

MODELS AND METHODS IN SOCIAL-SCIENTIFIC INTERPRETATION:  
A RESPONSE TO PHILIP ESLER

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In a recent issue of *The Journal of Theological Studies*, Philip Esler paid me the honour of writing a lengthy review (Esler 1998a) of my book, *The Social Ethos of the Corinthian Correspondence* (Horrell 1996; hereafter *SE*). In that review, rather than focus on the exegetical sections of the book, Esler chose ‘to concentrate on the theoretical issues’ which were raised in Part I of the study, the methodological discussion (Esler 1998a: 255). Esler’s decision to focus on these issues of theory and method makes a response of wider significance than might otherwise have been the case. A number of recent writers, reviewing the state of social-scientific interpretation of the New Testament, have highlighted the need for discussion of the methodological and theoretical issues which underlie the exegetical practice. Susan Garrett, for example, insists: ‘It is... increasingly urgent that scholars of Christian origins engage in sustained reflection on the philosophical implications of the perspectives and models they chose to employ’ (Garrett 1992: 93). Dale Martin speaks of ‘a spectrum of opinion about what precisely a social-scientific method should be’, with much of the debate, in his view, centering ‘on the use of “models” derived from the social sciences’ (Martin 1993: 107). Furthermore, like Garrett, Martin maintains that the debate reflects ‘profound theoretical differences, particularly epistemological ones’ (Martin 1993: 109-10).<sup>1</sup> Discussion and reflection are particularly important to foster and promote dialogue among those who seek to make use of the social sciences in New Testament interpretation, especially given the sometimes rather firm division between two groups who have come to be referred to as ‘the social historians’ and ‘the social scientists’.<sup>2</sup> In the following essay, I shall attempt to respond to Esler’s arguments concerning the use of models, the validity of Berger and

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<sup>1</sup> See further Martin 1993: 107-10; Osiek 1989: 269-74, 277; Horrell 1999: 19-21.

<sup>2</sup> See Osiek 1989: 268-69; Martin 1993: 107-10, both of whom refer to the different groups constituted under the auspices of SBL.

Luckmann's theory of the social construction of reality, and, finally and more briefly, the major argument of *SE*.

### *1. The use of models*

In *SE* I took issue with a widely represented view that a social-scientific<sup>3</sup> approach to the New Testament essentially involves the use of 'models' (*SE* 9-18). I argued — and will not simply repeat those arguments here — that there were a number of problems with such a conception of social-scientific method and maintained that it was certainly not the *only* way to approach the New Testament 'sociologically'. Not the least of these problems, it seems to me, is the way in which a model-based approach can lead to historically and culturally variable evidence being interpreted through the lens of a generalised model of social behaviour. The rich diversity of human behaviour is thereby homogenised and explained in terms of what is 'typical' — which can actually be no real explanation at all (cf. *SE* 287-89). Although Esler is conscious of these dangers, and insists on the need for sensitivity to cultural variation and specificity (see Esler 1998b: 4-5), his most recent work still exhibits the problem, as will be shown below.

Esler makes two main responses to my criticisms of the model-based approach to social-scientific New Testament interpretation. First, he argues that the term 'model' has come to have a particular meaning in its use by New Testament scholars: 'Many practitioners of social-scientific interpretation use the word "model" as a fairly general expression for the various ideas and perspectives they employ in their exegesis' (Esler 1998a: 254). In this

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<sup>3</sup> I take Esler's point (1998a: 253, 255) that the label 'sociological', which I then used, is somewhat inappropriate given the use of a range of 'social-scientific' approaches in New Testament study, deriving not only from sociology but also from anthropology etc. (cf. now Horrell 1999). There has been a shift in terminology generally: cf. e.g. John Elliott's early description of his approach as 'sociological exegesis' (Elliott 1981: 7-11) and his more recent use of 'social-scientific' (Elliott 1993). Nevertheless, the term social-scientific may imply a closer proximity to 'scientific' methods than some would consider appropriate for the hermeneutic, humanistic discipline of (social) history (cf. Martin 1993: 103, 109). Because of my belief that there is no legitimate distinction to be drawn between history and social science — appropriately conceived — I adopted the label 'socio-historical' for the approach taken in *SE* (see pp.26-31)

sense, a ‘model’ is, very broadly, any kind of presupposition, theory, approach, or method. As such, ‘modelling is unavoidable’ and ‘explicit modelling is preferable to implicit modelling’ (Esler 1998a: 255). Esler points to similar uses of the term ‘model’ by the ancient historian Josiah Ober (1989) and the anthropologist Stanley Barrett (1996). Thus, according to Ober, models are essentially the ‘*a priori* assumptions and analogies’ without which it is impossible to do history at all (Ober 1989: 134; cf. also Carney 1975). Barrett’s approach, however, is actually quite different: unlike Esler, he rejects the idea of establishing a model prior to undertaking research (see below).

