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Response

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Response

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It is a pleasure to respond to the comments on my book, *British Society 1680-1880: Dynamism, Containment and Change*, offered by Timothy Alborn, Francis O’Gorman and Joanna Innes. Although most of the reviews of the book when it appeared were considered and judicious, the readings by these three commentators give me particular pleasure. Academics do not need to be praised to be flattered; nor do they need to find agreement. It is compliment enough when the ideas that one offers are taken seriously. Each of these respondents has clearly taken the book and its argument seriously. They have raised questions and criticisms that demand proper consideration. I plan to do that in so far as space will allow and in so far as I am able. These comments draw my attention to weaknesses and absences that were not fully evident at the time I wrote the piece. But I do not intend to present the results of new research. Indeed, in the past few years my focus has shifted away from the questions I was concerned to address in *British Society*. I am now engaged on a project that looks at British history from the perspective of the frontier of empire in the early nineteenth century. As a consequence my sense of the historiography of the eighteenth and nineteenth century has shifted more towards the body of work that deals with empire. Thus, my response will comment more on the conceptual and methodological implications of the comments on my original argument. In particular, I will address topics that in one form or another are raised by the papers presented here. Those issues are: the context in which the book was conceived, especially as it relates to the matter of periodization; the question of the ‘age of reform’ and the dangers and advantages of using contemporary categories to analyze a period; the absence of culture in my book; the unarticulated historical dynamic that underpinned its argument; the issue of empire in the schema of the book; and, finally, the question of British modernity.

I can best approach the first point – context and periodization – by admitting what was obvious to the commentators: that this book was written as a kind of summing up of many years of engagement with some dominant themes in the historiography of modern Britain. And I think it is not out of place to expound a little on the assumptions and purposes that lay behind that project, because it is relevant to any assessment of the strengths and weaknesses of the argument of the book. As Joanna Innes rightly remarks, I came out of an historical world of the 1960s and 1970s when a different approach to social history was opening up new and at the time exciting vistas. I had long thought that one

of the missions of the social history that came out of that era should be to re-write the general history of Britain. This ambitious agenda may have been misguided. We are all aware of the way meta-narratives distort the complexity of the historical process and erect new forms of disciplinary exclusion. Still, it seems to me important not to dismiss entirely the advantages of arguments that try to explain a broad period. They are, I think, twofold.

First, they oblige us to think about change. I continue to believe that one of the central justifications of history is that it helps us explain change. Naturally, there should be argument about how we understand change. It may very well be that scholarship rooted in a focus on socio-economic forces tends to slip too easily into positivist teleologies. But the underlying project of *British Society*, to explain how we got from one point to another in time, seems to me to lie at the heart of what historians should do. This question seems often to be lost in the contemporary interest in culture as the site of historical determination. Thus, to take an example from the currently popular treatment of empire as a cultural formation, there is a tendency to deploy concepts such as modernity as signifiers that float on a sea unbounded by horizons of time or even place. Empire in the eighteenth century, for example, is treated as a source of that century's modernities in the same way as it is offered as the origin of the more advanced modernity of the early twentieth century. Yet no necessity to define what these modernities were is recognized, let alone explain how one kind of modernity was different from another, nor what their relation to one another was.¹

And this suggests a second advantage of taking the long view, also a foundational premise of *British Society*: the exploration of relational associations between the various spheres of society. Along with explaining change over time, this is a further distinctive mission of an historical methodology. Thus, to draw again from the cultural scholarship on empire, we can note a failure to make any attempt to integrate questions of political economy into its perspective. The unfortunate result is that one kind of holistic framework replaces another. Whereas some of us used to believe that a combination of Marx, Lenin, Hobson and Schumpeter contained the key to understanding imperialism, that holy grail has now passed to Edward Said and Homi Bhaba. There is much that can be said in favour of these scholars. But they are not that much interested in contextualizing analysis in time or space. They are interested in the epistemologies of representation. As a consequence, they are not particularly concerned to ask how one aspect of a social formation relates to other aspects of social formations. The end result, I would argue, is a framework of inquiry that individually can be very

interesting and thought provoking, but which soon reaches the limits of its ability to reveal new or sophisticated insights. If we are not particularly interested, for example, in the relationship of the culture of empire to the other signs of empire – its military, diplomatic, political, sociological and economic formations – then there is a distinct ceiling on what culture can be used to explain. There is evidence already of sclerotic tendencies in the ‘new’ scholarship on empire precisely because it is analytically confined to a set of well-established cultural precepts, and essentially uninterested in anything outside of their boundaries.² In any case, the underlying architecture of my book rested on the two combined assumptions: that explaining change is a central task for historians, and that one mark of superior historical arguments is their success in linking various spheres of societal experience. This returns us, finally, to the value of arguing about periods.

