Terranology: Vice or Virtue? [and Discussion]

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Terranology: vice or virtue?

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The concept of terranes and the methodology of terranology are not intrinsically new ideas and methods that have advanced our understanding of orogenic and plate boundary processes. They are, rather, a new formulation of ideas inherent in earlier concepts of displacement partitioning in plate boundary zones and much earlier ideas about orogenic elements and zones. Terranology has had the useful effect of emphasizing, and awakening the tectonic community at large to, the possibility of very large orogen parallel strike–slip motions that predate continental collisions.

INTRODUCTION

The recognition, by Wilson (1966, 1968), Moores (1970), Monger & Ross (1971), Dewey *et al.* (1973), and Roeder (1973), among others, of the interlacing mosaic-like geometry and the complex evolution of orogenic belts (see, for example, Dewey 1975, 1976), was reformulated recently in the form of the mainly descriptive allochthonous or suspect tectonostratigraphic terrane concept (Monger 1975; Coney 1978, 1981, 1989; Coney *et al.* 1980; Kerr 1980; Ben-Avraham *et al.* 1981; Jones *et al.* 1982, 1983*a*, *b*; Saleeby 1983; Howell 1989; Silbering & Jones 1984). This idea holds that major orogenic belts consist of 'collages' (Helwig 1974) of fault-bounded crustal and/or lithospheric fragments, termed terranes, of diverse origin and various sizes, the continuous movements of which, including dispersal and reunion through sundry tectonic processes, are believed to have imparted on the orogenic belt a mobility supposedly hitherto unrecognized and largely unsuspected. For this 'late' recognition, 'classical' (Coney 1981, p. 27) plate tectonic models have been blamed, which are considered to be 'either too facile or inappropriate when applied to complex geologic settings for two reasons: (1) they are two dimensional and ignore the complexities inherent in systems rapidly evolving in space and time; and (2) they assume that genetic relations must exist between adjoining but different domains (terranes). This assumption has led inexorably to confusion between fact and interpretation...' (Jones *et al.* 1983*a*, p. 103). These geologists were led to the conviction that the plate tectonic approach to orogenic belts had been dominated by models that were oversimplified, deterministic, if not entirely naive, and that a new methodology, involving objective criteria to recognize individual terranes and to 'prove' their spatial relationships before any plate tectonic interpretation could be attempted, was necessary (Schermer *et al.* 1984, p. 112). This 'new' methodology has been called terrane analysis (Jones *et al.* 1983*a*, *b*; Schermer *et al.* 1984; Howell 1985*a*, 1989; Howell *et al.* 1985) and has spread to other orogenic belts applied both by the originators of the concept (see, for example, Ben-Avraham *et al.* 1981; Nur & Ben-Avraham 1982*a*, *b*; Nur 1983; Schermer *et al.* 1984; Howell 1985*a*, 1989; Howell *et al.* 1985; Xiang & Coney 1985) and by others, for example, in the Appalachians (Williams & Hatcher 1982, 1983), the British Caledonides (Hutton & Dewey

1986; Bentley *et al.* 1988; Mason 1988; McKerrow & Gibbons 1988); the Scandinavian Caledonides (Stephens & Gee 1985); the European Hercynides (Weber 1986); the Tethysides of the Tibetan–Himalayan region (Chang *et al.* 1986); the Qin-Lang (Jia *et al.* 1988); Chinese orogenic belts in general (Guo *et al.* 1984); Yenshanian orogeny (Ye *et al.* 1984); the Alps (Tollmann 1987); Proterozoic orogeny (Grambling *et al.* 1988); and Circum-Pacific orogenic belts (Hashimoto & Uyeda 1983; Howell 1985*b*; Leitch & Scheiber 1987; Monger & Francheateau 1987). Because, in the past few years, interpretation of continental tectonics in terms of terranes has been such a dominant topic, reactions to which range from enthusiastic applause to abusive rejection, it is useful to review 'terranology' in the light of the following questions: (1) Is the terrane a new concept or jargon for old methods and ideas? (2) Is it a better word than fragment, block, sliver, etc.? (3) Has it been dangerous or obscuring in any way and/or has it had useful effects? Is terranology a useful methodology? (4) Does it have some objective methodology not inherent in other 'methods' of field work, regional geology, data analysis and synthesis? (5) Is it model-independent, and, if so, is this useful? (6) Is it a new way of looking at orogens that gives new insights and does it, as maintained by some (e.g. Howell 1985*c*), go beyond plate tectonics? (7) Does it advance our understanding of processes in tectonics? (8) Are terrane and terrane analysis temporary or permanent concepts? To attempt answers to these questions we summarize the evolution of thought (Sengör 1990*a*) on three dimensional mobility in orogenic belts, which allegedly culminated in the theory of terranes, to show that the stage of development represented by terranology had been reached and surpassed in at least one orogenic belt even before the advent of plate tectonics. We then look at our present understanding of orogenic mobility to evaluate the role of terranology.

EVOLUTION OF THOUGHT ON OROGENIC MOBILITY

That mountain belts consist of numerous fault-bounded blocks was the first general conclusion reached by geologists, especially those working in the Alps. Although this view accounted for local distribution of rock types and structures by postulating *ad hoc* fault-bounded blocks as need arose, it made delineation and predictions of a general orogenic architecture exceedingly difficult. The faults were ascribed to vertical uplift, to which the entire orogenic mobility was reduced.

Arnold Escher von der Linth's observations in the Swiss Alps in the first half of the 19th century showed that much of the great variability in Alpine geology was more a result of complicated folding than faulting. Escher's postulate of a regularly arranged set of folds being responsible for the main outlines of Alpine structure also was in agreement with the somewhat later results of other workers' studies in other mountain ranges (see, for example, Rogers & Rogers 1843) in the Appalachians, de la Beche (1849) in Southwest England and Wales). Mainly through the work of Swiss and American geologists, the view that mountain belts are compressed and folded portions of the Earth's crust became the conventional wisdom, to the point of calling all major mountain chains 'foldbelts'. Following Elie de Beaumont's analogy, the formative mechanism was likened to the operation of the jaws of a vice, introducing a second, horizontal, dimension into orogenic mobility. The ideal foldbelt was held to be symmetric with respect to an axis following its crest.

Suess (1875) was the first to argue convincingly that mountain belts had an asymmetric structure verging mainly towards their 'external' sides, towards which most orogens were convex. This idea of asymmetry in orogenic architecture was difficult to reconcile with postulated large, symmetric folds, of which the Glarus double-fold had become perhaps the most famous. In 1884 Marcel Bertrand reinterpreted the Glarus double-fold, in the light of Suess's views, as a single north-vergent nappe indicating a stratal shortening of some 40 km. With Bertrand's nappe hypothesis, the two-dimensional (uplift and subsidence with shortening across the trend of orogens) evolution of orogens became entrenched, to which Suess (1891) later added an extensional component on the basis of his study of the East African Rift Valley.

