

WHITHER SOCIAL-SCIENTIFIC APPROACHES TO NEW TESTAMENT INTERPRETATION? REFLECTIONS ON CONTESTED METHODOLOGIES AND THE FUTURE

David G. Horrell

The modern phase of social-scientific criticism, though it has substantial earlier precursors and precedents,¹ began in the early 1970s, with the twin stimuli of Gerd Theissen's singularly innovative and influential essays² and the formation of a Society of Biblical Literature group devoted to the study of the social world of early Christianity.³ Among the group's founding members was Wayne Meeks, who had already begun to draw on perspectives from the sociology of knowledge to develop an interpretation of the Fourth Gospel as a reflection and legitimization of a sectarian community alienated from the world (Meeks 1972). Many innovative works were published in the following years, such that already by 1980 scholars were offering overviews and bibliographical surveys of the state of research in this vibrant area of New Testament studies (e.g., Scroggs 1980; Harrington 1980).

The First Urban Christians, published in 1983, is a landmark study, representing the mature flourishing of this initial period of innovative activity. A wide-ranging and synthetic treatment of various central aspects of the social life of the Pauline congregations, it draws on a variety of sociological and anthropological

perspectives, as well as works in Roman social history and New Testament studies, to construct a picture of what life was like for those "ordinary" people who joined the Christian movement. The work of Theissen is a notable influence at a number of points, particularly in the chapter on the social level of the Pauline Christians.⁴

In terms of the book's methodological stance, Meeks declares that his approach is that of a social historian (p. 2). The use of social-scientific theory is "suggestive, rather than generative in the manner of the experimental sciences" (p. 5). Rejecting the idea that such an inquiry into the lives of the early Christians might aim "to discover or validate laws about human behavior in general," Meeks describes his approach as "analogous to Clifford Geertz's description of the social anthropologist's task as an ethnographer, a describer of culture. The description is interpretative" (p. 6).

The use of social science in the volume is, as Meeks puts it, "eclectic. I take my theory piecemeal, as needed, where it fits" (p. 6). If there is an overall theoretical orientation, it is that of a "moderate functionalism," where "the sort of questions to be asked about the early Christian movement are those about how it worked" (p. 7). Thus, Meeks draws on a variety of approaches and theories—from Mary Douglas and Victor Turner on ritual, to Seymour Martin Lipset, Gerhard Lenski, and others on status indicators and status inconsistency—where they seem potentially to illuminate the facets of early Christianity under consideration.

Reactions to *The First Urban Christians*, insofar as they relate specifically to its methodological stance,⁵ reflect in part an emerging difference between those, like Meeks and Theissen, whose approach was labelled that of a "social historian," and others, notably Bruce Malina, John Elliott, et al., who became known as the "social scientists." Thus Theissen declared himself "deeply impressed" with Meeks's book (Theissen 1985: 113), while Malina and Elliott criticized its lack of consistent theoretical foundation (Malina 1985; Elliott 1985).⁶ Elliott, for example, argued that "Meeks's entire enterprise would have gained considerably in methodological clarity and perhaps also cogency if he had identified his theories and their sources, explicated his conceptual models, and clarified his sociological as well as historical and theological premises" (Elliott 1985: 332). This distinction between the so-called social historians and the so-called social scientists—though I shall argue that it is an unhelpful and intellectually unsustainable one—became

1 See esp. Hochschild 1999 on the earlier history of socio-historical (*sozialgeschichtliche*) exegesis. Judge 1960b is often cited as an important stimulus for the development of the modern interest in the socio-historical aspects of the Pauline communities. Theissen 1993: 19 n. 23, for example, states that "this little book deserves a place of honor in the history of modern sociological exegesis."

2 The initial cluster of essays was published between 1972 and 1975 and collected in Theissen 1979 (3rd ed., 1988). For English translations of the key works, see Theissen 1982; Theissen 1993.

3 For a fuller outline of the modern emergence of social-scientific approaches to the New Testament, see Horrell 2002: 3–15.

4 Meeks 1983: 51–73 (repr. in Horrell, ed., 1999b). For discussion of this topic, see Bruce Longenecker's chapter below.

5 I leave aside reactions to the more substantive topics and proposals, which are discussed in the following chapters.

6 See also Stowers 1985 for an extensive engagement, which stresses the need for critical examination of methods and theories used, and criticizes, among other things, the functionalism of Meeks's study, the use of the concept of status inconsistency, and its tendency to find ritual everywhere in the imagery of the texts. For criticisms of functionalism, see also Horrell 1996: 33–38.

embedded in the different Society of Biblical Literature groups that emerged during the 1980s.⁷

Before addressing some of the methodological issues at stake in this distinction, I shall first set the scene by sketching what seem to me key developments in socio-historical and social-scientific approaches to New Testament interpretation since the mid-1980s.⁸

