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

James W. Kirchner Department of Geology and Geophysics, University of California, Berkeley, California 94720

ABSTRACT

The remarkably regular geometric relations observed in stream networks have been widely interpreted as evidence of a distinctive structure that reflects particular geomorphic processes. These relations have also been interpreted as evidence that stream networks are topologically random, formed by the laws of chance. Neither of these inferences is justified. The oft-cited geometric properties are not specific to particular kinds of stream networks or to topologically random networks; instead, they describe virtually all possible networks. They therefore compel no particular conclusion about the origin or structure of stream networks.

OBSERVED REGULARITIES IN STREAM NETWORK STRUCTURE

Stream networks are important, both as controls on drainage basin hydrology (Kirkby, 1976) and as indicators of geologic processes. The patterns formed by stream channels are thought to reflect regional tectonics (Ollier, 1981; Cox, 1989; Burbank, 1992) and local geologic structure (Abrahams and Flint, 1983), as well as prevailing erosional mechanisms (Dunne, 1980) and climate (Gregory, 1976; Daniel, 1981).

The morphology of stream channel networks is often characterized in terms of Horton's (1945) "laws of drainage network composition" (Fig. 1). Horton's "law of stream numbers" states that N_ω, the number of streams of order ω, decreases geometrically with stream order:

$$\frac{N_\omega}{N_{\omega+1}} \approx R_B \text{ or } N_\omega \approx R_B{}^{\Omega-\omega}, \qquad (1)$$

where Ω is the order of the network's main stream and R_B is the "bifurcation ratio." Horton's "law of stream lengths" holds that L_ω, the mean length of streams of each order, increases geometrically with stream order:

$$\frac{L_\omega}{L_{\omega-1}} \approx R_L \text{ or } L_\omega \approx L_1 R_L{}^{\omega-1}, \qquad (2)$$

where L_1 is the mean length of the first-order tributaries and R_L is termed the "length ratio." The "law of stream areas," proposed by Schumm (1956) in the spirit of Horton, holds that drainage-basin area A_ω increases geometrically with stream order:

$$\frac{A_\omega}{A_{\omega-1}} \approx R_A \text{ or } A_\omega \approx A_1 R_A{}^{\omega-1}, \qquad (3)$$

where A_1 is the mean area draining into each first-order tributary and R_A is the "area ratio."

Today, many textbooks teach Horton's "laws" as central principles of drainage-basin structure (e.g., Chorley et al., 1984; Selby, 1985; Press and Siever, 1986; Ritter, 1986; Skinner and Porter, 1987; Judson and Kaufman, 1990; McKnight, 1990; Bloom, 1991; Summerfield, 1991; Easterbrook, 1993). Dozens of studies on stream networks in diverse landscapes have confirmed that, as equations 1–3 predict, semilogarithmic plots of stream lengths, numbers, and areas vs. stream order (e.g., Fig. 1B) are nearly linear. Except in networks subject to structural controls, these studies have also shown that bifurcation, length, and area ratios are restricted to relatively narrow ranges; R_B generally varies between 3 and 5, with a modal value of 4, R_L usually ranges between 1.5 and 3, with a modal value of roughly 2, and R_A typically ranges between 3 and 6 (Chorley, 1957; Smart, 1972; Abrahams, 1984).

Many geomorphic theories yield networks that satisfy Horton's laws and give values of R_B, R_L, and R_A that closely resemble those typically observed. Horton's laws have been widely used as empirical tests of specific models of drainage network development (e.g., Roth et al., 1989; Rodriguez-Iturbe et al., 1992). They have also been used to test more general claims that stream networks form from successive generations of rills (Horton, 1945), that networks develop according to principles of maximum entropy or maximum efficiency (Leopold and Langbein, 1962; Woldenberg, 1969; Leopold, 1971; Rodriguez-Iturbe et al., 1992), and that networks are topologically random, developing largely by the laws of chance (Shreve, 1966, 1969, 1975).

