

# Scribal Habits in Early Greek New Testament Papyri Papers from the 2008 SBL Panel Review Session

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Abstract: The 2008 SBL meeting in Boston included a panel review session on James R. Royse's book, *Scribal Habits in Early Greek New Testament Papyri* (NTTSD 36; Leiden: Brill, 2007). A number of the reviews of this important work are presented here, along with Royse's response. The papers were produced as oral presentations and have been kept in their original shape, except for some straightforward corrections.

## 1. Review by Juan Hernández Jr

### Introduction

It is an incredible privilege for me to be here tonight to honor Dr. James R. Royse and to offer a few reflections on what his work has meant to me. I will leave it to others on this panel and to history to confirm what I already know to be true: that this is a work of *singular* importance—extraordinary for its immense learning, comprehensive scope and painstaking detail. But perhaps more importantly, Royse's work is an exemplar for all who aspire to do justice to the study of scribal habits. (Of course, the conclusions are also groundbreaking.) I, on the other hand, hope to offer a glimpse of how one book made a difference to the scholarly trajectory and pursuits of a fledgling Ph.D. student, who was essentially at his wits end as to what to "write on." I think it's safe to say that if it were not for Royse's *Scribal Habits in Early Greek New Testament Papyri*<sup>1</sup> my own academic interests, endeavors, and development over the course of the last few years would have been far different.

### Dissertation Work at Emory: Search for a Project & Method

As most here will no doubt know, Ph.D. work can be a very lonely, solitary experience. There is a sense in which it is all up to you. Only *you* can undertake your project and execute it to the bitter end. Only *you* can, as they say, "make it happen." And while there are no doubt friends, mentors, and colleagues along the way to encourage one to press on, the fact remains that you alone bear the responsibility for your work.

Back in 2003 I found myself at one of those crossroads. I had recently completed my coursework at Emory, where I was pursuing a Ph.D. in NT, and had moved beyond my comprehen-

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<sup>1</sup> James R. Royse, "Scribal Habits in Early Greek New Testament Papyri" (Th.D. diss., Graduate Theological Union, 1981), now published as *Scribal Habits in Early Greek New Testament Papyri* (NTTSD 36; Leiden: Brill, 2007).

sive exams—a veritable dark night of the soul. Now, however, I faced the unenviable task of coming up with a dissertation proposal, and one that needed to pass muster with the entire NT department and see me through to the end of my tenure there. But there was a problem. My interests all along had been in textual criticism, but I now found myself at a university that not only did not *have* any textual critics on board, but also had not *produced* a text-critical dissertation in some years. And although Emory could boast of once being the home of Ernest C. Colwell, *that*, I'm afraid, was a long time ago—a detail not missed in Epp's text-critical obituary of my institution. You can imagine how "encouraged" I was to read his article on the "Twentieth Century Interlude," where he wrote that: "[t]he grand tradition of textual criticism at the University of Chicago from Goodspeed to Willoughby, Colwell, and Wikgren apparently has ended ... and other text-critical centers, such as Emory and Duke, no longer seem to be active ... in the field."<sup>2</sup>

Regrettably, Epp's words, originally written in 1973, were still true thirty years later.

So the dilemma I faced was this: How does one embark on a venture that is true to one's own interests at a place where there are no specialists in one's chosen field? I had only two options before me: books; and accosting textual critics with my emails. First: my missives.

My email venture was less than promising. I contacted a number of textual critics, hoping to get a sense of not only what *had* been done, but also what they thought *ought* to be done. By the time I had begun sending out my emails I had narrowed my focus to the Apocalypse, which I considered progress. This, however, appeared to prompt an allergic reaction in most. The majority, it seemed—either directly or indirectly—encouraged me to *stay out* of textual criticism (or at least go somewhere else). Apparently, the notion of studying an esoteric work, within an arcane field, at an institution that specialized in neither, struck most as an especially bad idea. One scholar even told me, pointblank, that there was nothing to be found, adding that Josef Schmid had in fact "settle[d] *all the significant questions*."

But perhaps the most cutting response came from a world-class practitioner, whose opening lines to me were: "You may consider my response to your request for some potential ... topics to be either a 'cop out'—or even an insult—or *both*." He then went on to tell of all the mediocre projects that were produced because a student merely followed "someone else's ideas." To his credit, he also offered some valuable suggestions, but never without the caveat that the ideas *had* to be my own. His closing remarks were only slightly less painful than his opening words. He concluded: "So I throw the question back [at] you. When *you* arrive at an issue that excites you, *you* will make it your own and the task of pursuing it, though never easy, will more likely be carried through with enthusiasm and even affection."<sup>3</sup>

Ironically, these words proved to be prophetic. In fact, it was this curtly worded reality check (not to mention crushing blow to my ego) that was decisive. I now knew that I had no other choice but to turn to books—a decision that, unbeknownst to me, would put me on a trajectory that led inexorably to the work of James R. Royse. The path, however, would not be direct or immediate.

<sup>2</sup> Eldon J. Epp, "The Twentieth-Century Interlude in NT Textual Criticism" in *Studies in the Theory and Method of New Testament Textual Criticism* (ed. Eldon Jay Epp and Gordon D. Fee; SD 45; Grand Rapids, Mich.: Eerdmans, 1993), 108.

<sup>3</sup> The email came from Eldon J. Epp, dated April 17, 2003.

### The Quest for Exemplars: Promise and Limitations

I began by consulting all the standard works on textual criticism. Armed with a sense of resolve—and not a little bit of anger—I undertook a comprehensive reading program that put me in touch with every available text-critical work I could get my hands on, often reading them two, three or even four times. Particularly worn were Metzger’s third edition of the *Text of the New Testament*,<sup>4</sup> coupled with Epp and Fee’s essays in the *Studies in Theory* volume,<sup>5</sup> as well as Ehrman and Holmes’s *Text of the New Testament in Contemporary Research*.<sup>6</sup>

But there were two works in particular (just before I discovered Royse) that intrigued me: Epp’s *Theological Tendencies in Codex Bezae*<sup>7</sup> and Ehrman’s *Orthodox Corruption of Scripture*.<sup>8</sup> Despite the fact that the Apocalypse played absolutely no role in Epp’s program and Ehrman’s offered up only a few footnotes to Revelation, their distinctive approaches to theological variation captured my imagination. Is it possible, I wondered, for these studies to serve as analogues for a similar approach to the Apocalypse?

The answer, I would soon discover, was “no,” or at least not an unqualified “yes.” The Apocalypse’s peculiar textual history, coupled with its idiosyncratic research record simply disallowed it. The MSS of Epp’s study for example, Vaticanus and Bezae, do not preserve the Apocalypse. Even if we abandoned these two codices and broadened our study to a comparison between Alexandrian and Western witnesses, we find that there is no characteristically “Western” form of the text for the book of Revelation.<sup>9</sup>

On the other hand, Ehrman’s discussion of scribal tendencies capitalizes on large swaths of the MS tradition and builds upon the consensus of multiple generations of text-critical scholarship. Regrettably the very conditions that make Ehrman’s study possible are *precisely* what the Apocalypse lacks—a rich and unproblematic MS tradition and a scholarly consensus on its textual history.<sup>10</sup>

Finally, the theological issues that drive Epp and Ehrman’s respective programs—anti-Judaic tendencies and early Christological controversies—do *not* appear to be what concerned the scribes of the Apocalypse. In short, these approaches, while stimulating, intriguing, and even groundbreaking in their own right, had limited applicability to the book of Revelation.<sup>11</sup>

<sup>4</sup> Bruce M. Metzger, *The Text of the New Testament: Its Transmission, Corruption, and Restoration* (3rd enl. ed.; New York; Oxford University Press, 1992).

<sup>5</sup> Eldon J. Epp and Gordon D. Fee, *Studies in the Theory and Method of New Testament Textual Criticism* (SD 45; Grand Rapids, Mich.: Eerdmans, 1993).

<sup>6</sup> Bart D. Ehrman and Michael W. Holmes, *The Text of the New Testament in Contemporary Research: Essays on the Status Quaestionis* (SD 46; Grand Rapids, Mich.: Eerdmans, 1994).

<sup>7</sup> Eldon Jay Epp, *The Theological Tendency of Codex Bezae Cantabrigiensis in Acts* (SNTSMS 3; Cambridge: Cambridge University Press, 1966).

<sup>8</sup> Bart D. Ehrman, *The Orthodox Corruption of Scripture: The Effect of the Early Christological Controversies on the Text of the New Testament* (Oxford: Oxford University Press, 1993).

<sup>9</sup> Juan Hernández Jr., *Scribal Habits and Theological Influences in the Apocalypse: The Singular Readings of Sinaiticus, Alexandrinus, and Ephraemi* (WUNT 2.218; Tübingen: Mohr Siebeck, 2006), 41.

