The Origin(s) of the "Caesarean" Text

Eldon J. Epp's famous essay of thirty years ago on the twentieth-century interlude in textual criticism has decried the lack of progress in understanding the theory and history of the New Testament text since Westcott and Hort,[1] and the rise and fall of the so-called "Caesarean" text-type is a case in point. The twentieth century began confidently with the work of Lake and Streeter, in identifying a group of seemingly related manuscripts consisting of the Washington Codex W, Codex Koridethi Θ, family 1, family 13, 28, 565, 700, and the text of Origen when he lived in the city of Caesarea.[2] As the century wore on, however, the expansive notion of a Caesarean text-type disintegrated in the light of additional scrutiny, particularly in the work by Hurtado, who identified various methodological failures among its proponents.[3]

Textual criticism, of course, is not limited to the field of the New Testament, and different methods have been developed in editing other texts. For example, stemmatics is a method commonly used in classical textual criticism to edit classical texts, in which the surviving witnesses to a text are organized into a family tree or pedigree called a *stemma codicum*, typically by recognition of common errors.[4] The stemma explicitly represents a hypothesized historical development of the text from an *archetype*, the most recent ancestor of all the extant witnesses. The textual critic reconstructs the archetypal text by selecting those readings from the variant carriers closest to the archetype based on internal evidence. The resulting archetypal text is then examined for primitive errors and conjecturally emended, if necessary, to produce a critical text. Although stemmatics has been a popular approach in classical philology, the vast number of manuscripts and the occurrence of mixture have confounded attempts to apply stemmatics broadly to the text of the New Testament.[5]

It is my view that rational eclecticism has produced a very good text of the New Testament for the past century and a quarter, but it has taken us about as far as we can reasonably go without an explicit history of the text. Nevertheless, I believe that recent advances in analogous fields, especially the field called *cladistics* among computational biologists, have finally made it possible to produce a history of the text on the scale of a manuscript tradition as complex as the New Testament and allow us to apply stemmatics to editing the New Testament. If so, we may finally have found our way beyond the Interlude.

Part I of this paper addresses the history of the investigation into text-types with particular emphasis on the Caesarean text, because most of the methodological problems involving text-types have been implicated by the Caesarean text. Part II of this paper explains the theory, practice, and limitations of cladistics, and explores some proposals for modifying cladistics to handle the mixture of different textual lineages. Finally, Part III applies my modified cladistics method to a large section of Mark, the so-called Bethsaida section, Mark 6:45–8:26, and discusses the resulting stemma of 58 witnesses.

To cut to the chase, the results of my investigation suggest that many of the so-called Caesarean texts do constitute a genetic group, more closely related to a branch of the Western text-type containing Codex Bezae and the *Itala*-type Old Latin texts than to the Alexandrian text-type. Its best representatives are family 1 and codex 28, and this group also includes *k*, an Old Latin *Afra* text, plus one of the ancestors for the Byzantine text-type. Origen's text in this section of Mark also belongs to this group, and, given family 1's connection to the library in Caesarea,[6] this group can plausibly be called "Caesarean." Contrary to Streeter, Θ and W are not this group's leading members, but poor, rather impure representatives. The text of W is very divergent, showing more scribal changes than any other witness in this study except for the Old Latin Codex Colbertinus *c*. Codex Θ, along with 565 and 700, is a member of a small sub-family that has been considerably assimilated to a Bezan-type text. Even in this sub-family, 565–not Θ–is the leading member.

I. The History of the So-Called Caesarean Text-Type.

The Caesarean text, of course, did not exist in the seminal theory of the New Testament text propounded by Westcott and Hort in 1881.[7] Many of the most important members of the Caesarean text, P45, W, and Θ were yet to be discovered and, among the four forms of the text they identified (neutral, Western, Alexandrian, and Syrian),[8] many of the other Caesarean members—namely, 565, family 1, family 13, 22, 28, and 157—were merely considered to support Western readings.[9]

In their theory of the text, Westcott and Hort "emphatically" endorsed the role of genealogy: "ALL TRUSTWORTHY RESTORATION OF CORRUPTED TEXTS IS FOUNDED ON THE STUDY OF THEIR HISTORY, that is, of the relations of descent or affinity which connect the several documents."[10] This genealogy is discovered by applying the principle that, apart from accidental coincidence, "identity of reading implies identity of origin."[11] This formulation is actually incomplete and potentially misleading, because Westcott and Hort properly disregarded agreements in original readings and used scribal errors and other forms of alterations in identifying their textual groups. A more careful formulation of the genealogical maxim was the "old rule" quoted by Kirsopp Lake: "community of error implies unity of origin."[12]

As famously observed by E. C. Colwell, Westcott and Hort did not actually employ the genealogical method to its fullest extent; they did not prepare any stemma of the New Testament textual tradition.[13] The reason for this, of course, was mixture, which, if not taken into account, threatens to invert the genealogical relations and distort the stemma. For example, in FIG. 1(a) there are two lineages A-B-C and D-E-F, with mixture from C to F. If mixture is not accounted for, and if the connection between C and F looks strong, the stemma in FIG. 1(b) may be erroneously inferred, in which the genealogical relations among D, E, and F are inverted. Westcott and Hort, therefore, approximated the history of the text by relying on a few, well-chosen "best documentary representatives" for the different forms of text that they found.[14] Among their four forms of text, they considered the neutral text, best exemplified in Codex Sinaiticus (01) and especially Codex Vaticanus (B), as the purest form of the original text, while the Syrian (now called "Byzantine") text was viewed as the latest, being a recension of the previous three forms.[15]

