

# Rethinking the Origins of the Eucharist

## A Socio-Historical Approach

*Martin D. Stringer*

In his classic work on the origins of Christian Worship, Paul Bradshaw likens the work of the liturgist faced with the paucity of evidence in the first two centuries of the Christian era to a person looking at a blank sheet of paper with a few faint dots scattered across it.<sup>1</sup> Famously, Bradshaw suggests that there are two possible responses in this situation, which he labels as the splitters and the lumpers.<sup>2</sup> The lumpers, he suggests, try to develop a unified story that manages to join up all the dots and to create a single coherent narrative on the origins of Christian worship. The splitters, on the other hand, tend to focus on each dot as a unique individual instance, and resist any attempt to join them up or to create a coherent narrative; there are as many different narratives, describing many different origins, as there are dots on the page. Presented in this way, what I am suggesting is, possibly, something of a caricature and it does not really do justice to the sophistication and subtlety of Bradshaw's own argument. However, there is, I would suggest, some value in presenting these two perspectives as stark contrasts and then looking for other possible positions that might sit between them. What always remains, however, wherever we stand between the splitters and the lumpers, is that stark white sheet of paper and those few, faint dots; the sheer paucity of useful evidence for the practice of Christian worship in the first two centuries of the Christian era.

When I wrote my own work on the Sociological History of Christian Worship<sup>3</sup> I became particularly interested in this very early period and in the origins of the Eucharist. Of course, in a book that aimed to cover two thousand years of Christian history and the whole world's experience of Christian worship, I had very little space to develop any new ideas I might have had about the origins of the Eucharist. What got into the book, therefore, was the merest suggestion of what I was thinking on this topic<sup>4</sup> and I always intended to come back and to explore the issue in more depth at a later date. I am now in the position of having almost completed this text and the manuscript should be with the publishers by the summer.<sup>5</sup>

<sup>1</sup> P.F. BRADSHAW: *The search for the origins of Christian worship. Sources and methods for the study of early liturgy* (London 2002) 20.

<sup>2</sup> BRADSHAW: *The search* ix, drawing on the language of Robert WRIGHT: 'Quest for the Mother Tongue', in *The Atlantic Monthly* (1991) 68.

<sup>3</sup> M.D. STRINGER: *A sociological history of Christian worship* (Cambridge 2005).

<sup>4</sup> STRINGER: *A sociological history* 26-57.

<sup>5</sup> M.D. STRINGER: *Rethinking the origins of the Eucharist* (London in press).

What interested me, however, as I was working on the text, was the issue raised by Bradshaw's blank sheet of paper. This was brought into particular focus for me by the response of one of a number of anonymous readers of the original book proposal. This particular scholar was very wary indeed of what I was aiming to do. She (or it could have been he) took the view that as I had not claimed to have discovered any new evidence, a new dot to place on the blank sheet of paper, and as I was not claiming to have made any significantly new insight into the specific interpretation of any of the pieces of evidence that we already had, then I had nothing new to add to the total sum of liturgical scholarship in this field. What I was claiming to add, which was focussed, as I will go on to explain, on the questions we might want to ask about the way in which the dots can be joined together, was, in this reviewer's opinion, 'mere speculation' and therefore not worth the bother of publishing in a book that claimed to be a work of liturgical scholarship. Needless to say the publisher did not share this particular reviewer's stance, and nor do I. However, it did start me thinking about what it might mean for a scholar to add to the body of liturgical scholarship and it did raise very serious questions for me about my own methodology and exactly what I was trying to achieve.

The same reviewer also came back to a question that was raised by a number of responses to my book on the Sociological History. In short this was to ask the question 'what exactly is 'sociological' about my approach?' This question, I would suggest, is closely related to the wider question of methodology that I have just raised. I am an anthropologist by training, and my first book, *On the Perception of Worship*,<sup>6</sup> is clearly a work of anthropology as it focuses on fieldwork and the way in which congregations understand their worship. The second book, which claimed to be sociological in its title, drew on clear sociological theory to develop a wider framework within which to understand the overall history of Christian worship.<sup>7</sup> My presentation of this third book, on the origins of the Eucharist, made no claim to be either anthropological or sociological, although it did claim to draw on the work of social historians. But what, the question still remains, can a sociologist, or an anthropologist, or a social historian, add to the study of the origins of Christian worship that the liturgist has not already achieved? Is there a distinctive approach? Are there different methodological starting points or assumptions? Can we begin to ask different kinds of questions of those few faint dots on the otherwise blank sheet of paper? These are the kinds of question that I want to address in this paper.

