TOO MUCH KINGDOM, TOO LITTLE COMMUNITY

by Jonathan Z. Smith

Although John Gager's *Kingdom and Community: The Social World of Early Christianity* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1975) has not received the major reviews it deserves within academic religious circles, journals, and societies, it is not an isolated work. It is rather representative of a wide and growing interest among Biblical scholars and historians of early Christianity in sociological and anthropological approaches to their subject matter. Such topics have not been stressed since the so-called Chicago School of New Testament studies in the first decades of this century. While Gager's work lacks both the imaginative daring and theoretical brilliance of some of the emerging leaders in these studies (particularly the various articles by Gerd Theissen), his more modest contribution, designed for classroom use, is of greater value for our discussions precisely because it is so representative. Because of this I should stress that the critical stance I shall take toward Gager's book is a thoroughly friendly one. I shall argue that Gager does not go far enough, that, despite his intentions, he has remained too wedded to that remarkable nineteenth-century synthesis of historical and theological concerns that has dominated New Testament and Early Church studies for far too long. I can summarize my critique with the title of this paper, which plays on Gager's own, "Too Much Kingdom, Too Little Community."

Consequences of Concreteness

The single, most serious charge I can bring against Gager is the imprecision of his aims. What is it that the book actually seeks to accomplish? Or, put in another way, how are we to understand the subtitle of his book, *The Social World of Early Christianity*? The subtitle has two parts, and I am puzzled equally by both. What approach is

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signaled by the phrase "social world"? What phenomenon is delimited by the phrase "early Christianity"? The first is a theoretical and methodological question which requires clarification but, once clarified, is a matter on which scholars of goodwill can disagree usefully. But the second, while necessarily arbitrary (i.e., any scholar is free to stipulate his or her domain), is a necessary prerequisite for serious consideration and debate. At the most basic level a social world, a community, must exist in some place at some time; it cannot be in general, in the abstract. I find Gager unclear about his goals, theories, and methods and utterly vague about his domain. If I am correct in this assessment, then his book must be judged a noble failure even before joining issue on any particular point.

In a position paper delivered in 1973 at the organizing meeting of the study group on the social world of early Christianity jointly sponsored by the Society for Biblical Literature and the American Academy of Religion I attempted to map out several of the possible directions which the original working title of the group, "The Social Description of Early Christianity," might take us:

> The first sense in which the subject might be taken is as a *description of the social facts* given in early Christian materials, i.e., the *realia* which they contain. With few exceptions there are no detailed monographs or indices on such topics as early Christian occupations or foodstuffs drawn from both literary and archeological sources. . . . Closely related to this first understanding of the topic is a second—the achievement of a genuine *social history* of early Christianity. . . . The third sense in which the topic might be taken is the most traditional: the *social organization* of early Christianity in terms of both the *social forces* which led to the rise of Christianity and the *social institutions* of early Christianity. . . . The fourth possibility for our topic is that most consonant with contemporary social theory: early Christianity as a *social world,* as a creation of a world of meaning which provided a plausibility structure for those who chose to inhabit it.

I by no means intended this list to be exhaustive or to suggest that these options are mutually exclusive, but each does entail a different horizon of research with differing strategies, methods, hypotheses, and theories.

In his opening pages Gager appears to take a clear stand: "I do not intend to produce a social history. This is not primarily a study of social teachings, social impact, social surroundings, or social institutions" (pp. 10–11), although he in fact has used each of these terms to describe his work in the previous eleven pages (e.g., pp. xi, xiii, 5, 8). Rather he will explore early Christian "processes of world-construction and world-maintenance" (p. 11). Drawing upon Peter Berger and Kenelm Burridge, he consistently suggests a processual understanding of social world. He is concerned with "early Christianity as a social world in the making" (p. 2), "early Christianity as a new world coming into being" (pp. 2, 8), as he spells out a set of elements that should provide an agenda for his research:

> All new religions, then, are directed toward the creation of new worlds: old symbols are given new meaning and new symbols come to life; new communities define themselves in opposition to previous traditions; a new order of the sacred is brought into being and perceived by the community as the source of all power and meaning; new rituals emerge to remind the community of this sacred order by creating it anew in the act of ritual celebration; mechanisms are established for preserving this new world and for adapting it to changing circumstances; and eventually an integrated world view may emerge . . . whose task is to give meaning not just to the community itself but to all other worlds as well. [P. 11]

Unfortunately, having settled on this initial perspective and set of topics, he fails to address such matters again, except in the most incidental of ways. Only the most obvious point, a new group defining itself in opposition to previous traditions, is treated at any length. But no matter what perspective is adopted, one could scarcely miss that in the New Testament! The notion of social world does not recur, except in two discussions of legitimation (pp. 75, 83, 96). I can find no place where Gager explicitly treats the important issue that he announces as his central concern: "To be precise, this is not a book about developed theologies . . . but about the ways in which it [early Christianity], like other new religions, *created a world so that certain ideas* of God and salvation, *and not others, seem peculiarly appropriate*" (p. 10, emphasis added).

I suspect that this reticence is in part due to the fact that, except for some brief phrases about "projection" (e.g., pp. 8, 18 [n. 49]), Gager has not really wrestled with the implications of adopting a stance grounded in the presupposition of the social construction of reality; when he has been sociological at all, it is to adopt an all too easy functionalism (for quite different examples see pp. 50–57 and 140). More usually Gager simply has translated theological terminology into pseudoanthropological terms without altering either the older sources or presuppositions (e.g., for "delay of the *parousia*" now read "millenarian," for the "scandal of the Cross" now read "cognitive dissonance"; as for "charisma," "routinization," and the like, the sociological terms were already reflections of Protestant theology).

But of more central significance is Gager's failure to achieve concreteness. If it is indeed his "basic conviction that the process of generating a sacred cosmos or a symbolic universe is always rooted in *concrete communities* of believers" (p. 10, emphasis added), then these communities must be located and described. But there are no realia, no dates, no places, no names, no anything except vague generalizations and ideas attributed to any of the communities Gager describes. Indeed he calmly declares, "whatever its date and location" (p. 50), of the only text which he analyzes in any detail. This sentence alone is sufficient reason for jettisoning his book as a serious piece of social inquiry. For example, all that we learn from chapter 1 (significantly entitled "The Rise of Community" rather than "communities") was that the earliest Christians were relatively socially and economically disadvantaged Palestinian Jews (although this is sometimes retracted, e.g., pp. 25, 28) who "stood in the mainstream of Jewish apocalyptic thinking" (p. 43). That much I could have learned from any New Testament theology, and I must repeat against Gager what he has quoted from me against others: "We have been seduced into [a description of] a *Sitz im Leben* that lacks a concrete (i.e., nontheological) 'seat' and offers only the most abstract understanding of 'life'" (p. 17, n. 37).

Gager's book is organized according to the traditional dogmatic schema: from Paul (chap. 1) to Constantine (chap. 5). He is not willing to pay the cost of his analytic frame. If it is truly not possible to discern concrete social realia and communities from the New Testament text then it ought to have been abandoned as not yielding the kinds of data necessary for the social analyst and one must turn either to other, later literary texts (Gager uses only canonical or "orthodox" Christian textual materials) or nonliterary remains (which Gager never employs). But I am not as pessimistic. Is it not possible to learn something from the more than fifty names mentioned in Paul's epistles (*pace* Gager, p. 33, who focuses on the address rather than the salutation)? Can we learn nothing from the fact that a Paul would write a letter and that some of his letters were preserved? Are not the concrete issues dealt with by Paul some indication of the map of social concerns, the points of vulnerability in several early Christian *cosmoi*? If all that Gager can determine of the social world of early Christianity is that early Christians were, by and large, not Roman aristocrats, that "Christian congregations provided a unique [!] opportunity for masses of people to discover a sense of security and self-respect" (p. 140), then we might as well abandon both early Christian literature and Gager's book as capable of illuminating anything about social worlds.

