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Depth to pedogenic carbonate horizon as a paleoprecipitation indicator?: Comment and Reply : COMMENT

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FORUM

Depth to pedogenic carbonate horizon as a paleoprecipitation indicator?: Comment and Reply

COMMENT

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Royer (1999) has recently used a large data set from the U.S. Natural Resources Conservation Service (NRCS) to reexamine the relationship between mean annual precipitation and depth to the calcic horizon in soils. The extremely poor correlation between these variables in his NRCS data ($r^2 = 0.03$) stand in contrast to good correlations (which he has confirmed) in previously published data sets of Jenny (1941), Arkley (1963), and Retallack (1994: r^2 = 0.56–0.64). As Royer indicates, his own correlation is virtually useless, but this does not mean that the others need be abandoned. Jenny (1941), Arkley (1963), and Retallack (1994) took pains to constrain competing variables in soil formation. The global compilation of Retallack (1994), for example, is limited to soils of friable sedimentary parent materials (not limestone, basalt, granite) and moderate development (some 10^3–10^4 years indicated by nodular calcic horizons; not pseudomycelia, veins, plugged or laterally continuous carbonate) that were undisturbed by human activity (unploughed, not overlain by construction materials or eolian dunes) and in sedimentary settings such as alluvial fans and riverine plains (not hill slopes, plateaus, or paleokarst). Contrary to Royer's assertion, I have not found these features difficult to determine from paleosols in sedimentary sequences when using these relationships to estimate paleoprecipitation. The soil pits of Arkley (1963) and Jenny (1941) were carefully chosen and measured by them in the field, and their empirical relationships also remain useful for interpreting paleosols. Such climofunction research aims to be exclusive, whereas soil survey data like that of NRCS aims to include all mappable soils, so it is not surprising that NRCS data, including bedrock, nonnodular, and hill slope soils (Royer [1999] did exclude disturbed soils and surficial calcic horizons), shows such a poor correlation between depth of calcic horizon and precipitation. Royer's graph and correlations are an impressive display of brute computing power, but also an outstanding example of the old computer adage, "garbage in, garbage out."

Royer (1999) suggested that the most useful paleoclimatic indicator to emerge from his data comes from the observation that 95% of his calcic soils are in climates with mean annual precipitation of less than 760 mm, but such paleoclimatic implications for paleosols may only be true for the Rocky Mountains, Great Basin, and California, United States (source of 81.5% of his data). The boundary between calcareous and noncalcareous (pedocals versus pedalfers) soils in the midwestern United States is near the 500 mm isohyet in cool Minnesota but closer to 600 mm in warm Texas (Birkeland, 1984). The boundary between calcareous and noncalcareous soils (ustic versus udic moisture regime) is near the 750 mm isohyet in Natal, South Africa, where 75% of rainfall is in summer, compared with 350 mm in Israel, where almost 100% of rainfall is in winter (Yaalon, 1983). This criterion is thus unreliable for interpreting paleosols.

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REPLY

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The data of Jenny and Leonard (1934) strongly influence the worldwide compilation of Retallack (1994b). These data, in particular those with a depth to carbonate horizon (D) > 100 cm ($n = 35$), anchor the regression of Retallack (1994b). Removal of these 35 data points, which decreases his sample size by only 11%, reduces the r^2 of the regression from 0.62 to 0.44 (Royer, 1999). Removal of all $D > 100$ cm data from the data set of Retallack (1994b) ($n = 67$) further reduces the r^2 to 0.38. Most studies applying this modern relationship to the geologic past use paleosols with $D < 100$ cm