I want to begin by looking at the kind of questions that can, and should, be asked of the evidence that we do have, and to ask, by implication, what is a

<sup>6</sup> M.D. STRINGER: *On the perception of worship. The ethnography of worship in four Christian congregations in Manchester* (Birmingham 1999).

<sup>7</sup> STRINGER: *A sociological history*.

legitimate question for the liturgical scholar to ask, and what is ‘mere speculation’? Second, I want to widen the debate to ask whether there is a distinctive approach that can be brought to these questions from sociology, anthropology or social history, and in the process of doing that I would also hope to distinguish between these three approaches. Finally I will draw on the arguments that I have already developed to offer a glimpse of the kind of argument I am developing in the new book, which you can explore further at your leisure when the book is finally published.

## Ideas and Practices

When looking at the kind of questions that can, and should, be asked, there are two factors that need to be taken into account. The first is to ask exactly what it is that we wish to know. The second is to ask about the nature of the evidence. The first of these two questions might appear obvious, but is probably the area that most scholars fail to be entirely clear about. The real issue relates to the relationship between practice and theology. As historians and as liturgists we may be interested in both of these, and of course, it is never easy to separate the two. However, unless we are clear whether we are actually looking for evidence of specific practices, or whether we are looking at the history and development of ideas, then we are probably confusing two very different kinds of activity.

The origins of the Eucharist are particularly interesting in this respect, simply because we need to ask what it is that we actually mean by the word ‘Eucharist’. The word itself was around before Christians took it up in relation to a specific ritual act. It is not always clear, especially, in the earliest texts, whether the word is being used in a general or a specific sense and it is only towards the middle of the second century that it begins to take on a normative cultic meaning within Christian writings. This is all, however, part of what is primarily a history of ideas, or words and their meanings. If we want to focus on the question of practice, then, I would argue, the word actually raises all kinds of other issues. When Ignatius uses the word ‘Eucharist’ in his letters, for example, what is the practice that he is referring to? Unfortunately this is not entirely clear.<sup>8</sup>

The other problem has been with the scholars themselves in that there has always been a tendency, especially among biblical scholars, to label any reference to a meal and every reference to the sharing of bread, or of wine, or both, as ‘Eucharistic’ without being clear whether the text itself, or those associated with the text, might have used the word or not. By doing this such scholars can, inadvertently, associate many different practices under one technical and cultic

<sup>8</sup> W.R. SCHOEDEL: *A commentary on the letters of Ignatius of Antioch* (Philadelphia 1985).

label that may not be helpful in distinguishing the histories of the different practices in and of themselves. My own tendency has been to hold back on the word 'Eucharist', except where this is used by the text, and to focus, so far as it is possible, on what we are told of specific practices, whether meals, sharing bread, blessing wine, or whatever it is.

We cannot, however, even if we take this approach, dissociate ourselves entirely from the history of ideas. The words used by different texts are important. They may, and clearly do, reflect different strands of thought, but they may also, and probably do, reflect different strands of practice. A good example here is the way in which Bradshaw has highlighted the distinction between the use of 'body' and of 'flesh' in relation to the breaking and sharing of bread.<sup>9</sup> These words are associated in the texts with a range of other concepts and they form clusters of meaning, which can be tracked through the texts. Body language can be seen in Paul and the Synoptic Gospels and is associated with ideas of the Passover and the Last Supper.<sup>10</sup> Flesh language is found primarily in John and is distanced from Passover and Last Supper associations.<sup>11</sup> What is interesting, as Bradshaw demonstrates, is the way in which these clusters of use are picked up and developed by different later authors, eventually merging in the work of Justin and other later second-century writers.<sup>12</sup> Bradshaw does not speculate on the possible difference of practice that underpin these clusters but on the basis of his suggestions then we could argue, and I think convincingly, that while we cannot pinpoint precisely what the differences in practice might have been, we do see here two potentially distinct traditions with two divergent, and eventually convergent, trajectories through the evidence.

## Texts

When it comes to looking at the evidence that we have to work with then there is no question. The evidence comes in the form of texts. There is some archaeological and epigraphical evidence that can be drawn on, and I will come back to that later, but none of this, within the first two centuries, deals with what we would call Christian communities. It is to texts, therefore, that we have to turn. As a sociologist I have some times been accused of ignoring the texts and undervaluing the need to undertake a thorough analysis of those texts. This is not true. In Birmingham we have one of the most significant centres for textual study and many of my closest friends within the academic world are textual scholars. The liturgist, especially the liturgist who is interested in the

<sup>9</sup> P.F. BRADSHAW: *Eucharistic origins* (London 2004) 89-91.

<sup>10</sup> BRADSHAW: *Eucharistic origins* 6.

<sup>11</sup> BRADSHAW: *Eucharistic origins* 89.

