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As its title implies, Peter Bell's monograph applies structures derived from sociology, specifically those focusing on conflict theory and resolution, to the Eastern Roman Empire in the sixth century. For a field as perennially under-theorized like the study of the 'Byzantine' history remains, such efforts are laudable and deserve attention and consideration by anyone working on subjects connected to the eastern Mediterranean in the 6th century. In its current form, the book emerged from the author's PhD thesis and is aimed at a relatively specialist audience already familiar with the basic issues and chronology of late antiquity. Yet Bell's approach stands apart as something unique, for he sows his interpretation in the ground of social theory yet nurtures it with autobiographical fertilizer. From this emerges a narrative which, although at times uneven and certainly not meant for every audience, offers a refreshingly novel interpretative framework with which to approach late antique issues.

The book was published by Oxford University Press. It is well-illustrated with black-and-white maps and images. The footnotes are copious - another product of the work's origins as an academic thesis - but are engaging and effusive. Bell utilizes them to quietly inject insight and humour into the work, and the attentive reader will find plenty of diversions there. The work also injects some sorely-needed theory into a field (Late Antiquity and Byzantine studies) which has not had an overabundance of engagement with theoretical approaches to the problems it presents. Bell's monograph should cause scholars of the field generally - and particularly anyone working on issues concerning the reign of Justinian I - take note.

The book's eight chapters, each of which has its tone established by a few quotes at its beginning, are divided into four parts. The first two chapters comprise part one, and are the most jarring to read if one picks up the book expecting a fundamentally historical narrative. Chapter one, 'Understanding social conflict', introduces Bell's vision for using social theory as a necessary complement to empirical history, which, he argues, can be self-defeating in its goal of describing the world 'as it actually was'. Instead of pure empiricism, Bell opts for a broad approach to historical thought and the human condition. He draws also on historiographical theory, autobiography, and modern cultural references for the analogies with which he seeks to understand and explain the past. Of all the chapters, the second, 'The analytical framework', is most indicative of the work's roots as a PhD thesis - it presents a detailed, theoretically-informed framework for the rest of the work with extensive discussion of the intellectual background. At the heart of the chapter lie two implicit questions. First, does social conflict arise from - and therefore defined by - class or status? Second, is conflict or consensus the natural internal state of a society? After considering multiple theories (foremost the 'classics': Marx, Durkheim and Weber), in answer to the first question Bell offers a syncretic model that, while recognizing the importance of economic sources of tension, prioritises status as the primary means by which groups were mobilized. To the latter question,

he downplays the dichotomy of the two concepts, which suggests that they are part of the same tension which underlies all social interaction. Part two explores conflicts among groups within society. Chapter three, 'Social conflicts in the countryside and town', casts a wide net. Bell presents the challenges of trying to give general statements about the economy when the period is characterized by localized variations. As a result, the chapter utilizes case studies – covering both geographical case-studies like the Syrian highlands and eastern Asia Minor alongside thematic exploration of the imperial law code. This continually-shifting focus makes the chapter stand out as one of the most difficult of the book for a reader to follow. The chapter also suffers stylistically; just one example is the first paragraph of p. 106, which is a single, 10-line sentence containing one aside, three parenthetical statements, one list of lists, and two concepts in 'scare quotes'.

However, the narrative thread recovers with aplomb again in chapter four, 'Two Empire-wide conflicts: the factions and the Christians', which at 90 pages is also the longest single chapter by far, more than a quarter of the whole book. This chapter begins by summarizing the histories first of the Circus factions and then of the Miaphysite-Chalcedonian divide, demonstrating how issues ostensibly about races and Christology in fact represent deeper divides within society. Although the factional strife appears, at first, to be indicative of social conflict and instability, instead what emerges from Bell's narrative is a vision of these groups' utility as *unifying* forces. They provided common activities across the geographical breadth of the Late Antique Roman world, which included men from all social and economic stations of life, and which reinforced communities' ideological support for the regime (if not always the individual) ruling in Constantinople.

Part three contains three chapters, all concerning ideology – as a result, the focus of the narrative shifts decisively from the provinces to Constantinople. Chapter five, 'Ideological conflict in the reign of Justinian I', focuses on Christian-Pagan social issues in the context of the empire as a whole. Although religious conflict between the groups forms part of the picture, the larger narrative is concerned with cultural – specifically, elite – syncretism. In the private domain, this is most thoroughly examined in the context of education, where the Christian classics were incorporated alongside the classical and 'pagan' *paideia* as Roman society's cultural foundation. In the public sphere, the marriage of Christian to Roman ideology served to reciprocally increase the legitimacy and power-base of both.

Chapter six, 'Constructing legitimacy', continues the analysis from the previous chapter while moving the focus of the narrative firmly onto the court of Justinian I. This chapter is most firmly grounded in traditional political narrative of the period – the *Nika* uprising, the re-conquests, and the ravages of the plague of 542 all find a place in its narrative. In it, Bell presents Justinian's vulnerabilities followed by a discourse on policies which the regime utilized to legitimize its rule, supported specifically by evidence from the legislation. Other evidence is presented in chapter seven, 'Hagia Sophia: ideology in stone – a case study', which does what

its title says. Bell's narrative is tight, his thinking well-signposted, and this wraps up the core of the work on a high note.

The work has a few limitations. The focus on already comparatively well-studied geographical areas (such as Syria, Asia Minor and Constantinople) means that there is something of a lost opportunity to apply these theoretical approaches to the better-documented but still less-studied (and incompletely integrated into the mainstream study of Late Antiquity) province of Egypt.

