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ORIGINAL PAPER

R. Trümpy

Why plate tectonics was not invented in the Alps

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Abstract In the Alps, folds were recognized in the early eighteenth century, thrusts in the middle of the nineteenth century. The nappe theory, developed from 1884 to 1902, led to a mobilistic approach, implying large-scale relative movements of Europe and of a prong of Africa (Argand, Staub). The existence of Mesozoic oceans or ocean-like basins was also realized. Ampferer introduced the concept of subduction (Verschluckung). Around 1935, Alpine geologists somehow became afraid of their own courage, and failed to present a coherent interpretation of the structure and evolution of the chain. The theory of plate tectonics was developed primarily by geophysicists at sea, who took little account of the Alpine evidence.

Keywords Alps · History of geology · Nappe tectonics · Plate tectonics · Ampferer · Argand · Staub

Introduction: the obvious answer

Like Venus, the theory of plate tectonics is very beautiful and born out of the sea.

Land-based paleomagnetic research, especially by K. Runcorn (e.g. 1955) had already shown that the apparent polar wander paths from different continents did not coincide. Most geologists did not immediately grasp the significance of these results.

Almost all other foundations came from marine geology and particularly geophysics. F.A. Vening-Meinesz' submarine cruises with his pendulum gravimeter (from 1926 onwards) may have started it all. World War II brought a bonanza to marine science, especially in the United States; a tremendous amount of bathymetric, gravity, seismic and magnetic data were assembled and later gradually released. Names like Maurice Ewing, B.C. Heezen, Harry Hess and Edward Bullard stand for highly active and successful teams. The interpretation of the symmetric magnetic anomalies along oceanic ridges by Vine and Matthews (1963) was a decisive step. The theory was ripe; it was announced most clearly by John Tuzo Wilson (1963, 1965). The JOIDES campaigns of deep drilling in the oceans corroborated its predictions.

The development of the plate tectonics theory has been amply discussed by geophysicists, geologists and historians. The most lively, though hardly the least biased account, is that of R. Muir Wood (1985). The theory is ocean-born, and the answer to our title question is a very simple one: because of the modest tonnage of the Swiss and Austrian navies.

But there is another and more pertinent question. Why did the Alps and similar mountain chains, which provided clear evidence for large-scale relative displacements between continents or continental fragments, play such an insignificant part in the establishment and acceptance of the plate tectonics theory? The very same question arises with the close tectonic, stratigraphic and paleontological links between the southern continents, up to Jurassic time. What were the reasons for this neglect?

Folds and nappes

Folds had been recognized and drawn in the Alps since the early eighteenth century, notably by Luigi F. Marsili and Johannes Scheuchzer (Scheuchzer 1716–1718; Koch 1952; Ellenberger 1995; Vaccari 2001). Johannes Scheuchzer explained them as due to upheavals when the waters of the Deluge, which had

This paper is a modified version of an evening talk, presented at Tübingen on 22 September 1999. The author, emeritus of the ETH Zurich, was born in 1921; his age may partly excuse the insertion of personal reminiscences [in square brackets].

R. Trümpy (✉)
Allmendboden 19, 8700, Küsnacht, Switzerland
Phone: +41-1-9104520

previously softened the sediments, retreated into their subterranean cavities. Late in the same century, Horace-B. de Saussure (1796) was the first to attribute folding to compression (refoulement latéral, see Masson 1976). The volumes by Heim (1878) provided splendid illustrations of Alpine folds from thin section to mountain scale.

While folds could be observed in the field, the existence of thrusts could only be established once the relative age of the footwall and hanging wall rock bodies had been determined by the painstaking work of the early Alpine stratigraphers, from about 1820 to 1850. Arnold Escher (1841, 1846) recognized major thrusts in the Glarus Alps, Bernhard Studer (1853) in central Switzerland. In 1848, Escher led one of the greatest luminaries of the time, Sir Roderick Impey Murchison, over the Segnas pass and convinced him of the presence of "one great overthrow" (Murchison 1849).

But then, at some time between 1848 and 1866 (possibly before 1854) Escher's mind took a strange turn (e.g. see Trümpy 1991). He appears to have been a very cautious, even timid person, and at the same time he had the conviction of searching for the Truth, with a capital T. His favourite slogan was "*besser zweifeln als irren*" (better to doubt than to err); being wrong was considered as a serious defect and not, as most of us see it now, as an inevitable step in the development of science. Escher was afraid that proposing a great overthrust in the Glarus Alps would lead people to question his sanity. In an attempt to minimize the amount of lateral displacement, he invented the weird Glarus Double Fold: two recumbent folds facing each other and enclosing a "tobacco-pouch" syncline of Tertiary flysch (in those days, the raw material for most tobacco-pouches was furnished by rams). Escher's disciple, Albert Heim (1878, 1891), fully endorsed the Double Fold theory; thanks to his descriptions and splendid drawings, it was almost universally accepted and became a Church Triumphant. As late as 1891, Heim declared "einen langen Athem hat die Wahrheit" (Truth hath a long breath).