1. KEY DEVELOPMENTS SINCE THE 1980S

First, there has been a continued diversification in the range of approaches used and the spread of topics addressed. Some studies orientated to social issues and topics in the study of earliest Christianity—such as the social status of the leaders of the Pauline communities—while interacting with the work of Theissen, Meeks, and others, have resolutely avoided using theories or models from the social sciences (Clarke 1993; cf. also Winter 2001). Other studies have drawn on social theory, such as the constructionist sociology of knowledge represented by Peter Berger and Thomas Luckmann, to study such phenomena as institutionalization (MacDonald 1988).⁹ Some recent studies have begun to focus particularly on the economic dimensions of earliest Christianity (Megitt 1998b; Friesen 2004), and on the political aspects of early Christianity's existence in the Roman Empire (e.g., Horsley, ed., 1997a, 2000a, 2004b). Some have looked to psychology, and specifically social-psychological approaches to understanding group identity, to analyze the role of early Christian texts in constructing a new sense of identity (Theissen 1987; Esler 1998a, 2003). Perhaps most notable of all is the development of interest in anthropology, a trend captured in a comment by Dale Martin, writing in 1993: "most scholars engaged in social approaches to the New Testament claim to find sociology less and less helpful and anthropology and ethnography more and more interesting" (Martin 1993: 115). This turn to anthropology is especially exemplified in the work of Bruce Malina and the Context Group, to which I will return below. My point here is simply to illustrate, albeit very briefly, the wide range of topics and approaches which use or represent some kind of socio-historical or social-scientific engagement with the New Testament. It would be impossible to establish any kind of common approach, method, or focus to define the field.

My second observation is an extension of the first: many of the themes, approaches, and topics that were innovative in the period prior to *The First Urban Christians* have since become well-established in the mainstream of New

Testament studies. In 1972 it was novel to talk about John's gospel as representing a sectarian community; now that kind of label would be routinely adopted and discussed.¹⁰ (Ironically, though, as is often the case, just as such designations become widely accepted as "standard," so some start to question whether the terminology is useful or apposite at all: there has been a significant challenge in recent years to the deeply rooted assumption that the Gospels were written for specific local, "sectarian," communities;¹¹ and, more directly related to Meeks's work, one may also question how cogent it is to speak of "Pauline communities," as if there were Pauline congregations ideologically and physically distinct from other early Christian congregations: see Horrell 2008.) Similar things could be said about the development of an essentially sociological understanding of Paul's arguments about the Jewish Law: James Dunn's influential proposals have made discussions of the social function of boundary markers in defining identity central to the ongoing debate on this long-established topic (Dunn 1983). Ben Witherington's series of socio-rhetorical commentaries exemplifies this integration of social-scientific perspectives into the mainstream, popular voice of the discipline. Both Witherington and Vernon Robbins, independently, it seems, have fused social-scientific and rhetorical approaches and, in Witherington's case, used this fusion as a basis to interpret the whole range of New Testament texts.¹²

In short, to use the famous, now almost clichéd, phrase of Robin Scroggs (Scroggs 1980: 166), to a considerable extent there has indeed been a rejoining of "body and soul," such that in New Testament studies generally there is very frequently the engagement with the kind of social aspects of early Christian life that Meeks found so lacking in the discipline as he entered it (cf. Meeks 1983: ix, 1–2; Meeks 2002: xi–xxviii). This diffusion of concerns and approaches originally associated with the innovative attempts to develop a sociologically informed reading of the New Testament indicates the success of the movement: the kinds of questions Meeks and others had in view, in works such as *The First Urban Christians*, are now established facets of the range of issues central to the discipline. It also illustrates, though, the impossibility of identifying clearly or meaningfully what does and does not "count" as an example of a social-scientific approach to the New Testament.

My third observation, however, runs somewhat counter to the previous two, which have stressed the diversity and integration into the mainstream of broadly social-scientific concerns. I mentioned above the increasing focus on anthropology rather than sociology among those interested in the social dimensions of

¹⁰ Also influential on the use of the sect model was an early study by Robin Scroggs (Scroggs 1975; repr. in Horrell, ed., 1999a).

¹¹ Bauckham 1998. For challenges to the "sectarian" reading of John, see Barton 1993; Barton 1998, esp. 189–93.

¹² See Robbins 1999a, 1999b; Witherington 1995, the first in a series of socio-rhetorical commentaries that now covers almost the entire New Testament.

⁷ See further Osiek 1989: 268–74, and Martin 1993: 107–10.

⁸ I make no pretense to offer a bibliographical survey here. For broader studies and bibliographies, see Elliott 1993; Horrell, ed., 1999a; Blasi, Duhaime, and Turcotte, eds., 2002.

