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Wignall, P.B. and Best, J.L. (2002) Reply to: The Western Irish Namurian Basin reassessed - a discussion by O.J. Martinsen & J.D. Collinson, Basin Research (2002), 14, 523-542. Basin Research, 14 (4). pp. 531-542. ISSN 0950-091X their model (Wignall & Best, 2000; fig. 14(c), right-hand side) also shows a thickened Ross Formation-Tullig Cyclothem succession in North Clare. In fact, the succession here is some 300 m thick, compared to 1200 m in the south (Fig. 2).

SUMMARY

Whilst discussion and modification of earlier interpretations are important in advancing understanding, we feel that Wignall & Best's (2000) reassessment of the basin model for the Western Irish Namurian Basin has been based on consideration of an incomplete and biased data set. In our view, the previously published model by Collinson *et al*. (1991) takes more fully into account the totality of the regional data. It also consistently explains regional thickness trends of several detached depositional systems through decaying differential subsidence above the Iapetus Suture along the Shannon Estuary and gradual infill of the basin with onlap towards the north, without the need for any dramatic or unreasonable tectonic movements. The structures related to the Iapetus Suture below the Shannon Estuary are documented on deep seismic and accord well with thickness trends (Figs 2 and 4) and other geological data (see above). In addition, the style of basin fill compares in many respects with basins in Northern England where subsidence also decayed through the Namurian following Dinantian extension, i.e. turbidites in deep basins were overlain by slope and deltaic successions (Gawthorpe, 1987; Besly & Kelling, 1988; and references therein; Collinson, 1988).

Whilst we welcome new interpretations, which may inspire us and colleagues to rethink former and present interpretations of the Western Irish Namurian Basin, we feel that such new models should honour all the data available or, best of all, present new and previously unpublished observations.

ACKNOWLEDGEMENT

Trevor Elliott reviewed the manuscript and made valuable suggestions towards its improvement.

REPLY

Paul Wignall and James Best
Department of Earth Sciences, University of Leeds, Leeds, UK, E-mail: p.wignall@earth.leeds.ac.uk

We are pleased that our alternative model for the Western Irish Namurian Basin (WINB) has prompted a debate from Martinsen and Collinson since, as they and many others have stated in the past, this basin has significance beyond its regional setting. However, it is unfortunate that they appear to have given our paper only a cursory inspection with the result that they attack claims that we never made and do not address the full spectrum of arguments in favour of our model. We certainly agree that 'in order to command respect, new interpretations should be based on a re-assessment of *all* available data', but find it ironic that they then only focus on one part of the Namurian basin fill, the slope system recorded by the Gull Island Formation, and also choose to ignore datasets presented in past work. Our revised model sought to include considerations for the *entire* basin fill, a key aspect that their discussion neglects. Martinsen and Collinson also document the nature of Dinantian carbonate deposition in the region because, they erroneously claim, our model requires 'a vast amount of subsidence must have taken place in northern Clare (in the late Dinantian), while the Shannon Estuary region must have suffered uplift'. We never postulated such nonsense, and clearly stated in the introduction to our paper (and elsewhere) that 'Lateral facies changes in the uppermost Visean (Dinantian) sediments indicate that a southerly dipping ramp was present in County Clare with the deepest water conditions occurring in the Shannon Trough, an ENE–WSW-orientated depositional axis centred on the present-day Shannon Estuary' (Wignall & Best, 2000; p. 60). Martinsen and Collinson's spurious parody is based on the notion that palaeobathymetry is only controlled by subsidence rates. They take no account of sedimentation and so fail to appreciate that water depths in even the most rapidly subsiding basin can remain shallow given sufficient sediment influx. In constructing our revised model, we sourced and used all the *published* data and papers that have been peer-reviewed in journals, and hence believe Martinsen and Collinson's criticism is misplaced. We also suggest that a full and detailed reading of papers, which they have evidently not accomplished of our paper, is essential to driving scientific debate forward. Intemperate and unfounded comments based on an incomplete representation of what is published is, we feel, not a desirable way to develop thought. Below we will address the key areas raised by Martinsen and Collinson, referring to their discussion and the diagrams they use that are taken from Martinsen *et al*. (2000). In our further pursuit of using all available data, we also will incorporate key new work on the area published by Elliott (2000), Martinsen *et al*. (2000) and Strachan (2002).

AGGRADATION VERSUS PROGRADATION

It should be stressed, at the outset, that a fundamental error pervades the arguments of Martinsen and Collinson: that the greatest sediment thickness/site of maximum subsidence *always* equates with the site of deepest water within a basin. This assumption is true if sediment accumulation is purely aggradational, but it is a highly unlikely circumstance for the WINB fill. That it is possible for the advance of a prograding system to be retarded at sites of high subsidence has happened countless times in the geological record (one need only examine contemporaneous basins in northern England for further examples (e.g. Rippon, 1996)) and is also readily demonstrated by recent basin modelling experiments. For example, the

