Review Comments of the "Evaluation of Sediment Transport Data for Clean Sediment TMDL's as prepared by Roger Kuhnle and Andrew Simon in ARS Report No. 17 of November, 2000.

By
Dave Rosgen, P.H.

Page 1. "The technique assumes that each of the stream types will have a unique dimensionless sediment transport rating that can be used to establish a baseline or reference condition"… This not a correct assumption… a "***reference***" condition assumes a natural stable channel (a river, over time, in the present climate, that is able to transport the sediment and flows of its watershed in such a manner that it's dimension, pattern and profile are maintained without either aggrading nor degrading…Rosgen, 1996). Stream types are a morphological description of the integration of variables that effect dimension, pattern, profile and channel materials**…** ***IT DOES NOT IMPLY STABILITY***. My database involving measured variables for over 500 rivers includes rivers that are stable as well as unstable for ***the same stream type.*** That is the reason that I included chapter 6 in my book (level III river assessment, Figure 1) that describes methods to determine stability and chapter 7 (level IV river assessment) that describes validation techniques to measure stability. One cannot use stream types (Level II river assessment) to determine stability. A correct assumption is that a departure in stability of a given stream type can be associated with a change in sediment supply and may be detected using sediment rating curves. For example for a river with measured sediment rating curves (Joe Wright Creek, 1979, In: Rosgen, 1996 pg. 7-10) changed stability due to highway and reservoir construction reflected in a shift in the sediment rating curves comparing above vs. below construction activities (Figure 2.). The stream became so impacted that the morphological types changed from a B3c to an F4 stream type.

Stream types have unique sediment rating curves, but changes in stability can occur within the same stream type, thus reflecting a shift in sediment rating curves due to a change in sediment supply from channel adjustment processes (such as bank erosion). The dimensionless sediment rating curve idea was to normalize the sediment rating curves for a given reference condition involving both channel stability and stream type. It was thought that departure might be detected when stability and or stream types would change to the degree that would be detected in a dimensionless sediment rating curve. In summary, two different rivers could be of the same C4 stream type, but if one was a reference river (stable), and the other unstable, then one would not expect to have a similar sediment rating curve for both rivers. If the departure from stability was significant, then possibly the dimensionless sediment rating curve may also reflect the change. Just because a stream is an F type does not imply that all F types are unstable. There are geologic, naturally stable F4 types (such as the Colorado River and Lower San Juan) that are not the same stability as an F4 that has recently evolved from an E4 to C4 to G4c To F4 in a meadow due to change in local base level from river straightening (Figure 3, pg 6-10, Rosgen, 1996). This is similar to the channel evolution model cited showing changes in evolution stage due to degradation. One would expect a change in sediment supply due to such an instability (disequilibrium) that caused a stream to go from stage I (E4) to Stage III (G4c), and to stage IV (F4) with such a corresponding change in channel morphology (stream type).

There was an incorrect assumption made in regards to the statement that some of the Rosgen classification types are inherently unstable and cannot be used for reference condition. First stability relations need to be assessed by stream type for potential departure comparison. For example in Alaska, below glacial outwash terraces, exist braided "D" stream type channels whose dimension, pattern, and profile have not changed and have neither aggraded nor degraded in the present climate. They are active in terms of scour and fill due to the convergence/divergence bed features, but are the stable form of river in balance with the landforms, valley types, sediment, and glacial-fed flow regime. These can be reference types for these geomorphic settings as they are operating in equilibrium. That is the reason that the conversion of stability ratings of the Pfankuch stability rating procedure was modified to include braided (D) streams whose values for Good could be up to 107, where a rating of 107 would be associated with a poor condition for a B4 stream type. That's why stability ratings need to be stratified by stream type…"one size" does not fit all rivers!

I agree with the use of a channel evolution sequence. I have referenced similarity between quantitative relations of changes in morphology associated with various stream types with the channel evolution model by Simon and Hupp, (1986) in an AWRA paper (Rosgen, 1999), Figure 4.

Troendle and several biometricians conducted statistical analysis involving over 160 rivers with concurrent suspended sediment measurements, bedload measurements, hydraulic data, stream classification, and stream stability where over 320 models were tested. One of the results (that could have been shared with the authors had they communicated with either Troendle or myself) showed a significant shift in the dimensionless sediment rating curve due to changes in stability comparing poor versus good/fair stability ratings using the Pfankuch rating procedure. Stream type, by itself did not show significant departures in the dimensionless suspended and bedload rating curves. Based on the previous discussion, without a stability analysis, stream type alone was never intended to infer stability and corresponding dimensionless ratio sediment rating curve shifts, unless the instability resulted in stream type shifts (as in the C4-G4-F4 discussion, Figure 4).

I agree with the need to include a biologic integrity evaluation as part of the TMDL process. This method only deals with the physical aspects of river and any associated physical habitat interpretations.

There are four papers that will be presented at the 7th Interagency Sedimentation Conference that are portions of the methodology being developed for a method of watershed/river stability assessment to meet clean sediment TMDL requirements: (A Practical Method of Computing Stream bank Erosion Rate, D. Rosgen), (A Stream Channel Stability Assessment Methodology, D. Rosgen), (A Hierarchical River Stability/Watershed-based Sediment Assessment Methodology, D. Rosgen), and

(Developing a "Reference" Sediment Transport Relationship, Troendle, Rosgen, Ryan, Porth, and Nankervis). A review of these papers will provide the basis for what the ARS report should have included in their evaluation. This information would have been made available to the authors had they requested it, or had I known that this report was forthcoming. I received the ARS report by request only upon third hand in mid February. These papers will be included in the appendix of my review documentation.

Page 3. 4th paragraph. The hypothesis quoted is incorrect…stability ratings by stream type to show departure was designed for statistical analysis…not stream type to infer stability and thus sediment rating curves. I have been collecting suspended sediment, bedload sediment, streamflow, and channel stability data concurrently with stream types since 1973. I am totally aware of the complexity influencing sediment rating curves, both from a temporal as well as a spatial distribution standpoint. The hypothesis for our research for procedures for clean sediment TMDL's is ***not*** as stated in this report.

