Plate tectonics, terranes and continental geology

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Abstract: The 'modern revolution' in the Earth sciences is associated with the emergence of plate tectonics in the late 1960s. The assumption that the crust of the Earth was composed of a small number of rigid, non-deformable, mobile plates enabled a quantitative, kinematic description of current geological processes and reconstructions of past plate interactions. The simple model of plate theory *c.* 1970, for example its depiction of a subduction zone, has since undergone considerable refinement. However, some geologists, especially those concerned with questions of continental tectonics, contend that plate theory in its current form is of limited value in addressing questions of continental tectonics, and prefer to employ the concept of allochthonous terranes in characterizing, describing and interpreting regional geology. These geologists may understandably take the view that plate tectonics is a kinematic grand generalization but thus far not particularly useful in making sense of the rocks at the local level.

The 'modern revolution' in the Earth sciences is associated with the emergence of plate tectonics in the late 1960s.[1] This had two major phases. In the first, which could be called the sea-floor spreading phase, the concept of sea-floor spreading provided not only a plausible mechanism for the horizontal displacement of continents but also explanations for such recently discovered phenomena as magnetic striping of the sea floor, relatively high heat flow over the oceanic ridges, the distribution of deep- and shallow-focus earthquakes, and the age profile of different parts of the sea floor. This 'dynamic' and empirically based phase was followed by a phase marked by the emergence of more idealized, kinematic models of plate interactions in which blocks of crust were treated as idealized crustal units rotating around Euler poles constrained by correlations between oceanic and continental rock ages based on the rapidly developing magnetic reversal timescale (see Glen 1982).[2] Plate theory marked the culmination of a half-century of debates over crustal mobilism and, in a grand synthesis, drew together developments in many branches of the Earth sciences. The rapid and widespread acceptance of plate theory, J. Tuzo Wilson forcefully argued (1976, p. vii), 'has transformed the earth sciences from a group of rather unimaginative studies based upon pedestrian interpretations of natural phenomena into a unified science that holds the promise of great intellectual and practical advances'. Over the past 30 years, it could be argued with some justice that a major feature of this revolution has been a gradual shift toward a more physical and quantitative geology. However, some geologists judge plate theory in its current form to be of limited value in addressing questions of continental structures and tectonics. One response to the perceived shortcomings of plate tectonics, especially with respect to problems of regional geology, is the employment of the concept of what have been variously denominated accreted, exotic, suspect, allochthonous or tectonostratigraphic terranes.

Well into the early 1960s, there was little reason for geologists not to assume that explanatory frameworks based on the study of the continents over two centuries could be readily applied to processes and structures beneath the seas. For most geologists in the English-speaking world, the crust of their Earth was relatively stable. North and South America, for example, were each thought to be composed of an ancient core to which mountain belts had accumulated through the geosynclinal cycle. Mountain chains might be elevated or eroded, the continents might grow slowly on their margins but, broadly speaking, the continents and ocean basins had been essentially permanent features of the surface of the globe since their formation. Few took seriously the notion of continental drift. Extrapolating from the relatively well-known continents to the little-known ocean basins, it seemed obvious that granitic continents could

[1] This story has been told in varying ways by Marvin (1973), Hallam (1973), Menard (1986), Le Grand (1988), Stewart (1990) and Oreskes (1999).
[2] This point has been emphasized to me by H. J. Harrington (pers. comm.).

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not possibly move through the unyielding basaltic oceans. Plate tectonics strikingly reversed this situation. By the early 1970s, the revolution was essentially over. A vast amount of new and unexpected data had been harvested from the ocean floors. Their history, structure, volcanicity, magnetization, heat flow and other characteristics were different from anything known about the continents. Along with this new data had come both new theories of, and new evidence for, great lateral motion of the continents. Plate tectonics, the triumphant version of continental drift, was developed largely by geophysicists and geologists to make sense of this deluge of novel geophysical and geological data from the sea floor, much of which had no continental counterparts.

By the mid-1970s plate tectonics formed for most Earth scientists the theoretical framework for understanding and describing the workings of the Earth's outer shell or, as one influential textbook (Wyllie 1976) was titled, *The Way the Earth Works*.[3] From one perspective, it constituted only the culmination of a series of theories of global mobilism originating with Alfred Wegener in the early years of the twentieth century. Wegener, Alexander du Toit and others over a period of half a century had gathered and marshalled palaeontological, stratigraphic and geophysical evidence, taken mostly from the continents, to support the view that the continents had once been joined together but had been broken apart and moved to their current locations. Ironically, for many early plate theorists the continents were merely uninteresting excrescences on a fascinating sea floor. Could scientists apply this new framework to the continents to solve or resolve problems that had bedevilled generations of land-based researchers?

For plate theorists, their rigid, non-deformable plates and plate boundaries were almost geometrical entities which one could use to calculate the kinematics of plate motions and interactions over time. Dan McKenzie's Earth, for example, was a geometrical construct on which transform faults were arcs of circles defined by the poles of rotation of idealized plates; ridges and trenches were merely 'lines along which crust is produced and destroyed' (McKenzie & Parker 1967, p. 1276). Jason Morgan (1968, p. 1959) offered his version as 'a geometrical framework with which to describe present-day continental drift'. The third member of the early plate triumvirate, Xavier Le Pichon, put forward 'a geometrical model of the surface of the earth' (1968, p. 3661) which, though necessarily involving 'great simplifications and generalizations' (p. 3679) enabled him to give 'a mathematical solution which can be considered a first-approximation solution to the actual problem of earth's surface displacements' (p. 3674).

Field geologists concerned with the problems of continental geology and its history could not easily begin to use ocean-derived plate theory to solve them. They were in possession of an enormous store of knowledge and detailed maps of the geology of the continents, but knowledge of ocean-floor geology consisted of a rapidly growing fund of widely scattered data. Only very coarse models were available to indicate how oceanic crust might be connected to and interact with continental crust.[4] Once one moved away from the geometrician's globe, the local expressions of past and present plate movements were conjectural, diverse and different from place to place. How might one infer from this large-scale, general and coarsely grained idealized model solutions for the finer grained, specific problems of local geology that, though well known and well mapped, had seemed heretofore intractable? If this could be done, then the resistance of land-locked Earth scientists to the new sea-born theory could be overcome. From a cognitive perspective, the challenge lay in the necessarily uncertain and speculative extrapolation from processes thought to occur in the relatively youthful sea floor to explain the much more ancient structures of the continents. But, I suggest, that challenge lay too in the scientific and social interests of most land-based geologists. They rapidly gave assent – or at least lip service – to plate theory at a general level but controversies abounded over its applications to regional and local geological problems.[5]

There was considerable initial resistance to the very idea of global mobilism, especially from more senior Earth scientists who had invested their careers in a fixist approach to continental

[3] Cf. Glen (1975).