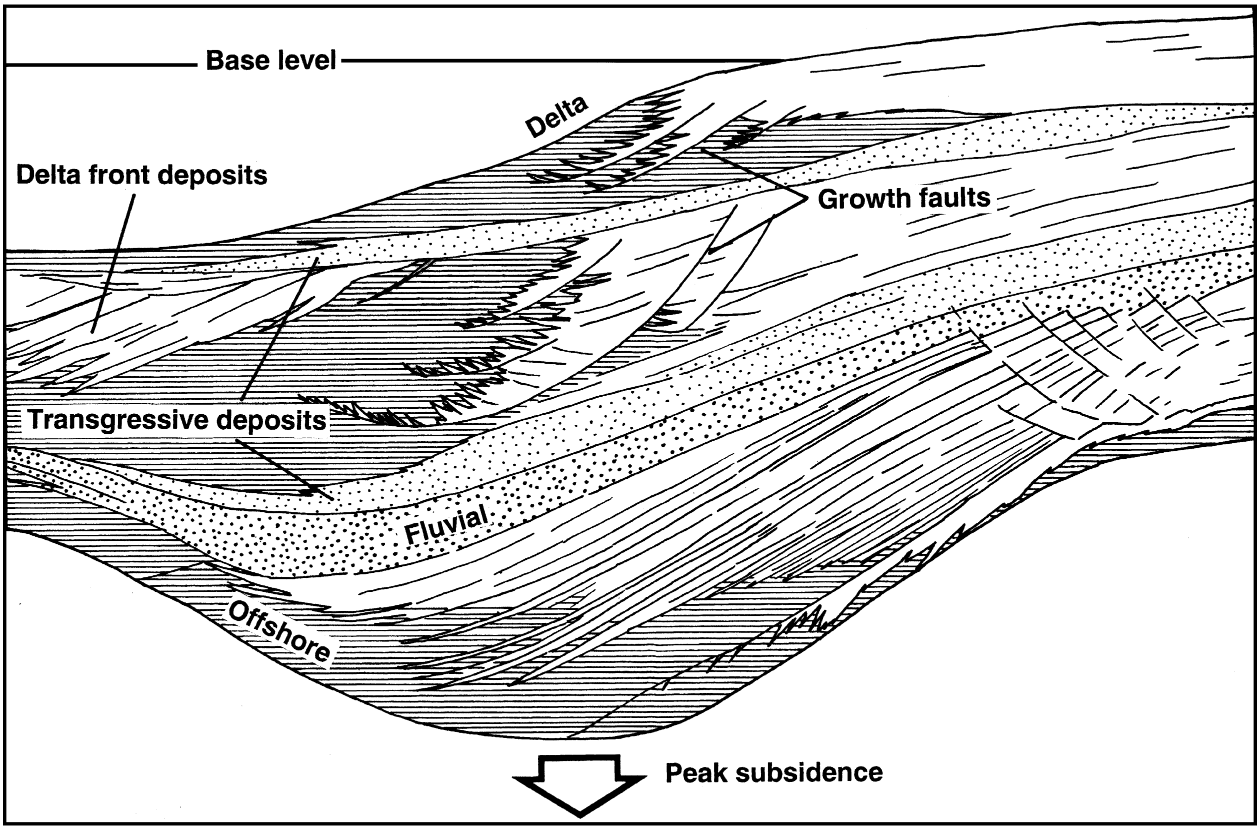

Fig. 6. Flow parallel cross section through a basinal stratigraphy modelled in a large experimental basin (EarthScape (XES) Basin) in which peak subsidence rates occurred in the centre and decreased uniformly to the margins. Sediment was fed into the basin from the right hand edge and two high amplitude base-level changes were imposed causing cycles of progradation and retrogradation. Redrawn from Paola *et al.* (2001; fig. 5).

sophisticated physical experimental modelling of Paola's University of Minnesota group has come close to reproducing our view of the WINB infill (Paola *et al.*, 2001). Figure 6 shows one of their examples of a progradation-dominated system that, even in its detail such as the abundance of growth faults, bears strong comparison with the WINB. In this example, sediment is supplied from the right hand margin of the experimental basin and flow is consistently to the left even though the thickest pile of sediment accumulates in the centre where subsidence rates are highest. Thus, the left-hand side of the tank was consistently 'downslope' during this experiment and a distal, relatively thin succession accumulated there. The Namurian succession in northern County Clare is thus equivalent to the left-hand side of the experimental basin in our model and we would draw Martinsen and Collinson's attention to this simple, but central point.

THE NEW MODEL

In order to clarify the debate, and correct the sophistry of Martinsen and Collinson, we have sought to more clearly summarize our basin model in Fig. 7 and represent the evolution of the basin at five key stages.

Stage 1: Prior to the Namurian, facies evidence clearly indicates that a southerly dipping carbonate depositional ramp was present in the County Clare region, as we clearly noted in our paper (although Martinsen and Collinson fail to recognize this point).

Stage 2: Carbonate deposition ceased at the end of the Dinantian and in the deeper areas of the basin, in the Shannon Estuary, carbonate sediments pass conformably upwards into black shales of the Clare Shale Formation. This carbonate-to-black-shale transition is widespread throughout northern Europe at this time suggesting an extrinsic control on the change in depositional style, perhaps a climatic, eustatic or a regional tectonic event or a combination of several such events. At this stage, water depths in northern County Clare are envisaged to have been shallower than in the south (as they were in the preceding Dinantian) with only a highly condensed veneer of phosphatic pebbles accumulating.

Martinsen and Collinson wrongly claim that 'Wignall & Best (2000) argue that the presence of phosphates at the base of the Clare Shales in North Clare is evidence for deep-water conditions there.' In fact we first carefully documented and illustrated new data on the phosphatic facies (Wignall & Best, 2000; pp. 63–64) and then concluded that the basal phosphate bed records 'both shallow-water ... and deep-water conditions in its prolonged depositional history. . .which spans an interval of substantial deepening' (Wignall & Best, 2000; p.73). Martinsen and Collinson chose to infer absolute water depths for phosphate accumulation using the suggested range of figures proposed by Tucker (1981). We consider it better to base basin history on facies analysis and evidence from the field rather than rely on the generalizations of an introductory textbook. More relevant reviews include the work of Jarvis *et al.* (1994) and Föllmi (1996) who highlight the frequent association of phosphogenesis with intervals of base-level rise and sediment starvation. These were probably the two key factors in the generation of the County Clare phosphates.

It is also worthy of note that the southern edge of the basin is far more poorly exposed that the northern margin, but that the Clare Shales do thin to the south. Such thinning of strata to the south should be expected away from the location of maximum differential subsidence (the

