abundance of the lighter isotope (Shimizu and Hart, 1982), which will result in apparently low $^{207}Pb/^{206}Pb$ ages. Several attempts have been made to constrain the limits for fractionation in SHRIMP $^{207}Pb/^{206}Pb$ measurements by comparison with thermal ionization analyses on the same samples, but a convincing value has not been established. In some cases no fractionation could be proven (e.g., Compston et al., 1984). The estimate for mass fractionation of 3‰/amu suggested by Wiedenbeck and Watkins (1993) is an artifact due to their use of 572 Ma for the $^{207}Pb/^{206}Pb$ age for the reference zircon SL3 instead of 552 Ma. It is agreed that the unknown Pb isotopic fractionation during sputtering remains a problem, but it should be noted that 3‰ Pb isotopic fractionation causes only ≤5 m.y. underestimation in the ages of the 3600–3900 Ma samples presented by Nutman et al. (1993).

I stress that the universal problems in data assessment raised by Wiedenbeck are second order in the consideration of dating Archean zircon populations having low U contents. In no way do these problems alter the conclusion of Nutman et al. (1993) that the Amîtsoq gneisses of West Greenland contain many groups of unrelated plutonic rocks emplaced between 3600 and 3900 Ma.

ACKNOWLEDGMENTS

I thank colleagues at the Research School of Earth Sciences for their suggestions on this Reply, Trevor Ireland modified the original data assessment program of Stephanie Maxwell to incorporate any excess scatter.

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Statistical inevitability of Horton's laws and the apparent randomness of stream channel networks: Comment and Reply

COMMENT

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Kirchner (1993) raised some interesting questions concerning the value of Hortonian analysis of drainage networks. We comment here on his conclusions and offer another perspective on this problem. Generally, Kirchner argued that Horton's laws are "statistically inevitable" and therefore that Hortonian analysis is a weak test of theoretical models of network structure. We demonstrate here that this conclusion is not in general justified. Although, as Kirchner showed, Horton's ratios may yield weak tests of certain types of structural peculiarities, they will provide powerful tests of other types.

We restrict our discussion to the bifurcation ratio, R_B, in this Comment, but similar arguments may be advanced concerning the length and area ratios as well. Let S_n be the set of all configurations of networks of magnitude n (i.e., with n first-order streams). Kirchner generated networks from a union of the sets S_n for n ranging from 20 to 1000 and analyzed this mixture; results shown in his Figure 2A depict clearly the tendency for R_B to be clustered around 4. What must be recognized, however, is this crucial fact: The distribution of R_B depicted in Kirchner's Figure 2A is very much dependent on the computer algorithm (a Monte Carlo method of Shreve, 1974) that he used to generate the networks. When Shreve's algorithm is used, the assumption is that all elements of S_n for a given n are equally likely; in other words, a uniform distribution on S_n holds. (This, incidentally, is the primary assumption of the random topology model.) Thus, the "statistical inevitability" of R_B values near 4 (Kirchner, 1993) is in fact really a property of networks having a uniform probability distribution on S_n. We could quite easily design a computer exercise that would produce networks having an R_B distribution that looks quite different from the one in Kirchner's Figure 2A; this is simply a matter of sampling from a suitably chosen nonuniform distribution, such as the one we discuss below.

Kirchner implied that the method he used to generate networks is in some sense preferred—he referred to his sample as "unbiased." But it is not at all clear in what sense the term "unbiased" is to be interpreted here. It is true that there is a natural correspondence between a uniform distribution and simple enumeration of points in a finite sample space (S_n here); simple enumeration makes an implicit assumption that all points in the sample space are equally likely. But in performing such an enumeration we must be careful not to fall into the trap of thinking that our equal probability enumeration has any implications about what patterns might arise in nature. It is perfectly conceivable that real-world networks might tend to lie in a small (perhaps very small!) subset of the sample space S_n. Without first collecting data for actual networks and computing R_B, we do not know whether such values will tend to cluster around 4, or near 2, or near 5. If we collected data and found that R_B tended to lie near 2.5, for example, it would be evidence for a nonuniform distribution on S_n in nature. Accordingly, in that situation, a more natural and "unbiased" Monte Carlo–generated sample would be one that comes from a nonuniform distribution. Now, the fact that after collecting data on real basins, we observe that R_B values do tend to have a relative frequency that closely resembles Kirchner's Figure 2A is an indication that the uniform distribution tends to hold for real basins. That is, it is one piece of evidence in favor of the random topology model. There is, however, no "statistical inevitability" for such behavior, and there is no way we could have, without data, predicted that the uniform distribution would be a good approximation for real networks.