[4] As early as the 1930s, the National Research Council included, among the several major geophysical research problems for the community to address, that of the nature of the 'join' between the oceanic and continental crusts, particularly at what we now know to be a passive margin, e.g. the Atlantic Ocean floor joining the North American and South American continents. In spite of enormous advances in several branches of geophysics that could be brought to bear on this problem, a detailed cross-section of that join is still extremely tenuous more than a half-century later.

problems. The social interests of those who had achieved positions of authority through their work on continental features might well lead them to oppose extrapolations from the sea floor to the continents. These land-based Earth scientists had invested years in meticulous local mapping, fieldwork and analysis, and ever more refined synthesizing and theorizing. They had thereby achieved positions of authority in their chosen period or region or technique or structure.[6] 'Teddy' Bullard incisively commented (1975, p. 5): 'Such a group has a considerable investment in orthodoxy. . . . To think the whole subject through again when one is no longer young is not easy and involves admitting a partially misspent youth'. Mason Hill, for example, the architect of the previously accepted view of the San Andreas system, 'used to shake with rage when somebody would get up and talk about the San Andreas transform'.[7]

'The new global tectonics' was the agenda for the Penrose Conference, organized by Bill Dickinson, held on 15–20 December 1969 at the Asilomar Conference Grounds in Pacific Grove, California. It marked a major turning point in attempts to apply plate tectonics to the continents.[8] Among the participants were John Dewey, Jack Bird, Seiya Uyeda, Clark Burchfiel, Clark Blake, Greg Davis, Tanya Atwater, Peter Coney and Warren Hamilton. Dewey and Hamilton were already formulating approaches to continental geology based upon plate tectonics and their first papers bracketed the conference. Atwater gave a presentation that inspired many of the participants to try their own hand at plate tectonics-based interpretations. Several of those present were also to take part in the later development of, and debates about, the terrane approach to regional geology.

Dewey, soon after the adumbration of plate tectonics, and just before Asilomar, had begun to apply the new tectonics to construct an overview of orogeny on convergent Atlantic-type plate margins. Pursuing a suggestion of Tuzo Wilson (1966), he proposed that the Appalachians and other mountains bordering the Atlantic had been pushed up through collisions resulting from the openings and closings of the Atlantic, e.g. a second convergence of the Atlantic and African plates had thrust up mountains in Virginia and Pennsylvania (Dewey 1969*b*). Subsequent to the conference, Dewey and Bird extended this view to other convergent plate margins including the North American Cordillera, the Andes and the Himalayas in a broad-brush paper that was to be quite influential. They believed, contrary to some at the time, that 'plate tectonics is too powerful and viable a mechanism in explaining modern mountain belts to be disregarded in favour of *ad hoc*, actualistic models for ancient mountain belts', and that understanding of all mountain belts could come only from the new global tectonics (Dewey & Bird 1970, p. 2626). Their presentation included many sketches of cross-sections of crust presenting in simplified form their ideas.

Warren Hamilton was one of the very few North American geologists in the early 1960s to advocate large-scale crustal mobility as a solution to regional geological problems. In 1961, for example, he proposed as a 'speculation' and a 'radical explanation' for geological correlations that Baja California had once been part of that mainland but had been both shifted 100 miles to the west and transported northward some 250 miles along the San Andreas Fault to its present location (Hamilton 1961, p. 1307). By late 1967 Hamilton was 'aware of this great surge in plate tectonics, but didn't comprehend it . . .'.[9] In the fall of 1968, he visited the Scripps Oceanographic Institution where he encountered a group of graduate students including Tanya Atwater and Jean Francheteau who were 'totally up to speed on plate geometry'. In his words he was 'led by the hand' by them through plate tectonics. The new global tectonics 'meshed beautifully with my . . . background in

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[5] As is common with novel, over-arching, conceptual frameworks, plate theory was assimilated at different rates and to different depths in different specialties and in different regions. At a functional level it could be said that different groups of geoscientists were operating with different versions of plate theory depending on their backgrounds and the problems that they were trying to solve (Glen, unpublished data; Le Grand 1988, pp. 75, 80–99, 163–164).

[6] For the roles of technical and social interests in scientific controversies see, *inter alia*, Bourdieu (1975), Latour (1987), Le Grand (1988, pp. 80–99), McAllister (1992).

[7] Interview with D. L. Jones taped by H. E. Le Grand on 18 January 1990, Berkeley.

[8] One rule for the Penrose Conference is that proceedings are not published as such nor are formal minutes kept; the emphasis is upon frank and free-ranging discussion initiated by a few speculative, provocative presentations. Dickinson (1970*a*) did, however, publish a report and overview of the Conference and has kindly supplied considerable additional information.

[9] Interview with W. B. Hamilton taped by H. E. Le Grand on 22 January 1990, USGS, Denver.

descriptive global geology. All of sudden here was a framework for it'. He set out to write a synthetic paper on the geology of California. 'Mesozoic California and the underflow of Pacific mantle' (Hamilton 1969) appeared the same month as the Asilomar Conference. In it he proposed that much of California was made up of island arcs, oceanic crust, abyssal hills and other sea-floor materials that had been scraped off more than 2000 kilometres of Pacific floor that had been subducted beneath the North American plate. Dewey recalls Hamilton saying at Asilomar, 'My God, we must be able to explain things like the Franciscan and the Coast Ranges, all those things, in terms of plate tectonics'.[10] Hamilton's interpretation was certainly a radical one at the time. As he later remarked, 'it was totally contrary to the way practically every Californian geologist looked at it . . . and there was quite a bit of resentment among the natives'.[11]

The most notable event at Asilomar was the presentation by Tanya Atwater. She proposed an elegant solution of the San Andreas Fault in terms of plate kinematics guided by sea-floor magnetics as an age control. She drew together both continental geology and oceanography in a quantitative way and provided refinements in the geometrical kinematics of plate motions. She thought that plate movement could be related to many of the features of continental geology (Atwater 1970, p. 3513) and provided models which were designed to provide 'testable predictions for the distribution of igneous rocks' and also the timing and amount of deformation. Although she treated only schematically configured, not geologically specific, crustal units, her approach made an immediate and profound impression on many of the participants.[12] Davy Jones, who was to be an architect of the terrane programme, describes his reaction when he learned of her work as follows: 'That was the first application of plate tectonics to a real setting and she was able to show people who had been fussing with the San Andreas Fault all of their lives that they were completely missing the story. It was a marvellous paper and that's what convinced us that plate tectonics was the way to go'.[13]