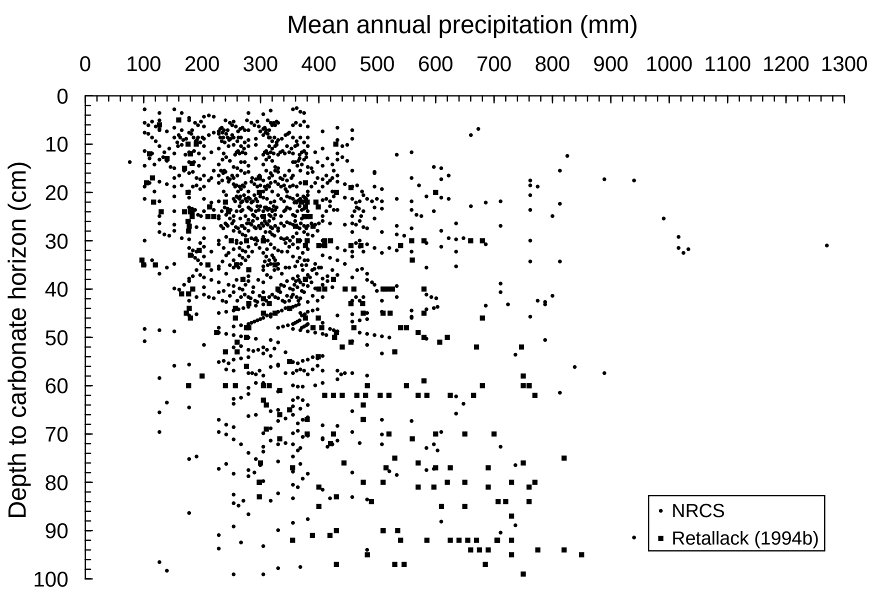

Figure 1. Relationship between depth to carbonate horizon (*D*) and mean annual precipitation (*P*) for $D < 100$ cm. r^2 for combined data sets (NRCS + Retallack) is 0.11 ($n = 1407$). r^2 for Retallack data set only is 0.38 ($n = 246$).

(Retallack, 1992, 1994a, 1994b, 1997; Caudill et al., 1996). Given these relationships, "garbage" paleoprecipitation estimates will surely result.

Furthermore, the Natural Resources Conservation Service–based data set of Royer (1999) suggests that the correlation between *D* and mean annual precipitation (*P*) is even poorer than previously considered at all depths. Unlike the data set of Retallack (1994b), which is based largely on transect studies, the NRCS-based data set approximates a random sample. Therefore, unlike transects studies which are designed to highlight differences in *D*, the NRCS-based data set better quantifies the frequency distribution of *D*. For the United States, soils with $D < 100$ cm dominate (Royer, 1999). Soils with $D > 100$ cm exist, and can be included in transect studies (e.g., Jenny and Leonard, 1934), but unfairly bias regressions for *P* estimates when the paleosols under study have $D < 100$ cm. When only data with $D < 100$ cm are plotted, a discouraging "bull's-eye" pattern emerges (Fig. 1).

In contrast to Retallack's assertions, care was taken in selecting pertinent soils for the NRCS data set. As mentioned, disturbed soils and soils with surficial carbonate horizons ($D = 0$) were eliminated. Ninety-four percent of the soils were described as containing carbonate masses, nodules, accretions, flakes, or weakly cemented grains (Royer, 1999). While parent material and texture were not controlled for, removing soils with carbonate-rich parent material and/or a given textural class (sand, silt, clay) did not affect the regression (Royer, 1999). The only parameter not considered was sedimentary setting.

I agree that the presence versus absence of pedogenic carbonates as an indicator of *P* below or above 760 mm, respectively, should only be applied to paleostudies overlapping in geography with this study's, namely the Rocky Mountains, Great Basin, and California, United States. I regret the oversight in the original publication.

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The Channeled Scabland: Back to Bretz?: Comment and Reply

COMMENT

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The major controversy regarding the origin of the Channeled Scabland is whether the landforms were formed mainly by multiple periodic floods or by a grand-scale cataclysmic flood from late Pleistocene Glacial Lake Missoula. Evidence for the multiple flood—as many as 40 as estimated by Waitt (1980; 1984; 1985)—hypothesis is derived mainly from lake deposits in Ninemile Creek and flood deposits in Burlingame Canyon. Shaw et al. (1999) reevaluated these sedimentary sequences and concluded that the sequences do not imply multiple floods separated by decades or centuries. They suggest that the sediment records indicate that there were sources of water immediately north of the Channeled Scabland, and the scabland flooding might have partially originated from an enormous subglacial reservoir that extended over much of central British Columbia.