About twenty years later, the first major step toward the formulation of the Caesarean text was taken by Kirsopp Lake.[16] In his edition of Codex 1 and its allies, Lake modified the old text-critical rule that "community of error implies unity of origin" to account for mixture from Byzantine manuscripts. Specifically, Lake argued that genealogical relations for late manuscripts "can be deduced, in the absence of direct information, by studying the variations from the standard text which they share in common."[17] This standard text, of course, is the Byzantine text. In practical terms, however, Lake used the *Textus Receptus* as the standard of comparison when he edited the text of four members of family 1, recognizing that the unusual readings of the *Textus Receptus* have to be noted.[18]

After working out a stemma for family 1, Lake proceeded to consider the family's connections with other manuscripts. Even though this phase of the investigation evaluated readings in early manuscripts, Lake continued to exclude any reading that agrees with the

Byzantine text, worried that Byzantine contamination could still have affected the archetype of family 1. Among the remaining non-Byzantine readings, Lake also excluded those readings which were found in almost all of the witnesses because such readings were probably original and therefore not indicative of "the special affinities of the text of *fam*[1]."[19] Then, Lake compared the uncommon, non-Byzantine readings in family 1 with other leading witnesses and found that while family 1 seems to have "a more definite connection with the Old Syriac," there is a "close connection between *fam*[1] and *fam*[13] 22 28 565 700."[20] Lake concluded that these manuscripts belong to a "larger family [that] seems to represent a local text or local texts which were current in a comparatively limited region in the East."[21]

Lake's justification for filtering out Byzantine readings may seem reasonable when working out the relationships within family 1, because one source of accidental coincidences in readings is independent assimilation to the current standard text. The standard text was probably memorized by monks who have spent a greater part of their lives studying, reciting, and copying the gospel texts. In fact, Silva Lake reported: "In collating aloud in Greek monastic libraries I have found that the librarian most almost always breaks in with a correction when I have read from a ms. a phrase which differs from that which he is accustomed to hear."[22]

Lake's reasoning for excluding Byzantine readings when assessing the affinities of family 1 with non-Byzantine texts, however, makes less sense because many of those texts were not contaminated by Byzantine mixture so the risk of over-counting accidental coincidences should be lower. One wonders whether there was a more pragmatic basis underlying Lake's decision: manuscripts were routinely collated against the *Textus Receptus*, and it actually requires additional work to include rather than exclude Byzantine readings. In other words, the analysis may have been made to fit the form in which the evidence was collected. Regardless of the demerits of Lake's first step, Lake's second step properly excluded original readings from being used to erect groups, and Lake thus arguably found a limited community of error implying a common origin for family 1, family 13, and manuscripts 22, 28, 565, and 700.

Lake's alliance was expanded over the ensuing decades as new manuscripts were discovered and published. For example, in 1923, Lake and Blake added Θ, the Koridethi Codex, to this alliance, and considered Θ to be its leading member.[23] In 1924, Burnett Hillman Streeter accepted their findings of a family Θ and connected this family to the text of Origen when he lived in Caesarea.[24] At this point, however, Streeter changed Lake's method in a manner that would prove disastrous. Streeter kept Lake's dubious first step in

filtering out all agreements with the *Textus Receptus*, but instead of looking for agreements in error, he calculated rates of agreements with Alexandrian and Western representatives and considered any manuscript that fell somewhere in between to be "Caesarean."[25] The problem with Streeter's method is that it now groups anything not clearly Byzantine, Alexandrian, or Western into a catch-all category without any regard for how their readings are patterned. In effect, Streeter treated the Caesarean text as a wellspring of variants that could turn up in any manuscript, and Streeter designated so many collateral members of other text-types as secondary or tertiary witnesses to the Caesarean text that almost any non-standard variant could be classified as Caesarean. For example, Streeter thought that the Purple Uncials (N, O, Σ, and Φ) were secondary Caesarean witnesses.[26] In the years to come, Streeter continued to find additional members, such as 157, 1071, the uncial U, and, for that matter, most of von Soden's I-text.[27]

Streeter's approach is also deeply flawed because it too easily elides the distinction between a manuscript, its text, and its readings. A text is not a hodgepodge of variant readings; it is a pattern of sequential readings embodied in a document. Though Streeter identified a collection of interesting readings, it was questionable whether a substantial majority of these variants were ever embodied in the text of a real manuscript that actually existed in history. In other words, it was unclear whether Streeter's Caesarean text corresponded to historical reality or whether it was merely an artifact of his defective method.

In his second edition of *The Four Gospels*, Streeter responded to these criticisms with a new appendix arguing that Codex W from Mark 5:31–16:8 was the purest authority for his Caesarean text: "I found a total of 260 readings in which W differs from the T.R. In 37 of these the readings of W is supported by no other MS. or version. ... Ignoring these, there remain 223 readings. Of these 189, *i.e.* all but 85%, are, I found, supported by at least one, and usually by several, of the six above-named authorities for *fam.* Θ."[28] Streeter's argument may sound impressive, but some of the agreements are in original readings and are therefore not indicative of genealogical affinity, and of the remaining agreements Streeter tallied any agreement found even in one member of his Caesarean gaggle of witnesses. But this procedure presupposes that his Caesarean set of texts was already a true community of error having a common origin, not an artificial assortment of variants.

Streeter's approach also invited terminological confusion. Everything–both readings and manuscripts–that was not clearly Byzantine, Alexandrian, or Western, he called "Caesarean." If Streeter's grab-bag of manuscripts is to be broken up, to which member should the name be given? In taxonomy, nomenclatural stability is best preserved by

anchoring the name to a particular member designated as the *holotype* of the group or "type" for short. When a group is broken up, that portion of the old group that includes the holotype keeps the name. The selection of the nomenclatural type is a matter of convention (biologists use temporal priority of publication), which means that it can be arbitrary as long as it is consistently followed. As a practical matter, the most useful type designations are those which correspond to the meaning of the name or are based on the group's leading member, rather than on a wild, mixed, or fragmentary representative whose textual affinities are unclear.