As Francis O’Gorman points out in his piece, periods are an everyday currency in historical or literary studies. But – as with the aforementioned cultural studies of empire – periods seldom seem to receive much scholarly attention in their own right. They are seldom examined for the assumptions that are contained within the choice of a particular set of years. Yet their significance should not be underestimated. Writing about history can hardly escape a chronological framework and choosing that framework is itself a statement about historical understanding. Periodization is not simply a convention of convenience that provides us with one way of limiting and controlling our questions and analysis. Periods are necessary for two elementary but important reasons. First they are essential to contextualizing historical analysis. How can we understand the significance, for example, of late nineteenth century ‘modernities’ without contextualizing our particular aspect of interest within a wider vista of historical relationships? And, second, periods are essential to a conception of the dynamic of historical change over time.

The question of the ‘age of reform’ in early nineteenth century Britain is a case in point. It is worth focusing on this for a moment because how we conceive of the 1830s and 1840s underpins perhaps the most serious objection to the argument of my book. Indeed, both Joanna Innes, and Martin Hewitt (the latter in a paper soon to be published in *Victorian Studies*), have rightly made this the fulcrum of their criticisms of my notion that the Victorian period needs to be folded into the wider chronology of 1680-1880.³ In essence they have argued that this period contained sufficiently new features to mark a significant break with the past and gave shape to the Victorian period.

This period has long been regarded as one in which the old world of

the eighteenth century finally gave way to the new world of the nineteenth century. Exactly how it is to be assessed in the longer-term sweep of British history from the eighteenth to the twentieth century, however, is precisely the subject of debate. The period's importance in this respect lies in the claim that it is the origin of nineteenth century modernities. But how one assesses the significance of these decades depends to a considerable extent on how we treat the contemporary definitions of 'reform'. Both Hewitt and Innes (and her collaborator Arthur Burns) take contemporary assertions of reform seriously and see their task as elaborating and exploring exactly what was at stake in these claims. This is also the essence of Francis O'Gorman's comments from the perspective of literary history. I have no objections to that procedure. Indeed, it is a valid way to practice the historian's craft; and it may very well lie at the heart of what literary historians do. There is a whole school of history that places a first priority on entering the mind of the past. O'Gorman's formulation puts this procedure rather nicely. Explaining the importance of understanding what the Victorians meant by 'reform' is essential, he argues, to understanding what they thought they were doing and why they acted the way they did.

This is quite right. But the difficulty comes when we want to move beyond that and take a slightly broader perspective, of either concept or time. We know how the story ended, and as historians we are entitled to ask of past actors how what they did was connected both with what preceded, and followed, their actions. These connections were not necessarily of interest to the actors of the time. Yet in order for us to see why what they did took the shape that it did, it is useful to move beyond their own mental world. And this was my purpose in *British Society*. It was informed by a belief that much of the foundational historiography of the Victorian period had not only adopted the categories of the time, but in a sense had been captured by them. In the process, I believed, the categories of contemporaries had been reified.⁴