There have been numerous examples of nonuniform distributions on topological network configurations in the geomorphology literature; see, for example, Werner (1972), Dacey and Krumbein (1976), and Van Pelt et al. (1989). In these works one sees that R_B

may indeed differ consistently from 4, even in the limit as the number of sources n grows large. A further illustration that there is nothing sacred about a uniform distribution in nature may be found in Karlinger and Troutman (1989) and Troutman and Karlinger (1992). In this work, the set of possible network configurations (analogous to S_n above) is composed of spatial networks draining a fixed set of grid points. Although we first assumed a uniform distribution (Karlinger and Troutman, 1989), we found, after further investigation (Troutman and Karlinger (1992), that real networks do in fact constitute only an extremely small subset of the set of all the possible networks. Here is an example in geomorphology where generation of networks according to uniform distribution would give a very skewed picture of what might be regarded as "statistically inevitable." This situation is the rule rather than the exception in statistical mechanical systems, where typically large parts of the configuration space end up being assigned a very small probability.

What about the usefulness of R_B in testing network models? Let us say that we want to test a uniform (null hypothesis, H_0) vs. a nonuniform model (alternative hypothesis, H_A) using R_B as a test statistic (Kirchner used the terms "random" and "nonrandom" instead of "uniform" and "nonuniform"). The power of the test will depend on the distribution of networks under H_A. Kirchner (1993) correctly pointed out that the test using R_B will not be very powerful in testing certain alternative models, such as those with certain restrictions on diameter, network width, source height, or source height divided by diameter. But why should we expect a bifurcation property to be powerful in these situations? If we are really interested in testing bifurcation structure, it makes more sense to pose a parametric model that explicitly incorporates this structure. Although there are many ways one could define such a model, a very simple probability model may be defined by taking the probability of a network s to be proportional to $\exp[-\beta R_B(s)]$, where s is a particular network in S_n, $R_B(s)$ is the bifurcation ratio for s, and β is a parameter. This model is patterned after the Gibbsian model in statistical mechanics (Troutman and Karlinger, 1992); the techniques in this paper may be used to generate networks from this distribution. When β is 0, this model reduces to the random topology (i.e., uniform) model. Thus, the hypotheses of interest become H_0; $\beta = 0$ vs. H_A: $\beta \neq 0$. Under H_0, the distribution of R_B will look like Kirchner's (1993) Figure 2A. Under H_A, however, this distribution can have central tendency anywhere between the minimum (2, or near 2, depending on n) and maximum (n) value that R_B may take on. Thus, using R_B to test the random topology model against this set of alternative models will indeed lead to a powerful test; Kirchner's claim that R_B is "profoundly indifferent to network structure" certainly does not hold here. All we have done is to emphasize the obvious: R_B is useful for detecting differences in bifurcation structure, even though it may not be useful for detecting other types of differences.

We agree with Kirchner's assertion that there are many interesting and important properties of networks that Horton's laws do not address and that Horton's ratios cannot test effectively. Determining what morphometric properties are distinctive and structurally important and devising tests to look at these properties do indeed "remain central problems in quantitative fluvial geomorphology." Horton's ratios do tell us something substantive, however. Let us not be quite so hasty in discarding them.

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REPLY

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I appreciate the interest Brent Troutman and Michael Karlinger have shown in my work, but I regret that they have misconstrued my argument. They portray me as claiming that natural channel networks are topologically random, and that therefore Horton's laws and the commonly observed values of Horton's ratios are unavoidable. That's not what I said, as my paper (Kirchner, 1993) plainly shows. My argument was, and is, as follows. (1) The vast majority of all possible networks have Horton ratios similar to the values reported for natural channel networks, so the Horton ratios observed in nature are not special or peculiar to natural networks. (2) Many different models of network structure predict functionally identical distributions of Horton's ratios, so Hortonian analysis cannot be used to test these models against one another. (3) In particular, many nonrandom sets of networks have Horton ratio distributions similar to those of the random model, so the fact that topologically random networks yield "realistic" Horton ratios does little to support the notion that natural channel networks are topologically random.

Some of Troutman and Karlinger's difficulties stem from their fixation on the phrase "statistically inevitable," which occurs four times in their comment but is found only in the title of my paper, never as part of the assertions in which they incorporate it. That Troutman and Karlinger have misread my work is illustrated by their third paragraph, in which they wonder about my use of the term "unbiased," and suggest that a nonuniform distribution would yield a more "unbiased" sample if real networks are distributed that way. My paper, however, clearly refers to an "unbiased sample *of all possible networks*" (emphasis added), not an unbiased sample of real networks. My simulated networks are not intended to model real stream networks, but rather to sample the universe of all possible network configurations without preferentially selecting any particular kinds of networks. These simulations permit me to estimate the Hortonian behavior expected for networks that are not "special" in any particular way, and thus to show that Hortonian analysis apparently cannot distinguish real stream networks among the class of all possible networks. In other words, I used topologically random networks as a *null hypothesis* (not a hypothesis for how actual networks are formed), and showed that within the Hortonian framework, one cannot reject this hypothesis; nor—an important point—can one reject a wide variety of competing hypotheses. Because R_B is a purely topological property of networks, the obvious null hypothesis for such a study is a topologically unbiased subset of all possible networks. The "crucial fact," as Troutman and Karlinger put it, that they could contrive a different algorithm to give different R_B distributions, is true but irrelevant. Any contrived distribution would be less useful as a null hypothesis, because it would not properly span the universe of all possible networks.