For the "Caesarean" text, there are four viable candidates for the holotype. The name "Caesarean" was given based on Origen's residence, so Origen's text at that residence is the obvious choice. However, that text may be too fragmentary to be useful. Family 1 may be a better candidate: it too is connected to Origen,[29] but it has a continuous text, and it was the first identified leading member of the larger group. Codex Θ was championed by Lake, Blake, and Streeter, and most of the literature since then is premised on Θ as the nomenclatural focus for the Caesarean label.[30] Finally, there is Streeter's 1926 opinion that Codex W as "the oldest, and much the purest, authority for this ancient and interesting type of Eastern text."[31] The viability of these candidates depend in large part on whether a Caesarean textual family truly exists, and, if so, which of its members are the best representatives.

This issue became important in the 1930s when P45, the earliest papyrus of Mark, was discovered and studied. At first blush, P45 appeared to confirm the existence of Streeter's Caesarean text since P45 was early and quite similar to W, but upon further scrutiny it led to the breakup of Streeter's Caesarean text. For example, Teofilio Ayuso divided Streeter's Caesarean text into two parts: one is "pre-Caesarean" (P45 W family 1, family 13, etc.) versus a proper Caesarean text, consisting of Θ, 565, and 700.[32] According to Larry Hurtado's surveys of the history of the research into Caesarean text-type, Ayuso's division of the Caesarean text has been largely accepted and even prefigured to some extent by contemporary critics.[33]

Starting in the 1950s, Ernest Cadman Colwell began publishing a series of papers calling for a methodological refocus in the practice of New Testament textual criticism.[34] Most importantly, Colwell established that a text-type is a group of documents, not a collection of various readings.[35] Colwell also criticized the practice of only counting agreements among deviations from an external standard, namely, the *Textus Receptus*.[36] One example that Colwell gave was the Terrell Gospels manuscript, a leading K^r manuscript, whose few variants from the *Textus Receptus* happened to be shared by variable combinations of Caesarean manuscripts.[37]

In a very influential paper co-authored with Ernest Tune, Colwell proposed a "quantitative method," in which all pairs of manuscripts were to be compared against each other directly, rather than indirectly through the *Textus Receptus*.[38] Colwell and Tune applied their ideas to a sample of 18 manuscripts and their correctors and found that, for their sample, a minimum of 70% total agreement in a discriminated set of variants with a 10% gap corresponded well to the distribution of known text-types. Colwell and Tune expressed hope that their method could be extended to greater numbers of manuscripts and that their 70% agreement and 10% gap criteria would carry over generally.[39]

This quantitative approach differs from stemmatics in the kinds of agreements that are counted. Lachmannian stemmatics counts agreements in error, but the Colwell-Tune approach counts all agreements in the variation units, whether their readings are original or secondary. As a result, the quantitative approach does not actually identify groups having a common ancestry, although it may approximate such groups under appropriate circumstances.

For example, it is possible for the quantitative method to yield three different answers for the same genealogical pedigree, depending on what part of a long lineage is being sampled. FIG. 2(a) shows a stemma for a set of six manuscripts, O, A, B, C, D, and E. In this figure, A and B are closer together and will have a high rate of overall agreements, as will D and E. As for witness C, it is genealogically more closely related to D and E than to A and B because it shares a common ancestor with D and E that is not an ancestor of A and B. In FIG. 2(a), C comes from near the end of lineage from the ancestor of A and B to the ancestor of D and E and so will tend to have high rates of overall agreement with D and E instead of with A and B, and the quantitative approach will classify C with D and E in one group and A and B in another group. So far, so good.

If, on the other hand, C came from near the base of the lineage from the ancestor of A and B to the ancestor of D and E as shown in FIG. 2(b), C will instead have a higher rate of agreement with A and B rather than with D and E, and a quantitative analysis using overall similarity will classify C with A and B instead of D and E. The genealogical relationships, however, remain the same: C still shares a common ancestor with D and E that is not an ancestor of A and B. Moreover, if C comes from about halfway down the lineage as in FIG. 2 (c), the overall similarly will be calculated to be midway, providing only ambiguous support for classifying C with either A and B or with D and E. Even more troubling, C's location halfway down will destroy the similarity gap between the A-B group and the D-E group. Further, tabulating rates of overall similarity also has difficulty discerning whether the whole

group of A, B, C, D, and E, are more closely related to each other than to some other witness, say, O.

In its favor, the quantitative approach outlined by Colwell and Tune is easy to calculate by hand and yields a rough, first-order approximation of which manuscripts are probably related and which are going to be worth studying in more detail. In the examples of FIG. 2, this method was useful in identifying at least the existence of the A-B and D-E groups, though not necessarily the placement of C or whether the D-E group is a distant descendent of the A-B group. On the other hand, the stemmatic common-error criterion is capable of detecting that C is more closely related to D and E for no matter where on the lineage C comes, since errors found in C's exemplar will also transmit to D and E. If the goal is the history of the text, the quantitative method was not ultimately designed for that purpose.

The major contemporary study of the Caesarean text, of course, is that of Larry Hurtado, who started with the quantitative analysis proposed by Colwell and Tune and modified by Gordon Fee.[40] Hurtado's study was directed to studying the nature of the "pre-Caesarean" text, especially as embodied in the Washington Codex W, and whether this text is closely related to the so-called "Caesarean" text of Θ and 565.[41] Hurtado chose a few key manuscripts to represent each of the leading text-types, i.e., 01 and B for the Alexandrian text-type, D for the Western text-type, and A and the *Textus Receptus* for the Byzantine text-type. Θ and 565 were chosen to represent the "Caesarean" text-type, and Hurtado then tested the relationship of three of Ayuso's "pre-Caesarean" members–P45, W, and family 13–with respect to each of the four text-types.[42] Significantly, Hurtado found the quantitative agreement of the "pre-Caesarean" members, though nearing the text-type level amongst each other, were rather low when compared with the allegedly Caesarean Θ and 565.[43] Thus, Hurtado's quantitative study supported Ayuso's separation of these manuscripts in two groups and raised doubt as to their historical connection.