Reading both Innes and Hewitt's remarks from several years later, I readily concede that it would have been better if my book had discussed in more detail exactly how that period of 'reform' could be conceived within the longer term context of the argument. Had I done so I think I would have admitted the formative importance of the 1830s and 1840s for shaping much of significance in the social and political world of the mid-Victorian period. Indeed, one of the revisionist claims in the book of essays edited by Burns and Innes is to push that period back to include the aftermath of the American War, and to expand the meaning of 'reform' beyond the exclusive focus on parliamentary reform to include moral and social reform. But for my taste making these moves

fails sufficiently to escape from the trap of sanctifying and elaborating the meanings of the time. It is correct to point to the fact that ‘reform’ was a keyword for this period. But its early nineteenth century dominance surely began with the Wilkite movement and then extended into a wider context of moral renewal, both for individuals and society as a whole. This is extremely important, and it suggests that a fruitful line of approach to understanding this period may be to follow historians like Dror Wahrman into the world of subject formation rather than class formation.⁵ If that is the case, then I would again point to fact that the origins of this notion of ‘reform’ surely extends back to the later seventeenth century. It was at this moment that reform gets secularized, as it were, in organizations like societies for the suppression of vice which were detached from an essentially religious foundation of social morality (although they were not detached from religion itself) and were focused very much on altering individual sensibilities.

But it is a reflection of the instability that is attached to this kind of periodization that Wilkes gets only passing mention by Burns and Innes. Of course, it would have been quite possible to have framed their book from the Wilkite moment as well as from the moment of evangelical reform in the 1780s. Indeed by extending the ‘age of reform’ back to the last third of the eighteenth century, they are again adopting the rhetoric and language of the well known reformers of that period. In short, I do not think these historians have escaped from the trap that I was concerned to get us out of.

Furthermore, I would also argue that my concept of dynamism and containment can accommodate the evidence and arguments that are presented for arguing for the pivotal importance of the ‘age of reform’. Here is where I would agree that my conceptualization would have been improved by a deeper and more complete explication of how change and containment worked within the period as a whole. In other words, I did not present a mechanism that explained my sense of how change was accommodated within limits that did not essentially disrupt the social and political formations derived from 1688. Nor did I explain the various phases of change within that broader period. It is impossible for me to do this here. But my point was that the quality of change did not shift the terrain of political, social or economic relationships into new landscapes that needed different organizational structures to govern them, nor a vocabulary to explain them that would have been incomprehensible to the world of the late seventeenth century. Thus, the forces for change worked within limits that were frequently quite consciously conservative. To cite just one example from Burns and Innes’ book: the intent of Daniel O’Connell in pushing successfully for

catholic emancipation was quite explicitly to *preserve* social and political relationships in Ireland, not to disrupt them.⁶ Reform in this context was clearly within the Burkean tradition rather than the Painite tradition.

But of course change was dynamic in the sense that it continually tested the boundaries of prior arrangements. Thus, again to take a pertinent example, the eruption of rebellion in the North American colonies caused a tectonic movement of the existing formations that underpinned political and social relations. The end result was that the question of the political arrangements that had emerged from the later seventeenth and early eighteenth centuries once again were pushed onto the political agenda. Burns and Innes are quite right to contextualize the 1830s and 1840s in terms of the later eighteenth century. The debate and the contest over what to do about the new pressures for change was protracted and contentious. And it was so because the legacies of 1688 were at stake. This was clearly recognized by those on both sides of the political debate which climaxed in the years 1828-32. But, once again, the point at issue for my thesis is how we read the events of those years within the context of the past. My reading of that process was that the limits of 1688 were restored by 1832 rather than were broken.⁷ The political nation remained constricted and closely defined. Most importantly the place of the 'people' in the political world remained confined to the noisy world of out of doors politics, which was precisely where it had been since the 1690s. This does not mean there was no 'change'. Obviously, there were new and significantly new elements in the configurations of politics. Leadership styles, for example, were modified; organizational structures were developed that were fundamentally new. Chartism, for example, may have used the rhetoric of eighteenth-century politics, but its organizational methods looked way beyond even the practices of the mid-nineteenth century. Still, this element of change was not sufficient to introduce the organizational or ideological structures of 'class politics' that came to shape politics after the 1880s.

Hewitt's case for the 1830s and 1840s is a bit different. The strongest part of his argument against my framework lies in his deployment of evidence from scientific culture, particularly the openings to new conceptions of the visual and optical during these years. I am not qualified to take issue with this particular objection. I just am not familiar enough with the scholarship on theories of knowledge or perception. However, I think the question about such theories should be: what role did they play in reshaping the way society was culturally, politically or socially organized? Were they agents of destabilization in the mid-

Victorian period or were they agents that redrew boundaries? Still, in focusing on this issue Hewitt points to a serious absence from my book, which others have noted too. I did not deal with culture. This was as much a strategic decision as it was an intellectual decision. I did in fact collect quite a lot of material on religion and religious culture over the period. But I could see no way to do justice to the category of culture – even defined quite narrowly – without an overly large extension to the book. In addition, the historiography I was coming out of and reacting to, was largely that of a Marxian social-economic and political approach that had been formative for my generation of British historians.