However, while I take Esler’s point that ‘meaning is determined by the usage of practitioners, not by genealogy’ (1998a: 255), and accept that the term ‘model’ is often used rather broadly to refer to any ‘theory’, ‘ideal-type’ etc.,<sup>4</sup> to use the label ‘model’ in this very general way seems to me highly unsatisfactory, for two main reasons. First, it renders the term almost entirely vacuous: if every presupposition, assumption, analogy, perspective, or theory should be called a ‘model’ then, indeed, we all use models all the time, whether consciously or not, and the term becomes little more than a convenient label to describe the widely accepted truth that human perception is never purely objective or detached. In this case, as Terry Eagleton says of an over-generalised use of the term ‘ideology’, ‘the term cancels all the way through and drops out of sight’ (Eagleton 1991: 9). Secondly, and more importantly, this over-generalised use of the term ‘model’ is in danger of obfuscating the significant differences — which Esler has elsewhere discussed (1995a: 4-8) — between those who argue for a ‘model-based’ approach to social-scientific interpretation and those who favour a more ‘interpretive’ approach.<sup>5</sup>

Models, as abstract and simplified representations of reality,<sup>6</sup> are generally formulated as a *result* of empirical research,<sup>7</sup> and serve to simplify, abstract, or generalise the findings,

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<sup>4</sup> Cf. Johnson 1995: 180; Outhwaite 1983: 244; Bhaskar 1979: 39-47; Kuper 1992b: 1-2, 12-14.

<sup>5</sup> Cf. Auger 1995: 107, on the ‘two paradigms’ in social science research: ‘scientific/positivistic’ and ‘humanist/interpretive’. On the shift in recent anthropology from scientific to humanistic discourse, see Barrett 1996: xii, 31-32, 43-44, 139-204.

<sup>6</sup> Cf. the definitions of Gregory 1986: 301; Outhwaite 1983: 244; Johnson 1995: 180.

<sup>7</sup> As, for example, in the case of ‘models’ derived from investigations into various ‘Mediterranean’ societies (see below), and of models derived from Bryan Wilson’s studies of

such that they can be further tested elsewhere. As such, models in the strict sense — except when they serve only to condense and present the findings of a specific research project (cf. Barrett 1996: 216) — are based on a philosophy of human action that regards such behaviour as predictable and regular,<sup>8</sup> presentable in generalised and typical patterns which occur cross-culturally, and which might, albeit tentatively, enable the formulation of (social) laws, or at least generalisations, concerning human behaviour.<sup>9</sup> This connection between models

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contemporary religious sects. While Wilson's typology of sects has formed the basis for sect-models applied to the New Testament, it is important to note that Wilson himself objected to any use of typologies in which 'instead of being useful short-hand summaries of crucial elements *in the empirical cases they are meant to epitomise*, they become caricatures remote from empirical phenomena'. Such 'ready-made formulae... do not explain the world, but rather obscure its richness and diversity' (Wilson 1967: 2, my italics; cited in SE 15). Cf. also Barrett 1996: 216-20.

<sup>8</sup> For example, Bruce Malina's definition of models, which is quite close to that of Derek Gregory (1986: 301), mentions their purpose as being for 'understanding, control or prediction' (Malina 1982: 231), a phrase which echoes Auguste Comte's famous dictum on the purpose of social science: *savoir pour prévoir, prévoir pour pouvoir* (see SE p.23).

<sup>9</sup> Esler (1998a: 256) is 'adamant that there is no such thing as a social law' — and I quite agree — but the concern which underlies model-building (taken in its specific sense) is clearly related to the 'scientific' aim to be able to discover repeatable regularities, and to predict (and thus to control? — see above note) their occurrence and outcome. Thus model-building is linked with the discernment of generalisable and typical patterns of behaviour, whether or not those generalisations ever acquire the status of a social law (see e.g. Turner 1987 esp. 156-61). This aim clearly underlies Philippe Descola's approach, which Esler cites with approval (Descola 1992; Esler 1995a: 6). Descola seeks ways beyond what he sees as an unscientific subjection to relativism and ethnography on the part of anthropology. He seeks 'an explanatory theory of the variability of cultural phenomena, the goal being the elucidation of the deep structures that regulate ideologies and behaviours' (Descola 1992: 109). His programme, clearly indebted to French structuralism, sees varied cultures as 'local expressions of cultural invariants' and seeks to discern these cultural invariants, and the deep structures and principles which order them (1992: 123). He does acknowledge, however, that this 'search for cultural invariants has gone out of fashion' (1992: 123).

(understood in the specific sense) and a particular philosophy of human behaviour is precisely why some human and social scientists reject the model-building enterprise together with its philosophical and epistemological foundations.<sup>10</sup> The fact that such people are nonetheless highly concerned with theory and with the theoretical foundations of their various approaches highlights one of the problems with Esler's over-generalised application of the term 'model' to all modes of theoretical formulation and application.