We have developed a 3-dimensional numerical code in order to simulate the flood processes in a more realistic manner than the conventional 2-dimensional models. The code can compute movement of a water element under the influence of gravity by the Manning Equation. We assumed that the discharge rate calculation (O'Connor and Baker, 1992) at the Spokane Valley–Rathdrum Prairie conducted by the 2-dimensional HEC-2 program using high-water marks obtained in the field is reliable. The Spokane Valley–Rathdrum Prairie reach is immediately downstream of the breakout point of Lake Missoula, hence the estimated maximum discharge rate by O'Connor and Baker (1992) fairly accurately constrains the maximum possible outflow condition from Lake Missoula. We adopted the hydrograph proposed by O'Connor and Baker (1992) with the maximum discharge of 17×10^6 m^3/s and total amount of water (2184 km^3) equal to the volume of Lake Missoula. We also assumed that the depth of flood erosion is small compared with the maximum water depth and the present-day topography is close to the late Pleistocene one. The simulated water flow was thus fit to the modern-day topography. Our simulation has been able to reproduce accurately the reconstructed flood behaviors, such as hydraulic ponding in the Pasco Basin (Fig. 1). The major finding was that the calculated depth of water in each flood reach except for the Spokane Valley–Rathdrum Prairie is much shallower than the observed depth derived from field evidence. For example, the calculated water depth at the Pasco Basin–Wallula Gap transition zone is about 190 m, significantly less than the 280–300 m indicated by high-water marks. In addition, some flood reaches such as the Cheney-Palouse Scabland are not fully covered (Fig. 1). This is the case despite the fact that we adopted a relatively large Manning coefficient n = 0.1 for this kind of calculation. This implies that there was not enough water from Lake Missoula to explain the observed water depths in the downstream reaches. Furthermore, we also found that if we run the simulation with a smaller discharge rate, for example 2×10^6 m^3/s (Clarke et al., 1984), some major reaches including the Cheney-Palouse Scabland are not even covered by floods since the water then finds a more efficient path through the Columbia River valley. This means that a flood of magnitude about ~10^6 m^3/s could have not made the high-water marks.

The conservative estimate of the total volume of water in the subglacial reservoir by Shaw et al. (1999) is on the order of 10^5 km^3 that is far exceeding 2×10^3 km^3, the volume of Lake Missoula. Our simulation indicates that the volume of water necessary to cover the observed flooded area and to reach the high-water marks is about three times greater than that of Lake Missoula assuming the maximum discharge rate of 17×10^6 m^3/s, Manning coefficient n = 0.1, and water supply from Lake Missoula. Apparently, even the whole lake-draining scenario of Lake Missoula cannot explain the field evidence of high-water marks if our simulation results are valid. This is consistent with the view that, although floods from Lake Missoula certainly played a role for the formation of the Channeled Scabland, there were other sources of water. The subglacial flooding from the north

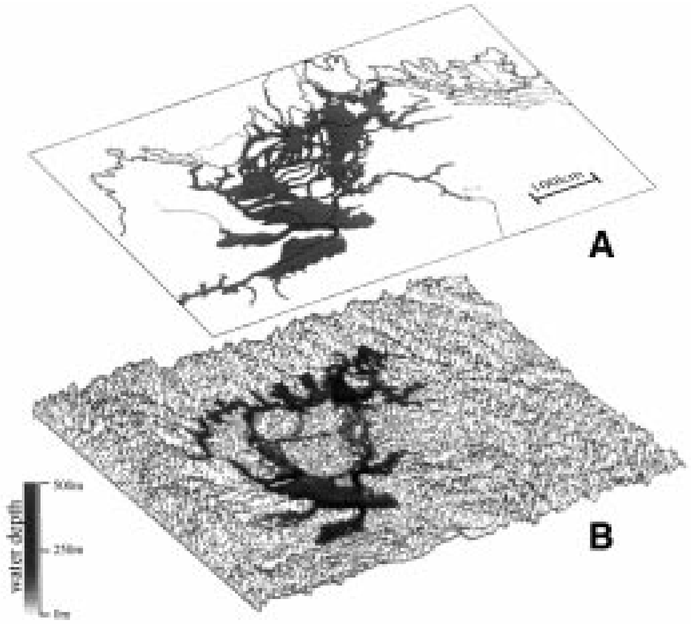

Figure 1. Area coverage of the Channeled Scabland (A) and the flood simulation result for an outburst from Pleistocene Glacial Lake Missoula on 4th day of flooding (B). The simulation was constrained by the best estimate of the maximum possible discharge rate (17×10^6 m^3/s) from Lake Missoula and the volume of the lake. Note that the calculated area does not cover the some major flood reaches fully. This indicates that other sources of water may be needed to explain the field evidence.

proposed by Shaw et al. (1999) may provide an explanation for the indicated increased volume of water required to explain the high-water mark evidence in the Channeled Scabland.

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We thank Victor Baker for providing us useful insights for this project.