As I just explained, the quantitative method is not the right tool for discovering genealogical relationships, but Hurtado's study of the Caesarean text did not end there—he followed up by examining the agreements with W in detail, looking for agreements in arguably genetic variants. And he found mighty slim pickings indeed. Many of the apparent agreements in error between W and Θ seemed to be of a trivial nature, easily attributable to common scribal improvements or faults such as assimilation to the synoptic parallels. More importantly, Hurtado found that many of the more impressive genetic agreements between W and Θ were also found in D, or if not in D, in other texts commonly thought to be Western witnesses (e.g., the Old Latins, the Old Syriac, etc.).[44] Then, Hurtado looked at the scribal habits underlying the text of W and found that many of the variants that happen to agree with

Θ are the kind that W's scribe would independently introduce.[45] For these reasons, Hurtado concluded that W and Θ were not so closely related as to constitute a text-type, as least not one that could be differentiated from the Western text-type.[46] Moreover, Hurtado called into question the appropriateness of Ayuso's appellation of W as "pre-Caesarean," in fact, arguing that the moniker should be abandoned.[47]

II. Cladistics

At approximately the same time that Colwell and Tune were developing their quantitative approach, systematic biologists also began investigating numerical techniques for the classification of biological organisms.[48] One of the first attempts in this area by Robert Sokal and Peter Sneath, called "numerical taxonomy" or "phenetics," also calculated percentages in overall agreements. Its proponents, however, merely conceived of their method as an aid to classification, not a technique for inferring genealogical relationships.[49]

In 1963 and 1964, two population geneticists, Anthony Edwards and Luca Cavalli-Sforza worked out a family tree of human populations based on blood group gene frequencies using the principle of "parsimony," which is that the tree with smallest number of changes would correspond to the organisms' "phylogeny" or historical pedigree.[50] As scientists, biologists had good access to mainframe computers in the 1960s and they developed algorithms for calculating the number of changes on a set of possible trees and choosing the optimal tree.[51] By 1970, one of these computational biologists, James S. Farris, noticed that a German entomologist named Willi Hennig had provided the philosophical justification for using parsimony as a metric for evaluating different phylogenies as far back as 1950.[52] Hennig's justification or "auxiliary principle" is that "the presence of apomorphous characters in different species 'is always reason for suspecting kinship ..., and that their origin by convergence should not be assumed a priori.'"[53] In other words, Hennig reinvented Lachmann's common error principle articulated a century earlier.

By the 1970s, researchers began noticing deep analogies between cladistics in biology and stemmatics in philology. In fact, one article, published in 1977, urged biologists to use cladistics for classifying organisms based on shared, derived characteristics precisely because of the success in textual criticism of the common error principle in stemmatics for recognizing kinship.[54] By 1987, cladistics had been applied to manuscript traditions,[55] yet it was four years later that cladistics managed to pass a significant test. Peter Robinson, who had laboriously constructed a stemma of an Old Norse narrative based, in part, on direct external evidence, posted an on-line challenge in 1991 to reconstruct his stemma solely by mathematical techniques. One of the entrants was a biologist who ran the data through a

cladistics computer program and successfully found all of Robinson's textual groups.[56] Robinson later used cladistics in the editing of the Canterbury Tales.[57] In fact, cladistics was even applied to the text of the New Testament, specifically manuscripts of the Epistle of James.[58]

Cladistics works by generating many different possible stemmata, calculating the number of changes each stemma requires, and choosing the stemma with the minimum number of changes. This can be illustrated with a New Testament example in the Gospel of Mark using the following witnesses: A, B, D, E, and L. At Mark 1:2, there is a variation unit in which an Old Testament quotation is introduced by either "in the prophet Isaiah" (read by B D and L) or "in the prophets" (read by A and E). Further to this example, the next variation unit at Mark 1:4 has "John the baptizer" (read by B and L) vs. "John was baptizing" (read by A, D, and E). FIG. 3(a) shows one possible stemma, which requires two changes in the textual history it represented. FIG. 3(b) shows a competing stemma, which necessitates, however, three scribal changes. If this second tree were the real tree, then there would have been two independent changes for the Mark 1:4 variation unit, one on the lineage to D and another one on the lineage to the most recent common archetype of A and E. Because the second tree requires three changes while the first tree requires only two changes, the maximum parsimony principle used in cladistics prefers the first tree over the second tree. In fact, the first tree also corresponds to our sense of what is actually happening, in that B and L are Alexandrian, A and E are Byzantine, and D is "Western."

Unfortunately, standard cladistics computer packages make several assumptions that are out-of-place for manuscript filiation. For example, most computer packages assume a strictly dichotomous view of descent, in which each ancestor has exactly two immediate descendents.[59] Moreover, they also assume that every ancestor is lost, or, in other words, no extant organism is the ancestor of any other organism being studied. The most serious assumption, however, is that lineages only diverge, never converge. All of these assumptions are problematic when applied to manuscripts: manuscripts may have more than two copies made from them; some extant manuscripts are known to be exemplars of other manuscripts, and contamination is sufficiently common that it cannot be ignored. In fact, mixture is the major problem when studying the Caesarean text, and off-the-shelf computer packages produce strange results, such as P45 being a late and highly derived member of the Byzantine text.