Esler's adoption and defence of a model-based approach seems partly to be motivated by a desire to avoid what he clearly regards as the great perils of postmodern relativism (see Esler 1995a: 6). It is indeed the case that the collapse of the foundations of objective knowledge have fomented a crisis in at least some of the social and human sciences — not least anthropology — whose practitioners have been left wondering what status can justifiably be claimed for their descriptions and explanations of the cultural practices of the world's varied peoples (see Rubel and Rosman 1994; Moore 1994; Barrett 1996: 5, 30-32, etc.). But it is highly questionable whether a retreat to the supposed objectivism of explicit modelling can provide the solution to this epistemological crisis. The critique of foundationalism is too penetrating, too powerful, to be resisted in this Canute-like manner. Better perhaps to concede that all our attempts to write history, ethnography, social science or whatever, are perspectival, biased, creative constructions — fictions, in the etymological sense of something 'made' — but that they can nevertheless be more or less faithful readings of the evidence, open to critique and correction, better or worse in their attempt to do justice to others in their otherness, or to the past in its pastness. Thus, perhaps, we may resist not relativism *in toto*, but the nihilistic absolute relativism (an oxymoron if ever there was one) which reduces every writing to an *arbitrary* fiction.

Esler's second response to my critique of the use of models is essentially that the 'deficiencies' I find 'in model use as currently practiced are illusory' (1998a: 256). Esler insists that the way in which he and others use models in no way corresponds to the pattern outlined above. Rather, models are used as 'tools, heuristic devices, and not social laws' (Esler 1998a: 256). The test for success is pragmatic; it is not a question of whether a model is true or valid, but of whether it works, whether historically plausible results are produced.<sup>11</sup>

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<sup>10</sup> See for example Hindess 1977, esp. 142-63.

<sup>11</sup> See Esler 1998a: 255-57; also Esler 1995a: 4-7; 1994: 13; Carney 1975: 35.

However, quite apart from the apparently empiricist presuppositions which underlie this approach<sup>12</sup> and Esler's interesting admission that the results must be judged according to whether or not they are 'historically plausible',<sup>13</sup> the question is whether the 'models' Esler adopts really function only in this heuristic, question-posing way. I shall attempt to show that they do not.

Esler draws a contrast between 'model-users' and 'interpretivists' (1998a: 254; cf. 1995a: 4-8) suggesting that interpretivism — the approach of immersion in a culture, resulting in a 'thick description' (Clifford Geertz) — is far more difficult to use in the face of the partial and fragmentary data from the New Testament world, compared with models, 'with their distillation and accentuation of empirical data' (1998a: 254). Although on Esler's definition of models, everyone uses them, whether consciously or not, these comments point to the significant distinction between those who endorse this more specifically 'model-based' approach and those who do not. It is important to emphasise that there is a wide variety of contemporary approaches (with quite diverse theoretical foundations) in the social sciences — rather more diversity than the either/or of interpretivism versus model-use might imply — so it is not only the 'model-users' among New Testament critics who can claim to be developing a genuinely 'social-scientific' approach.

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<sup>12</sup> The data will show whether the model is useful or not, so we do not need to worry about how the theoretical presuppositions underlying a model may shape the way in which the data is interpreted. In this regard, it seems to me, Esler's view of model-use suffers from a naivety concerning the extent to which all data are interpreted, 'theory-laden'. Since theories and models, as well as subconscious commitments and presuppositions, shape the way in which 'data' are selected and perceived, the claim that a model is 'useful' cannot obviate the need for a careful and critical consideration of its theoretical basis.

<sup>13</sup> Esler 1998a: 256: 'The test for success is a pragmatic (and modest) one, namely, whether plausible historical results have been produced in the process.' But how is what is 'plausible' to be judged? Apparently from some knowledge or awareness of the historical context which must thus be known *apart* from the model-testing process, although this contradicts Esler's claim that 'modelling' is unavoidable and ubiquitous, whether implicit or explicit. Again we see, among other things, the problem with using the term 'model' to label both a particular type of social-scientific study and the notion that all human perception is subject to presuppositions.

The model-users' work has often involved approaching the New Testament data with a set of models of Mediterranean culture, derived from the work of various anthropologists and presented in Bruce Malina's now classic book *The New Testament World: Insights from Cultural Anthropology* (1981; <sup>2</sup>1993).<sup>14</sup> In this work, Malina presents a variety of models which epitomise the cultural values of the Mediterranean region and which, he maintains, are assumed and reflected by the New Testament writers. These models of Mediterranean culture have since provided the starting point for a whole series of interpretations of New Testament texts.<sup>15</sup> However, anthropologists writing on the Mediterranean (and elsewhere) in recent years seem to describe their method more in terms of the humanistic task of *interpreting* than the 'scientific' one of adopting and testing models (though this is something of a point of debate and division in anthropology specifically and social science generally). Roger Keesing, for example, in his well-known introduction to cultural anthropology, says of anthropological fieldwork: 'There is nothing to measure, count, or predict. The task is much more like that of trying to interpret *Hamlet* ... Most essentially, it [sc. research] involves deep immersion into the life of a people' (Keesing 1981: 5-6). Another introductory work states that anthropology's 'most important method is participant observation' (Erikson 1995: 9). Furthermore, although anthropologists are concerned with comparative study, they focus particularly on the distinctive practices and culture of each specific group of people they study. J. G. Peristiany and Julian Pitt-Rivers, for example, whose early work on 'Mediterranean Society' (Peristiany 1965) has exerted considerable influence in the field, explicitly disavow the idea they 'were proposing to establish the Mediterranean as a "culture area". This was not the case.' They were in fact 'as much interested in the differences of culture as in the similarities among the peoples surrounding the Mediterranean' (Peristiany and Pitt-Rivers 1992: 6). Michael Herzfeld has sharply questioned the value of generalisations about supposed Mediterranean values and called instead for 'ethnographic particularism' (Herzfeld 1980: 349).<sup>16</sup> Anthropologists, whether they are commencing field-

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<sup>14</sup> Other models have of course been used, including for example those of the religious sect (e.g. Scroggs 1975) and of the transformation of a reform movement into a sect (Esler 1987).

