

What we can and cannot know about the early Christian Eucharist

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1. The study of early Christian liturgy challenged by social scientists and self-confessed splitters

Recently, the study of early Christian liturgy has been confronted with two different challenges that seem to be difficult to reconcile.¹ Scholars doing research on issues related to early Christian liturgy are, so to speak, caught between two fires. On the one hand, in the eyes of scholars trained in social sciences – sociology, cultural or social anthropology, ethnography, ritual studies – most of the publications dealing with the history of Christian liturgy make a somewhat stuffy impression.² Liturgical scholars are considered to be too much engaged in the description and analysis of rituals prescribed by normative sources and, in addition, they are criticized for one-sidedly focussing on the textual elements mentioned in these sources. Moreover, it is argued that liturgical scholars are too much preoccupied with the search for ‘origins’: what were the oldest forms of Christian rituals and where did they come from? Did they arrive from Judaism or rather from Hellenism? Last but not least, publications by liturgical scholars are regularly criticized – and often with good reason – for being heavily affected by all sorts of theological and ecclesiastical agendas.

Instead, scholars using the methods employed in the above-mentioned disciplines – for example, Martin Stringer – are calling for a more dynamic approach which considers liturgical celebrations as practices. They are interested in what people participating in rituals, forms of worship, are actually doing (gestures, actions, images, singing, in brief in the performance). Moreover, they strongly suggest we should move beyond the rituals and forms of worship themselves. We should pay attention to the perception and reception of forms of worship. These scholars also emphasize the importance of contexts, whether cultural or social. How do liturgical and ritual patterns relate to social structures? What power relations are reflected in the rituals, in the celebrations? When confronted with all of these tantalizing perspectives and challenging questions,

¹ I would like to thank dr. Elizabeth Boddens Hosang for correcting and improving the English text.

² See for the following in particular M. STRINGER: *A Sociological History of Christian Worship* (Cambridge 2005) esp. 1-25. Cf. also IDEM: *On the Perception of Worship. The ethnography of worship in four Christian congregations in Manchester* (Birmingham 1999) and J. BOSSY: ‘The Mass as a Social Institution 1200-1700’, in *Past and Present* 100 (1983) 29-61, esp. 29-31.

most scholars involved in the study of early Christian liturgy are obliged to admit that they are working in a highly traditional way. They may even feel themselves to be outright old-fashioned, since they are basing themselves almost exclusively on texts, even on texts that social scientists will consider highly suspect, because they most probably provide a distorted picture of the practices of ordinary people.

Unfortunately, the scholar who will have become convinced of the necessity of broadening her or his approach, does not only have to face the challenges of social sciences, ritual studies and the like. Whether he or she may pick up the gauntlet and accept the challenge posed by ritual studies, social sciences, ethnography or not, he or she will have to respond to a different sort of criticism, namely the one that comes from the self-confessed splitters and sceptics, like Bradshaw and others, who have succeeded in deconstructing most of the widely accepted theories about the origins, Jewish or otherwise, and initial development of early Christian liturgy.³ Whereas social scientists urge the liturgical scholar to develop more adequate theories which do more justice to the complexity of early Christian rituals, the deconstructionists are attempting – often successfully – to question and overthrow many a hypothesis that has been developed concerning these rituals during the last few decades. There are very few widely accepted assumptions concerning early Christian liturgy that have not recently been questioned by members of the splitters' approach.

When confronted with these difficulties and challenges, the question which unavoidably comes up is: what can and can we not know about early Christian liturgy? What can be said with any certainty about early Christian liturgy? Or do we simply have to accept that the study of early Christian liturgy has ended in a deadlock and that historical agnosticism is the only possible adequate answer to this situation. What I particularly appreciate in Martin Stringer is that on the one hand he takes the difficulties posed by the research on early Christian liturgy seriously and on the other hand tries to find a way out of the impasse. I want to emphasize that I basically agree with his overall approach. I am convinced of the relevance and the fruitfulness of the socio-historical approach Stringer has been advocating in his publications. I also fully agree with his critical and prudent attitude towards the use of the rare sources that are available. Still, his paper provoked me to make some short comments about some of the issues addressed. They are not primarily meant as objections. Quite to the contrary, some of my comments may even serve to reinforce his argumentation and the sole purpose of my remarks is to contribute to a further clarification of the methodological issues addressed by Martin Stringers paper.