In the light of Bertrand's idea, many hitherto-unexplained structural and stratigraphic anomalies of Alpine tectonics became intelligible (Sengör 1990*b*). In particular, Schardt (1893) used the nappe idea to solve the riddle of the *Préalpes*, where rock assemblages had been mapped, that appeared completely exotic with respect to their surroundings. The then 'orthodox' hypotheses to explain their origin ranged from barely plausible sunken mountain ranges (see, for example, Studer 1843) to the entirely fantastic transport of whole mountain massifs by now-extinct, monstrous rivers (see, for example, Früh 1888). Schardt (1893) showed that, if these totally 'foreign terranes' containing coeval but utterly different rock types and structures with respect to the rocks amidst which they were now located, were viewed as remnants of a large nappe of southerly provenance, the riddle of their emplacement could be solved without resorting to improbable means. Schardt's views were adopted and amplified by Lugeon (1902), who showed that the entire northern margin of the Alps represents a stack of nappes of southerly provenance and speculated that the internal, metamorphic parts of the chain also consisted of north-vergent, gneiss-cored nappes. Lugeon argued that, by 'undoing' the deformation represented by the nappe pile, one could reconstruct the pre-orogenic palaeogeography of the Alps. To guide such a reconstruction, Lugeon (1902) devised the 'rule' that 'the higher a nappe is located in the pile and the farther it now extends northwards, the farther to the south it must root'. Application of Lugeon's rule necessitated the completion of the mapping of all the major nappes in at least one cross section across the Alps. Argand completed this task, which culminated in the discovery of the highest nappe in western Switzerland. To reproduce the pre-orogenic palaeogeography by undoing the Alpine deformation, Argand combined Lugeon's kinematic rule with a stratigraphic rule inspired by Haug (1907). Argand (1911, 1916) maintained that every major nappe had a uniquely stratigraphy reflecting a geological history different from its neighbouring nappes. The basic pattern of all the major Penninic nappes was characterized, according to Argand (1916), by neritic sedimentary rocks occupying the nose region of the nappe, 'comprehensive' geosynclinal sequences in the back and foredeep clastics with local breccias under the nappe. Applying Lugeon's rule to this general stratigraphic picture, Argand concluded that it reflected the order of original distribution of tectono-sedimentary environments in the 'Tethys', which was caused by the northerly march of giant recumbent folds, the 'nappe embryos', that became exaggerated later into the present Pennine nappes (Argand 1916, fig. 1). Argand's theory was an immediate success and remained the only working hypothesis well into the 1940s, despite some dissension from outside Switzerland (mainly by Haug 1925).

As a result of his 'embryotectonics', postulating a continuous shortening of the Alpine Tethys between the Carboniferous and the present, Argand quickly acknowledged that the prevailing 'fixist' theories of orogeny could not accommodate his scheme and he opted for continental drift (Argand 1916, 1924). The highly sinuous outline of the Alpine orogenic belt led him to the hypothesis of colliding irregular continental margins, a consequence of which was strike–slip movement subparallel with the overall trend of the orogenic belt (which Argand called 'flow along the shore' alluding to the margin of the 'Alpine geosyncline' (Argand 1924, fig. 14).

Argand's theory finally brings us to a three-dimensional theory of orogenesis, in which movements both across (in vertical and in horizontal planes) and in horizontal planes) and along the orogens were acknowledged. With the advent of the idea of nappes and its elaboration by Argand's application of his embryotectonics to continental drift, Alpine tectonics reverted back to its starting point, in being viewed as a pile of fault-bounded blocks. Instead of the dominantly steep to vertical faults of the late 18th and early 19th century, however, faults separating the nappes were thought to be dominantly roughly horizontal. After Argand, the standard procedure in alpine tectonic studies became the recognition of fault-bounded blocks with geological histories distinct from surrounding blocks. Contrary to conventional wisdom, all three kinds of faults were believed to surround the nappes, and displacements exceeding tens of kilometres on each type had been mapped on each type (Sengör 1990*b*). Argand (1924, figs 26 and 27) also recognized post-orogenic disruption because of ongoing continental drift and the complexities implicit in its kinematics, although most Alpine geologists did not follow his lead until the rise of plate tectonics mobilism.

The history of thought in Alpine tectonics up to this point makes it clear that the answer to the first question posed above, whether the 'terrane' is a new concept or new jargon for old methods and ideas, has to be the latter. The answer to whether it is a useful word *versus* nappe, strike–slip duplex, fragment, block, sliver, etc., is generally 'no'. If a fault-bounded package can be identified for what it is, why not call it that? Structural geology has developed a rich nomenclature for fault-bounded blocks, depending on the nature, attitude, and displacement of their bounding faults and it would be a retrogressive step to employ a lump term for them all. The word 'terrane' is an unfortunate choice to designate tectonic entities because it has long been used in the geological literature as an informal *stratigraphic* designation (also as *terrain*, but both the 1989 edition of the *Oxford English Dictionary* and *Webster's Third New International Dictionary of the English Language*, 1966, prefer the spelling terrane when the term is used in geology).

Before biostratigraphy, in the late 18th and early 19th century, terrane referred to the ensemble of all formations dominated by the same rock type implying coeval deposition in the Neptunist framework. Later, this definition changed following a change in providing views about Neptunism, especially of its temporal connotation. In 1828, D'Aubuisson de Voisins (pp. 268–269) wrote: 'The word *terrane* is often taken as a synonym of *formation*. However, its employment is more multifarious and less precise, especially regarding the epoch of production.…One could say that in geognosy formations are the species and, up to a point, terranes are the genera.' In 1841, Dufrénay & de Beaumont (1841, p. 35) stated that '…beds, related to one another by a regular succession which is remarkable and formed under the same conditions constitute what is called a terrane…The expression terrane therefore always implies an ensemble as opposed to *rock*, which by contrast, refers to the homogeneous material of each bed taken in isolation'. In the continental literature, the term terrane gradually came to signify a *stratigraphic system*, but the *First International Geological Congress* recommended that system be preferred. Usage in English always has been similar to D'Aubuisson de Voisins's 1828 definition quoted above. For example, Dana (1880, p. 81) wrote: '*terrane*…is used for any single rock or continuous series of rocks, of a region, whether the formation be stratified or not.

It is applied especially to metamorphic and igneous rocks, as a *basaltic terrane*, etc....'. Since the last century, the term terrane has now been used in geology in this imprecise sense. In the 1989 edition of the *Oxford English Dictionary*, it is defined as 'a name for a connected series, group, or system of rocks of formations, a stratigraphic subdivision'. *Webster's* (1966) gives a similar definition: 'a rock formation or a group of formations...' the area or surface over which a particular rock or group of rocks is prevalent'.

The AGI *Glossary of geology* (Gary *et al.* 1972) defines terrane as an obsolescent term applied to a rock or group of rocks and to the area in which it outcrops. The term is used in a general sense and does not necessarily imply a specific rock unit or group of rock units.' However, a perusal of the literature of the early 1970s shows that the term was not at all obsolescent! The future promulgators of tectonic terranes themselves used terrane in its old stratigraphic sense as late as 1972 (see, for example, Jones *et al.* 1972, caption to fig. 3).