<sup>12</sup> BRADSHAW: *Eucharistic origins* 89-91.

origins of Christian worship, has to be fully conversant with the methods and theories of the textual critic, to know what can and cannot be said about a manuscript, and the text it contains, from the kind of evidence the textual critic collects. It is also essential that the liturgist can engage with the language of the texts and on a personal note the most difficult aspect of this particular study has been in coming to grips with Greek. Our primary evidence is textual and it is to the text that we must always turn.

The question comes, however, as to whether there is, in fact, anything new that can be generated from a close study of the very few texts that we have. Biblical scholarship, which also deals with a limited number of texts, is always inventing new ways of reading those texts or asking new questions of the texts. Some of these work, and produce interesting insights; some seem rather strange. These techniques also seem to move in waves, almost as fashions within the world of biblical scholarship. There was a time, through the middle of the last century when form criticism and redaction criticism meant that we could never trust the coherence of any text and the aim was to identify all the different voices or editorial hands that had been at work on the text. On the whole, in contemporary biblical criticism, the drift has been back towards seeing these texts holistically, recognising that they probably are the work of one individual and that the relationship of the parts cannot be one simply of distinct and unrelated units. This again is an area that the liturgical scholar has to be aware of.

As a social scientist I am, perhaps, drawn most to the work of those scholars who aim to both set each text within its social context and to see how far it is possible to read that social context out of the text itself.<sup>13</sup> It is always tempting to move from the first of these elements, the social context, to the second, the social world as expressed in the text, but this, I would suggest, is one of those processes that leads to an excess of speculation. I will come back to why this might be the case later, but for now I simply want to state that the first task of the sociological critique of any ancient text, in fact of any text, is to begin with the text itself and to ask about the internal evidence of the text. Sometimes that text can offer very little internal evidence, apocalyptic is a very difficult genre in which to engage this kind of critique. Other forms appear to offer too much. It is at this point, therefore, that we need to be very careful about what kind of social context we are looking for, and looking at within the text.

When looking at any text with a sociological eye we have to distinguish between what the text tells us of the author, what the text tells us of the community to whom the text is sent and what the text tells us of the community, or social context from which it originated. These are not always easy to distinguish in practice but failure to do so can lead to some classic errors.

<sup>13</sup> See e.g. P.F. ESLEIR (ed.): *The early Christian world* (London 2000).

The author of many ancient texts is not always easy to determine. With the authentic letters of Paul we are probably on fairly safe ground, recognising as I have suggested that most contemporary critics no longer see the texts that we have inherited as later edited versions of Paul's originals.<sup>14</sup> When we look at the Gospels, however, it is more difficult to identify the author, even if we accept the strong editorial hand of a specific individual. Texts such as the Letter to the Hebrews or, from a later date, the Didache, are almost impossible to see in relation to a specific author; Hebrews because it provides little if any internal evidence and the Didache because it is clearly a collection of material gathered together by a community with only a very limited editorial work.<sup>15</sup>

The other problem with identifying the author arises from the technique of providing an authorial voice for a text, which is not necessarily that of the author. There are possible examples of this with Revelations and certainly with all the pseudonymous texts of the New Testament. However, can we say with certainty that the Paul as projected by, for example, the Letter to the Galatians, is an accurate reflection of the Paul who wrote the letter? In attempts to determine the practice of the earliest communities this may not be a major issue, but it does raise some questions when we come to ask about the exact nature of the dispute at Antioch, which was clearly about the sharing of food, but beyond that it is difficult to say what the real issues were, and still more difficult to determine the practice that generated these issues.

If we move on to identifying the practices of the community to whom the text is addressed then we have even more difficulties. In many cases the texts that we have are not addressed to a specific audience and so this issue does not arise. In the case of Paul's letters, and perhaps those of Ignatius for example, then we might have a clearer view. Our understanding of what was happening at Corinth, for example, to initiate the writing of 1 Corinthians is entirely taken up with this issue. The first point to make is that we must rely solely on the evidence of the text itself. It is so tempting, on so many occasions, to make assumptions based on evidence from other texts or our own preconceptions. Hurd, for example, produced an excellent analysis of the possible correspondence between the Corinthians and Paul in the 1960s.<sup>16</sup> Much of that analysis is still quoted and used by contemporary scholars because it was based almost entirely on evidence that was internal to the text. Where Hurd moves beyond the text, by making assumptions about the role of the Jerusalem Conference on Paul's thought, it becomes speculation and is largely rejected by subsequent

<sup>14</sup> See e.g. D.R. HALL: *The Unity of the Corinthian Correspondence* (London 2003).

<sup>15</sup> K. NIEDERWIMMER: *The Didache. A commentary* (Minneapolis 1998); A.J.P. GARROW: *The Gospel of Matthew's dependence of the Didache* (London 2004).