The use of modern anecdotes to illustrate points of comparison provides an interesting insight into the late antique world; however, by their nature, many are very specific. As foreign national resident in the United Kingdom, I'd like to think that I understand many of the contemporary cultural references. However, Bell's main point of reference for understanding social conflict stems from his time as a civil servant in Northern Ireland. I vaguely remember being aware of the Good Friday agreement when it happened, but only as something that happened to other people far, far away. As a student of late antiquity, I sometimes found the contemporary analogies required as much effort to understand as the late antique history they were meant to illuminate. Of course, all cultural reference points are relative, so different readers will get different traction.

It would be nice if the headings were uniformly helpful. Some parts of the book (such as chapters six and seven) are well-marked and informative, but others are not. One would not guess, judging from its title, that the 'ideological conflict' referenced in the title of chapter five was specifically Christian-Pagan. Or, if one were skimming through the volume looking for discussion on how emperors before Justinian dealt with the conflict between Christian sects, that such was the primary topic of the section under the heading 'Imperial policy 2 - its implementation (2): specific initiatives'. My point is perhaps a minor one, but for a researcher trying to quickly reference something, clear sign-posting can be invaluable. It also means that for the reader who might not have time to work through everything between the covers, pin-pointing what she or he may be interested in becomes that much more difficult.

Part four contains only the conclusion, chapter eight, besides the bibliography and appendix that one would expect from such a work. The main points of the work are restated, chapters summarized, and great thoughts pondered. You may safely read these dozen pages and get the main points of the work, but, unfortunately, you would be missing out. Like the conclusion to any good thesis, it is subdued, business-like, and to the point; if that is all one read, one would miss out on the literary experience Bell has to offer. So much of what makes this work interesting is the author himself, who integrates his own experiences, observations, and cultural musings firmly into the narrative. I would not want to mislead the reader to suppose that all work equally well - at times, following the thread of the modern anecdotes can distract a reader from the argument itself - but nevertheless they form part of the experience. If, as historians, you believe that everything is 'grist for the mill', you will welcome Bell's work as one which attempts to put that principal into

practice. Historical narrative, sources, social theory, and personal anecdote all went in; what emerges is a study which self-consciously embraces a unique paradigm for the understanding of the age of Justinian.

Author's Response

Peter Bell

Douglas Whalin's review of my 2013 book on social conflict in the age of Justinian, or, perhaps better, in the 'long sixth century', was doubly welcome. Not only was it distinctly favourable (despite inevitable criticisms of style and detail, some justified), and therefore good for authorial vanity, but more importantly it recognised – and applauded – what for me was a main purpose of the book: showing, by example, the value for all ancient and Byzantine historians of exploiting concepts and frameworks which social scientists, including social psychologists, continue to put before us. We must not treat 'theory' as a noxious potion that those on the dark side might pour into their work in places like Princeton or Cambridge. However, despite the examples of Averil Cameron or Chris Wickham, for instance, and the trends I sketch in my initial chapter, this is still a far from universal infusion, certainly in Oxford. So, thank you, Douglas, very much indeed. I am particularly glad that you have endorsed the way in which, having sown my 'interpretation in the ground of social theory', I have 'nurtured it with autobiographical fertiliser'!

That does not mean I endorse all Whalin's takes on more specific arguments, even when favourable. For example, while I do indeed argue that it is often hard, even impossible, to distinguish clearly the *class* positions of ruling elites from their positions in the *status* hierarchy of the late Roman Empire, we dare not wholly lose sight of the former concept. For this seeks to clarify where groups stand in the political economy and their dynamic relationships with each other; who are the political and economic beneficiaries of prevailing economic arrangements; and, above all, who is exploiting whom?

Likewise, I too wish I had said more about other areas of conflict, including Egypt, and, above all, conflicts between the imperial authorities and the Jews. I did, however, have quite a lot to say about land-tenure in Egypt, again absolutely fundamental to the political economy and class and power-relationships – and not just by the Nile; I discuss Justinian's *Edict 13* (p. 539) which tried to stop a range of administrative abuses by the administration itself; just look at my index under 'Egypt'. But in a relatively short monograph, you cannot tackle all the subjects you might wish; if I have adduced enough material to sustain and justify a broadly satisfactory model of a wide range of social conflicts and their inter-relationships across the Empire, that must suffice for now...

One final point. I am glad that my reviewer pays special attention to my treatment of the circus, that is chariot races, and the churches (not to mention the decline of traditional, Pagan religion). My approach here

rested less on sociology than on social psychology. We cannot but acknowledge the enormous political salience of the games (including theatrical performances) and of religion throughout the whole empire, yet I did not want, in the absence of any clear class linkages in both areas of activity, to reduce my account to a banal factual narrative, or to pass over the reality, however remarkable, that for many what went on in churches and circuses was of enormous existential importance. Drawing on social psychology, I developed an approach which I had already found useful in helping understand community politics (and hatreds) in Northern Ireland — one rooted in a human disposition to build group identities, capable of being both passionate and murderous (and cutting across other loyalties, in the case of the circus factions). Whalin helpfully recognises this, together with my refusal to see such passions as invariably malign or divisive. Were I to take issue with his analysis, I would argue that while there was certainly a group bonding *within* each faction and church, and while such loyalties might stretch across the empire, the institutional nature of the churches and the much wider scope of their activities and their ideologies, as well as the growing power of their hierarchs meant that they were sometimes far from force for unity and the common good. As historians, we must set institutions in their wider social context. But this is a relatively small criticism of a very helpful review. I only hope that other readers will reflect critically -and positively - on what I have written.