For a few years after Asilomar, there seems to have been an almost euphoric belief, or at least an incautious optimism, that problems of continental geology would quickly yield to the new global tectonics. The initial successes of Atwater, Dewey, Bird, Hamilton, Dickinson (1970*b*) and others seemed to show the way forward. In the first few years of the 1970s, there was a revolutionary fervour: many geologists rushed into print with redescriptions of their patches of ground with reference to so-called plate tectonics corollaries; those who forbore such descriptions were regarded as troglodytes. More than one Earth scientist refers to that era as being cluttered with premature, simplistic, cursory or naive interpretations. Plate tectonics was not to be confused with continental tectonics. The marine magnetic record that had proved so critical for Atwater represented only a small fraction of the geological timescale. Dewey and Bird's sketches were only that, and drawn to a very large scale. Hamilton's syntheses, though highly suggestive, were grand generalizations. Indeed, Hamilton (1995, p. 3) himself recently commented that the complex nature of plate interactions and their boundaries 'invalidates many of the tectonic and magmatic models which clutter the literature' and even now 'few of the geologists and petrologists who work with the structures . . . produced by convergent-plate interactions, and few of the geophysicists who model subduction, have familiarized themselves with the characteristics of actual plate systems'. How helpful would this global theory be in explaining this outcrop or that group of hills? For a field geologist to apply plate tectonics to his 'patch' is not unlike trying to explain the flight of a cricket ball using general relativity theory. One has to make certain simplifying assumptions. For example, may one properly treat the plates as absolutely rigid, knowing full well that the continents, which presumably record previous plate movements, also record considerable deformation?

[10] Interview with J. F. Dewey taped by H. E. Le Grand on 21 December 1988 at Department of Geology, Oxford University.

[11] Interview with W. B. Hamilton taped by H. E. Le Grand on 22 January 1990, USGS, Denver.

[12] Interview with B. C. Burchfiel taped by H. E. Le Grand on 26 April 1990 at Earth and Planetary Sciences, Massachusetts Institute of Technology, Cambridge; interview with P. J. Coney taped by H. E. Le Grand on 15 February 1990 at Department of Geosciences, University of Arizona, Tucson; interview with G. A. Davis conducted at the University of Southern California, Department of Geological Sciences by telephone by H. E. Le Grand on 14 May 1990; interview with J. F. Dewey taped by H. E. Le Grand on 21 December 1988 at Department of Geology, Oxford University; interview with W. B. Hamilton taped by H. E. Le Grand on 22 January 1990, USGS, Denver.

[13] Interview with D. L. Jones taped by H. E. Le Grand on 15 May 1990, Berkeley.

Dan McKenzie, one of the pioneer plate theorists, himself sought to 'modify plate tectonics to describe continental, as well as oceanic, tectonics'. But, as he has recently remarked (McKenzie, pers. comm., 2000), '[T]his problem'is much harder than plate tectonics.... Plate tectonics was clearly defined as a kinematic theory: one that is concerned with geometry. It is not a dynamic theory.... I myself do not describe continental tectonics as plate tectonics, because continental deformation occurs in wide zones where the idea of rigidity is of limited use'. Peter Molnar, a noted tectonician, takes a similar view. For him, the major importance of plate tectonics for most geologists in the 1970s was that it convinced them that continental drift had occurred. However, though it was a useful framework on a global scale, in terms of unravelling problems of continental tectonics, 'plate tectonics is a poor approximation for the tectonics of many continental regions' (Molnar 1988, pp. 131–133). Plate tectonics may account well for the behaviour of oceanic lithosphere but continental lithosphere differs in buoyancy, thickness and rheology. In particular Molnar holds (p. 133) that 'The broad, diffuse deformation of the western United States ... is much more complex than the rigid-body displacements of a small number of large plates, and finding a simple and accurate way to represent the deformation of continents remains a major task'. Molnar bluntly concludes (p. 137), 'The tectonics of continents has found plate tectonics an inadequate paradigm'. He muses more recently (P. Molnar, pers. comm., 1999) that it is 'no wonder field geologists had not discovered plate tectonics, for diffuse deformation and widespread strain makes recognising rigid plates within continents difficult'.

Dewey's work in the mid-1970s similarly brought home to him the complexities of applying plate tectonics to smaller-scale geological problems at plate boundaries:

> What I found out from this work was that not only is plate tectonics too simple to understand the geology of rock masses at plate margins, in fact it's bound to lead to such bloody enormous complexity that we may never work it out. ... Plate tectonics is a simple concept but the kinematics tends to some immense complications. You can build wonderfully complicated models as I did in that paper ... but taking the results [of fieldwork] and working backwards to a model, a unique model, whew! Very hard! The value of models is not that they give us solutions but give us an idea of how to proceed.[14]

In this respect, Tanya Atwater's model remains a 'unique masterpiece'. In a similar vein McKenzie (pers. comm., 2000) comments that 'Geologists such as John Dewey and Jack Bird recognized that the geological continental record contains structures and stratigraphy produced by plate boundaries, and have sketched plate geometries that could generate the features concerned. But, they are unable to show that the motions involved were those of rigid plates, and in many cases I suspect, but cannot yet prove, that they were not'.

It is in this context that controversy over what Earth scientists call variously accreted, suspect, exotic, allochthonous or tectonostratigraphic terranes erupted in the 1970s and continues today. Geologists – not rocks or other forms of evidence – open, sustain and close geological controversies. Geologists make extensive use of field observations and other data, preferred techniques, and information presented in journals, books, reports, maps and so forth. However, neither the rocks nor other facts speak for themselves: it is the geologist who makes 'the mute stones speak' for one or another side in a controversy. It is only after a controversy is settled that the 'facts of the matter' are agreed. What is at the centre of arguments over the terrane concept is not a clash between rival theories but rather preferred means of extending the global theory of plate tectonics to address problems of local geology. There is a perhaps inevitable tension in this respect between divergent and convergent thinkers. Convergent thinkers, that is, Earth scientists aiming at a global or regional synthesis, especially an explanatory one, often must make various potentially treacherous simplifying assumptions, generalize from myriad particulars of varying quality and enter less familiar subjects that strain the synthesizer's understanding. Divergent thinkers, that is, Earth scientists who are intimately familiar with a wide range of the particulars of a patch of ground, and indeed may have spent much labour collecting them, may well mount heated objections to the effect that various details have been ignored, distorted, misunderstood or misinterpreted. Such, for example, was part of the negative reaction of

[14] Interview with J. F. Dewey taped by H. E. Le Grand on 21 December 1988 at Department of Geology, Oxford University.

specialists to Wegener's promulgation of continental drift.[15]

The terrane concept[16] and research programme were initially developed in the 1970s to account for several puzzling features of western North America. These included the presence of an apparently 'Asian' assemblage of fossils in the Cache Creek group in Canada, the highly complex Franciscan Formation in the San Francisco Bay area, and the structure of the Klamath Mountains in California. Most of the loose-knit group who were involved in the early days of the terrane programme had for some years prior to the advent of plate tectonics conducted fieldwork in one or more of these locations and were associated with either the United States or Canadian Surveys.