Thus the English and French word *terrane* has been used both geographically and stratigraphically. Because these older usages of terrane in geology remain current, there is no need to add a vague tectonic meaning of the word terrane to the already vague geographic and stratigraphic meanings. It is our opinion that the words block (that conjures up the image of a roughly equant rather angular shape), sliver (an elongate object with pointed ends), or fragment (implying an object broken off a larger piece) are more informative than terrane, because the latter simply denotes a nondescript surface or expanse, and the former are not burdened by any previous geographic or stratigraphic connotation. We suggest that the genetic terms nappe, extensional allochthon, strike–slip sliver, fragment, and the non-genetic but geometrically informative terms sliver and block are preferable to terrane.

There is another category of genetic terms that may be used in place of terrane. To consider those, we need to go back to the history of thought on orogenic mobility and pick up the story from the time when Argand's views were refuted. The main breakthrough in Alpine tectonics came with the criticism and eventual refutation of Argand's views. In the 50s, it was shown that no one-to-one correspondence exists between the 'pre-orogenic' palaeogeographic entities, originally interpreted by Argand as nappe embryos (which he compared with present-day island arcs: Argand 1924) and the later nappes (Trümpy 1955, 1960). What created the former had been extension and not shortening as Argand has supposed; the nappes formed under a later, compressional régime. Trümpy (1960) summarized this revolution in Alpine thinking in terms of the recognition that the 'pre-orogenic' structures, and the strain régime that created them, had had no relationship with the 'orogenic' structures and the associated strain régime. To interpret the geological history of the Alps correctly, Trümpy argued, one had to distinguish a 'pre-orogenic' group of objects and an 'orogenic' one, whose relationships were complex.

After the advent of the theory of plate tectonics, it became clear with what these two groups of objects correspond; the former group consists of microcontinents, island arcs, ocean floors, and continental margins, whereas the latter was formed from the fault-bounded rock packages generated by the deformation of a collage consisting of the members of the former group. In a plate tectonic interpretation of the Alpine System, Dewey *et al.* (1973) recognized, and made extensive use of, this distinction. Their various microcontinents, island arcs, or small ocean basins, for example, do not correspond in any way with the individual nappes that Argand had established (Dewey *et al.* 1973, figs 1 and 12). In fact, fragments of Dewey *et al.*'s (1973) *primary orogenic collage components* are now found distributed in a number of *secondary orogenic collage* *components* that include the major Alpine nappes (cf. Sengör 1990*a*, *b*). This same distinction was made by Wilson (1968) in the western North American Dordillera, where he distinguished offshore island arcs and a western land that impinged on the original margin of Western North America and distinguished those from 'fragments' that formed by 'great dislocation between the Cordilleras of Canada and those of the United States' (Wilson 1968, p. 316).

With mobilism, especially after the advent of plate tectonics, fault-bounded entities making up an orogenic belt were seen to be of two fundamentally different kinds. One consists of non-subductable objects, such as island arcs, microcontinents, or large accretionary complexes that collide with and become accreted to a continental nucleus and/or to each other, forming the primary orogenic components. Before, during, and after such accretion the primary orogenic collage components become chopped into nappes, strike–slip duplexes, and extensional allochthons, individually or collectively, forming the secondary orogenic collage components. The faults that accomplish this chopping may have considerable displacements on them, although both the normal and thrust varieties have dynamic limits to their displacements; only strike–slip faults have no such dynamic limits. In studying the evolution of orogenic mosaics, our most important goal therefore becomes the identification of its primary collage components first, as precisely as we can. We then use them to study the effects of the secondary disruption, to which the primary components are related as strain markers.

Figure 1 shows schematically and in a very simplified sequence the formation of primary and secondary orogenic collage components. Terranology is not only not new, but it represents a return to a tried and abandoned methodology. Some may find this perfectly permissible in an anarchic philosophy of science, in which 'anything goes' (Feyerabend 1988), but it is our view that already-discarded methods of inquiry seldom bear fruit when rehabilitated. Terranology

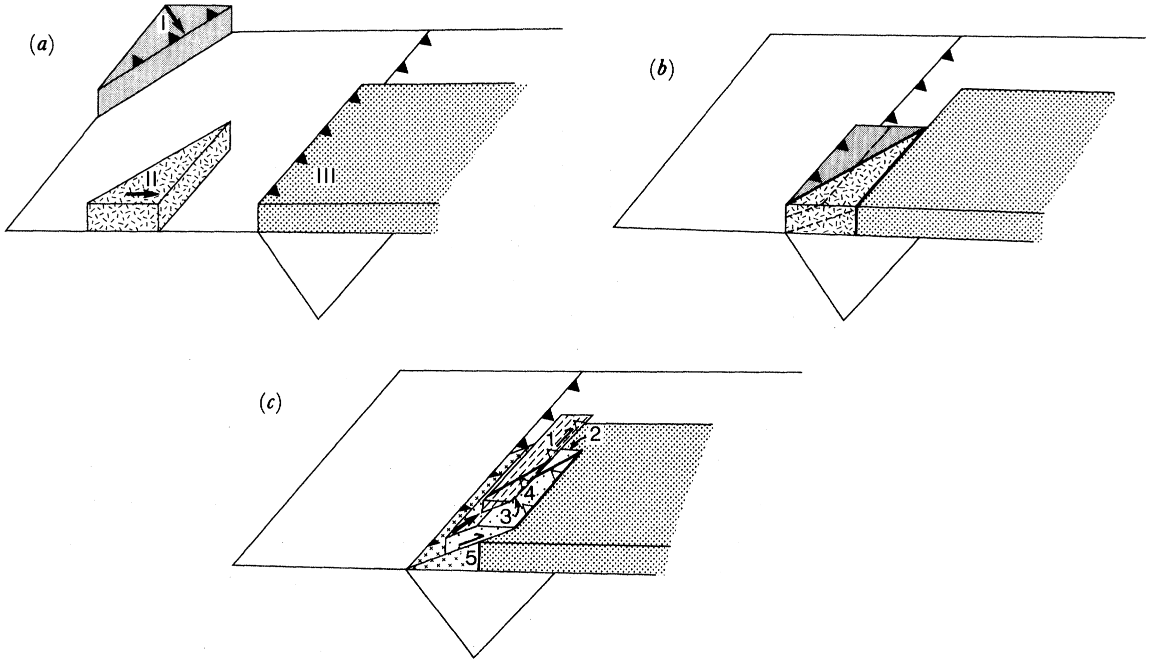

FIGURE 1. Sketch illustrating the relationship between primary (I, II, III) and secondary (1, 2, 3, 4, 5) collage components.

not only does not go beyond plate tectonics, it takes a backward step; by confusing primary and secondary collage components, it confuses also their genetic implications.

In the following section, we outline the plate tectonic causes of orogenic collage formation and argue that different sorts of collages form and evolve along different sorts of orogenic belts that are distinguished from one another by (1) the size of ocean lost along them, (2) the number of plates involved, and (3) the dominant orientation of convergence vector along the orogen during its history.

