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**Joseph Bergin**, *Church, Society and Religious Change in France, 1580-1730*. New Haven and London: Yale University Press, 2009. xvii + 506 pp. Maps, tables, notes, bibliography, and index. \$55.00 U.S. (cl). ISBN: 978-0-300-15098-8.

Reply by Joseph Bergin, University of Manchester.

To be reviewed in *H-France Forum* is a bracing experience, not just because authors know that they will be given a decent run for their money, but even more so because finding oneself reading, in just one sitting, no fewer than four inevitably different reviews is itself a pretty rare occurrence. In this case, thanking the reviewers for the time and effort they put into reading and reporting on my book is not just a matter of convention or a *clause de style*. Their observations, criticisms and suggestions are thought-provoking, even where I disagree with them. So much so that they have made me return to the actual text of my book, something that I usually find too excruciating for a long while after publication! The fact that all four reviewers ultimately think well of the book facilitates my task, to the point of allowing me to write a more succinct response than I could have anticipated. Despite that, it would be misleading to suggest that their readings are identical. Therefore, my response first addresses general issues and subsequently turns to more specific points raised by the individual reviewers.

What kind of historian am I or—more to the point—what kind of history is my book? All of the reviewers were drawn, in one way or another, to this question, but they ended up offering different views. Nicole Lemaitre regards the book as “une histoire sociale des phénomènes religieux.” Keith Luria concludes that “with its focus on institutional history, it is definitely a history of the church rather than of religion.” Jotham Parsons largely agrees with this verdict, describing the book as an insider’s view of the French church, whose “focus and explanatory framework ... [are] essentially institutional.” Susan Rosa thinks that although it is an internal history of the French church, “it in no way implies a narrow institutional focus that neglects the social and cultural impacts of ecclesiastical reforms.” Such a diversity of judgements allows me, I believe, to add my own pennyworth. I have always seen myself as a social historian of the political, in the broadest sense, though this book has brought me much nearer to religious history than any previous one. It is true that institutions have figured in many guises in what I have written over the years, but my view of institutional history is the same as the one of institutional geography that I presented in my book: “largely descriptive, with limited explanatory power on its own” (p. 17). As far as institutions are concerned, I have always been primarily interested in what societies do with them and, especially, how they “populate” them. Having written, many years ago now, about Richelieu and the kinds of power-bases that he created for himself, I think that I am as aware as other historians of this period of the limitations of the classic institutional history of politics, and of the need to understand the invisible personal and family networks that made almost every aspect of government and society work. I am entirely with Parsons when he sees parallels in the ways in which both church and monarchy had to negotiate with and accommodate local society in order to achieve their aims. So I would like to think that when I write about, say, church benefices, religious orders or confraternities, it is the individuals or groups who are directly involved with them as beneficiaries or members rather than the institutions themselves that are the central to the analysis. If institutions come

and go, it is in large part due to changing social demand, taste or interest. But with something as old and durable as the Catholic church of seventeenth-century France, some institutions were “harder” and less malleable than others. Consequently, the challenge to the historian, as I see it, is how to get inside the skin of institutions and their social dimension in order to see how they operate and, despite the difficulties involved, how they change over time. Indeed, what I set out to do, especially but not exclusively in the first half of this book, was to show, as Rosa acknowledges in the conclusion of her review, “how the church actually *worked* in seventeenth-century France.” If my efforts have, as she herself puts it, “taken her to a new level of understanding” of those workings, then I can begin to feel that my book has achieved something worthwhile. In this context, I can accept the verdict of Jotham Parsons, who is the most adamant of the reviewers in seeing my book as institutional history, that my work is that of an insider. Anyone who has worked in broadly the same field for a generation or more is almost bound to become an insider of some kind, but as far as I am concerned, my long familiarity with church hierarchy and organization does not bring any distinctive agenda or ideological baggage with it.