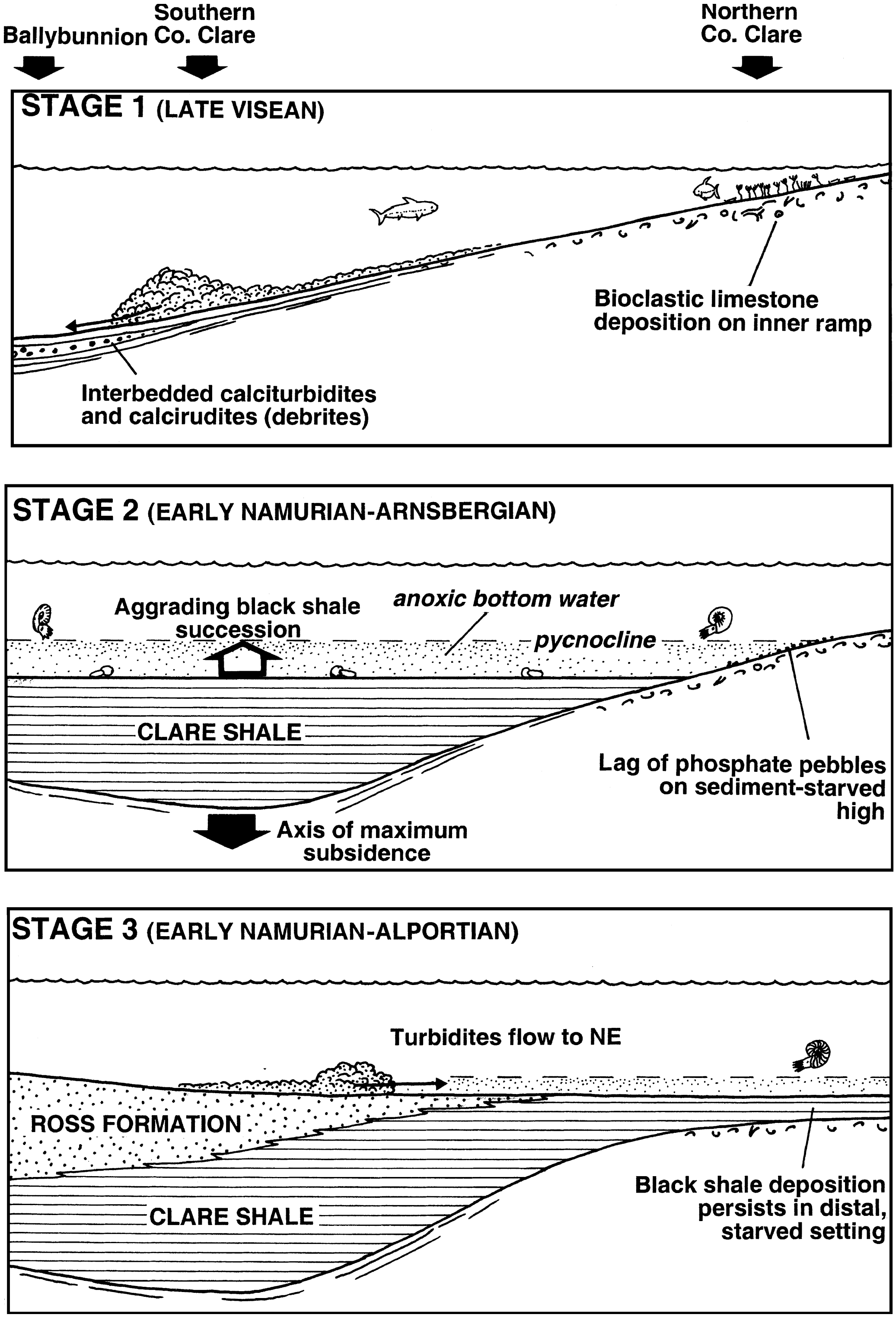

Fig. 7. (cont.)

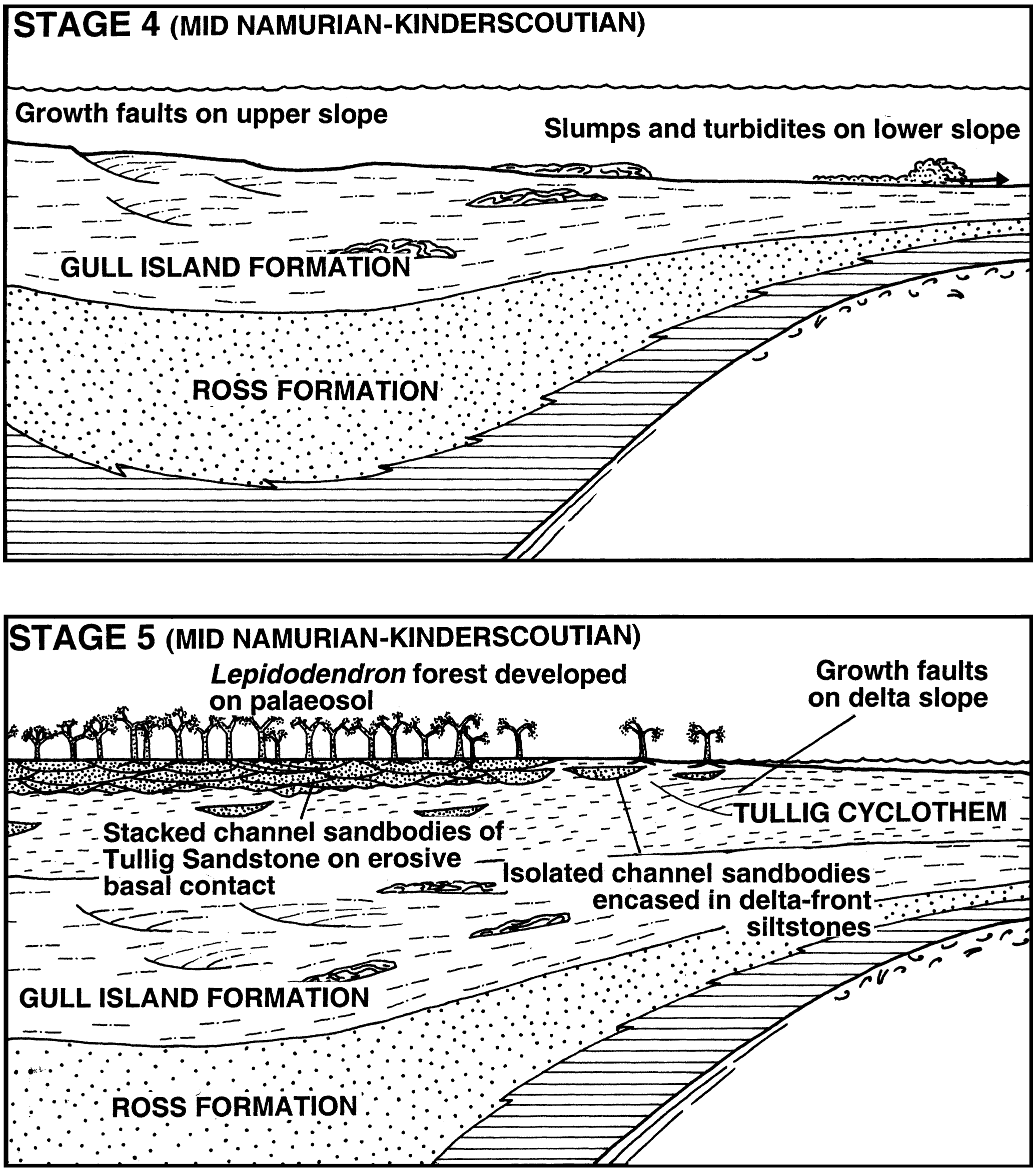

Fig. 7. Our model for the evolution of the WINB beginning with the Late Dinantian ramp topography (Stage 1), followed by shutdown of carbonate productivity and cessation of deposition in the north of County Clare with the result that this area becomes progressively deeper during the early Namurian (Stage 2). This accommodation space initially remains unfilled as a north-easterly prograding turbidite system is ponded in the Shannon Estuary region due to the high subsidence rates in this area (Stage 3). The basin-floor turbidite system is replaced by a prograding slope system prone to collapse (Stage 4). Only with the arrival of a delta system is sediment accumulation able to build up to base level (Stage 5) and persistent emergence is seen in the more southerly outcrops.