<sup>15</sup> Among a large number of studies that might be mentioned, see e.g. Esler 1994: 19-36; 1998: 12; Neyrey 1991. For further references and a brief overview, see Horrell 1999: 12-15.

<sup>16</sup> See further Herzfeld 1987; Brandes 1987: 121. David Gilmore agrees with Michael Herzfeld that 'the classic honor/shame model has been reified and that this has led to

work in a contemporary community or doing anthropology on the basis of literary works and records (a growing field in recent years),<sup>17</sup> seem to approach their chosen subject of study with an openness to listen, a desire to enter sympathetically into the cultural world of the ‘other’, rather than with a ‘model’ which they seek to validate or even to use as a heuristic starting point. The cultural anthropologist, according to David Mandelbaum, ‘is less likely than are other social scientists to begin with a model or set of abstract propositions and direct his fieldwork to the testing of the model or the propositions. He is apt to shape his concepts more from the ground up than from the abstract formula down’ (Mandelbaum 1968: 315). Even those anthropologists who do want to engage in the task of model-building seem to envisage this as a task subsequent and secondary to the task of listening to the ‘native’ culture.<sup>18</sup> Stanley Barrett, to whom Esler refers in his review of *SE*, insists that he is ‘quite opposed’ to the procedure in which a model is ‘erected prior to research’: ‘the risk is too great that the fieldworker’s perceptions and interpretations will be distorted. The model then, should *not* be a guide to research, but a product of it.’<sup>19</sup> This does not mean, however, that anthropologists and other social scientists are uninformed about or uninterested in *theory*; but, as I have suggested above, that is not the same thing as using, or starting with, a model.

The problem with starting with a model is that — despite Esler’s protestations — it can lead the researcher to view the evidence in a particular way, or to assume that a certain pattern of conduct must be present. Ignoring the vast cultural differences over both space and time in ‘the Mediterranean’, a model of Mediterranean social interaction can be used as a kind of ‘trump card’, which *per se* demonstrates that a particular interpretation must be right,

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circularity and reductionism in some literature’ (Gilmore 1987b: 6) though he also maintains that honour-and-shame ‘may be seen as a “master symbol” of Mediterranean cultures’ (1987b: 17).

<sup>17</sup> So Barrett 1996: 194 n.4. See for example Schapera 1977; Whitehead 1995; Hill 1995. I am indebted to Louise Lawrence for pointing me to these and other references in this paragraph.

<sup>18</sup> See e.g. Kuper 1992b: 1-2: the anthropologist’s first task is to find out what the ‘natives’ think. She may then engage in cross-cultural comparison and engage in model-building, but ‘ethnography is generally regarded as the privileged partner’.

<sup>19</sup> Barrett 1996:216, my emphasis; cf. 187, 216-20, 226. Barrett insists that ‘ethnographic research is maddeningly inductive’ (p.220).



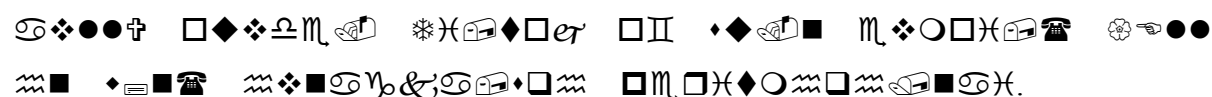
whether or not there is evidence in the text to support it. For example, one of Malina's models might be summarised in the following way: the quest for honour (and its counterpart, avoiding shame) dominates public life in the Mediterranean; public encounters (among men) are thus essentially contests for honour, which take the form of challenge-and-response. If this model were used in the heuristic way Esler claims to espouse, one might expect that the researcher would seek to appreciate whether or not a particular encounter did in fact reveal any explicit concern for honour, whether the vocabulary used in (or to describe) the encounter is actually that of honour and shame, and if not, what the significance of the variations in terminology might be, and so on.<sup>20</sup> In this way, new questions might be posed, ethnocentric and anachronistic assumptions challenged, and new insights gained — and indeed Malina's work has led to some such significant gains. Yet an example from Esler's most recent book (1998b) suggests that his use of models is more than heuristic. Examining Paul's account of his relations with Jerusalem in Gal 2.1-10 (Esler 1998b: 127-40; based on Esler 1995b), Esler sees Paul's visit to Jerusalem not as reflecting any need or desire on Paul's part to gain the approval of the leaders of the Jerusalem church, nor even a desire to secure agreement, but rather as an agonistic challenge to them and their community (for we know that the Mediterranean was a viciously 'agonistic' society, according to Malina's models).<sup>21</sup> Gal 2.3, where Paul reports that Titus was not compelled to be circumcised,<sup>22</sup> is interpreted by Esler as

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<sup>20</sup> See further Herzfeld 1987; Brandes 1987; Chance 1994: 148. I should stress that my concern is *not* to deny that the cultural values of honour and shame are instantiated in various ways among the rich diversity of human groupings which have inhabited the Mediterranean basin (though not exclusively there!). Rather, I want to raise questions about the model-based approach which tends to homogenise all of these diverse cultures (including those represented in the New Testament) into ones in which, in an apparently undifferentiated way, 'honour and shame' are pivotal values. An approach based upon the interpretative aims of 'ethnographic particularism' would be more inclined to listen for the indications as to whether, how, and in what specific ways, either or both of these values were reflected in particular ways in particular socio-historical settings and among particular social strata.