⁹ MacDonald's study also draws heavily on *The First Urban Christians*.

earliest Christianity. There is one book which exemplifies this turn to anthropology, and has indeed provided the basis for a specific, model-based method of social-scientific interpretation: Bruce Malina's *The New Testament World: Insights from Cultural Anthropology*. First published in 1981, this book "was written in particular for the student beginning to study the New Testament" (Malina 1981: iii), lacking the detailed notes and bibliography typical of the scholarly monograph but including, at the end of each chapter, a section with questions and texts intended to allow the reader to test the proposed model or hypothesis. Despite this student-focused style, Malina's book was ground-breaking, setting out a series of models of the pivotal values of ancient Mediterranean culture, which contrasted sharply with the cultural values of modern Americans.¹³ These models depict the key values of honor and shame, dyadic rather than individual personality, the perception of limited good, distinctive rules of kinship and marriage, and a set of purity rules to distinguish clean and unclean.¹⁴ The aim of the book was to enable its readers to appreciate the strangeness and difference of that Mediterranean cultural context, thus correcting the tendency (on the part of Americans in particular, as the primary audience for the book) to make ethnocentric and anachronistic assumptions in interpretation.

Malina's book has provided the foundations for an approach to understanding the New Testament in its cultural context pursued by the Context Group.¹⁵ Formally begun as such in 1989, but building on long-standing collaboration between Malina, Elliott, and others, including Jerome Neyrey, John Pilch, and Richard Rohrbaugh, the Group has met annually since 1990 and its members have published, individually and collaboratively, a wide range of diverse works.¹⁶ It is important to stress, however, that, despite the influence of Malina's ground-breaking work, there is a diversity of method and approach among those who participate in the Context Group.

13 Hochschild takes the book's character as a *Lehrbuch* to indicate that the social-scientific approach was already institutionalized and accepted in the mainstream of the discipline: "Textbooks always presume a certain degree of institutionalization of a research movement. In this respect this book is not only exemplary of a cultural-anthropological interpretation of the New Testament, it is at the same time an indication of the growing acceptance, in a period of less than ten years, of the use of social-scientific models in New Testament exegesis" (Hochschild 1999: 225, my trans.). I think this underestimates the innovative character of Malina's book, the style of which probably reflects Malina's institutional context rather than the factors Hochschild suggests.

14 The third edition of the book (Malina 2001) adds two new chapters, on envy and the evil-eye, and on the evolution of the Jesus groups.

15 See Esler 2004: 52–54 for a member's comments on the fundamental influence of Malina's models on the Context Group's approach.

16 For an initial orientation to this output, see Elliott 1993: 29–30. Further bibliography at: <http://www.contextgroup.org/biblog/biblogall.doc> (accessed December 15, 2008). See Esler 2004 for an (occasionally defensive) account of the Group's origins and work. Some publication dates, however, need to be corrected: Malina's *New Testament World* and Elliott's *Home for the Homeless* were published in 1981, not 1978, and Theissen's book on the *Soziologie der Jesusbewegung* in 1977, not 1974 (p. 49).

What interests and concerns me here, in the context of a discussion taking its initial point of departure from Meeks's book, is the claim made, particularly by Malina, that the kind of approach practiced by members of the Context Group is properly "social-scientific," in a way that others are not. Indeed, it may in part be the strong claim to the label "social-scientific" on the part of Malina and some others in the Group, and their sometimes strident criticism of others' work, that in part accounts for the reluctance of others to claim the label.

2. WHAT COUNTS AS SOCIAL-SCIENTIFIC WORK? BRUCE MALINA'S CRITERIA

There are various facets to this claim, as presented by Malina. First, there is the insistence on the use of models as the only proper and recognizably social-scientific method. In a recent essay, Malina defines a social-scientific approach as follows:

Social-scientific interpretation of New Testament documents involves reading some New Testament writing by first selecting a suitable model accepted in the social-scientific community, and using the model to form adequate scenarios for reading the document in question (Malina 2002: 3).¹⁷

An associated principle for such biblical interpretation is that "if a social-scientific model is called into question or rejected by reputable social scientists, we would do well not to apply it in social-scientific interpretation" (Malina 2002: 15 n. 1).¹⁸

Second, there is an insistence on a distinction between social history and social science: "As a rule, social-scientific interpretation of the Bible is confused with social history . . . 'Social description' and 'social history' are no more 'sociology' than 'policy' and 'politics' are identical" (Malina 2002: 15 n. 2).¹⁹ What counts as social-scientific interpretation should be defined by the standards of the social sciences themselves: "Use of the social sciences in biblical interpretation should

17 Cf. also Malina 1982: 231–37; Elliott 1986: 3–9; Esler 1987: 6–9; Esler 1994: 12–13; Esler 1995a: 4–8.

18 Cf. also Malina 1982: 240, though more cautiously expressed there: "Models borrowed from the social sciences have to be good social science models. For purposes of biblical scholarship, the assessment of social science models would have to follow the criticisms of social scientists as well as the degree of fit with the area under consideration."

