

over the most important service rendered by Jeffrey's text may be his application of the "hermeneutics of suspicion" to those who invented the very term.

This may well be the timeliest of all timely books for anyone interested in the convergence of literature and religion, aesthetics and ethics: the world and the Word. Christian scholars and theologians have of late been getting it from all sides—charged, on the one hand, by post-structuralists and their ilk with regressive logocentric longings, and on the other by neo-Romanticists like Colin Falck, for being unwilling to marshal their spiritual resources against the sterility of postmodern criticism. David Lyle Jeffrey and the *People of the Book* answer both challenges.

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*Eyewitness to Jesus: Amazing New Manuscript Evidence About the Origin of the Gospels.* By Carsten Peter Thiede and Matthew D'Ancona. New York: Doubleday, 1996. ISBN 0-385-48051-2. Pp. xvi+206.

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In its attempt to popularize scholarly research on gospel origins, *Eyewitness to Jesus* seems to have successfully stimulated the interest of readers who are not specialists in the field. It was one such "layperson" who encouraged me to review the work. And indeed I found in *Eyewitness to Jesus* a spirited and entertaining introduction to the field of New Testament studies and an initiation to important issues of papyrology (the study of papyrus manuscripts). The work's sensational claims about the history of the gospels and of their transmission constitute its most important—and problematic—element. The following evaluation will concentrate on these claims, their importance, and the argumentation from which they stem.

The issue behind the provocative title of the publication is the recent effort by papyrologist C.P. Thiede to redate the Magdalen Papyrus, an assortment of three manuscript fragments displaying verses from chapter 26 of Matthew's Gospel, found in Egypt around 1900 and stored since 1901 in the Library at Magdalen College, Oxford. While contemporary scholarship has settled on an approximate date of 200 C.E. for the production of the Magdalen manuscript (also called p64 by specialists), Thiede claims that he can make an irrefutable case for an early dating to approximately 66 C.E.

The authors use this result to affirm that Matthew certainly wrote his gospel within a *context of eyewitnesses* to the living Jesus. If Jesus died c. 30 C.E. (as a majority of scholars would agree), the Gospel of Matthew, which contains an account of his death, would have had to be written after 30 C.E. and well before 66 C.E., that is, within some thirty years of the crucifixion. Moreover Thiede and D'Ancona contend that if this "eyewitness context" for the composition of Matthew is proven, the entire history of gospel origins as understood by most scholars today will have to be rewritten. This would challenge the commonly held premise that the authors of the four New Testament Gospels can provide us with no guar-

antee of the authenticity of the contents of their writings, because they wrote as non-eyewitnesses in a context of non-eyewitnesses (some thirty-five to seventy years after the crucifixion).

Without denying the possibility that one day this theory of a very early Matthew may be found true, the data this book adduces and the argumentation it deploys are not sufficient to establish the point. Two questions must be dealt with: (a) Do Thiede and D'Ancona have a case in the redating of p64 to c. 66 C.E.? (b) Can an eyewitness context for Matthew necessarily be inferred from this redating of p64?

Let us begin with the second question: assuming that the authors were right in establishing the actual date of the Magdalen Papyrus to somewhere between 30 and 66 C.E., would this prove that the Gospel that was copied onto it was written for eyewitnesses, and probably by an eyewitness? Not necessarily. For one thing, an important principle of historical methodology must be considered here: whenever a result of papyrological dating is used in the reconstruction of an historical event or process, it is always the latest possible date that must be used.

The application of the principle in this case should go like this: if p64 was copied somewhere between 30 and 66 C.E., then, for the sake of a prudent reconstruction of the history of Matthew's gospel, we must date p64 to c. 66 C.E. This means that the gospel itself could still have been written as late as 60 C.E., in which case the "tunnel period"—the interval "separating the life of Jesus from the work of the Evangelists" (164)—could still span some thirty years. This figure is simply too high to ensure "eyewitness context" to the degree of certainty that the authors claim. Compounded with the difficult issue of the tunnel period's duration are the questions of the location and circumstances of composition of the Gospel, as well as the identity of the addressees. It is very plausible that the Gospel could have been written in a "non-eyewitness" context.