Shannon Estuary), but this point does not invalidate our model. Additionally, it should also be noted that the North Cork section shown by Martinsen and Collinson (their fig. 2) does in fact lie to the south-east of the line of section drawn in County Clare and is normal to the propagation direction of the Ross turbidites. The sections in North Cork probably lie towards the eastern edge of the basin. The 'southern' edge of the basin, along the main line of section they draw through County Clare, in fact lies along the Cork coastline where only the Devonian outcrops and the Namurian has been removed. Thus, Martinsen and Collinson's assertion, that the North Cork sections represent the 'southern' basin margin, is tenuous at best.

Stage 3: Deposition was principally aggradational during stage 2 of the WINB's evolution (we agree with Martinsen and Collinson on this point), and only with the arrival of turbidites to the south of the Shannon Estuary (Wignall & Best, 2000; fig. 7) does the progradational phase of basin infill begin. These initially downlap the Clare Shales accumulating to the south of the Shannon estuary (we fail to see why Martinsen and Collinson consider this relationship unsupportable in our model). Due to the continuing high subsidence rates in the Shannon Estuary area, much of the turbidite infill is ponded in this area. The accumulation of hundreds of metres of turbidite sandstones is sufficient to decrease water depth in the region to less than that in northern County Clare where the phosphatic pebble lag is overlain by a condensed succession of black shales. The evidence for this downslope direction comes from palaeocurrent vectors in the Ross Formation which are overwhelmingly to the NE (a fact not in dispute and not challenged by Martinsen and Collinson). To emphasize the point, the reason for greater water depths in northern County Clare is because the area was subsiding from the Brigantian to Chokerian Stages, an interval of probably several million years, but no sediment was accumulating (Wignall & Best, 2000; p. 73). We do not suggest 'a vast amount of subsidence must have taken place in North Clare' but rather only modest subsidence over a prolonged period of time, a point clearly made on p. 73 of our paper but selectively ignored by Martinsen and Collinson. We note that recent work on the Ross Formation has also confirmed the dominance of NE palaeocurrents. For instance, Elliott (2000) provides the first detailed work documenting channels in the Ross and documents mean flow directions to 050°, to the N–NNE and 050° at Kilbaha, Rehy Cliff and the Bridges of Ross, respectively. Martinsen *et al.* (2000) also find a mean flow direction of 042° for the entire Ross Formation, including flows to the NW in turbidite channels (Martinsen *et al.*, 2000; p. 540). We find it difficult to reconcile these palaeocurrents with the NE–SW slope orientation proposed by Martinsen and Collinson: they state the basin axis is ENE–WSW trending and their past work suggested that the trough in which the turbidites accumulated may only be 15–20 km wide (Martinsen *et al.*, 2000; fig. 9). No evidence for reflected flows is found within the Ross Formation (Elliott, 2000; p. 345) and hence we are required to envisage a turbidite system being fed from the N/NW entering a trough that is 20 km wide and that nearly all the flows were then turned to flow towards a mean flow direction of the NE (i.e. oblique to the basin orientation as suggested by Martinsen and Collinson). The channels feeding that system are also orientated to the NE with some being directed back towards the source envisaged in Martinsen and Collinson's model, with no evidence for reflection being present for any of these flows. It appears to us that this series of suppositions present in their model are unsustainable when viewed in context of the controlling physical processes. A much simpler, and more appealing solution is to view the Ross Formation turbidites feeding sediment, via a series of channels, out to the NE into a more distal location.

Stage 4: The basin-floor turbidite system was replaced by a siltstone-dominated slope system of the Gull Island Formation that prograded to the NE (see discussion below).

Stage 5: The Tullig delta system progrades broadly from south to north (i.e. downslope) as indicated by abundant palaeocurrent evidence (Rider, 1974; Pulham, 1989). For instance, Pulham (1989) provides a large database on palaeocurrent trends including 368 palaeocurrents from the fluvial Tullig sandstone. These produce a mean flow direction of 040° and back up Rider's previous model of flow to the N/NE. We would suggest that Martinsen and Collinson should refer to this data also in their discussion rather than ignoring this very large and reliable database. At the peak progradation of the system, mature palaeosols were developed in the top bed of the Tullig Sandstone, which displays the large root systems of club mosses (Fig. 8a and b). However, these features are not seen in the northern developments of the Tullig Cyclothem where only transient club moss growth is indicated by the presence of root systems interbedded with wave-rippled sandstones – a 'mangrove swamp' type facies (Fig. 9). No mature soil horizons are seen in the northern outcrops of the Tullig Cyclothem because, in our model, this more distal, delta plain development was not prone to the prolonged subaerial exposure recorded in more southerly outcrops. Martinsen and Collinson's claim that emergent conditions are only seen in the Tullig Cyclothem in northern County Clare is plainly erroneous.

So why did we propose a revised model for the WINB? The principal answer is that nearly all the published evidence for flow directions/slope orientations in County Clare indicates progradation to the NE (Rider, 1974; Pulham, 1989). Martinsen and Collinson choose not to challenge this evidence, neither do they provide an alternative explanation, rather they focus on the Gull Island Formation which contains flow vectors from turbidites and slope indicators from slumps and growth faults. The turbidite evidence is equivocal for both our model and the old model. Palaeocurrents are highly variable but indicate a considerable spread from NNW to SE as shown in their new data, our data (Wignall & Best, 2000; fig. 9) and their previously published data (Collinson *et al.*, 1991; figs 8 and 10). We suggested that this flow variability, the greatest for any unit in the WINB infill, may be due to interaction of turbidity flows with a complex seafloor topography caused by the presence of numerous slumps (Wignall & Best, 2000; pp. 67–68): a suggestion that Martinsen and Collinson chose to ignore. However, the main flow direction of the turbidites was to ~045° (Elliott, 2000; Martinsen *et al.*, 2000), in accord with slope indicators in the remainder of the WINB fill.