In 1950 geologists found some unusual marine microfossils while mapping in the Canadian Cordillera near Cache Creek in British Columbia. These assemblages of fusilinids dating from the Permian period (290–200 Ma) were very different from those typical of nearby areas and of the southern and southwestern interior of North America. Instead, they seemed to be identical with those common in rocks in Asia. The presence of this 'Asian' fauna in Canada was explained in terms of a Tethyan 'seaway' connecting Asia with North America (Thompson *et al.* 1950). Wilbert R. Danner (1965, p. 120) commented that the juxtaposition of this Permian so-called 'Tethyan fauna' with the distinctive North American Permian fauna had from that time been 'commonly believed' to be due to the deposition of Tethyan (i.e. Asian) fauna 'in a Permian Tethyan seaway extending from the Mediterranean region to New Zealand and Japan and apparently extending across the Pacific Ocean to the Yukon, British Columbia, Washington and Oregon'. In other words, if one assumed that the continents did not move laterally but might be uplifted or flooded, apparent palaeontological anomalies could be explained by adducing 'sea bridges' analogous to the 'land bridges' used by others to explain similarities between groups of land animals on continents now separated by oceans. Danner himself, however, had reservations about this (Danner 1965 p. 120): 'The difference between the Tethyan faunas and other Permian faunas of North America, however, may be more that of an environmental facies than that of an isolated seaway'. He reaffirmed this in a 1966 paper that had a wider circulation (Johnson & Danner 1966). Danner's own scepticism concerning the existence of a Tethyan seaway seems to have made little impact, though the identification by him and others of an 'Asian fauna' in northwestern North America soon generated an abortive attempt at explanation in terms of plate tectonics.

Tuzo Wilson (1966) had met quick success with his proposal that many of the major features of eastern North America were due to collisional tectonics arising from the opening, closing, and reopening of the Atlantic Ocean. But, as West Coast geologists are fond of saying, the Pacific is different. Wilson sought to apply his model to the Pacific. He speculated by analogy with the finding of 'European' deposits in eastern North America that the presence of Asian fusilinids in northwestern North America meant that Asia had collided with North America through plate action and he suggested that Alaska was a part of ancient Asia left behind when the Pacific had reopened (Wilson 1968). As one geologist (no attribution by request) active at the time puts it now: 'Wilson made some great calls but this was not one of them!' Geologists familiar with the details of the geology agreed that the Asian fusilinids presented problems but there were several lines of evidence to suggest that the West Coast had faced an ocean for at least 600 Ma: there was no analogy with the Atlantic. Danner himself (1970) was highly critical of Wilson's suggestion of large-scale crustal mobility and he preferred an explanation in terms of facies changes due to environmental differences that was consonant with crustal fixity. Wilson's suggestion was ignored or dismissed as an interesting but ill-founded speculation, no doubt confirming for the moment the fears of many analysers and fieldworkers who were concerned about the use or, worse, misuse of their hard-won data by plate enthusiasts.

James W. H. Monger made more effective use of the new global tectonics in trying to account for the Permian 'Tethyan fauna' near Cache Creek described by Danner and others. Danner supervised Monger's thesis at the University of British Columbia on the stratigraphy and structure of a complicated package of rocks in the Cascade Mountains. In 1965 he joined the Canadian Geological Survey and there met John Wheeler, who had worked with Danner on the

[15] See, for example: Frankel (1976), Le Grand (1988, pp. 55–99).
[16] Terranists prefer the term 'concept' to 'theory' or 'hypothesis', in part because they regard it as subsumable within plate tectonics and in part because they consider it to be an empirical generalization.

Cache Creek, and Hubert Gabrielse. They were engaged in applying the geosyncline concept to explain the existence in the Cordillera of what appeared to be a number of parallel, tectonic 'belts' of rocks which 'were very strange' in that they differed considerably from one another in their composition and fossil content. Monger's first field assignment was to 'go and look at a group called the Cache Creek Group which runs right down the middle of British Columbia'.[17] For three years Monger worked on the stratigraphy. To assist him with the fossils, especially the fusilinids, he enlisted Charles Ross, who was already establishing a considerable reputation as a palaeontologist with special expertise in fusilinids and other foraminfera. For well-studied index fossils it is possible for geologists to consult standard monographs in order to classify and date their own specimens. Nonetheless, recourse will often be had to a recognized expert even for 'routine' fossils, because there is often much tacit knowledge and much else that does not find its way into the literature.

Monger & Ross (1971) concluded that the western part of the Cordillera could be divided into three separate, parallel belts. The eastern and western belts were very similar. They contained fusilinids mostly of the family Schwagerenidae along with other marine fossils forming the kind of non-Tethyan Permian assemblage commonly found elsewhere in North America. However, between these two belts was a very different one. It was a typically 'Asian' or Tethyan assemblage, consisting of fusulinids mostly of the family Verbeekinidae together with remnants of crinoids and algae (Monger & Ross 1971, p. 261). Ross's expertise led them to conclude that although there were marked differences in the groups of fusilinids, they were contemporaneous, at least in part. Besides differences in the fossils, the rocks themselves were so different as to suggest differences in the environment of their formation, e.g. the central belt contained extensive, thick deposits of nearly pure limestone whereas the other two contained only scattered, thin deposits mixed with other material (Monger & Ross 1971, p. 270). The central belt also contained an abundance of ribbon cherts, characteristic of a clear, deep-water marine environment.

How could one make sense of this puzzle? Monger recalls that the geosyncline concept was of little use: 'We had the "eugeosyncline" which really means nothing but some marine volcanics and sedimentary rocks and this was the model: you had the eastern Mallard Belt, the western Fraser Belt, the eugeosyncline and the miogeosyncline, which didn't work for anybody. There was no other model and it didn't tell you anything other than you called this a eugeosyncline and that a miogeosyncline. It was more labelling than explanation'.[18] Monger & Ross (1971) put forward two alternatives. The first was that the difference in fusilinid assemblages was due mostly to local environmental differences, but of course this did not address the underlying issue of how the 'Asian' fusilinids had got to Canada. The second alternative, and the one that they favoured, though cautiously, involved large crustal mobility: 'the possibility exists that faunas living in widely separated regions [with different environments] subsequently may have been transported bodily for considerable distances and brought into contact with one another' (Monger & Ross 1971, p. 273). They put forward with some diffidence several plate tectonics-based models of how this might occur. Monger stresses that at the time 'The physical basis of plate tectonics really didn't concern us at all. We just tended to accept it and, said, "look, we have these different things side by side and here's a way things could come together"'.[19] Their preferred model was one in which the eastern and western belts had once formed a single, coherent island arc which had been emplaced first, followed by a piece of oceanic crust. Then, transcurrent faulting had slid part of the inner, island arc belt to the 'outside' of the oceanic belt thus enclosing it (Monger & Ross 1971, p. 276). Their preferred model had one significant implication that they did not immediately pursue. Suppose this part of the Cordillera had indeed not formed in place, but had been added to the North American continent from parts unknown. What would this mean for the five hundred or so miles of Canada that lay to the west of these belts?

