertical uplift theory, visualized the structure of mountain belts. By the beginning of the 19th century, therefore, two important concepts had been developed already by the proponents of the vertical uplift theory: (1) that mountain belts owed their origin to vertical uplift of originally horizontally layered strata, and (2) that the uplifting agent, whether plutonic or volcanic, came from inside the earth, i.e. it was related to the internal heat (and therefore to the indigenous energy) of the planet.

Thus, these early theorists recognized that rocks somehow had to be uplifted to form mountains (i.e. they had not been deposited where they were now as Werner had assumed) and that this uplift was motored by the earth's internal energy. Reduced to this simple formula, both of these conclusions remain valid today. Where they erred was in their view of how the uplift was actually accomplished. The reason why they erred on this point was that they had a grossly incorrect conception of the internal structure of mountain belts. The people who first formulated this view (mostly Pallas and Hutton) knew nothing of biostratigraphy (it had not yet been invented!) and therefore could not have recognized the structures that eventually helped to refute the vertical uplift theory. The geological methods they had at their disposal (e.g. the recognition that some of the fossil shells belonged to once living marine organisms, that coastal terraces represented former shorelines) were sufficient for them to see a single dimension, that of uplift. We can therefore characterize this early theory of orogeny as being one-dimensional (not counting the ever-present time dimension), although de Saussure recognized that horizontal shortening might have been responsible for folding. He did not, however, use this inference to develop a general theory for the origin of the Alps.

About three decades after the formulation of the vertical uplift theory of the mountain ranges, biostratigraphy was invented independently by William Smith in England and Cuvier and Brongniart in France. This powerful tool revolutionized geology and rapidly developed into being its main foundation.

The first important effect biostratigraphy had on orogenic geology was the realization by Dufrénoy and Elie de Beaumont, in 1848, that in France regions of orogenic deformation (the southern part of the country, the Pyrenees and the Alps and their foredeeps) had been also regions of thick sediment accumulation before the onset of orogeny. They separated the undeformed (or very gently deformed) parts of the country from those that had been highly deformed. In an essay published posthumously in 1873 (written before his death on March 20, 1857) Dufrénoy further pointed out that the Cretaceous fauna of these deformed areas of thick sediment accumulation (he specified the Pyrenees) was conspicuously different from that in northern France.

In 1836 Herschel pointed out, in a letter written to Lyell, that a depression was necessary to accumulate a thick package of sedimentary rocks and that after a certain amount of deposition, the weight of the sediments could sustain the subsidence by itself and result in even thicker deposits. Both he and Babbage explored the thermal consequences of such subsidence and found that it must weaken the base of the crust (Babbage, 1838, pp.209–224).

If not the results of Dufrénoy and Elie de Beaumont, at least the work of both Herschel and Babbage were known to James Hall (Fig.

2e) when he developed his view of large accumulation of sediments in a synclinal trough along the axis of the Appalachians before their deformation (Hall, 1859, esp. "Note E" on pp.95–96 where reference to Babbage and Herschel is made and the latter's figure of a sedimentary basin reproduced; Fig. 2f). Hall's starting point was much the same as that of Dufrénoy and Elie de Beaumont. He noticed the difference between the predominantly shallow marine (in those days all sandstones were thought to be of shallow water origin) sedimentary rocks of enormous thickness of the Appalachian orogenic belt and the less complete and much thinner sequences of its foreland (in New York State and in Pennsylvania, and in the U.S. Midwest in the Mississippi valley and in Iowa: Hall, 1857a, b, 1883; also see Merrill, 1924, p.346). He also noticed that the deformed rocks corresponded spatially with the areas of thickest sediment accumulation. He thus inferred a causal relationship between the depression, subsidence, and deformation of strata (but *not* uplift!). He wrote "if the accumulation should go on to many thousands of feet in thickness, and the ocean bed be depressed accordingly, we should have a greater deflection of the strata from the original horizontal position (this far Hall's views had been anticipated by the extraordinary theory of Albertus of Saxony in the 14th century: see Hölder, 1960, pp.19–20; and by the views of Lazzaro Moro in 1740: also in Hölder, 1960, pp.30–32); and, as I conceive, this depression must be accompanied by folding or plication..." (Hall, 1859, p.96). For uplifting the depressed and folded strata, Hall appealed to Herschel's version of isostasy! (also anticipated by Albertus of Saxony: Hölder, 1960, pp.19–20)." . . . he (Herschel) says, every continent depressed has a tendency to rise again... It is this ultimate rising of continental masses, that I contend for, in opposition to special elevatory movement along the lines of mountain chains" (Hall, 1859, p.96: as Hunt, 1883, pointed out, Hall was here also following the views of Count de Montlosier, who had earlier contended that the mountain ranges in Europe represented all that had remained from erosion following a wholesale uplift of the continent).

Hall's views were thus developed in a framework of the vertical uplift theory although he himself found it distasteful. As late as 1864 he wrote to Vose: "If I can sustain the great principle which I advocate viz., that mountains are not produced by upheaval but by accumulation and continental elevation I shall feel that I have done something to advance the Science of Geology in true principles. I feel quite sure of its ultimate adoption because I feel quite sure that it is the only true explanation, the only mode of making mountain ranges, for they cannot be made without material and no imaginary upheavals will ever explain their existence." (Merrill, 1924, p.688.) Hall showed that vertical depression had first occurred along at least one mountain chain before it had been raised along with the entire continent and he speculated that the same was probably also true for the Cordillera (Hall, 1883). In this depression he saw the reason for folding and, following Babbage and Hunt, metamorphism. He realized that if enormous thicknesses of rock accumulate in one place and then deform, ensuing eventual uplift of the continent (because of Herschel's isostasy) must make the site of thicker sediments into mountains. Before the formal announcement of his theory in full, he had written in the report of the Geological Survey of Iowa that "...It is this great thickness of strata, whether disturbed and inclined as in the Green and White mountains and the Appalachians generally, or lying horizontally as in the Catskill Mountains, that gives the strong features to the hilly and mountainous country of the East and which gradually dies out as we go westward just in proportion as the strata become attenuated ... The thickness of the entire series of sedimentary rocks, no matter how much disturbed and denuded, is not here (i.e. in the U.S. Midwest) great enough to produce mountain features (quoted after Merrill, 1924, p.346; see also Hall, 1883, pp.55–57, and 68–69). Hall wished to avoid appealing to special uplifting movements for mountains, as it conflicted with his extreme uniformitarian views (see esp. Hall, 1883). Later Dana (1866, p.210) (Fig. 2i) called Hall's theory "a theory for the origin of mountains with the origin of mountains left out", alluding to Hall's opposition to "special elevatory movement along the lines of mountain chains". This was unfair (for Hall's reply to Dana see Hall, 1883, pp.68–69; also Hunt, 1883, pp.69–71). Hall did have a theory for the origin of mountains, that of Herschel's isostasy. What he did not have was a theory for the origin of basins. Hall attributed the subsidence to the weight of the overlying sediment, because he had not understood Herschel's physics, that a basin in the first place was necessary to accumulate enough sediment to cause *further* subsidence.

Thus, between about 1830 and 1860 biostratigraphy helped to formulate the concept, if not the term, of the geosyncline (Fig. 2f: see esp. Hall, 1883, on the importance of biostratigraphy in his work) and thus extended the theory of vertical uplift to one of vertical subsidence *and* uplift. It is important to underline here that it was within the framework of this 'one-dimensional' theory that the *concept* of a geosyncline was conceived.

Biostratigraphy also helped geologists to recognize the complicated compressional structures of the mountain belts. This recognition led to the second important step in our understanding of orogeny which was taken by Elie de Beaumont in 1831 (Fig. 2g) and carried to completion by such people as Arnold Escher von der Linth (Fig. 2h), James Dwight Dana (Fig. 2l), and Eduard Suess (Fig. 2j) by 1875.

The views of the Rogers brothers forms a bridge between the first and the second episodes of orogenic research. The following quotation from H.D. Rogers' "On the Laws of Structure of the more Disturbed Zones of the Earth's Crust" (1857) reveals the essence of the two brothers' ideas: "We suppose the strata of such a region (*i.e. an orogenic belt*) to have been subjected to excessive upward tension, arising from the expansion of molten matter and gaseous vapours, the tension relieved by linear fissures, through which much elastic vapour escaped, the sudden release of pressure adjacent to the lines of fracture, producing violent pulsations on the surface of the liquid below (*i.e. the 'liquid' interior of the Earth—not necessarily molten! see Gregory, 1916, p.22*). This oscillating movement in the fluid mass below would communicate a series of temporary flexes to the overlying crust, and these flexures would be rendered permanent (or keyed into the forms they present) by the intrusion of molten matter (*so far, the ideas are related to those of the first episode of orogenic research*). If, during this oscillation, we conceive the whole heaving tract to have been shoved (or floated) bodily forward in the direction of the advancing waves, the union of this tangential with the vertical (*two-dimensional motion! Here we see already elements of the ideas related to the second episode of orogenic research*) wave-like movement will explain the peculiar steepening of the front side of each flexure, while a repetition of similar operations would occasion the folding under, or inversion (*asymmetry of structures*), visible in the more compressed districts." (Rogers, 1857, pp.463–464; italicised phrases in parentheses are mine.) Thus, although the Rogers brothers noticed the evidence for horizontal shortening across mountain chains and the dominant asymmetry in their structure, the ultimate cause of orogeny they sought in primary vertical motions ("expansion of molten matter and gaseous vapours" causing "excessive upward tension").