The evidence for Martinsen and Collinson's favoured model principally rests on their interpretation of the downslope movement direction of the slumps of the Gull Island Formation, which they have consistently stated to be towards the SE (Collinson *et al.*, 1991; figs 8 and 10). The 'evidence' consists of arrows marked against logs in their earlier publications and the stereonet plots presented in their discussion. We are not told where the new data were obtained despite the fact that they state 'well-documented localities' hold the key to interpreting the basin history. Thus, they berate us because 'Outstanding key localities, which have significant evidence that opposes their (i.e. our) model, are not used.' Not wanting to make this mistake twice, we here look at two 'key localities', the large slump at Fisherstreet Bay in the north of County Clare and the large growth fault/slump structure at the 'Point of Relief' in the south of the county. These sections

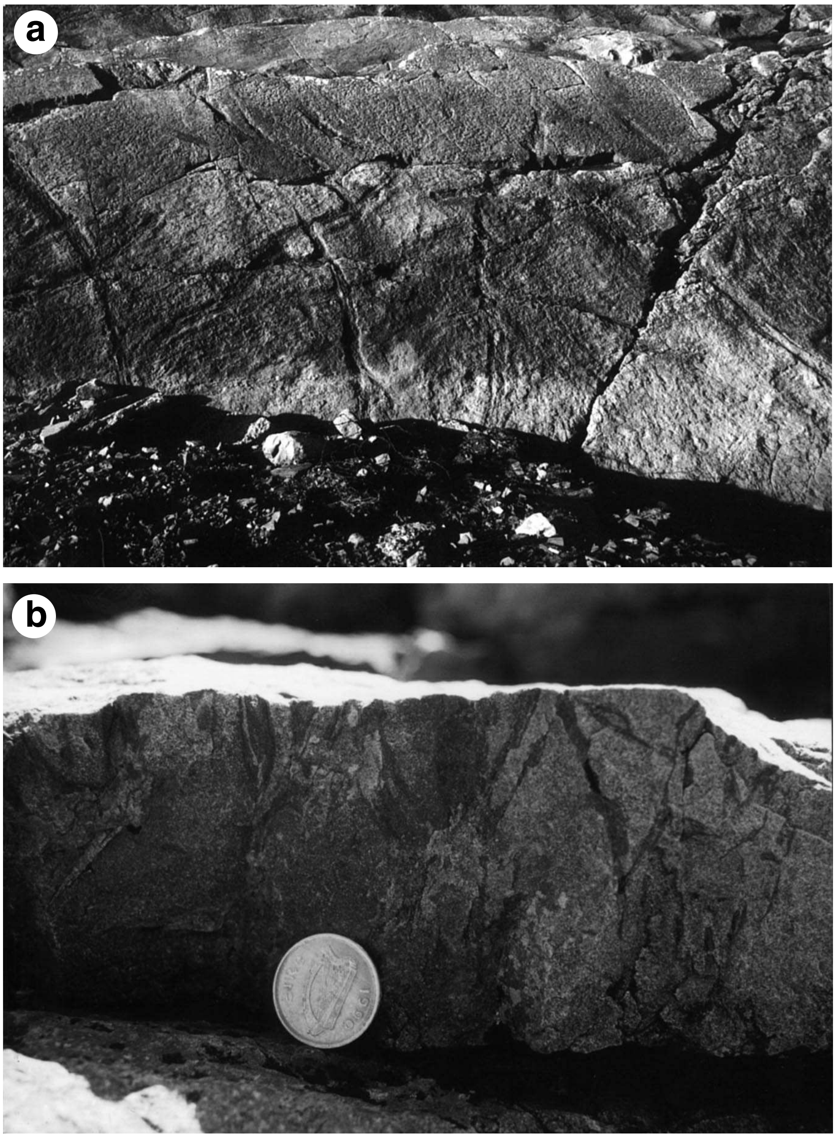

Fig. 8. (a) Large bedding plane view, 25 m wide, of the top surface of the Tullig Sandstone at Killard (Grid ref. 952678) showing large, straight to gently sinuous stigmarian roots approaching 10 m in length. (b) Profile through the same palaeosol horizon seen in (a) showing a leached, quartz arenitic soil horizon (ganister) penetrated by numerous rootlets. This horizon records an *in situ* development of a *Lepidodendron* forest in a well-drained delta top setting.

are the most extensively illustrated and discussed of the Gull Island Formation (Gill, 1979; Martinsen, 1989; Martinsen & Bakken, 1990; Collinson *et al.*, 1991) and are therefore 'key localities' by anyone's criteria. They record some of the most unambiguous indicators of propagation direction from large-scale soft-sediment deformation events. Recumbent folding and syn-sedimentary faulting in the Fisherstreet slump is illustrated in both Martinsen & Bakken (1990; fig. 3) and Collinson *et al.* (1991; fig. 7). In both figure captions, movement direction is interpreted: 'Inferred downslope direction is to the left' and 'Transport direction is to the left', respectively. In neither case is the 'left' direction specified but, as anyone who visits this splendid Fisherstreet location will appreciate, the photographs were taken looking towards the SE with the result that 'left' is towards the NE, our preferred downslope direction. This movement direction is also supported by Martinsen *et al.* (2000).

At the 'Point of Relief' large growth faults downthrow to the NW and slumps developed on hanging walls also clearly indicate movement to the NW (Fig. 10a and b). This downslope interpretation is implicitly acknowledged in Collinson *et al.*'s (1991; fig. 6) figure caption of this location where they note 'The (deformation) complex is cut by major syn-depositional faults, dipping to the left (the photograph was taken looking towards the NE)'. It is also of interest that Martinsen & Bakken (1990; p. 159), in their excellent documentation of the Point of Relief, note that the slumps associated with the growth faults (see Fig. 10b) may have occupied these positions due to the 'residual depression in front of the western fault'. They then continue: 'it is possible that this slump also utilized

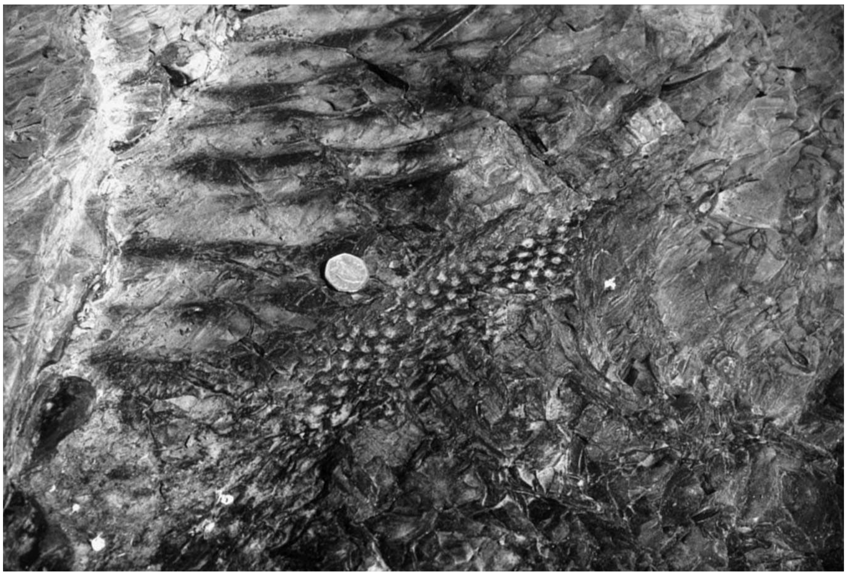

Fig. 9. Stigmarian root with side rootlets attached, indicating the *in situ* growth of *Lepidodendron* on a wave-rippled sandstone from the upper part of the Tullig Cyclothem at Furcera Bay, Liscannor, County Clare (Grid ref. 038882). The roots occur at several horizons in over a metre of strata and are interpreted as transient colonization in a swampy delta-front setting ('mangrove' facies).