19 A number of authors are listed as evidencing the "unfortunate" tendency to import these "imprecisions . . . into the lexicon of biblical scholarship," including "the works of Barton, Holmberg, Kee, Malherbe, Meeks, and . . . Peterson [*sic*] . . . and, most recently, see Horrell 1999" (Malina 2002: 15 n. 2). The quotation given in the main text above is also found in Malina 1982: 241, where he comments that "[i]t would be unfortunate if these imprecisions in terminology would be imported into the lexicon of biblical scholarship." Twenty years later, Malina clearly senses that his fears have been realized.

at least be acceptable to and recognizable by social scientists and by undergraduate students who take courses in the social sciences" (Malina 2002: 15 n. 2).²⁰

Third, there is a confident rejection of works that fall outside this definition of social-scientific research. For example, in an edited textbook outlining resources for social-scientific interpretation (Rohrbaugh 1996)—from the Context Group perspective—Malina provides a bibliography in his guide to "Understanding New Testament Persons." Here, certain works are asterisked as "inadequate for a valid understanding"; they are cited "as examples of outdated or dead-end approaches to the subject" (Malina 1996b: 56–57). The list includes Meeks's *The First Urban Christians* and Theissen's *Psychological Aspects of Pauline Theology*. Malina elsewhere criticizes Abraham Malherbe scathingly for his attempt to claim the label "sociological" for his work.²¹ This tendency to criticize harshly—as failing to adopt a proper social-scientific method—work which stands outside the methodological scope of the Context Group's approach is matched by a similar tendency simply to ignore works which do not cohere with the method. In the recent co-authored *Social-Science Commentary on the Letters of Paul* (Malina and Pilch 2006), for example, the bibliography does not list anything by Meeks, Theissen, Bengt Holmberg, Dale Martin, et al., though it does include twelve items by Philip Esler, fourteen by Malina, and sixteen by Pilch. This is presumably because the works of the "social historians" are deemed to have failed—absolutely, it seems—to grasp the crucial cultural values central to ancient Mediterranean culture, and so cannot even begin to provide appropriate "reading scenarios."

In what follows, I want to mount a critique of this stance, not by criticizing *per se* the constructive work or methodological approach of the Context Group, but by questioning the grounds on which the attempt to distinguish such work from other social-scientific or socio-historical work is based.

3. A CRITICAL RESPONSE

I have elsewhere outlined my reasons for finding a model-based approach open to question, in dialogue with Philip Esler, and will not repeat myself here.²² My main observations are simply that: (a) a good deal of contemporary social science, whether in anthropology or sociology, does not appear to proceed according to the

method Malina presents as the standard for social-scientific interpretation, i.e., by first outlining a model and then proceeding to consider if and how the data fit; (b) insofar as social scientists do talk of models, they are generally regarded as simplified or idealized representations of real-world phenomena which are constructed in order to summarize key features of a series of empirical cases.

I happen to agree with Malina, Elliott, and others, that there is considerable value in outlining a coherent and explicit theoretical framework at the outset of one's work; this, one might recall, formed the main basis for their criticism of Meeks's book. I do not agree, however, that such theoretical groundwork is equivalent to, or best described as, selecting and presenting a model. But even this stance, as we shall see, does not by any means represent a "standard" for social-scientific inquiry.

Malina is right to be cautious about biblical scholars using models or theories that are rejected by contemporary social scientists. In interdisciplinary work there is always the danger that one will be using out-of-date or flawed material from the discipline in which one is less at home. However, just as the outline of social-scientific methodology presumes far too great a unanimity of approach in the social sciences, similarly, to propose that no model be used if it "is called into question or rejected by reputable social scientists" implies a level of agreement among social scientists that seems far from the case: profound empirical, methodological, and theoretical debates characterize many contemporary disciplines, not least the social sciences, which are in many ways deeply contested, particularly since the advent of postmodernism. (It is perhaps worth imagining the implausibility of a corresponding statement on the part of a social scientist seeking to use methods from New Testament scholarship. To insist that no New Testament method be used if it "is called into question or rejected by reputable" New Testament scholars would rule out almost every approach adopted in contemporary scholarship, given the realities of today's diverse and highly contested discipline.)

As with the previous points, so Malina's insistence on a sharp distinction between social history and social science is based on a conviction about the nature and methods of the social sciences. Any social-scientific method of New Testament interpretation must be "acceptable to and recognizable by social scientists . . ." in terms of its methodological stance.

One reasonable way to probe what might count as an acceptable method would seem to be to examine some recent works produced by social scientists and see what method they adopt. The following examples are by no means claimed as representative or somehow "standard" in terms of method. Indeed, part of my point is to reject the very idea that there is anything like a single "social science" method.

My first example is from the field of sociology, and specifically the sociology of religion. Grace Davie, Professor of Sociology at (as it happens) my own university, the University of Exeter, is one of the most prominent contemporary

²⁰ Cf., again, Malina 1982: 241. The remainder of the note (in Malina 2002: 15–16 n. 2) reasserts some of the standard supposed distinctions between history and sociology, that the former is characterized by "implicit, arbitrary, and unsystematic" conceptualizations and an untheoretical, narrative approach, while the latter uses explicit and systematic concepts and is interested in underlying structures and cultural values. Cf. also Malina 1985: 346. I have elsewhere addressed many of these issues, which seem to me, at best, a dated caricature of the distinctions between the two disciplines. See Horrell 1996: 18–32.

²¹ Malina 1996a: 217–41, esp. 226–28.