The Double Fold was a local phenomenon, and furthermore a geometrical monstrosity. Thanks to the authority of Albert Heim, it delayed the recognition of the Alpine nappe structures for 30 years. We may consider it as an epicycle – the first epicycle – in the development of Alpine tectonics. (In pre-Copernican astronomy, epicycles referred to the retrograde movements of a planet's apparent path.) In passing, it should be noted that G.V. Dal Piaz (1996) has shown that, in 1869, Felice Giordano also considered, but then discarded, the hypothesis of huge basement nappes – Argand's future Dent Blanche nappe – in the Pennine Alps. Giordano was probably the first to use the term of falda (=nappe, Decke) in the tectonic sense.

In 1884, Marcel Bertrand reinterpreted the Glarus structures and showed that a single, north-directed thrust was far more plausible than the bi-vergent Double Fold. Almost at the same time, even greater thrusts were recognized in the Scottish Highlands and in the Scandinavian Caledonides, by Lapworth, Törnebohm and others. Eduard Suess visited the Glarus region in 1892 (see Trümpy and Oberhauser 1999) and became convinced that Bertrand was right; he tried to persuade Albert Heim as well, but the Zurich professor remained obstinate. Hans Schardt's admirable 1893 paper demonstrated the far-travelled nature of the Prealps and heralded the breakthrough of the nappe concept. Maurice Lugeon (1902) and Pierre Termier became its most eloquent spokesmen; Termier (1904) was also the first to describe nappe structures in the Eastern Alps. Within a decade, the allochthonous character not only of the Helvetic and Prealpine nappes, along the northern margin of the chain, but also of the internal parts (Lugeon and Argand 1905) was firmly established.

Implications of the Alpine nappe structure, 1884–1934

The nappe structures, analysed during this half-century and particularly from 1902 onwards, implied shortening of several hundreds of kilometres, as measured along the interface between pre-Pennsylvanian basement and Mesozoic cover rocks (Late Paleozoic formations occupying intermediate and variable positions). An exact figure could hardly be advanced; the influence of stretching, by lamination and bedding-plane slip, and of thickening, by small-scale folding, could only be evaluated crudely. 300 km of shortening was a minimum, 500 km a plausible and 1,000 km a possible value, based on this exclusively geometrical reasoning.

Marcel Bertrand and especially Albert Heim, after his conversion to nappe tectonics (1902), regarded all nappes as exaggerated recumbent folds. This was also, to a large extent, the viewpoint of Emile Argand (1911). Others, such as Hans Schardt, Rudolf Staub (1937) and the Austroalpine geologists (Ampferer and Hammer 1906) identified thrust-planes, without inverted limbs, at the base of most nappes. Pierre Termier (1906) distinguished first-order nappes (recumbent folds) and second-order nappes (thrust-sheets). The former prevail in the deeper, the latter in the shallower parts of the edifice. There are numerous transitions between the two types.

Paleogeographic analysis advanced together with or immediately after the geometrical unravelling of the nappe structures. According to Sengör (1996), Dufrénoy (in Dufrénoy and Elie de Beaumont 1848, p. 154) was the first geologist to suggest in rather ambiguous terms that the sediments in mountain belts differed fundamentally from those of the surrounding areas with flat-lying beds. E. Haug (1900) imported the "American" concept of geosynclines to Europe, modifying it in so far as he regarded these furrows, or part of them, as filled with deep-sea ("bathyal" or even abyssal) sediments. Steinmann (1905) interpreted certain formations (e.g. bedded cherts with radiolaria) as oceanic. Arnold Heim (1916) was the pioneer of palinspastic reconstructions; his excellent but little-noticed paper of 1925 announced modern sedimentological ideas (see also Arbenz 1919). Argand (1916, 1920), Staub (1917) and Haug (1925) distinguished a number of troughs ("geosynclines") and linear rises ("geanticlines"; this usage is, of course, quite different from the original sense of Dana's term). A reasonable minimum breadth of these paleogeographical elements had to be taken into account, in order to accommodate the supposed original emplacement of the sediments now found in dêcollement nappes, and also to provide the source areas of detrital (e.g. flysch) formations.