<sup>10</sup> Hernández, *Scribal Habits and Theological Influences*, 41.

<sup>11</sup> Hernández, *Scribal Habits and Theological Influences*, 41.

## Discovering Royse

It was at about this time that I came across Royse's 1981 dissertation through an article he had written, titled "Scribal Tendencies in the Transmission of the New Testament."<sup>12</sup> To be frank, there was nothing fancy or spectacular about the article. No nefarious scribes lurking in the background, hell-bent on corrupting the scriptures; and there were certainly no rhetorical flourishes. What I found, however, was an essay that reflected—and even promoted—a certain sensibility. Ostensibly the piece was about measuring transcriptional probability, but its subtext—as I read it—was that "claims *must* be tested," "sweeping generalizations *must* be avoided," "the received and hallowed traditions of our text-critical enterprise may *not* be the final word," and whenever possible "check everything yourself." It was Royse's palpable, unrepentant scrutiny of every claim, coupled with a pious devotion to meaningful detail that drove me to his dissertation and gave me hope of finding the "right" approach.

## The Dissertation—an Expansion of Colwell

### *Colwell's Contribution to Method*

For those of us who have seen and even worked with Royse's 1981 dissertation, you'll know that it is not an attractive book. Depending on the edition you get, one will either be looking at a one or two-volume, UMI publication. The paperback cover will be either a plain blue or black, and the whole thing looks like it was done on a typewriter. And of course, there are no pictures. Even back then, however, the work—standing at over 700 pages—was already considered groundbreaking. And despite the fact that it was not published for another 26 years, it would nonetheless remain the standard work on scribal habits throughout that period.

The dissertation, I discovered, was an expansion of Colwell's well-known study of  $\mathfrak{P}^{45}$ ,  $\mathfrak{P}^{66}$ , and  $\mathfrak{P}^{75}$ .<sup>13</sup> As most here will no doubt know, Colwell was critical of the indiscriminate use of Hort's genealogical method to construct an eclectic text. It appeared to him that the major role of scribal corruption was somehow being overlooked in such endeavors. For Colwell, however, a detailed knowledge of scribal habits was a *prerequisite* for the proper editing of the Greek New Testament. After all, it was precisely this knowledge that informed our conjectures on transcriptional probability and enabled the proper weighing of variants.

To address this problem, Colwell laid out a method that had two distinct but interrelated goals. The first was to gain knowledge of an *individual* scribe's copying habits. The second was to gain knowledge of scribal habits in *general*. To that end, and guided by the dictum: "peculiar readings reveal scribal peculiarities," Colwell used the singular readings of the three papyri to offer specific data on the copying tendencies of each scribe. In the end, Colwell succeeded in moving beyond the assertion that "scribes can err in certain ways" to demonstrating that a *particular* scribe, or even scribes in general, have made *specific* types of error. Colwell closes his study with a number of important conclusions and one expressed wish—which brings us here today—that someone, someday, would publish "a commentary on the singular readings of these papyri."<sup>14</sup>

<sup>12</sup> James R. Royse, "Scribal Tendencies in the Transmission of the Text of the New Testament" in *The Text of the New Testament*, 239-52.

<sup>13</sup> Ernest C. Colwell, "Method in Evaluating Scribal Habits: A Study of  $\mathfrak{P}^{45}$   $\mathfrak{P}^{66}$   $\mathfrak{P}^{75}$ ," in *Studies in Methodology in Textual Criticism of the New Testament* (NTTS 9; Grand Rapids, Mich.: Eerdmans, 1969).

<sup>14</sup> Colwell, "Method in Evaluating Scribal Habits," 124.

*Royse's Dissertation vis-à-vis Colwell*

As noted, Royse's dissertation expanded Colwell's study, turning a 19 page article into a juggernaut of over 700 pages. The dissertation itself consists of an introduction, nine chapters, a conclusion, two appendices and 1,319 footnotes—and this was as it stood *twenty-seven years ago*. To Colwell's three papyri Royse adds three more. And although the singular readings of these six papyri form the core of Royse's study, he also devotes considerable attention to their corrections, as well as to the relationship between the Ethiopic and  $\mathfrak{P}^{46}$ .

It would be a mistake, however, to assume that Royse simply gives us more “stuff” to work with. Although he certainly does that, Royse also nuances, corrects and updates Colwell's study in a number of important ways. His method for determining singularity, his rubric for organizing the singulars, as well as his discussion of the authenticity of readings goes well beyond Colwell's original work.

Regarding singulars, Colwell believed that they were not simply inauthentic, but were the textual creation of the scribe. As he put it, a singular reading is one that “can with moral certainty be assumed to have been introduced into the textual history by the scribe.”<sup>15</sup> Royse generally agrees with this assessment, but charts a more cautious path, using statistical data and tempered language when referring to readings of doubtful authenticity. After all, it is theoretically possible for the opposite to be true, particularly in the case of Revelation, with its high incidence of singulars and haphazard MS tradition.

As far as method is concerned, Royse adopts a more rigorous practice for determining singularity. For Colwell, a singular reading was one that had no Greek support in Tischendorf's apparatus, “augmented”—as he put it—“by more recent finds.”<sup>16</sup> Moreover, Colwell also restricted his collation of singulars to the consensus of the text-type to which the MS belonged. Royse, on the other hand, avails himself of a much broader collation base, checking the singular readings of the papyri against Tischendorf, von Soden, NA<sup>26</sup>, UBS<sup>3</sup>, as well as Hoskier and Schmid for  $\mathfrak{P}^{47}$ . And, as if that were not enough, Royse throws in a host of published facsimiles and collations for additional comparison. Thus, he offers a far more comprehensive system of checks and balances, as well as avoids the pitfalls of classifying early papyri according to “text-types.”

Finally, regarding Colwell's classification of singulars, here too Royse goes beyond his predecessor, offering a specificity and nuance missing in the original. Royse classifies singular readings as significant and insignificant. The latter consists of orthographic and nonsense singulars, while the former includes the remaining, “sensible” singulars. Excluded from the study are “itacisms” of a certain type, largely diphthongs that evolve into  $\epsilon$ ,  $o$ , or  $\iota$  and a handful of other inessential features. This represents a departure from Colwell, who did include these, although it's important to note that Colwell also dismissed them as insignificant after a brief discussion.

One further distinction pertains to their respective usage of corrected singulars. Colwell uses the singular readings of a MS *prior* to their correction. Royse, on the other hand, assesses the singulars *after* they have been corrected by the original scribes. Like Colwell, the bulk of Royse's study focuses on the *significant* singulars and what they reveal about scribal copying habits. These are arranged according to omissions, additions, transpositions, various types of harmonizing, variations in grammatical form and differences in proper names. Throughout, Royse's primary goal is to cast into bold relief the copying patterns of each scribe.

<sup>15</sup> Royse, “Scribal Habits,” 165

<sup>16</sup> Colwell, “Method in Evaluating Scribal Habits,” 108-9.

### Rationale

The rationale for such exertion, of course, is Roysse's concern over the repeated use of text-critical canons *absent* a knowledge of scribal activity. In fact, Roysse argues that the canons are routinely applied on the basis of a priori reflections on how scribes behaved (or *must* have behaved). But perhaps even more problematic is the anachronistic nature of such application. As Roysse notes, the canons were developed on the basis of later MSS, raising questions about their application to 2nd and 3rd century witnesses.

In the end, Roysse's statistical study led him to conclude that *lectio longior potior* would be a more useful canon for the early papyri with only three exceptions:

[Where t]he longer reading appears to be late on genealogical grounds;

[Where t]he longer reading may have arisen from various types of harmonization; or,

[Where t]he longer reading may have arisen from an attempt at grammatical improvement.<sup>17</sup>

The inversion here of Griesbach's *lectio brevior potior* is not a *denial* of the venerable canon as much as it is a statement that the burden of proof has *shifted* with this study. Of course, with characteristic caution, Roysse concedes that later MSS may exhibit the opposite tendency—appropriately limiting his claims to the six papyri.

### Transparency and Imitative Quality

Now, despite the original work's dizzying detail, forbidding length, and potentially mind-numbing minutiae, I was electrified by the study. Frankly, Roysse's work gave me something no other book had given me until then—*access*; *access* to the careful and judicious thinking of a seasoned textual critic; *access* to every primary and secondary source of relevance to the topic; *access* to the skillful practice of selecting, arranging, presenting, and more importantly, analyzing the text-critical data. Everything, it seemed to me, was all right here in the book—available for all to see. For me, Roysse's dissertation was nothing less than a gigantic “how to” manual.

It's not that all of the others were somehow injudicious or careless in their thinking; far from it. Rather, it was the fact that Roysse's work was permeated by a transparency that was without peer. I was never at a loss for *why* he made certain decisions, *where* he was headed, or *what* his sources were. Everything was copiously documented and available for critical scrutiny.