In 1831 Elie de Beaumont argued that local lateral compression due to thermal contraction was the cause of tilting and folding and consequent magmatism and uplift of originally horizontal strata. [7] In his major work of 1852, *Notice sur les Systèmes des Montagnes* (Note on Mountain Systems) he repeated that the cause of the horizontal compression inferred from the folds in the mountain ranges was the thermal contraction of the planet (Elie de Beaumont, 1852, p.1222). It was especially Dana in the United States (see Dana, 1873, and the previous references cited therein) and Suess (1875) in Europe who elaborated the contraction theory, emphasizing that *horizontal motions acting perpendicular to the trend-lines of mountain ranges*, the so-called tangential component of contraction (as opposed to the radial component that resulted in subsidence and supposedly created the ocean basins), were the main factor in forming orogenic belts and that *uplift was a by-product of shortening* (Dana, 1873, pp.428–433; Suess, 1875, p.151). Both Dana and Suess revived de Saussure's second important conclusion, the *asymmetry* of mountain belts in opposition both to the earlier verticalists and to Elie de Beaumont (Fig. 2, l and m), who had developed the jaws-of-the-vice analogy of mountain-building and argued for a symmetric, fan-shaped structure of mountain belts (Fig. 2k). Dana and Suess showed, following the lead of the Rogers brothers, that most internal structures of mountain belts had a vergence in one dominant direction, called the *external side* by Suess. Unfortunately, both Dana and Suess attributed this asymmetry to the physically absurd notion of 'one-sided pressure' believed to act in the direction of the external side.

As the theoretical considerations reviewed in the preceding paragraph show, this second episode in orogenic research was dominated by an emphasis on horizontal motions at right angle to the trend of the mountain ranges. Dana believed that Hall's postulated great synclinal depression along the Appalachians —which he named *geosynclinal*—was a fold of gigantic dimensions. Not only the origin of the geosynclinal (or, as Dana later wrote, *geosyncline*), but also its compressional demise were ascribed to what Dana called 'lateral pressure'. This emphasis on horizontal motion received important support from Escher von der Linth's discovery of the Glarus thrust in Switzerland (Heim, 1929; Escher interpreted his observations as a double fold with two converging recumbent anticlines: Fig. 3a; see also Şengör, 1982a, fig. 1.1) and culminated in its reinterpretation by Bertrand (1884) as a north-vergent nappe, which raised Escher's original estimate of a 15 km shortening to 40 km!

That mountain ranges have been shortened across their trend, that uplift ultimately results from this shortening, and that igneous activity and metamorphism in orogenic belts are not the cause, but the consequence of deformation are the remaining valid conclusions of this episode of research. That geosynclinal sinking is a prerequisite for subsequent orogeny also became a widely accepted axiom during the latter part of this episode.

The views generated during this second episode of orogenic research may be characterized collectively as being *two dimensional* (i.e. shortening and subsidence + uplift). Later, beginning with Suess' 1891 paper on the East African rift valleys, which interpre-

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[7] There seems to be confusion in the literature as to when Elie de Beaumont first was converted to the two-dimensional view, thus abandoning the earlier, 'uplift only'-models. Laudan (1987, pp. 180, 197 and 245), for example, seems to think that this happened by the time Elie de Beamont wrote his famous 1829–1830 article. Neither in that nor in a more popular version of it published in 1830 was I able to find any reference to tangential shortening. The first time Elie de Beaumont mentioned it was in a long communication he sent to Sir Henry de la Beche to be included in de la Beche's *A Manual of Geology* upon the latter's request. Elie de Beaumont's account was an abridgement *and* a significant update of the popular 1830 article. His update pertained both to the number of 'epochs of upheaval' *and* to the mechanism that he now thought was tangential shortening resulting from the secular cooling of the globe. Sir Henry found the communication too long to be included in his small book (de la Beche, 1931, p. 496, footnote). He sent it on to the *Philosophical Magazine*, in which it eventually appeared as an article (*not* as a translation of a previously published French paper as Laudan seems to think) (Elie de Beaumont, 1831), and also abridged it further and included it in his book (de la Beche, 1831, pp. 496–501). It is thus a curious coincidence that the famous Frenchman's theory first appeared in English!

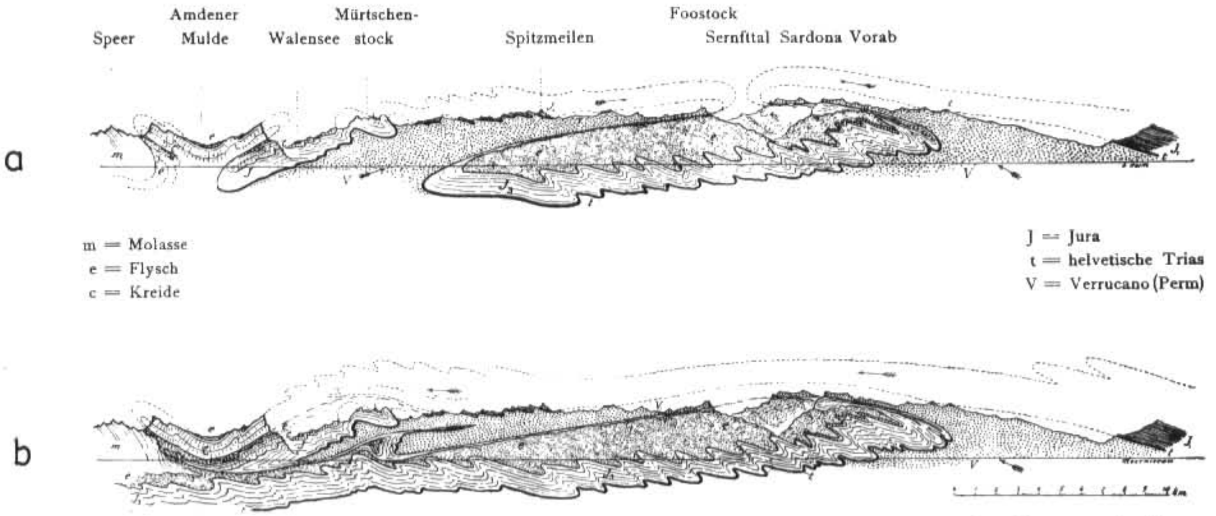

Fig. 3. a. Escher's interpretation of the geological structure along the Linth valley in the Canton of Glarus, Switzerland, as a 'double fold' requiring a north–south shortening of about 15 km. b. Marcel Bertrand's reinterpretation of Escher's cross-section as a single, north-vergent nappe. In this case, the required stratal shortening is about 40 km. (Both sections are from Heim, 1921, p. 12.)

ted them as extensional structures, contractionists also had to contend with extension. But this only added another *direction* to the second dimension of motion which they had already been working with (i.e. *shortening* and *extension* were thought to take place along the *same horizontal dimension*, namely at right angle to structural trend; Fig. 2II), much like Hall and his predecessors adding the *sinking* direction to the already existing *vertical dimension*, along which *uplift* had already been discovered.

Although strike-slip faults had been discovered in the 1850's by Escher in Switzerland (Escher von der Linth, 1878, pp.71 and 231, and Suess, 1904, p.116) and Suess (1904) later emphasized their more common occurrence than previously believed, important strike-slip motion parallel with the trend-lines of orogenic belts was a 20th century discovery. On April 18, 1906 the San Andreas fault slipped right-laterally for some 21 feet and destroyed both San Francisco and a number of other smaller towns along its course. Within two years of this tragic, but immensely instructive event, Lawson (1908) applied to it Willis' theory of the lateral spreading of ocean floor (Willis, 1908) and argued that a general sub-surface flow towards the north would produce strains and earthquakes along the western coast of North America of the kind observed during the 1906 San Francisco catastrophe. In his (in retrospect) interesting paper, Willis had concluded, on account of the forms of mountain ranges bordering the Pacific Ocean, that the bed of the ocean was spreading and crowding against the land! So Lawson saw *both* the cause of orogeny *and* the cause of strike-slip faulting *along* the mountain chains in one and the same process. But, unfortunately, Willis' most interesting hypothesis and Lawson's ingenious applications of it to the San Andreas fault [8] did not develop any further. After the advent of the theory of continental drift in 1912–1915, Willis became an arch-opponent, and ideas on strike-slip faulting parallel with orogenic belts had to be cultivated later on European soil independently of Lawson's remarkable early insight.

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[8] In the brief note written by R.L. Faris on the lecture by Willis, which Lawson quotes, there is no indication that Willis himself had thought of applying his theory to the San Andreas fault, although Lawson seems to give the credit to him.