²² See Horrell 1996: 9–18; Esler 1998b; Horrell 2000; Esler 2000.

sociologists of religion. The work which first established her profile is *Religion in Britain since 1945: Believing without Belonging* (Davie 1994). In this book, Davie presents a wide range of empirical material relating to the changing patterns of religious belief and participation in religious (and other) institutions in post-war Britain. The subtitle summarizes her main thesis: that levels of religious belief (belief in God, etc.) remain high, but that levels of participation in institutional religion have declined—at the same time as participation in *all* kinds of institutions has declined, suggesting that some wider cultural change is in view, rather than a simple rejection of (established) religion. *After* the presentation of the pertinent material, Davie offers a “theoretical postscript” in the final chapter, in which she attempts to make sense of the material in terms of a theory of religion and its changing role in modern society.

In a subsequent work, *Religion in Modern Europe: A Memory Mutates* (2000), Davie broadens the scope of her interest to religion in Europe. The opening chapter of the book presents a series of facts and figures about the profile of religion in modern Europe. In the second chapter she asks, “How, then, can we begin to interpret the mass of data presented so far?” (Davie 2000: 24). Three possible approaches are outlined: Steve Bruce’s secularization thesis, José Casanova’s work on the nature of religion in the modern world, and Danièle Hervieu-Léger’s approach to religion as collective memory. It is this latter approach that Davie finds most fruitful and develops in her work.

In terms of method, Davie nowhere appears to talk of models, and certainly does not open her work by setting out the models she is going to adopt. She does, as we have seen, draw on theories of religion and its role in modern society in order to make some sense of her data, but neither describes these theoretical frameworks as “models” nor uses them to preface her presentation of data.²³

My point is not to claim that Davie’s method is standard, nor that it is not open to critique (e.g., Bruce 2002: 104–105). But however vigorous the debate or the methodological critique, it would be nonsense to claim that Davie’s work did not “count” as recognizable social science.

My second example comes from the field of anthropology: David Sutton’s *Remembrance of Repasts: An Anthropology of Food and Memory*. Sutton’s book is primarily an ethnographic study of “the relationship between food and memory on the island of Kalymnos, Greece” (Sutton 2001: ix). Through extensive observation and recording of everyday activities and conversations around food—from

buying and growing to cooking and eating—Sutton develops a rich analysis of the ways in which food practices constitute an important form of cultural memory and a crucial site for the sometimes conflictual encounter between the local and the global, the traditional and the modern. The first main chapter of the book considers ritual, as “a key site where food and memory come together” (p. 19). Sutton touches briefly on some relevant theoretical resources, such as Paul Connerton’s treatment of social memory, and mentions recent anthropological debate about the definition of ritual (pp. 19–21). But there is no “model” of ritual set out, no set framework for the inquiry. The chapter swiftly moves, then, to a description of some of the ways food functions in everyday life on the island. Subsequent chapters deal both with classic topics in anthropology, such as gifts and exchange, and with themes such as the role of sensory memory in the construction of worlds. Throughout, Sutton’s discussion is theoretically informed—in that he weaves into his ethnographic description broader theoretical and conceptual discussions that can offer some ways to make sense of the data—but it is primarily focused on a rich description of the multifarious ways in which food plays a significant role in everyday life on the island. There is nowhere, so far as I can see, where he refers to models, nor does he make any attempt to set out from the outset the chosen models that will shape and guide his investigation. As he puts it, his plan is “to be fairly eclectic in suggesting possible paths and theoretical approaches, illustrated primarily through my Kalymnian material,” with which to address the topic of food and memory (Sutton 2001: 15). His approach, in short, is that of “theoretically informed ethnographic exploration” (Sutton 2001: 163). Here, we might suggest, we find a sort of intuitive eclecticism not dissimilar to that espoused by Meeks.

These examples, chosen simply *ad hoc*, could, I suggest, be multiplied almost indefinitely. But, as I have stressed, my intention is not to claim that these particular works represent a “standard” social-scientific methodology, nor to deny that one might also find social-scientific work which proceeds through an explicit and developed use of models. My point, rather, is simply to illustrate that established and recognized social science proceeds in a variety of ways, does not necessarily adopt a model-based approach, and is, in fact, highly diverse in its methods and theoretical commitments. There is an enormous range of work and of theoretical and practical approaches, from empirical studies of religious beliefs and attitudes in sociology of religion to postmodern literary ethnography in anthropology, from experimental observation of group behavior in social psychology to Marxist economic history, and so on.