Page 4. Watershed size was ***not*** a variable tested. The report states one of the objectives of this study is to evaluate the "Rosgen-Troendle" technique in physiographic provinces different than where the techniques was first developed. To properly test the procedure, a stable reference reach needs to be compared to a disturbed reach. There were ***no*** stable reference reaches selected for ***any*** of the two sites for ***any*** of the physiographic provinces to properly test departure. Any summary statements on how this procedure may work in areas beyond where it was developed would be mute since the correct procedure was not followed. A stable stream type in the same valley type and physiographic province (Sierra Cascade Mountains) could have been selected for reference if it had not been altered by the volcanic eruption in 1980, or if pre-volcano sediment data was available for the same reaches. Both of these streams were altered by the volcano and have been in an altered state of adjustment subsequent to the eruption. The Goodwin Creek data is associated with incised rivers from straightening and accelerated adjustment of base level and bank erosion. No reference reach sites were selected on Goodwin Creek for correct departure analysis. The total sediment data that was presented, was ***computed***…***not measured***. For evaluation purposes, computed values cannot be compared with a procedure that used ***measured*** data. The data from sediment rating curve slopes shown in Figure 18 for West Tennessee sites and Goodwin Creek should have had data selected from a stable stream of the same type (Stage I) for reference condition to properly evaluate the sites chosen at level III and IV/V. Since I am very familiar with the West Tennessee sites, and they are also on the list shown in Table 7. Why wasn't a Stage I site selected for a reference condition to compare departure for stage III and IV for these river systems? I conducted an analysis of the West Tennessee sites that were shown in Figure 18 and listed in Table 7, since sediment rating curve data was available. I had previously conducted detailed inventory on these sites collecting stream type, channel stability, and bankfull calibration at the gage stations where the sediment data was collected. In reviewing a 1989 paper by Simon on "The Discharge of Sediment in Channelized Alluvial Streams" I recognized the same reaches that I had previously collected river morphology, stability and bankfull discharge. The stable reference reach for this evaluation was the Hatchie River, an E5 stream type with a Pfankuch stability rating of 74 (good). The riparian vegetation is extensive and this reach was not channelized. The

E5 stream type corresponds to an evolutionary stage I category that was similarly described (evolutionary stage I) in the 1989 paper by Simon. An incised reach in the same hydro-physiographic province/basin due to channelization was the South Fork of Forked Deer River, which also had suspended sediment data. This reach was an F5 stream type with a channel stability rating of 140 (poor). This corresponded to an evolutionary stage IV category that was also similarly described in the 1989 paper by Simon. According to Simon (1989), the average suspended sediment yield was 163 tons/square mile/year from the non-channelized Hatchie River (Stage I), compared to 2,490 tons/square mile/year for the South Fork Forked Deer River (Stage IV). Also the Simon paper (1989) had documented that the Hatchie River has exhibited a stable profile during the period in which modified West Tennessee channels were undergoing drastic morphologic changes. Thus it appears that the Hatchie River is a good choice for a reference stream. Suspended sediment rating curves (from USGS data) are shown for both the Hatchie River and South Fork Forked Deer River (Figure 5). (Note: To match the slopes of the published sediment rating curve data, mg/l had to be converted to tons/day for both sites. The slope of the sediment rating curve in mg/l for the Hatchie River was basically flat. The sediment rating curves in Simon's, 1989 paper were converted from mg/l to tons/day-see figure 2 in the Simon paper). The relations in Figure 5 reflect the large differences in slope of the sediment rating curves that were described in Simon (1989) and in Figure 18. Bankfull discharge was then obtained for both rivers and dimensionless ratio sediment rating curves were established for both measurement sites (Figure 6). The conversion to dimensionless ratio sediment rating curves ***did not*** "mask" or "collapse" the differences between these streams. A statistical test was conducted on the dimensionless relations to determine if these two streams were statistically significantly different from one another. The statistical results showed a highly significant difference at the 0.95 level. A p value was obtained of .00000029. This means that the dimensionless sediment rating curve by stability/by stream type/stage of evolution showed a significant departure from the reference stream. There is a statistically significant difference in the dimensionless ratio sediment rating curve for a stable E5 stream type versus an unstable F4 stream type. I had called Andrew 9 months ago reporting my initial findings, thinking that these sites could be selected for a "test" of the methodology. Unfortunately, I never heard back from Andrew.

Page 5. There is a misunderstanding of the stream classification system based on the statement that gradient, as a delineation variable is included to **a lesser degree**. This is **not true**. Gradient is a critical variable and is included at the initial level of delineation (Level I). Level I does not include particle size, however, the level appropriate for this purpose in level II, includes channel materials. The processes of channel form influenced by sediment are integrated into the morphology of rivers described by stream type, yet changes in sediment supply (availability) have to be identified more specifically by stability analysis (level III, Figure 1). This fact seems to have been overlooked in the discussion in paragraph three and four.

The statement that "channel evolution, being a process-based classification scheme should be used rather than stream types" does not make sense to me. The channel evolution model describes a series of channel changes due to incision and adjustment processes the channel undergoes to eventually reach quasi-equilibrium. There are no measurements that describe these changes, nor quantitative descriptions. Stream classification is not a substitute for channel evolution, but rather a quantitative morphologically based process system for describing the changes the channel undergoes in its metamorphosis. Also I have identified multiple (not just one) scenarios of channel evolution (Rosgen, 1999, 2001). Modifications of the most recent scenarios are shown in Figure 7, where 9 different morphological scenarios are identified. The status of channel evolution and stream classification should be complimentary, not conflicting inventories.

Page 6. There seems to be some confusion on how the stability rating and stream type may be used for extrapolation where transport data is lacking. The use of any stability rating is to correlate with measured sediment rating curves data by stream type, and then be converted to dimensionless ratios for extrapolation. To actually use such an extrapolated curve, measured values of sediment and discharge need to be obtained at the bankfull stage. These values are then multiplied with the dimensionless values in the rating curve, converting to actual sediment (dimensioned) data. Additional data at low flow can verify the slope and intercept values as predicted from the dimensionless rating curve by stability by stream type.

A semi-quantitative empirically based ranking was suggested as an alternative to the Pfankuch scheme. In review of this scheme I generated the following initial questions:

1. Since bedrock ranks 0, boulder/cobble ranks 1, gravel ranks 2, sand ranks 3 and silt/clay ranks 4…does this mean that I cannot have a stable channel in silt/clay? Or that bedrock or boulder/cobble are the only stable channel sizes? I have a lot of reference reach (stable) data that are sand and silt/clay channels.
2. I was totally confused by the reference elevation percentages of incision???
3. Degree of constriction (change in width from upstream to downstream) assumes no change in stream type or width/depth ratio. If the stream type changed in the downstream direction from a C to E stream type this would be called a constriction and ranked with higher points for instability, yet this would not be associated with instability.
4. Why, in a 4 point spread, would a channel evolution stage of III (actively incising gulley) be only 0.5 points different than Stage VI (quasi-equilibrium)??? Since this is a strong emphasis for your stability, why such a **little** emphasis (0.5 pts) for such a **big** shift in stability??
5. Streambed exposure is a flat percentage…wouldn't these vary by river size or river type?
6. The riparian zone widths are ranked by total widths. Wouldn't this vary by bankfull width ? Not all small streams should be discounted if they don't have a riparian width greater than 20 meters.
7. A sinuosity of 2 is very sinuous, but only gets half the potential points ?
8. Why are rivers with more than 80 per cent pools rated better than 50 per cent pools obtain half the points? Shouldn't this be based on meander geometry, gradient and bankfull width? Many rivers may be at their potential, but unless they have at least 80 per cent pools, they will graded down.