Truth be told, I was far less interested in the results of his study than I was in how he *conducted* such a study. There was a curious twist, however. My primary interest, you'll recall, had been in theological variation, but I was at a loss as to how to undertake such a study in the Apocalypse. Having found other approaches ill-suited to the book of Revelation, I was convinced that Roysse was the way to go; that this was in fact, *the* canonical standard for *any* study of scribal habits. But there was a rub: Roysse was no friend of sweeping generalizations, much less easy claims of “theologizing.” Of the thousand-plus singular readings of the early papyri, he regarded only *three* to be possibly theologically motivated—and these limited to *one* papyrus.<sup>18</sup>

The irony is that rather than serving as a deterrent, Roysse's extreme caution and even antagonism towards casual claims of “theologizing” only made his approach *all the more appealing*—even urgent. There was something extraordinarily right about requiring a variant to undergo a battery of tests *before* claiming “theological motivation.” And even if one were to arrive at such a variant, it had to be assessed within the broader context of *other* scribal errors, as Roysse does with  $\Phi^{72}$ , where variants that appear to defend the full deity of Christ are

<sup>17</sup> Roysse, “Scribal Habits,” 608.

<sup>18</sup> Roysse, “Scribal Habits,” 488.

discussed against the backdrop of other, less glamorous changes. In fact it is precisely Royse's protectionist attitude toward the integrity of the full record that inspires confidence in both his method and its results.

### **Application to the Apocalypse**

It was crystal clear to me, then, that if I was to carry out my own project with any viability whatsoever that I would have to learn from Royse. I would have to put out of my head any thoughts of theological variation or dreams of spectacular yields and start at the beginning: *with* the facsimiles, *with* the collations, *with* the textual apparatuses, *with* the unending procession of monographs in multiple languages; in short, *with* the arduous and the mundane. The learning curve would truly be steep and the patience required unimaginable. But I found that everything I would ever need was already expertly modeled for me by a premier textual critic in this work. And the very minutiae that most would consider cumbersome, disinteresting, and even prohibitive served as the pedagogical building blocks of my own education.

### **Royse<sup>2</sup>: the 2007 Monograph**

#### *What Has Changed*

Now, for those of us beholden to the original, the most recent installment is nothing less than the fulfillment of a long-awaited promise. The culmination of 26 years of painstaking research, Royse's 2007 monograph supersedes the original in a number of important ways.

All of the databases for collation have been updated and previously unavailable files have now been incorporated into the study. Every imaginable scholarly contribution (no matter how remote its relationship to the papyri) appears to have been consulted and made a full conversation partner in the work. Royse also adds several chapters and expands all of the original ones. For example, what was once a single, introductory chapter is now three, bringing the total number of chapters in the book to eleven. The six chapters on the papyri have also been substantially revised and reorganized. For example, the collations that originally began each chapter have now been moved to an appendix, making for easier reading. Royse also adopts a series of more nuanced categories for the singulars, particularly, for the variety of corrections that occur in the papyri.

Brand new to the work are the "Supplementary Notes" at the back, consisting of twenty-four items keyed to remarks made in the body. These include discussions of the sigla, the use of pronouns, challenges to various text-critical arguments, further analysis of variants, and an exploration of the relevance of "Freudian slips" to scribal errors in addition to many other topics.

Finally, four separate appendices round out the work. The first is a collation of all the singular readings of the papyri; the second, a collation of all their corrections; the third, an exhaustive register of all of the orthographic phenomena of the papyri; and the fourth is a summary of the study's results. The only thing missing from the newer work is Royse's separate treatment of  $\Psi^{46}$  and the Ethiopic. However, that is slated for future publication in a separate article!

#### *What Has Not Changed*

Remarkably, despite the continued march of scholarship and Royse's apparently unending revisions, a couple of things have remained fairly stable about the work. First, the number of singular readings has not changed by much. The original tally of 1,068 has been reduced to

1,004—a difference of only 64 readings. (Not too bad considering the intervening decades of research). Second, although nuances abound in the work, the conclusions of the study are generally the same as they were in 1981. Omissions *still* outnumber additions in the papyri so the burden of proof remains on the defenders of the shorter reading canon.

With respect to theological variation, Royse's attitude is no less cagey than it was two-and-a-half decades ago. If anything, recent studies have merely afforded him the opportunity to expose the rashness of certain judgments. One only need examine his excoriating of a recent proposal to identify theological singulars in  $\mathfrak{P}^{75}$ .<sup>19</sup> In keeping with his original dissertation, however, he maintains that  $\mathfrak{P}^{72}$  does exhibit some theological singulars. Interestingly, he also appears to be willing to accept their presence in some later MSS of the Apocalypse.<sup>20</sup>

## Conclusion

You would think that with an achievement 26 years in the making, double the size of the original and with triple the number of footnotes that there would be some room for a claim of being "exhaustive." However, with characteristic humility, Royse downplays the completeness of his own work, stating that:

[t]he analyses presented of the six individual papyri cannot, of course, be viewed as an exhaustive study of even the singular readings of these manuscripts. In fact, only a beginning could be made here, a beginning that has isolated certain tendencies within the copying techniques of these six scribes, but has by no means dealt with all the potential issues.<sup>21</sup>

If this is in fact the case then I despair of there ever being a truly completed study of scribal habits in the papyri. Despite his protestations, I do think that Royse has not only fulfilled Colwell's wish for a commentary on the singular readings, but that he has surpassed it by producing a veritable *encyclopedia* on them—an encyclopedia that deserves to take its rightful place as *the* canonical standard for *any* study of scribal habits for generations to come.

## 2. Review by Peter M. Head

### Introduction

I would like to begin by thanking our chair Anne-Marie for the invitation to participate in this panel. I was pleased and honoured to be invited and of course to take part in the critical honouring of the author for his great achievement in this book. I readily accepted the invitation for three reasons:

a) I had interacted with James Royse's ThD in my first academic publication<sup>22</sup> (Head, 1990) as well as subsequently (Head, 2004, 2008)<sup>23</sup>—these are actually discussed on 720ff;

<sup>19</sup> Royse, *Scribal Habits*, 698-793.

<sup>20</sup> Royse, *Scribal Habits*, 613-14.

<sup>21</sup> Royse, *Scribal Habits*, 737.

<sup>22</sup> "Observations on Early Papyri of the Synoptic Gospels, especially on the 'Scribal Habits'" *Biblica* 71 (1990), 240-47.

<sup>23</sup> "Singular Readings in the Early Fragmentary Papyri of John: Some Observations on the Habits of New Testament Copyists" *Biblica* 85.3 (2004), 399-408; "Scribal Behaviour and Theological Tendencies in Singular Readings in P. Bodmer II (P66)" in *Textual Variation: Theological and Social Tendencies? The Fifth Birmingham Colloquium on New Testament Textual Criticism* (eds H.A.G. Houghton & D.C. Parker; Texts and Studies, Third Series; Gorgias Press, 2008), 55-74.

b) I had also met James Royse at SBL meetings and had the idea that the work had been much expanded over the years; and

c) I saw the price of the book and knew that the chance of a free copy was not to be passed by.

I take the opportunity to apologise to the author for sending him a copy of my review a little late ... but draw some comfort from the 27 year delay between his dissertation (henceforth: *Diss*) and the publication of the book (henceforth: *Book*).

### General Introduction and Summary

How should I proceed in responding to this book? Well, after a general introduction I shall note some of the differences between the dissertation and the book (having been well trained in redaction criticism from my youth) and then focus on just one chapter, the one concerning  $\Psi^{66}$ , as a sample.

In this book (as in his earlier dissertation) Royse seeks to undertake a comprehensive study of the six early and extensive Greek NT papyri with a view to ascertaining what can be learnt about scribal habits so as to inform our understanding of “transcriptional probability” in NT textual criticism. Taking his lead from Hort and most especially E.C. Colwell he uses the “Singular Readings” of the papyri as a means of accessing the habits or behaviour of the particular scribes: both because the individual character of each of these witnesses is relevant to the consideration of their witness (“knowledge of documents should precede final judgement upon readings”, quoting Hort in *Book*, 1) and because generalizations about scribal behaviour on the basis of such a quantifiable approach to the evidence ought to contribute to our own generalizations about transcriptional probabilities in the early period of transmission.