the fault hollow as a *transport path*' (our italics), thus acknowledging that the slump moved towards the west into the depression in front of the growth fault, a geometry rather difficult to equate with a slope that was prograding to the SE in the opposite direction! It is also worthy of note that Fig. 10b shows the difficulty of interpreting slope direction from smaller scale folds and slumps, since the internal dynamics of these features may produce very complex patterns. The slumps present at the Point of Relief show a range of fold axes and internal thrusts and demonstrate the clear need to both declare and assess the scale of the structures that orientations are measured from, which Martinsen and Collinson do not provide for their dataset. In fact the data in their fig. 5 could just as easily be used to support our model. The majority of palaeocurrents have a NE vector, most normal faults dip to the NE and only the thrusts and faults suggest movement to the SE but the interpretation of movement directions from such structures, particularly if small scale, is difficult. More reliable movement indicators are provided by sole lineations but these are not given by Martinsen and Collinson. However, in a recent study of the large slump horizon exposed at the Bridge of Ross, Strachan (2002) published detailed data on a range of fold hinge orientations and sole lineations, from which she inferred a downslope direction of 040°. We feel that an equally detailed, rigorous study of some Gull Island deformation horizons is clearly required. Martinsen and Collinson have been claiming, for over a decade (e.g. Collinson *et al.*, 1991), that this information is present in Martinsen's unpublished thesis and has been 'built into the Collinson *et al.* (1991) model', but it is time that this data was fully presented.

WHY A REVISED MODEL IS NEEDED

Since the publication of our paper, the old model has been reiterated and modified by Martinsen *et al.* (2000). Previously, Collinson *et al.* (1991) considered that the lower part of the basin infill (i.e. the Ross and Gull Island Formations) consists of turbidite and slope deposits filling an elongate depression along the axis of the Shannon Estuary. Subsequently, Martinsen *et al.* (2000) suggested that this elongate depocentre was filled by the end of Ross deposition so that turbidity currents were able to reach the northern margin of the basin during early Gull Island Formation deposition (Fig. 11). However, 'these conditions probably only existed for a relatively short period of time before the south-eastwards advancing basin slope prograded across the basin floor setting and the filled trough' (Martinsen *et al.*, 2000; p. 546). Their modified model is summarized in three diagrams (Martinsen *et al.*, 2000; reproduced here) which conveniently encapsulate the problems with each stage of the basin evolution in the old model, and also highlights many inconsistencies in the views of Martinsen and Collinson.

Stage A: The Ross turbidite system is confined to a narrow axis in which flows were to the east/east-north-east, along the Shannon axis. However, as all the available data (e.g. Martinsen *et al.*, 2000; fig. 8) shows palaeocurrents are to the NE, this is an obliquely upslope direction towards the basin margin in their model. Martinsen *et al.* (2000) show a vector mean of 042° for the entire Ross Formation, with some currents going to both south-east and also north and north-west (i.e. back towards the source region in their basin model). Clearly, this inconsistency undermines their model but they 'avoid' the problem by suggesting several orientations for the basin axis. Thus, they state here and elsewhere that there was 'a deep central trough aligned along the Shannon Estuary' the locus of thickest sediment accumulation; a cursory glance at a map shows this orientation to be E–W. However, when discussing palaeocurrents they are forced to note 'currents were flowing dominantly towards the north-east, that is, parallel with the general trend of the inferred basin axis' (Collinson *et al.*, 1991; p. 234), and thus rotate their basin through ~30–45°. Unsurprisingly, Martinsen and Collinson have