If this is the stability and ecological system that is proposed, I suggest you have several trained, experienced field observers test this method. Perhaps with more documentation provided, my questions would be answered. How is this scheme related to sediment rating curves and processes of instability.

Page 7. The interpretation that using bankfull as a normalization variable to create dimensionless sediment rating curves obscures all differences at bankfull is a misunderstanding. The dimensionless ratio sediment rating curves are used at more detailed levels of assessment where actual data is available to test for departure from reference stable conditions. It can be used for extrapolation where ***measured values at bankfull*** can be used to convert dimensionless ratios back to actual values. This will not obscure the bankfull values; rather establish them for a particular river in a unique lithology and for rivers of different size, but of the same stability and stream type. This is the part that needs additional testing over a wide range of river types and river stability throughout the U.S. If one wants to observe actual sediment values by stream type and stability then converting back to dimensioned data would be required.

I agree with the need to estimate the potential for sediment transport. That is included in the stability analysis procedure included in the channel stability paper presented at the FISC (see appendix).

Page 9/10. If high water conditions at the Toutle River sites permitted only one cross-section per site, how was the thalweg profile obtained on the same sites (deepest water wading) and appropriate pebble counts made? The pebble count discussion also indicated a misunderstanding of field procedures. The statement was made that… "This method has the conceptual flaw of defining the average or median particle size (D_{50}) statistics…combining bed and banks…"A channel with silt/clay banks and gravel bed may have an identical D_{50} to a channel composed completely of sand." The average size and median are ***not the same and are not interchangeable*** in the method…the median size is used with the exception of a bi-modal distribution. When a bi-modal distribution occurs, then the procedure used is documented on page 5-26 in my book (Rosgen, 1996), where the dominant size is obtained from the sample population that constitutes the majority of observations for a broad size grouping. This avoids the potential problem presented by these authors. Also in my book, P.5-27, it is described that the bank material can be kept separate from the bed material in the form and corresponding graph shown in Figure 8. This avoids any potential problem of selecting a dominant size that may not even be present in the channel as posed by the authors. For purposes of relative roughness calculations, it is standard to only include the active channel bed size distribution. For stream classification, the boundary of the bankfull channel indicates materials available for transport including banks. The normal systematic sampling method (equal spacing between samples ***per unit width***) prevents disproportionate sampling. In other words, the amount of exposed width of stream channels have a higher percentage of area of bed than the streambanks up to the bankfull stage. In review of the field work done as summarized in Table 6, there are some serious errors or inconsistencies that are evident. As a minimum, 100 samples are required based on a stratified-systematic sampling procedure. The streambanks consistently make up a low percentage of the samples since they make up a lower proportion of the width of the channel. However the samples obtained in this study had the following percentages of the sample obtained from the ***streambanks*** compared to the total sample: Kid Valley: **42%,** Tower Road**: 49 %,** Goodwin Creek 2: **71 %,** and Goodwin Creek 5: **85 %.** These results are highly irregular and inappropriate for representative pebble count and corresponding dominant channel material selection.

Page 11. The dimensionless sediment rating curves for all 4 sites are ***incorrect***. I calculated what the values should be and discovered the errors. The values in Figure 13 should be switched so that Tower Road should read Kid Valley and visa versa and Goodwin Creek 2 should be labeled Goodwin 5, and visa versa. The same problem exists in Figure 14 of the ARS report. In the conversion back from dimensionless to actual values a result in a major switch of the slopes and intercept of all of the sediment rating curves would occur.

Goodwin Creek would have classified as a C5 rather than E5 stream type. Based on the continuum of channel form, the system allows for an adjustment of +/- 2 units of width/depth ratio (see Key to classification, page 5-6 in Rosgen, 1996). Since Goodwin Creek No. 5 has all of the characteristics of a C5 and is within the width/depth ratio adjustment of 2.0, (presently 11), the stream would be classified as a C5. It appears that this stream reach is still trying to increase its belt width to establish a wider floodplain. It may have recently evolved from an F5 stream type. The rest of the classifications would need to be described at level II as F3 or F4 rather than just "F" (that's why a correct pebble count should be done).

Page 12. Total load sediment rating curves, to be compared to either reference reach or altered states in this method should be ***measured-not calculated values***. It is difficult to compare when error terms associated with calculated transport is added to total load, making potential differences due to calculation rather than actual transport. This is not a proper test for evaluation of our method. Sites should not only be selected to test that have a true "reference reach" but where suspended sediment and bedload are ***measured*** for a wide range of flows.

I suspect the reason that the Goodwin Creek 2 (1981-1982) and Toutle River at Kid Valley (1987-1988) rating curves collapsed when made dimensionless, was that they were not statistically significantly different from one year to another, (in other words no change in stability and/or sediment supply). This was consistent with our findings as well. For the dimensionless rating curves to depart, there has to be a significant change in sediment supply (availability) due to changes in stability. The example presented in my review of the contrast of the Hatchie River (E5, stable) to the South Fork Forked Deer River (F5, unstable) showed that the dimensionless sediment rating curves ***did not collapse*** because there were significant differences in the rating curves. Based on the problems, errors and inconsistencies in properly applying and evaluating this system, the statement made by the ARS report that this method is "flawed and we do not advocate its use" is unsubstantiated. We need to have rigorous scientific review to evaluate the dimensionless sediment rating curve approach in many other areas…but it should be a fair, thorough, unbiased test.

The conclusion topic relating to rejection of the hypothesis in that the "Rosgen Classification is related to form rather than process" needs to reconsider that <u>channel form results from an integration of many physical processes that mutually adjust</u> including size of sediment, concentration of sediment, streamflow and flow hydraulics. Variables that effect sediment supply can be associated with channel adjustments due to channel instability leading to a change in morphologic stream types. In other words stream type (based on morphology, and the physical process interactions) is a dependent variable in the sediment relations. Stream types are used in this method primarily to stratify differences that exist in sediment and stability condition indices for comparative purposes (comparative to the reference condition of the stable stream type). The stability assessments are necessary to determine rate, magnitude, direction and consequence of change and cannot be separated as part of this method.

