

state of lithification may sometimes be clear – for example, if late-stage diagenetic or metamorphic minerals have demonstrably been involved in the deformation – but if these features are not visible, or they are overprinting earlier structures, then the situation can be difficult. As Chapter 1 emphasized, tectonic deformation of rocks at depth cannot be assumed, or just guessed, as being more likely. Judging by modern earth processes, gravity driven processes and near-surface sediment deformation, for example, are too widespread for that.

But how can pre-lithification deformation be identified? The question has been asked many times over the decades (e.g. P.F. Williams, Collins and Wiltshire 1969; Helwig 1970; Cowan 1982; Elliott and Williams 1988). In fact most papers on pre-lithification structures include

some discussion about how they were recognized. Unfortunately, many earlier discussions of the problem have been aimed at the meaningless distinction between ‘tectonic’ and ‘soft-sediment’ deformation (see section 1.1.1). Moreover, the latter term has been used frequently merely as a synonym for gravitational slumping. Some features that have been used as hall-marks of a pre-lithification origin do not hold up to scrutiny. Rather than merely catalogue here the numerous past efforts, one historical episode is recounted in some detail in Historical vignette 3. Apart from encapsulating the conceptual difficulties of recognizing pre-lithification deformation, this vitriolic dispute, between two leading British academics of the first half of this century, rehearsed the arguments on identification criteria.

Historical Vignette 3: Possible slump deformation in North Wales

The early interests of P.G.H. Boswell were diverse. They included the link between geology and the new science of soil mechanics, including properties such as the thixotropy and dilatancy of clays. Boswell conducted research into other aspects of applied geology, and introduced into English the term ‘isopachytes’, now widely used in oil geology and elsewhere as isopachs. The considerable irony in all this will become apparent shortly. In 1917, Boswell became the first Professor of Geology at the University of Liverpool, and felt it prudent to undertake field studies close to his new University. For this he selected the nearby but little known Denbigh Moors region of North Wales. Boswell’s investigations there soon revealed puzzling things about the structures, which he began to explain by thrust movements. It seems that once Boswell had embarked upon this direction of geological thinking, it became increasingly easy to invoke tectonically induced dislocations to explain other problematic structures. Features such as ‘confused masses of highly cleaved and crushed flags, mélanges of shattered rock, often ‘balled-up into spheroidal masses’ and sharply separated from normal beds’ were diagnosed as being due to thrusting (Boswell 1928). Other geologists were shown the structures and seem to have accepted this interpretation.

These must have been happy times for Boswell. He built up a respected department at Liverpool and then became head of the department at Imperial

College, London. He received honours such as election to the Royal Society in 1932, and his work in North Wales was much respected. He was publishing numerous papers on the area, and knew the geology better than anyone else. It must have seemed like ‘his ground’. Enter, however, in 1935, O.T. Jones. In some ways, Jones was a long-standing rival of Boswell, in that he also had a long and successful history of geological field work in Lower Palaeozoic rocks (in Central Wales) and he was head of a rival department at Cambridge. Both were leading figures in the Geological Society and both were Fellows of the Royal Society. But Jones had seen rocks elsewhere with features that had registered with him the importance of subaqueous sliding as a process. The concept seems to have stayed dormant in Jones’s mind until his ‘active interest was awakened on reading Boswell’s papers and studying the photographs’ from the Denbigh region. It appeared to Jones ‘as though the sediments had been violently stirred while still in a pasty condition’. He eventually gave a report on the Denbigh Moors phenomena – the benchmark paper of 1937 – to the Geological Society, saying that ‘there is no doubt that the disturbance of the contorted rocks had been completed before the deposition of the overlying sediments’ (Jones 1937). ‘A long-standing engagement’ prevented Boswell from attending the lecture.