<sup>16</sup> J.C. HURD: *The origin of 1 Corinthians* (London 1965).

scholarship. The work of Theissen and Meeks can be assessed in the exactly the same way.<sup>17</sup>

The other problem that faces us in looking at the practices of the community receiving the text is that the author may not know, or may object to certain practices that are contained within the letter. It is now a common place of liturgical scholarship, for example, that if a particular text is opposing some practice then it is more than likely that it is the practice that is opposed that is the one that was occurring rather than the alternative that is being proposed. This is especially the case if we find a number of different authors opposing the same kind of practices. The situation in Corinth is a classic example of this. The factionalism and the consequent disruption to the Lord's Supper is opposed by Paul, but we have no evidence that what Paul proposed as an alternative was ever introduced, and it is clear from the Letter of Clement that factionalism of a kind was still present in Corinth some fifty years later. It is highly probable that Paul's letter had no significant impact on actual practice at all.<sup>18</sup>

Finally, therefore, we have the question of the community, or social context, of the author. There was a clear fashion towards the end of the twentieth century to talk of the community of Mark, the community of Matthew or the Johanne community with the assumption that the final edited version of the Gospels reflected something of the contemporary community of the final editorial voice. It is clear that in some ways, and whether consciously or not, the social context of an author or editor is bound to be reflected in the text, assuming that any kind of social context is. Identifying this, however, has always proved to be more difficult. Whatever we make of contemporary debates about the status of Q or the existence of 'sayings sources' and such like,<sup>19</sup> it is clear that the Gospels in particular are made up of a range of different kinds of material that are brought together and adapted by the final author/editor. There will be elements of the text that point to the contemporary practice of that author or the author's social context, but it may not always be easy to distinguish what this is.

<sup>17</sup> G. THEISSEN: *The social setting of Pauline Christianity. Essays on Corinth* (Philadelphia 1982); W.A. MEEKS: *The first urban Christians. The social world of the apostle Paul* (New Haven 1983).

<sup>18</sup> Clement never identifies himself by name in the letter but is referred to in the Shepherd of Hermes as the one who writes to other churches on behalf of the churches of Rome and is mentioned by Dionysius, Bishop of Corinth, as the one who wrote a letter to the Corinthians. R.E. BROWN & J.P. MEIER: *Antioch and Rome. New Testament cradles of catholic Christianity* (London 1983) 160-161.

<sup>19</sup> M. GOODACRE: *The case against Q. Studies in Markan priority and the synoptic problem* (Harrisburg 2002).

Another problem arises when the text that is being reviewed is actually dealing with history. We all recognise that the nature of the Christian community was changing rapidly in the first fifty years of its existence, and an author such as Luke may well have experienced a rapidly changing series of assumptions and practices within the very wide range of communities that he was involved with. Which of these practices, therefore, can we say are reflected in his work? Those which are contemporary with the writing of the text? Those which he remembers from previous times and places? Those which come from the context of the sources that he is using? People change, situations change, communities, practices and ideas change. It is very difficult, therefore, to identify exactly what Luke is referring to when he talks about the sharing of meals in the Gospel or the breaking of bread in Acts. He is not really concerned with the day-to-day practices of the communities he is discussing and so we have to treat his evidence with the utmost care and hesitation.

We have to recognise, therefore, that while it is always important to begin with the text, it is never an easy or straightforward process to decide exactly what it is that the text is telling us. Far too often, however, especially in biblical scholarship, assumptions are brought to the text that are not entirely justified by the text itself. It is still commonplace, as I have already suggested, for commentaries and other biblical works to define any reference to eating or a meal as 'Eucharistic' or to assume that all the first Christian communities, from the very earliest date, held a regular weekly meal that had some kind of Eucharistic context. I would want to propose, however, that there is no significant evidence, anywhere in the New Testament texts, that supports such an assertion. We have to look at the texts in detail and we have to begin with what it is that those texts are actually telling us.

## Theory

The problem, of course, is that the text themselves, tell us nothing of any real significance. None of the texts that we have from this period, with the probable exception of the *Didache*, are written to inform the reader about the day-to-day practice of the earliest Christian communities. They are written to serve many other diverse purposes. What they do tell us, is only ever mentioned in passing, or inferred from other kinds of information. The *Acts of the Apostles*, for example, mentions the breaking of bread on three occasions, but each is very different, with a different kind of community and in a very different context and none of the references goes into any kind of detail about what might have occurred. We cannot, therefore, from the text itself draw any useful conclusions about this at all. We either have to compare one text with another, and that causes its own questions of compatibility (was the same thing happening in Corinth as was happening in Jerusalem, how fast did practices change over



time?), or we have to set the texts themselves into a wider social and cultural context, drawing on a wider body of material to make assumptions about how meals were practiced, or the nature of religious organisations in the society of the time. It is at this point that we move into the sociological or socio-historical approach to the texts.