NEED FOR A NULL HYPOTHESIS

Attempts to test geomorphic theories against Horton's laws have generally suffered from the lack of an appropriate null hypothesis. They have implicitly (and sometimes explicitly) assumed that Horton's laws, and the observed Horton ratios, characterize a distinctive type of network structure and thus represent a strict empirical test of whether the theories outlined above yield realistic networks. Here I demonstrate that this premise is false by showing that almost all possible networks fit Horton's laws and have Horton ratios similar to those observed in nature.

Directly enumerating all possible networks is impractical, because in networks

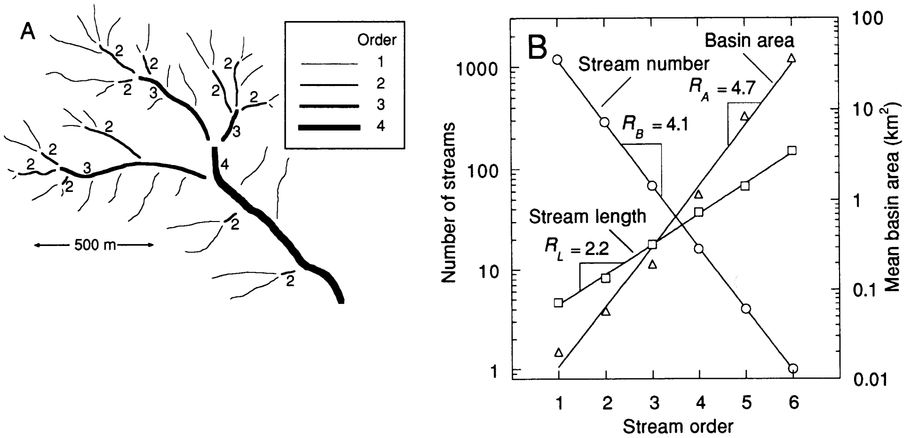

Figure 1. A: Exploded diagram of stream channel network in Marin County, California (Montgomery and Dietrich, 1989), illustrating application of conventional stream-ordering rules (Strahler, 1952): (1) all streams without tributaries are first order; (2) where two streams of order ω join, they both terminate and a stream of order ω + 1 begins, and (3) where two streams of unequal order meet, lower-order stream terminates, and higher-order stream continues through junction. B: Number (circles), mean length (squares), and mean drainage area (triangles) of streams in drainage network of Daddy's Creek, Tennessee (data of Morisawa, 1962), plotted as function of stream order, with bifurcation, length, and area ratios (R_B, R_L, and R_A; see equations 1–3) calculated from slopes of regression lines.

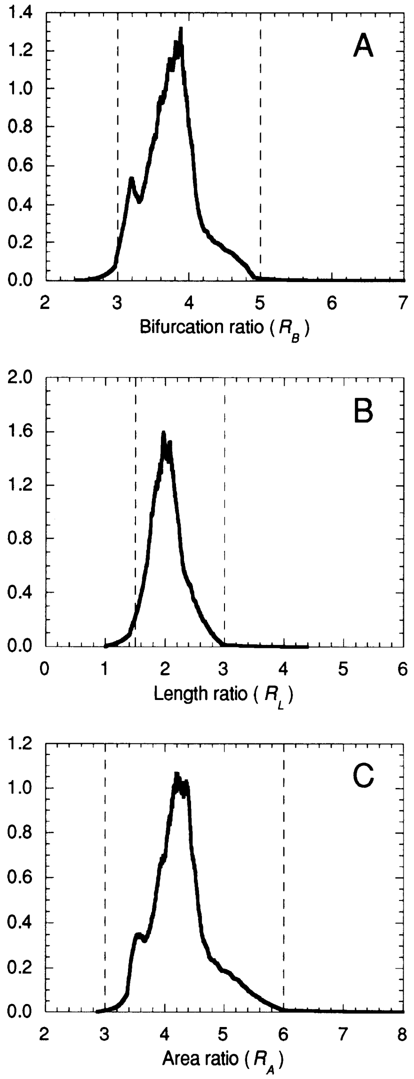

Figure 2. Probability density functions (p) for bifurcation, length, and area ratios for unbiased sample (see text) of 9800 networks evenly distributed between N_1 = 20 and N_1 = 1000. Note that distributions are narrow and that almost all networks in this Monte Carlo sample have Horton ratios that fall within ranges typically observed in natural stream networks (dashed lines).