The assumption guiding the work is that of Colwell (quoted on *Book*, 39) to the effect that singular readings provide access to “scribally created readings”, either as actually representing textual creations of the scribe (which seems to be Colwell’s view), or at least as a means of approximating towards the individual scribal contribution to his particular textual tradition. A “singular reading” is defined as a reading not otherwise attested in continuous-text Greek witnesses. Practically this was determined by initial comparison with Tischendorf and subsequent checking against a wider range of editions and closely related texts (*Book*, 65-67).<sup>24</sup>

The major chapters involve extended discussions of the singular readings, and analysis and grouping of them; initially into either: orthographic, nonsense, or significant readings; and then further analysis of the significant singulars as additions, omissions, transpositions, harmonizations, etc.

### Comparison between Book and Dissertation

The general shape of the dissertation is retained in this book, with the exception that the opening chapter is divided and expanded into three chapters (dealing with “The Study of Scribal Habits”, *Book*, 1-38; “Singular Readings”, 39-64; and “Methodology”, 65-102). The external

<sup>24</sup> This double process explains the use of double asterisks in the list of singular readings on *Book*, 775-848: as I understand it every reading which looked singular on the basis of comparison with Tischendorf gains an entry into the list; but those with double asterisks were found, on the basis of other editions or comparison with particular manuscripts, to not actually be singular readings. They are retained on the list since they may on occasion require some discussion as sub-singular, or not in a genealogical relationship with these other witnesses.

shape of the remainder is identical with chapters devoted to each of the six extensive early Greek papyri: P<sup>45</sup> (103-97); P<sup>46</sup> (199-358); P<sup>47</sup> (359-98); P<sup>66</sup> (399-544); P<sup>72</sup> (545-614) and P<sup>74</sup> (615-704), followed by a chapter entitled “The Shorter Reading?” (705-36), and a brief one entitled “Concluding Remarks” (737-42)—all of these last eight chapters have the same titles as the dissertation.

A series of 24 “Supplementary Notes” follow (743-73)—these seem to be something like extensive footnotes to the already extensive footnotes of the main body of the text. Then a series of appendices provide some of the core data: Appendix A lists “The Singular Readings of the Papyri” in turn (775-848)—this material was more or less at the heart of each of the six major chapters in the dissertation, but has now been displaced to the first Appendix. Then we have “Appendix B. The Corrections of the Papyri” (849-84); “Appendix C. Orthographic Phenomena” (885-96: new material cf. the dissertation); and “Appendix D. Summary of Results” (897-906: also new material cf. the dissertation). Forty pages of bibliography and ninety pages of indices round out the book to 1,051 pages in total.

The dissertation was pretty long itself (and late!, judging by the preface), at 746 pages of double spaced typing, but I would guess that what we have here is something like *three times* the length of the dissertation (smaller print, single spaced, 1,051 pages in total). Before we ask where the new material enters into the discussion it is worth remarking on where it doesn't. That is, *where have things stayed the same?* One fundamental area is in the summary conclusions to be drawn concerning the habits of the six scribes—these six summary conclusions at the end of the main chapters are practically identical with the statements in the dissertation (so e.g. P<sup>45</sup> on *Book*, 197 is practically identical to the dissertation; so also for P<sup>46</sup> on 358; P<sup>47</sup> on 397-8; P<sup>66</sup> on 544; P<sup>72</sup> on 614; P<sup>75</sup> on 704). Arising from this consistency is the obvious fact of the general conclusions: specifically that the observed scribal habits of these six scribes offers a challenge to the rule that the shorter reading should be preferred. So the general shape, the primary data, and the fundamental conclusions remain the same as the 1981 dissertation (as of course the title). In the preface Royse writes “While the main lines of argument in the dissertation remain, the evidence and the conclusions based on it have often been revised.” (*Book*, xiv) But it is not clear to me which conclusions are different or have been revised from the dissertation.

So to return to the question, where is the new material? What has been expanded? I firstly offer a general impression and then some details. My general impression is that the dissertation treated the papyri in a somewhat detached manner as sources or reservoirs of singular readings—once the singular readings could be isolated and listed then the analysis could proceed. Here my general impression is that the papyri are treated more carefully as artifacts and the objects of multitudes of studies which are taken into account and discussed in detail (especially if it concerns a scholar reading the text differently). Another very significant factor is the attention given to more recent discussions (including often discussions of his own dissertation).

From the first chapter we note:

17-19 on *Provenance of the Beatty and Bodmer Papyri* (interesting that this section has no clear close, I assume it goes to around 19—i.e. that this is new material inserted into *Diss* 21);<sup>25</sup>

<sup>25</sup> By the way, Royse's statement about the provenance of the Chester Beatty papyri needs revisiting in the light of C. Horton, “The Chester Beatty Biblical Papyri: A Find of the Greatest Importance” in *The Earliest Gospels: The Origins and Transmission of the Earliest Christian Gospels—The Contribution of the Chester Beatty Gospel Codex P45* (JSNTSS 258; London & New York: T & T Clark International, 2004), 149-60.

44-51 on *Singulars in Revelation* (discussing Delobel's observation that singular readings of Alexandrinus may very often [60/210 times according to B. Weiss] represent "the original reading" — i.e. preserved only in one witness);<sup>26</sup>

56-60 on *On O'Neill's Method* (discussing O'Neill on Vaticanus in NTS 1989);

60-63 on *Barbara Aland's Method*.

### Focus on $\mathfrak{P}^{66}$

The wealth of material here presented about these six manuscripts is extremely extensive and represents something of a small monograph on each one. I propose to take one of them in order to say something more fully about Royse's approach, that is  $\mathfrak{P}^{66}$ .<sup>27</sup>

I begin again with a brief note of changes and new elements added between the dissertation and the book. Taking as his basis the corrected text of  $\mathfrak{P}^{66}$  (unlike Colwell, and to which we shall return) in the dissertation Royse counted 129 singular readings, which he classified as: 14 orthographic, 8 nonsense, and 107 significant (*Diss*, 391). In the book Royse now counts 128 singular readings, which are classified as 14 orthographic, 5 nonsense, and 109 significant (*Book*, 409). On *Book*, 407 note 45, he explains that in the *Dissertation* he had classified the singular reading in John 12.12 as an orthographic singular on the basis of the reading of Koridethi. Further, on *Book*, 408, Royse discusses the IGNTP edition's reading at 18.37 which would, if followed, provide an additional singular reading, but in an extended footnote discussing the particular ink traces at this point in the manuscript, he decides to follow the text as presented or reconstructed by the original editor. (*Book*, 408 note 47, displays an impressive command of the details).

In the *Dissertation* the corrections of  $\mathfrak{P}^{66}$  were discussed on 391-7. Here in the *Book*, we are treated to an extensive discussion (*Book*, 409-90) with what almost amounts to a commentary on the corrections of  $\mathfrak{P}^{66}$ —a great piece of work for which we are very grateful. But the basis for the treatment of the corrections is interesting. As I already mentioned Colwell had studied the singular readings in the original text of  $\mathfrak{P}^{66}$  and hence concluded that the scribe of  $\mathfrak{P}^{66}$  was "careless and ineffective", and that "wildness in copying is the outstanding characteristic of  $\mathfrak{P}^{66}$ ."<sup>28</sup> By contrast, Royse (in *Diss* and here in *Book*), takes the decision to base his discussion on the corrected text of  $\mathfrak{P}^{66}$  (hence the conclusion regarding the scribe of  $\mathfrak{P}^{66}$  that "the total activity is indeed rather careful" (*Book*, 498), and that he "exercises great care to render a literal copy of his *Vorlage*" (*Book*, 502).

Here in the *Book*, Royse has greatly expanded the treatment of the corrections, arguing that the scribe himself was responsible for the vast majority of the corrections (421), analyzing the process by which corrections were made as a basis for understanding the scribal behaviour. But I think that more could be made of this. I am not convinced that we need to choose between Colwell's approach and that of Royse. I think that a comparison between them might

<sup>26</sup> It is interesting that in his interaction with this point he studies only the singular readings in Rev 1-2 and does not deal with the whole range of this evidence. Jongkind's work is very relevant here as demonstrating that singular readings do actually represent scribal creations.

<sup>27</sup> Hopefully other contributors will gravitate to other aspects of the work.

<sup>28</sup> E. C. Colwell, "Scribal Habits in Early Papyri: A Study in the Corruption of the Text" in *The Bible in Modern Scholarship* (ed J. P. Hyatt; Nashville, 1965), 370-389. This study was later republished under a different title as "Method in Evaluating Scribal Habits: A Study of  $\mathfrak{P}^{45}$ ,  $\mathfrak{P}^{66}$ ,  $\mathfrak{P}^{75}$ " in *Studies in Methodology in Textual Criticism of the New Testament* (NTTS IX; Leiden, 1969), 106-124 (cited from here in what follows). The quoted phrases are from Colwell, "Scribal Habits", 118, 121.

also illuminate which types of scribal behaviour are more likely to be corrected. Just to take one example now: Royse analyses the 465 corrections as:

1. those which were corrected in the act of writing (49)
2. corrections of slips (164)
3. those that were significant and corrected after checking the *Vorlage* (126)
4. those that arose from comparison with another *Vorlage* (107).