Although the discovery of complicated nappe structures notably within the Pennine zone of the Alps (e.g. Lugeon and Argand, 1905) provided one explanation for de Saussure's third observation, namely what seemed to him as an almost chaotic disorder of attitudes and azimuths of beds and of rock types, its full explanation had to await the idea of sideways motion along the trend of the Alps (Argand, 1916; Steck, 1984) and that was only possible through an attitude that allowed great horizontal freedom for the moving rock masses in mountain ranges and within the framework of some sort of continental drift. This attitude, which now also embraces plate tectonics, Argand (Fig. 2s) called 'mobilism' as opposed to 'fixism' of the older orthodoxy at the time (Argand, 1977, p.125). [9] He summarized the main philosophy of this new tectonic worldview with the following words: "First, restitutions of structures and movements that are limited to one or two dimensions always remain more or less analytical or episodic. There is no tectonic synthesis without the vision of a continuum in three dimensions undergoing deformation" (Argand, 1977, p.3). Argand thus implicitly added what was explicit in Lawson (1908), namely the third dimension of motion missing from the orogenic theories of the contractionists: that of horizontal motion *along* the strike of the chains (Fig. 2III).

Despite the fact that it seemed clear to Argand that continental drift implied movement in orogenic belts along all of the three dimensions, he himself rarely explicitly used motions along the trend of mountain belts, presumably because he thought he could largely account for what he saw in terms of complex horizontal movements acting across the chains and their vertical component (Fig. 2u). Also much of his energy was devoted to fighting the fixist camp and thus he could not (or did not find necessary) to exploit the implications of mobilism fully. In only two of his figures (Argand, 1916, fig. 14, and 1920, fig. 1) there is an explicit reference to strike-slip motion parallel with the trend of mountain ranges, which he called "drift along the shore" (alluding to the 'shore', i.e. margin of the 'geosyncline').

A much clearer, and, to our present views much closer analysis of orogeny in terms of continental drift was developed in a series of four truly remarkable papers by Suess' geologist son Franz Eduard Suess (Fig. 2t). In these incredibly modern-looking contributions Suess (1937, 1938, 1939, 1949) skillfully combined his father's model of asymmetric orogenic belts with the theory of continental drift and blended his own observations from the internal, highly metamorphic zones of the Hercynian orogen in Europe with those of Argand from the Alps into one harmonious synthesis of orogeny. Like Argand, Suess too spent much of his time arguing for continental drift and consequently he too did not systematically explore its implications fully. Again like Argand, Suess too only rarely spoke of lateral motion parallel with the trend of mountain ranges (e.g. Suess, 1949, p.181), but it is as much implicit in his writings as it was in Argand's.

Despite the tremendous insight both Argand and F.E. Suess showed into orogeny, fixist views and two-dimensional thinking dominated orogenic research until the late fifties under the influence of such men as Kober (Fig. 2n) and Stille (Fig. 2o) in Europe and Bucher in the United States. (See Kober's cross-section in Fig. 2p.) It was in such a dominantly fixist—and therefore adverse—atmosphere that recognition of the importance of major strike-slip faults parallel or subparallel with the trend of orogenic belts grew independently of the mobilist tectonic theoreticians.

A resurgence of interest in strike-slip faults parallel with orogenic belts occurred after

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[9] References to Argand's *La tectonique de l'Asie* in this paper are all to its translation into English published by Carozzi. As Carozzi also indicates the original pagination in his translation, I have found it unnecessary also to give the pagination in the original. *La tectonique de l'Asie* was originally published in 1924.

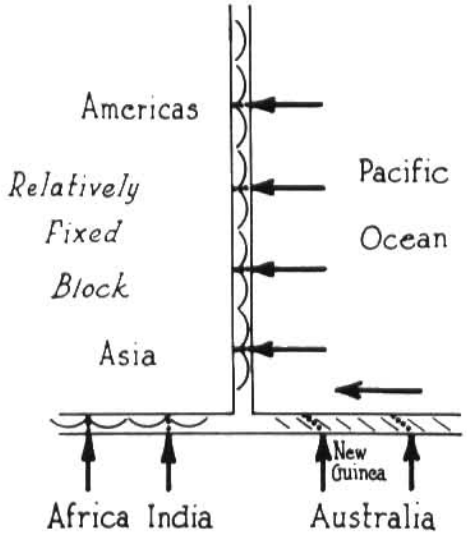

Fig. 4. Motion of the floor of the Pacific Ocean with respect to its surroundings creating head-on shortening along some of its borders, while giving rise to strike-slip *and* shortening along others (from Wilson, 1954). Notice the analysis of motions in terms of rigid blocks with respect to a relatively fixed block, much like the one used later in plate tectonics.

Kennedy's (1946) demonstration of significant strike-slip offset along the Great Glen fault in Scotland. Shortly afterwards Ketin (1948) in Turkey and Wellman (in Benson, 1952) in New Zealand demonstrated the strike-slip nature of the North Anatolian and the Alpine faults, respectively. These were followed by other papers by Wellman (1952, 1955, 1956) and by Hill and Dibblee's (1953) demonstration of a minimum of 350 km right-lateral offset along the San Andreas fault in California.

In 1954 Wilson published—again, in retrospect—a most remarkable figure (which, incidentally, did not make much sense in the framework of the contraction theory he was advocating in the same paper as Löwl's (1906, p.173) objections to Suess' (1878) views also applied to those of Wilson), which showed that while some orogenic belts appeared to have formed by pure compression (those along Asia and the Americas in Wilson's map: Fig. 4), a significant component of sideways motion along others (New Guinea in Wilson's map; also see Brouwer, 1951) seemed inevitable! Two years later, Moody and Hill (1956) carried this view to an extreme in claiming that most 'geosynclinal' sinking and subsequent orogeny were consequences of wrench-fault tectonics!

In two papers published in 1955 and 1958, Carey developed his view of an expanding earth. This hypothesis placed great importance on major strike-slip faults (megashears of Carey) and substantially contributed to the recognition of their widespread occurrence and significance.

Thus, as the review of Billings (1960) clearly shows, by the sixties all three dimensions of motion (vertical, horizontal across and horizontal along the orogenic zone) involved in orogeny had been recognized and widely discussed with shortening across the trend of the orogen being viewed by most geologists as by far the most dominant. However, dominance of motion along any one of the three dimensions was claimed by different theorists (e.g. the Soviet oscillationists preferred the dominance of vertical motions, orthodox contractionists opted for horizontal shortening across the orogenic belts, expansionists argued for extension across the chains, whereas the proponents of rhegmatic shears insisted on strike-slip along them) and because much of the discussion among them was raging across platforms rising on fundamentally different basic assumptions, it was usually fruitless. What was clearly lacking was a comprehensive theory that could explain all of these motions with equal facility. During much of the time interval between World War II and 1960, thermal contraction remained a generally favourite framework in which to discuss the various aspects of terrestrial tectonics, especially orogeny (e.g. Wilson, 1954, 1959; Bucher, 1956), but the number of observations showing horizontal motion not accountable by contraction were rapidly increasing (see, for example, Vening Meinesz, 1959, and Jeffreys, 1973).

This is a convenient place to end our synopsis of the evolution of thought on the spatial aspects of orogeny in pre-plate tectonic days and to summarize its salient results. The most prominent feature of the history we reviewed is the very gradual recognition of the presence and importance of both vertical and horizontal motion across and along the trend of mountain ranges. Vertical motions were the first to be noticed, because their recognition required the least sophisticated information. Next was the recognition of horizontal motion across orogenic belts. Geologists became aware of it after they had completed the first crude mapping of folds and tilted blocks across an orogen. The last dimension to impress itself upon the geologists was the one along the mountain chains. Movement along this dimension was proven by Lawson et al. (1908) and argued for by Argand (1916, 1920) and Suess (1949), but its widespread recognition had to await the fifties, because evidence for strike-slip, especially in zones of complex deformation, where much of the regional strike is parallel or subparallel with sideways motion, is much more difficult to recognize. *One of the major points of this review is that the realization (and justification) of the extreme importance and locally the dominance of horizontal motion along the trend of mountain belts is probably one of the most important contributions that plate tectonics made to our understanding of orogeny.*

Temporal aspects of orogeny [10]

The discussion in the preceding subsection was concerned with spatial aspects of orogeny, in which we have noted a progressive growth of recognition from the 18th to the 20th century. By contrast when we look at the history of ideas on the temporal aspects of orogeny, i.e. on timing of events and on how orogens evolve in time, we see a history of conflict between two fundamental schools of thought, rather than one of progressive growth of understanding.

As in the case of spatial aspects, the invention of biostratigraphy marked a turning point—and, in terms of understanding the only real progress before plate tectonics—in the history of the evolution of ideas on the temporal aspects of orogeny. It made orogenies datable. But no sooner they became datable than a major conflict arose between Sir Charles Lyell and Elie de Beaumont about how to interpret the observations on dating in orogenic belts. A measure of the progress made since can perhaps be given by pointing out that the very same conflict was the subject of a famous debate between Gilluly (defending Lyell's position) and Stille (representing Elie de Beaumont's side) in 1950, and again recently of a Fermor lecture by Hsü (1989: in his lecture Hsü took Lyell's and Gilluly's position), in which Trümpy (as represented by his 1987 farewell address on the occasion of his retirement from active teaching: Trümpy, 1987) appears as the principal adversary (representing the camp of Elie de Beaumont and Stille). Because the main points of this long-lasting debate have remained almost the same, I quote the positions of Elie de Beaumont and Lyell in the following to indicate the origin and to summarize the basic content of the debate.