with realistic numbers of first-order streams, the number of possible configurations is very large (Shreve, 1966). Instead, I use a Monte Carlo method (Shreve, 1974) to select an unbiased sample of all possible networks formed by a given number of first-order channels. I then apply the ordering rules in Figure 1A to each network; tally the numbers, lengths, and drainage areas of streams of each order; and calculate the resulting bifurcation, length, and area ratios.

In a branching network, the number of streams of each order is a purely topological property; it depends only on the sequence in which the channels merge (Melton, 1959). Stream lengths and drainage areas depend both on network topology and on the length and contributing area of each channel segment (or "link") connecting adjacent nodes. Here, for simplicity, I assume that average link lengths do not vary with order, so length ratios can be estimated from the topological lengths (i.e., the number of component links), rather than physical lengths, of streams. I also assume that the average contributing area per link does not change with order, so area ratios can be estimated from the total number of links, including tributaries, that make up each stream in a network (these assumptions are addressed in more detail below). Thus, by tallying the number of streams in each order, the number of links in each order, and the number of links in each order's drainages, I can estimate R_B, R_L, and R_A by linear regression (as in Fig. 1B) for each Monte Carlo network (in accordance with typical practice, if a single point corresponding to a short main stream would exert undue leverage, it is omitted from R_L regressions).

In an unbiased sample of all possible networks that can be formed by merging N_1 first-order channels, $20 \le N_1 \le 1000$, 96% of all bifurcation ratios (R_B), 95% of all length ratios (R_L), and 98% of all area ratios (R_A) fall within the ranges considered typical for natural stream networks ($3 \le R_B \le 5$, $1.5 \le R_L \le 3$, $3 \le R_A \le 6$), and the modal values are close to those observed in nature (Fig. 2). The Horton plots for each of the three ratios (as in Fig. 1B) are also very straight; 96% of R_B and R_A plots have regression coefficients $r^2 \ge 0.98$, and 95% of R_L plots have $r^2 \ge 0.80$. In other words, almost all possible networks obey the same Hortonian "laws" observed in studies of natural channel networks.

If stream networks have distinctive characteristics, Horton's ratios fail to identify them, yielding only the singularly imprecise conclusion that natural stream networks are some subset of all possible networks. It is not surprising that Horton's laws are successfully predicted by many different theories and models, because only a small fraction of possible networks violates Horton's laws. Therefore, the observed regularity of stream networks, as viewed through the traditional stream-ordering protocols and Hortonian analysis, is an exceedingly weak test of theoretical models of network structure.

RANDOM AND NONRANDOM NETWORKS

Statistical methods like those used above have previously been used to draw a very different conclusion—that in the absence of structural controls, natural channel networks develop by chance and are in fact topologically random, like the Monte Carlo sample in Figure 2 (Shreve, 1966, 1969, 1975). Today, it is generally believed that stream networks obey Horton's laws because they are topologically random, but the data do not justify that interpretation. Topologically random networks certainly obey Horton's laws, but so do networks that clearly violate the assumption of topological randomness, as I show below.