Nevertheless, it always lurked at the back of my mind that certain aspects of cultural history over this period did lend themselves to the arguments that I put forth. In particular, the social history of religion and especially evangelicalism would seem to fit the case for melding the nineteenth century into the earlier period. Methodism was a direct product of the early eighteenth century, in spite of the later attention that was given to it by early historians of the labour movement as a quintessentially working-class religion. The moral impetus that underlay the evangelicalism of the great ethical causes of the ‘age of reform’ surely makes most sense if it is seen as part of an ongoing concern with the reformation of manners that runs through the period from the 1690s. Indeed, the moral offensive – the rational recreation movement – against popular culture from the 1830s would have been thoroughly familiar to people like Jonas Hanway a century earlier or the London methodists who launched a series of prosecutions against immorality in the 1760s.⁸ Many of the central strategies that Victorian philanthropy used to address social problems were far more like eighteenth century initiatives than they were like anything to be found after the later nineteenth century.

Another issue that was not confronted as explicitly as it could have been in the book was raised by Joanna Innes: the question of the historical dynamic that underlay my conception of change within continuity in the period 1680-1880. She raises the question in particular in relation to the end of the process in the mid to late-nineteenth century. There seems to be agreement that the later nineteenth century did see the emergence of new social formations. But Innes raises the excellent point that how these new terrains arrived is not addressed. It was a problem that I was aware of when I finished the book, but only briefly addressed. Indeed, I contemplated a new project that would take another look at the mid-Victorian period from that perspective. I think that it would be possible to construct an argument that the mid-Victorian period was dominated precisely by the tensions and in-

stabilities that were created by the successful restoration of order in the reforms of the 1830s and 1840s. The idea that this was a period whose stability followed from the defeat of Chartism is one that dies hard in the historiography, and it is not entirely wrong. But it is only one half of the story. The old formulation of 'equipoise' that was enunciated by W.L. Burn was a brilliant insight into this period. But equipoise suggests a sort of natural balance, when in fact I would prefer to see it as something that was in constant danger of slipping out of control and had to be continually maintained. This was after all the period when what Mary Poovey called the 'ideological work of gender' moved into high gear to produce and secure the authoritative boundaries of sexual and gender difference.⁹ Was this a product of the complete hegemony of domestic ideology, or rather of its need to negotiate a stable dominance?

As we now more fully appreciate, Chartism did not immediately disappear, but tended to shift its focus to a more local level, where it continued to inject an element of instability into the social and political landscape of equipoise. A similar instability ran through the conduct of politics. The popular politics of the mid-nineteenth century continued to be dominated by an 'out of doors' rowdiness that was part of the disorders of the Hanoverian era. Likewise, the continuing debate about parliamentary reform did not go away. It remained a major pre-occupation within parliament and the formal political nation.¹⁰

So what happened to disrupt this stability? I personally think that is a question that could well occupy the attention of nineteenth-century specialists. It is impossible for me to answer it here. I can only suggest as I did in the book, that a dynamic composed of forces both internal and external to British society converged to disrupt the structures of social, cultural and political authority that had been restored in the age of reform. These forces surely took different forms in the different spheres of societal experience. At one level, they represented a growth of systems and forces that had been implanted earlier. Thus, as Theodore Hoppen has pointed out, one distinguishing feature of the mid-Victorian period was the way what he calls 'industrialism' established itself in the economy. By this he means the expansion of industrial forms that had been prefigured in the first industrial revolution, and were to achieve general dominance during the second industrial revolution.¹¹ Other internal forces intruded themselves more rudely into the social terrain. The relatively sudden displacement of religion as the main source of cultural authority in the 1860s and 1870s, for example, could not have been predicted in 1830. At another level, there was the question of unintended consequences that followed from actions during the age of reform. Here the political reform of 1867

would offer prime evidence for this aspect of the process.