<sup>21</sup> Esler 1998b: 130-31; cf. 47-49, 65; Malina 1981: 32-33.

<sup>22</sup>



Paul's gloating over his success in getting away with taking an uncircumcised Gentile with him to Jerusalem, rather than as recording the Jerusalem leaders' acceptance of Paul's circumcision-free gospel. According to Esler: 'Here we see Mediterranean man revelling in typical fashion in relation to his success over his adversaries' (1998b: 131).<sup>23</sup> This assertion is made not on the basis of what is in Paul's text but purely on the grounds of what 'Mediterranean man' typically does (according to the generalised model of challenge-response as the means to increase honour in an agonistic society). Here the model not only supplies the understanding of Paul's methods and motives — such evidence being lacking in the text — but also 'trumps' without exegetical argument any other interpretations of this verse, since Esler's interpretation is based on what Mediterranean man would clearly do. The assumptions that underlie this approach should be self-evident: the model encapsulates the typical, generalisable pattern of Mediterranean social interaction; Paul as a typical Mediterranean man naturally obeys the dictates of this cultural logic, and can be assumed to do so even when the evidence for such behaviour is not explicit in the text. Quite apart from the questionably broad and generalised portrait of 'Mediterranean culture' which underpins Esler's approach, this seems to me to represent precisely the kind of 'derogation of the lay actor' about which Giddens complains, the assumption, in Garfinkel's phrase, that human actors are 'cultural dopes' (see *SE* 21).<sup>24</sup> While Esler insists on the need to 'be open to the fundamental cultural diversity represented in the documents of the New Testament' (1998b: 4) and maintains that critics working with models can be sensitive to human agency (1998a:

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<sup>23</sup> For further generalisations about Mediterranean culture see pp. 47-49, 127-40, 150, 187, 193, 230 *et passim*.

<sup>24</sup> This concern for the 'lay actor', it should be stressed, has nothing to do with any theologically grounded regard for Paul as unique, or somehow 'above' his culture. Rather it is a theoretical stance with regard to every human actor, whose individuality, as Anthony Cohen (1994) has so forcefully argued, should be appreciated and acknowledged. Indeed, Cohen makes the important point — which tells ironically against the insistence by Esler, Malina *et al.* that their view of the Mediterranean as a 'collectivist' culture is important to avoid ethnocentricity — that it is highly ethnocentric to credit ourselves with the capacity for reflective self consciousness but to deny that capability to members of other cultures (see esp. pp.1-22).

257) — thus finding my criticisms of model use ‘illusory’ (1998a: 256) — it seems to me that his use of models in practice bears out my criticism.<sup>25</sup>

Accordingly, I stand by the arguments against model-use which I presented in *SE*. Without harbouring any naive expectations as to their acceptance, I offer the following theses by way of concluding this section:

- (i) The term model should not be applied to every kind of presupposition, theory, method etc., with which interpreters consciously or unconsciously approach their subject matter. The term thus becomes too broad to be useful and obscures the very real differences between those who use models and those who use other kinds of theoretically-undergirded approach.
- (ii) Various kinds of theoretical perspectives, ideas, frameworks etc. can usefully be adopted from the social sciences — as Esler among others has shown — for the purpose of developing new approaches to and opening up new questions about early Christianity. However, the use of such theoretical discussion to provide a framework within which research questions may be asked is quite different from the testing of a model on the data. Accordingly, the term ‘theoretical framework’, or similar, is to be preferred, except in the cases where a model as such is used.
- (iii) It is certainly possible to adopt a model-based approach to the social-scientific interpretation of the New Testament, using a model or models derived from other social contexts or historical periods, or representing generalisations across wider fields of time and space, or even representing the result of empirical study of the early Church.<sup>26</sup> However, this approach has serious weaknesses and dangers, both at the theoretical/philosophical level and

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<sup>25</sup> I want to be clear here that my criticism is not intended as a blanket criticism of Esler’s (or anyone else’s) work. From his earliest (1987) to his latest (1998b) book, Esler demonstrates the fruitfulness of using various theoretical perspectives from the social sciences to engender new and illuminating approaches to the New Testament. I would argue, though, that his use of the label ‘model’ to describe all of these theoretical perspectives is unhelpful (cf. Esler 1998b: 9-21) *and* that it is when Esler’s use of ‘models’ is closest to model use, in its strict sense, that the criticisms raised above are most telling.