In relation to the 126 significant corrections he notes that these places do exhibit “what errors the scribe initially made” (461):

Additions	12	9.5%
Omissions	61	48.4%
Transpositions	9	7.1%
Substitutions	41	32.5%
Conflations	3	2.4%

Royse notes “the strong tendency to omit rather than to add is apparent” (461). But one could also look at this and suggest that of all the errors which the scribe produced himself, omissions were those most likely to be spotted and corrected.

If it is the case that the same scribe wrote initially carelessly, and then self-corrected against the same exemplar, then we can learn about scribal behaviour from both of these actions, and perhaps even more from comparing the two. It is interesting to see the “pure” habits of the uncorrected text, it is worth noting what types of singular errors are corrected, and it is worth inquiring about the way the final text reflects the exemplar (although there are complications here which were not adequately acknowledged by Royse and which need to be addressed).

There are other features that arise from this discussion that should be noted here. Firstly Royse is very critical of the treatment of the corrections on  $\mathfrak{P}^{66}$  in the IGNTP (*Book*, 410 and *passim*; esp. 434-5: “corrections made *in scribendo* have systematically been cited very misleadingly in the IGNTP” ... “In none of the readings that were corrected *in scribendo* does the IGNTP make clear what happened.”<sup>29</sup>); he is also critical of the proposal of Comfort and Barrett (based on the earlier research of Berner) that three separate correctors can be identified (*Book*, 421).

### Concluding Comments

I really appreciate the full listing of evidence within this *Book*. For example, not only are the singular readings for each of the six manuscripts displayed, but one can see how the list was revised in various ways (as more evidence was checked) throughout the process. Beyond the amount of evidence adduced there are also very full discussions of many issues and extensive treatments of the corrections made to the manuscripts.

I appreciate the detail with which the argument can be followed and is documented. There is a kind of transparency here in tracking the scribal practice and behaviour. I also appreciate the general approach to evidence that is adopted (e.g. not as strict as the IGNTP on a number of issues and readings).

This offers fairly full documentation for a range of topics within scribal behaviour that are not particularly controversial, but which seem to be well supported with early evidence—e.g. harmonization to near context.

More things could be discovered from this basic evidence. For example, elsewhere in this conference we have debated the question as to whether the scribes of early Christian manu-

<sup>29</sup> Whereas Royse offers a fairly full commentary on these corrections, notes and explanations are not standard in the IGNTP.

scripts were Christians or not. The singular readings and scribal habits could contribute to this debate. Royse, for example, argues that the scribe of  $\mathfrak{P}^{66}$  is certainly a Christian (*Book*, 501) on the basis of the harmonizations to parallel passages elsewhere in the NT (as well as the use of *nomina sacra* and the staurogram). This could be extended similarly to the other papyri discussed here:  $\mathfrak{P}^{45}$  has harmonizations to parallels in other (canonical) gospels 8 times in the singular readings;  $\mathfrak{P}^{46}$  has harmonizations to the LXX (2 times), and from 1 Cor 11.24a to the parallel gospel text of the words of institution (Matt 26.26);  $\mathfrak{P}^{47}$  has a harmonization to Luke 4.33 at Rev 14.15;  $\mathfrak{P}^{72}$  has 7 harmonizations to remote parallels (in Col, Heb, Rev etc.);  $\mathfrak{P}^{75}$  has 5 singular readings which harmonize the text to remote (NT) parallels. This data suggests that the scribes have a general awareness of other NT texts, which suggests they were probably active participants in the life of the church.

I suppose it is worth noting the wider issue that Royse is attempting to revise the traditional canons—developed on the basis of medieval manuscripts and generalizations about scribal habits from them—on the basis of singular readings in the early papyri, which by definition made no impact on the wider scribal and textual tradition. On this question I think we still need to do some more thinking. Scribal habits determined on the basis of singular readings do I think (cf. Jongkind) reveal something about scribal behaviour, but may not be so clear about the general tendency of the textual tradition.

The dissertation was written on a type writer (according to the preface to the *Book*), an IBM Selectric typewriter, with a Greek “element” and other languages (and Greek accents and upper case, it seems to me) written in by hand. The book has been carefully prepared using modern technology—with Nota Bene 8.0c on a PC—and beautifully presented to us. I would congratulate the author, the series editors (Drs Epp and Ehrman), and the publishers on such a splendid piece of book production. I searched long and hard and have found one typo (401 note 14:  $\mathfrak{P}^{46}$  should presumably be  $\mathfrak{P}^{66}$ ); and one moment of unclarity (xvii para 2).

### 3. Review by Dirk Jongkind

There are many good things to be said about this book (as I am sure you will have noted by now). It gives us a close reading and discussion of the text and readings of six of the most important papyri of the New Testament, it provides a wealth of useful discussion of individual readings and particular phenomena, and it shows awareness of a vast range of literature. Besides, the original dissertation has been very influential on my own work and, among many things, I have benefited enormously from the introductory chapters in the original dissertation. I have seen the benefit of presenting all the evidence in an accessible format, I have learned from Royse about the most pragmatic way of reconstructing the reading of the *Vorlage*, and I learned about the important distinction between formal classification and integrated discussion of singular readings.

There are many good things, and whatever else I may be seen saying further down, it does not take away from the fact that this is a good, solid piece of work. Royse’s book is easy to criticize, but not because it is a bad book with blunders or a faulty argumentation. Any book that by its sheer size constitutes a serious assault on the strength of your bookshelves is bound to contain some inconsistencies or oversights. Any number of years of revision won’t get rid of that problem, on the contrary.

Which leads me to a general comment, which perhaps has to do with personal preference more than anything else. And that is this, the rather prolonged genesis of the book clearly shows, and does not make the work stronger. What I mean in practice is that there are too many footnotes commenting on too many, sometimes tangential points, using too much prose.

The footnotes go off into all sort of areas, comment on other areas, sometimes even start new discussions: it makes reading a single chapter sometimes feel like you are reading two or three articles at the same time: the main text, and the discussions going on in the footnotes. And the huge number of footnotes and references to secondary literature makes the modern author index often impractical—for many authors there are simply too many references, no-one is ever going to check all the references Royse makes to Kenyon, Lagrange, Metzger, or himself.

In addition I think that, in a sense, it is a little disappointing that after hundreds of pages and a myriad of footnotes on the singular readings of each of the six papyri, the summary of the copying activity of these manuscripts, the scribal habits this tome is all about, amounts to less than a single page per manuscript. The book is full of details, but the integrated reflection on these details is frustratingly short. An example is the following. At several places Royse observes that a transposition can be explained best by assuming that initially the scribe forgot a word, noticed this, and inserted it somewhat belatedly at the first possible opportunity. Because Royse pointed this out, I was able to see the same phenomenon in other manuscripts as well. I would have loved to see this type of scribal behaviour spelled out in the concluding sections, rather than to keep it hidden in the commentary. Royse makes many an excellent observation on scribal habits, but perhaps he could have provided a more ready access to these.

I would like to comment and discuss three points.

### 1. Royse on Barbara Aland: Singular Readings which Are Part of an Isolated Tradition

One of the problems in the use of singular readings as a window on scribal activity is the problem of the origin of a singular reading. Was it created by the scribe of our manuscript or did the scribe correctly copy a reading from his *Vorlage* which was created by a previous scribe but is labelled as a singular reading because the *Vorlage* or any other copy did not survive? Or in other words, when we have a singular reading are we dealing with the accumulation of errors of various scribes or of only one scribe?

Royse's argues against the assumption of singular readings as the accumulation of multiple scribes under three points. First he points out the highly contaminated nature of the textual tradition of the NT ("very few real 'dead ends'", 50). Secondly he endorses Colwell and Tune's view that many singulars will have been corrected by later copyists. This latter answer, of course, begs the question of how then any variant would come into existence: it seems to me that almost every existing non-authentic reading started its career as a singular reading that just happened to create a lot of offspring. Thirdly, and here I start to have some doubts, he says this (51; argued in 51-55):

Even if a manuscript under study were the outcome of some isolated stream of tradition, for most of our purposes this will simply not matter.

And a little further on (53):

Whether this frequency is due to the fact that the actual scribe of *a* [*a* is a manuscript accumulating the errors of two generations of scribes; DJ] is twice as careless in this respect as the scribe of *x*, or, as in the example, to the compound effect of two scribes equally careless in this respect as the scribe of *x*, is irrelevant to such evaluation.