Before looking directly at the question of social context, however, I want to say a few words about social theory. When other scholars tell me that they do not think my work is 'sociological' enough then one of the issues that they appear to be referring to is the presence or absence of social theory. There is theory in the Sociological History, and that forms an important part of the overall structure of the book,<sup>20</sup> however, it is balanced with a certain amount of historical narrative and it is was never my intention in that book for the work to be 'theory driven'. The book on the origins of the Eucharist will have even less theory and that is what I want to address very briefly at this point.

There are many different forms that specifically sociological theory can take. However, there are two features that appear to be typical of the theoretical work that tends to be applied to early Christian history. The first is that it is primarily based on the abstraction of observations made in the contemporary world, and second, it claims to be predictive, in other words it can fill in the gaps that are left by the texts. The most famous of these, which has been largely rejected by most contemporary scholars, is the church/sect theory deriving from the work of Weber, Treoltch and more recently Stark and Bainbridge.<sup>21</sup> This originated in an attempt to categorise and classify different types of religious organisation in the contemporary world and then to draw certain conclusions from the classification. Once we could identify a particular organisation as a 'sect' for example, or a 'denomination' then we would know that it had certain features, and we could predict that it would behave in certain ways. Essentially the theory provides a series of features of the different kinds of organisation and if we find a particular group with some of these features then it was generally assumed that the others would be there also. Unfortunately, the theory did not appear to be as flexible or as widely applicable as some scholars would have wished and it has been impossible to say whether the earliest Christian community was a classic 'sect', a 'cult' or perhaps 'sect like' with other features that do not fit the classic model. More worryingly such theoretical work tends to begin with a lumpen attitude to the early church and fails to take into account that the organisation of the Christian community in rural Palestine might actually have been very different from that in urban Corinth. On the whole, as I have already

<sup>20</sup> STRINGER: *A sociological history* 6-14.

<sup>21</sup> For a good summary of the issues involved see the papers in P.E. HAMMOND (ed.): *The sacred in a secular age. Toward revision in the scientific study of religion* (Berkeley 1985).

suggested, such theoretical, sociological, models are less frequently used in contemporary scholarship.

A slightly different kind of theory building is, however, still quite common. This is an attempt, drawing on the work of anthropologists, to derive some kind of cultural model of the society within which the Christian communities developed. In this case that is either contemporary Jewish society, or more widely, the Hellenistic culture of the time. The number of works that have aimed to situate the Gospels, or Paul's thinking, or the Johanne community into its Jewish and Hellenistic culture is enormous. But what is the basis of the 'Jewish' or 'Hellenistic' culture that has been constructed? Some earlier attempts followed a similar model that offered by the church/sect theory. They identified elements of the contemporary Greek or middle eastern culture and applied these to the first century. A serious discussion of the 'shame culture' came under this category.<sup>22</sup> More recent attempts have used the methods of cultural anthropology to draw out principles from the many different texts that exist for this period, and a recent interest in client based relationships seems a promising area of development based as it is on considerable evidence from the period in question.<sup>23</sup> John Elliot turns this around and talks of the importance of recognising that the 'culture' of the first century is not the same as that which we are all familiar with and it is this distancing feature of such theories that is most important.<sup>24</sup> Again we have to ask the question whether there was in fact one single 'Hellenistic' culture, or how far this varied from city to city or region to region, and I am not sure that this has really been taken account of in many recent studies, but the basic principles do appear to be correct.<sup>25</sup>

This takes us, therefore, to the question of social context and the work of social historians. If we cannot say very much about the practice of different early Christian communities from the investigation of the early Christian texts then we have to ask whether wider investigation of the surrounding social context can take us any further. There are two basic attempts to do this within the writings on the origins of the Eucharist. The first looks at the role of meals within Jewish and Hellenistic cultures, and the second looks at the nature of clubs and other kinds of social organisation, including the synagogue.<sup>26</sup> The first thing to say is that as more work is done in this field the less it appears that we really know. This is particularly true for the Jewish context. While the writings from

<sup>22</sup> B.J. MALINA: *The New Testament world. Insights from cultural anthropology* (Louisville 2001).

<sup>23</sup> J.K. CHOW: *Patronage and power. A study of social networks in Corinth* (Sheffield 1992).

<sup>24</sup> J. ELLIOTT: *Social-scientific criticism of the New Testament* (London 1993).