³ See in particular P. BRADSHAW: *The Search for the Origins of Christian Worship. Sources and Methods for the Study of Early Liturgy* (London 2002) and moreover: M.E. JOHNSON: 'The Apostolic Tradition', in G. WAINWRIGHT & K. WESTERFIELD TUCKER (eds.): *The Oxford History of Christian Worship* (Oxford 2006) 32-76; C. LEONHARD: *The Jewish Pesach and the Origins of the Christian Easter. Open Questions in Current Research* (Berlin / New York 2006 = *Studia Judaica* XXXV).

My remarks and questions relate to three issues: a) the difficulty of applying methods that have proven fruitful in cultural or social anthropology and ethnography, and more in particular in the research of modern ritual and liturgical practices, to earlier phases in the history of Christian liturgy and, even more specifically, to the earliest phase of Christian liturgy (the period of the origins); b) the availability of the sources, more precisely the paucity of the evidence which is stressed by Stringer (it will perhaps turn out that, in this case, I am a little less sceptical than some of the most prominent splitters and perhaps than Stringer himself); and c) the importance of context for understanding the texts (in spite of the risks this may entail).

2. What we would like to know, but cannot know

First of all, I would like to emphasize that there are a lot of things we might like to know, but we simply cannot know (here I fully agree with Stringer). One of the major difficulties in this regard is that while studying liturgies, forms of worship of the past, we cannot take advantage of the refined methods developed by disciplines like social and cultural anthropology and ethnography.⁴ We cannot interview dead people. We basically have to rely on texts and even more specifically on authoritative or even prescriptive texts most of the time representing dominant discourses of church leaders, church fathers and so on. From these texts we can learn very little about the practices of so-called ordinary believers, let alone about the ways in which they perceived and received these practices. The only thing we can do is compare different texts with each other and while doing so, be struck by the great variety of forms of worship which existed in various regions and in various local communities. If we carefully study these texts, we may sometimes catch a glimpse of the various ways the various forms of worship were perceived and received by the different groups of Christians (Jewish Christians, Gentile Christians, Gnostics and so on). We may also get some ideas of the tensions and the discussions these various liturgical practices might provoke. Still, the information we can draw from the written sources will remain very limited. Of course, here the question can be raised whether we really have to rely exclusively on written sources. What about archaeological data? Actually, one cannot deny that sometimes interesting information can be obtained from archaeological data. It may even contain important information and help questioning conclusions all too readily drawn on the basis of texts. It may also help placing such conclusions into perspective. Indeed, archaeological remains may contribute to situating texts in their context. Unfortunately, and this is especially so with regard to the first three centuries, archaeological data are very scarce (from the fourth century onwards we at least have church buildings). All in all, they cannot compensate for the lack of living

⁴ See for these methods for instance M. STRINGER: *On the Perception of Worship*.

witnesses, for the lack of people whose behaviour can be observed and who can be interviewed by ethnographers. Archaeological data may be used as witnesses, but they are most of the time silent witnesses, even more silent than texts are. In this, I completely agree with Stringer.

3. What we can surmise: the necessity of ‘What-ifs’

It will have become clear by now that there is a tremendous amount we cannot know about early Christian liturgy. The question then arises what we can know. Is it possible to say, or at least to surmise, something with a reasonable degree of plausibility? Actually, I think this is the case. Perhaps, there is even more to be said about it than the critical scholars to whom Stringer refers in his paper, suggest. While questioning a number of almost generally accepted assumptions concerning early Christian liturgy, the so-called splitters have made a very important contribution to the study of early Christianity. Still, it does not mean that resigning to a complete historical agnosticism – which unavoidably will have a paralysing effect – is the sole possibility left. I would like to argue that it is not necessary to arrive at such a radical conclusion. What is more, once some traditional but problematic assumptions have been deconstructed and abandoned, some new perspectives, new opportunities open up and offer possibilities for plumbers who, for better or for worse, will try to reconstruct the origins and early development of early Christian liturgy. On this point I readily agree with Martin Stringer. More in particular, I agree and sympathize much with the emphasis he lays upon the necessity of hypotheses, of ‘what ifs’. ‘What ifs’ are indispensable in any sort of innovative research. By the way, without ‘what ifs’, there will be no work left for the splitters who need audacious hypotheses in which to get their teeth. Still, the question remains whether our hypotheses are more than merely wild and arbitrary conjectures and whether there is at least a minimum evidence available to warrant their plausibility. With regard to this point, I tend to follow a line of argumentation which, in some respects, differs from the one suggested by Stringer or perhaps is rather complementary to it. In the final two sections of this paper I want to draw attention to two issues which, in my view, deserve to be developed more – and also in a different way – than has been done in Stringer’s paper.