Indeed, the point can be sharpened and, at the same time, broadened. In and beyond the so-called social sciences themselves there are hardly clear disciplinary boundaries: When does sociology become philosophy or politics? What distinguishes human geography from sociology or anthropology? Do postcolonial and cultural studies belong in the field of the Arts, in departments of literature, or are

²³ It would be possible, following an argument often made by Malina, Esler, and others, to suggest that writers such as Davie do in fact have an implicit framework for the interpretation of data, which might better be made explicit at the outset, and which we might call a “model” (cf. Malina 1982: 231–32; Esler 1994: 12; Esler 1995a: 4; Elliott 1985: 334; Elliott 1993: 40–48). The relevant point here, though, is that social scientists such as Davie—whose methods Malina et al. claim we should be following—do not explicitly proceed in this way nor describe their theoretical frameworks as models.

they facets of contemporary political science? How can history be defined over against economic history, historical geography, or historical sociology? While some set of core convictions and approaches can generally be argued to lie at the heart of each of these distinct disciplines, these are often more the product of the history of scholarly communities rather than intellectually sustainable distinctions, and in individual cases it is often hard to see why one researcher's work belongs in one particular discipline rather than potentially in various others. Moreover, as I have already hinted, the appropriate methods and character of these disciplines are themselves profoundly contested: the postmodern critique of traditional ethnography, for example, has led to fundamental questions about how anthropology should conceive itself as a discipline (see, e.g., D. C. Moore 1994; Rubel and Rosman 1994). Generalizations about the distinctions between "social science" and "history"—in terms of the former's systematic, model-based or theoretically conscious approach and the latter's untheoretical, narrative focus on individuals and unique events—clearly fail to do justice to either set of contemporary disciplines, with the diversity of theoretical and philosophical commitments in each.

To return to the domain of New Testament studies and the implications of the discussion above, it is not my intention here to imply or develop any critique of the substantive work presented by Malina and other members of the Context Group (and again I stress the diversity of work undertaken by participants in the Group).²⁴ Rather, the key point is to argue against the dismissal of other types of social-scientific work on the grounds that they fail to conform to a recognizable and model-based mode of social science research. That latter claim is simply unsustainable, and unhelpful too, since it avoids rather than engages in methodological debate.

This does not mean, of course, that methodological debate is unhelpful or inappropriate, quite the contrary. But the arguments about whether, say, something more like a Geertzian ethnographic "thick description" is preferable to an approach based on a broader, more generalized model of the central values of a certain culture are arguments about the strengths and weaknesses of different kinds of social-scientific research, not about what counts as proper social science.²⁵ Indeed,

24 Though it does seem to me that there are critical issues to be raised at the broad theoretical level. For example, in a penetrating review, Justin Meggitt (Meggitt 1998a) suggests that Malina's work (Malina 1996a) presents a rather homogenized view of the ancient and modern Mediterranean, and disturbingly depicts negative stereotypes as characteristic not only of ancient but also modern Mediterraneans/Arabs, an approach subjected to powerful and classic critique in Said's *Orientalism*. For a developed and polemical critique along these lines, see now Crossley 2008, chs. 3–4. For a critical engagement with the topics central to Malina's models of Mediterranean culture from the perspective of a literary ethnography of the Gospel of Matthew, see Lawrence 2003.

25 For an argument in favor of the former approach, see Garrett 1992. For the opposing view, see Esler 1995a: 5–8; Esler 2004: 57–58.

the debate on this topic in biblical studies mirrors similar debates in the social sciences, as well as in other disciplines, such as ancient history.²⁶

The implication of my argument, then, is that the enormous diversity of work in contemporary New Testament studies that employs, or is influenced by, some aspect of work from the social sciences all represents, to a greater or lesser extent, a kind of social-scientific or socio-historical approach. Since the kinds of questions and approaches introduced by the pioneers of social-scientific criticism have spread into the mainstream of the discipline, it is impossible to draw any boundary around what does and does not count as social-scientific work—just as it is impossible to say, for example, where history gives way to historical sociology or historical geography. Moreover, there is no sustainable distinction to be drawn between what is "social science" and what is "social history."

All that is to be welcomed, in my view, even if it makes it harder—perhaps now impossible—to offer any kind of comprehensive survey of contemporary social-scientific interpretation. It also makes it harder to talk about the kind of methods and approaches that might appropriately inform future work on the New Testament from a social-scientific perspective, since the field is potentially so diverse and unbounded; the range of possible disciplinary connections is huge. Nevertheless, as a way of drawing positive consequence from my argument for a broad, loosely defined, but vigorously debated notion of social-scientific interpretation—and despite the risk of simply promoting one's favorite perspectives—I shall conclude by suggesting a few possible areas for fruitful ongoing development.²⁷

4. SUGGESTED AREAS FOR FRUITFUL DEVELOPMENT

First I mention *literary ethnography*. While ethnography has traditionally involved fieldwork, the literal and geographical immersion of the anthropologist into a foreign culture—a kind of participant observation impossible for the student of ancient texts—a turn toward literature in some anthropological studies has opened up possibilities for ethnographic readings of New Testament texts to be informed by, and to interact with, readings of *texts* by anthropologists. These New Testament readings would be concerned to immerse themselves into the world constructed

26 See, e.g., Cartledge 1994: 5, on the distinction between ancient historians "who believe it is possible and fruitful to generalize across all modern Greece (and sometimes, more broadly still, to 'the Mediterranean world', for example)" and those who believe "that such comparison should be used chiefly to highlight fundamental cultural difference rather than homogenize heterogeneous cultures." See further Horrell 1999b: 19–21.