If there is a real loser in the approach that I have taken in my book, it would seem that it is the history of (religious) ideas. Both Rosa and Parsons make this point with some insistence. Rosa writes, “Bergin’s distinction between intellectual history and other kinds is not helpful in enriching out understanding of the religious history of seventeenth-century France.” Parsons argues, after a discussion of my handling of Jansenism, that “one lesson of that story is that ideas, even apart from their institutional instantiations, mattered profoundly for early modern Catholicism.” Rosa does acknowledge that a book with a strong emphasis on religious practices is likely to give ideas short shrift, but she still wishes that I had found more room for a “rigorously contextualised intellectual history.” To respond, my criticism of the “history of ideas” approach was made in the context of a discussion of the history of Jansenism, which has too often been written and understood as primarily a history of ideas, with scarcely any consideration for how it fared in the real world or who was attracted by it. I shall return to this point later, but in the meantime I would like to point out that the history of ideas is not wholly absent from my book, and in more than one chapter I do discuss ideas and wherever possible their diffusion. I do so, for example, when discussing the kinds of spirituality—mysticism included—available to seventeenth-century people, the ways in which the moral theology concerning sin and confession evolved and, of course, the evolution of Jansenist thought itself. Had I been writing another kind of book, I would probably have written more of the intellectual history in question, but the format and content of the book that I was trying to write meant that I was seeking primarily to understand those ideas that could be demonstrated to have been diffused and had an impact on French society. Rosa wishes that I had been more ambitious than that and holds up Moshe Sluhovsky’s recent book on mysticism and the discernment of spirits as a model of how religious history should be done.[1] If I failed to cite Sluhovsky’s book, it is simply because my book was completed before I came across his. It would certainly have alerted me to possibilities in tracing seventeenth-century spirituality that I overlooked, but given that Rosa concluded her own review of Sluhovsky’s book by saying that he had “restored a now venerable picture... the outline of ...[which] remains the same,” I am unsure of how much she thinks that engagement with it would have altered my approach or conclusions.[2] As generalisations go, I have no reason to disagree with Parsons when he writes, “there is still a good deal of work to do to integrate the intellectual and the institutional histories of the century of saints,” but I do think that the degree of integration will always vary according to the primary focus of individual works of history.

I now turn to the individual reviews to discuss the particular points that they make. I do this in no particular order. The issue most extensively discussed by Keith Luria in his review relates to the explosion of religious orders and congregations, male and especially female, during the

seventeenth century. In order to answer the central question, as he sees it, of what “actually motivated the recruits who joined new orders and congregations,” he suggests that a different approach to their history than mine is needed, “one that can assess the subtle combination of the recruits’ piety and search for spiritual purity along with their desire, perhaps, for a different sort of public reputation.” Understanding the attitudes of the ever-growing numbers of men and women entering the orders is, clearly, a legitimate desideratum, but I am not sure which approach would enable historians to make tenable judgements on the subject. I have seen scarcely any empirical studies that would lay bare the motives of individuals when making the decision to join an order.<sup>[3]</sup> Years ago, Thomas V. Cohen conducted a study of those joining the Jesuits during the half-century after the Company’s foundation, but that analysis was only possible because the individuals in question were required to respond to a thirty-point questionnaire asking them to explain, well after the event, why they had joined. Intriguingly, those sixteenth-century responses were quite the opposite of what we might have expected from Jesuits. Most of the respondents said that they joined to flee a sinful world in order to serve God and save their souls: the great “works” that the Company set itself did not figure much in their explanations of their motivation!<sup>[4]</sup> Where records from French religious houses survive, for both men and women, they are virtually never retrospective. Those responding to questions about their motivation usually did so before admission to full profession, and they had evidently learned by heart what to say in advance because anything else would probably have meant rejection.

Until we find some way of differentiating motivation from rationalisation in statements of why individuals joined religious orders, we probably have to think in terms of opportunity rather than motivation. The proliferation of convents, especially as they became more diverse in their way of life and activities and as they spread to relatively small towns, opened up opportunities that had simply not been there previously, since the older female convents had been mostly remote and socially exclusive. It is easy to underestimate how big a change this represented, especially when, for women, either a very small, or no dowry at all was required and when new recruits were not faced with the constraints of being classic “choir” nuns. The piety of those individuals, especially female, joining the newer orders of the seventeenth century, was very close to that of the parish *dévots* who previously belonged to devotional confraternities or of the so-called “third” orders, whose members were already close to being “regulars.” The distance between these two “worlds” narrowed hugely; the prospect of living together in active local communities whose spirituality and activities corresponded to their preferences and capabilities was far more real than in the past. Historians usually invoke the spiritual energy released by the Catholic League. But given that most of them see that effect tapering off by around 1640, it cannot be a full explanation of the growth in numbers joining the orders, especially among women, which was far greater after than before 1640.

