

core of Northern fundamentalists (and their heirs in early Neoevangelicalism), but whether this model will work for the majority of post-1960s evangelicals is seriously in question.

Finally, two further features of these essays should be noted: 1) the rich bibliographical references in the footnotes read like a Who's Who of the most important edited symposia and monographs in recent evangelical historiography, providing a ready guide to the more recent Reformed, Princeton-oriented evangelical studies; 2) Eerdmans is to be commended for using footnotes rather than endnotes, thereby providing quick and easy reference.

Indeed, Marsden's *Understanding Fundamentalism and Evangelicalism* could be profitably employed in a survey course on American church history for undergraduates or even serve as supplementary reading in a graduate seminar on evangelicalism or 19th- and 20th-century intellectual history.

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Meier, John P. *A Marginal Jew: Rethinking the Historical Jesus, Vol. 1, The Roots of the Problem and the Person*. New York: Doubleday, 1991. x + 484 pp. \$25.00.

From the late eighteenth century onward, those who have rejected the NT picture of Jesus have struggled to come up with a "historical" Jesus, a Jesus whose persona can be confirmed through conventional historical research. The results of these "quests for the historical Jesus" have been disappointing at best. Though each new picture of the historical Jesus meets initially with enthusiastic scholarly acclaim, it is never long before the "new" historical Jesus is scornfully rejected by those with a different image to put forth. In *A Marginal Jew*, John P. Meier reexamines the quest for the historical Jesus and once again sets out to see what, if anything, can be known about Jesus through the application of the historical method—or at least through what he maintains is the historical method.

Meier devotes much of the first half of this volume to showing just how limited the sources for the historical Jesus are. Secular material, i.e., the scattered references in Tacitus, Lucian, Suetonius, and Josephus, show that Jesus lived and was executed and give a rough estimate of when these things happened, but do little more. Nor are the many recently discovered apocryphal gospels of much use in discovering the historical Jesus, since they are demonstrably dependent on the canonical gospels.

The accounts of Matthew, Mark, Luke, and John are thus the only really valuable sources in reconstructing the historical Jesus. But here, too,

Meier has some reservations. He rejects, out of hand, any attempt to equate the biblical picture of Christ with the historical Jesus as "naïve."

But if one refuses to accept the entirety of the biblical account, how can one glean from these texts any reliable picture of the historical Jesus? Meier suggests five primary criteria for identifying elements of the biblical account that are properly associated with the historical Jesus, the most important of which are the principles of "embarrassment" and "discontinuity." Essentially, he maintains that we are to accept as authentic those passages on Jesus which the church might have found embarrassing or which do not fit with the doctrine of the supposed redactors of the Gospels.

The criteria Meier chooses mirror the central fallacy of most redaction criticism, namely the assumption that incidents used to illustrate a particular theological point most probably stem from the redactor. For example, to argue against the historicity of these narratives, Meier uses the fact that the infancy narratives of Luke and Matthew each emphasize the author's christology. But is it valid to question the historicity of information simply because it happens to support an idea the writer is trying to convey? All ancient historians select from the available evidence those incidents that will best illustrate whatever point they are trying to make. Herodotus, for instance, is especially likely to include material that shows men punished for their pride. Suetonius records in detail unusual events surrounding the births of each of his 12 caesars in an attempt to show that there were portents of future prominence in every case. Both writers are making "theological" points, but few contemporary historians would automatically reject the evidence they present on this basis. Indeed, if they were to adopt such a standard, it would become next to impossible to write history at all. The actual standard generally used by historians with nonbiblical material is to accept (at least tentatively) the evidence presented by the sources unless there is a fairly strong reason for doing otherwise. Thus, the historical method which Meier applies to the Bible—rejecting its evidence unless there is compelling reason for not doing so—is really the reverse of what historians most commonly do in evaluating their sources.

Nevertheless, Meier's hyperskepticism would be valuable if applied consistently. Unfortunately, his argument is somewhat less than rigorous. For instance, his uncritical acceptance of the evidence of the Talmud for events of the first century is particularly troubling. Even worse is his failure to treat seriously the very strong objections to Marcan priority and the two-document hypothesis made in the past two decades. He dismisses the arguments of William Farmer, John Rist, and Hans-Herbert Stoldt against Marcan priority with a single glib footnote and blithely ignores Bishop Butler's arguments for Matthean priority altogether.

Meier's attempt to reconstruct the historical Jesus thus rests on no very firm foundation and produces no assured results. Those who want solid information on the historical Jesus are far better off turning to Matthew, Mark, Luke, or John, no matter how "naïve" it might be to do so.

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Primus, John H. *Holy Time: Moderate Puritanism and the Sabbath*. Macon, GA: Mercer University Press, 1989. viii + 184 pp. Hardcover, \$24.95; paperback, \$16.95.

Since the 1960s there has been a flurry of new interest in the phenomenon of English Sabbatarianism. Articles by Patrick Collinson, Herbert Richardson, Winton Solberg, Richard Greaves, and books by James T. Dennison, Kenneth L. Parker, witness to this. John H. Primus describes his contribution to this discussion as "a re-examination of the relationship between the emerging Puritan movement and the phenomenon of Sabbatarianism" in order to shed "additional light on the complex dynamics of the sixteenth-century Church of England" (vii). In his research he responds to current discussions and especially to Parker, who has "reopened the fundamental question of the origin of Sabbatarianism and its relationship to Puritanism" (2, 3).

Holy Time is not intended exclusively for specialists in Tudor Puritanism. For this reason, Primus includes very helpful contextual and explanatory paragraphs on events already known to experts (vii).

Part 1 is a brief historical sketch that highlights certain emphases which Parker tends to overlook. Chap. 1 describes the high Sabbath views in England already evident in the early Reformation. By the end of the sixteenth century "Sabbatarianism had become the linchpin in the Puritan program for more complete reform in England" (17), with one of its distinguishing characteristics being "the divine appointment of Sunday as the new day of rest" (20).

Primus makes a unique contribution in chap. 2 by discussing the unpublished papers of the important Dedham debate in the 1580s which demonstrate a lively controversy on the Sabbath. Central to the debate was a serious conflict about whether Sunday became the New Testament Sabbath by divine authority or by tradition.

The author then shifts in chap. 3 to a discussion of a "Cambridge circle" of theologians who advocated Sunday absolutism during the latter part of the sixteenth century. Primus describes them as "moderate Puritans" who appealed to the authority of the apostles or of Christ for the change of the day of worship from the seventh to the first, accepted the