1867 opened the door to a series of legal and administrative challenges that in their turn pointed to an expanded electorate. But the nature of the discourse itself that surrounded 1867 demonstrated the extent to which the restoration of the 1830s had decayed. The exact confection of forces that created the opening offered by 1867 is, of course, very complex. But the central question in 1866-67 continued to be the question that had animated the politics of reform in 1830-32. How much reform was needed to bring politics back into balance?¹² This question was much less loudly trumpeted in 1867; it was also recognized to be much more complicated than in 1832 – witness the intricate legal definitions that were resorted to in order to secure a bill. As Disraeli learned when he tried to calculate statistically the safest level at which to set the franchise, the possibilities of containing politics to the old idea of a limited political nation were much weaker now than they had been thirty years before. It was as much a symptom as a cause of such an erosion that Gladstone, for example, had already begun to move – however cautiously and with strong mental reservations – towards identifying his political future with the idea of a democratic, working-class electorate.

Outside the confines of the islands, external forces were also operating to undermine the terrain of the older social, political and cultural structures. Here the leading force was surely the changing place of empire in British society. Empire came to possess a different valence in the culture and politics of the later nineteenth century than it had possessed before.¹³ Over the mid to late-nineteenth century the role that empire played in the composition of Britishness underwent a sea change. The traditional role that empire played in national identity derived from the original idea that Britain's empire was a Protestant empire that brought liberty and freedom in its wake. The most recent version of that ideology was the christian humanitarianism of missionary culture. This notion was displaced as the central discourse of empire over the course of the mid-nineteenth century. By the late-nineteenth century, empire was justified as critical to Britain's competitive role in the changed world political economy and as morally legitimate by dint of new theories of racial hierarchy. The older christian humanitarianism does not disappear, of course. But its place in scale of justifications for empire had changed, along with the way it was articulated.

Similarly, the way empire is placed in the politics of the nation changes. Disraeli shifted the discourse in that respect. He put empire into popular politics in a way that it had not been before. The association between this and the growing democratization of the political

nation is an obvious one. The political discourse of empire was a way of creating a new kind of national unity at a time when it was necessary to find ways of overcoming competing loyalties (like class) in politics. The fact that empire was also increasingly present in the popular culture through things like product advertising or popular songs and stories, only reinforces the argument that the relationship of empire to national culture is decisively altered during this period.

Empire, it is generally agreed, was a critical component of 'modernity', although, as I remarked earlier, the idea of modernity has been used with insufficient attention to contextual specificities. In the late-nineteenth century, however, it is easy to demonstrate the particular configuration of empire's contribution to 'modernity'. It was central to the subjectivity of Britishness. It was central also to the idea of economic modernity. And this provides me the opportunity to comment on Timothy Alborn's interesting remark about the unresolved character of British economic and social modernity that runs through my book. I regard this as a fair comment. This was another issue that was incompletely addressed.

To explain this, I have to circle back to where I begin in this brief 'Response' and speak again about the subjective experience from which the book grew. My own historical practice emerged from a scholarly context which was heavily inflected with the idea that the British experience constituted the norm of twentieth-century modernity. The book was written against the backdrop of a whole generation of scholarship that placed Britain at the forefront of the path to twentieth-century modernity. Thus, the starting point for my own reconsideration of the period 1680-1880 was a series of notions that for a long time dominated the historiography of Britain and that saw British history broadly as a series of 'firsts'. They included the idea that Britain was a political model in respect of the growth of parliamentary democracy from the seventeenth century; the notion that she was the first industrial nation with the first 'modern' economy; that she had acquired the first of the modern empires; and that she was also the first to experience mature class politics, emanating both from the bourgeoisie and from the working class.

But I think that Alborn's way of drawing a conclusion from my argument is right. It is not helpful to conceive Britain as playing such a role. Indeed, if twentieth century economic modernity is defined as constituted centrally by industrial capitalism, then Britain's relationship to that modernity was characterized by deep ambiguities that flowed from the legacies of the past that were described in *British Society*. It was not that Britain somehow 'lost' the 'spirit of industrial capitalism' at some

point in the twentieth century.¹⁴ It was that she never actually had it in the first place. If we wish to speak in terms of historical ‘modernities’, the occasion of British modernity was the moment when the empire of commerce and trade left its deep imprint on the culture, traditions and institutions of Britain. And that moment was the subject of my book, *British Society 1680-1880*.

