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FLYSCH AND MOLASSE: THE ELUSIVE MODELS. REPLY TO DISCUSSION BY G. HACZEWSKI[1]

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It is difficult to reply to the discussion by G. Haczewski (1988) without repeating many of the arguments in my original paper (Miall, 1984). Haczewski appears to argue that the definition of flysch as a recurrent facies found in orogens, but without reference to tectonic setting or orogenic stage at the time of deposition, is a satisfactory use of the term. He quotes definitions by Hsü (1970), Bates nad Jackson (1980) nad Mitchell and Reading (1978) in support of this view. He is correct that this is one of the three definitions of flysch offered by Bates and Jackson, and one that was omitted in my original article. However, the other two definitions, and those of Hsü (1970) and Mitchell and Reading (1978, and their revised chapter published in 1986) all make reference to tectonic setting.

Hsü (1970, p. 9) offered, as a possible new definition of flysch:

> Flysch, as a term for a recurrent facies, includes marine shales with alternating sandstone and/or some impure limestone layers, which constitute a well bedded sequence in an **alpine-type mountain chain with a tectonic setting**, and sedimentological features similar to the Alpine Flysch in its more typical development (emphasis added).

Mitchell and Reading, in their original definition (1978, p. 445) stated that they "prefer to define flysch indepedently of tectonic setting". However, in the revised version of this article (Mitchell & Reading, 1986, p. 477) they have changed this, as follows:

> We suggest that the word [flysch] be used for any thick succession of alternations of sandstone, calcarenite or conglomerate with shale or mudstone, interpreted as having been deposited mainly by turbidity currents or mass-flow in a deep water environment **within a tectonically active orogenic belt** (emphasis added).

Parts of the second and third definitions of Bates and Jackson (1980) were quoted in my original article, and both also refer to tectonic setting. It is therefore not correct of Haczewski to state that "none of the authors [including others quoted in my original article] relates the term flysch to tectonic setting of deposition".

It is certainly correct that some authors prefer to use the term flysch in an entirely "facies" context, but I would argue that it is completely useless to do

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so. The thick ocean floor turbidite deposits of the Gulf of Mexico and the Atlantic Ocean, and many turbidites of Alpine lakes and glacial margin lakes would qualify as flysch under this definition. This would not be a useful use of the term, in my opinion. Neither the ocean floors nor the lake environments mentioned are in any sense of the word in a tectonically active setting.

Haczewski argues that some authors, including Hsü (1970), in their reference to tectonic setting, are implying present occurrence and not the tectonic setting of deposition. I cannot agree with this interpretation, on re-reading the articles quoted. If that *was* what was implied, I would have to disagree with it, because the present tectonic setting of a fold belt or sedimentary basin may have nothing whatever to do with the tectonic setting of that area during deposition of the flysch-like facies within it. To confuse the two would be a gross error indeed. Consider, for example, a passive or divergent continental margin prism (miogeocline) that acts as the leading edge in a plate collision as its associated ocean is subducted. "Flysch"-like deep water facies commonly form on such margins, for example on the present Atlantic margin of the United States, which could similarly be involved in collision should the Atlantic close. Although situated next to an orogenic belt and suture following collision, the "flysch" would have no genetic relationship to the collision tectonism. To classify the deposits as flysch because of mere juxtaposition to the orogenic belt would be quite incorrect.

Haczewski maintains that I have not made a case for the inconsistent use of the word flysch as a facies term. In my original article (Miall, 1984) I quoted Crostella (1977), who uses the term flysch without defining it, and classifies carbonates and deep water turbidites as molasse because of their post-orogenic setting. I also quoted Tandon and Okada (1982) who use a similar line of reasoning. Haczewski is correct in one sense, in that the evidence in both these articles relates mainly to the use of the term molasse, rather than flysch, but these terms are so commonly used in association with one another that I felt (and still feel) that the two papers supported my case. Admittedly these two papers may not be typical, but the fact that both appeared in respected international publications means that there is a body of opinion to this effect which has to be faced.

I am very concerned that Haczewski would state "doubts exist about using the term for deposits found in submarine fans, trenches or other settings in modern marine basins", and that "the problem of using or not the facies term for these deposits is not urgent at the moment, as only one deep-sea anologue of flysch has been drilled until now". These are astonishing statements, as these are precisely the locations where "marine shale with alternating sandstones ... which constitute a well-bedded sequence ... [and] sedimentological features similar to the Alpine Flysch" (from Hsü's 1970 definition of flysch) occur at the present day. If this is not "flysch", in a facies sense, what is?

Haczewski states: "Summing up, the term flysch conveys a clear meaning for those who use it for a facies characteristic of orogens. Those for whom the

term also bears interpretative tectonic connotations, object to using it". But to restrict the use of the term to orogens is to start out with a tectonic component to the definition! It was one of my main arguments that words such as "orogen" and "Alpine-type fold belt" are too generalised to be useful in an age when plate tectonics has provided us with increasingly powerful tools for regional basin analysis.

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