27 In this closing section I draw heavily on an unpublished paper presented by invitation to a session on "The New Testament and the Social Sciences After Thirty Years: Retrospect and Prospect," in the Social-Scientific Criticism of the New Testament Section, Society of Biblical Literature Annual Meeting, Philadelphia, November 20, 2005. I would like to take this opportunity publicly to thank the Chair, Dennis Duling, and other members of the Committee, for that kind invitation.

and presented by a particular text, not read in glorious isolation but seen in the light of other materials, both contemporary and distant, and to analyze the ways in which the text deals with areas classically the focus for anthropological study: power, religious practice, exchange, kinship, gender, and social stratification (cf. Lawrence 2003, 2005). Such approaches—and here I reveal my own bias and methodological preferences—are likely to be somewhat closer in approach to the Geertzian mode of thick description than the model-based generalized picture of culture, but this does not mean that a broader picture of the key traits of a wider cultural context cannot valuably inform the investigation, only that the researcher will be equally and especially attentive to the distinctive facets of the constructions of culture found in any given text.

Second I would point to what we might very loosely call *Marxian economic and social history*. I deliberately use the label Marxian rather than Marxist for these approaches, since, especially insofar as they are useful for the study of early Christianity, they will reflect (I would suggest) not a doctrinaire Marxism but rather the *influence* of Marx and Marxist studies in a focus on the economic dimensions of life, on doing history “from below,” and on class conflict, popular culture, and popular protest. Marxist approaches as such have never gained very much attention in the field of social-scientific criticism of the New Testament, though this is less true in continental Europe than in English-speaking scholarship, and also less true in the case of political approaches to the Bible influenced by liberation theology. As James Crossley has pointed out, the major movement of Marxist historians, who were so influential in history-writing around the 1960s, was largely ignored in New Testament studies, primarily, Crossley argues, because New Testament scholars, mostly Christians, were ideologically opposed to Marxism and the kinds of historical analysis it fostered (Crossley 2006: 5–34). It is interesting, for example, that Meeks briefly mentions Marxist approaches to early Christianity from a former generation, only to swiftly dismiss their potential on the grounds of their “reductionist” view of religion (Meeks 1983: 3).²⁸

Some scholars have, for some time, pursued a radical approach to the New Testament influenced by the approach of historical materialism, as well as by liberation theology and feminism. One thinks in particular of Luise Schottroff and Wolfgang Stegemann in Germany and, in the USA, Richard Horsley and the influential SBL groups fostered and facilitated by him, with their focus on emancipatory readings of the New Testament, and on issues of power, empire, and politics.²⁹ More recent studies, without in any way being explicitly or strictly Marxist as such, have also begun to show the value of attending to the specifically

economic dimensions of early Christianity, in ways informed by the radical historians such as E. P. Thompson (notably Meggitt 1998b) and in ways concerned to highlight the unacknowledged influence of contemporary (consumer) capitalism on the approaches to (or neglect of) economic poverty in studies of the Pauline churches (Friesen 2004).

Third I would point to *iconological studies*, or to *studies of New Testament iconography*, that is to say, studies of the meaning and impact of New Testament (textual) images in the light of their visual representation in their wider cultural world. Archaeology, of course, throws up lots of examples of visual depictions which “formed” the cultural and social world which the auditors of the New Testament texts inhabited. Moreover, there are various theoretical perspectives—from semiotics and structuralism to social constructivism, and so on—which can help us to conceptualize and analyze the reception and impact of images, visual and textual. A recent volume edited by Annette Weissenrieder, Frederike Wendt, and Petra von Gemünden (Weissenrieder, Wendt, and Gemünden 2005) sets out some of these theoretical resources and contains a number of suggestive and insightful studies of specific texts and images (cf. also Lopez 2008). For example, Philip Esler interprets Paul’s athletic imagery against the background of visual depictions of Greek athletes; Hanna Roose provides an indication of how John’s readers might have understood the image of Babylon the whore, given material representations of (aging) prostitutes in antiquity; Harry Maier interprets the integrative imagery of Colossians (3:11) in the light of visual representations of Rome’s conquering and incorporating achievements. These few examples indicate too how this innovative approach has the potential to draw together a number of strands in existing work: archaeology, studies of the New Testament in its Roman imperial context, classic theoretical resources such as Peter Berger and Thomas Luckmann’s work on the social construction of reality, and so on.