<sup>26</sup> It is also possible to construct a model as a concise and schematic way of presenting the overall *result* of a research project — which is the approach to models taken by Barrett (1996: 216-20). While even this approach can imply a spurious degree of objectivity it is clearly quite distinct from one which presents its model(s) as the initial stage of research.

in its tendency to impose the model upon the evidence; a merely pragmatic assertion that the model ‘works’ cannot obviate these deeper problems.

(iv) In view of the pitfalls associated with model-use and the problems of its philosophical/epistemological foundation, there are good reasons to side with those social scientists (including the anthropologists mentioned above) whose appreciation of cultural diversity, human agency, the ‘systematic unpredictability’ of human behaviour (MacIntyre 1981: 84-102), the need for contextual and specific rather than generalised explanations etc., leads them to adopt a theoretically-informed ‘ethnographic particularism’ (Herzfeld 1980: 349).<sup>27</sup>

## 2. *The critique of Berger and Luckmann*

In the second chapter of *SE* I examined critically two theoretical traditions — functionalism and Berger and Luckmann’s (1966) theory of the social construction of knowledge — before turning to Anthony Giddens’ structuration theory as a theoretical framework which enabled the important aspects of these other theoretical perspectives to be retained while avoiding their apparent weaknesses. A substantial section of this chapter therefore involved a presentation and critique of Berger and Luckmann’s theory (*SE* 39-45).<sup>28</sup> Since their work has been widely used in New Testament studies, and continues to be of significant influence, an appraisal of its weaknesses is of some importance.<sup>29</sup> In Esler’s view, however, my ‘critique of their work has its problems’, and thus my preference for Giddens’ structuration theory is based on ‘shaky grounds’ (Esler 1998a: 257). He proceeds to defend Berger and Luckmann against the criticisms I raise.

Before attempting to defend the cogency of my criticisms of Berger and Luckmann, I want to reiterate my view that their theory ‘offers a number of elements which are of

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<sup>27</sup> However, I am also in agreement with Keesing 1987, who argues that symbolic anthropology’s concern with culture needs to be located within a broader social theory which enables issues of power etc. to be addressed: ‘cultures must be situated historically, viewed in a theoretical framework that critically examines their embeddedness in social, economic, and political structures’ (p.166). That concern to link ethnography with critical social theory forms an important connection with the next section of this essay.

<sup>28</sup> A revised version of criticisms first published in Horrell 1993.

<sup>29</sup> Cf. *SE* 39 with the references in nn.41-42; now also Pickett 1997: 29-36.

considerable value to a sociological approach to the New Testament... there are important strengths and insights to be retained' (*SE* 40, 44). Indeed, their approach to 'the social construction of reality', which attempts to grasp the dialectical relationship between human beings who produce society which (simultaneously and continually) shapes, structures, and constrains the lives of human beings, shares at least a broad family resemblance with a number of other attempts to theorise this central problem in social theory, including Giddens' structuration theory.<sup>30</sup> Nevertheless, I contend that their valuable notion of the symbolic universe (though I prefer the term 'order' to universe; *SE* 53 n.127) and their important emphasis on the ways in which symbolic orders legitimate structures of social interaction need to be taken up into a theoretical framework which avoids the shortcomings of their theory.

Firstly, Esler objects to my criticism that Berger and Luckmann's stress upon the objectivity which the social world acquires 'obscures the extent to which social order is continually reproduced only in and through the activities of human subjects, and hence neglects the important relationship between reproduction and transformation' (*SE* 41). Esler insists that this observation does not negate the truth that 'social reality' is 'experienced by individuals as an external reality characterized... by coercive power' (Esler 1998a: 257). My own view is based, he suggests, 'on a presupposition concerning human behaviour which is itself biased in favour of maximising individuals' freedom and power' (Esler 1998a: 258). The presupposition, in fact, is based on a view of social 'reality' to which Berger and Luckmann seek to do justice: that social structure is a human construction, and *only* a human construction, even if it may sometimes appear, to certain groups, as an unquestionable 'given'. I follow Giddens in insisting that social structure, like the 'rules and resources' of a language, has only a 'virtual' existence; except for their codification in written documents, such rules and resources exist *only* insofar as they are drawn upon, used, reproduced and transformed, by acting human subjects (see Giddens 1976; 1982; 1984). They have no existence aside from, or distinct from, their instantiation in human action — and to posit such existence is the error of reification. Of course the social order may frequently be *experienced* as an objective 'given', as 'the way things are', as coercive power and unquestionable

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<sup>30</sup> Cf. Giddens 1976: 171 n.6; Bhaskar 1979: 39-47; Gregory 1981: 10-11, all of whom, however, regard Berger and Luckmann's theory as an inadequate and unsuccessful attempt to theorise the relationship between human action and social structure.

facticity, but I still question the way in which Berger and Luckmann's theory grasps and presents this. The fact that the social order is experienced by many as 'coercive power' is not necessarily to be explained by the idea that *the* social order is 'externalised' and 'objectified' (to use Berger and Luckmann's terms). Rather — as the examples Esler gives in fact show precisely!<sup>31</sup> — it is to be explained by the relative powerlessness of certain groups (even of the majority) within a society. *What Berger and Luckmann present as a feature of the construction of social reality is in fact a feature of the unequal distribution of power and resources*, and it is their failure adequately to theorise these issues which makes the need for a more *critical* social theory apparent.