Page 14. I don't understand how Goodwin Creek 2 (F5 stream type) and Goodwin Creek 5 (C5 stream type) are at the same stage of evolution (V). What are the variables that can be measured to verify what stage the stream is? Isn't an F (no floodplain with active lateral erosion, a high width/depth ratio, low entrenchment ratio, low sinuosity) more associated with a level IV? It appears that Goodwin Creek 5 may still be evolving from one stage to another, but continues to have a high sediment supply.

Page 15. How would the channel stability index be used to relate to sediment supply/sediment transport data? It is not a correct assumption that the channel evolution model (Stage I-VI) is appropriate for all altered streams. I agree with the six stages you describe for incised river systems, but there are many other scenarios that need to be included (Figure 7).

A geomorphic threshold of change that shifts sediment rating curves, channel stability and even stream type can exceed a clean sediment TMDL, without understanding toxic limits to fish. I agree we need to continue research to determine biological thresholds. We have tried to connect channel instability with physical habitat to provide a connection with impairment of designated uses (fisheries) until there may be sufficient data to quantify sediment levels damaging to different species and their life cycles.

Page 16. What are the limits of substrate movement allowable for different streams? Does bed mobility infer instability or adverse biological effects? Several of our stable reference reach sites have mobile beds, are stable and have a good fishery. I use a procedure for competence calculation to evaluate stability, but it generally results in entrainment of particles larger than the D_{50} of the bed material.

Summary

The conclusions resulting from "testing" the Rosgen-Troendle technique should be rejected until proper testing and evaluation is conducted following the correct method with a data base of sufficient size to show statistical validity. Any alternative method should stand the same field tests and statistical rigor of previous efforts.

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APPENDIX

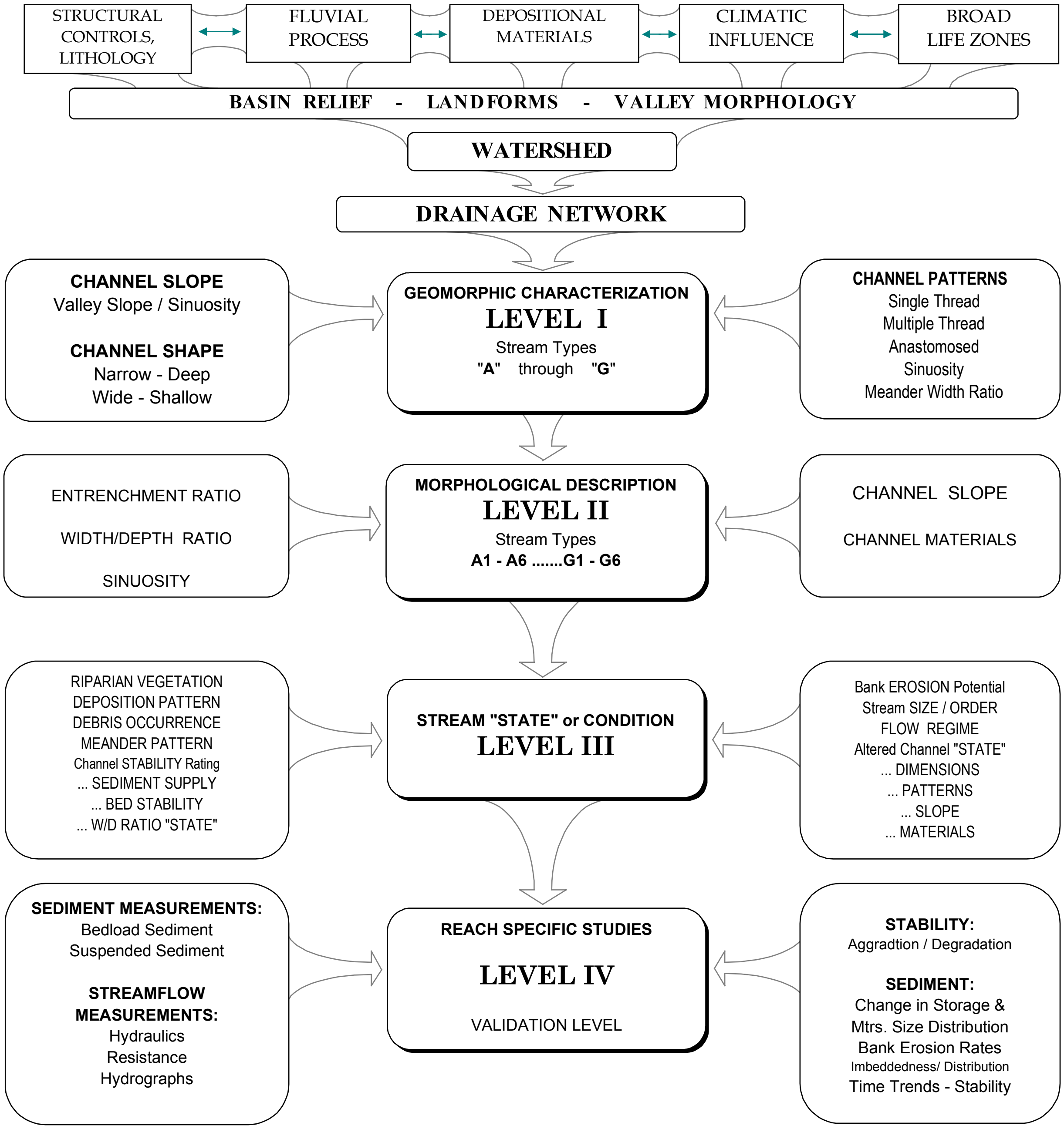


Figure 1. The hierarchy of river inventories showing various levels of investigation (from Rosgen, 1996)

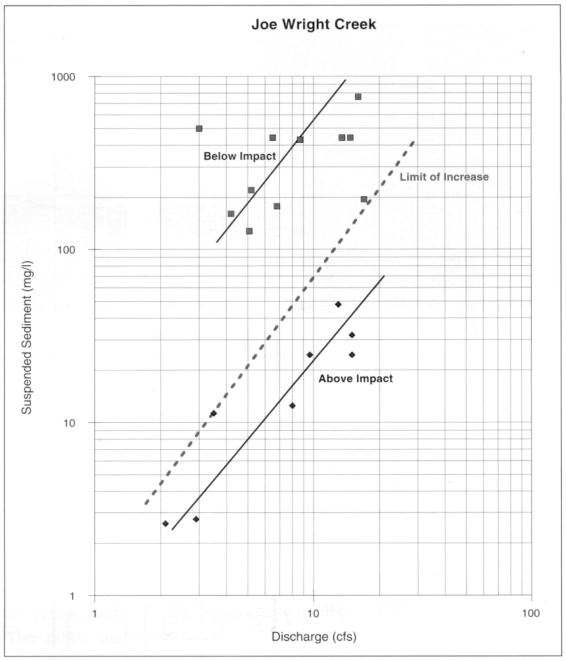


Figure 2. Example of sediment rating curve shifts on Joe Wright Creek, Colorado comparing above vs below impacts from construction (from Rosgen, 1996)