Nevertheless, various approaches have been attempted among computational biologists in adapting cladistics to handle mixture, hybridization, horizontal gene transfer, and other forms of what they generically call "reticulation." Some of these ideas are unfeasible. For

example, some methods first generate a family tree assuming no mixture and attempts to add links to account for reticulation.[60] The problem with this approach is that contamination distorts the tree, even reversing the direction of development in some of the branches (see FIG. 1). Merely adding reticulating links without fixing the reversed branches will not correct the distortion. Another approach is that of reduced median networks,[61] which is too restrictive in the input it accepts, being limited to binary characters with no missing data,[62] and unrealistic in its output–in one case, generating 8517 hypothetical manuscripts based on an original set of 82 manuscripts.[63]

Two approaches in the literature, however, did provide useful building blocks. For example, in 1995, John Alroy proposed a new phylogenetic method he called "Continuous Track Analysis" that adds links between the existing witnesses until some stopping point is reached.[64] Alroy reformulated the parsimony objective of minimizing the number of changes to be one of minimizing the number of introduced variants, which he called "continuous tracks." Because any extant witness can be linked to any other, Continuous Track Analysis straightforwardly handles extant ancestors, multiple children, and reticulation. The major drawback to Continuous Track Analysis is that it is very poor at inferring hypothetical ancestors, and it is best used when nearly all the members of the group being studied have been sampled.

A different approach proposed by Allan W. Dickerman is the use of "hypertrees," which are trees with additional, "reticulating" links added to connect different lineages.[65] Under Dickerman's hypertree idea, the additional link represents a set of possible non-reticulating trees, and only one of those trees in the set is applied at each variation unit, but the tree is chosen to minimize the number of changes in the hypertree. Because each reticulating link will reduce the score for the hypertree, a stopping point in adding links is not immediately apparent, so Dickerman recommended imposing a cost for every additional link until no more can be added. The major problem with Dickerman's approach, however, is that the calculation of the states of the hypothetical ancestors is very expensive, and, in fact, computationally tractable only for binary characters–those with exactly two states. This is a significant limitation given the number of manuscripts, variation units, and variant readings of interest to textual critics.

My approach builds on a combination of these methods. Like Continuous Track Analysis, I permit multiple children, extant ancestors, and reticulation by adding links between two witnesses, both extant and hypothetical. In addition, I provide two new operations that adds a link in conjunction with a new hypothetical ancestor. One operation, shown in FIG. 4 (a), takes an extant ancestor and replaces it with a hypothetical ancestor with a new link from

the new ancestor to the old. Another operation, shown in FIG. 4(b), resolves a polytomy by adding a hypothetical child that is ancestral to two of the current children.

Like Dickerman's hypertrees, I impose a slight penalty for each case of mixture, but I do not calculate states each time. Rather, I calculate the ancestral states for each variation when an hypothetical ancestor is first introduced and carry those states along. Also, I do not add links to a fully resolved tree. Rather, it is a "pay as you go" system: in order to add a reticulating link, I must first remove a link somewhere else in the stemma, either by removing some other reticulating link or by performing the inverse of the operations shown in FIGS. 4. Nevertheless, Dickerman's suggestion that there should be a cost for each case of mixture is a good one, and I assign a "reticulation cost" to keep the mixture down to a manageable amount, that is, to account for the mixture that could distort the stemma. The most workable way to estimate an appropriate cost is by computing the average number of introduced variants per manuscript in the stemma. It is not necessary to generate links for all possible cases of mixture, since low levels of mixture will show up as another form of accidental coincidence and can be identified by looking at the final set of changes made to each manuscript.

Moreover, to detect the existence of lineages inverted by contamination, I borrow an idea from *stratocladistics* and assign a number corresponding to the age of the manuscript, which can be used to compute a time cost.[66] The main reason for doing so is that lineages that start with the youngest member and go to the oldest have a higher time cost than those which start with the oldest and progress to the youngest. Mixture inverts the lineages, but those inverted lineages can be detected because they will tend to incur a higher "time cost" than otherwise expected.

It is also necessary to avoid the problem that befell Streeter. When there is a lot of mixture in the witnesses and most of the variation units have only two variant readings, there is a tendency for the readings to sort themselves into two different ancestors, which become the putative ancestors of a large number of mixed manuscripts. This set of affairs is illustrated in a portion of a stemma depicted in FIG. 5, in which hypothetical ancestors α and β are parents of mixed manuscripts A, B, Γ, Δ, E, Z, and H where one of the readings would be assigned to ancestor α and the other reading to ancestor β. In a data set involving the Caesarean text, ancestor α would end up with all the Byzantine readings, and ancestor β would be assigned the non-Byzantine variants present in only two of the manuscripts A, B, Γ, Δ, E, Z, and H.

This configuration is not unlike Streeter's 1924 vision of the Caesarean text and subject to the same criticisms. A text is not merely a collection of diverse readings but a

sequence of readings that is embodied in a manuscript. Thus, the evidentiary basis for a hypothetical text must be the texts of its descendents. Although unmixed descendents provide the requisite sequential evidence for the reconstruction, mixed descendents do not—they can only substantiate the readings that the other parent did not supply. If, however, all of the descendents of a hypothetical ancestor are mixed, there is no evidence of a connected sequence of readings in the descendents left for the reconstruction. In fact, the problem is still evident with a single unmixed child and multiple mixed children, because any two of the mixed children can out-vote the attested text of the unmixed child. For this reason, I impose a constraint that rejects any stemma in which the number of mixed children of a hypothetical ancestor exceeds the number of non-mixed children. Extant manuscripts are not subject to this constraint since we have the direct evidence for their text.

This constraint upon mixture is consistent with the way contamination is handled by classical textual critics working with open traditions. For example, in his study of the text of Cyprian, Maurice Bénevot discussed the importance of finding opposing pairs, triples, or larger sets of manuscripts, which he defined as "when, though each varies often from the resultant text, their variants agree together only very rarely."[67] Michael P. Weitzman, approving of Bénevot's approach, called such opposing manuscripts *poles*, and explained that a stemma can be constructed even in the presence of contamination by assuming that the archetype has only two poles, an assumption he called "far less restrictive than that of total absence of contamination."[68] Like the maximum parsimony principle that chooses the stemma with the least amount of coincidental error, the "bipolarity assumption, reasoning from consistent pairs to parent groups, chooses whichever possibility posits the least contamination."[69] My mixture constraint finds the opposing poles of Weitzman's bipolarity assumption within a maximum parsimony evaluation framework.