Jones’s interpretations were to a large extent intuitive; in any case there was little direct informa-

tion available at the time on how sediments would behave during sliding. Much of his evidence, as is often still the case, depended on describing the variability and intricacy of the structures, and viewing them as incompatible with the relatively ordered results expected from tectonism. His mapping had shown that there were many disturbed horizons, some less than a metre in thickness and others hundreds of metres thick, covering a hundred square kilometres or more. Jones had no problem envisaging submarine sliding taking place at all these sorts of scales, but to Boswell this was a major difficulty. Other objections raised by Boswell in his written discussion were readily viewed by Jones as a natural part of the sliding process: a discordant base, for example, noted from one of the presumed slides, was simply due to the moving material 'digging in' to the floor during rapid movement. Also prescient of many discussions today, Jones remarked that 'some confusion was necessarily introduced by the word tectonics. Small stresses acting on weak, unconsolidated materials give rise to structures difficult to distinguish from the effects of large 'tectonic' stresses operating on consolidated rocks'.

The next shot was fired by Boswell, in a paper which was published back-to-back with Jones's! He concluded that 'if features characteristic of slumping were once present, they have been almost, if not entirely, obliterated' (Boswell 1937). He evidently was happy to accept submarine sliding as a viable process, but not on a large scale. 'If one small area is accepted as due to slumping, then the similarity of the features elsewhere means that the entire region must have been affected', which Boswell viewed as unrealistic. Boswell's next tack was to emphasize stratigraphical arguments. He felt that on the basis of the observed structures alone 'it may be difficult or almost impossible to distinguish between the effects of contemporaneous sliding and those of later earth-movement'. However, he asserted, 'the interpretation... of large-scale slumping is not... in harmony with the stratigraphic evidence' (Boswell and Double 1938). He began to grasp at stratigraphy as his major weapon in the battle, and went on to spend much energy in refining it.

Jones gave a second paper devoted to submarine sliding, an influential and much cited publication (Jones 1939a). Now, just as Boswell's descriptions tended to have thrusting as a premise, so Jones's writings assumed the importance of slumping. He began, 'the distinguishing feature of the

district is the occurrence of large numbers of slumped beds. These are so clearly displayed that this district may be regarded as the most favourable area for the detailed study of such occurrences'. Jones went on to detail what he saw as the supporting evidence, and to discuss how he envisaged the processes happening. He provided detailed maps and numerous field sketches, a few of which are reproduced here as Figure 9.37. He argued that the upper boundaries of the disturbed sheets were critical. The sharpness of the junctions, with no signs of movement along them, the slight grading upwards of the overlying sediment, indicating *in situ* deposition, and the lithological similarity between the disturbed and the overlying materials were, to Jones, wholly incompatible with a tectonic origin. He specified 39 localities where such crucial upper contacts could be clearly examined.

To most of the listeners at the Geological Society the evidence was overwhelming, but Boswell was less than happy. Again he was absent from the meeting, and bitterness and sarcasm tainted his written response. Boswell was becoming entrenched in a line of defence based on detailed graptolite stratigraphy. He announced that he had succeeded in collecting numerous graptolites from horizons inferred by Jones to be slumped, and that the sequence was complete and intact overall. To Jones, himself a graptolite worker of renown, the resolution of the graptolite zonation was insufficiently fine for the stratigraphical arguments to carry weight in the present argument. He believed that the observed structures and lithologies held the key. Boswell, on the other hand, appeared convinced that the graptolite succession was no problem for his own structural interpretations but undermined the idea of slumping. Indeed, he persevered for much of the rest of his life with his studies on the graptolite zones of the region. According to Alan Wood (personal communication 1987), Boswell felt he had been driven into a corner, and he was taking the dispute very personally. He fervently hoped that something would eventually emerge from his toils with the graptolites that would prove Jones wrong.