Finally, I want to highlight the potential of *postcolonial studies* for understanding the New Testament in its socio-historical context. Here in particular it is impossible to pin the approach (postcolonialism) down to a specific disciplinary location. Indeed, insofar as it has one, it is often in departments of literature. Yet it—or rather, the diversity of theories, approaches, and topics that fall under this broad umbrella—has considerable relevance to the study of the ways in which the early Christians negotiated their existence in the Roman Empire. Much of the energy in postcolonial biblical studies has been focused on examining the ways in which (readings of) the Bible served—and serve—to legitimate the interests of modern colonial powers and on considering how readers in colonial and postcolonial contexts can and do generate their own (counter-)readings of the text (see, e.g., Sugirtharajah 2002). Yet postcolonial theory also has considerable potential for helping us to analyze the ways in which the New Testament texts, in various ways, represent strategies of both accommodation and resistance to empire. This is especially so given that postcolonial theory, on certain construals at least, is concerned not only with the situation after the (ostensible) end of

28 Though note the attention given in Scroggs 1980: 177–79. See also Dale Martin’s comments in his essay below.

29 See, e.g., Schottroff 1985 (ET in Horrell, ed., 1999a); Schottroff and Stegemann, eds., 1984; Gottwald and Horsley, eds., 1993; Horsley 1993; Horsley, ed., 1997a. On materialist exegesis as a dimension of social-scientific interpretation, see also Hochschild 1999: 228–32.

colonial rule but equally with the period following the time of initial colonization (i.e., the “post”) may be construed in two somewhat different senses; see Gandhi 1998: 3). James Scott’s studies of the forms of resistance found among peasants, and other subordinate groups, most often hidden and covert rather than open or violent, have already provided fruitful resources to inform studies of New Testament texts (e.g., Horsley 2004a; Carter 2004). Postcolonial writers such as Homi Bhabha offer valuable ways to explore the profoundly complex and ambivalent relationship between colonizer and colonized.³⁰ Scott infamously quotes an old Ethiopian proverb: “when the great lord passes, the wise servant bows deeply and silently farts” (Scott 1990: v). Bhabha speaks of a “sly civility” (Bhabha 1994: 93–101). Put more prosaically, there is a whole range of possible strategies of resistance, from overt and forceful rebellion to silent, “internal” opposition, practiced alongside what may, to all intents and purposes, be fully conformist behavior. These kinds of theoretical resources may help us to develop more nuanced analyses of the relationship between church and empire, as it is variously depicted and promoted in the New Testament texts, rather than the somewhat blunt alternatives of opposition or conformity that have sometimes characterized analyses to date.

If, then, we allow a broad, methodologically diverse notion of social-scientific interpretation to define the field and the future—something I have argued best fits the character of the contemporary social sciences on which we draw—these are (only) a few of the many possible directions in which New Testament study might fruitfully continue to develop. It will, I think, remain the case that a more socially grounded approach characterizes New Testament studies broadly, in a way that was not so twenty-five years ago: that, in part, represents an achievement of those who have energetically promoted this aspect of New Testament study. But given this fact, and the diverse directions in which future work will go, it will, I think, be harder and harder to draw any legitimate or meaningful boundary around “social-scientific” interpretation, defining what does and does not “count” as such. Nor should this fuzzy, indefinable world be a cause for our concern or regret: unlike sociologists, anthropologists, geographers, and historians, we have a defined body of texts and a circumscribed historical period that determine the central focus of our discipline. Determining what does and does not count as social-scientific criticism is unimportant and almost certainly unachievable. What matters much more is that we continue to forge the kind of interdisciplinary connections that will inform and invigorate New Testament studies, and continue to engage in vigorous debate about the strengths and weaknesses of our various methods and about the extent to which they make cogent historical sense of the varied phenomena of earliest Christianity.

30 See Bhabha 1994; Moore 2006: 109–10; Horrell 2007: 121–22.

Chapter 2

CONTOURS OF THE URBAN ENVIRONMENT

Peter Oakes

The five chapters of Wayne Meeks’s *The First Urban Christians* that relate social issues to Pauline Christianity (namely, chs. 2–6) are underpinned by an initial chapter that sets out key aspects of the urban environment of Paul’s churches. Even though the chapter is the longest in the volume (forty-one pages plus fifteen pages of endnotes), the vastness of the field required Meeks to be selective about which topics to bring up, especially as he wanted to give his picture a good amount of detailed support from both primary and secondary sources. After some brief, but wide-ranging, introductory sections he focuses on four main areas: mobility (physical and social); possible entry routes that a city might provide for Christianity; diaspora Jewish life; and specific cities in which Pauline Christian groups were established. He also includes a (sadly) short section on women in urban life.

The chapter stands up very well after twenty-five years. A key factor in this is that the 1970s saw the emergence of views about first-century society that broadly remain part of the scholarly consensus. In particular, Meeks was able to draw on the work of Ramsay MacMullen, whose *Roman Social Relations* (MacMullen 1974) remains a standard study. Meeks also undertook work in collaboration with Robert L. Wilken (Meeks and Wilken 1978), another scholar with continuing influence. However, there has been a deluge of work on Greco-Roman society in the past quarter-century, especially as social issues have moved more and more to the center stage of academic activity. Moreover, there are parts of the chapter, especially on the study of particular sites, where Meeks is (often unavoidably) dependent on work that was already quite old by the time of his book. On Philippi, for example, his main source is now more than seventy years old (Collart 1937).

The selectivity of Meeks’s approach also raises a broad issue that we will attempt to tackle in this essay. His initial approach to characterizing the urban environment is nicely dynamic and comprehensive. He introduces the Greco-Roman city by tracing the entry and increasing influence of Romans in the cities