III. A Cladistic Analysis in Mark 6:45–8:26

For this study, I wanted a large section of text, approximately two chapters, that included P45, the earliest textual witness to Mark. I also wanted to reduce the opportunity for harmonization to a synoptic parallel, so I chose the so-called Bethsaida section, Mark 6:45 –8:26, which corresponds to Luke's "great omission." Further limiting the role of synoptic harmonization, this section includes a lengthy pericope not paralleled by Matthew (Mark 8:22 –26) and a fair number of sentences completely lacking in Matthew (e.g. Mark 7:2–5).

For most of the Greek witnesses, I obtained their readings from Swanson's published collations.[70] I also included additional members of family 13 from Lake's edition,[71] and I used Jülicher's *Itala* edition for the Old Latin witnesses.[72] For the text of Origen in this

section, I relied on Klostermann's GCS edition of Origen's commentary on Matthew.[73] Unfortunately, this phase of the project did not include Syriac witnesses, but P. J. William's guidelines should prove helpful in the future.[74] In collecting the evidence, I excluded very low-grade evidence, namely orthographic variants and easily corrected nonsense readings, but I included all singular readings in order to falsify hypotheses of direct ancestry between two extant manuscripts.

In all, a total of 58 witnesses were used. There were a total of 829 variation units, of which 262 were singular and 567 were phylogenetically informative (the "Type-II" variants: at least two readings shared by at least two witnesses). The reticulation cost was determined to be 27 (or almost 5% of the informative variation units). After running my program for about 48 hours, the most optimal stemma had a cost of 2757. This stemma is shown in Figure 6.

As a preliminary matter, the stemma is consistent with well-accepted parts of the textual tradition unrelated to the issues surrounding the Caesarean text. For example, the stemma identifies the existence of the Byzantine text (labeled Branch alpha), and a neutral text with 01, B, L, and Δ (Branch beta). Most of the Westerns, specifically Codex Bezae (D) and the *Itala*-type Old Latins constitute a distinct textual branch (Branch delta). In addition, smaller families are also present in the stemma, including family 1 (Branch lambda) and family 13 (Branch phi). The stemma also finds that manuscripts K and Π are closely related and belong with M outside of the mainline of Byzantine manuscripts. As for mixture, it is well accepted that Vulgate manuscripts belong to a recension of *Itala*-type Old Latins and an early form of the Byzantine text. This too is shown in the stemma.

To focus on the Caesarean text (FIG. 7), the results of this investigation support the long-observed distinction noticed by Ayuso and more-or-less confirmed by Hurtado between P45, W, family 1, family 13, and 28 on one hand, and Θ, 565, and 700 on the other hand. The former group, which I shall provisionally call "Branch gamma," also includes the text of Origen, Codex Bobbiensis *k* of the *Afra* form of the Old Latins, and one of the archetypes for the Byzantine text (the other archetype is an Alexandrian text related to C, 33, and 579). Moreover, three distinct sub-branches of Branch gamma can be discerned: (1) a branch led by family 1; (2) a branch led by P45 and W; and (3) a branch with family 13 and 28. Some of the members of Branch gamma are mixed. In particular, family 13 exhibits considerable Byzantine mixture, and W shows a slight, but discernible Western influence.

Codex 565 is the best representative for Branch theta, as Codex 700 appears to be about one-third mixed Byzantine, and about one-quarter of Codex Θ has been corrected

against a neutral text. The origin of Branch theta itself is due to mixture, which may explain Hurtado's observation that Θ and 565 seem to be more closely related to Codex Bezae (D) than to W. According to this investigation, Branch theta is a mixture of a non-W sub-branch of Branch gamma (led by 28 and family 13) with a text substantially similar to D. To complicate matters further, D itself appears to be influenced by another non-W sub-branch of Branch gamma (led by family 1). Thus, out of the three sub-branches of Branch gamma, the sub-branch with W in it has the least connection to Branch theta.

The stemma also shows the catch-all nature of Streeter's label for anything that was not clearly Byzantine, Western, or Alexandrian. For example, Streeter designated U, 1071, and 1424 as tertiary witnesses to his "Caesarean" text, but all are divergent Byzantine. Streeter called Codex N "Caesarean," even though it is really a very primitive member of the Byzantine text-type. Much the same criticism can be leveled at many members of von Soden's I-text.

The practice of anchoring the "Caesarean" label on the branch containing Θ and 565 now appears unwise, since Θ and 565 come from a family that originated as a late mixture of Branch gamma (to which Origen's text belongs) and a Western text substantially similar to D. If its semantics of the name important, the branch most deserving of the "Caesarean" appellation is the family that actually includes the text of Origen, which is Branch gamma on the stemma—not the branch with Θ and 565 as Ayuso had supposed in reliance on Lake's list of variants from the *Textus Receptus*.[75] On the other hand, if terminological continuity is desired for maintaining the Caesarean name for Θ, Branch gamma could be termed "Eastern" in accordance with Streeter. Whatever Branch theta is called, naming the group of texts in Branch gamma as its precursor, as Ayuso did in calling the W-type text "pre-Caesarean," is unsatisfactory. Branch theta has two precursors, and Branch gamma is the precursor to two different types of text.

The stemma also provides additional support for Westcott and Hort's idea that the Byzantine text arose by a process of recension. It appears here as an equal mixture of a Branch gamma and an Alexandrian text related to C, 579, and 33 (Branch eta). Westcott and Hort did not know about the Caesarean text, of course, and had to characterize the non-Alexandrian ancestry of the Byzantine text as part neutral and part Western. Actually, this description is quite apt, because the gamma branch is a sister of the *Itala*-Western branch, so the readings it has in common with the Western branch would be considered Western, and the primitive readings it preserved against Western developments would be labeled as neutral. Its own unique readings, however, would be tarred as Byzantine.