Complexities of orogenic evolution

(*a*) *Complexities generated by finite plate evolution*

A persistent terranological criticism of 'classical' plate tectonic methodology was that it had remained largely two dimensional and had been, therefore, unable to account for complexities inherent in systems rapidly evolving in space and time. Atwater (1970) was the first to emphasize the tremendous complexities that arise as a consequence of the finite evolution of three plates, which inevitably results in an extremely complicated history recording complex overprinting relationships among rapidly changing tectonic regimes. Complications also arise in a very simple 2-plate system that evolves without involving any change in relative motion rate or direction (Dewey 1976, figs 1 and 2). The stratigraphic correlation chart (Dewey 1976, fig. 2) reveals a history much more complicated than is intuitively expected from such a simple system. Localities 45 and 55, for example, become neighbours on the same plate although they have drastically different geological histories, and are now separated by a fossil fault. The same is true for localities 31 and 58, and 10 and 60. The faults separating localities 10, 31, and 45 from 55, 58 and 60 juxtapose points created far away from one another, thus satisfying 'the important criterion...that movement be sufficient to completely disrupt original facies relations and thus render uncertain original genetic relations' (Schermer *et al.* 1984), p. 118). These localities, especially 10, 31, and 45, must be separated from each other by faults also because of the peculiarities of fracture zone evolution (De Long *et al.* 1979). Along a single, steady, and stable plate boundary, relative plate motion of the simplest type thus can create 'fault-bounded geologic entities...with geological histories different from neighbouring localities'.

If three or more plates are in relative motion, at least one boundary must shift with respect to the instantaneous pole of rotation between neighbouring plates implying that at least one boundary must change character continuously (Dewey 1975). This change in character may in places lead to complex boundary configurations between adjacent plates, especially if non-subductable material is carried by at least one of them (Dewey 1975, fig. 14). In extreme cases, plates break up and form smaller plates (such as happened to the Caribbean plate (Pindell & Dewey 1982)). Pole shifts require that transform faults also change character or shift. If such shifts occur as jumps, exchanges of areas between plates may occur (Dewey 1975).

The discussion so far has concentrated on showing how complicated orogenic evolution must be if it results from finite plate evolution. But all these complexities are still at the plate level only and are not relevant to intraplate or inter-plate boundary zone phenomena. In the next section, we discuss briefly the consequences of superimposing on these plate-level complexities, some geological phenomena that dominantly result from the changing rheological properties of the lithosphere both horizontally and vertically in response to changes in rock types, pre-existing structural fabric, temperature, pressure, and availability of water.

(*b*) *Complexities generated by geological processes*

In 1975, Dewey observed that 'It is a truism that orogenic belts are exceedingly complex and poorly understood. If orogenic belts are the result of the relative motion of plate mosaics carrying continents and island arcs, this is not surprising. The writer has heard, on numerous occasions, the criticism that the application of plate tectonics in attempting to analyse orogenic belts grossly oversimplifies the geology of orogenic belts. In the writer's view this emphasizes the scale problem. It is one thing to say that a blueschist-melange marks the site of a former subduction zone but quite another to relate, in detail, the structural and metamorphic evolution of that zone to changing rates and directions of subduction. The broad geologic corollaries of plate tectonics are a matter of observation; the fine subtle corollaries that should now concern us are not understood. It is the main purpose of this paper to show that the evolution of plate mosaics carrying continents and island arcs must generate deformation sequences so complex that, with the difficulties of establishing relative plate motion solutions for the past, they may never be completely understood in plate tectonic terms' (Dewey 1975, p. 262). It is impossible to review, here, all the 'subtle' geologic corollaries of plate tectonics that may create a variety of 'terranes', but one can summarize them under four headings as follows.

(i) *During rifting.* Rifting is a complex process that takes place at all stages of a Wilson cycle. It not only creates volcanic and non-volcanic Atlantic-type continental margins (White *et al.* 1989), it also generates microcontinents of diverse shapes and aspect ratios, that may be generated in company of abundant or sparse magmatism, and have different subsidence histories. All these microcontinental pieces are eventually caught up in orogeny, forming various classes of terranes. Extant examples include the Lomonosov Ridge, Madagascar, the Seychelles, the Nazarene and the Saya de Malha blocks, the Danakil Horst, Sardinia–Corsica block, Lord Howe Rise, Norfolk Ridge, and Yamato Bank. In orogenic belts, such microcontinents are well known from the Tethysides, ranging in size from the immense Cimmerian Continent occupying a considerable length of the orogen to the tiny Brianconnais and Margna fragments in the Alps.

(ii) *During strike–slip faulting.* Strike–slip faulting is a process that also occurs at all scales and during all stages in a Wilson cycle. It commonly creates slivers during the establishment of a through-going strike–slip fault by linking up Riedel and P shears and then breaking new faults across the originally sinuous trace, or isolating strike–slip duplexes by reactivating older parallel and sub-parallel fault segments that lie along its course. Crack propagation may give rise to rhomb-shaped strike–slip duplexes while the main throughgoing fault is forming (Bhat 1983). Stick–slip behaviour and its inhomogeneous distribution along an established strike–slip fault zone may throw the fault off course, generate a sinuous trend, and the breaking of a new straight segment across the sinuous trace may result in new strike–slip duplexes.

Both positive and negative flower structures form along strike–slip faults depending on the regional and local orientation of the slip vector with respect to the fault trace. Positive flowers tend to generate flakes that are progressively detached from their roots and rotate, by up to 180°. In the course of their rotation, they may disintegrate and form a number of small flakes and klippen that come to rest on totally foreign substrates. By contrast, negative flowers tend to form blocks that rotate like ball-bearings or like circular disks rotating, where no block coupling exists, with an angular velocity of half of the vertical component of the vorticity of an underlying fluid (McKenzie & Jackson 1983). If the shape of the generated block in a negative flower is more like an ellipse than a circle, the rotation will be much slower than just half the vorticity and will depend on the aspect ratio of the block (Lamb 1987). In an evolving shear zone, Riedel–Anti-Riedel pairs may isolate blocks or slivers that rotate with the sense of shear.

If various rift segments are linked by long transform fault segments during continental separation, imperfections along the transform traces may result in the ripping of rectangular or triangular blocks from the separating corners of the continental margins (Scrutton 1976).

If strike–slip motion occurs parallel with a pre-existing continental margin, the fault that localizes the motion will be located inland of the ocean–continent interface. Its precise location is a function of the original stretching and the magmatic history of the margin and the thickness of the sedimentary prism that developed on the margin. Margin-parallel shear may generate thin slivers that may move past their parent continent for thousands of kilometres as parts of other plates (e.g. the Levant fragment west of the Dead Sea Fault). If the continental margin has an irregular shape, this may lead to strike–slip separation of slivers as in the case of the Baja California.