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REPLY

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We thank Komatsu et al. for their comments on meltwater volumes, discharges and depths required to flood areas of the Channeled Scabland. Their results are exciting, as they demonstrate that, using the maximum discharge rate from Glacial Lake Missoula (cf. O'Connor and Baker,1992), the volume of water stored in Glacial Lake Missoula (2184 km^3) was insufficient to reach maximum flooding levels in the Channeled Scabland; at least three times more water was required. This important result is determined from their development of a 3-dimensional model that incorporates gravity relationships in water flow. Komatsu et al. reach the same conclusion as we did (Shaw et al., 1999): that additional floodwater sources are needed to flood the Channeled Scabland. Their estimate on the volume of water required to flood all Scabland channels to the required levels is lower than our estimate of storage beneath the Cordilleran Ice Sheet (Shaw et al., 1999). However, our volume estimates relate to work in progress and subsequent fieldwork should help to derive a more accurate figure on subglacial water storage.

Although Komatsu et al.'s model does not provide insight into the number of floods that affected the Channeled Scabland, it does demonstrate that there had to be *at least* one enormous flood, with water from more sources than just Glacial Lake Missoula. It is exciting that two studies relating to different aspects of Channeled Scabland flooding result in similar conclusions: Other floodwater sources are necessary to explain geomorphic and sedimentary relationships (Shaw et al., 1999), and other floodwater sources are necessary to explain the degree of flooding within the Channeled Scabland tract (Komatsu et al.). These results highlight the need for further research on proglacial and subglacial hydrodynamic modeling along with the integration of detailed field observations.

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COMMENT

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Shaw et al. (1999) say that only one big flood went through the Channeled Scabland during the last glaciation. They also say this flood did not come from Glacial Lake Missoula; they propose that the water flowed southward from a reservoir beneath the Cordilleran ice sheet.

These ideas clash with a wealth of evidence that Lake Missoula sent dozens of big floods through the Channeled Scabland (numbered examples

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Data Repository item 200062 contains additional material related to this article.

in Figs. 1 and 2, and in Table 1).[1] At the site of that lake, varved deposits record at least 34 occasions when Lake Missoula drained partly or completely after having filled for several decades (1–3). West of Lake Missoula, on the north margin of the Channeled Scabland, varve-bounded beds record dozens of floods into other lakes, particularly Glacial Lake Columbia (4–6). Varve counts show that most of these floods—like the drainings of Lake Missoula—repeated at intervals of several decades (10). In three valleys headed by ice sheet lobes, current indicators show that major floods entered those valleys from the south and initially ran northward, toward the Cordilleran ice sheet (7–9). One such valley contains a 115 m section in which 2000–3000 of Lake Columbia's varves are regularly intercalated with the deposits of 89 last glacial floods that probably came from Lake Missoula (5). At least 60 of these floods are probably recorded by deposits in Pasco Basin (16–18)—where time between floods is evidenced by several widespread tephra layers: by loess, colluvium, and mud cracks, and, most abundantly, by animal burrows (12–15). East of Pasco Basin, about 20 last glacial floods crossed a high divide in the Channeled Scabland (19). Six floods below Pasco Basin probably exceeded 6 x 10^6 m^3/s (20).

Most scabland floods from non-Missoula sources were probably confined to the lowest scabland channels. An example is a series of some 45 non-Missoula floods that entered Lake Columbia late in the last glaciation (11). These floods were probably smaller than any Missoula flood of their time. But they unavoidably went through the Grand Coulee—not because of their size, but because that scabland channel served as Lake Columbia's usual outlet (3).

[1]GSA Data Repository item 200062, Figure 2, Index map, and Table 1, Examples of evidence for dozens of colossal floods from Glacial Lake Missoula, is available on request from Documents Secretary, GSA, P.O. Box 9140, Boulder, CO 80301-9140, editing@geosociety.org, or at www.geosociety.org/pubs/ft2000.htm.