This pre-compressional, essentially Mesozoic pattern implied an original width of the same order, or rather greater than the one derived from nappe geometry alone. Basins with oceanic sediments could hardly have been only a few kilometres wide. An original width of 300 km (barely possible on geometrical grounds) became improbable, 500 or 1,000 km credible. These figures apply to the time just before compression, i.e. in modern terms, after the spreading of the Alpine oceans. French and Swiss geologists believed that all the shortening had happened in Cenozoic times, whereas the Austrians stressed the importance of mid- to Late Cretaceous compressional events.

The syntheses by Argand (1911, 1916), Kober (1912, 1923) and Staub (1924) provided the "classical" image of Alpine tectonics. The well-written books in English by Bailey (1935) and Collet (1935) commanded wide recognition. The Alps were considered as a model for most mountain chains, and many students, particularly in extra-Alpine Europe, had to learn about their geology.

Tectonic theories in the Alps, 1884–1934

At the end of the nineteenth century, the cause of the folding in mountain belts was perfectly obvious: it was due to the shrinking of a cooling Earth. Pre-existing zones of weakness, the geosynclines, explained the location of the folding in narrow belts. Albert Heim and the greatest Alpine geologist of the time, Eduard Suess, firmly adhered to this interpretation.

The contraction theory became discredited at the turn of the century, partly because it failed to account for the strong crustal shortening in the Alps and similar mountain chains, but mainly because of the influence of radioactive decay (Joly 1903). For a while, there was no alternative theory to replace it. This does not seem to have unduly worried the early nappe tectonicians; elucidating the structure and history of the chain provided a sufficient challenge.

Alfred Wegener's (1912, 1915) hypothesis of continental drift at last brought an acceptable explanation of Alpine evolution. It was immediately taken up by Emile Argand (1916, 1924b) and followed by Rudolf Staub (1924). In this new, mobilistic view, the Austroalpine nappes became the front of the Africa-related Adriatic or Apulian prong, having overridden the oceans of the Penninic nappes and the European continental margin of the Helvetic belt.

Most of the French, Italian and Swiss geologists working in the Alps welcomed Wegener's thesis. Argand's great, widely admired, though less understood paper on the tectonics of Asia (1924a) attributed all structures, including those of continental interiors, to horizontal displacements. Staub pushed the notion of unilateral movements in the Alps, stressed in Suess' 1875 booklet, to its extremes. In 1928, he produced his own version of global drift tectonics, with the *perpetuum mobile* of Poldrift and Polflucht (movement of continents towards and away from the poles) and rather surprising transatlantic mountain connections.

In Austria, the development followed different paths. Leopold Kober (1923) clung to some sort of thermal contraction (both Kober and Staub had, to put it mildly, rather personal views on the laws of physics). Kober did, of course, fully accept the existence of large nappes, but regarded all orogens as bivergent and more or less symmetrical. This led to a more autochthonist picture. Otto Ampferer (1906, and in Ampferer and Hammer 1911) introduced the notion of Verschluckung ("swallowing" or, in more fashionable terms, subduction) of crustal slabs into the depths of the Alpine edifice. Ampferer published his highly interesting ideas in many small papers, but their significance was hardly noticed by his contemporaries (e.g. see Trümpy and Oberhauser 1999).

Alpine, and particularly Swiss, geologists must also be blamed for not taking into account the contributions of Arthur Holmes (especially 1929), who spent a summer term as guest professor at the University of Basel. Holmes' concepts incorporated convection currents in the mantle, continental drift, oceanic as well as intra-continental rifting and spreading; of all the theoretical views of these early times, his came closest to the plate tectonics model.

Wegener's hypothesis had at first been welcomed in most of Europe (e.g. Gagnebin 1922), including Britain (e.g. Holmes 1926). Beyond the Atlantic, its reception was at best cautious, becoming frankly hostile after a few years. The sad story has been thoroughly analysed by Naomi Oreskes (1999). The dogma of continental accretion and the presence of a true land bridge, the Isthmus of Panama, may have been instrumental in the development of this attitude. Continental drift was considered as a fairy-tale (*ein Märchen*, Bailey Willis 1944), and as a sort of un-American activity.

In the 1940s, only a few geologists, mainly from the Alps and from Gondwana, still spoke out for continental drift. Foremost of the latter was Alexander du

Toit (e.g. 1937), and indeed the case for the pre-Jurassic coherence of the southern continents was as sound as the Alpine evidence for large-scale relative motions of Europe and Africa.