I am not sure whether I am happy with such a description. What is meant by "most of our purposes"? What is the purpose of studying singular readings? Is it to study the characteristics of a manuscript or is it to study the scribe? (I admit there is an overlap but there is also a distinction between the two.) We can say that, rather than studying the work of a single scribe and having a look at his or her copying technique, we study the work of a tiny scribal tradition consisting

of multiple scribes and thus study the nature of a manuscript rather than of a scribe. But why, then, does Royse critique those scholars who claim to study the scribal habits of papyri and include non-singular readings? What is the theoretical difference between studying a group of readings taken from the early papyri that includes singular and non-singular readings, as for example Barbara Aland has done, and limiting oneself to just singular readings whilst admitting that these may form part of an accumulative process? If the purpose is to describe the tendencies as they exist in a manuscript, which is what Royse's discussion in this section seems to imply, then there is no reason why one should exclude non-singular readings. If, on the other hand, one's aim is to describe the copying technique and scribal habits of an individual scribe, one must make a better case for the notion of why the singular reading reflects the work of an individual. To an extent, in studying and describing the six papyri, Royse never comes back to this notion that he is studying the accumulated errors of different scribes, and I believe rightly so. In my own work on *Codex Sinaiticus* I found that the type and frequency of singular readings changed noticeably from scribe to scribe within the same manuscript and book within that manuscript, and that a high proportion of the singular readings are scribe-related, rather than manuscript-related. I think that Royse can be confident that by and large we are looking at the signature of specific scribes.

## 2. Royse on Klaus Junack: How Are the Papyri Copied?

On page 43, note 16, we are helpfully informed that Hurtado's work on Codex W appeared in the same year as Royse's dissertation. Another important work also appeared in 1981, namely Klaus Junack's contribution to the Metzger Festschrift of that year "Abschreibpraktiken und Schreibergewohnheiten in ihrer Auswirkung auf die Textüberlieferung,"<sup>30</sup> an article reflecting on the nature of copying and of scribal habits. This is, in my opinion, an extremely important essay, analysing how the copying of a Greek text written without word-division works out in practice and how the various stages of copying, from reading the text to the actual action of the hand in writing, can affect the outcome of a single act of copying.

Royse is aware of this work. There are references to this essay: in a footnote on page 84, in the context of studies on how reading aloud affects copying; on page 86 where Junack's rejection of the use of dictation for Codex Sinaiticus is noted; on page 100, noting that Junack did not find any indications from the ink for the unit of copying; on page 521, note 648, where a couple of singular readings of  $\mathfrak{P}^{66}$  are discussed and similarly for readings of  $\mathfrak{P}^{72}$  on page 572 and 573. On page 663, note 261, Junack is reported as seeing a tendency in  $\mathfrak{P}^{75}$  of omitting certain particles, but this notion is not taken up for discussion any further. On page 672, note 299, another reading of  $\mathfrak{P}^{75}$  is mentioned, which Junack mistakenly took as a singular reading, and finally, in a footnote on page 718 of the chapter on "The Shorter Reading", Junack is quoted approvingly as describing the tendency that during writing our thoughts tend to go quicker than our hand can follow.

Why this overview? First of all, it shows that an author index to a book can come in quite useful, as long as there are not too many references to a single author. But more importantly, in none of the above-mentioned places is there a reference to an interesting point Junack also made in his essay. At one point Junack directly attacks some of Colwell's conclusions regarding  $\mathfrak{P}^{45}$ ,  $\mathfrak{P}^{66}$ , and  $\mathfrak{P}^{75}$ . Colwell had written that " $\mathfrak{P}^{75}$  copies letters one by one;  $\mathfrak{P}^{66}$  copies syllables, usually two letters in length;  $\mathfrak{P}^{45}$  copies phrases and clauses." Junack (288-9) disagrees thoroughly

<sup>30</sup> Klaus Junack, "Abschreibpraktiken und Schreibergewohnheiten in ihrer Auswirkung auf die Textüberlieferung," in *New Testament Textual Criticism: Its Significance for Exegesis. Essays in Honour of Bruce M. Metzger*, ed. Eldon Jay Epp and Gordon D. Fee (Oxford: Clarendon Press, 1981), 277-95.

with these conclusions, or at least with the conclusion as applied to letter by letter, or syllable by syllable copying. This would reduce copying to a mechanised process in which just signs rather than meaningful units were copied. Any error based on nonsense meaning would only show up in the correction phase but not during the actual copying of the letters or syllables as the copying is reduced to a level below meaning. Without doubt Junack is correct in his critique, also because it is based on a fuller understanding of what copying is (as explained by Alphonse Dain). Moreover, Junack here directly attacks the results, the integrated reflection on the nature of singular readings made by Colwell. How does Royse deal with this?

With regard to  $\mathfrak{P}^{66}$  Royse simply repeats the conclusion of Colwell that its scribe copies syllables (493). Royse also refers to Fee who notes, more cautiously, that dropping a syllable “is a regular habit of the scribe” (493, n. 502). This underlines the point I want to make here: there is a vast difference between describing the actual resultant types of errors, such as dropping a syllable or swapping two letters, and the conclusion one makes regarding the way a scribe actually goes about his work in practice. Copying a single syllable, a single letter?

The discussion of  $\mathfrak{P}^{75}$  does not fare better in this regard. On page 653 Royse’s data “confirm Colwell’s view that the scribe of  $\mathfrak{P}^{75}$  copies (at least usually) letter by letter, or at any rate syllable by syllable.” I believe that Colwell’s phraseology was careless, that Royse took it over too easily, and that a golden opportunity was missed by ignoring Junack on this point. Scribal habits are not just about the types of errors we find; it is, I believe, also about how a scribe copies. And conclusions on how the scribe copies are important, they are even necessary, but they should also be carefully considered, weighed and discussed.

### 3. Royse and Griesbach: The Shorter Reading?

I am on record as having written that “perhaps Royse comes close to misrepresenting Griesbach”.<sup>31</sup> This has to do with how Royse discussed Griesbach’s canon of preferring the shorter to the longer reading in his dissertation. The word “misrepresenting” had to do with the fact that I thought that Royse had not given enough weight to the exceptions to this canon that Griesbach himself lists. My argument was roughly as follows:

- Griesbach prefers the shorter reading.
- However he lists exceptions to the rule.
- These exceptions are the following, which means that the longer reading has to be preferred in the following cases:
  1. homoeoteleuton
  2. what was omitted does not appear correct to the scribe (for a whole range of reasons)
  3. what is lacking does not harm the sense or the structure of the sentence
  4. if the longer reading is more in accord with the author’s style
  5. if the shorter reading makes no sense
  6. if harmonisation to parallel passages plays a role.
- Most of the omissions which Royse has found fall under the first three of Griesbach’s exceptions.
- Therefore Griesbach’s canon still stands; the only thing Royse did was to flesh out Griesbach’s exceptions; Griesbach was mainly talking about the real, substantial variants, not about the stuff that tends to fill up the category of singular readings.

[This is a slight caricature of what I wrote, but I think I should be allowed to do that.]

The reason why I want to save something of Griesbach is that it is my impression that

<sup>31</sup> Dirk Jongkind, *Scribal Habits in Codex Sinaiticus*, Texts and Studies, Third Series, Volume 5 (Piscataway, NJ: Gorgias Press, 2007), 139.

traditions tend to grow over the course of centuries: they pick up extra phrases, extra verses, sometimes an extra ending, or a whole story is inserted. Though on one hand we see that it is in the nature of the actual process of copying that a text is more likely to come out shorter than longer—as I believe has been convincingly demonstrated by Royse (and others since)—there must be a way to account for all the extra bits that texts tend to pick up. I need something of a canon like Griesbach formulated to account for this overall growth of texts.

We have of course the explicit tendency expressed by Eusebius (*ad Marinum*):

On the other hand, someone else, who *dares to set aside nothing whatsoever of the things which appear, by whatever means, in the text of the Gospels*, says that the reading [long ending of Mark; DJ] is double ...<sup>32</sup>

There is a reluctance to throw anything aside that could be part of the Gospel tradition. This tendency may go back to at least the early third century with Origen, who decided to obelise the passages in his Greek Old Testament rather than to excise them altogether. This may be part of the phenomenon Griesbach wants to describe.

A second factor that would lead to a growing text are editorial readings, believed to have a tendency to expand an initial shorter reading. I am not convinced yet that this phenomenon has been demonstrated clearly. Are editorial scribal creations always longer than the original? I believe it is true for the longer text of Acts, but I do not think it is true for Matthew and Luke where they edited Mark (this last example is of course beset with problems, but interesting nonetheless), and it has been claimed that *Codex Vaticanus* has a tendency to produce too clean a text. There is clearly space for further investigation into scribal habits here.