The Francisan Formation constituted a second problem. It literally surrounds Berkeley and is on the doorstep of Stanford University and the Menlo Park branch of the US Geological Survey. P. B. King, in his influential

[17] Interview with J. W. H. Monger conducted by H. E. Le Grand on 12 February 1990 at Canadian Geological Survey, Vancouver.
[18] Interview with J. W. H. Monger conducted by H. E. Le Grand on 12 February 1990 at Canadian Geological Survey, Vancouver.
[19] Interview with J. W. H. Monger conducted by H. E. Le Grand on 12 February 1990 at Canadian Geological Survey, Vancouver.

overview of 1959, described it as 'an odd-looking, much deformed, thoroughly indurated series of greywacke, shales, bedded cherts, limestone lenses, and interbedded basaltic lavas cut by many ultramafic serpentine intrusions.... This assemblage is typically eugeosynclinal. A curious feature of the Franciscan is that its base is nowhere visible'. Edgar (Ed.) Bailey of the USGS and head of the 'Franciscan Friars', a group of USGS geologists that included among others Clark Blake, Porter Irwin and D. L. (Davy) Jones, had made a systematic effort over the years to unravel the complexities of the Franciscan but to little avail. Their summary in 1964 of all that was then known about the Franciscan acknowledged that 'both major and minor structures in nearly all areas of the Franciscan are inadequately understood, in spite of the mapping that has been done by many competent geologists over a period of more than half a century' (Bailey *et al.* 1964, p. 148). In the new paradigm of plate tectonics, the Franciscan was interpreted as a subduction complex, but little detailed guidance could be gleaned from the new global theory in terms of the origins of the pieces, their interactions over time, or their relationships.

For Porter Irwin, the Klamath Mountains, extending from northwestern California into southwestern Oregon, were his back garden. He began work on them in 1953 and from 1957 was USGS project chief for their systematic mapping. For nearly three decades he spent his field seasons there, walking over the rugged landscape and mapping its geology. In the 1950s he divided the Klamaths into four belts: Eastern Jurassic, Western Palaeozoic and Triassic, Central Metamorphic, and Eastern Klamath. Each had distinctive rocks and fossils and, as one moved to the west, the belts appeared to be progressively younger. Irwin recollects that it seemed clear the belts 'hadn't grown together there, they were pieces that had formed somewhere else and been brought together, I didn't know whether they were formed ten miles or a thousand miles away'.[20] To try to express this sense of three-dimensional juxtaposed blocks of crust he decided: 'Well, rather than calling them "belts" I'd call them "terranes"'. Irwin's (1972) definition was a descriptive one with no reference to fault-boundedness: 'The term 'terrane' as used herein refers to an association of geologic features, such as stratigraphic formations, intrusive rocks, mineral deposits, and tectonic history, some or all of which lend a distinguishing character to a particular tract of rocks and which differ from those of an adjacent terrane'.

The original terrane group, dubbed the 'Menlo Park Mafia' by some, began to coalesce around the USGS in Menlo Park, California. Davy Jones assumed the role of Godfather: the foremost, and most ardent, spokesperson for the terrane concept. A senior palaeontologist at Menlo Park, in the early years of the terrane programme he provided not only much of the drive but also considerable cognitive and social resources. Alaskan geology was particularly fertile ground for demonstrating the potential of what was emerging as the terrane programme. Put simply, Alaska was a mess: a seemingly senseless jumble of rocks once characterized by Hamilton as the 'garbage dump of the Pacific'. Jones had from the late 1950s spent many field seasons in Alaska. One of the major difficulties he and others faced in trying to unravel this jumble was establishing the stratigraphic correlation or lack thereof of adjacent packages of rocks. But, many of the packages contained igneous and heavily metamorphosed rocks, for which the techniques of palaeontologists were worthless, and radiolarian cherts that, though sedimentary and common in the Cordillera, could prior to 1972 be dated only coarsely. The importance of such palaeontological controls were forcefully brought home to Jones in 1972 when he claimed – largely on the basis of lithology and indirect fossil evidence – that a large Palaeozoic section of SE Alaska was a terrane which had been moved from northern California. It was rejected by *Science* but appeared as a USGS publication (Jones *et al.* 1972) that, according to Jones, was still in the mid-1970s 'being laughed at'.[21] Jones was not, however, deterred by this unfavourable reception from pursuing the use of terranes as a means of mapping and investigating relationships among packages of rocks in the Cordillera.

The catalyst for formal co-operation in understanding the Cordillera in terms of the new tectonics seems to have been a discussion among Monger, Greg Davis, Jones and others at the Annual Meeting of the Geological Society of America in Washington, DC, over 1–3 November 1971. On 1 November, a symposium session, 'Plate tectonics in geologic history', was convened by Jack Bird, Clark Burchfiel and Gary Ernst. Papers included one by Dickinson on 'Evidence for plate tectonic regimes in the past',

[20] Interview with P. Irwin taped by H. E. Le Grand on 19 January 1989, Menlo Park.
[21] Interview with D. L. Jones taped by H. E. Le Grand on 15 May 1990, Berkeley.

Hamilton on Indonesia, Monger and others on 'Plate tectonic evolution of the Canadian Cordillera', and Peter Coney on 'Cordilleran tectonic transitions and North American plate motion' and Dewey on 'Plate models for the evolution of the Alpine Fold Belt'. On the following day, Burchfiel gave his and Davis's paper 'Nature of Paleozoic and Mesozoic thrust faulting in the Great Basin area of Nevada, Utah and southeastern California' (previously selected as one of the two 'outstanding papers' given earlier at that year's Cordilleran Section meeting). During the meeting, Monger, Davis and others also talked about the Franciscan. Jones offered Menlo Park as a meeting place to discuss unravelling the whole North American Cordillera. When this meeting on 'Cordilleran Tectonics' was held, Monger recollects: 'twenty or so of us got in a room for a weekend and just talked about whether or not correlations could be drawn between parts of the Cordillera in terms of tectonic entities and each person was given an area to describe ... the USGS had Alaska, the CGS had British Columbia ... and everybody was feeding off everybody else'.[22] The result over the next few years was a large number of papers, including reviews and overviews. Jones himself had a more particular agenda.