[In the autumn of 1948, I had the privilege of participating in an excursion to the Scottish Highlands. The field trips had to end early in the afternoon, in order not to miss the five o'clock tea at the hotel. A fortyish Canadian and I asked our leader, Sir Edward Bailey, for permission to climb Bidean nam Bian, south of Glencoe. Watching a glorious sunset, I tried to convince my senior companion of continental drift; the Canadian laughed at me good-naturedly and told me that it was all fancy and physically impossible. His name was John Tuzo Wilson.]

The second epicycle of Alpine geology, 1935–1950

The great impetus of Alpine tectonics petered out in the mid-1930s. Beautiful geological maps continued to be made; petrology and micropaleontology developed new methods. Many papers from this period deal with regional problems, such as the along-strike correlation of individual nappes. Such questions were indeed important for Alpine geologists who were attempting to reconstruct the paleogeographic pattern and the evolution of the chain, but of little interest to outsiders. Few constructive ideas arose. Tercier's (1939) attempt to compare Alpine sediments with those of the present seas around Indonesia went almost unnoticed.

This stagnation was not only due to scientific reasons, but also to parochial tendencies and to a decline of international cooperation and discussion. This chauvinism was least marked in Italy (e.g. see Dal Piaz and Dal Piaz 1984), in spite of the Fascist regime. In France, authoritarian university structures and enclosed study "terrains" (no trespassing) prevailed. In Switzerland, consulting for hydroelectric power plants took up much of the time and energy of university geologists. Autarkic geology was popular in Austria, before and especially after 1938; the supposedly exaggerated confidence of the geologists working in the western half of the chain on nappe correlation was branded as "nappism" (e.g. see Flügel and Trümpy 1994).

The first or Double Fold epicycle grew out of Escher's and Heim's reluctance to accept the consequences of their own observations and their endeavour to make crustal shortening as modest as possible. Likewise, the second epicycle resulted from an attempt to minimize the effects of Alpine structuring, to reduce the amplitude of crustal shortening as far as or further than the observations allowed. The mobilistic views of Wegener, Argand and early Staub were criticized, even if it was difficult to disavow them entirely. We have already mentioned that Kober (e.g. 1955) upheld fairly autochthonist views. Even Staub, in some of his late works (1953), reverted to a neo-fixist stance. The strangest blossom of this antimobilist backlash flowered as late as in the 1950s, with the "*Gebundene Tektonik*" of some German authors (e.g. Kockel 1956), which tried to resurrect the mushroom-and-tobacco-pouch structures of ante-nappe times.

In this state of affairs, when many geologists tried to downplay the importance of Alpine crustal shortening, the theory of nappe emplacement by gravity gliding from geotumours (Haarmann 1930) presented itself as an easy solution. In the Alps, it was first invoked by Daniel Schneegans (1938) and then endorsed by several authors (e.g. Lugeon and Gagnebin 1941; Gignoux 1948). These Alpine geologists were fairly cautious, considering gravity gliding as one of several processes; others tried to explain all nappe structures by this mechanism (e.g. van Bemmelen 1954; Beloussov 1962).

There is no doubt that gravity gliding does occur in the superficial and external part of orogenic belts, e.g. at the margin of the Rif–Tell–Peloritanian chain between Morocco and Sicily. The generalized theory, however, leads to absurd constructions, linear geotumours high above sea level, decorated by signposts forbidding erosion. These bulges should develop prior to the nappe movements. There is no evidence for such monstrous welts; the uplift only took place after the emplacement of the nappes. Marcel Bertrand (in Bertrand and Ritter 1896) realized long ago that most nappes formed at great depth. Early post-tectonic metamorphism in the Alps is predominantly of greenschist and, in the deeper parts of the edifice, of amphibolite grade. It is surprising that such excellent geologists as O. Ampferer (1934) or A. Holmes seriously considered gravity gliding as a major factor. In Ampferer's case, his application of the gravity model to the Glarus Alps led to the rejection of his ideas by Swiss geologists, who spilt the healthy child of subduction (Verschluckung) with the lukewarm bath water of gravity tectonics.

During the first third of the twentieth century, the Alps had served as a model for all mountain chains. Due to the timidity and to the parochial squabbles of Alpine geologists, they now became a sort of special case, of minor significance for tectonics at large. The leading German tectonicians, Stille and even Cloos, took little interest in the chain. Martin Rutten's 1969 book expresses an almost paranoid hatred of classical Alpine geology; only gravity tectonics were hailed as really "modern".