Context of Models

I turn now to the second part of my remarks. Up to this point I have been taking, far too seriously, Gager's theoretical pretensions as set forth in his "Preface" and "Introduction." But the bulk of the work, chapters 1–3, is quite different. (I omit chapters 4 and 5 as woefully superficial). Although the previous questions and criticisms will haunt these chapters, here Gager employs a different strategy: "The method I will follow in succeeding chapters is to examine specific problems in terms of theoretical models from recent work in the social sciences. In each case the model has been formulated independently of Christian evidence. My procedure will be to test them against information based on early Christian documents" (p. 12).

I shall not attempt to evaluate the anthropological or sociological merit of the models used by Gager. I shall stipulate their currency. Nor shall I dwell on the fact that while some were framed "independently of Christian evidence" they are not free of Christian presuppositions. I welcome Gager's comparisons as a refreshing break with the more usual contextless citation of Greco-Roman or Jewish materials which have so disfigured previous studies, although I will have to inquire as to whether Gager's comparisons are not equally contextless. Rather I shall inquire as to whether the models have been understood properly, whether they fit the Christian materials as cited by Gager, whether Gager's juxtaposition has yielded useful results. I shall use as my test case the first chapter as this employs theses that have been well received by New Testament scholars, both dependent on and independent of Gager's book.

Chapter 1 is concerned with various aspects of early Christianity considered as a millenarian movement. Gager appeals to two theoretical models. The first is drawn from studies of contemporary cargo cults. Gager relies most heavily on Burridge. He also employs Peter Worsley and I. C. Jarvie but is seemingly unaware of the deep theoretical differences among these three scholars (see p. 37). The second is L. Festinger's theory of cognitive dissonance (with corrections by Jane Allen Hardyck and Marcia Braden and R. Brown). I shall take up only the first as it is the presupposition of the rest of the book.

Gager begins by appealing to a general description of millenarian traits developed by Jarvie to establish that early Christianity was a millenarian movement. I am prepared to agree but must note some sleight of hand here. Jarvie's four criteria (Gager adds "the central role of a messianic, prophetic, or charismatic leader" [p. 21]) were not developed independently of Christian evidence. Indeed Jarvie makes an analogous claim in terms of his culture area: The four criteria for the "general phenomenon of millenarianism will be taken from outside Melanesia"; they were derived from Norman Cohn's description of Palestinian Jewish eschatology, R. Eisler's description of early Christianity as "yet another millenarian movement," H. Zinsser's description of the Saint Vitus phenomenon and other Medieval Christian ecstatic movements, and Arnold J. Toynbee's characterization of Marxism as "pre-rabbinical, Maccabaean Jewish apocalypticism." No cognitive dissonance here; Gager has a self-fulfilling prophecy!

Gager begins what he terms an "explanation" of the millenarian character of early Christianity by appealing to the general condition of Palestinian Jews as politically disinherited. In general I might agree, although Josephus is scarcely an unimpeachable source. But this can be only a barely necessary and scarcely sufficient cause. Surely all Palestinian Jews did not feel or find themselves politically disinherited. And of those that did, surely not all (not even a majority) turned to zealotic, apocalyptic activities. What might we learn about the specific social characteristics of those Jews who became revolutionaries, apocalypticists, or early converts to Christianity? If we cannot answer this question on the basis of our extant documentation, the model is of no use. Gager neither raises nor answers this question. Instead he turns to economic deprivation.

