Defining things is a necessary but thankless task in any scientific endeavor; indeed, the importance of definitions is so great that entire texts have been written on the subject (e.g., Miller 1980; Gorsky 1981). Definitions (of which conceptual models are one form) are necessary because they provide the fundamental basis on which technical communication depends. They may even determine the path of scientific research by encapsulating the fundamental attributes of an object or concept, as they are understood at a point in time. However, it is this latter aspect of definitions that makes their formulation thankless, because there is commonly little agreement, except in retrospect, about what attributes are truly fundamental.

Such is the fate of our attempt (Dalrymple et al. 1992) to define and model estuaries in a way that has greater utility and predictive power than preexisting oceanographic definitions based on salinity. In our view, the fundamental attribute that determines the characteristics of incised-valley estuaries, including the organization of their deposits, is the interaction of fluvial and marine processes, which in turn depends primarily on the nature of the dominant marine process (waves or tidal currents). Thus, we defined the estuary as corresponding to this zone of interaction, and demonstrated that the distribution of estuarine facies is predictable, due to the predictable variations in the relative intensity of fluvial and marine processes along the length of the estuary (see figures 1, 4, and 7 of Dalrymple et al. 1992).

We believe that this concept is particularly powerful because of its simplicity; however, like all simplifications (i.e., like any model), it cannot completely reproduce the details of any particular example because of (minor) factors that were not included in the model (Walker 1984, 1992). While such factors may have a significant influence on the local nature of estuarine facies, we consider them to be of second-order importance because: (1) they do not control the basic geomorphic organization of estuarine facies; (2) their influence is less pervasive or less intense than that of the fundamental interaction of fluvial and marine processes; and/or (3) the nature or distribution of their influence is controlled by the fluvial/marine interaction (i.e., the latter factor is more fundamental).

In their discussion of our paper, Washington and Chisick mention a number of factors which they claim to have an important influence on estuarine sedimentation but which, in their estimation, we did not consider sufficiently. These attributes include: the in situ production and accumulation of biogenic material (peat and carbonate); the rate of sea-level rise relative to the rate of marine-sediment input; and climate (temperate vs. tropical). We readily admit that each of these can have an important influence on the nature of an estuarine deposit; for reasons outlined below, we maintain that, according to the criteria outlined above, all of these attributes are at most of second-order significance, and that the model needs no fundamental modification.

Autochthonous Sediment.—Washington and Chisick are correct in pointing out that autochthonous sediment can be significant in some estuarine systems. Most geologists are well aware that organic material is a common constituent of estuaries; peat deposits are mentioned in many of the examples cited in table 1 of Dalrymple et al. (1992). It is perhaps less well known that carbonates may also form in estuaries under certain circumstances, although we question calling Shark Bay an estuary due to the lack of fluvial influence and the unrestricted connection with the open ocean. Furthermore, we suggest that, in their zeal to right our supposed oversight, Washington and Chisick have erred in at least two significant respects.

First, their suggestion that siliciclastic sedimentation can be “overwhelmed” by autochthonous material reverses the normally accepted, cause-and-effect relationship between siliciclastic and autochthonous sedimentation, namely that peat and carbonate accumulate only in areas where the input of siliciclastic sediment is minimal or absent (McCabe 1991; James and Kendall 1992). Thus, the presence or absence of autochthonous sediment, and its distribution, if present, is controlled by the patterns of siliciclastic sedimentation, and not vice versa. We would argue that our model of estuarine-process distribution summarizes accurately the patterns of siliciclastic sedimentation, and is capable of predicting the occurrence and distribution of autochthonous sediment in at least general terms. Clearly the central basin of wave-dominated estuaries, and marginal areas in all estuaries, are the most likely sites of autochthonous sedimentation, as noted by Washington and Chisick, because they are the locations farthest removed from the sources and pathways of siliciclastic input to the estuary. Only in the extreme case where siliciclastic input from all sources is negligible does autochthonous sedimentation dominate.

Thus, estuaries dominated by autochthonous material and those with subequal mixtures of autochthonous and terrigenous-clastic sediment are likely to be rare, due to the propensity of estuaries to trap siliciclastic material, and the resulting inhibition of autochthonous sedimentation. It follows from this that Washington and Chisick’s triangular classification of estuaries on the basis of sediment source (their figure 1) is likely to have limited utility. Conversely, general models such as ours are of greatest utility if they describe and/or predict the characteristics of the most common examples. In our opinion, our model does this. Indeed, the simple realization that sedimentation defaults to autochthonous material in the absence of siliciclastic input is all that is needed to adapt our model to the situations cited by Washington and Chisick. Additional models designed to accommodate extreme cases seem to us to be of minimal practical value.

Second, we believe that the unstated definition of estuary that underlies Washington and Chisick’s discussion, as well as their publications on the Cambro-Ordovician succession of the Champlain Valley, New York (Washington 1992; Washington and Chisick, in press), is incorrect. In the latter two publications, “estuarine” appears to be used indiscriminately for all incised-valley deposits (real or inferred) and all associated siliciclastics, regardless of possible depositional environment, while in their discussion they state that “carbonate estuaries do not end at the shoreline, but extend out across the shelf...” Geologically, the inclusion of the continental shelf as part of an estuary is unacceptable; such imprecise and unconstrained usage of the term estuary is a backward step that impedes communication, one of the primary roles of a good definition (Jevons 1879; Gorsky 1981). Incised valleys and estuaries are not synonymous!

Rate of Sea-Level Rise.—Washington and Chisick also imply in their fifth paragraph that our model should explicitly include the rate of sea-level rise relative to the rate of marine-sediment input, because it is this ratio that, according to them, determines whether the seaward part of an

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