Now to the first, most important question: is the redating of the Magdalen Papyrus from c. 200 to c. 66 C.E. convincing? Again, the answer cannot be affirmative, because the process of palaeographical classification of p64 cannot be verified by the reader. This procedure aims to connect the handwriting style of the fragments with a particular time period. With this objective in mind, the authors call to the stand a number of other ancient manuscripts, in an attempt to show two things: (a) that the handwriting technique of p64 has many dissimilarities with the manuscripts dated at the end of the second century; and (b) that it bears many resemblances with a number of manuscripts dated between 50 B.C.E. and 70 C.E. Throughout the section controversial opinions about similarities and dissimilarities of style are affirmed without photographs. This is all the more disappointing in that in a previous chapter, the study of the Qumran fragment 7Q5 (done for the sake of illustration) is accompanied with detailed photos!

The *pièce de résistance* is the stylistic comparison of p64 with "key witness" P. Oxy. II 246. This manuscript exhibits a personal letter written on papyrus and dated with certainty to 66 C.E., because the date of composition is actually displayed in the text ("In the year 12 of Nero the Lord"). But here again only an affirmation of style similarity with p64 is afforded, and there is no rigorous proof, no photos. What adds to the reader's disappointment is a very untimely discus-

sion of a secondary issue of vocabulary (125). While this is not uninteresting, it is irrelevant to the argument and strikes the careful reader as a red herring.

Despite its failure to make a sufficiently strong case for the redating of the Magdalen papyrus, the volume is provocative in the important questions of historical method that it raises: what place and authority should papyrology be given in the study of gospel origins? Are the results of this science precise enough to take precedence over literary and historical-critical observations? How should these results be used? It is simply unfortunate that the answers given tend to exaggerate the precision of papyrology's results and disdain other modes of historical inquiry.

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*Curators of the Buddha: The Study of Buddhism under Colonialism.* Ed. Donald S. Lopez, Jr. Chicago and London: University of Chicago Press, 1995. ISBN 0-226-49309-1. Pp. 298.

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*Curators of the Buddha* represents the first in what is likely to be a long line of scholarly efforts in the genre of "postcolonial" Buddhist studies. The overall aim of the volume is to contribute to a cultural analysis of the history of the study of Buddhism. Following the kind of Orientalist critique modeled by Edward Said, each of the essays that comprise the book traces the genealogy of one of the *idées reçues* within Buddhist studies. This is achieved primarily by delineating the social and historical context of the scholarly figures associated with them.

In an introductory chapter Donald Lopez outlines Orientalist themes in the history of Buddhist studies. While he rightly acknowledges that there are limitations in applying Said's critique to Buddhist studies—most importantly in that the direct political role of the study of Islam in colonialism is not apparent in the study of Buddhism—he highlights several other features of Orientalism that can be said to characterize Buddhist studies. Most notable are: the enduring tendency of Buddhologists to privilege ancient texts in classical languages over vernacular, oral traditions; the tendency to overlook the context within which texts were produced and a marked neglect of non-religious aspects of Asian history and culture. He argues that these features have led to the creation of a reified object called "classical" or "original Buddhism" that has served for the Orientalist as a kind of transhistorical repository of ancient wisdom, and a standard by which to judge, usually unfavorably, the practices and beliefs of contemporary Buddhists in Asia. The general thesis of the book is that Buddhist studies has largely functioned as a kind of Romantic Orientalism. In the remaining chapters the details and nuances of the theme of Buddhist studies as Romantic Orientalism are elaborated.

Charles Hallisey's "Roads Taken and Not Taken," for example, explores the history of privileging ancient, classical sources over modern, vernacular texts through the work of T.W. Rhys Davids and some of his lesser-known colleagues.