Here his model becomes something of a muddle. First he appeals to Burridge's notion of the introduction of money into the reciprocal exchange economy of Melanesia. But this is no parallel (even granting Burridge's romantic portrait of pre-Colonial Melanesian harmony). So he turns to a symbolic understanding of money. Money equals power from which numbers were barred. Therefore they inverted the normal order: ". . . early believers came primarily from disadvantaged groups and that in return they were rewarded with the promise that poverty, not wealth, was the key to the kingdom" (p. 24). Perhaps. But what is the evidence? There are as many texts which suggest that abandoning of riches was the case as those which suggest a state of poverty. Qumran, to which Gager appeals, likewise has complex rules for the abandoning of riches to the community which, collectively, was quite wealthy if the so-called Copper Scroll (3Q15) is to be believed and be associated with Qumran. While Gager gives an impressive list of negative evidence from the New Testament to demonstrate "that early converts did not represent the established sectors of Jewish society," he offers no exegetical principles that allows him to dismiss, as he does, some evidence to the contrary in Acts (p. 25) or figures such as Joseph of Arimathea, Nathanael, and Nicodemus. (I would dismiss them too, but Gager provides no reasons.) Finally I doubt that an idealization of poverty is itself a mark of actual poverty. I can think of no more aristocratic theme in Hellenistic literature than the idealization of the naked sage (Diogenes versus Alexander and the like).

Gager attempts to back out of this by appealing to David Aberle et al. on relative deprivation and thereby loses his entire argument in admitting that the earliest converts "were by no means limited to the poor and ignorant, that the earliest believers did not necessarily come from the lowest social and economic strata. Thus we are forced to conclude that the ideology of poverty does more than simply mirror social reality. It exaggerates and idealizes this reality" (p. 28). Why? By what mechanisms? By whom? Gager has no answers because he has not taken his own theory seriously. To use his terms, what is the symbolic world created by the language of poverty, a symbolic world which constitutes, shapes reality regardless of the external economic facts?

This latter charge is the most serious one that I can level against Gager and one which brings us back to my initial observations. Gager is concerned neither with social construction, with an analysis of symbolic worlds, nor with asking hard-nosed social questions.

Gager might have used an organizing principle such as Peter Brown's attempt to describe the social world of Late Antiquity in terms of access to systems of articulate and inarticulate power and might have gone on to describe some of the strategies used by those attempting to manipulate each, some of which do have recoverable social contexts. He has focused his energies on an ahistorical conflict between the haves and the have-nots (replicated in his later discussion of the ahistorical conflict and sequence: old rules/no rules/new rules [p. 35 et passim]) rather than describing the configurations with which his have-nots found themselves to have by reconstructing their world.

While his theoretical structure is impossible (e.g., his appeal to the individual therapeutic strategies of Sigmund Freud and Claude Lévi-Strauss which he translates without question to the social), and his literary analysis of the Book of Revelation is unconvincing, he approaches such a concern most closely in his argument that the Book of Revelation represents a ritual experience which overcomes time, which, despite "the real world" which "in the form of persecution reasserted itself with dogged persistence for Christian communities," allowed an "ephemeral," a "fleeting experience of the millennium" (p. 56). But if Gager's analysis—lacking any theory of ritual, of the symbolic—is correct, then it is a fantasy.

I could go on through each of Gager's points, but the argument would be the same. Gager exhibits in a more elegant form the sort of difficulties that have plagued New Testament and early Christian historians as they have attempted to take seriously anthropological and sociological perspectives on their subject matters. To summarize: (1) a refusal to accept the consequences of concreteness; that is, at the present stage of research we cannot conduct the enterprise on the New Testament; (2) a refusal to take seriously the context of the anthropological and sociological models they employ; (3) above all, a refusal to engage in serious methodological and theoretical meditation on issues raised by the perspectives they are attempting to employ. It is not a question of new ways of seeing the old data which may be appropriated simply by the student of early Christianity. The new ways must be thought with; the data must be reconstituted. The easy synthesis represented by Gager and others is thoroughly premature. But it is to be welcomed, like the parables of the Seed, as a small beginning which, in time, might produce fruit.