The strain on Boswell began to show. In 1938 he resigned his position at Imperial College, because of poor health. A few years after that he retired to a somewhat reclusive life in North Wales. His scientific interests remained, however, and these were to see a series of ironic twists. He had had a long-standing interest in sediment mechanics; now

Jones was publishing a paper on how muddy sediments consolidated (Jones 1939b). Perhaps this once again seemed to Boswell like Jones entering his own time-honoured territory. Jones, with a series of simplifying assumptions and a complete neglect of diagenesis, argued that consolidation would typically take about 5000 years. With this in mind for the Denbigh Moors deposits, 'it is not surprising that these rocks could take on and retain the remarkable and beautiful structures which they acquired in the process of sliding on the sea-floor' (Jones 1939b). Boswell would soon attempt to refute this line of approach in an expanded effort on sediment mechanics, but for the moment the battle remained centred on the field evidence. It was at Jones's critical upper contacts that Boswell had to aim, and he began by listing 22 localities where contacts between disturbed and undisturbed material could be seen (Boswell and Double 1940). In these, the passing of jointing across the junctions without modification was felt to 'furnish the best evidence in favour of contemporaneous movement (slumping) of the deposits, but this "most favourable evidence" is only visible at four of the localities'. Hence, it followed that slumping was only of local significance. Boswell was not prepared to accept Jones' upper contact argument. 'The evidence for contemporaneous sliding of sediments should be unequivocal if deductions relating to a large area or great thickness of deposits are made from it. The gradual passage from disturbed deposits up into normal beds is not unequivocal evidence. Only if the contact is a plane of erosion and the graptolite faunas immediately above and below the disturbed material are identical, is the evidence irrefragable. Such requirements are rarely fulfilled'.

By 1943, Boswell believed that most of the structures 'exemplify the effects of earth movements' (Boswell 1943) but did allow small-scale sliding at a few localities, affecting horizons up to about 5 m in thickness. Any hint in this of a growing compromise, or of the disagreement settling into a stable equilibrium, were dashed by Boswell's charge that the 'disturbed beds cannot be mapped as a unit' and that 'Professor Jones has included many outcrops of "normal" cleaved rocks in his disturbed beds'. Jones replied briefly but biting. 'I am grateful to find that Professor Boswell adduces some evidence that the disturbances were contemporaneous'. However, 'his conversion to the view is still only partial' and he 'betrays some confusion' about the evidence, particularly that 'crucial test of

the date of origin of the "disturbed beds", the upper contacts'. The reasons why the upper boundaries had to be depositional were recounted by Jones, before he launched into comments of a more personal nature regarding the alleged omission of disturbed beds from his map. 'It is not without an element of humour that Professor Boswell charges me with having missed some slumped beds' (Jones 1943). 'After such a promising start it is disappointing to find that Professor Boswell has recorded a beautiful series of slumped beds' that occurred nearby, 'as if they were normal beds'.

Boswell had by now, after 20 years of study, reported on virtually the entire Denbigh Moors area. It remained for him to synthesize the information (Boswell 1949). He produced a mainly detailed descriptive account, but reviewed and discussed the evidence regarding the disturbed beds. Although he accepted slumping in certain situations, such as where normal bedding can be seen passing into gentle folds, and through overfolds and recumbent folds into 'a tumbled unstratified mass', in other cases 'the evidence as to the origin is by no means always so clear'. Many folds show a consistent appearance throughout a supposed slide sheet, the self-same characteristics as folds in known tectonic belts. The lack of fractures at fold crests, Boswell argued, implies low deformation rates incompatible with sliding. Some fractures in disturbed beds were filled with quartz, which seemed to imply advanced lithification. Concretions must have formed and hardened before disturbance, because they now lay in a variety of orientations. Disoriented, jointed and allegedly cleaved fragments within disturbed material implied post-cleavage disturbance, Boswell argued. He appreciated that particular difficulties of recognition arise where 'both contemporaneous and subsequent movements have operated' in the same place, and also because it is possible that 'the earth-movements took place at no distant date after the deposition of the sediments, that is, at a time when the shaley or clayey beds were still only partly consolidated and in a plastic state'. He envisaged a situation where sediments at depth were undergoing tectonic deformation, with their varying degrees of lithification influencing the nature of the junctions between disturbed and undisturbed units, and the same movements triggering some local sliding at the sea floor. To Boswell, 'the crux of the question is, not whether the disturbed deposits are all due to contemporaneous or to subsequent movement, but

which of them are the result of one or other, or both, causes'. Although Boswell was failing to envision the vast scale and frequency now documented for the submarine sliding process, this general scenario is close to that visualized today for many sedimentary piles.