My approach is to take the Monte Carlo networks in Figure 2 (which are topologically random) and rank them according to various topological criteria. Dividing a ranked set in half yields two topologically

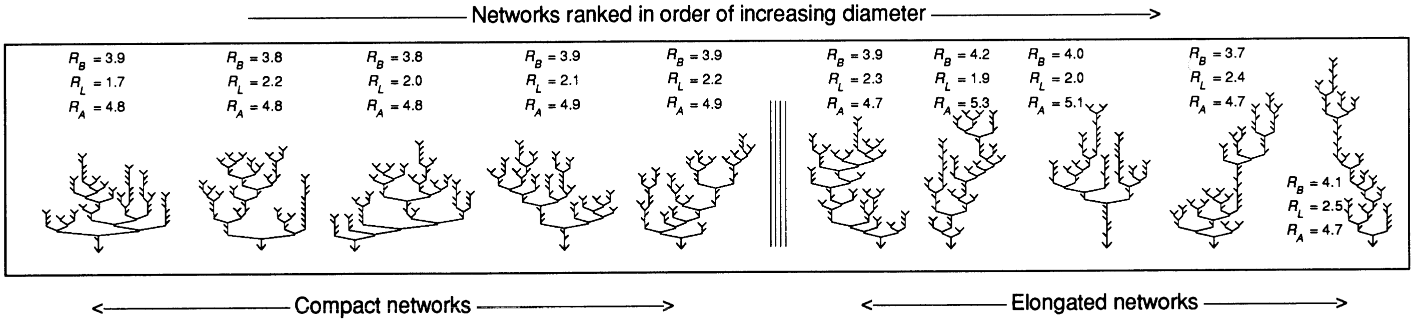

Figure 3. Topologically random set of ten networks formed from 60 first-order streams, ranked in order of increasing diameter. Stick-figure diagrams describe network topology (i.e., order in which channels merge), but do not show size, shape, or orientation of channel links. Arrow represents outlet of network, and vertical position of individual links indicates number of junctions separating them from outlet. Note that obvious differences between elongated and compact networks are not reflected consistently in bifurcation, length, and area ratios. Subsets shown are topologically nonrandom because they include only networks that are more elongated, or more compact, than median diameter.

nonrandom subsets of the original random set. I then test whether the Horton ratios of these topologically nonrandom networks deviate significantly from those of the topologically random networks.

In Figures 3 and 4, I divide the networks according to diameter, which is the number of links separating the outlet from its farthest first-order tributary (Smart, 1978). As Figure 3 shows, networks with larger diameters are topologically longer, whereas networks with smaller diameters are more compact. The Monte Carlo sample shown in Figure 2 includes ten networks for each number of first-order channels N_1. For Figures 3 and 4, I rank the ten networks for each N_1 in order of increasing diameter, then divide the ranked set in half. The two resulting sets of networks are mutually exclusive and markedly nonrandom; one set contains only networks that are more elongated than the median diameter for their N_1, and the other set includes only networks that are more compact than the median diameter.

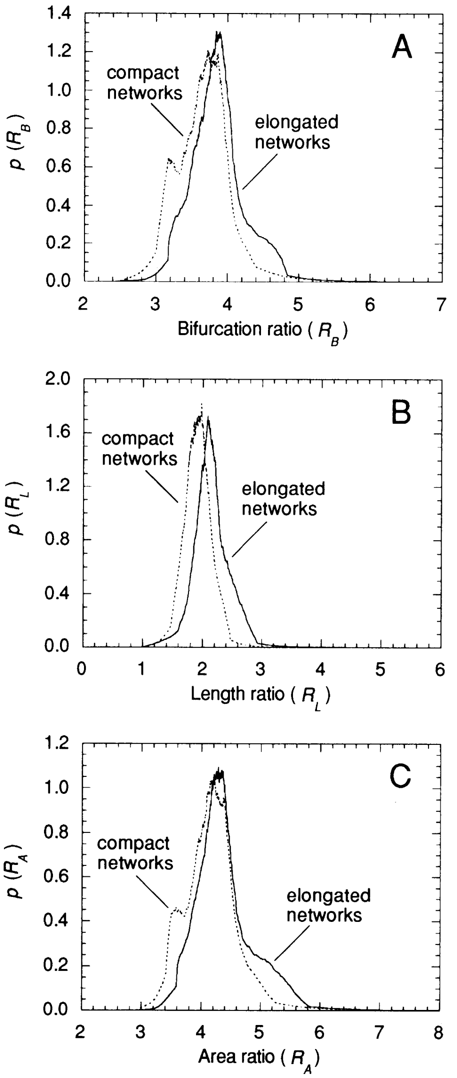

Figure 4. Distributions of bifurcation, length, and area ratios for topologically nonrandom sets in which each network is more elongated (larger diameter) or more compact (smaller diameter) than median for each number of first-order streams (see Fig. 3). Distributions for compact and elongated networks are almost indistinguishable from each other and from distributions for topologically random networks (Fig. 2).