Strike–slip faulting occurring in arcs commonly is placed either along the active volcanic axis (Dewey 1980) or, in less common cases, along the forearc region as in the case of the Atacama fault or both (Katili 1970). Such faulting is most commonly a result of oblique convergence (Fitch 1972), whereby the convergence vector is partitioned into head-on subduction and pure strike–slip behind the frontal (Dewey 1980) or sliver (Jarrard 1986) plate. Coastwise transport (Beck 1988) may move such sliver plates for thousands of kilometres as independent entities. During their transport, they may disintegrate into a number of equant blocks that rotate in harmony with the sense of shear as in Chile (Jeniskey *et al.* 1987) and in Alaska (Scholl 1989). Alternatively, the strike–slip fault bounding them against the 'arc' may migrate perpendicular to its trace thereby either slicing the sliver plate into ever thinner slivers and stringing it out along the subduction front (E. Irving & P. J. Wynne, this symposium) or adding to it newer bits as it moves along, creating what may be called a 'horizontal stack'. Strike–slip faults that connect those along the magmatic axis (or just behind an accretionary forearc) with the trench may excise or repeat segments of forearcs along cross sections across the convergent zone (Karig 1980; Dewey & Shackleton 1984).

Strike–slip faults in collision zones are much more diverse and complicated individually than those located in other tectonic environments because they affect generally larger areas of the continental lithosphere that has a lower shear strength than oceanic lithosphere and may have been softened further by arc magmatism (Dewey 1980; Smith 1981). Strike–slip faulting generated by continental collosion may be viewed in two broad classes: (1) strike–slip along the suture and (2) strike–slip within the colliding masses that may also cut the suture.

Strike–slip along the suture may be caused by oblique collision (e.g. along the Himalaya) or by change of direction of relative motion upon collision (e.g. along the Zagros). Such strike–slip motion could form complex flakes, negative flowers, or horizontal stacks along the suture zone, considerably complicating its geometry. An extreme case is known from Iran, where the coastwise left-lateral movement of the Podataksasi arc for some 2000 km disrupted the original Palaeo-Tethyan suture zone.

Strike–slip faulting within colliding masses creates large intra-continental transform faults or their shallower intro-lithospheric equivalents, of which the Altin Dagh fault is a particularly fine example. Nearly all of its offset of hundreds of kilometres is taken up by shortening in the

Yorkand arc and in Nan Shan. Very little of it is dispersed into east China to form the Circum-Ordos graben systems. These faults serve to shorten the lithosphere parallel with the convergence direction and lengthen it sideways. If they join with subduction zones elsewhere they then become proper 'escape' avenues, of which the finest example is possibly still the North Anatolian fault in Turkey (Ketin 1948; Sengör 1979).

Strike–slip faults confined to shallower levels than the lithosphere (e.g. crust, upper crust, sedimentary cover, décollement sheets within the sedimentary cover) form abundantly in collision environments, but displacements along them decrease with decreasing penetration. Those that cut through the crust may have displacements on the order of 100 km; those that are confined to levels within the crust can have offsets up to about 100 km; those confined to the sedimentary cover usually have displacements not exceeding about 10 km. Penetration of strike–slip faults is a function of the disruption they cause.

Although there is little doubt that some limited escape of lithospheric and crustal masses occurs in collision belts, its extent seems much less than that maintained by Molnar & Tapponnier (1975) and Tapponnier *et al.* (1980) in southern and eastern Asia. Argand's (1924) old picture of dominant shortening and thickening with not much sideways motion in front of India seems largely vindicated by recent work (Dewey *et al.* 1989).

(iii) *During subduction.* Subduction is an extremely complicated process that creates a wide variety of structures mainly in the upper plate (Dewey 1980; Jarrard 1986). Whether an Andean-type compressive mountain range, or a Mariana-type extensional orogen with numerous marginal basins and remnant arcs, or indeed just a Makran-type 'neutral' arc will form above a given subduction zone seems to depend on whether the trench-line is fixed with respect to a stationary or slowly moving asthenospheric reference frame, and on the date of the roll-back of the hinge-line of the subduction.

In extensional arcs, diverse buoyant slivers may be created by pulling out bits from a pre-existing continental margin in the form of migratory ensialic arcs (e.g. Tyrrhenian–Calabrian arc system, Japan arc system) or by creating ensimatic arcs by successive episodes of marginal basin opening as in the case of the Mariana–Philippine Sea System. In neutral arcs, the most common way of creating buoyant slivers is by slicing the arc by strike–slip faults sub-parallel with its axis. In compressional arcs, the main method of forming fault-bounded geological entities is thrusting on a large scale. Much of this thrusting takes place behind the arc, at elevations lower than about 3 km (Dewey *et al.* 1988). If the subduction zone is steep enough to allow the presence of a mantle wedge beneath the arc (as under Altiplano); a well-developed magmatic arc forms and allows the concentration of shortening in a retroarc fold and thrust belt, whose basal detachment may root into the softened arc core (Dewey 1980). If no mantle wedge is present owing to shallow subduction angle, no magmatic arc develops and shortening becomes distributed in the 'cold edge' of the craton, creating Rocky Mountain-type basement-cored pop-up structures (e.g. the sierras Pampeanas (Jordan & Allmendinger 1986)). The displacements on the thrust faults created along the retroarc region of the compressional arcs are small compared with that along coast-parallel strike–slip faults active in the forearc regions. Both in the Canadian Cordillera and in the Andes, the total retroarc shortening has been estimated to be around 200 km.

In accretionary forearcs, large discontinuities separate distinct packages of mélange that differ from neighbouring packages in terms of matrix composition and age, block composition and age, and structural and metamorphic history. Such discontinuities may result from thrust and normal faulting (Suppe 1972) or strike–slip faulting (Karig 1980) of considerable magnitude (greater than 100 km). Many of the thrust faults in subduction–accretion complexes are mini-sutures that mark lines along which material was accreted to the toe of a growing wedge, although some faults are very difficult to distinguish from those that form by the internal failure of the wedge. Out-of-sequence thrusting and thrust faulting involving the post-accretion inner slope basin strata may be criteria to distinguish accretion faults from wedge failure faults.

Strike–slip faulting, apart from Woodcock's (1986) trench-linked faults, appear in the form of conjugate sets in accretionary complexes. Commonly, one set is dominant and displacement along its members is not significant. The main post-accretion disruption in such complexes appears to be associated either with intra-wedge thrusting or strike–slip faulting related to oblique subduction.

(iv) *During collision.* Collision shortens and thickens the continental crust mainly by thrust faulting and, to a more limited extent, by homogeneous bulk shortening and thickening. As crust thickens, conjugate strike–slip faulting takes an increasingly more significant part in the shortening process by sideways elongation of the shortened region and may accomplish much disruption of pre-existing structures. Escape occurs in front of colliding premontories and towards nearby oceanic pockets, although this process seems less common than hitherto believed.

Major strike–slip faulting in collision orogens may be divided into two classes; those that result from the closure of small oceans and/or where collision was preceded by convergence at high angles to former continental margins, show less disruption by strike–slip faulting than those collisional orogens that grew out of the destruction of large, Pacific-type oceans in which subduction had a variety of orientations with respect to the colliding continental margins before collision. The Alps and the Caledonides provide examples of two such cases respectively.