<sup>25</sup> J.M.G. BARCLAY: 'Diaspora Judaism', in D. COHN-SHERBOK & J.M. COURT (eds.): *Religious diversity in the Graeco-Roman world. A survey of recent scholarship* (Sheffield 2001) 47-64.

<sup>26</sup> D.E. SMITH: *From symposium to Eucharist. The banquet in the early Christian world* (Minneapolis 2003).

Qumran have been extremely valuable, much of our evidence for Jewish organisation and practice at this time comes either from Christian documents or from later proto-rabbinical works and these have the same kinds of problems and issues associated with them as the texts that we have already investigated.<sup>27</sup> There is an increasingly 'splitter' approach to first- and second-century Jewish life and as I have indicated, the more we look at the scarce evidence that we have, the more we realise just how little we actually know.<sup>28</sup>

It is not much better when we move out to look at the wider Hellenistic culture. There is always a problem in working within a discipline that is not our own, and most liturgical and biblical scholars are not actually trained in the analysis of data from the wider classical world. Again I have had to draw heavily on the expertise of my colleagues at Birmingham who work on popular culture, religion, gender, and everyday life in Hellenistic and Roman society and I am very grateful for the contribution they have made. Once again, however, some of us might actually be rather surprised at how little real evidence there is for meal practices or local organisations during this period. If we look at the work of others we find scholar after scholar quoting the same sources and asserting the same assumptions and we rarely see liturgical scholars, for example, questioning those sources or investigating in detail where the assumptions have come from.

Peter Gooch lists twenty-four literary sources on meals in the period from 200BCE to 200CE, of which about seventeen deal with the period 0-150CE.<sup>29</sup> Most of these, like the Christian texts, are not setting out to say 'this is what meals are like'. The mention of meals comes, somewhat in passing, during a wider discussion of other kinds of issues. The greater majority also deal with meals within the upper circles of the society; grand social occasions among the elite citizens. There is little here, in and of itself, that can give us much insight into the everyday meals of the ordinary workers, or the more cultic meals of the many small religious and community organisations that clearly existed within the cities, and probably in many rural communities as well. The advantage we have for the wider social context is that we can in fact draw on a much wider body of archaeological and other evidence to supplement the material we can gain from the texts.

It always amazes me just how much information a good archaeologist can derive from the most limited of evidence. However, even here it is not always easy for us to extrapolate from a specific site, or a particular set of examples, to the kind of generalisation that would be useful for our own purposes. Most of

<sup>27</sup> C. HEMPEL: 'The Essenes', in COHN-SHERBOK & COURT: *Religious diversity* 65-80.

<sup>28</sup> BARCLAY: 'Diaspora Judaism'.

<sup>29</sup> P.D. GOOCH: *Dangerous food. 1 Corinthians 8-10 in its context* (Waterloo 1993) 28.

the interesting evidence for communal dining comes from temple sites across Greece and Anatolia. Many temples were surrounded by smallish spaces where ten or twelve people could gather for a meal, reclining on benches and with communal kitchens elsewhere in the complex.<sup>30</sup> What can this inform us, however, of Christian practices away from the temple environment? Likewise we have evidence of a number of Jewish and other cultic buildings developing out of previous domestic spaces across the Roman Empire.<sup>31</sup> This is interesting and certainly raises the probability that Christian spaces could have been developed in the same way, but these offer no indication about any kind of meal practice that occurred at these sites.

More significantly for our purposes is the fact that Roman and Hellenistic culture at our time encouraged the use of inscriptions and we have an ever-increasing body of carved inscriptions from many different kinds of building and organisation. Much of the evidence for the development of ideas about client-based relationships in the ancient world is based on these inscriptions. Likewise these inscriptions provide a wide range of evidence for different kinds of social organisation, their rules and statutes, and their annual practices.<sup>32</sup> From these inscriptions we can develop some kind of understanding of the way in which small social organisations, such as the earliest Christian communities, might have been organised and the kind of the meal practices that other such organisations engaged in. This cannot tell us what the Christians did, but it can tell us whether it was possible for certain kinds of practice to be considered probable or even normal. The fact that meals play a very important part in the annual cycle of many, if not all, of these organisations (of whatever class and for whatever purpose the organisation is founded) suggests that a shared meal within a Christian context is certainly possible. The related fact that a significant number of these meals only appear to have taken place on a monthly or an annual basis raises different kinds of questions about the possibility of a weekly meal among Christian communities. It cannot rule out that possibility, but neither can such organisations be used as evidence to support the possibility of a weekly meal either.

It is at this point, therefore, that I want to turn to my own thinking about the origins of the Eucharist.

<sup>30</sup> GOOCH: *Dangerous food*.