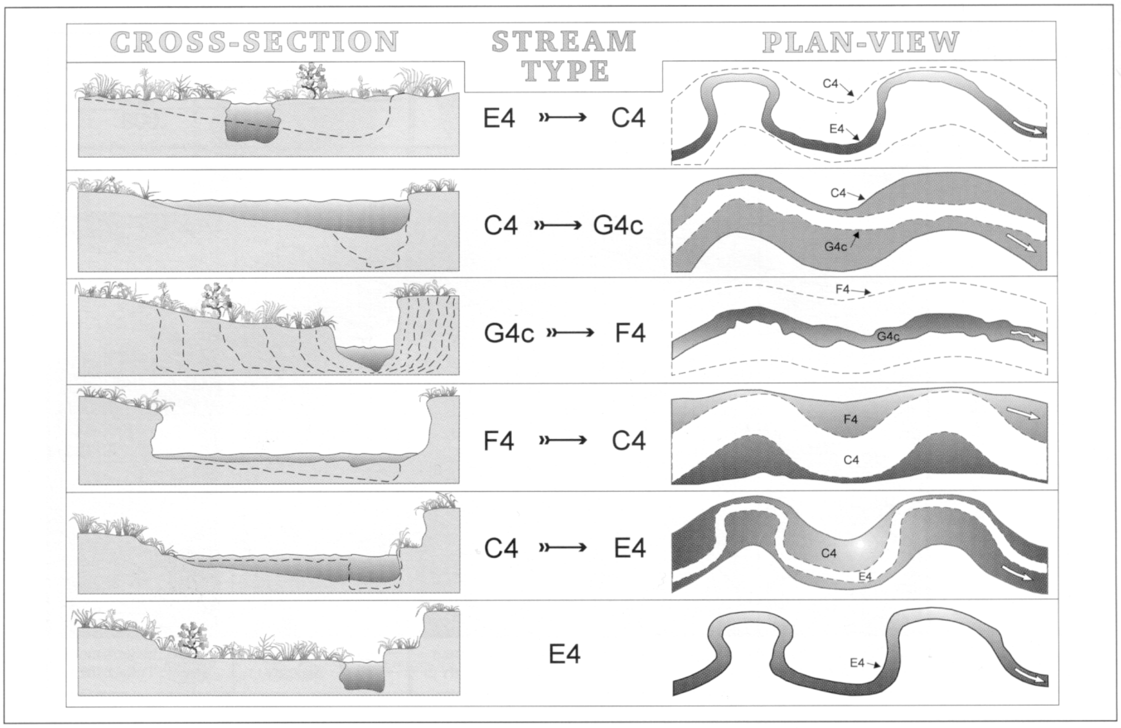
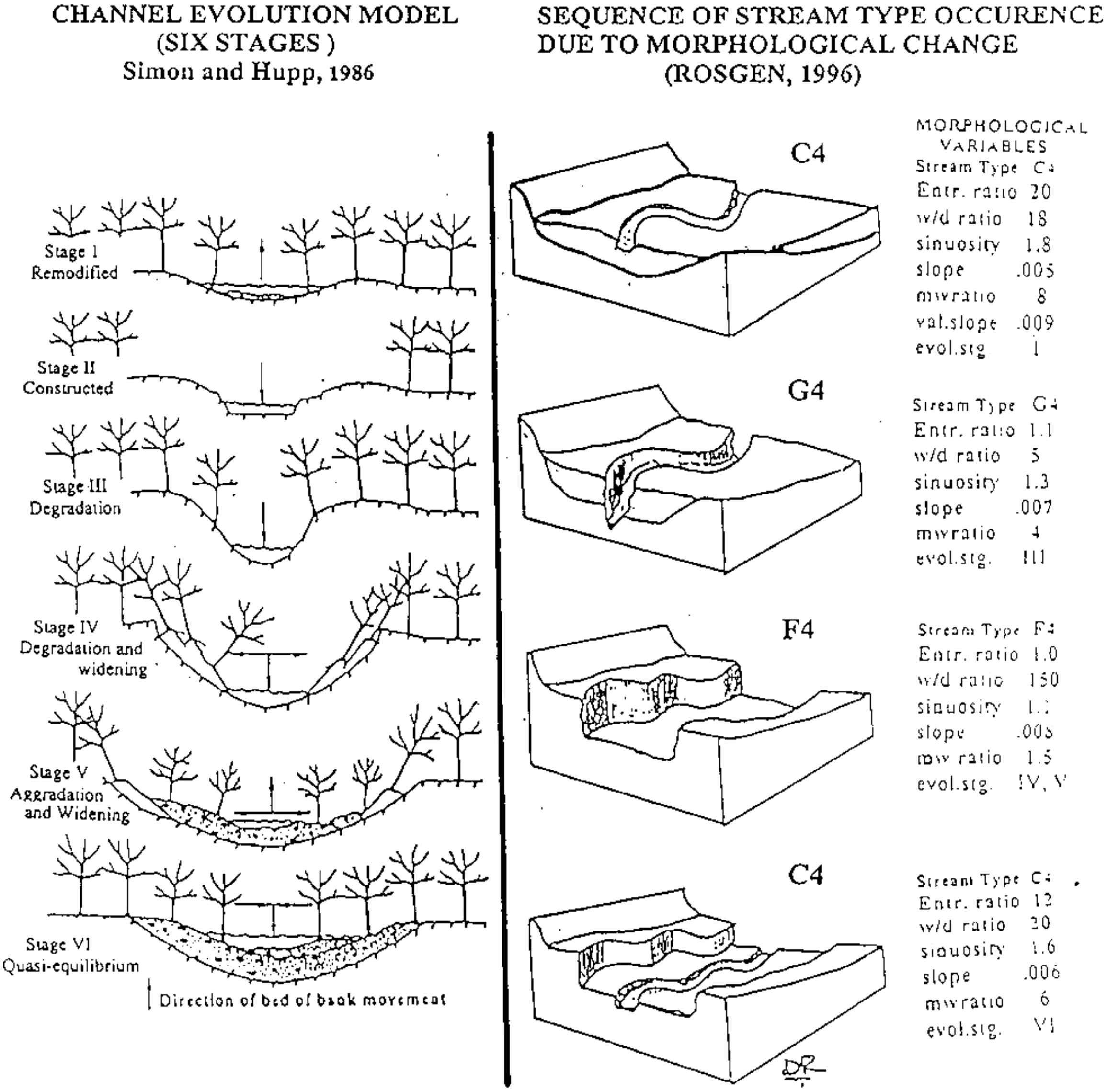