Orogenic complexity has two main sources: plate kinematics and rheology of the lithosphere. When these two factors are combined, the resultant spectrum of geological processes that generate structures in orogenic belts is theoretically so wide that some of the predicted complexities have not yet been mapped in the field (Dewey 1975). Geological structures in orogenic belts had been catalogued extensively before the rise of plate tectonics. With the appearance of the theory of plate tectonics, the first stage in orogenic research was to accommodate these old observations and the ideas they had given rise to in the new theory and to sieve out or modify those that looked obsolete such as tectogenes and geosynclines. The second present stage was to expand the theory with new observations made in its light. The full range of many of the geological processes that we described above were first recognized during this second stage of orogenic research in the light of plate tectonics.

Terranology manifestly does not advance our understanding of the processes in tectonics unless genetic questions about spatial relationships across discontinuities in orogenic belts are asked during all stages of an investigation. This terranology forbids: 'Where tectono-stratigraphic sequences are not unequivocally correlative, we take a conservative stance and treat them as separate terranes. This taxonomic splitting permits a variety of palinspastic interpretations and reconstruction *once* pertinent data are gathered' (Schermer *et al.* 1984, p. 112, our italics). Here, Schermer *et al.* (1984) allow interpretation only after the pertinent data are gathered but they do not specify how the data are to be gathered; in the light of what question, what theory? What does the 'pertinence' they mention refer to? Pertinent data is a meaningless concept if there is no problem and no theory, in the light of which they can be gathered. Schermer *et al.* (1984, p. 112) further state that 'The scepticism about genetic relationships that is inherent in terrane analysis allows more objective collection and interpretation of data, as facies relationships must be proven before they serve as the basis for a model'. This is unrealistic, because it implies that no model of orogenic evolution could ever be developed as it is not possible to prove all facies relationships in an orogenic belt, if for no other reason than some have already been eroded away.

The implied and advocated scepticism (agnosticism in disguise) allows terranologists to define terranes in the most general and vague terms. Coney *et al.* (1980, p. 330), for example, 'define' the Tracy Arm terrane as follows: 'Structurally complex assemblage of marble, pelitic gneisses and schist of unknown ages.' This description is consistent with almost any environment in an orogenic belt and consequently says very little of boundary relationships, indeed, it says nothing at all. Had Coney *et al.* (1980) given an interpretation of the Tracy Arm assemblage, their readers could have deduced certain predictions from it not contained in the above description and perhaps used them in testing Coney *et al.*'s model and/or used it in a more comprehensive evaluation.

Map view of orogenic evolution: importance of strike–slip faulting

In this section, we try to answer the question whether terranology has some objective methodology not inherent in other 'methods' of field work, regional geology, data analysis and synthesis by reviewing the methods that led to the widespread recognition of the immense horizontal mobility in orogenic complexes. As the recognition of the importance of strike–slip faulting played a dominant role in this process, we concentrate on the history of the growth of understanding of this particular facet of orogenic evolution.

Although strike–slip faulting at high angles to the trend of orogenic belts had been recognized in the middle of the last century, strike–slip faulting parallel or subparallel with orogenic trend was a 20th-century discovery prompted by the 1906 San Francisco earthquake. It was later incorporated into mobilist theories by Argand (1916, 1924) and Suess (1949) but was not emphasized until an upsurge of interest in strike–slip faulting and its role in orogeny in the 1950s.

Four different approaches led to a revival of interest in strike–slip motion parallel or subparallel with the trend of orogenic belts. The first was the recognition, in the Canadian Cordillera, of anomalous, equatorial ('Tethyan') fossil assemblages of late Palaeozoic age that contrasted with coeval but higher latitude taxa of the cratonic North America. This evidence was used by Wilson (1968) to suggest that parts of the Canadian Cordillera had originated away and farther to the south than where they are now. This implied a path of motion for these pieces, which had a large component parallel with the trend of the Cordillera. Wilson (1968) also drew on Roddick's (1967) evidence to argue for significant strike–slip motion along the Tintina–Rocky Mountain trench system. Later work on 'Tethyan' fossil assemblages (see, for example, Monger & Ross 1971), and especially on palaeomagnetism suggested, in the 80s, that a much larger amount of strike–slip faulting must have occurred during the development of the North American Cordillera.

The second approach has been seismological and involved study of motions along active convergent plate boundaries. McKenzie (1970, 1972) noticed that collision of irregular continental margins leads to disintegration of colliding headlands by sideways expulsion, into Eastern Mediterranean-type oceanic embayments, of lithospheric slivers bounded by conjugate strike–slip faults striking at low angles to the overall trend of the collision zone. Dewey & Burke (1974) pointed out the general applicability of this model to most orogenic belts. The sideways expulsion of material away from colliding promontories was called 'tectonic escape' by Burke & Sengör (1986).

Seismological studies along collision zones also documented widespread shallow (8–25 km) detachments (see, for example, Seeber & Armbruster 1979) that are deformed by strike–slip faults with strikes at low angles to the convergence front. Aric (1981), for example, documented a left-lateral wrench system along the Mur-Mürz-Leitha line in the Austroalpine nappes of eastern Austria forming the southern boundary fault of the thin-skinned Vienna pull-apart basin (Royden 1985). In this case, it is clear that this entire system of wrench fault is confined to the Austroalpine thrust sheets and do not penetrate any deeper.

The third approach has been palaeomagnetic, revealing either by direct demonstration of palaeolatitudinal change motion parallel with roughly meridional orogenic belts such as the Cordillera of North America (see, for example, Beck 1976, 1980; Monger & Irving 1980; Alvarez *et al.* 1980; Cox 1980) or suggesting the incidence of such motion by documenting its effects such as block rotations (see, for example, Beck 1976, 1980; Cox 1980).

The fourth and the most difficult method has been that of traditional field geology combined with some seismic reflection profiling and other geophysical methods such as magnetics. This approach has suggested and documented broad zones of shear along orogenic belts involving substantial rotations (Carey 1958); it also documented orogen subparallel shallow strike–slip faults both in zones of continental collision (see, for example, Trümpy 1977) and in subduction–accretion complexes above subduction zones, deeper, arc-parallel strike–slip faults (Allen 1965; Katili 1970), escape-related strike–slip faults (Ketin 1948; Sengör 1979).

Through a combination of all these approaches a vast amount of strike–slip faulting and associated disruption of orogens creating mosaics of deformation (fig. 10) had been discovered, mapped, and discussed before 1980. There is little doubt that Carey's (1958) prophetic vision was influential in stirring up mobilist interpretations in the Cordillera and reviving it in the Tethyan system. All such interpretations found a ready home in plate tectonics, and, although initially neglected (except by Wilson 1968), they were later incorporated into models of orogenic evolution (see, for example, Dewey *et al.* 1973; Hamilton 1979).

From the foregoing historical review, it seems that there is nothing novel about the methodology of terranology. The answer to the question, therefore, whether terranology has some objective methodology not inherent in other 'methods' of fieldwork, regional geology, data analysis and synthesis, must be no.