Meanwhile, the skirmishing between the two adversaries had switched back to the related field of sediment mechanics. Jones (1944) expanded his earlier account of sediment consolidation; Boswell responded with a series of papers on sediment thixotropy, eventually to be synthesized in his book '*Muddy Sediments*' (Boswell 1961a). Discussing the extent to which clays might produce, for example, landslides, Boswell taunted that 'even a slight acquaintance with the physical properties and behaviour of sediments should give us pause before we theorize about, for example, their rate of compaction and their ability to slide under their own weight'.

By 1953, Boswell felt it appropriate to use his continuing graptolite work to launch yet another salvo on the origin of the Denbigh Moors structures. In his article entitled 'The alleged subaqueous sliding of large sheets of sediment in the Silurian rocks of North Wales', he claimed that although disturbances in many regions were now being attributed to subaqueous sliding, invariably 'no indisputable evidence is provided' (Boswell 1953). None of the intricate schemes of graptolite zonation that he had assembled, and none of his detailed maps of graptolite distributions even hinted at such vast disturbances. These data demanded, according to Boswell, either abandoning belief in graptolites as time indicators or in the reality of Jones's major slump sheets. Jones fired up his sarcasm in reply. 'Professor Boswell alleges "no indisputable evidence" that... the disturbances are due to subaqueous sliding. It is not quite clear whether he disbelieves that they are subaqueous or are due to sliding, or both' (Jones 1953). 'The evidence is as conclusive as any geological evidence can be. It has been stated before but apparently it needs to be repeated'. In summary, Jones's evidence was that there were numerous clear exposures of disturbed horizons sharply overlying lithologically similar, normal sediments, but with pieces of the underlying material detached, disoriented, and 'sometimes much crumpled'. The overlying normal sediments commonly had graded bases, which adhered closely to the irregularities in the upper surface of the disturbed bed. 'It is certain that there has been no sliding on that surface

since it formed – it is an original undisturbed contact'. Pieces containing the contact could be hammered off. The graptolite zonation was irrelevant.

Boswell's next published contribution concerned itself mainly with a theoretical analysis of compaction and porosity loss in clayey sediments. It is a remarkably similar treatment, given the circumstances, to Jones's 1944 paper on the compaction of muddy sediments. Jones's article was not, however, referred to by Boswell. Perhaps it is worth reminding ourselves at this point that this protracted and spiteful quarrel was not between two ambitious geological upstarts, but two worldly gentlemen who had already reached the highest academic pinnacles their country had to offer! However, by now, the Denbigh Moors structures seem to have been just about wrung dry of argument. After all, it had been going on for 15 years. Despite its bitterness, it had raised numerous useful points, many of equal relevance today.

Unfortunately, the antagonism did not end here; it expanded into the use of graptolites, and to field-mapping techniques. At one point, Jones (1955) reviewed once more the overall Lower Palaeozoic evolution of Wales, citing, incidentally, 10 of his own papers and none of Boswell's, but making much use of isopachytes – the device which, as mentioned earlier, Boswell had first introduced to English usage! Jones's synthesis was couched in terms of the accepted model of the time, the 'Welsh Geosyncline'. Boswell's next article was entitled 'The case against a Lower Palaeozoic geosyncline in Wales' (Boswell 1961b)! In this he argued that the area was really a complex system of connecting depressions rather than one enormous trench, and would be better referred to as a 'Welsh Basin'. It was to be his last publication. After gradually failing health, Boswell died in 1960 in North Wales, not far from the area that had proved his geological nemesis. By now, the idea of widespread slumping accounting for the structures in the Denbigh Moors was widely accepted, and Boswell had become virtually isolated. Jones's interpretation was later further vindicated by the systematic British Geological Survey investigations of the region (Warren *et al.* 1984). However, as a final irony, when Jones died in 1967, plate tectonics ideas were just being applied to Wales. This new thinking heralded the demise of the notion of a Welsh Geosyncline, which Jones had long championed – and the swift acceptance of the idea of a 'Welsh Basin'.