4. Neglected dots and unexpected trajectories

Most scholars dealing with early Christian liturgy have been deeply impressed by the arguments of critical colleagues who have emphasized the paucity of evidence. However, the strength of this argument should not be exaggerated. It should rather be placed in perspective. First, liturgical scholars dealing with early Christian liturgy are not the only scholars that have to make do with a few

faint dots scattered across a blank sheet of paper. Every scholar specialized in the history of early Christianity has to cope with the same difficulty. The problems raised by the search for the historical Jesus are just one example. If we really want to be as prudent as some splitters recommend, entire departments of New Testament might better be closed. What is however of more relevance in this connection, is the fact that there are more dots on the paper sheet than many scholars involved in the study of early Christian liturgy usually have taken into account. Many scholars tend to limit their research to sources that have been incorporated in the canon of the New Testament, ignoring for instance sources like the Didache, Gnostic writings, apocryphal gospels or Acts and limiting themselves to so-called mainstream Christianity. A telling example that I would like to mention in this regard is the study of the celebration of the Eucharist in Syriac Christianity. Traditionally, liturgical scholars based themselves almost exclusively upon the so-called anaphora of Addai and Mari, a prescriptive liturgical text.⁵ Remarkably, the (Encratite and highly ascetic) Acts of Thomas that originate from the same region, contain various quite extensive descriptions of Eucharistic celebrations that bear a clear similarity with some passages of Addai and Mari.⁶ This might also hold true for the (Valentinian) Gospel of Philip for that matter.⁷ However, until recently these texts were hardly taken into consideration by liturgical scholars (one of the rare exceptions was Hans Lietzmann's impressive and wide-ranging study *Messe und Herrenmahl*).⁸ Admittedly, this situation has changed and several publications have been devoted to this source in the last few decades.⁹ Still, it remains remarkable

⁵ The best and most recent edition is that of A. GELSTONE: *The Eucharistic Prayer of Addai and Mari* (Oxford 1992).

⁶ Cf. G. ROUWHORST: 'Bénédiction, action de grâces, supplication. Les oraisons de la table dans le Judaïsme et les célébrations eucharistiques des chrétiens syriaques', in *Questions liturgiques* 61(1980) 211-240, esp. 221-239; IDEM: 'La celebration de l'eucharistie selon les Actes de Thomas', in C. CASPERS & M. SCHNEIDERS (eds.): *Omnes circumstantes. Contributions towards a history of the role of the people in the liturgy, presented to Herman Wegman* (Kampen 1990) 51-77; IDEM: 'Die Rolle des heiligen Geistes in der Eucharistie und der Taufe im früh-syrischen Christentum', in B. GROEN & B. KRANEMANN (Hg.): *Liturgie und Trinität* (= Quaestiones disputatae 229) 161-184.

⁷ See in particular H. SCHMID: *Die Eucharistie ist Jesus. Anfänge einer Theorie des Sakraments im koptischen Philippusevangelium* (NHC II, 3) (Leiden / Boston 2007 = Vigiliae Christianae. Supplements 88).

⁸ H. LIETZMANN: *Messe und Herrenmahl* (Berlin 1926) [English translation by D. REEVE: *Mass and Lord's Supper* (Leiden 1979)].

⁹ Apart from my own publications dealing with this subject I want to mention here in particular : R. MESSNER: 'Zur Eucharistie in den Thomasakten. Zugleich ein Beitrag zur Frühgeschichte der eucharistischen Epiklese', in *Crossroads of Cultures. Studies in Liturgy and Patristics in Honor of Gabriele Winkler* (Roma 2000 = OCA 260) 493-513; G. WINKLER: 'Weitere Beobachtungen zur frühen Epiklese (den Doxologien und dem Sanctus). Ueber die Bedeutung der Apokryphen für die Erforschung der Entwicklung der Riten', in *Oriens Christianus* 80 (1996) 177-200; IDEM: 'Nochmals zu den Anfängen

that other sources coming from the same area, are still hardly being taken into account in studies dealing with the celebration of the Eucharist in early Syriac Christianity. I mean here in particular the writings of Ephrem the Syrian and Aphraates which contain numerous passages referring to or making allusion to the Eucharist.¹⁰ Joining up these and similar dots that until now have been practically overlooked by nearly all scholars opens up new perspectives on the development of the Eucharist in early Syriac Christianity. It produces a surprisingly new trajectory, a new narrative, so to say, shared by various groups of Christians, whether orthodox or heterodox from the perspective of present-day churches, who were celebrating Eucharists – often without wine – that found their point of culmination in the invocation of the Spirit and the breaking of bread, with the commemoration of the death of Christ occasionally being assigned at best a very subordinate place, and that even in the middle of the third century!¹¹