Yet, much fuss has been made by terranologists about the methodological superiority of their approach as opposed to former 'classical' plate tectonic models. The following quotations, expressing this view, have been selected at random from the writings of the original promulgators of the concept (all italics ours).

1. '...the procedure of identification of suspect terranes has proven *operationally* very fruitful. It has spawned a completely new type of tectono-stratigraphic map...the compilation of which is providing great insight into Cordilleran tectonic evolution' (Coney 1981, p. 23).

2. 'The application of terrane analysis to specific geologic settings, however, produces a *fundamentally different perception of geologic history than that gained through application of plate tectonic "models"*, particularly those dealing with relations along active continental margins...'.

'Tectonic synthesis is the ultimate step in understanding the origin and evolution of continents, and *terrane analysis is the most objective and successful means of achieving this synthesis*' (Jones *et al.* 1983*b*, p. 103).

3. 'The suspect terrane concept...*provides new insights*.... It is a surgically clean analytical approach and a *superior framework* in which to view the anatomy of any complex orogen' (Williams & Hatcher 1983, p. 33).

In the following and final section, we single out the distinctive aspects of terranology to provide an answer to our final question as to whether terranology has been dangerous or obscuring in any way.

CONCLUSIONS AND DISCUSSIONS: METHODOLOGY OF TERRANOLOGY

For traits of terrane methodology distinct from other, older approaches we must again turn to the statements of the originators of the idea. The central methodological claim of terranologists has been that terrane analysis is an objective, model-independent method of looking at orogenic belts. Schermer *et al.* (1984, p. 112) wrote, for example: 'Therefore, one must be cautious in applying plate tectonics models that assume that tectonic domains that are presently spatially juxtaposed are genetically related. Separation of fact from interpretation is essential here: *one does not define a terrane because it fits or does not fit a model but because of its distinctive stratigraphy and geologic history*.' The terranalogist's own practice belie this statement. In the following, we present a random selection of quotations taken from those sections of various terrane papers devoted to 'terrane characterization' or 'terrane stratigraphy' or 'depositional history' and expressly not from their 'interpretative' sections. The quotations are thus from those parts of these papers that allegedly do not depend on whether they 'fit or (do) not fit a model'.

'Where exposed, older parts of the (Wrangellia) terrane consist dominantly of sedimentary *and arc-related volcanic rocks* that nowhere may be older than Pennsylvanian and, from indirect evidence, may have been, in part, *deposited on oceanic crust*' (Jones *et al.* 1977, p. 2565).

'Upper Triassic and Lower Jurassic marine sandstone and argillite...structurally overlies on the northwest a *dismembered ophiolite* (*oceanic crust*...' (Jones *et al.* 1977, p. 2571).)

'...have already called attention to the significance of the Chulitna district as possibly representing a *suture zone* along which unlike terranes have been juxtaposed' (Jones *et al.* 1977, p. 2571).

'G, Goodness (composite) – includes three terranes: ... (3) Mesozoic *arc-derived volcanic flows*, tuffs, and greywacke with interbedded chert' (Coney *et al.* 1980, p. 330).

'V, Vizcaino (composite) – includes... Upper Jurassic *arc-derived* volcanic and volcaniclastic rocks...' (Coney *et al.* 1980, p. 331).

'The geologic record of the Chulitna terrane commences with *formation of ocean crust* (*ophiolite*) in late Devonian time.... Conditions changed markedly after Mississippian time...presumably *this volcanism reflects rifting*...' (Jones *et al.* 1980, pp. A9–A10).

'The characteristic stratigraphic elements of this terrane are (1) an upper Paleozoic *magmatic arc assemblage*...' (Jones *et al.* 1982, p. 3713).

'YID Yidun area: (Baiyu–Yidun terrane of Xiong, Zhongza terrane of Zhang *et al.* (1983); may be a *microcontinent caught in subduction complex* of MEK...

'YMU Yushu-Muli: Tr *island arc volcs*, Upper Tr melange; ophiolite and blueschist belt is eastern boundary.

'ZFB Zunggarian foldbelt:...some upper Pz ophiolitic and *island arc rocks...*; *ophiolite possible obducted in late Permiean during collision of island arc...*' (Howell *et al.* 1985, p. 25).

In the above quotations we have italicized those parts of the 'descriptions' that are dependent on plate tectonic interpretations, i.e. 'models'. It is thus clear that, as logically there can be no observation without an *a priori* framework, i.e. a model (see Popper 1968; Feyerabend 1988, p. 63), terrane identification and description are no exceptions.

During this meeting, one of the most distinguished exponents of terrane analysis, David L. Jones, told us in discussion that our criticism on this point was unfair. He pointed out that model-based concepts are of course used to *characterize* a unique sequence of *events*, whose record make up a terrane. He indicated that terranology was really directed against certain specific, individual plate tectonic models that had assumed spatial relations between terranes. This assumption of unwarranted conclusions was what they had thought harmful. If this is so, then terrane analysis may be nothing more than plate tectonic modelling, and neither more nor less model-dependent or objective than any other kind of tectonic modelling. We do not believe, however, the situation to be as advocated by Jones. By its own admission, terranology rose against certain individual plate tectonic models. We can further specify this by stating that it was devised as a refutation of the hypothesis that much of the western North American Cordillera consisted of *in situ* tectonic units, i.e. the present spatial relations of its constituent tectonic units reflect original relations. This may be formulated in a different way as follows: In the western North American Cordillera, there are no tectonic units that were originally so far away from their present locations that they have no genetic links with their present surroundings. This is a universal statement on the scale of the North American Cordillera and, like all natural laws, it prohibits the occurrence of certain events or the presence of certain objects (Popper 1968, pp. 68–69). As such, it has proved to be a very fruitful hypothesis. Terranology rose against this by citing individual examples where this hypothesis failed; it began in Alaska (Berg *et al.* 1972; Jones *et al.* 1972; Jones *et al.* 1972; also cf. Howell 1985*a*) and then spread southwards via Canada (see, for example, Jones *et al.* 1977) to the U.S. and Mexico to engulf the entire Cordillera (see, for example, Coney *et al.* 1980). The individual examples cited all negated the universal statement that 'there are no tectonic units that were originally so far away from their present locations that they have no genetic links with their present surroundings' by showing that there *are* such units. Thus, by nature, these statements all have the form of what Popper (1968, p. 68) calls *strictly existential statements*. The negation of a strictly universal statement (such as a scientific hypothesis) is always equivalent to a strictly existentialist statement and vice versa (Popper 1968, p. 68). Scientific hypotheses are refuted by purely existential statements (also called 'there-is' statements because they refute laws of the form 'there-is-no...'). Terranology thus consists of a large number of purely existential statements and these statements are taken to be 'objective' because they are based on observation (and therefore subjective as every singular observation, unless reproducible, remains subjective: see especially Popper (1968), p. 45ff). We have, however, seen above the hypothetical character of all observations. But even if we do not consider the hypothetical character of observations, there appears another problem associated with the purely existential statements: They are not refutable because no singular statement can possibly contradict an existential statement. The very idea of 'suspect' terranes, which may be formulated as 'there are (may be) terranes that originated far away from North America, so that they have no genetic ties with their present surroundings' cannot possibly be contradicted by any number of observations because however many 'native' terranes one may find, there may still be those that are exotic. This is so because our observations cannot possibly be exhaustive. Even if we managed to check all of the Cordillera and found no terrane that had come from afar, the possibility of the existence of a now-eroded otherwise-removed one or cryptic terrane can never be excluded, as in many, deeply eroded Precambrian orogenic belts.