Within six years not only was there a dating scale based on Mesozoic radiolarian cherts, but Jones had established the Rad[iolarian] Lab at Menlo Park and was in the process of building a dating scale for Palaeozoic cherts.[23] As Jones later put it, the ability to date and correlate by age radiolarian cherts, 'just blew the whole Cordillera apart'.[24] For Jones, plate tectonics provided the mechanism for chopping up, combining and moving around pieces of crust through which he could make sense of Alaska. The pieces themselves could be characterized in terms of Irwin's definition of 'terranes'. Moreover, Jones succeeded in gaining very substantial funding from George Gryc, an old Alaska hand and collaborator, who in 1976 was appointed head of the newly established Office for National Petroleum Reserves in Alaska (ONPRA). This not only aided the rapid development of the Rad Lab, but also enabled Jones to take teams from Menlo Park to Alaska to apply the terrane concept first-hand. Peter Coney, who was a frequent visitor to Menlo Park and coined the term 'suspect terrane' (Coney *et al.* 1980), received funding from the USGS to support summer research in Alaska while on leave from his university position. He recalls: 'one year when we were working in Alaska Davy put together a team – two palaeomagicians, geochemists, biostratigraphers, a structural geologist, a sandstone petrologist – all working together and the interdisciplinary character of it was very exciting because you realized there was no way you were going to solve it by yourself'.[25]

Jones's core set included Irwin and Clark Blake and others at Menlo Park, Monger at the Canadian Survey, Peter Coney, and several scientists at Stanford. The most notable of the latter was Alan Cox, who as a former member of the USGS at Menlo had been a central figure in the development of the reversal magnetostratigraphy so important to the final acceptance of sea-floor spreading (Glen 1982) and was then dean of science at Stanford. By the mid-1970s they aimed to develop the terrane concept further and to apply it to remap western North America in terms of terranes. Their approach was underpinned by the conviction that most of western North America is made up of a chaotic assortment of rock packages that have come from elsewhere, sometimes from thousands of miles to the south and west, and then have been plastered onto the older North American craton. To underpin this approach, they used not only published and unpublished information on stratigraphy, palaeontology, palaeomagnetism, structural geography, petrology and geochemistry gathered by others, but also conducted their own fieldwork.

The value of such collaborative endeavours is well illustrated by the first major success of the terrane programme: the identification in 1977 (by Jones, Silberling & Hillhouse) of the *Wrangellia* terrane, that earlier Monger & Ross (1971) had described as a fragment of oceanic crust, not a continental fragment. Jones and his collaborators (Jones *et al.* 1977, p. 2565) now

[23] For a study of Jones's construction and use of the Rad Lab as a 'choke-hold' and 'stronghold', see: Le Grand & Glen (1993).

[24] Interview twith D. L. Jones taped by H. E. Le Grand on 18 January 1990, Berkeley.

[25] Interview with P. J. Coney taped by H. E. Le Grand on 15 February 1990 at Department of Geosciences, University of Arizona, Tucson. The range and sophistication of methods has grown steadily over the past two decades. For example, a recent reassessment of terranes in Scotland and northern England (Stone *et al.* 1999) is based upon the analysis and computer mapping of a large database of systematic geochemical analyses of stream sediments, allowing the different terranes of Scotland to be mapped and revealed according to their different subsidiary chemical contents.

proposed that this large crustal unit dubbed *Wrangellia*, was a large 'coherent' terrane extending along the Pacific margin of North America from the Wrangell Mountains in Alaska to Vancouver Island and that it had come from far to the south of its present location. Their claim was reinforced by the combination of data from the three specialties of the authors. Jones was a palaeontologist and able to draw upon data from the Rad Lab. Silberling was a stratigrapher and also a palaeontologist. Hillhouse was a palaeomagnetist. The combination of palaeomagnetic with more traditional data was of key importance. One might argue endlessly about whether or not fossils indicate distant origin or compressed facies change, but the consilience of tropical fossils with a tropical palaeolatitude proved to be very persuasive. The 'hard' empirical evidence was telling: even geologists who were in general opposed to the claims of the terranists accepted that *Wrangellia* had come from afar.

In 1978, the year following the identification of *Wrangellia*, the Pacific section of the Society of Economic Palaeontologists and Mineralogists organized a symposium at Sacramento, California, on the Mesozoic palaeogeography of the western United States (Howell & McDougall 1978). It provided an opportunity for the terranists to present their case. David Howell, who began at Menlo Park in 1974, soon joined the Mafia as a result of his work as an editor. His aim was to produce a palaeogeographic volume 'based upon a palinspastic reconstruction we all agreed upon' but in California, as he relates, 'everything fell apart, because all of the Mesozoic bodies of rock were being hotly contested and you couldn't relate one to the other in any agreeable fashion'.[26] This gave him contact with both the terrane concept and key people, including Jones, in the Mafia. Beginning in the mid-1980s Howell codified the terrane concept through his textbooks (1989, 1995) and spearheaded the extension of the programme world-wide through joint projects and conferences (Howell 1985; Howell *et al.* 1988).

The tone was set by the editors (Howell & McDougall 1978, p. viii) who stated that the border of the western United States was a passive margin in the Palaeozoic (and subject to tensional strain). In the Mesozoic an episode of mountain-building occurred as the result of plate convergence (compression). They claim this marked 'the beginning of a protracted series of continental rifting, accretion, and island arc construction that persisted through the Mesozoic'. They noted the difficulty of making a palaeogeographic reconstruction for the Cordilleran region because of the allochthonous nature of many terranes. They also stated: 'In most instances the allochthonous aspect of specific terranes is documented, but conclusions regarding the original setting remain unknown or equivocal'. The symposium was also significant for the change of rhetoric vis-a-vis allochthonous terranes. There is little evidence of the tentative nature of earlier papers; it seemed, at least for most of the people involved in the symposium, that there was no argument about whether there were such things as allochthonous terranes. They were an accepted geologic fact. By 1987 the programme launched at the 'secret meeting' at Menlo Park, to remap the Cordillera in terms of terranes, had been completed in the form of four maps covering the Cordillera from Alaska to Mexico (Silberling & Jones 1987). Perhaps equally significant was the official adoption by the USGS of rules for the nomenclature of terranes.

From 1983 terranists, including many new recruits, had begun to extend this approach to other regions of North America, e.g. the Appalachians and even to the North American craton itself. Indeed, for North American terranists virtually all of the vast lands west of longitude 111° from the imposing Brooks Range in Alaska south down the Cordillera through California and then almost all of Mexico are 'suspect' as to their birthplace and history. Conferences in other countries further promoted the terrane approach (e.g. Hashimoto & Uyeda 1983; Howell *et al.* 1984; Howell & Wiley 1988). Collaborative projects were undertaken to construct world-wide maps of terranes (e.g. Howell 1985), and the terrane programme begun by the Menlo Park Mafia is actively pursued in other regions around the Pacific Rim, including Japan, China, Australia, South America and New Zealand as well as in Europe. The aims of this ambitious undertaking are four-fold according to the 'manifesto', as the leading terranists describe it (Jones *et al.* 1983 p. 32): '(1) to identify, characterize, and portray on terrane maps all major allochthonous terranes; (2) to relate their faunal and floral characteristics through time to major palaeobiogeographic provinces; (3) to establish palaeolatitudes through time; (4) and finally, in the case of the Cordillera and Pacific region, to attempt palaeogeographic reconstructions of the palaeo-Pacific Ocean (Panthalassa) and surrounding

[26] Interview with D. G. Howell taped by H. E. Le Grand on 19 January 1989, Palo Alto.

cratonal regions'. More informally, such activities had proselytizing as a fifth aim: '[O]ur purpose was to go to a variety of places and in part use the conference as a vehicle to explain to the indigenous community where we were with the whole [terrane] concept and in part at gaining familiarity with new areas to which the terrane approach could be applied'.[27]

As was the case in North America, there were arguments over the novelty and utility of the terrane concept as well as skirmishes between the more enthusiastic converts to terranes and both plate theorists and committed field geologists. For terranists, this expansion in domain, if successful, would generate additional cognitive and social power. Clearly, the terrane programme and its supporters would become more significant if the programme could be shown to apply to regions beyond the Cordillera. To date, over four thousand publications, maps and abstracts which invoke or critique terranes have appeared since that term took on the specific connotation of 'fault-bounded geologic entities of regional extent, each characterized by a geologic history that is different from the histories of contiguous terranes' (Jones *et al.* 1983, p. 22).