Considering the extensive use that I made of Luria’s own work on several of the subjects that I discussed, I was relieved to read his verdict that I was “not consistently dismissive of ‘popular’ religion.” As the title of my book suggests, I did not set out to write a history of “popular religion,” partly because of the problematic nature of the concept, and partly because I wanted to avoid conveying the impression that the elements that constituted popular religious practice could be neatly bundled together under a single heading. As far as the seventeenth century is concerned, I am more impressed by the flow of religious ideas, practices, and projects involving both laity and clergy, which cut across categories such as “popular” or “elite” religion. Sticking with these labels would have made it impossible to engage in the more extensive discussions that I attempted in chapters ten through twelve, focusing on the cult of the saints, the sacraments, religious education, and so on. It is probably inevitable that an approach that focuses on religious change will give disproportionate attention to new forms and those advocating them. By contrast, “popular religion” tends, unless I am mistaken, to be a static

rather than a dynamic entity. My intention in analysing the various features of religious change was to get close to the tensions between old and new, and to measure the persistence of the familiar in matters of religious activity at the parish level, whether it be in the preference for healing saints rather than intercessory saints, or for confraternities that were social and associative rather than devotional in their objectives. For that precise reason, I can readily agree with Luria's assessment of the double-edged character of the confraternities, especially on the continuing capacity of the Penitents to resist clerical programs of religious reform. I hope that readers will not find that my approach entailed a dismissive *attitude*, let alone a consistently dismissive one, towards popular religion, so much of which can only be observed through the eyes of witnesses who were not always favourable to its manifestations. One final point here: Keith Luria regrets towards the end of his review that I did not discuss French overseas missions. I did actually (p. 291), albeit briefly, in order to make the point that some of the challenges and lessons of foreign missions were indeed channelled back into discussions about how to conduct missions within France itself. Had I expatiated more fully on this issue—and on many others of genuine interest—I fear that I would have written a much longer and perhaps more shapeless book.

Nicole Lemaitre's review provides a running commentary on my book and is no less challenging for all that. In its core passages, which are a model of clarity and economy, she offers an acute reading of the religious historiography as practiced in France itself. When she characterises what I have written as a history of both clerics and laypeople, it is specifically my analysis of confraternities that she has in mind. The long set of questions that she raises about female religiosity and its relation to confraternities constitutes on its own a substantial research program that would entail detailed investigation of confraternities in rural parishes as well as in the more familiar urban areas. I do not believe that her question about the "l'impact réel de ces écoles de la piété individuelle" can be answered with any confidence as yet. Marie-Hélène Froeschlé-Chopard has suggested that the more successful devotional confraternities were, the more they led people towards an increasingly private and individual form of piety, the logical outcome of which would be the redundancy of confraternities and their "external" religiosity built around an associative sociability!<sup>[5]</sup> The logic of this sounds plausible enough, but was that point ever reached before the later eighteenth century? It is certainly far from clear that devotional confraternities massively replaced their older counterparts, and it is even more unlikely that they did so in rural areas and small towns.

Lemaitre also notes that we are far better informed about the confraternities or sodalities that depended on the Jesuits than those connected to the Carmelites or the Dominicans. My guess is that this discrepancy is not just the result of differences in the survival rates of the archives of the different orders but probably represents the different ways that the individual orders recorded such activities at the time. All of them would presumably have had registers of members, but the Jesuits were almost certainly far more methodical in collecting and using data on *all* their activities—and not merely about their devotional sodalities—and the annual reports by individual colleges and other Jesuit houses to their Roman superiors ensured that some at least of the information they collected would survive in archives that were not subject to the damage inflicted on those within France after 1789. It is no accident that Louis Châtellier's pioneering study of Europe's *dévots* focused so strongly on the devotional associations affiliated to the Jesuits.<sup>[6]</sup> The mapping of France's confraternities would have been a major help to historians, placing the expansion of the Jesuit associations alongside the many others. As I hinted in my book (p. 362), however, a project of that kind was undertaken, but despite including major scholars such as Michel Vovelle, it proved unable to overcome the obstacles—essentially the insuperable discrepancies between records from diocese to diocese—in its way. Lastly, Lemaitre's contrast between sixteenth-century Paris hounding and murdering its Protestants and eighteenth-century Paris refusing to persecute the Protestant artisans of its

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suburbs came as a revelation to me, and it opens up the possibility of revising the historiography of the capital's religious evolution during the seventeenth century.