Figure 3. Adjustments of channel morphology leading to stream type changes through an evolutionary cycle (from: Rosgen, 1996)



Stream Type	C4
Entr. ratio	20
w/d ratio	18
sinuosity	1.8
slope	.005
mwratio	8
val.slope	.009
evol.stg	I

Stream Type	G4
Entr. ratio	1.1
w/d ratio	5
sinuosity	1.3
slope	.007
mwratio	4
evol.stg.	III

Stream Type	F4
Entr. ratio	1.0
w/d ratio	150
sinuosity	1.2
slope	.006
mw ratio	1.5
evol.stg.	IV, V

Stream Type	C4
Entr. ratio	12
w/d ratio	20
sinuosity	1.6
slope	.006
mwratio	6
evol.stg.	VI

Figure 4. Application of channel evolution model (qualitative) to a morphological stream classification system (quantitative) (from: Rosgen, 1999)

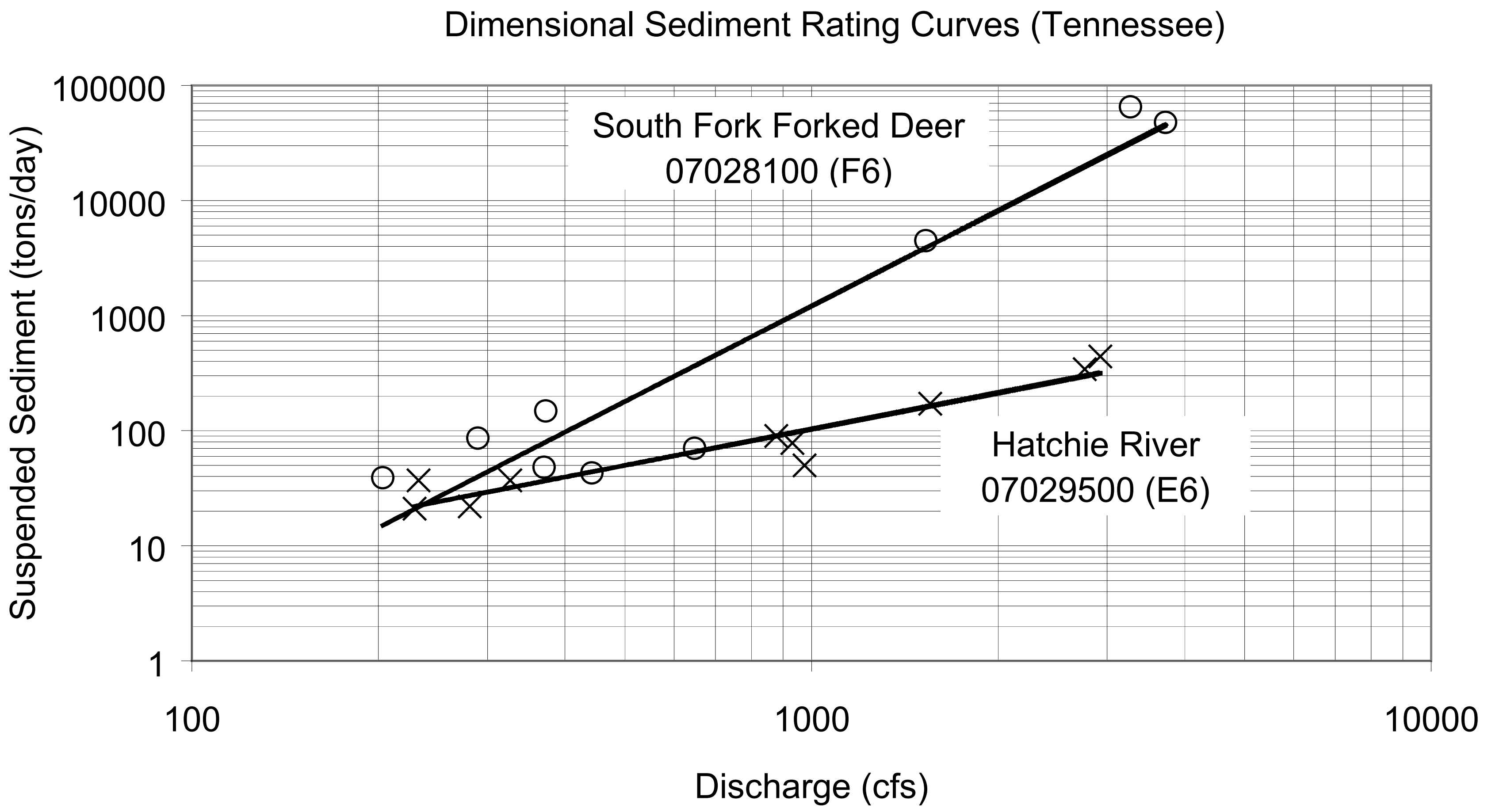


Figure 5. Dimensional suspended sediment rating curves for the Hatchie River (Reference Reach, E5, stable stream type) and for South Fork Forked Deer River (an altered unstable F5 stream type)