A third factor could be the conflation of readings, especially the incorporation of corrections as additions rather than as substitutions. But again, I am not sure to what extent this case has been made empirically. In the same vein, one of the main reasons for why texts grow could well be that in the correction phase it is more likely to add something to a text (correctly or incorrectly) than take away something from the text. If a scribe compares his freshly made copy with another manuscript, it may be more likely that he is looking for things to add to his own copy than to take out. However, I do not have extensive data on this (though it is true in the case in Sinaiticus - Chronicles) and it would make for a good dissertation topic. Probably the data for the six early papyri could be dug up from Royse's work somewhere.

Yet, ultimately, I have to agree with Royse on Griesbach's canon. Royse is absolutely correct in dismissing the short and even the more nuanced version of the *lectio brevior lectio potior* canon; it puts us on the wrong foot. Every canon should in my opinion start with an awareness of the hazardous nature of copying and the many types of complicated clerical errors that can arise. Instead of formulating the canon in terms of the shorter reading, the term "expansionary" might be better. A reading which appears to be an expansion of an alternative reading should not be preferred, thus bringing the actual content of the extra words into play. But more remains to be said here and though Royse made a start in his book, I am not sure we are there already.

All in all, I want to thank James Royse for his work. It has been a really positive influence on my own work, and likewise I hope it will prove so to be on the rest of the discipline. Its main thesis is correct, I believe, and the study provides a wonderfully deep insight into each of these papyri.

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<sup>32</sup> Translation taken from James A. Kelhoffer, "The Witness of Eusebius' *ad Marinum* and Other Christian Writings to Text-Critical Debates concerning the Original Conclusion to Mark's Gospel," *ZNW* 92 (2001): 85.

## 4. Response by James R. Royse

First, let me thank AnneMarie Luijendijk for organizing this session, and my fellow panelists for their perceptive and generous remarks. It is, of course, an honor for an author to have one's work reviewed by such distinguished scholars of the text of the Bible. And I am pleased, and more than a little embarrassed, that my work, begun when we were all very much younger (if alive at all), has received such positive reception. Time does not permit a full discussion of all the points that they raise. Very generally I would wish to second the calls for further investigation of various issues; there is much to be done in the study of these six papyri and other important manuscripts, and in the study of scribal habits, and there are too few of us to give adequate attention to all the readings in all the manuscripts. Nevertheless, the panelists have raised some points to which I would like to reply, in the spirit, well illustrated by their remarks, of scholarly cooperation in advancing our common goal of shedding light on the process of the transmission of the text of the New Testament.

I will basically follow the order of the speakers, but with some cross-references.

First, there is Juan Hernández:

I appreciate Hernández's comments, and am pleased to have been associated in some small degree with his doctoral work. I confess that some of the details of my dissertation have faded from my mind with time, and have been overwritten with the revisions and expansions found in the current book. Indeed, hearing Hernández's description was in some ways like hearing of someone else's work. But I am very grateful for his kind words, and for his comments about the role that my dissertation played in his own study of the New Testament text.

As a very small footnote to all this, the appendix in my dissertation on  $\mathfrak{P}^{46}$  and the Ethiopic, to which Hernández refers, was not included in the revised book. Rather it is cited as a forthcoming article. At some point I decided that separate publication would be more appropriate, and would save some space in the current book. However, I never seem to be able to put the finishing touches on the article. Perhaps the enthusiasm of the current session will enable me to conclude that little work as well, although for now it exists only as an appendix in the dissertation.

Next, Kim Haines-Eitzen:<sup>33</sup>

Both Haines-Eitzen and Jongkind draw attention to the brief summaries of scribal habits for each of the six papyri. Haines-Eitzen questions whether the summaries "help us get a sense of a scribe," and Jongkind remarks that a one-page summary of scores of pages of analysis is not all that helpful. Of course, to provide extended "integrated reflection" (as Jongkind puts it) on the scribal habits would have been to risk expanding the book even more. But perhaps in such extended discussion I could have avoided some of the tensions (shall we say) in the summaries that Haines-Eitzen points out. Alternatively, perhaps it would have been better simply to forego such summaries and let the analyses speak for themselves. At least that would have avoided the shortcomings, and omission is always so much more tempting than addition.

I turn to Haines-Eitzen's concluding three points (or "questions").

Point 1: I would concur that the physical features of manuscripts can be crucial. Haines-Eitzen, of course, has given much attention (in her provocative study, *Guardians of Letters*) to the peculiarities of  $\mathfrak{P}^{72}$  and of the codex of which it forms a part (or perhaps two parts, 1–2 Peter and Jude), giving careful attention to the curious features (scribal and otherwise)

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<sup>33</sup> Editors' note: Kim Haines-Eitzen's review was not available for publication.

of the disparate texts joined there into this “third-century miscellany” (as she calls it), and arguing that it was the product of an early scribal network. And Jongkind, in his recent work, *The Scribal Habits of Codex Sinaiticus*, creatively combines many aspects of that very complex manuscript, such as the arrangement of the quires, the nature of the paragraphing, the use of *nomina sacra*, and the scribal tendencies to produce certain sorts of variations, all sorted out among the three scribes and the various correctors. Here again, I would not wish to be seen as in any way claiming completeness or finality in my analysis, and I would welcome further and more comprehensive discussions. Especially the codex to which  $\mathfrak{P}^{72}$  belongs seems, as Haines-Eitzen well observes, to be a different sort of physical object than the usual New Testament manuscript, and it would be natural that its unique properties should have implications for our understanding of its text and for our evaluation of its readings for purposes of textual criticism. I might refer further to the fascinating analysis of the “Bodmer Miscellaneous Codex” by Tommy Wasserman in chapter two of his recent *The Epistle of Jude: Its Text and Transmission*, which integrates codicological and textual considerations of this compilation.

Point 2: I would not wish to appear at all certain on such a complex topic as the theological corruptions of the text. But I would observe that it is (I believe) perfectly consistent to hold that dogmatic changes began to occur around 300 and that the majority of textual variants arose during the first three Christian centuries (that is, before 300). We have to keep in mind that the vast majority of textual variants do not involve (as it seems) theological corruption. So, while most textual variants may have arisen early, the comparatively few theological corruptions could have been late on the scene. Of course, others have thought to find theologically motivated readings in, say,  $\mathfrak{P}^{46}$ . I have not been inclined to agree, but in any case the numbers of such readings would be, I believe, comparatively small; but that doesn’t mean that they didn’t exist.

Point 3: I used the term “textual type” in connection with the papyri with some hesitation (see, e.g., 15 note 52). Of course, in calling, for example,  $\mathfrak{P}^{72}$  Alexandrian, I was simply repeating what others have said, and while such terminology may be anachronistic for the early papyri or otherwise problematic, it seems to me to be useful shorthand for describing the textual relations. But I would hope that my investigation does not depend to any great degree on such characterizations.

Next, Peter M. Head:

All of the speakers have remarked upon the history of this work, its first appearance as a dissertation, and then its second appearance in the present form. But Head has given most attention to the continuities and changes, and seems better able than I am to describe what has happened. Certainly, as I revised and expanded the dissertation I was well aware that there were some dangers in the expansion, and that perceptive readers (such as our panelists), trained to detect layers of textual accretion, would be able to see that the material from 1981 did not always flow smoothly into the material from 2006, as both Jongkind and Head on occasion note. I am honored that Head has given such careful attention to the development of this work.

Also, his decision to look at the work on  $\mathfrak{P}^{66}$  in more detail reflects my own understanding of the importance of that manuscript. Particularly the study of the some 465 corrections seems to me to shed much light on the nature of copying a New Testament book around the year 200. Although I devoted a great deal of time to an analysis of those corrections, crucially aided by the studies of Gordon Fee and Errol Rhodes and many other scholars, I suspect that there is yet much to be discovered. What is especially interesting is that in  $\mathfrak{P}^{66}$ , as also in  $\mathfrak{P}^{46}$  and again some 150 years later in Codex Sinaiticus, the extensive corrections preserve in one manuscript several layers of textual change that can, at least in theory, reveal much about what was hap-

pening to the text during the early period. Of course, the challenge is to organize what lies in such layers in a perspicuous manner.

I mention a few points very briefly. In his note 5 (note 26 above, ed.) Head wonders about my decision to confine attention to singulars in A occurring in the first two chapters in Revelation; I chose the first two chapters for examination simply in order not to digress too much, and my point there was a very limited one. Naturally, a wider and more thorough investigation, as we now have in Hernández's work, *Scribal Habits and Theological Influences in the Apocalypse: The Singular Readings of Sinaiticus, Alexandrinus, and Ephraemi*, is much to be welcomed.

With respect to Head's finding "one moment of unclarity" at page xvii, second paragraph, line 2: let me note that one should delete the phrase "and asterisked"—I honestly don't know what happened there, but it is an unwarranted addition. And at page 401 note 14, line 2 from the end: yes, "Ϡ<sup>46</sup>" should be "Ϡ<sup>66</sup>."