It is striking to see how closely this proposed stemma generated by a completely new technology supports the venerable conclusions of New Testament textual critics, going all the way back to Westcott and Hort. Perhaps the main reason why there seems to have been so little progress in advancing the history of the text since Westcott and Hort is that Westcott and Hort's theory of the text is so substantially correct that there is actually little left to advance.

[1] Eldon Jay Epp, "The Twentieth Century Interlude in New Testament Textual Criticism" in Eldon J. Epp & Gordon D. Fee, eds., *Studies in the Theory and Method of New Testament Textual Criticism* (Grand Rapids, Mich.: Eerdmans, 1993), 85-108. See also, Eldon Jay Epp, "A Continuing Interlude in New Testament Textual Criticism?" in *Studies in the Theory and Method*, 109–123; L. W. Hurtado, "Beyond the Interlude? Developments and Directions in New Testament Textual Criticism" in D. G. K. Taylor, ed., *Studies in the Early Text of the Gospels and Acts* (Atlanta: SBL, 1999), 26–48.

[2] Kirsopp Lake, *Codex 1 of the Gospels and its Allies* (TS 7; Cambridge: UP, 1902); B.H. Streeter, *The Four Gospels: A Study of Origins Treating of the Manuscript Tradition, Sources, Authorship, & Dates* (1st ed., 1924; 2d ed., London: Macmillan, 1926); and Kirsopp Lake, Robert P. Blake, and Silva New, *The Caesarean Text of the Gospel of Mark* (Harvard: UP, 1928).

[3] Larry W. Hurtado, *Text-Critical Methodology and the Pre-Caesarean Text: Codex W in the Gospel of Mark* (SD 43; Grand Rapids, Mich.: Eerdmans, 1981; see also, Bruce M. Metzger, "The Caesarean Text of the Gospels," in *Chapters in the History of New Testament Textual Criticism* (NTTS 4; Grand Rapids, Mich.: Eerdmans, 1963).

[4] See generally, Paul Mass, *Textual Criticism* (English trans. Barbara Flowers from 3d German ed., 1957; Oxford: Oxford, 1958).

[5] E.g., Kurt Aland and Barbara Aland, *The Text of the New Testament: An Introduction to the Critical Editions and to the Theory and Practice of Modern Textual Criticism* (2d ed.; English trans., Erroll F. Rhodes; Grand Rapids, Mich.: 1989), 34.

[6] See, e.g., Amy S. Anderson, *The Textual Tradition of the Gospels: Family 1 in Matthew* (NTTS 32; Leiden: Brill, 2004), 70.

[7] B. F. Westcott & F. J. A. Hort, *Introduction to the New Testament in the Original Greek, with Notes on Selected Readings* (later ed., New York: Harper, 1882; repr. Peabody, Mass.: Hendrickson, 1988).

[8] Westcott & Hort, *Introduction*, 178.

[9] Westcott & Hort, *Introduction*, 165.

[10] Westcott & Hort, *Introduction*, 40 (small capitals original).

[11] Westcott & Hort, *Introduction*, 40.

[12] Lake, *Codex 1*, xxiii.

[13] E. C. Colwell, "Genealogical Method: Its Achievements and its Limitations" in *Studies in Methodology in Textual Criticism of the New Testament* (NTTS 9; Grand Rapids, Mich.: Eerdmans, 1969), 65–66.

[14] Westcott & Hort, *Introduction*, 59.

[15] Westcott & Hort, *Introduction*, 210 and 132–135.

[16] Lake, *Codex 1*.

[17] Lake, *Codex 1*, xxiii.

[18] Lake, *Codex 1*, xxiii.

[19] Lake, *Codex 1*, xlviii.

[20] Lake, *Codex 1*, l.

[21] Lake, *Codex 1*, liv.

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[22] Silva Lake, *Family II and the Codex Alexandrinus: The Text According to Mark* (SD 5; London: Christophers, 1936; repr. Salt Lake City: U. Utah Press, 1965), 17 n.1.

[23] Kirsopp Lake and Robert P. Blake, "The Text of the Gospels and the Koridethi Codex,' *HTR* 16 (1923): 267–286.

[24] Streeter, *Four Gospels*, 91–102.

[25] See Streeter, *Four Gospels*, 81–84.

[26] Streeter, *Four Gospels*, 575–577.

[27] B. H. Streeter, "Codices 157, 1071 and the Caesarean Text" in Robert P. Casey, Silva Lake, and Agnes K. Lake, eds., *Quantulacumque* (Kirsopp Lake FS; London: Christophers, 1937), 149–150.

[28] Streeter, *Four Gospels*, 599.

[29] At least in Matthew, see Amy S. Anderson, *Textual Tradition*, 70.

[30] See, e.g., Silva Lake, *Family II*, 4 n.5

[31] Streeter, *Four Gospels*, 599.

[32] Teofilio Ayuso, "¿Texto cesariense o precesariense?" *Bib* 15 (1935): 369–415.

[33] See Larry W. Hurtado, *Text-Critical Methodology*, 1–4, and "P45 and the Textual History of the Gospel of Mark" in Charles Horton, ed., *The Earliest Gospels, The Origins and Transmission of the Earliest Christian Gospels. The Contribution of the Chester Beatty Gospel Codex P45* (JSNTS 30; London: T&T Clark, 2004), 132–148.

[34] These are republished in E. C. Colwell, *Studies in Methodology*.

[35] E. C. Colwell, "Method in Grouping New Testament Manuscripts," in *Studies in Methodology*, 9.

[36] Colwell, "Method," 4 n.4, credits Harold Murphy, "Eusebius' New Testament Text in the *Demonstratio Evangelica*," *JBL* 73 (1954): 167-168, for this insight.

[37] Colwell, "Method in Grouping," 5.