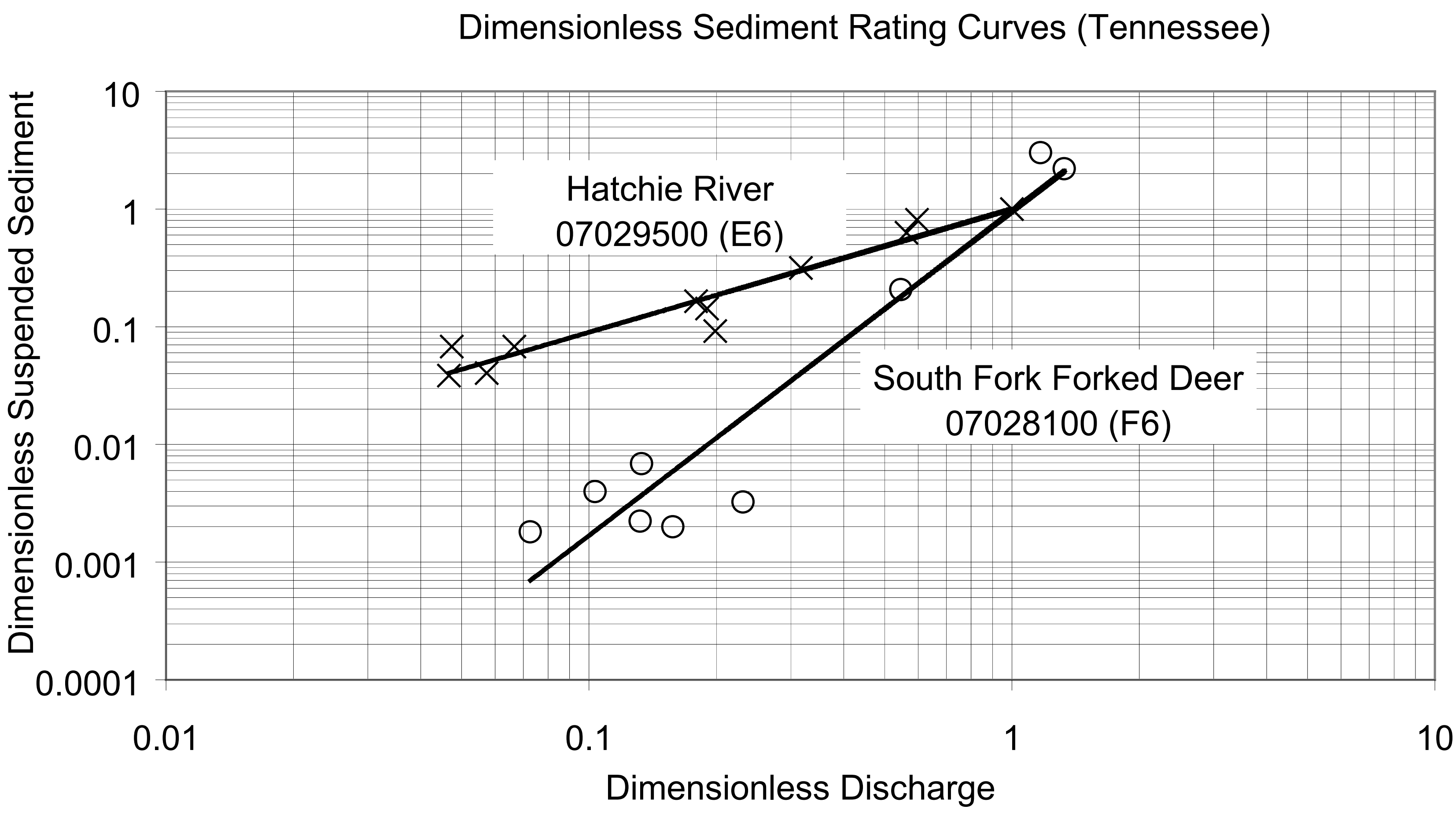


Figure 6. Dimensionless ratio sediment rating curves for the Hatchie River (a reference reach, stable E5 stream type) and for South Fork Forked Deer River (an altered unstable F5 stream type)

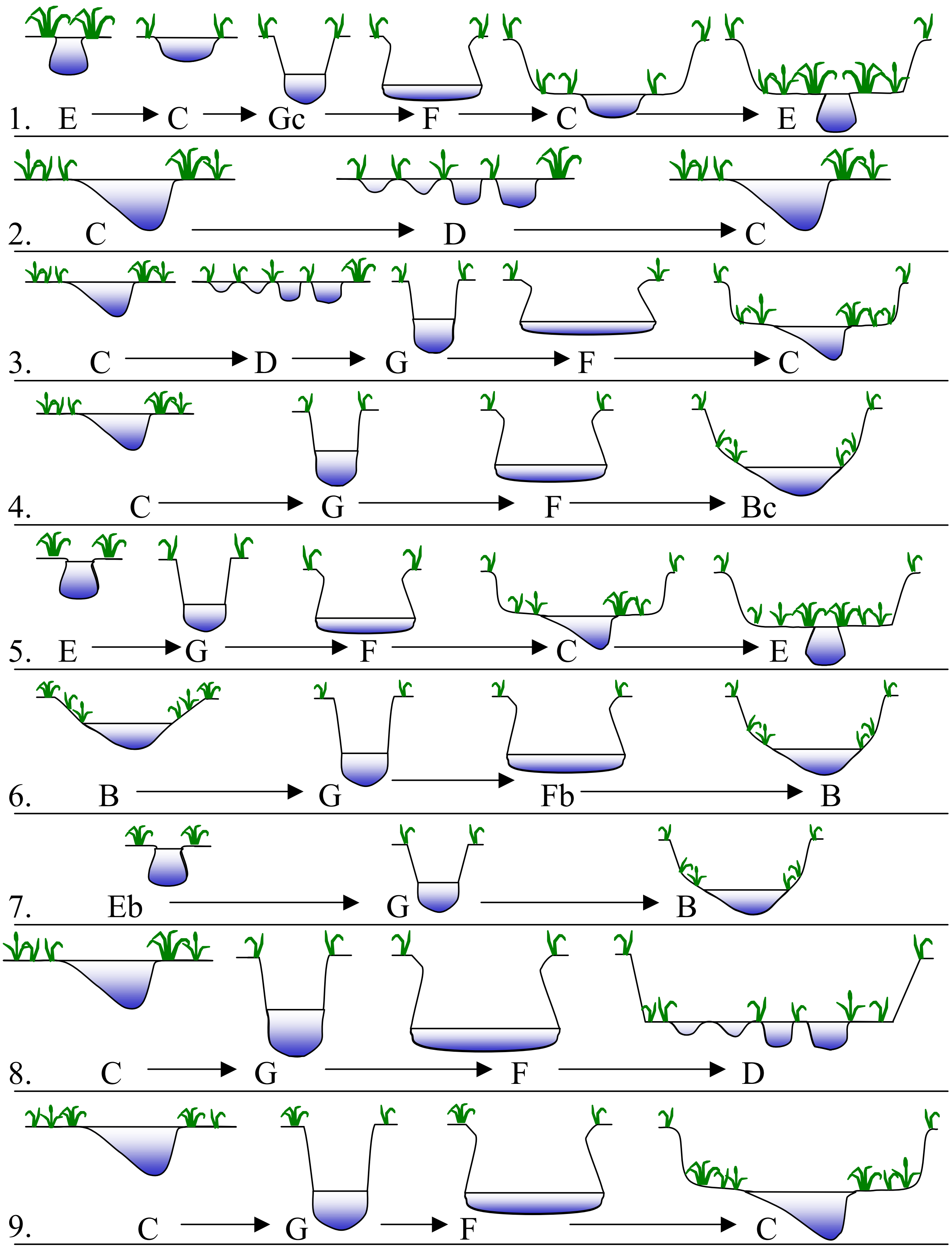
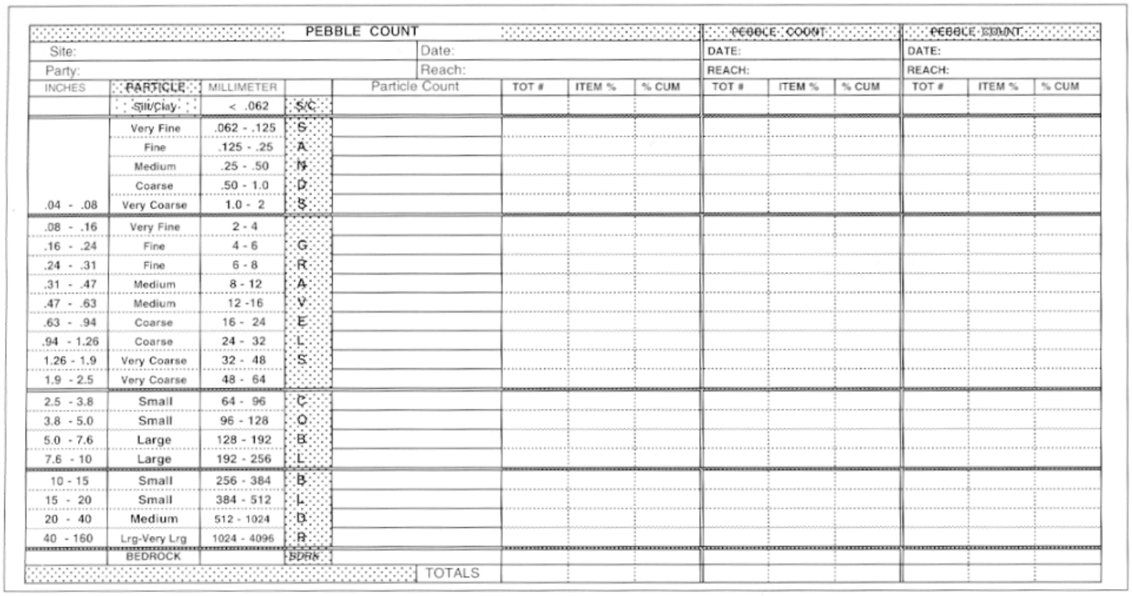