In response to my second criticism of Berger and Luckmann, that their theory conceptualises the dominant social order in such a way as to present alternatives and change as threatening and liable to create anomic chaos, Esler insists that 'when Berger and Luckmann are referring to the problem caused by deviant versions of the symbolic universe, they are merely addressing themselves to how things would appear to the maintainers of the symbolic universe'. They 'devote a large proportion of the text to describing the process by which rival symbolic universes come into being and struggle to survive in the face of opposition from the initial symbolic universe' (Esler 1998a: 258). However, my point was not to suggest that Berger and Luckmann fail to give attention to the development of 'rival' symbolic orders, but rather, and linked with my first criticism, to consider the implications of the manner in which they describe and conceptualise this dimension of social life. Because of Berger and Luckmann's presentation of the social order as something external and objective (so my first criticism, discussed above), their *conception* of social conflict is one in which 'the' (objectified, externalised) symbolic order, is protected against 'rivals'. What this obscures is both the extent to which every symbolic order is continually being produced and reproduced by the human beings who inhabit it — it does not exist in a detached, protectable form — and also that this is a *contested* process. Thus, alternatives do not (except in a few exceptional circumstances) represent anarchy and chaos but, rather, different views of how

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<sup>31</sup> Esler (1998a: 258) mentions the Palestinians on the Gaza strip and the Dust Bowl farmers thrown off their land, both of which, it seems to me, illustrate exactly my point: it is not that 'the social order' or 'the way things are' is taken for granted by such people as an unquestionable reality (so Berger and Luckmann), but rather that they are relatively powerless in the face of more powerful social groups or institutions.

the social order should be shaped (i.e. reproduced and transformed). *What Berger and Luckmann present as 'social order' versus 'chaos and anomie' is more often a contest between different versions of what the social order should be.* Of course, members of a dominant group or class seeking to protect an 'established' form of the social order (one which serves their interests?) would like to *present* those who argue for an alternative as offering nothing but anarchic chaos, but whether that is the best way to theorise such challenges is quite another matter! Moreover, certain groups may claim that they are seeking to protect and sustain an established social order — thus legitimating it with the mark of antiquity, and denigrating alternatives as promoting 'modern' or new-fangled values — whereas in fact they are seeking to impose a version of it which represents something unavoidably distinctive and different.<sup>32</sup> My argument, therefore, was that the theoretical framework which Berger and Luckmann provide tends to *present* social conflict in terms of a battle between the protectors of the symbolic universe (the demise of which threatens anomie) and promoters of 'rival' universes, whereas this conflict is often more adequately understood as comprising power struggles among groups who inhabit essentially the same symbolic universe but seek to sustain and transform it in different ways. This alternative theoretical perspective again highlights the lack of a critical analysis of power, conflict and ideology in Berger and Luckmann's theory.

In my 1993 article on Berger and Luckmann and the Pastoral Epistles, I sought to show how the problems I have tried to highlight in my criticisms of Berger and Luckmann were reflected in exegetical interpretation of the New Testament. I argued that there was a clear 'convergence' between the interpretative perspective which Berger and Luckmann's theory offers and the ideology of the author of the Pastoral Epistles. Using Berger and Luckmann as her primary theoretical resource, Margaret MacDonald (1988) ends up largely reiterating the perspective of the author of the Pastorals, rather than penetrating it critically. So, she sees the author as engaged in 'protecting' the community, protecting its symbolic universe in the face of threatening rivals (MacDonald 1988: 159, 220, 228, 236 etc.). These terms are, of course, those suggested by Berger and Luckmann's theory, but they are also

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<sup>32</sup> One thinks, for example, of the way in which some contemporary proponents of 'family values' appeal to Victorian times, as if their contemporary programme for the family were essentially the same as that lived out then. On the wider issue here, see Hobsbawm and Ranger 1983.

terms (their anachronism notwithstanding) which the author of the Pastorals would surely be happy to endorse. He (a safe assumption in this case, I think) seeks indeed to present himself as a protector of ‘the faith’, opposing those who would destroy it with their godless gossip (1 Tim 1.3-11; 4.1-5; 5.11-15; 6.3-10; 2 Tim 2.14-26; 3.2-9; Titus 3.1-11). But *is* the author simply ‘protecting’ and preserving the faith, the symbolic order, or would it be more sociologically adequate (and sympathetic to those who are stigmatised in the Epistles) to suggest that what we find in this situation is a conflict, indeed a power struggle, between groups which inhabit the same symbolic order but have opposing views as to how it should be shaped and reproduced? The author of the Pastoral Epistles represents a group within the Church which seeks to transform the symbolic order in a particular direction (a direction which serves their social interests?), to stigmatise, disempower and marginalise rivals; these ‘opponents’ have a different view about the character of the symbolic order and of what kind of Christian practice would be true to Paul. To portray one group as protectors of the symbolic universe and the others as threatening its demise and with it the demise of the Church is to legitimate the perspective of the canonical author — something Berger and Luckmann’s theory seems by its form of conceptualisation to do.