[38] Colwell & Tune, "Method in Establishing Quantitative Relationships Between Text-Types of New Testament Manuscripts" in *Studies in Methodology*, 56–62.

[39] Colwell & Tune, "Quantitative Relationships," 59.

[40] Hurtado, *Text-Critical Methodology*, 10–12.

[41] Hurtado, *Text-Critical Methodology*, 13.

[42] Hurtado, *Text-Critical Methodology*, 12.

[43] Hurtado, *Text-Critical Methodology*, 24–29.

[44] Hurtado, *Text-Critical Methodology*, 30–43, summarized at 44 and 86.

[45] Hurtado, *Text-Critical Methodology*, 67–80, summarized at 81 and 87.

[46] Hurtado, *Text-Critical Methodology*, 86–87.

[47] Hurtado, *Text-Critical Methodology*, 89.

[48] For an excellent textbook on cladistics, see Joseph Felsenstein, *Inferring Phylogenies* (Sunderland, Mass.: Sinauer, 2004). The historical overview presented here is based on pp. 123–138.

[49] Felsenstein, *Inferring Phylogenies*, 123–124.

[50] Felsenstein, *Inferring Phylogenies*, 125–128.

[51] Especially Camin and Sokol, according to Felsenstein, *Inferring Phylogenies*, 129–130.

[52] Felsenstein, *Inferring Phylogenies*, 137.

[53] Willi Hennig, *Phylogenetic Systematics* (English trans, D. Dwight Davis & Rainer Zangerl; Urbana, Ill.: U. Ill. Press, 1966, repr. 1999), 121.

[54] Nelson I. Platnick & H. Don Cameron, "Cladistic Methods in Textual, Linguistic, and Phylogenetic Analysis," *Systematic Zoology* 26 (1977): 380–385. See also H. Don Cameron, "The Upside-Down Cladogram: Problems in Manuscript Affiliation" in Heny M. Hoenigswald & Linda F. Wiener, *Biological Metaphor and Cladistic Classification: An Interdisciplinary Perspective* (Philadelphia: U. Penn. Press, 1987), 227–242.

[55] Arthur R. Lee, III, "Numerical Taxonomy Revisited: John Griffith, Cladistic Analysis and St. Augustine's *Quaestiones in Heptateuchum*," *Studia Patristica* 20 (1989): 24–32, orig. deliv. (Oxford: 10th Int'l Conf. of Patristic Studies, 1987).

[56] Peter M.W. Robinson & Robert J. O'Hara, "Report on the Textual Criticism Challenge 1991," *Bryn Mawr Classical Review* 3 (1992): 331–337.

[57] Robert J. O'Hara & Peter M.W. Robinson, "Computer-Assisted Methods of Stemmatic Analysis," *Occasional Papers of the Canterbury Tales Project* 1 (1993): 53–74.

[58] Matthew Spencer, Klaus Wachtel, and Christopher J. Howe, "The Greek Vorlage of the Syra Harclensis: A Comparative Study on Method in Exploring Textual Genealogy," *TC: A Journal of Biblical Textual Criticism* [http://purl.org/TC] 7 (2003).

[59] This recalls Bédier's complaint against the bifid stemma; cf. Colwell, "Genealogical Method: Its Achievements and its Limitations," 63–83, at 78–79.

[60] So P. Legendre & V. Makarenkov (2002), "Reconstruction of Biogeographic and Evolutionary Networks Using Reticulograms," *Systematic Biology*, 51 (2002), 199–216; Gareth Nelson, "Reticulation in Cladograms" in *Advances in Cladistics* 2 (Norman I. Platnick & V. A. Funk, eds.; New York: Columbia UP, 1983), 105–111.

[61] So Matthew Spencer, Klaus Wachtel, and Christopher J. Howe, "Representing Multiple Pathways of Textual Flow in the Greek Manuscripts of the Letter of James Using Reduced Median Networks," *Computers and the Humanities* 38 (2004): 1–14.

[62] Spencer et al., "Multiple Pathways," 11.

[63] Spencer et al., "Multiple Pathways," 5.

[64] John Alroy, "Continuous Track Analysis: A New Phylogenetic and Biogeographic Method," *Systematic Biology* 44 (1995): 152–172.

[65] Allan W. Dickerman, "Generalizing Phylogenetic Parsimony from the Tree to the Forest," *Systematic Biology* 47: 414–426 (1998).

[66] See generally, Felsenstein, *Inferring Phylogenies*, 550–552.

[67] Maurice Bénevot, *The Tradition of Manuscripts: A Study in the Transmission of St. Cyprian's Treatises* (Oxford: UP, 1961; repr. Westport, Conn.: Greenwood Press, 1979), 148.

[68] Michael Weitzman, "The Analysis of Open Traditions" in *Studies in Bibliography* 38 (1985): 82–120 at 102.

[69] Michael Weitzman, "Open Traditions," 104–105.

[70] Reuben J. Swanson, *New Testament Greek Manuscripts: Mark* (Sheffield: Academic Press, 1995).

[71] Kirsopp Lake & Silva Lake, *Family 13 (The Farrer Group): The Text According to Mark with A Collation of Codex 28 of the Gospels* (SD 11; London: Christophers, 1951; repr. Salt Lake City: U. Utah Press, 1965).

[72] Adolf Jülicher, *Itala, das neue Testament in altlateinischer Überlieferung* 2 (Berlin: de Gruyter, 1940).

[73] Erich Klostermann, *Origenes Werke* 10–12 (GCS 38, 40, 42; Berlin: Akademie-Verlag, 1938–1941).

[74] P. J. Williams, *Early Syriac Translation Technique and the Textual Criticism of the Greek Gospels* (TS 3:2; Piscataway, N.J.: Gorgias, 2004), esp. 293–295.

[75] Cf. Ayuso, "¿Texto?", 379: "estudiando las citas de Orígenes y de Eusebio que nos ofrece LAKE."