Figure 7. Various stream type evolution scenarios



PEBBLE COUNT								PEBBLE COUNT			PEBBLE COUNT		
Site:				Date:				DATE:			DATE:		
Party:				Reach:				REACH:			REACH:		
INCHES	PARTICLE	MILLIMETER		Particle Count	TOT #	ITEM %	% CUM	TOT #	ITEM %	% CUM	TOT #	ITEM %	% CUM
	Silt/Clay	< .062	S/C										
	Very Fine	.062 - .125	S										
	Fine	.125 - .25	A										
	Medium	.25 - .50	N										
	Coarse	.50 - 1.0	D										
.04 - .08	Very Coarse	1.0 - 2	S										
.08 - .16	Very Fine	2 - 4											
.16 - .24	Fine	4 - 6	G										
.24 - .31	Fine	6 - 8	R										
.31 - .47	Medium	8 - 12	A										
.47 - .63	Medium	12 -16	V										
.63 - .94	Coarse	16 - 24	E										
.94 - 1.26	Coarse	24 - 32	L										
1.26 - 1.9	Very Coarse	32 - 48	S										
1.9 - 2.5	Very Coarse	48 - 64											
2.5 - 3.8	Small	64 - 96	C										
3.8 - 5.0	Small	96 - 128	O										
5.0 - 7.6	Large	128 - 192	B										
7.6 - 10	Large	192 - 256	L										
10 - 15	Small	256 - 384	B										
15 - 20	Small	384 - 512	L										
20 - 40	Medium	512 - 1024	D										
40 - 160	Lrg-Very Lrg	1024 - 4096	R										
	BEDROCK		BDRK										
				TOTALS									

TABLE 5-4. Field form for documentation and analysis of Pebble Count Data

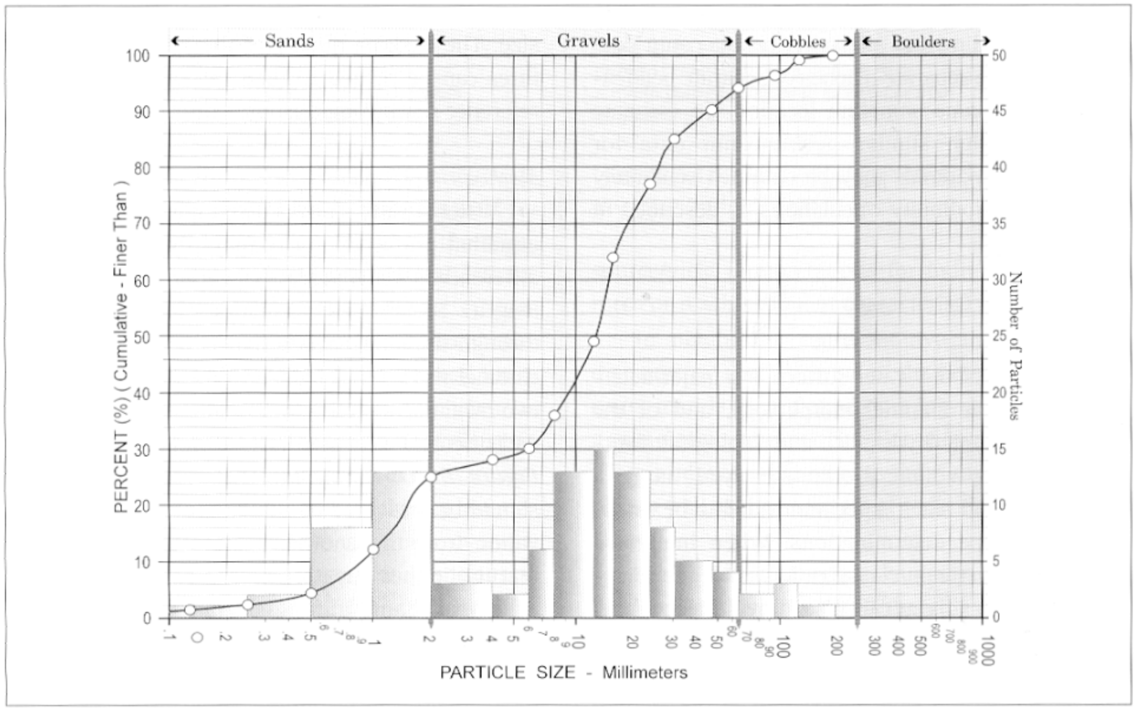


Figure 8. Example of pebble count form and plotting procedures