Even though both the elongated networks and the compact networks clearly violate the premise of topological randomness, they both have distributions of bifurcation, length, and area ratios (Fig. 4) that are almost indistinguishable from those of topologically random networks (Fig. 2). The degree of similarity between these distributions can be quantified through the following thought experiment. Imagine one had a sample of networks drawn from either of the nonrandom distributions shown in Figure 4. How large a sample would be needed to detect a statistically significant difference between either of these distributions and the random set of networks shown in Figure 2? Table 1 shows that roughly 170 R_B values, 60 R_L values, or 280 R_A values from elongated or compact networks would be needed to reject, at $p = 0.05$, the hypothesis that the networks were actually random. Horton's ratios cannot readily distinguish between random and nonrandom sets of networks. Therefore, the fact that the "random model" successfully predicts Horton's laws and the observed Horton ratios does not demonstrate that natural stream networks are actually random.

DISCUSSION

The argument presented above has two central points. First, almost all possible networks have Horton ratios similar to those observed in studies of natural channel networks, so those ratios are a very weak test of theories of stream network structure. Second, because both random and nonrandom sets of networks yield essentially the same Horton ratios, the ratios observed in nature do not show whether natural stream networks are random. The generality of these results depends on the following three questions.

First, do link lengths and contributing areas change with order, contrary to the assumptions used here? Some studies of natural channel networks indicate that links in first-order streams are somewhat longer, on average, than those of higher order streams, and others suggest that they are shorter (Shreve, 1969; Smart, 1972; Montgomery and Dietrich, 1989). First-order link lengths are sensitive to the criteria used to define where first-order channels begin, and there are very few published measurements of link contributing areas. However, plausible variations in link lengths and contributing areas are unlikely to affect the results reported here. If first-order links were 50% longer than higher order links, on average, as some field data suggest (Smart, 1972), the length ratio (R_L) values reported above would be reduced by an average of only 0.1. Doubling the average contributing area to first-order links (Shreve, 1969) would decrease the reported area ratio (R_A) values by an average of only 0.2. The effects of plausible variations in link lengths and areas are small compared to the effects of the stream-ordering definitions, which geometrically compound the numbers of links in each order.

Second, do these results depend on the

TABLE 1. SAMPLE SIZE NEEDED TO DISTINGUISH NONRANDOM GROUPS OF NETWORKS

Networks	R_B*	ss†	R_L*	ss†	R_A*	ss†
Random networks	3.8 ± 0.4		2.1 ± 0.4		4.3 ± 0.6	
Nonrandom subsets selected according to:						
Diameter						
≥ median	3.9 ± 0.5	169	2.2 ± 0.4	61	4.4 ± 0.7	276
≤ median	3.7 ± 0.4	169	1.9 ± 0.3	61	4.2 ± 0.5	277
Width						
≥ median	3.6 ± 0.4	143	1.9 ± 0.3	77	4.2 ± 0.5	232
≤ median	3.9 ± 0.5	140	2.2 ± 0.4	75	4.5 ± 0.7	227
Mean source height						
≥ median	3.9 ± 0.5	190	2.2 ± 0.4	64	4.4 ± 0.7	382
≤ median	3.7 ± 0.4	190	1.9 ± 0.3	64	4.2 ± 0.5	383
Mean source height / diameter						
≥ median	3.8 ± 0.4	12719	2.0 ± 0.3	4906	4.3 ± 0.6	3290
≤ median	3.8 ± 0.5	12506	2.1 ± 0.4	4855	4.3 ± 0.6	3262

*Means ± standard deviations for Monte Carlo sample of 9800 networks evenly distributed from $N_1 = 20$ to $N_1 = 1000$, and for nonrandom subsets created by dividing random sample in half according to various topological criteria (see text).