[At the tender age of 21, I gave a seminar talk on the "origin of mountain belts". I concluded that there was no satisfactory explanation, apart from "some sort of" continental drift (I had not read Holmes and misjudged Ampferer), and that we had to wait for a valid theory. We waited for another quarter of a century.]

Reappraisal and the impact of plate tectonics

The frontiers opened after World War II; international cooperation and discussion came to life again. The most important correlation problem, concerning the origin of the Prealpine cover nappes, was finally resolved, largely thanks to François Ellenberger (1952, 1958). Alexander Tollmann (1963) advanced refreshing ideas on the structure of the Eastern Alps. Much progress was made in analysing the relations between deformation and metamorphism; isotope age data began to be published in the 1950s. Pre-compressional tectonics was also reinterpreted.

In principle, the Alpine geologists should have been well prepared to accept the new theory of plate tectonics. Argand, Staub and others had shown that Eurasia and Africa had undergone relative displacements of the order of many hundreds of kilometres. Steinmann and others had realized that the Mesozoic seas between these two blocks had been, in part at least, of oceanic character. Ampferer had added the concept of subduction (the term itself had also been coined in the Alps, by André Amstutz 1955). Hardly anybody noticed Ampferer's little 1941 paper, invoking Atlantic spreading and subduction along the western margin of the Americas (Ampferer did not mention Arthur Holmes, who had foreseen all this back in 1929; Ampferer rarely quoted any publications, except his own, and his often remarkable notes would all have been rejected by present-day reviewers).

In spite of this, most Alpine geologists, with the exception of Hans-Peter Laubscher (1969), remained at first sceptical and became convinced only when the predictions had been confirmed by the deep-sea drilling campaigns. The first attempts to apply the plate tectonics model to the Alps and to the Mediterranean domain in general (Dewey and Bird 1970; Smith 1971) were not made by scientists from these regions, and did inevitably contain some reconstructions open to criticism.

[Shamefacedly, I must admit that I was not among the first Alpine geologists to grasp the promise of the new tectonics, even if I had been a lifelong drifter. Quite typically for a field-oriented conventional geologist, my doubts arose from two "local" problems, the Gibraltar quandary and the Chukchi quandary. Neogene dextral offset between the Pillars of Hercules is a meagre 15 km; but the Jurassic to Paleogene plate boundary does exist all the same, lying about 100 km north of the Strait and largely hidden under nappes. The second puzzle is more intriguing: no post-Cretaceous plate boundary has been found between Siberia and Alaska. My personal friend and scientific opponent Vladimir V. Beloussov, a staunch adversary of plate tectonics, has often insisted on this point.]

To the Alps, plate tectonics brought a better understanding of Alpine oceans and their margins, a plausible interpretation of ophiolites (even if the orthodox "Penrose model" can only partially be applied) and a convincing mechanism for subduction, high-pressure metamorphism and nappe formation. It introduced an independent, external control for the amounts of extension, transcurrent displacements and compression, at least for the last 165 million years. No model is perfect; at present, exciting discussions are going on, notably concerning the mode of oceanic extension.

Epilogue: why was the Alpine evidence ignored?

Why did the geology of the Alps and similar mountain chains have such a feeble impact on the development of the plate tectonics theory?

On one side, there may be a part of rich man's arrogance. As every writer of grant proposals knows, real science is very expensive and produces long lists of precise figures. Results obtained by such obsolete means as a hammer or a library, for example the relative movement of Africa and Eurasia or the pre-Jurassic fit of the Gondwana fragments, count at best as second-rate science or "natural history". Four courageous American geologists (Davis et al. 1974) tried to defend the "old global tectonics", but they received little thanks.

On the other hand, the Alpine geologists must assume a fair part of the blame. [This also includes the author of this paper, who had started to write a book on the Alps and who lacked the energy and courage to complete it.] Since about 1935, they had disposed of the data and ideas advanced by fellow highlanders, such as Argand, Ampferer and Staub, and by lowlanders, such as Wegener, Holmes and du Toit. This would certainly not have allowed them to construe a global model, but at least a coherent interpretation of Alpine structure and evolution. Would such an attempt have been heeded by the geophysicists who did establish the plate tectonics theory? We cannot tell.

In this paper, two epicycles of Alpine geology have been considered, namely the Double Fold epicycle (ca. 1860–ca. 1890) and the Small-Alps or Gravity Tectonics epicycle (ca. 1935–ca. 1950). Both had similar causes: the excessive caution of Alpine geologists and their reluctance to realize the consequences of their own observations.

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