"When examined with some care, it is seen that along almost all mountain chains recent beds extend in a horizontal position as far as the foot of the mountain, indicating that they were laid down in the sea or in lakes whose shores were partly formed by these mountains. By contrast other beds, which are upturned and which turn around the flanks of the mountains, reach, in some regions, as far high as the summit...

... But it is necessary to make a remark also, before all others, about this natural division of the beds into two classes in every mountain chain, namely those that have been upturned and those that have not: it is the constant sharpness of the separation of the two classes...

... Now a distinction, which is always sharp, and which allows no intermediaries, thus results from this observation between the

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[10] For a more detailed discussion on this topic see Şengör (1989a).

upturned and the horizontal beds. One concludes that the phenomenon of upturning was not continuous and progressive; it operated in a time interval between the periods of deposition of the two consecutive terrains and during which no deposition of regular beds took place. In one word it was brusque and of short duration.

Such a convulsion that upturns the beds in an entire mountain range necessarily interrupts the slow and progressive development of sedimentary terrains and it is clear that some anomaly must be observed nearly universally at a point in such series which corresponds with the moment at which the upturning of the beds took place" (Elie de Beaumont, 1830, pp.7–10).

These views of Elie de Beaumont (see also Elie de Beaumont, 1829, pp.5–9 and 1831, pp.242–243), which are almost identical to those of Stille (see especially Stille, 1918, 1922a, 1924) immediately came under fire from Lyell:

"Now all this reasoning (referring to the above quotation from Elie de Beaumont's 1830 paper) is perfectly correct, so long as the particular groups of strata b and c (upturned and horizontal beds of Elie de Beaumont, respectively) are not confounded with the geological periods to which they may belong, and provided due latitude is given to the term contemporaneous; for it should be understood to allude not to a moment of time, but to the interval, whether brief or protracted, which has elapsed between two events, namely, between the accumulation of the inclined and of the horizontal strata.

But, unfortunately, the distinct import of the terms 'formation' and 'period' has been overlooked, or not attended to by M. de Beaumont, and hence the greater part of his proofs are equivocal, and his inferences uncertain; and even if no errors had arisen from this source, the length of some of his intervals is so immense, that to affirm that all the chains raised in such intervals were *contemporaneous*, is an abuse of language" (Lyell, 1833, p.341).

Elsewhere (Şengör, 1982c) I have discussed at length that here we see a fundamental conflict between two entirely different schools of thought in tectonics, in fact in physical geology as a whole. [11] One, represented by Elie de Beaumont, was a catastrophist school that believed that nature behaved in a regular, perfectly deterministic manner. The other, to which Lyell belonged, had a uniformitarian philosophy and believed in an inherently irregular nature, in which indeterminism was a realistic position to take. The predecessors of these two schools were respectively the 'miners' and the 'physiographers' that I discussed above, and their descendants in the 20th century were represented by what I called the Kober-Stille school and the Wegener-Argand school, respectively. The former more or less corresponds with Argand's fixists, the latter with his mobilists, but the correspondence is not exact. For example, although Nopcsa (e.g. 1933) was a mobilist, he nevertheless belonged to the Kober-Stille school—he was a catastrophist and believed in the existence of regularity in nature (Weishampel and Reif, 1984). Similarly, the fixist J. Tuzo Wilson (he remained a fixist until about 1960) was nevertheless a Wegener-Argandian, because he believed in uniformitarianism and did not impose regularities on nature.

Kober-Stilleans and their predecessors have always believed that orogeny was an episodic (or, according to some, even periodic) event that took place everywhere in the world at the same time—during well-defined orogenic phases of short duration. Even before Elie de

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[11] These two schools correspond with the *historical* and *causal* traditions identified by historians of geology (e.g. Laudan, 1987). I have my doubts whether these two terms adequately describe the philosophical outlook of the proponents of the two schools as I defined them in my 1982 paper, mainly because they do not indicate why certain geologists might give preference to the establishment of the historical sequence of events, while others think first the nature and origin of the events need to be elucidated. See also Şengör (in press, a).

Beaumont, Leopold von Buch had developed similar ideas (von Buch, 1808, 1822a, b; for an excellent summary and rich literature references on von Buch's catastrophist views on mountain-building see Semper, 1914, pp.184–188), whose roots could be traced still further back to the 'miner's tradition', in which in fact both von Buch and Elie de Beaumont had been educated. Beginning with Elie de Beaumont, angular unconformities became the most reliable indicators of orogenic 'revolutions' (Vogt, 1847, esp. pp.238–295; Stille, 1924, 1950).

Wegener-Argandians and their predecessors acknowledged, as Argand (1977) pointed out, that locally orogeny could be an episodic affair, but that it did not have to be. Suess (1875), for example, argued that the formation of the Alps had been going on since the Mesozoic. They also had difficulty seeing the 'magic time markers' in angular unconformities. As Argand put it: "...unconformities and ... transgressions that mark lacunae serve to date certain noticeable phases of the movement, nothing more; ... these lacunae never prove the interruption of the movement, but only that of sedimentation" (Argand, 1920, p.4).

During much of the 19th century, the forebears of the Kober-Stilleans dominated the scene under the influence of such strong personalities as von Buch, Elie de Beaumont, Roderick Murchison, and Adam Sedgwick. Beginning with the 1870's the pendulum swung the other way, and the great predecessor of the Wegener-Argandians, Eduard Suess, dominated the scene.

The beginning of the 20th century, especially after 1914, witnessed tremendous social upheavals that for a long time disrupted world-wide scientific communication. This, combined with the rise of fervent nationalism, led to the development of what Muir-Wood (1986) with some justification calls 'reactionary' schools of thought in tectonics that sought solace in a deterministic tectonic world picture, in which slow evolutions, characterized by the secular subsidence of geosynclines and world-wide transgressions, were from time to time interrupted by rapid, world-wide orogenic revolutions that led to regressions and, also, to ice ages (e.g. Kober, 1923; Stille, 1924; Bucher, 1933; Grabau, 1940).

This essentially 'Beaumontian' world-view dominated thinking in tectonics. Notable critics such as Berry (1920), Shepard (1923), and Gilluly (1949) continued to point out the unreasonableness of this view, in the light of the uniformitarian principle, but few of these critics seem to have noticed that a very fundamental philosophical difference between them and their opponents, the way they both viewed the deterministic regularity and uniformitarianism in tectonics, was the real issue. Because this fundamental disparity was not discussed, the debates failed to convince either side.

By the fifties, accumulating field evidence, whose resolution had been increased greatly by micropalaeontology, began indicating, in the words of Krejci-Graf (1950, p.123), that "orogeny frequently lasts continuously through a number of epochs or periods. Locally it develops episodically, which indicates stress release in an inhomogeneous medium. Paroxysms are neither synchronous nor world-wide." Looking at what he called the "Middle North America", King (1951, pp.78–80) came to a very similar conclusion and Wilson (1954) summarized the prevailing opinion, especially in the English-speaking world, as follows: "To suppose with Stille that periods of (orogenic) activity are world-wide and alternate with world-wide periods of quiescence is held to be contrary to the geological evidence and would require a cyclical cause, the nature of which has never been satisfactorily explained. On the other hand, there is still something to be said for Stille's view: For any part of a continent there was a period when it was being actively mountain-built ... That period usually lasted a few hundred million years. Since then any area would have remained quiescent ... After these one or at most two cycles, any area has remained an inert part of a shield" (Wilson, 1954, p.182).

Thus, by the fifties two of Stille's three main theses, i.e. the world wide *and* contemporaneous nature of orogeny had been mostly given up and only episodicity had remained. But, as Krejci-Graf (1950) correctly pointed out, the observed episodicity was a *local* phenomenon, brought about by the mechanical behaviour of the deforming medium as Wegener-Argandians had long ago pointed out (Argand, 1977; Suess, 1937), and *not* a world-wide coordinated phenomenon as Kober-Stilleans made it to be! Thus, Stille's third main point, episodic orogeny *as a consequence of a world-wide coordinated phenomenon*, had also gone down the drain, but neither the Wegener-Argandians nor the Kober-Stilleans noticed the fundamental difference between episodic deformation as a local phenomenon and episodic orogeny as a world-wide phenomenon, although this aspect was emphasized by von Bubnoff in 1958 in a paper that received little circulation in the west. Owing to this confusion, the Kober-Stillean view continued to live a ghost-like post-mortem life even after the advent of the theory of plate tectonics.

By the sixties, all that was generally admitted of the Kober-Stillean world-view was the broad grouping of main deformational episodes such as Caledonian, Hercynian and the Alpine 'Orogenies'. Rutten (1962) summarized this position in the following imaginary dialogue between a coal geologist (C) and his one-time professor (P).