These criticisms of Berger and Luckmann’s theory indicate why I chose to endorse Giddens’ point that ‘their approach... completely lacks a conception of the critique of ideology’ (Giddens 1979: 267 n.8). Their failure adequately to theorise issues of power, conflict and ideology is, I think, somewhat more serious than Esler acknowledges, though he concedes that ‘Giddens offers richer theoretical resources here’ (Esler 1998a: 258). These criticisms, as I indicated above, should not be taken as a blanket rejection of Berger and Luckmann’s theory and its value. However, they do indicate — and here I reaffirm the argument of *SE* — that Berger and Luckmann’s important insights need to be woven into the framework of a more *critical* social theory, a theory which more adequately conceptualises both the relationship between human action and social structure and the issues of power, conflict and ideology.

### 3. *The Social Ethos of the Corinthian Correspondence*

Having focused on the theoretical issues raised in the early chapters of my book, Esler makes relatively few comments about the exegetical arguments of Part II (*SE* 61-280). He does, however, endorse as reasonable my argument that ‘in 1 and 2 Corinthians the social ethos Paul creates challenges the prevailing social order, whereas the ethos in 1 Clement, to some



extent, mirrors and legitimates it', mentioning my argument that Theissen's term 'love-patriarchalism' is applicable to 1 Clement but not to Paul's Corinthian letters (Esler 1998a: 259). Nevertheless, Esler also mentions points of criticism. One is that I show 'insufficient appreciation of the texts as products of a culture radically different from our own', a criticism which — with some qualifications — I would accept as carrying some weight, though I am less convinced that Malina's models of Mediterranean cultural values are an adequate solution to this problem (cf. §1 above; Esler 1998a: 259). His other major criticism seems primarily to be that the questions I pose are 'nearly always established ones'... My 'originality lies not in novel questions... but in situating [my] results within a new framework' (Esler 1998a: 259). I am not quite sure why entering an established debate and situating results within a new framework should draw criticism, except if the primary criterion on which work is valued is that of novelty and if previous work in New Testament scholarship is regarded as asking rather unimportant questions. It seems to me that the questions raised by Theissen's work in particular, especially his proposal that the success of Christianity was partly achieved due to the change in ethos from the radical teaching of the (rural) synoptic tradition to the moderately conservative love-patriarchalism of the (urban) Pauline tradition (see Theissen 1982: 107-10, 138-40, 163-64; 1992: 56-59), are of considerable significance, and the question of how Pauline teaching *shapes* social relationships well worth asking. My claim that 'love-patriarchalism' is an inappropriate summary of the teaching of 1 and 2 Corinthians seems to have found some measure of support.<sup>33</sup> Gregory Dawes' important question as to whether I claimed too often to find 'issues of social status' lurking behind the Corinthian material (Dawes 1998: 222) and Barbara Bowe's pertinent criticism that I have exaggerated the differences between 'Paul's countercultural ideology and 1 Clement's culturally legitimating ideology' (Bowe 1998: 568) would lead, it seems to me, at most to some modification, and not rejection, of my conclusions. More than this, however, Giddens' structuration theory enables these questions to be posed within the context of a 'new framework', one which not only gives close attention to issues of ideology and interests (issues which seem to me anything but well-established on the agenda of New Testament studies, except perhaps in the last few years) but also conceptualises the ongoing production and reproduction of the Christian symbolic order in a way which enables us simultaneously to

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<sup>33</sup> Notably in Witherington 1998a: 227-228; 1998b: 76; Dunn 1998: 706 n.168.

see both change and continuity as rules and resources are continually taken up and reapplied, sometimes with significant changes in social ethos.

#### *4. Conclusion*

I am grateful to Philip Esler for subjecting my work to the kind of searching criticism which is the life-blood of any scholarly discipline. Hopefully our dialogue can contribute something to the ‘sustained reflection on the philosophical implications of the perspectives and models’ we employ which Susan Garrett feels is so ‘urgent’ (Garrett 1992: 93, quoted above). I have sought here to give further reasons why the use of models is not the best social-scientific method with which to approach the New Testament, why (in dialogue with Berger and Luckmann) the concerns of critical social theory are essential to a genuinely critical engagement with the New Testament texts, and why both the framework and the conclusions derived from Giddens’ structuration theory hold some promise for developing our understanding of the social character of early Christianity.

#### ABSTRACT

Philip Esler’s lengthy review of *The Social Ethos of the Corinthian Correspondence* focused on theoretical and methodological issues which are important to the debate about how a social-scientific approach to the New Testament should be developed and practised. This essay responds to Esler’s arguments, particularly those concerning the use of models. Firstly, drawing on the work of various social scientists, especially anthropologists, it is argued that a model-based approach is open to serious criticisms. The problems with such an approach are illustrated from Esler’s most recent book. Secondly, the weaknesses in Berger and Luckmann’s theory of ‘the social construction of reality’ are further demonstrated. These weaknesses indicate that their important insights need to be woven into the framework of a more critical social theory. Thirdly, and most briefly, the significance of the major argument of *The Social Ethos* is defended.

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