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Endnotes

1. Examples of this tendency are to be found in Steve Attridge, *Nationalism, Imperialism and Identity in Late Victorian Culture. Civil and Military Worlds* (Basingstoke: Palgrave Macmillan, 2003) and Kathleen Wilson, ‘Introduction’, in *A New Imperial History. Culture, Identity and Modernity in Britain and the Empire 1660-1840* (Cambridge: Cambridge University Press, 2004).
2. See my piece ‘One Big Thing: Britain and its Empire’, forthcoming in *Journal of British Studies*, 2006.
3. Martin Hewitt, ‘Why the notion of Victorian Britain *does* make sense’, forthcoming *Victorian Studies* (Spring 2006). For a fuller elaboration of the view of Innes on this period see the ‘Introduction’ to the very important book of essays that she edited with Arthur Burns, *Rethinking the Age of Reform: Britain 1780-1850* (Cambridge: Cambridge University Press, 2003).
4. But this does not mean an inferior kind of history. The first edition of Asa Briggs’ *Age of Improvement* (London: Longmans, 1959), for example, remains a classic work of history because it conveys an interpretation of a period that successfully combines a meta-narrative with richly nuanced detail and attention to the place of different levels of social experience within the narrative.
5. *Making of the modern self: identity and culture in eighteenth-century England* (New Haven: Yale University Press, 2004).
6. See Jennifer Ridden, ‘Irish reform between the 1798 Rebellion and the Great Famine’, in Burns and Innes, *Rethinking the Age of Reform*.
7. But this is not to say that J.C.D. Clark’s ‘ancien regime’ may be extended beyond 1832. I do not subscribe to the notion that pre-1832 England (or Britain) may be seen as an ancien regime. Indeed, that is only possible if one fails to recognize that the landscape of British society was shaped by far more than politics and religious-political thought.
8. Paul Langford, *A Polite and Commercial People: England 1727-1783* (Oxford: Oxford University Press, 1989), 499.
9. Mary Poovey, *Uneven Developments: The Ideological Work of Gender in Mid-Victorian England* (Chicago: Chicago University Press, 1988). See also Margot Finn, *After Chartism: Class and Nation in English Radical Politics 1848-1874* (Oxford: Oxford University Press, 1993).
10. See James Vernon, *Politics and the People: A Study in English Political Culture c.1815-1867* (Cambridge: Cambridge University Press, 1993); Miles Taylor, *The Decline of British Radicalism 1847-1860* (Oxford: Oxford University Press, 1995).
11. K. Theodore Hoppen, *The Mid-Victorian Generation 1848-1886* (Oxford: Oxford University Press, 1998), 3.
12. This still seems to me to be the central story of 1867 in spite of the attempts to place race and gender at the centre of the issue. This can only be done by inference and

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implication. There is very little direct evidence that these were live or threatening issues in the political and social process that went to compose 1867. The leap in the dark was not a leap into an unknown gender or racial future, but to an unknown class future. This is not to deny that race and gender issues were part of the context of the 1860s, for they clearly were. The Jamaican rebellion was a profoundly important event in the way empire was played out in national politics. See Catherine Hall, Keith McClelland and Jane Rendall, *Defining the Victorian Nation* (Cambridge: Cambridge University Press, 2000).

13. Which is why most current scholarship focuses on the late nineteenth and twentieth centuries where it is relatively easy to demonstrate the centrality of empire to the culture and politics. For a very intelligent treatment of this see Andrew Thompson, *The Empire Strikes Back? The Impact of Imperialism on Britain from the Mid-Nineteenth Century* (London: Longmans, 2005).
14. Martin Weiner, *English Culture and the Decline of the Industrial Spirit 1850-1980* (Cambridge: Cambridge University Press, 1981); or from different angle see Corelli Barnett *The Audit of War: the illusion and reality of Britain as a great power* (London: Macmillan, 1986).