"C: Consequently, Sir, orogenetic disturbances must be the result of a local development in the crust. They might, of course, be due to some local development under the crust. But there is no general history underlying the orogenies of the earth. And consequently all geotectonic theories which use general, worldwide factors can be discarded. This at least I would like to call a positive result following from our discussion.

P: Please, my friend, hold your horses. Do not forget the general grouping into broad periods of orogenic unrest, such as hercynian and alpine orogenies. *There is some statistical worldwide contemporaneity in that*. Of course, it is easy to cite orogenetic movements having taken place outside of these broad periods, but *statistically* they are not so very important" (Rutten, 1962, p.606; italics mine).

The important message in Rutten's dialogue is the emphasis on *statistical contemporaneity*. It was thus becoming apparent to tectonicians that the Kober-Stilleans had been perhaps defending "a programme of giving a strictly deterministic theory of statistically distributed events (that would) lead nowhere" (quoting Popper, 1982, p.101 on the physicist Alfred Landé). The constant need to revise the timing and number of 'orogenic phases' by Kober-Stilleans had begun to suggest that perhaps orogeny was indeed an event statistically distributed in time and in space, analogous to random brownian motion of gas molecules. Just as in brownian motion, where the sum of discrete collisions with the walls of the container provides a constant (continuous) pressure, randomly distributed orogenic events as Rodgers (1972) suggested may provide justification for the view of continuity of orogenic events in time.

As we reached the magic year 1962 with Rutten's dialogue, it is appropriate the break off the narrative of pre-plate tectonic views on the timing of orogenic events. In summary, by 1962 most geologists in the world had come to realize that orogeny was perhaps a statistically distributed event, thus wittingly or unwittingly the geological community by and large had abandoned the Kober-Stillean schemata. Some of the orogenic events were truly episodic and very short-lived, whereas others lasted longer in time. Those who saw the episodic nature as fundamental thought they were 'neo-Stilleans', whereas those who considered orogeny continuous thought they were neo-Lyellians. Neither of the groups identified their ancestry correctly and, until plate tectonics, both lacked a theory to back up their suspicion that orogeny was not as regular a phenomenon as Kober-Stilleans had made it to be.

PLATE TECTONICS AND OROGENY—INITIAL CONCEPTS

Plate tectonics

Plate tectonics is the theory that holds that the earth's *lithosphere*, commonly including the crust and the upper mantle, is divided into a finite number of *torsionally* (but *not flexurally*) *rigid* caps, called *plates*, that are in constant motion with respect to each other along three kinds of boundaries: (1) *extensional* (also called 'constructive' or 'divergent'); (2) *compressional* (also called 'destructive' or 'convergent'); and (3) *strike-slip* (also called 'conservative' or 'transform') although only 20.5% of plate boundaries show normal convergence, 21% show normal divergence, and 14% pure strike-slip displacement. The rest show oblique motion across the boundary which is accommodated in diverse ways (Woodcock, 1986). By definition, these boundaries form an interconnected network encircling the globe and are marked by considerable seismicity (over 95% of the world's total seismic activity occurs along them: Isacks et al., 1968). Along most extensional boundaries oceanic lithosphere is created by *sea-floor spreading* (Dietz, 1961) along mid-ocean ridges and destroyed by *subduction* (White et al., 1970) along most convergent boundaries. Strike-slip boundaries connect other kinds of boundaries (or other strike-slip boundaries) with one another, whereby they 'transform' the motion from one kind of boundary to another. They are thus also called *transform faults* (Wilson, 1965) and extend between two *transform points* at which a transform fault is connected with another plate boundary (Wilson, 1965). Plate tectonics provides an instantaneous, kinematic description of the present large-scale deformation of the lithosphere.

The theory of plate tectonics was first proposed in its basic outline as summarized above by Wilson (1965), who noted that if the sea-floor spreading/subduction mechanism proposed by Hess (1962) was to account for the history of the ocean basins as we know them, a third kind of boundary, namely the transform faults, was a kinematic necessity. Thus, although the initial stimulus came from Dietz

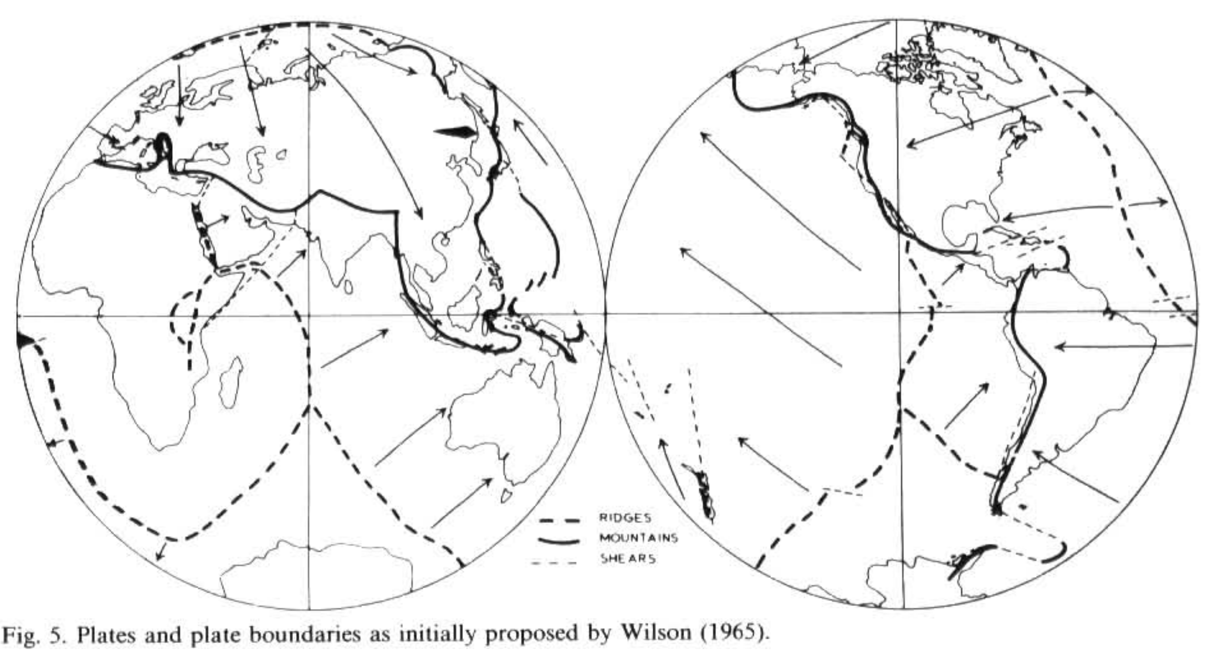

Fig. 5. Plates and plate boundaries as initially proposed by Wilson (1965).

(1961) and Hess (1962), the theory was born in 1965 with the three kinds of plate boundaries enclosing plates in constant motion relative to one another as explicitly stated by Wilson (1965) (Fig. 5).

McKenzie and Parker (1987) and Morgan (1968) elaborated the consequences of plate motions on a sphere and McKenzie and Morgan (1969), Atwater (1970), Smith (1972), Dewey (1975), Patriat and Courtillot (1984), and McKenzie (1984) further extended the kinematic theory and its geological implications. For more detailed statements of the theory see McKenzie (1970b, 1972a, b), Dewey (1972), Le Pichon et al. (1973) Chase et al. (1974) and especially Cox and Hart (1986). For good histories of the evolution of the theory I recommend Bullard (1975), McKenzie (1977), and Le Pichon (1986). For three excellent anthologies of the 'classical papers' see Cox (1972), Bird and Isacks (1972), and Schönenberg (1975).

Plate tectonics–orogeny relationships and J.T. Wilson

The application of the plate tectonic theory to orogeny was an affair entirely different from the further development of the basic kinematic theory (as exemplified, for instance by the triple junction paper by McKenzie and Morgan, 1969). Plate tectonics is essentially a kinematic theory describing and accounting for *displacements*. Orogeny, on the other hand, pertains to *strain*. In order to account for orogeny in terms of plate tectonics, one has to explain how the displacement, caused by plate motions, is converted into strain along zones of plate convergence, to make mountains. Le Pichon (1987, p.14) recently observed that "it is a curious point that plate tectonics, which is the universally adopted geodynamic framework today, totally ignored orogeny in its beginnings and to this day there is no well-founded theory on this topic, despite the fact that the origin of mountain ranges was the quasi-exclusive concern of the preceding theories of geodynamics".

As I briefly pointed out above, Wilson played a key role also in the application of plate tectonics to orogeny. He was eminently qualified to do so, for he not only had developed the theory, but had spent a good deal of his professional life worrying about how to account for orogeny, as we have seen above. Most importantly, Wilson was aware of the three conditions that Billings (1960) had thought any successful theory of orogeny had to fulfill (pp. 6 and 7). Wilson also belonged to the camp that considered orogeny a statistically distributed event both in time and in space. This important background knowledge that Wilson brought into the plate tectonics–orogeny relationships is reflected in a most significant paper he wrote in 1966 on a set of seven "tentative rules governing continental drift" that are worth quoting here (Wilson, 1966b, p.16; italics and italicised annotations in parentheses are mine):

"1. Compression of great but unmeasured amount may take place along active primary mountains (*i.e. subduction and collision belts*). Some rotation of the sides may accompany compression.