5. The importance of contexts

Finally, I want to make some remarks about the importance of studying texts in their context. In his paper, Martin Stringer very strongly stresses the fact that to identify the practices of early Christian communities, we should at least begin with solely founding ourselves on the evidence of the texts themselves, and not based on context. To test the plausibility of hypotheses, ‘what ifs’, two things should be done: it should be asked whether the possibility of a certain ‘what if’ is denied by the internal evidence and whether there is any real support within the text itself. We should, however, refrain from appealing to the context. This method, so he warns us, entails the tremendous risk of bringing in our own assumptions and preconceptions about what early Christian forms of worship might be or might look like. This is definitely true. Yet, the strong focus laid by Stringer on the study of the text itself, gives rise to at least three questions.

First of all, if we want to arrive at more or less convincing conclusions, we often simply cannot make do not to study contexts. A clear example is provided by the hypothesis Stringer launches concerning the annual celebration of the Lord’s Supper within the Christian community. For one, Stringer is not completely consistent in exclusively basing himself upon the source in question, Paul’s First Letter to the Corinthians, since he takes into consideration the

der Epiklese und des Sanctus im eucharistischen Hochgebet’, in *Theologische Quartalschrift* 174 (1994) 214-231; A. MCGOWAN: *Ascetic Eucharists. Food and Drink in Early Christian Ritual Meals* (Oxford 1999) 191-194; P. BRADSHAW: *Eucharistic Origins* (Oxford 2004) 124-128.

¹⁰ Cf. G. ROUWHORST: ‘Bénédiction, action de grâces, supplication. Les oraisons de la table dans le Judaïsme et les célébrations eucharistiques des chrétiens syriaques’, in *Questions liturgiques* 61 (1980) 211-240, esp. 221-239.

¹¹ See in particular my article ROUWHORST: ‘Die Rolle des heiligen Geistes’.

Gospel of Mark as well. Apart from this, one might ask whether on the basis of the criteria adopted by Stringer – the text should not contradict the hypothesis and there should be at least some positive indications within the text –, one might just as well argue that the community of Corinth met once a week or every two weeks or even two or three times a week. How to decide which hypothesis is the most plausible? In my view, there is only one solution. We should, indeed, start by first carefully reading the text itself. Afterwards we will need to go beyond the text and make a careful use of so-called external evidence. Only then will we be able to decide which one of the ‘what-ifs’ that comes to mind, actually is the most likely one.

There is a second reason why we should take into account not only the texts, but also their contexts. Actually, early Christian texts contain a lot of information which is presupposed, but not made explicit, because it is considered to be self-evident. Self-evident to whom? To the readers of the first, second or third centuries, but not to those of the twentieth or twenty-first century! Leaving implicit what was self-evident for people living in the early Church, not making it explicit, means stimulating anachronistic views of early Christian liturgy and, what is even more dangerous, providing an easy alibi for theological and liturgical ideas based on such anachronistic and unhistorical views. Of course, reconstructing the contexts of early Christian texts entails the risk of reading back our own preconceptions in those contexts – and texts –, but refraining from doing so entails even greater risks.

The final reason why I would like to emphasize the importance of the context is that, if we refrain from studying it, we unavoidably miss something of the dynamics of early Christian forms of worship. Those forms did not originate out of nothing. They did not appear out of thin air. They were the result of transformations of existing traditions, especially of pre-Christian traditions, either or Jewish or Hellenistic, or whatsoever. These traditions have been taken over by early Christian communities, but they have also been transformed, appropriated. Various Christian communities have made these traditions their own. They adapted and changed them. Understanding these adaptations and changes may help us reach a more profound insight into the inner dynamics of those rituals, those forms of worship, and also of the communities which practised them. It may be added that, while struggling with rituals and forms of worship and while adapting and transforming them, the various Christian communities – as well as the Jewish ones for that matter –, were involved in establishing and marking their own identity. Understanding those processes, that are definitely interesting from a sociological point of view, is only possible if we have some idea about the wider religious, cultural and also social context(s) of early Christian liturgy and early Christianity.

Of course, reconstructing contexts and situating texts in those contexts, is a very tricky affair and the risk of speculation lies around the corner. However, it is an illusion to think that research on early Christian liturgy is free of risk.

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