Our main methodological criticism of terranology is this property of irrefutability, because of the uncertainty or high probability (in the sense of admitting too much: terranes may be native or exotic, or even cryptic but the theory – if there is such a thing – is non-commutable and therefore cannot be tested) of its formulation. It negates a universal statement, but does not replace it with another one. It stays at the stage of an existentialist statement that is not informative. That the terranes are 'suspect' is not an informative statement, as anyone could suspect anything. In Coney's (1978, p. 38) original definition of suspect terranes, he specifically pointed out that it had been devised so as to avoid questions: 'They are best described as simply 'suspect' terranes. In this way, the question of how far they have travelled is avoided: But science does not avoid questions, it goes after them, because it wants to make the mistakes as far as possible! Scientists do not stay suspicious, but solve problems.

'Terranologists' fear of universal statements (i.e. bold conjectures, hypotheses: 'the terrane concept...is based on an uncertainty principle' (Jones *et al.* 1983*b*, p. 103)) and their tendency towards a much-praised 'in-built scepticism toward plate tectonic modelling between groups of terranes' that characterizes terrane analysis and the demand that 'genetic links must be physically demonstrated between adjacent terranes before palaeogeographic and tectonic models can be constructed using them as building blocks' seems to result from a mistaken view of objectivity. In the natural sciences, objectivity historically has meant, independence of opinions or personal whim and justifiability by anyone who is interested by direct observation. If a statement is to be called 'objective', it has been thought, it has to be understood and *verified* by anyone. The word 'verification' here provides the key to the following discussion.

As Popper (1968, p. 70 footnote 2) points out, if it is characteristic of empirical science to look upon singular statements (such as accounts of observations or experiments) as test-statements, then, with respect to these, universal statements are falsifiable only and existential statements verifiable only. Because objectivity has been thought to imply verifiability by anyone who is interested, good, i.e. objective science, it has been thought, should consist of verifiable, i.e. existential statements. But existential statements, although verifiable on endless individual cases, cannot be tested for universal, i.e. theoretical conclusions, because induction does not work. Thus objectivity of theories of statements pertaining to how the Earth works depends on their testability, for they can neither be verified nor justified by any means (cf. Popper 1968, p. 44ff). By contrast, observations that are not communicated, must remain subjective and untestable. It is this testability that makes theories independent of anyone's whim, because they can be tested, and if found to be false, discarded. This condition of testability is in fact included in the essence of the word 'objective' that comes from the Latin *objicere* meaning 'to oppose, to stand up to' (Lorenz 1987, p. 17).

The logical conclusion that existential statements cannot be falsified (i.e. cannot be tested for universal applications) causes terranology to consist of singular existential statements, not testable or much less testable than the plate tectonic models against which it rose. Because terranology is less testable, it becomes consequently less objective. It has been pointed out, however, that the purpose of terrane analysis is not the erection of new universal hypotheses but to show, in individual cases, that the ones so far erected do not work and that a study of orogenic belts must be approached by piecemeal field studies without having to deal with universal hypotheses at every step: 'Conveniently, it (i.e. terrane analysis) alleviates the immediate necessity of incorporating every facet of geologic development in a single all-encompassing model' (Williams & Hatcher 1983, p. 34). But this statement makes 'every facet of geologic development' much less testable, because it has to be looked at from much fewer viewpoints. We believe that this, as a principle, is not a merit but a vice (for it is equivalent to avoiding questions). Of course, there may be instances where some 'facet of geological development' may not be immediately incorporated into a 'single all-encompassing model', but such incorporation is the ultimate aim of our research. We pursue geology to find out how the Earth works and if we lose this ultimate aim from sight in principle, geologizing would deteriorate into an enumeration of singular, unconnected existentialist statements, from which no knowledge, in terms of understanding, can possibly emerge.

It is important to point out that all of what we said above about our methodological reservations is not new. Most philosophers of science after Popper have pointed out all this and they (including Popper) have had some important geologists as predecessors (e.g. Hutton 1794; Gilbert 1886, 1896; Daly 1914). Most terranologists do not follow their own advertised methodology either, most are too good and too experienced as scientists to do so! Like most scientists (except a large number of post-Einstein physicists, however, they too seem to have succumbed to the baconian myth that all 'good' science emerges from observations made by unpolluted minds, and they perhaps fancy that is what one should do to break loose of the fabrications of our minds and get to the 'facts of nature'. We think it is high time that we stop preaching this myth in conference halls and class-rooms.

This myth, and the advocated terrane methodology that clearly grew out of it, has indeed caused some harm. It has made more enumeration of taxonomic entities, i.e. terranes, respectable. Enumeration of geographically named real estate hardly improves our understanding of mountain belts, as was shown by a similar failure of the nappe theory in the Alps. Since Argand, few Alpine geologists had thought in genetic terms. They had, indeed, mapped all their major rock units, and the faults bounding them, but their relations had been inadequately questioned. This danger of using a mindless taxonomy to generate uninformative tectonic maps (of the kind of Coney *et al.*'s (1980) terrane map) is especially acute in places where there is no tradition of scientific critical thinking. In this proliferation of geographically named, but otherwise totally opaque tectonic units, we see the main danger of the application of terrane analysis.

The word terrane is a lump term for a number of older and more informative non-genetic (block and sliver) and genetic (fragment, nappe, strike–slip duplex, microcontinent, island arc, suture, etc.) terms. Because it is less informative, it is less useful than any of these and also because, historically, the term 'terrane' has had a number of different meanings, it is best avoided. Terrane analysis is neither a new way of looking at orogenic belts, nor a particularly helpful one. Its appeal largely stems from the fact that it takes the responsibility of interpretation of Earth history off the shoulders of the geologist. This, contrary to the common belief of the terranologists, reduces objectivity and makes statements about the tectonics of an area essentially untestable. It has done nothing to advance our understanding of processes in tectonics. We hope that its employment is only temporary and that research on orogenic belts will proceed in the light of daring hypothetical statements that are tested by field studies. This, we believe, was the spirit that led to the rise of plate tectonics.

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Discussion

A. H. F. ROBERTSON (*Grant Institute of Geology, University of Edinburgh, U.K.*). Surely the terrane concept has in fact proved to be very useful, particularly in that it has led to more emphasis on the nature of contacts and the possibility of major strike–slip displacements in many orogenic belts?

J. F. DEWEY, F.R.S. Yes, these are indeed the only two ways in which the terrane 'concept' has been useful.