2. Shearing may take place *along the same systems* or on independent transcurrent fault zones.

3. Expansion may occur at mid-ocean ridges and may be accompanied by some rotation of the sides.

4. Neither continents nor ocean floors have undergone random deformation. All distortion and movement can be traced to one of the above zones of fracture.

5. Mid-ocean ridges usually end in large transcurrent faults. They may pinch out (*at a transform point*) and their place be taken by compression ranges beyond the fulcrum (*which is the transform point*) (e.g. the Arctic Mid-Ocean ridge changing to the Verkhoyansk range).

6. Some places in the mantle generate much lava and such places (*i.e. hot-spots*) give rise to aseismic ridges. These ridges (*i.e. hot-spot tracks*) may show the locus of movement of the crust past such sources (e.g. Hawaiian islands).

7. Where two aseismic ridges lead either away from a volcanic island or a mid-ocean ridge to opposite continents, the ends of the ridges were once in contact (Walvis and Rio Grande ridges)."

In this remarkable list, only the first two items and partly item 5 deal directly with orogeny in terms of the new theory. I wish to underline here particularly Wilson's item 2, where he pointed out, that shearing, i.e. strike-slip motion may take place *along* orogenic belts as a general characteristic. Now, as our review of the spatial aspects of orogeny has indicated above, this was an idea much in vogue in the fifties and the early sixties *in pre-plate tectonic views of orogeny*. But, despite Wilson's early emphasis it somehow vanished from the literature, and in most of the 'initial papers' I listed above, we no longer see it! Only in the late seventies and eighties have orogenic geologists 'rediscovered' it (e.g. Trümpy, 1977; Woodcock, 1986). The reason why Wilson's extremely important observation was not taken into account early must be, I think, because most of the authors of the 'initial papers' had been trained in a fixist environment that encouraged thinking in two dimensions, along cross-sections only, i.e. in terms of horizontal shortening across orogenic belts. Also, there was the two-dimensional aspect of Hess' 1962 paper stressing only sea-floor spreading and subduction. To many, the new theory finally accounted for the cross-sectional view of orogenic belts, oceans, and rift valleys with which they had been mostly brought up. Despite Wilson's pointed remark, they forgot that the structures viewed in cross-section also had a third dimension, about which much data had already been gathered.

In 1968, Wilson published his short, but important paper entitled *Static or mobile earth: the current scientific revolution* in the not easily accessible *Proceedings of the American Philosophical Society*. There he extended the ideas he had first presented in Wilson (1966a) and argued that "if continental drift has been going on for an appreciable part of geological time, at such rates as recent work suggests, it means that a succession of ocean basins may have been born, grown, diminished, and closed again. Since ocean basins are the largest features of the earth's surface, and would dominate other features it seems useful to outline the stages in their life cycle *in terms of present examples*" (Wilson, 1968, p.312; italics mine). This outline Wilson presented in table form, which I have reproduced as Table I.

But the cycle Wilson outlined was not a simple concertina-style ocean opening and closing as frequently assumed and criticized (e.g. Woodcock, 1986, p.25)! On the example of the North American Cordillera, Wilson

TABLE I

Stages in the life-cycle of ocean basins and their properties (from Wilson, 1968)

Stage	Example	Motions	Sediments	Igneous rocks
1. Embryonic	East African rift valleys	Uplift	Negligible	Tholeiitic flood basalts, alkalic basalt centres
2. Young	Red Sea and Gulf of Aden	Uplift and spreading	Small shelves, evaporites	Tholeiitic sea-floor, basaltic islands
3. Mature	Atlantic Ocean	Spreading	Great shelves (miogeosynclinal type)	Tholeiitic ocean-floor, alkali basalt islands
4. Declining	Western Pacific Ocean	Compression	Island arcs (eugeosynclinal type)	Andesitic volcanics, granodiorite–gneiss plutonics
5. Terminal	Mediterranean Sea	Compression and uplift	Evaporites, red beds, clastic wedges	Andesitic volcanics, granodiorite–gneiss plutonics
6. Relic scar (geosuture)	Indus line, Himalayas	Compression and uplift	Red beds	Negligible

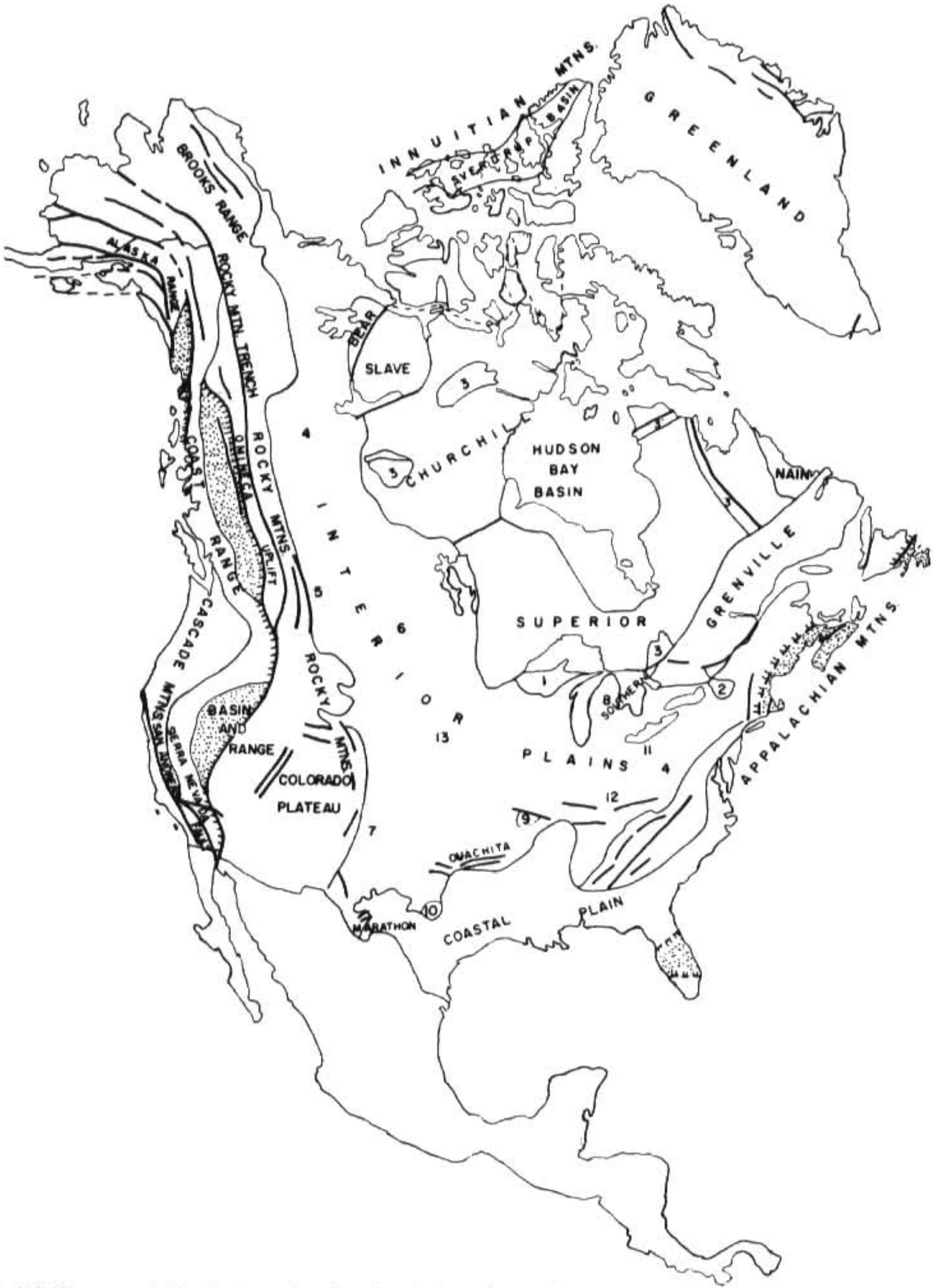

Fig. 6. Wilson's (1968) map of North America showing "junctions of formerly separate continents". Compare this map with one showing the 'terranes' in western North America (e.g. Coney et al., 1980, fig. 1).