Almost all Earth scientists today acknowledge that some accretion has occurred. To many, it seems that the terrane concept is a natural outgrowth and extension of plate tectonics theory; indeed, that it is merely an anticipatable corollary of plate tectonics. Yet, from the outset, considerable controversy has surrounded this approach. The controversy does not involve a clash between fundamentally incompatible theories; rather, more typical of much everyday science, it turns on and throws into sharp relief differing judgements of, and disagreements over, matters of approach, style, preferred methods and techniques, and aims within an shared, overarching theoretical framework. Conflict over facts and their interpretation is conjoined with the conflicting specialist and social interests of the geologists themselves.[28] One might explicate the terrane controversy in many ways. As noted earlier, I emphasize the tension between analysers or divergent thinkers who seek increasingly fine-grained information about, and an explanatory account of, a piece of the crust, and synthesizers or convergent thinkers who seek large-scale patterns and overarching conceptual schemes. For the former, maps may often be ends in themselves; for the latter, means to ends. This tension is certainly evident in the relationships between terranists and plate theorists and, perhaps ironically, between terranists and those specializing in the detailed study of smaller crustal units.

Terranists have been criticized by some plate theorists aiming at large-scale regional or global syntheses. Many proponents of terranes believe, not altogether without reason, that their work highlights the inadequacies of plate tectonics as applied to smaller-scale regional and local problems (e.g. Howell 1989, p. 51) and serves as a useful, empirically based corrective. Jones and others refer disparagingly to 'naive plate tectonics models' and 'Deweygrams'.[29] Rather than providing hasty generalizations, terranists argue, a more prudent course of action is to identify and characterize all the pieces of the puzzle, the terranes, before trying to interpret their emplacement and subsequent history in terms of plate tectonics.

This is reflected in the very terminology of terranes, which by USGS convention are named simply after some geographical feature associated with the terrane, thus avoiding any implied theoretical context or interpretation and any suggestion of genetic relationships with other terranes. Hamilton, like some other convergent thinkers, takes strong exception to this practice. He argues first that once a rock package is labelled as 'suspect', others in the field may well assume this without further analysis. Second, he contends that geographic names rather than

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[27] Interview with D. G. Howell taped by H. E. Le Grand on 19 January 1989, Palo Alto. This is a twist on the usual 'centre–periphery' relationship, in which data are normally gathered in the 'provinces' and sent to the 'centre' for processing, e.g. Jones's Rad Lab. Howell, Jones, and others at the 'centre' travelled to the 'periphery' to gain support and acquire data.

[28] Glen (1994, pp. 39–91) shows the influence of disciplinary specialization on theory choice and the selection and application of differing standards of appraisal in a continuing scientific debate.

[29] The term refers to idealized representations of plate interactions, together with cross-sections of crust such as those pictured in Dewey & Bird (1970). David Howell (1989), a member of the 'Menlo Park Mafia' and systematizer of the terrane programme, coined the phrase. He recalls (1989b, Tape 1, Side 1): 'I, in a public meeting, once referred to those as "Deweygrams" and I didn't mean to be insulting; I meant that they were very lucid and simplistic renderings of a perception of how, for instance, . . . an island arc accretes. But when you then go to an area you don't find just a big hunk of island arc with a knife-sharp boundary which is the suture zone and then the adjoining terrane or whatever it was. It clearly was much more complicated than that' interview with D. G. Howell taped by H. E. Le Grand on 19 January 1989, Palo Alto.

descriptive ones or ones which suggest genetic relationships in effect 'fence out anyone but the local aficionados because the outsider can keep track of lithologic packages or genetic names such as ophiolite or island arc or back arc basin . . . whereas if you hit him with thirty different geographic names in a long article he simply can't'.[30] Terranists understandably argue from their perspective that such a nominalist approach avoids premature interpretation and synthesizing.

Those critical of the terrane approach such as A. M. C. Sengor (1990*a*,*b*) deny significant value or originality to the terrane concept and relegate what some of them term 'terrane theory' – although few, if any, terranists themselves have claimed theoretical status for their conceptual structure – to the waste-bin of rehashed, conventional, descriptive geological methods and techniques.[31] These critics often regard it as just a corollary or conceptual adjunct of plate tectonics and decry what they perceive to be a 'terrane bandwagon' while denying to terranes any novelty or heuristic power. Critics of the terrane programme argue that terranists themselves have simplistic and outmoded understandings of plate theory; that is, they are not conversant with contemporary plate theory as opposed to the cartoons of the 1970s. This view is forcefully expressed by Hamilton:

> [T]he California group in general have a two-dimensional view of plate tectonics. They think of motion as either more or less perpendicular subduction or more or less strike-slip. . . . [S]o most of the Californians are still thinking of California as being assembled strictly by Andean-type, steady-state subduction, . . . of subduction as a one-sided process . . . picturing subduction as the rolling over a [fixed] hinge of a subducting plate, whereas in fact the hinge always migrates back into the subducting plate with extension occurring in the over-riding plate. They just haven't learned how modern systems work, so I think much of their work is going to have to be done again.[32]

There is of course, exaggeration on both sides of the fact–theory divide. Were one to take literally the first of the 'four steps' of the terranist manifesto, i.e. 'to identify, characterize, and portray on terrane maps all major allochthonous terranes', that itself appears to be a never-ending task which would mean that the next three steps would never be undertaken. First, as a methodological point, one would have to describe in some considerable detail without any preconceptions every conceivable crustal unit in non-theory-laden language. This is in accord with the formal presumption that every crustal unit is 'suspect' until proven 'innocent'; that is, that the crustal unit should be presumed to be allochthonous until proved otherwise and, moreover, that it is legitimate to infer fault boundaries even if they have not been directly observed. Moreover, there are social and cognitive mechanisms for generating more and more and smaller and smaller terranes. Each time one remaps an area, each time one uses new techniques, one tends to produce a finer and finer grained study and therefore more and more detail and to find new, previously unmapped features. In a very real sense there is an unspoken mandate to produce a more finely divided taxonomy than previous workers, thereby implying a more profound understanding. To remap an area and find nothing new could well be construed as a failure. Thus, every time an area is mapped, finer and finer subdivisions and greater and greater specificity of components and structures are delineated. Only partly tongue in cheek, one might predict that the number of terranes is inversely proportional to the distance from major centres of geological study and fieldwork and directly proportional to the number of geologists who have worked in the region.