I have already touched upon the comments made by Susan Rosa concerning the overall approach that I adopted in my book. It remains only to respond here to the one particular criticism that she makes of my treatment of Jansenism, which of course cannot be entirely detached from the question of the scope that I allow to intellectual history. She argues that she is "not sure" that she agrees with me that moral rigorism counted for more than the Augustinian theology of grace and free will in explaining the attraction of Jansenism. I take her "not sure" to mean that she does not think that the two ingredients are mutually exclusive and, therefore, that the disagreement between us is absolute. No doubt, there was a pure Augustinian version of Jansenism, beginning with Jansen's own great work, the *Augustinus*. But how many French Jansenists, even among the intellectually committed, ever read or were seriously influenced by this vast tome? I doubt if an intellectual history of Jansenism would come close to providing an accurate portrait of what French Jansenism was or why it evolved the way that it did from its Saint-Cyranian beginnings to its Quesnellian metamorphosis in just over half a century. The view of Jansenism presented in my book emphasizes the roundabout way in which Jansenism entered the bloodstream of French Catholicism. It was not via academic theology, nor by any fascination in France for theories of grace and free will, debates from which French theologians were singularly absent from the end of the Council of Trent to the 1640s. And when it did look as if French theologians might fall for Jansen's teachings, the response of the Sorbonne and the authorities was pretty drastic, as it had been over a century earlier against Luther. I continue to think that it is more fruitful to see French Jansenism as a religious movement driven primarily by ethical and pastoral concerns, even when we take account of the Arnaulds and other luminaries. This approach enabled me to present Jansenism as a force for religious change, the promotion of a vernacular liturgy, and the extension of lay activity within the French church. These efforts were naturally tainted by their association with Jansenism and have rarely been given their due by historians. In the end, Rosa seems to accept this when she adds a few lines later that "none of this should suggest that Bergin should have written a different book. Far from it!"

If I end with Jotham Parsons's review, it is because before making the general critiques of my methods that I quoted towards the beginning of this response, he wrote that "I can think of no more useful line of criticism than to discuss some of the avenues for future research that it [my book] suggests to me." Apart from Jansenism, his principal suggestions concern the impact of intellectual controversies on religion and the effect of a well-developed disputatiousness on the French church's ability to counter the challenges of the Enlightenment. But not having written about these topics, I can do no more than acknowledge their potential value for future work. There is certainly a body of serious scholarship already in existence which could serve as a launch-pad for investigating the impact of the controversies in both politics and intellectual life.[7] Parsons was not the only reviewer to comment on the relative absence of politics, the Huguenots, and political ideas generally from this book. This was a conscious decision that I did not make lightly, but in the end it was a pragmatic one. Having made the decision to write a particular kind of book, I was aware that the discussion of more than one topic there was weakened by the limited reference to politics, whether it be construed as ideas, government action, or other factors. In the preface (p. xiii) I briefly expressed the hope that a volume on religion and politics would follow in due course. That is still my hope, and the volume is my next venture.

## NOTES

[1] Moshe Sluhovsky, *Believe Not Every Spirit. Possession, Mysticism and Discernment in Early Modern Catholicism* (Chicago, Ill.: University of Chicago Press, 2007).

[2] Review in *Journal of Modern History* 81, no. 1 (March 2009): 162-4.

[3] One effort in that direction is Dominique Dinet, *Vocation et fidélité. Le recrutement des réguliers dans les diocèses d'Auxerre, Langres et Dijon (xvii<sup>e</sup> – xviii<sup>e</sup> siècles)* (Paris: Economica, 1988), in which the author insists that motivations are rarely simple.

[4] Thomas V. Cohen, "Why Jesuits joined, 1540-1600," *Historical Papers* (The Canadian Historical Association, 1974): 237-58.

[5] Marie-Hélène Froeschlé-Chopard, *Dieu pour tous et dieu pour soi. Histoire des confréries et de leurs images à l'époque moderne* (Paris: Harmattan, 2006).

[6] Louis Châtellier, *L'Europe des dévots* (Paris: Flammarion, 1987).

[7] For example, Alan Charles Kors, *Atheism in France 1650-1729, vol 1: The Orthodox Sources of Disbelief* (Princeton: Princeton University Press, 1990); Henry Phillips, *Church and Culture in Seventeenth-Century France* (Cambridge: Cambridge University Press, 1997); and more recently, Sylvio Hermann de Franceschi, *La crise théologico-politique du premier âge baroque. Antiromanisme doctrinal, pouvoir pastoral et raison du prince: le saint-siège face au prisme français (1607-1627)* (Rome and Paris: École Française de Rome and De Boccard, 2009), and Jean-Pascal Gay's forthcoming study of the casuistic controversies of the later seventeenth century.

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