(1968) also showed, partly following Argand's (1977) earlier example, that major orogenic belts were *composite features*, consisting of a number of island arcs now swept into them and strike-slip generated fragments (see also Coney et al., 1980; Kerr, 1980). He wrote (Wilson, 1968, p.316): "The reason for suggesting (one) or perhaps two more fragments (in the North American Cordillera) is the increasing evidence that there are large differences and great dislocation between the Cordilleras of Canada and those of the United States. For example the Rocky Mountain Trench, which seems to be an old strike-slip

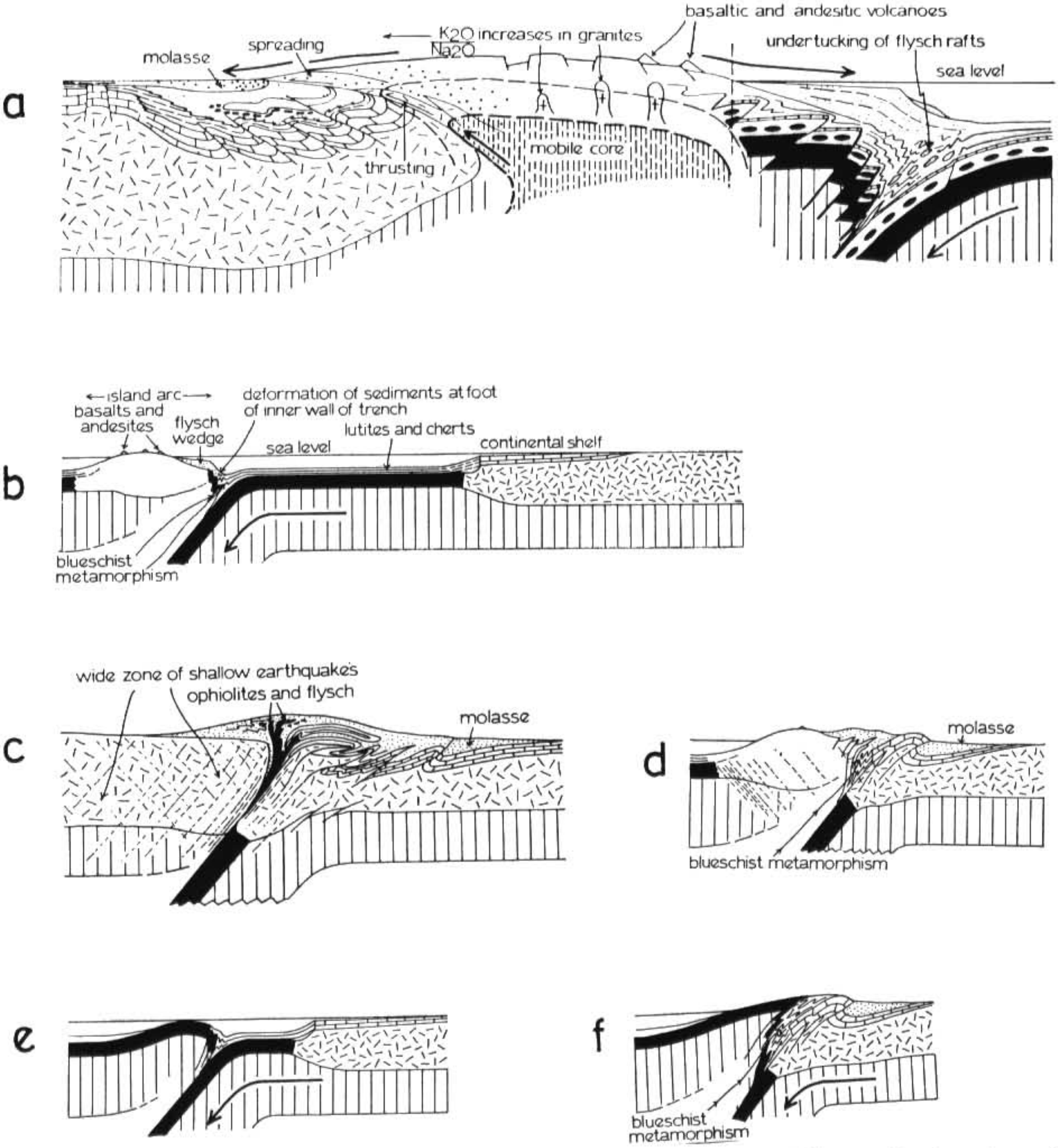

Fig. 7. Early plate tectonic classification of orogenic belts by Dewey and Bird (1970a). a. Continental margin magmatic arc orogen. b. Island arc orogen. c. Collisional orogen formed by continent–continent collision. d. Collisional orogen formed by continent–island arc collision. e and f. Ophiolite obduction and resultant deformation forming an orogenic belt (in f).

fault of great displacement (Roddick, 1967), ends in Montana at a great transverse discontinuity recently described by Yates (1968) and Hamilton and Myers (1967)." Fig. 6 reproduces Wilson's (1968) fig. 6, "a sketch map of North America showing junctions of formerly separate continents." This is partly what the recently developed "terrane analysis" (see below) has rediscovered, but, as we shall see later, not with the same insight and accuracy that Wilson (1968) originally displayed!

In summary, by 1969, the two important concepts for which plate tectonics provided the best explanation so far, namely the tremendous three-dimensional internal mobility of orogenic belts, and the fact that at least major mountain belts are formed from the agglomeration and later redistribution, mainly by strike-slip faulting, a number of smaller 'fragments', had been interpreted in terms of plate tectonics by Wilson.

Plate tectonics–orogeny relationships up to 1972

The years 1969 to 1972 inclusive witnessed the publication of most of the 'initial papers' on orogeny–plate tectonics relationships. These papers were mostly cross-sectional in their approach and mainly dealt with the explanations, in plate tectonic terms, of the older concepts of orogenic development, such as geosynclinal sinking, compressional deformation, flysch and molasse deposition, origin of mélanges, and also with the classification of orogenic belts. Both the mobilists (e.g. Argand, 1977; Suess, 1937) and the fixists (e.g. Kossmat, 1936) had earlier distinguished *intracontinental* or *geosynclinal mountain ranges*, such as the Alps or the Himalaya, from *continental margin ranges* such as the Andes. Dewey and Bird (1970a) transplanted this distinction into plate tectonics by showing that the former class was formed through continental collision and that the latter resulted from subduction of the oceanic lithosphere along a continental margin. Dewey and Bird (1970a) noted that subduction gave rise either to continental margin magmatic arc orogens such as the Andes (Fig. 7a) or to island arcs such as the Marianas or Japan (Fig. 7b). Consequently, collisional orogeny could take place between two continents, as had been the case in the Himalaya (Fig. 7c) or between a continent and an island arc (Fig. 7d) or between two island arcs.

Dewey and Bird (1970a) pointed out, using their earlier experience in the Appalachian/Caledonian chain (Dewey 1969a; Bird and Dewey, 1970), that orogenic evolution in any one belt could, in time, include the development of all three types of orogenic belts, as Argand (1916) and later authors had earlier indicated that mountain belts first pass through an island arc stage, before the belt as a whole is deformed in a final collisional paroxysm.

An important novelty in the early stages of the exploration of the relationships between plate tectonics and orogeny was the appreciation of the significance of ophiolites. With notably few exceptions (e.g. Argand, 1977; de Roever, 1957), most geologists had viewed the ophiolites in mountain ranges earlier as submarine effusions of mafic and ultramafic lava onto the floor of the 'geosyncline', through either axial (e.g. Stille, 1939) or flank eruptions (e.g. Brunn, 1960), that had later been incorporated into the orogenic edifice during deformation. Although early plate tectonic interpretations of orogenic belts inherited this view of ophiolites (e.g. Bird and Dewey, 1970), accumulating information on the thickness, layering and composition of the oceanic crust and upper mantle had already led to the proposition that ophiolites may be slices of these, tectonically emplaced into continents during orogeny (Dietz, 1963a; Hess, 1964; Gass 1968; Laubscher, 1969; Thayer, 1969). Both Coleman (1971) and Dewey and Bird (1971) later elaborated on the idea that they could perhaps be better explained as remnants of oceanic crust and upper mantle tectonically incorporated into the orogenic edifice. Coleman (1971) coined the term 'obduction' [12] to express the process of tectonic preservation of ocean floor as opposed to 'subduction' signifying its tectonic destruction (Fig. 8e). In places, it seemed that obduc-

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[12] W.B. Hamilton reminded me that in its initially proposed form obduction seemed to imply emplacement of ophiolites on continental margins on thrusts that develop antithetically with respect to a subduction zone. It is important to know that now this restricted usage is not current.

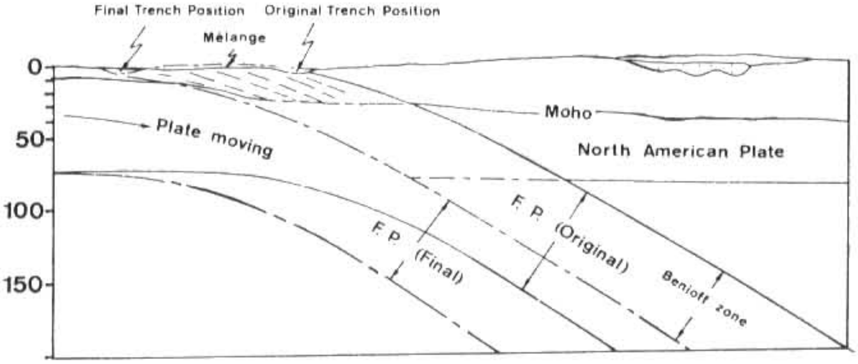

Fig. 8. Hsü's (1971) concept of mélange generation as an accretionary prism at the prow of an overriding plate. Dewey and Bird (1971) pointed out that ophiolites could be wedged into such an accretionary prism.

tion alone generated orogenic belts (e.g. Oman: Reinhardt, 1969; Ricou, 1971; Fig. 7f).