<sup>31</sup> L.M. WHITE: *The social origins of Christian architecture* 1. *Building God's house in the Roman world. Architectural adaptations among Pagans, Jews and Christians* (Valley Forge 1990).

<sup>32</sup> P.A. HARLAND: *Associations, synagogues and congregations. Claiming a place in ancient Mediterranean society* (Minneapolis 2003).

## Eucharistic Origins

What I want to propose may not come as much of a surprise at this stage in my paper as I have already dropped a number of hints throughout the text. What I do want to stress, however, is that I am not attempting to provide a definitive answer to the questions of Eucharistic origins. Nor am I suggesting that the different elements of my analysis are original in and of themselves. What I am proposing is a particular way of putting the elements together to provide a plausible solution to some of the problems that we have been facing within this field. The argument is developed along the lines of ‘what if’. In other words I am asking ‘what if such and such a text actually demonstrates this or the other practice’. I am then investigating the text to see whether such a possibility is denied by the internal evidence, and whether there is any real support for the possibility within the text itself. If the evidence does not contradict my ‘what if’ then I have moved on from there to the next possibility and have built the argument up in this way. I am certainly not saying that the outline that I am proposing is what happened. What I am saying is that the evidence does not contradict the solution I am proposing, and I would want to go on to suggest is that my solution is probably a better fit to much of the evidence in many cases than the standard model. This allows me to go beyond the evidence, while still remaining agnostic on the actual facts. Ultimately, however, the combination of the ‘what ifs’ once strung together as a history, provides a very different view of Eucharistic origins from that which is assumed by most contemporary Christians and if it were – even partially – accurate, would have to change the whole way in which we understand the role of the Eucharist in the contemporary church.

My ‘what ifs’ begin with Paul’s first letter to the Corinthians, our only detailed account of a meal taking place within any of the New Testament texts. What if this meal, I am asking, represents an annual event within the Corinthian community? There is nothing within the text that would rule this out as a possibility. The timing of the letter (written from Ephesus just before Pentecost) suggests that the meal may be a Passover of some kind, and other Passover imagery within the letter would support this. More importantly from my point of view, the sheer chaos implied by Paul in relation to the performance of the meal, would make it very difficult to see this as a regular weekly event. What, then, if the Lord’s Supper, as presented in 1 Corinthians, is an account of a very early form of Christian Passover?

Taking a hint from the suggestion that the account of the Last Supper may have been read as part of this Passover celebration,<sup>33</sup> I then want to ask ‘what if

<sup>33</sup> E. TROCME: *The passion as liturgy. A study of the passion narratives in the four Gospels* (London 1983).

the Passion narrative, in some shape or form, was produced as a text to be read at a Christian Passover?' Again there is no real evidence to contradict this proposal and, if I am fully honest, there is no evidence to support it either. However, the common structure, with a wide range of differences in detail, between the Passion narratives that we have, the role of the narrative within Mark's Gospel (where it forms the only single continuous narrative within the text) and the use of personal names within the narrative itself (suggesting a very early date of composition when these names would still have meant something),<sup>34</sup> all suggest at least the possibility of an independent tradition for the Passion narrative which predates any of the Gospels that we have. If this were the case then it is not too difficult to see the Last Supper narrative as a literary device within this wider text, rather than anything that is reflective of a specific meal that Jesus and his disciples actually took part in. There are other factors that support such a proposition that I cannot develop more fully here.

What we have so far, therefore, is a community in Corinth that celebrates a yearly meal that has some associations with the Passover and the reading of some elements of the Passion, more specifically the Last Supper text. There is, I would suggest, no evidence whatsoever for any other meal in any of Paul's writings, or in the writings of those who used Paul's name. We have no evidence, therefore, for any meal tradition in Anatolia, Greece or Rome before the end of the first Christian century. Where we do have a meal tradition is in the *Didache*, which we assume was written in, or more probably around, Antioch towards the end of the first century (although there is considerable dispute over this dating).<sup>35</sup> There is no question that there is a regular weekly meal referred to in this text and that it has eschatological features, although it has no association with the Last Supper or the idea of the body and blood of Christ.

The body and blood imagery is taken up again in the Johanne literature, although transformed slightly into flesh and blood imagery and disassociated from the Last Supper and the Passion.<sup>36</sup> Again we are given no clear indication of how often, or in what way, this imagery was associated with a specific meal.

We then need to jump almost sixty years to Justin to see a full cultic rite, within one of the many different Christian communities in Rome at the time, in which there is a sharing of bread and wine, without any meal, and with the association of the body and blood of Christ.<sup>37</sup> This is the first account of something that I would unambiguously call 'Eucharistic'. In between we have the letter of Clem-

<sup>34</sup> E. TROCME: *The formation of the Gospel according to Mark* (London 1975).