†Approximate number of samples from nonrandom distribution that would be needed to reject ($p = 0.05$) hypothesis that samples were drawn from random networks, using Kolmogorov-Smirnov test for differences in distributions. Student's t-test is not used, because distributions are both skewed and leptokurtic.

number of first-order streams (N_1) in the networks that were analyzed? Because the likelihood of particular bifurcation, length, and area ratios varies with N_1, the details of the distributions shown in Figures 2 and 4 will vary somewhat with the range of N_1 sampled. However, repeating this analysis with widely differing N_1 ranges ($20 \le N_1 \le 1000$, $20 \le N_1 \le 200$, $20 \le N_1 \le 500$, $500 \le N_1 \le 1000$, and $20 \le N_1 \le 10\,000$) yields substantially the same results as reported above.

Third, are the results shown in Figure 4 sensitive to the particular topological criterion used to create the two nonrandom sets of networks? I have repeated the analysis described above using three other structurally important ranking criteria: (1) network width (Kirkby, 1976), the largest number of links equidistant from the network outlet, (2) mean source height (Jarvis and Werrity, 1975), the average distance separating first-order links from the network outlet, and (3) mean source height divided by diameter, which expresses the degree to which the networks in Figure 3 appear top-heavy or bottom-heavy. In each case, the results agree with those reported above; the Horton ratios of the nonrandom networks are virtually indistinguishable from those of the random networks (Table 1).

These results do not show that natural channel networks are actually nonrandom; they simply show that Horton ratios are poor indicators of whether or not networks are random. Other measures have revealed systematic departures from topological randomness in natural stream networks, even where geologic controls are absent (Smart, 1978; Abrahams, 1984). The "random model" remains particularly useful as an explicit null hypothesis; because it is parsimonious, its premises can be stated concisely, and its implications can be calculated readily. However, failure to reject this null hypothesis does not indicate that networks are in fact random, if the measures used (such as Horton ratios) are not sensitive to randomness in network structure.

This analysis shows that R_B, R_L, and R_A are profoundly indifferent to network structure. Because the stream-ordering rules create a particular hierarchy of separate streams in channel networks, they constrain R_B, R_L, and R_A to relatively narrow ranges, thus enforcing substantial uniformity in the derived stream statistics whether or not there is structural uniformity in the underlying networks themselves. My results support earlier intuitive arguments that Horton's laws must be an artifact of stream-ordering methods (Bowden and Wallis, 1964; Milton, 1966; Smart, 1978), by showing that few possible networks lie outside the usual ranges of R_B, R_L, and R_A, and by showing that these ratios are insensitive to marked changes in network structure. For example, the Horton ratios of the networks shown in Figure 3 vary little and do not reflect, in a consistent way, the obvious differences between elongated and compact networks. These ratios vary over such a small range (Fig. 2, Table 1) and are so insensitive to pronounced differences in network configuration (Figs. 3 and 4, Table 1) that it is unsurprising that they are also insensitive to prevailing geologic and climatic conditions (Smart, 1978).

These results do not imply that stream networks lack distinctive geometric or topological characteristics. Rather, these results show that if stream networks have unique structural features, those characteristics are unlikely to be revealed in the numbers, lengths, and areas of streams defined through the conventional stream-ordering rules. Factors that regulate channel formation, such as competition for drainage area (Dunne, 1980; Abrahams, 1984) and erosional thresholds controlling channel incision (Montgomery and Dietrich, 1989), should give rise to distinctive network patterns. Devising morphometric techniques to detect the characteristic structure of natural channel networks and explaining that structure in mechanistic terms remain central problems in quantitative fluvial geomorphology.

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