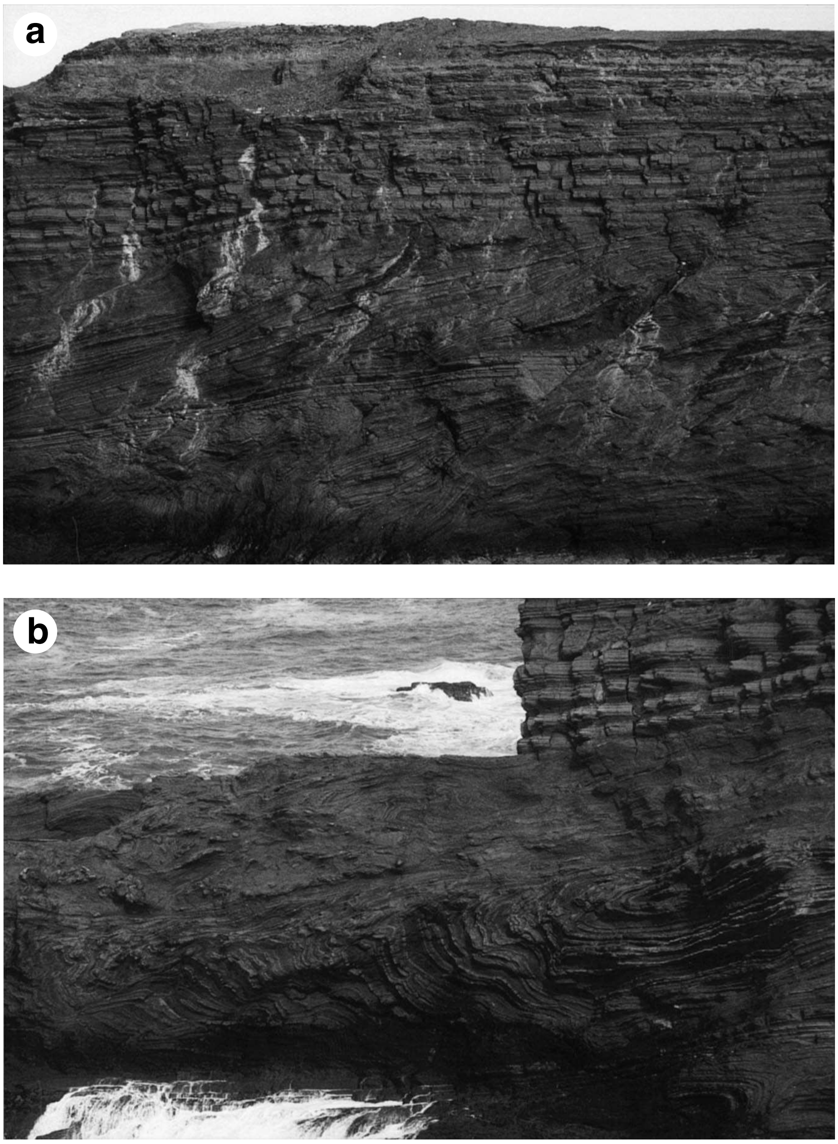

Fig. 10. (a) Large syn-sedimentary fault in the upper-most part of the Gull Island Formation at the Point of Relief in southern County Clare. A normal fault in the centre-right of the field of view downthrows to the W (to the left) and the hanging-wall accommodation space is partly infilled with a structureless lens of dark siltstone which is overlain by thin-bedded sandstones that prograde to the left. These units are in turn overlain by further siltstones that form the up-dip component of slumps seen in (b). Undisturbed sediments of the basal Tullig Cyclothem occur in the upper third of the cliff. Cliff height is 45 m (b) Western end of the Point of Relief cliff section showing a slump horizon overlain and partly truncated by a second slump horizon. These slumps have moved downslope to the west (left) into a depositional low point generated by the fault seen in (a), however, only the major, recumbent isoclinal folding seen in the lower slump faithfully records this movement direction (i.e. fold closure is to the left). The other more minor folds seen below, above and downslope of the major fold are much more variable and include many examples of upslope fold vergences. This emphasizes the difficulty of ascertaining slump movement directions from small-scale structures. Cliff height at right-hand edge of photograph is approximately 25 m.

never attempted to overlay their basin model on a map of western Ireland.

There are also inconsistencies in the water depths suggested for their supposed northern basin margin. Martinsen *et al.* (2000) infer water depths of 300–500 m (Fig. 11), but this is undoubtedly excessive and it contradicts their 'basin margin' interpretation (Martinsen *et al.*, 2000; p. 544) and also raises the question of when this accommodation space was infilled?

Stage B: Turbidites in the lower Gull Island Formation are now envisaged to flow *parallel* to the base of a prograding slope (why not orthogonal, as during stage A and C?), defined by numerous slumps, that was building to the SE. The 'evidence' for this slope progradation direction is Martinsen and Collinson's inferred slump movement directions but, as we note above, slope collapse was to the NE and NW. Furthermore, the turbidites of the Gull Island Formation occur interbedded with the slumps, not at the

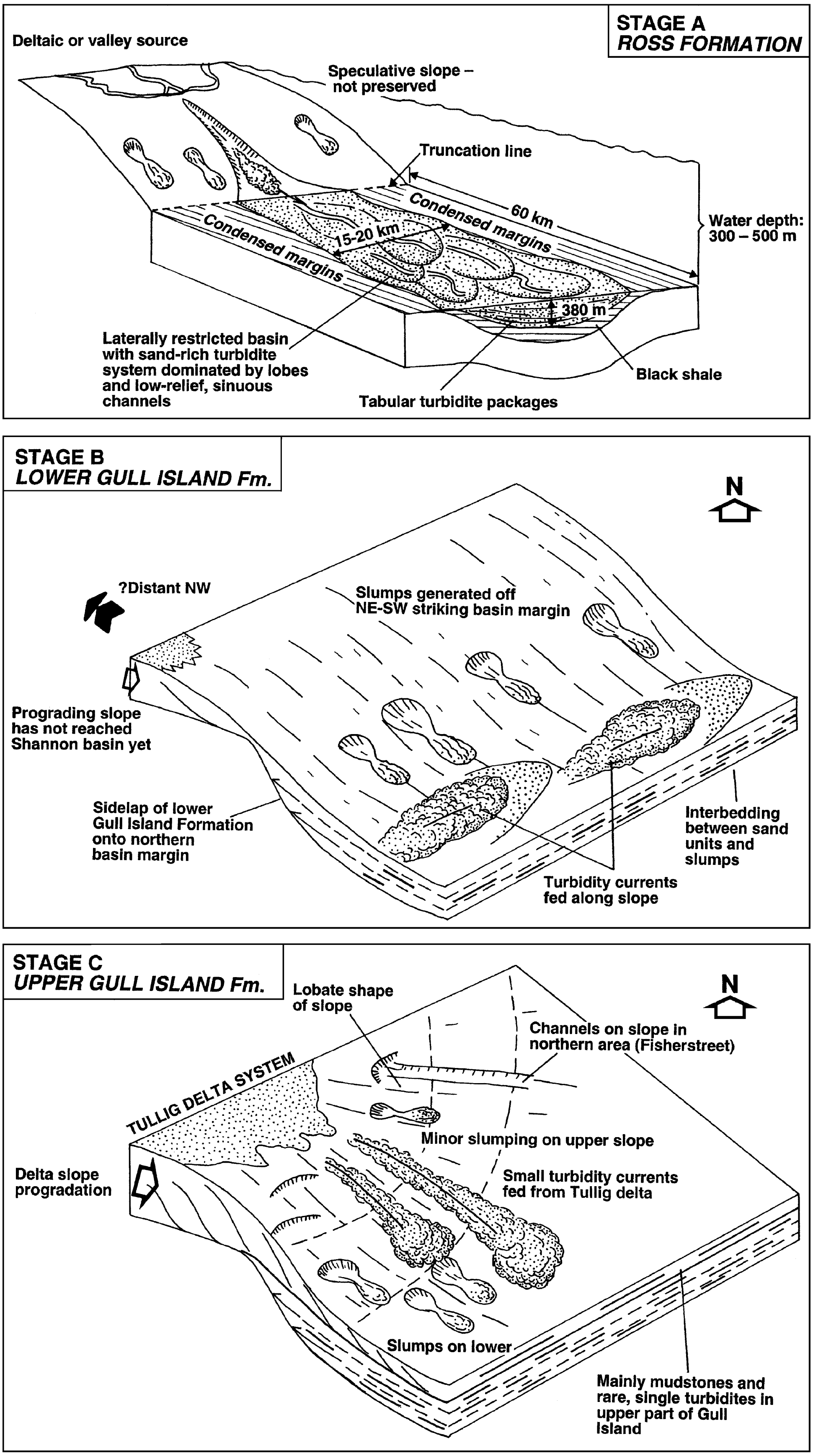

Fig. 11. Three phases in Martinsen *et al.*'s (2000) basin model reproduced here from their fig. 9 (Stage A) and 19 (Stages B and C).

base of the slumped succession, which requires the turbidity flows to have flowed along rather than down a slope; a physical impossibility. This stage of the model also fails to depict the great thickness variations in the Gull Island Formation (Fig. 11).