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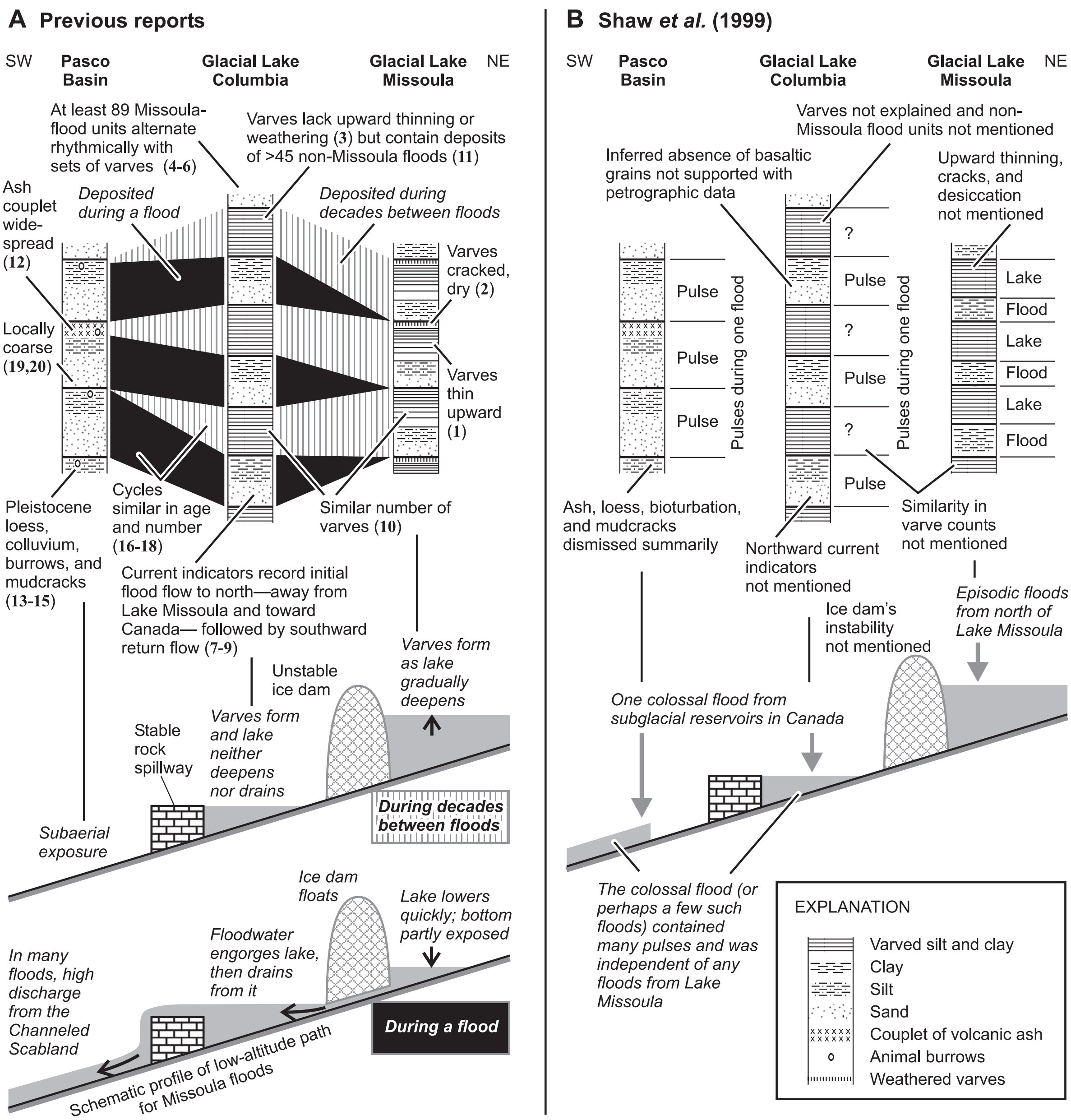

Figure 1. Two views of rhythmic deposits and their implications for last-glacial floods in the northwestern United States. Numerals in parentheses in A are keyed to an index map (Fig. 2, see footnote 1) and to details in Table 1 (see footnote 1).

REPLY

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We thank Atwater et al. for their comments on our paper and acknowledge their long experience in Scabland studies. We believe that some of their disagreement with our conclusions rests on their impression that we eliminate Glacial Lake Missoula as a source for the Scabland floods. Although we introduce other sources of floodwater, Glacial Lake Missoula remains important and is shown as a major reservoir on our Figure 6 (Shaw et al., 1999). The addition of subglacially stored water is crucial for supplying floodwaters to the western erosional tracts in the Channeled Scabland. Atwater et al. are also of the impression that our proposed reservoirs under the Cordilleran ice sheet drained independently of floods from Glacial Lake Missoula. Rather, we stated that sedimentation in Glacial Lake Missoula was independent of sedimentation in the Channeled Scabland. That is, beds in the Ninemile Creek section cannot be confidently correlated with beds at sites in the Channeled Scabland. We show clearly on our Figure 6 (Shaw et al., 1999) that water is released from Glacial Lake Missoula at approximately the same time as water is released from beneath the Cordilleran ice sheet.