Terranists have also been criticized for overemphasizing the far-travelled nature of terranes. There is a subtext here, particularly with reference to the early battleground of western North America. Through the late 1970s and into the early 1990s terranists claimed south to north displacements of thousands of kilometres for some terranes, largely on the basis of palaeomagnetic data. This seemed to be discordant with other geological data which suggested much smaller displacements and thus to pose a potential threat to the conventional plate tectonic analysis in terms of transport of crustal blocks northwards along the San Andreas transform system. Were processes at work that perhaps were not accounted for in terms of more

[30] Interview with W. B. Hamilton taped by H. E. Le Grand on 22 January 1990, USGS, Denver.
[31] Peter Coney, among others, explicitly makes the point that the terrane approach 'was never a theory. It never was. It was a method of analysis . . . to know what you've got, so you can talk about what's there'. Interview with P. J. Coney taped by H. E. Le Grand on 15 February 1990 at Department of Geosciences, University of Arizona, Tucson.
[32] Interview with W. B. Hamilton taped by H. E. Le Grand on 22 January 1990, USGS, Denver.

orthodox plate interactions? The mammoth, influential *Geology of North America* opted for large displacements (Oldow *et al.* 1989).

Alternatively, was there a flaw in the generally accepted apparent polar wander path for North America that served as the reference frame the palaeomechanisms involved?[33] Robert Butler (Butler *et al.* 1984) argued that data from plutons consistent with a larger-than-expected transport along the San Andreas might be explained by the tilting of those plutons that could lead to erroneous apparent polar wandering paths and thus the calculation of a greater latitudindal motion than had actually occurred. Support for this view came from the work of others. European palaeomagneticists (e.g. Courtillot *et al.* 1994) have pointed out that the apparent motion of the magnetic poles constructed from data taken from the southwestern United States conflict with that based on data taken from the northeastern United States, the former entailing far less transport along the San Andreas than the latter. Data taken from sites in Europe and Africa yield polar positions consonant with that from the southwestern United States. Since the Earth could at any one time have only one set of magnetic poles, they suggest – agreeing with Butler – that the apparent polar wandering paths which formed the basis for postulating larger-than-expected displacements on the San Andreas should be questioned. Recently, a further major study by Dickinson & Butler (1998, p. 1268) has reaffirmed this conclusion and presents a comprehensive argument that 'the hypothesis of major tectonic transport in excess of distances inherent in current models is unnecessary and in conflict with geologic observations'. Instead, they contend that any discrepancies are removed through better data sets and making proper allowance for the tilting of formations after magnetization and the effects of sedimentary compaction. Not all are convinced. Ted Irving (pers. comm., 2001) stands by his original determinations implying very large-scale transport and finds it implausible that compaction and tilting should exactly account for what are otherwise calculated to be significant displacements. Even if Butler's argument be accepted, however, it would seem to strike only at terranists' claims of large and rapid transport of crustal blocks, not at their key point that one should not assume that just because two blocks are now adjacent they were formed in place. Moreover, it might contribute to a rapprochement between terranists who are overtly critical of 'naive' plate tectonics and plate tectonicists who may regard such criticisms as touching upon the adequacy of accepted plate mechanisms.

Terranists have been criticized not only by those pursuing a more convergent approach. On venturing new interpretations of crustal units they have been harshly criticized by some field geologists for trespassing on the pieces of crust to which they have devoted their careers. Howell observes that most geologists work in only one or two areas for good reason: 'It's a lot of work to learn all the stratigraphies, the names, the places and you are never satisfied and there are no artificial boundaries to a given area – you can expand it as much as you can'.[34] Many were expert in, and had in fact mastered, the finely detailed geology of the crustal units and domains that the terranists sought to subsume in their newly defined taxonomy. Such a conceptual reordering, reinterpretation, redefinition and reclassification of what was regarded as well-established and defined geology appeared to turn bodies of knowledge won through strenuous efforts of mapping and study by teams of distinguished geologists into mere grist for the terranists' mill. Howell freely acknowledges this source of criticism from some old Alaskan hands: 'guys who have worked an awful lot, sweated blood, witnessed deaths, been threatened themselves to extract this information ... take a certain amount of umbrage at some dandy coming in and spending a summer reinterpreting all the rocks.... We were bold and at various times may have given the impression of arrogance; frankly, we were just enthusiastic'.[35]

Although the terrane programme's detractors may be correct in their claim that the terrane programme offers nothing theoretically or methodologically new, the terranists' claim of new solutions has mobilized efforts at reassessment of longstanding, intractable problems and led to the generation of new, refined data and their reinterpretation. A recent, particularly elegant example of this is a new approach to terrane identification and analysis using geochemical data (Stone *et al.* 1999). This uses a large data base of chemical analyses of stream sediments in Scotland: the sediments serve as a proxy for the composition of the underlying

[33] Hagstrum (1993), though a proponent of the Baja-British Columbia interpretation of large displacements, provides a balanced review of this controversy up to that time.
[34] Interview with D. G. Howell taped by H. E. Le Grand on 19 January 1989, Palo Alto.
[35] Interview with D. G. Howell taped by H. E. Le Grand on 19 January 1989, Palo Alto

bedrock (Stone *et al.* 1999, p. 146) and thus as an indicator of variations in the chemistry among, and within, possible terranes. The relative abundance of selected elements that are reflective of the mineralogy can then be mapped by computer to yield 'pictures' of terranes.

The terrane controversy has also helped draw researchers from the ether of plate theory abstraction down to the empirical, fine-scale reassessment of previous and long-established maps, data, fossils, monographs and interpretations in terms of the standards that define the terrane programme.[36] It may also have provided impetus to its detractors further to refine alternative explanations couched in plate tectonics terms. The terrane programme may be a bandwagon, but as Coney refreshingly put it: 'it was a bandwagon – and in part deliberately orchestrated, but it's been a great ride and a hell of a good way to do regional geology'.[37] John Dewey has occasionally put spokes in the wheels of that bandwagon but he too asserts the fundamental importance of field geology and mapping for, as he (pers. comm.) puts it: 'Rocks, fossils and minerals are immensely complicated systems but they – with geophysical and geochemical data and the ideas that stem from them – are the substance of our science'.

Many of the ideas presented in this paper were developed in lengthy dicussions with W. Glen (but I take responsibility for their form as presented here). I am also grateful to L. Harrington for his comments.

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[36] These are captured in what is sometimes referred to as the 'Manifesto' (e.g. Jones *et al.* 1983; Howell 1995).
[37] Interview with P. J. Coney taped by H. E. Le Grand on 15 February 1990 at Department of Geosciences, University of Arizona, Tucson.

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