Finally, Dirk Jongkind:

Jongkind has pointed to some specific features that could bear improvement. Some of his points are matters of style and presentation, and I am probably not the best judge of what is good or bad there. But I would like to respond briefly on a couple of points.

First, let me confess that I like footnotes. In the works of others, footnotes are often among my favorite parts. And, for what it is worth, some of my favorite parts of this book are in the footnotes. During the writing, as I pursued various paths of enquiry, I often thought of the comment of Herman Melville in *Billy Budd* (Chapter 4): "In this matter of writing, resolve as one may to keep to the main road, some bypaths have an enticement not readily to be withstood." Of course, I may have given in too often to such enticement, and perhaps for the sake of clarity and explanation I should have cited Melville's comment in a footnote.

Second, as already noted, Jongkind wonders about the value of the summaries. But let me say something about Jongkind's particular example of a scribal habit: "that a transposition can be explained best by assuming that initially the scribe forgot a word, noticed this, and inserted it somewhat belatedly at the first possible opportunity." This does seem to me to be an important point (although I would replace "forgot" by "omitted"), and I believe that one can often see this habit at work in various manuscripts. Perhaps I should have emphasized it more. But it does not seem to me to be "hidden in the commentary" (as Jongkind says). It is mentioned in the text in the discussion of the transpositions of each of the six papyri. And at each of those places there are—dare I say?—footnotes that will lead the reader down a bypath to find in Supplementary Note 9 (755–6) that this observation was made by Colwell and even earlier by Havet, Hoskier, and others. Of course, more could have been said. Perhaps some day I will say more on this topic, but in the meantime I encourage others to investigate this tendency in these six papyri and in other manuscripts. Many other tendencies deserve more extended treatment. After all, I attempted only a "partial fulfillment" (as I say on 101) of the goal of a commentary on the singular readings of these papyri.

Third, I appreciate Jongkind's comments on my use (or lack thereof) of the interesting and important essay by Junack. And I will concede that more could have been done with this issue of whether a scribe is copying by letters, by syllables, by words, or by some larger units. I appealed to the distinction as made by Colwell on occasion, but did not collect systematic data or attempt to draw the data together into some overall perspective. I would encourage others to do so. However, I would say that (a) my chief interest was "about the type of errors we find," and that (b) trying to say anything more seems to me to run the risk of attempting to gain insight into the psychology of the scribe. And this latter task is very difficult, to say

the least. I believe that we can see that a scribe tends to omit syllables, let us say, and that that “type of error” can inform our evaluation of the scribe’s readings. I would hesitate to say much more. However, at least one could attempt to determine what general patterns there are with respect to letters, syllables, and so on, in a scribe’s errors. And my own attempts there were not systematic.

Fourth, with respect to Jongkind’s discussion of isolated textual traditions, let me say that, in my opinion, the primary purpose of studying the scribal habits of manuscripts is to sharpen or revise our analysis of readings. This happens on different levels. At the first level, we can hope to find that a particular manuscript displays specific tendencies in its errors, and we can then use those tendencies in our assessment of the value of that manuscript at some particular variation unit. For example, if we find (as I believe we do) that  $\mathfrak{P}^{46}$  tends to omit portions of the text by a leap from the same to the same, then  $\mathfrak{P}^{46}$ ’s support for a reading that can be so explained may be, to that extent, discounted. That is, we will find it more likely that  $\mathfrak{P}^{46}$  created that shorter reading, and thus less likely that that shorter reading goes back to the exemplar of  $\mathfrak{P}^{46}$ . For such an inquiry the singular readings provide, I believe, the best evidence for the scribe’s tendencies. And then at the second level, we can hope to generalize on the tendencies found in specific witnesses. That is what happens in the canons of internal criticism. If we find (as I believe we do) that scribes in general tend to omit portions of the text by a leap from the same to the same, then we may reject readings that could have arisen in such a way.

Now, my general point about isolated traditions was that, for such purposes, whether we have the work of one scribe or the combined work of several scribes is irrelevant for the assessment of readings.

However, I completely agree with Jongkind’s summary comment that it is much more likely that we do not have such “complex scribes” in the New Testament tradition. In fact, though, Jongkind’s work on Codex Sinaiticus (144–7) provides one apparent example in the Septuagint: we have there a passage from 1 Chronicles, namely 9:27–19:17, inserted into 2 Esdras. Jongkind notes that this insertion is unique to Codex Sinaiticus, but that there is no sign of correction. What he then (reasonably enough, as it seems to me) infers is that the exemplar of Codex Sinaiticus (i.e., the exemplar at this point of the Septuagint) had the same insertion, and was used for both the initial transcription and the earliest stage of correcting activity. If this is so, we can see a little isolated tradition, consisting of that exemplar and Codex Sinaiticus. But, as Jongkind notes, the textual evidence for 1 Chronicles is comparatively weak, as Brooke and McLean cite only 25 manuscripts. And certainly the breadth and complexity of the manuscript tradition of the New Testament make such isolated traditions much less likely.

Fifth, I turn to Jongkind’s discussion of the shorter reading. Here of course I did attempt to integrate the results for the six papyri, and to say something about the implications those results have for the canon of preferring the shorter reading. Jongkind is correct in reminding us that Griesbach’s first canon is a much more nuanced, and much more complicated, piece of advice than the principle of simply preferring the shorter reading. Indeed, I suspect that the nuances and the complications are precisely what have caused it to be replaced in many subsequent lists of canons by simpler and more direct principles. That is, perhaps everyone will agree with Hort that “scribes were moved by a much greater variety of impulse than is usually supposed” (cited in chapter 1, footnote 35). Nevertheless, having a canon of criticism that tells us that scribes do this, and also that, and then sometimes something else except when they are doing some other thing, true as it may be, may not really provide much guidance in choosing among readings. Ultimately, editors of the text and most critics of the text want to make choices of some kind or other. And for that purpose simple, direct principles are the most useful. For example: “Prefer the reading of Vaticanus and Sinaiticus.” “Prefer the reading that is not

harmonized.” “Prefer the shorter reading.” Those are the sorts of principles that inform most modern texts. Indeed, we have on record in Metzger’s *Textual Commentary* the principles used to construct, or at least to justify, our current “standard” text in Nestle-Aland. And we see there nothing like Griesbach’s first canon. Moreover, it is not all that often that one sees even in the totality of discussion of a variation unit such disparate points that Griesbach combines. Of course, most variation units receive no discussion at all. But the ones that do often involve the conflict of two or three principles. The shorter reading is not supported by B and  $\aleph$  and friends, or the reading of B and  $\aleph$  agrees with a parallel. Under those circumstances the editors have to balance principles. But if one has to balance the sorts of things that Griesbach tries to balance in his first canon, one may give up in despair at ever reaching a decision. (Of course, such considerations do not show that Griesbach’s canon is false.)

Finally, I should note that Jongkind’s own study of Codex Sinaiticus has provided yet further evidence that early scribes tended to omit rather than to add. This adds to my conviction that the preference for the shorter reading is fundamentally mistaken. And I wonder if there is, or really ever was, any evidence at all that scribes tended to add. In any case, there is increasing evidence, from the work of Hernández on Revelation, of Head on the early less extensive papyri, and of Jongkind on Codex Sinaiticus, that omission was more common than addition, and thus that the scribal tendency underlying the preference for the shorter reading is illusory.

Of course, we have here an overall tendency. Within that tendency there may still be plenty of additions, as we see in these six papyri, which could account for Jongkind’s observation that “traditions tend to grow over the course of centuries”. Also, the text may have been affected not only by the changes introduced by scribes but also, as Jongkind notes, by the efforts of revisers, redactors, and editors. As extreme examples, we may think of the ways that Matthew and Luke, at least on the two-document hypothesis, handled the text of Mark. Clearly, their *overall* tendency was to expand. But *that* tendency to expand did not prevent Matthew and Luke from omitting on occasion, as at Mark 1:32, where each adopts one clause of a redundancy in Mark, or Mark 4:26–29, the parable of the seed growing secretly, where each of Matthew and Luke chooses to omit the passage entirely. Now, I would not wish to define precisely the difference between copying activity and editorial activity, although, like other things, we usually know them when we see them. And thus we distinguish the *scribe* of  $\mathfrak{P}^{46}$  or  $\mathfrak{P}^{66}$  from Matthew or Luke or Origen. But within the transmission of the New Testament (or the Septuagint or the Masoretic Text) there are many factors at work, and surely we cannot expect to explain the complications that we find by appeal to anything other than complex, sometimes conflicting, tendencies. However, despite such qualifications, I believe the evidence strongly supports the view that early scribes of the New Testament tended to shorten the text. And that is, if not the entire story, at least an important part of the story.

Again, I thank all of the panelists for their insightful and stimulating remarks.