Of course, Troutman and Karlinger are correct that the distribution of R_B values could diverge from my Figure 2A if the probability of a network s were proportional to, for example, $\exp[-\beta R_B(s)]$ with $\beta \neq 0$, or any other nonuniform probability distribution. Obviously, by preferentially selecting particular networks *according to* R_B *itself*, we can create populations of networks with different distributions of R_B. However, the fact that we can do this does not demonstrate—as Troutman and Karlinger imply—that R_B is an interesting or important network property. Note, for example, that we could use the same technique to select networks according to a perfectly meaningless parameter, such as a randomly assigned serial number. Therefore, Troutman and Karlinger's argument does not support their claim that "R_B is useful for detecting differences in bifurcation structure," except in the trivial sense that R_B can detect differences in R_B itself.

One could test a null hypothesis (such as $\beta = 0$ in the expression above) against alternative hypotheses (such as $\beta \neq 0$) for the distribution of R_B, as Troutman and Karlinger suggest. But what would we learn from doing so? We would merely be testing whether R_B was distributed one way or another, not testing theories about how networks are formed. For tests of R_B to be scientifically informative, different theories of network structure must predict R_B distributions that diverge measurably from one another. Many radically different channel network models predict roughly the same Hortonian behavior (because, as my [Kirchner, 1993] Figures 3 and 4 illustrate, pronounced differences in network structure can be obscured in the Horton statistics). The fact that each model agrees with the Horton statistics of real networks is taken by its authors as confirmatory, while it somehow escapes notice that the same statistics agree equally well with other incompatible models. To spur development of better network models, we need tests that the models will stand some chance of failing.

Troutman and Karlinger conclude that R_B is a useful measure that can provide powerful tests of network theories. Experience suggests otherwise. For example, while the papers cited by Troutman and Karlinger (Werner, 1972; Dacey and Krumbein, 1976; Van Pelt et al., 1989) all contain theories predicting different distributions of R_B, in none of the three cases were R_B distributions useful for testing the competing theories empirically. Although channel networks found in nature might hypothetically have bifurcation ratios falling far outside the range $3 \leq R_B \leq 5$, in practice they rarely do so. Many have viewed this as remarkable (rather than obvious) and have proposed theories to explain the observed values of R_B. These theories have usually been tested without the benefit of a null hypothesis. Instead, the common practice has been to first present a premise that implies R_B of roughly 4, then observe that natural channel networks also have R_B of roughly 4, and then declare the premise to be validated, without considering whether R_B could be 4 even if the premise were false. Thus, it is not valid to conclude (as Troutman and Karlinger do) that the R_B values observed in nature are "one piece of evidence in favor of the random topology model," because the R_B distributions predicted by random and nonrandom sets of networks are practically indistinguishable. (Troutman and Karlinger portray me as assuming that networks must be topologically random, whereas my work shows why this need not be the case.)

Although Troutman and Karlinger take issue with my assertion that R_B is "profoundly indifferent to network structure," their own experience illustrates my point. Their random-walk network growth model yielded R_B values typically distributed between 3 and 5, with means very close to 4, similar to the R_B distributions of real networks (Karlinger and Troutman, 1989), even though later work (Troutman and Karlinger, 1992) showed that other measures could clearly distinguish the simulated networks from real networks at $p < 0.01$. In other words, even though natural drainage networks occupied a very small subset of the model's sample space, their R_B values apparently differed little from those of the rest of the networks in the sample space.

We should not be too hasty in discarding Horton's ratios, but neither should we be too reckless in employing them. For five decades, Horton's ratios have been used under the presumption, as yet still unsubstantiated, that they reflect interesting and important properties of networks and provide useful tests of theoretical models. Such tests would require explicit hypotheses of network formation, and explicit null hypotheses, whose predicted Horton ratios differ sufficiently that they can be distinguished by a feasible set of empirical observations. Powerful hypothesis tests employing Horton's ratios may yet be possible, and I would welcome them. However, in the absence of evidence that particular hypothesis tests are meaningful, we should be cautious of studies that find confirmation in successfully predicting Horton's laws.

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CORRECTIONS

Chicxulub structure: A volcanic structure of Late Cretaceous age: Correction

Geology, v. 22, no. 1, p. 3–4 (January 1994)

Paragraph two of the article states that there are 350 m of Late Cretaceous age sediments overlying the volcanic sequence in the Yucatán No. 6 well. This number applies to the Chicxulub No. 1 well; the correct number for the Yucatán No. 6 well is 260 m. For the Chicxulub No. 1 well, the top of the Maastrichtian was cited at a well depth of 920 m and the top of the andesite at 1270 m; for the Yucatán No. 6 well the top of the Maastrichtian was cited at 1000 m and the top of the andesite at 1260 m.

Cancer and autoimmune disease: A Cambrian couple?: Correction

Geology, v. 22, no. 1, p. 5 (January 1994)

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