Stage C: Turbidites swing around once again to flow down a SE prograding basin slope that links updip to the Tullig delta system. Yet again this part of the model is completely at odds with the available data which show that the delta system prograded to the NNE. The evidence includes abundant palaeocurrent information (e.g. Rider, 1974; Pulham, 1989) and evidence for delta slope collapse (e.g. Wignall & Best, 2000; fig. 11). South-easterly flow vectors in the fluvial Tullig sandstone are present at some locations, as suggested by Collinson *et al.* (1991; pp. 235–236), and we have observed SE-directed currents within the Tullig Sandstone at Pulleen Bay. However, these flow directions are by far subordinate to the general NE palaeocurrents documented by Rider (1974), Pulham (1989), Williams & Soek (1993) and Wignall & Best (2000).

Throughout the stages of the Martinsen *et al.* (2000) model, the northern County Clare area is depicted as a basin floor/base of slope location (Fig. 11). This is despite repeated assertions in their text that this is a 'basin margin' area. By Stage C, Fisherstreet is clearly depicted as being a deeper water site than the slope settings of southern County Clare (Fig. 11). Thus, by modifying the diagrams of Collinson *et al.* (1991), Martinsen *et al.* (2000) come very close to mirroring our model for deposition of the upper Gull Island Formation (Wignall & Best, 2000; fig. 10), with only the orientation of the slope differing. Unsurprisingly, they fail to acknowledge this in the accompanying text. However, in their discussion of our paper they appear to return to the model of Collinson *et al.* (1991) when they state: 'We maintain that a shallow, sediment-starved basin margin or intra-basinal high in North Clare better explains the known facts'. They conclude that 'The Wignall & Best (2000) model of a deep basin in northern Clare gets even less credible when the thickness and facies are considered'. If a 'deep basin in northern Clare' is not credible then why do they depict one in Martinsen *et al.* (2000)?

FINAL COMMENTS

We proposed a new model for the WINB and chose to call the old version, 'Hodson's model' because he was the first to document the thickness variations of the Namurian stratigraphy of western Ireland and suggest that they equated with palaeobathymetric variations. We are therefore perplexed when Martinsen and Collinson suggest that we 'do not emphasize that a major conclusion of Hodson & Lewarne (1961) is the existence of a deep central trough'. This is precisely what we emphasized, and suggest that they carefully read pages 60–61 of our paper to see the extensive credit we give to Hodson's outstanding early work. Confusingly, later in their criticism, they appear to take offence at this acknowledgement of Hodson's work: 'Neither Hodson (1954a,b) nor Hodson & Lewarne (1961) created a full model for the entire basin-fill succession so the reference made by Wignall & Best (2000) is imprecise and erroneous.' Obviously, there is no pleasing Martinsen and Collinson, either we have given too much credit to Hodson or we have not, but they cannot have it both ways!

We are also accused of not using the most 'up to date' biostratigraphic information, but they then note that various key marine bands are not known and that this 'introduces uncertainty' in any correlation exercise. We agree entirely that this certainly does introduce uncertainty, and we contend that our correlations are as valid as theirs given the available data. However, they introduce a 'trump card' into their argument by stating that 'careful regional correlation using mouth bar sandstones within the overlying Tullig Cyclothem as a datum supports that the Fisherstreet Slide (is the) stratigraphic equivalent of the Gull Island Formation further south.' Although they provide *no* data, this is probably the first example of the science of 'mouth bar stratigraphy' but we doubt its accuracy.

Martinsen and Collinson also emphasize the widely held view that the Iapetus Suture underlies the Shannon Estuary, although magnetic evidence suggests an alternative location to the south of the Shannon Estuary. Whatever the position, the line of the Iapetus Suture may not have closely controlled the development of the WINB. Thus, in their overview of all the available evidence Sleeman & Pracht (1999; p. 48) state that the basin bounding faults lie well to the south of the proposed trace of the Iapetus suture. Sleeman & Pracht (1999; p. 50) also suggest that the differing dips of the Dinantian and Namurian rocks, as revealed in the Doonbeg borehole may indicate that the axis of the basin could have been different in Namurian times perhaps due to the growing influence of Variscan deformation. These features, together with the suggestion that the NW–SE trending Aran-Waterford line (see Wignall & Best, 2000; fig. 15) may have partially controlled both the extent and evolution of the Shannon trough during the Upper Carboniferous (Sleeman & Pracht, 1999; p. 48), all suggest that the shape, orientation and extent of the Shannon Trough are far more complex than that simplified by Martinsen and Collinson. Sleeman & Pracht (1999; p. 48) even suggest that the basin may have been 'asymmetrical, broadly elliptical rather than elongate in plan (at its eastern end)'.

Finally, it should be emphasized that we do not doubt that subsidence was greater in the Shannon Estuary axis than in northern County Clare during the Carboniferous: what is important is how this affected water depth. Subsidence, sediment supply and proximity to sediment source control this key factor (this should be a truism in any basinal study). In our model, the line of the Shannon Estuary lay closer to the source of the sediment and so sediment ponded there with the result that unrealized accommodation space and deeper water developed to the north. In Martinsen and Collinson's view (expressed in

Collinson *et al.* (1991) and their discussion here, but perhaps not in Martinsen *et al.* (2000)), northern County Clare lay closer to the sediment source with the result that water depths were persistently shallower in this area than in southern County Clare. The two models are readily testable by comparative facies analysis because northern County Clare should either record more distal deposition (in our model) or more proximal deposition (in the old model) than that seen in southern County Clare. In our view facies and palaeocurrent evidence strongly suggests that the northern County Clare sections are the distal equivalents of those seen in the south of the county. Additional evidence could come from provenance studies. As we noted in Wignall & Best (2000) our model predicts that the Namurian sediments were being sourced from a rising Variscan deformation front to the SW of County Clare, the old model derives its sediments from Caledonian terranes to the NW.

In conclusion, we greatly welcome discussion on this fascinating area, which may inspire us and colleagues to rethink former and present interpretations of the WINB. However, we feel that such discussion must be founded on a true and complete depiction of past work, rely on substantiated and published information and critically attempt to explain the *entire* basin history rather than focus on selected formations and ambiguous data.

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