<sup>35</sup> GARROW: *The Gospel of Matthew*.

<sup>36</sup> BRADSHAW: *Eucharistic origins* 89-91.

<sup>37</sup> R.C.D. JASPER & G.J. CUMMING (eds.): *Prayers of the Eucharist. Early and reformed texts* (Oxford 1980) 14-16.

ent to the Corinthians, which has some vague hints but nothing that can be used to provide clear evidence of any particular kind of practice, and the letters of Ignatius.<sup>38</sup> These, I would want to suggest, are probably the key and I have been surprised at how little sociological work, of the kind that Theissen, Meeks and more recent scholars have done on Paul's letters, have been done on these letters.<sup>39</sup> Ignatius uses the term 'Eucharist' in a technical sense and he clearly has something specific in mind when he uses it.<sup>40</sup> He never tells us what this is, however, and all we get is his insistence that the Christian communities of Asia Minor should each have one bishop who should represent the unity of that community within the Eucharist. There are clearly communities that do not hold a Eucharist, and by implication some communities that might hold more than one. I still have more work to do on these texts but two things stand out for me. Ignatius is clearly trying to change the current practice, and so what he is proposing is not what is actually going on, and that Ignatius himself comes from Antioch where we know that some groups at least have a regular meal tradition. What if, therefore (and this is my last and my most speculative 'what if') what if it is Ignatius who takes the idea of a cultic rite, that he calls a 'Eucharist' from Antioch to Rome, a rite that is more of a meal in Antioch, but which, by the time that Ignatius has crossed Asia Minor, has clearly become a rite of bread and wine associated with the Last Supper and the body and blood of Christ.

I cannot prove this and the book will be very clear to make a distinction between the elements of this analysis that can be shown to be found within the texts, and those elements that are formed from my own 'what if' questions. It is clear, however, that the origins of the Eucharist are complex and lost to us in anything like their details. It is also true that for the next 150 years, through to the beginning of the fourth century there were still many different practices, many different forms of Christian meal and many different interpretations. Justin is not the end of the story, in fact he may only be the beginning, picking up a rite that had actually only arrived in Rome with Ignatius some twenty to thirty years earlier. It's a possibility.

## Conclusion

This brings me back to Bradshaw's blank sheet of paper and the way in which liturgical scholarship should be developed. If we follow the advice of the reviewer of my text then all that the liturgical scholar should be doing is exploring

<sup>38</sup> SCHOEDEL: *A commentary*.

<sup>39</sup> But see e.g. C. TREVETT: *A study of Ignatius of Antioch in Syria and Asia* (Lewiston 1992) and A. BRENT: *Ignatius of Antioch. A martyr bishop and the origin of episcopacy* (London 2009).

<sup>40</sup> TREVETT: *A study of Ignatius* 152.

more and more detail about each of the dots on the page and/or hoping for new texts and new evidence that might provide another dot somewhere within the blank white space. How far, therefore, are the 'what if' questions within my own text 'mere speculation', fanciful ideas that do nothing to develop real liturgical scholarship? I would be happy to see these questions as 'speculation', but I would reject the adjective 'mere' with all that it implies. What I am aiming to do in these questions is to propose one possible trajectory through the dots that we have. I am not going back to the lumpier position of saying that only one path is possible and all the dots need to be made to fit that path. I am, however, proposing an alternative path and then I am seeing how many of those dots might fit, and which do and, just as importantly, which do not. I am not saying that I have answered all the questions.

This process, I am suggesting, is more akin to the development of a science rather than the process commonly developed in liturgical and other related scholarships. I am putting forward a hypothesis and saying that in my view it is the best fit that we have for the evidence that is available. I fully expect somebody else to come along in one, two or three year's time and to 'yes, well, up to a point'. The Stringer hypothesis fits here and here, it makes sense in relation to two or three of the dots on the page, but he really does not fit here and here, and I have a better fit for those particular dots. Then somebody will follow that scholar and do exactly the same thing. If we do not speculate, if we do not try out new hypotheses, attempt new trajectories through the dots, then the whole subject grinds to a halt waiting for that elusive new piece of evidence and meanwhile claiming to have nothing new to say. If, on the other hand, we go out on a limb, say this or that is wrong with the view we have today, ask new and exciting 'what if' questions of the data then, yes, we may have been forgotten in ten years time as the subject has moved on way beyond our current thinking, but we have at least made that forward movement in the discipline possible, and that, I would suggest, is a goal worthy of any scholar in any known discipline.

Martin Stringer is Professor of Liturgical and Congregational Studies, School of Philosophy, Theology and Religion, University of Birmingham.  
E-mail: M.D.Stringer@bham.ac.uk.