Another form of preservation of oceanic material was proposed by Hsü (1971) in his reinterpretation of the Franciscan assemblage in California as a mélange wedge generated along the prow of an overriding plate (Fig. 8). The concept of mélange has occupied an important place in orogenic studies since, mainly owing to its widespread occurrence in both subduction and collision-controlled orogenic belts.

Obduction may be succeeded by subduction in an opposite direction and the two processes are linked by an event called 'subduction zone flipping' first outlined by McKenzie (1969). This mechanism allowed also switches of subduction zone dip following collisions of island arcs with continents and thus provided an early plate tectonic rationale for 'episodic' orogeny in a regime of continuous plate motion.

An extremely important point in the initial development of the orogeny–plate tectonic relationships pertained to the geosynclines. Much ink was spilled initially to account for geosynclines in plate tectonic terms (e.g. Mitchell and Reading, 1969; Dewey and Bird, 1970b, Dickinson, 1971; Hsü, 1972, 1973; Wang, 1972) and Hsü wrote nostalgically "I have gone on at considerable length to criticize a fallen idol (i.e. the geosyncline concept), or as one might even say, to beat a dead horse. Is there any saving grace? Or should we delete altogether the word 'geosyncline' from our vocabulary? Personally I would not pursue such an extreme course, as a link with the past is necessary... Besides, the term can still serve as a useful descriptive abbreviation to connotate large packages of rocks" (Hsü, 1972, p.37). In his nostalgia, however, Hsü seemed to have forgotten that his question, whether one should abandon the word geosyncline altogether, had been answered in the affirmative by Suess some 60 years before him, and Suess also had pointed out that geosynclines had been invented as expressly structural features and not as rock packages. Suess had written: "Speaking generally, with the exception of bays belonging to rias coasts, no tract of the sea is known which has been transformed by lateral pressure into a syncline." Then he had elaborated further: "The lie of the sediments which fill it is possibly synclinal... This, however, is not the tectonic conception of the *geosyncline*... For this reason I regret that I at first employed the term geosyncline in this work; subsequently I avoided it." (Suess, 1909, p.627; see, however, Andrée, 1914, p.20, footnote 1, where Andrée seems to have misunderstood entirely Suess' point and claims that the term geosyncline refers only to a *morphological* feature and not to a specific structure. He thus feels that 'geosyncline' could no longer be excluded from geological terminology! See, in the next paragraph, Wegener's objection to a similar idea by Haug.)

Argand (1977) also confessed that the mobilist theories had somewhat neglected the geosyncline concept—because they had no need for it as explicitly shown both by Wegener (1915, p.35, footnote; Fig. 2r) and F.E. Suess (1937). Wegener wrote, in response to Haug's statement that mountain ranges grow out of geosynclines: "I hold that 'shelves' would be more correct than 'geosynclines', as a marginal shelf, such as the one out of which the Andes of South America grew, cannot very well be described as a syncline" (Wegener, 1915, p.35). Both Andrée (1914, p.73, based on Wegener's 1912 papers) and Wegener's friend, the fixist Hans Cloos, saw his point and Cloos developed the concept of a "geomonocline" as opposed to a "geosyncline" (Cloos, 1936, p.460). Indeed, this term was revived by Dietz (1963b), shortly after continental drift had become fashionable again through sea-floor spreading, in the somewhat altered form of a 'geocline', with its two familiar variants 'miogeocline' and 'eugeocline'!

Despite the terminological muddle that dominated discussions on geosynclines, it had become clear by 1972 that the *tectonic concept* of geosynclines was clearly wrong and many authors abandoned the term as a result. Those who have continued to use it have done so mostly to designate, as Hsü (1972, 1982b) recommended, 'packages of rock'. In the conclusion of this essay I shall come back to the question of the continued use of the term geosyncline to combat it by showing that it is both superfluous and may be very misleading.

The final issue that I need to mention in the development of orogeny–plate tectonics relationships up to 1972 is the problem of sideways motion in orogenic belts. McKenzie (1970a) was the first to tackle it in terms of plate tectonics when he noticed from the seismicity that in Turkey the dominant motion now was not across the trend of the orogen, but parallel with it, an observation McKenzie (1970a, p.4) thought was "surprising", in the light of the convergence of Eurasia with Africa. But he pointed out that these motions "are such as to minimize the work which need be done to move the African plate towards the Eurasian" (McKenzie, 1970, p.4), because it seemed easier to consume oceanic lithosphere by shoving slivers of continent onto oceanic embayments from nodes of collision than to go on thickening the continental crust against gravity.

Pre-collisional sideways motion in orogenic belts was also shown to be important by Fitch (1972). He ascribed it to oblique subduction, whose head-on component was taken up by the subduction zone, while the sideways component was accommodated by strike-slip faults within and parallel with magmatic arcs.

By 1972 most of the important spatial aspects of orogeny in terms of plate tectonics had been worked out, but not all of their implications had been yet exploited. Especially, orogenic evolution in three dimensions had not yet been tackled in any detail. Despite the recognition by McKenzie (1970a) and Fitch (1972) of the importance of strike-slip motion along present-day convergent boundaries, and despite the presence of a large amount of data on strike-slip along older orogenic belts (Billings, 1960), the importance of this phenomenon was not emphasized until about a decade ago. The papers by Roeder (1973) and Dewey (1975) were I think seminal with respect to three-dimensional treatment of orogeny, and only after 1975 the full potential of plate tectonics for orogeny began to be recognized by geologists.

The question of timing of orogenic events received only scant attention amidst general enthusiasm of process-oriented research in trying to apply this theory to all conceivable processes of orogeny. In his presidential address as retiring president to the Geological Society of America, Rodgers (1971) was the first to look at this question in the framework of the theory of plate tectonics and on the basis of the data from the Taconic orogeny concluding that "detailed analysis of the 'fine structure' of the Taconic orogeny combats the dogma that orogenies are sharp, discrete events punctuating the geological record ... and suggests instead that they reflect 'random walk' processes within the Earth, in all likelihood the same as those responsible for sea-floor spreading and the present tectonic state of the Earth" (Rodgers, 1971, p.1141). Until Trümpy (1973) revived the concept of episodic orogeny two years later, Rodgers' conclusion was the one most geologists implicitly or explicitly accepted. I shall also return to the question of the timing of orogenic events in the final section of this essay to combat neo-Stilleans such as Schwan (1985, 1986) and to show that Trümpy's (1973) data and even his interpretation of the *locally episodic* character of orogeny are in perfect agreement with a plate tectonics-based interpretation of orogeny and in no way necessitate a return to anything like Stille's phase schema (see also Şengör, 1989a).

So far I have looked back in an attempt to see what biases, what prejudices we have developed over the two-and-a-half centuries during which we have studied orogenic belts seriously, along with the tremendous progress that has taken place. In the second half of this paper I try to show where we stand now. My emphasis in the following paragraphs is on the structural (i.e. spatial) and temporal aspects of orogeny as illustrated on examples selected mainly from the Tethysides. I first review, in the next section, my present thoughts on the *cross-sectional* aspects of orogeny and orogenic belts, and there I introduce the types of orogenic belts (Table II) and their relationships in time and space and discuss some outstanding questions related to cross-sectional aspects such as post-paroxysmal uplift and the uplift mechanism of buleschists as examples of 'unexpected motions' in orogenic belts. Next, I consider their *map-view* aspects. This consideration shows, among other things, how a number of methods developed on cross-sections, such as balanced cross-sections and balanced restorations across the strike, break down. I then consider the *temporal aspects* of orogeny as we view them today, and finally briefly review the relevance of orogenic studies to such global topics as sea-level changes, world climate, and palaeobiogeography.

CROSS-SECTIONAL ASPECTS OF OROGENIC BELTS

As orogeny is defined as *a collective term for convergent margin processes*, we have to look at the present-day plate boundaries to see what kinds of orogenic belts there may be. By definition, *all* orogenic belts are products of *convergent boundaries*, i.e. of *plate convergence*. Plates converge in three major settings today, namely (1) across subduction zones, (2) across active colision zones, and (3) across constraining bends along transform faults. These are therefore the main types of orogenic belts. But when we look at each one of these main classes, a tremendous diversity is seen. Some subduction zones dip under island arcs, others under continental margins. Along some of these subduction-margins there are large orogenic belts such as the Andes; along others this is not the case. Some collisional orogens, such as the Alps, have large continental thrust sheets overriding the other continent, whereas some others such as the Himalaya never had them...

This tremendous diversity underlines one truism: every orogenic belt is unique. But, in a similar way, every animal is unique, although we still have the science of zoology applicable to all animals because they have a common structure, albeit broad classes, orders, families, genera, species and races may differ in different details. Similarly, amidst their bewildering variety, orogenic belts also have an underlying common structure and they too fall into broad classes in which there are differences of structure in detail.

Table II is a family tree of orogenic belts showing 20 main types. These 20 main types ('genera') are represented in a hierarchical classification borrowed from zoology to show