We respond to Atwater et al.'s detailed comments using the same numbering system:

1–3. Atwater et al. argue that varve-bound beds at the Ninemile section record at least 34 drainage events from Glacial Lake Missoula. While they say that these beds record drainage events, they consider *similar beds* deposited in Glacial Lake Columbia to be flood events. Their interpretation of flood beds in Glacial Lake Columbia is similar to our view of the Ninemile section sand-silt beds that we infer to have been deposited by jökulhlaups. We also do not see how upward-fining varves favor deposition in a periodically draining lake. Varve thickness depends mainly on sediment supply and usually decreases rapidly with distance from source. Furthermore, with respect to varves and flood cycles, 89 flood deposits in Glacial Lake Columbia, and at least 60 flood deposits in Pasco Basin, are related by Atwater et al. to 34 drainage events at the Ninemile section. Obviously, there has been no rigorous correlation of these units, although similarity in varve numbers for cycles at different sections is mentioned by Atwater et al. Anomalously thick clay beds cover the sand-silt "drainage units" at the Ninemile Creek section. While these thick clay beds are to be expected at the end of a year with very high sediment input to the lake, it is not obvious why they would form at the end of a multiyear, low-water event. Nor is it clear, if the coarse beds represent accumulation over many years, why clay beds are not found within sand-silt beds. Although we did not observe obvious desiccation cracks at Ninemile Creek, these features are as easily attributed to dewatering with loading caused by rapid deposition of the overlying sand-silt units.

4–6. These points are partly discussed above. The inconsistency that Atwater et al. point out in no. 6 is not clear to us. Our arguments for the origin of sedimentary sequences in Glacial Lake Missoula apply equally well to other lakes, including Glacial Lake Columbia, fed by meltwater from the Cordilleran ice sheet.

7–10. We do not dispute that water came from Glacial Lake Missoula. We state that it *also* came from the north (see Figure 6 in Shaw et al., 1999). Waitt (1980) indicates that the presence of ash is representative of subaerial exposure, yet he also indicates that the ash is contaminated in places (p. 664). This is clearly demonstrated in his photographs (p. 665). Thus we stand by our interpretation that the ash was likely deposited simultaneously with suspension sediments. Our conclusions on clast lithology at Sage Trig were drawn from gravel clasts, not grains. We were struck by the absence of basalt, which should be present if the floods originated from Glacial Lake Missoula. Also the paleocurrents at Sage Trig are in keeping with flow from the Columbia Lobe but not for flow from Glacial Lake Missoula (Atwater, 1983).

11. As stated earlier, jökulhlaups are recorded in sediments at Ninemile Creek and at sites representing Glacial Lake Columbia.

12–17. Shaw et al. (1999) clearly stated that gradational relationships between climbing ripples and massive silt are readily explained as waning stage deposition in an aqueous environment. Such beds are deposited in hours (e.g., Ashley et al. 1982), not decades. Smith (1993) talks of loess caps (Facies NA) on flood beds, but his photographs demonstrate the same gradational lower contacts expected of waning stage aqueous deposition. The same stands for Smith's (1993) Facies NB. His Facies NC represents aqueous deposition and has no bearing on understanding the degree of subaerial exposure. There is no doubt that thick silt beds would resemble loess, especially if they comprise reworked loess. In our minds, no detailed evidence was provided to substantiate the presence of loess.

18–20. It is difficult to envisage the correlation of 60 flood beds in the Pasco Basin with 34 fill-drain cycles in Glacial Lake Missoula. Floodwaters overtopping the divide at the south end of the Cheney-Palouse scabland tract do not singularly support multiple floods from Glacial Lake Missoula, or pulses within a single flood from multiple sources. We clearly show on our Figure 6 (Shaw et al., 1999) that water flowed from many, if not all, of the interior valleys of British Columbia, and not just from those that lay behind the Okanagan Lobe. These other sources along with Glacial Lake Missoula would easily overtop the said divide. We thank Atwater et al. for pointing out a mistake in our calculation of flood duration: Drainage times should read around 1000 days.

The purpose of our paper is to explore some alternative explanation for the Channeled Scabland. Our work in the Okanagan Valley to the north led us to the conclusion that large floods passed through that valley. It follows that the Okanagan Lobe, Lake Missoula, and possibly other sources of meltwater in central British Columbia may have contributed to Scabland erosion and the deposition of various flood beds. In their discussion of our paper, Atwater et al. make a strong case for the multiple flood hypothesis. At the same time, they do not falsify our suggestion of much fewer floods. The differences are largely based in sedimentary interpretation and correlation.

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