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Philip Benedict, *Season of Conspiracy; Calvin, The French Reformed Churches, and Protestant Plotting in the Reign of Francis II (1559-1560)*. American Philosophical Society Press, Philadelphia, 2020, 224 pp. ISBN 978-1-60618-085-3, \$37.00.

In some ways, Philip Benedict's *Season of Conspiracy* is superb; in other ways it is deeply flawed. These two appraisals track with the study's two aims. The study's first aim is to examine the plots that were contrived against the French monarch—particularly the Amboise Conspiracy and the Affair of Maligny—during the reign of King Francis II (July 1559 to December 1560). But its larger aim is to offer a reassessment of the connections between Calvin and the Reformed churches and these plots and, in this way, a rethinking of Calvin on the related themes of active resistance to authority, political acumen, and war.

On the plots themselves, the monograph is excellent. Benedict unearths extremely impressive details about these plots and does a wonderful job looking at the specific events, court cases, letters, legal testimony, wills, and other related materials associated with these conspiracies. Benedict exhibits a mastery of these documents; it really is outstanding. He speaks about a myriad of individuals from François Morel to Bertrand de La Roche Chandieu, Villemongis, Charles Ferré, Guillaume Prevost, and numerous other figures, major and minor. All of this is extraordinarily impressive.

The study's pursuit of its larger aim, however, is weak. It exhibits misunderstandings and ignorance of important scholarship. Benedict's thinking appears to have been that through his consideration of individuals involved in the plots, he can discuss Calvin and the possibility of, and character of, his involvement. Through this, he can move on to a reassessment of Calvin. Benedict does a marvellous job of exploring possible connections (direct and indirect) which Calvin may have had with these plots, identifying close friends of his as well as acquaintances who were involved, outlining their movements carefully, and so forth. But even granting this, we must say Benedict simply did not do the work required to pursue this larger aim.

First, Benedict exhibits a lack of awareness of relevant scholarship. He analyses a particular line of scholarship that focuses on the plots themselves. He considers works by Amédée Roget, Émile Doumergue, Lucien Romier, Henri Naef, Robert Kingdon, Alain Dufour, N.M. Sutherland, Arlette Jouanna, Hugues Daussy, and many others. These are brilliantly and meticulously analysed in chronological order. Yet, Benedict himself (rightly) identifies a second scholarly trajectory to which he must attend, namely, that of Calvin specialists. His lack of familiarity with this trajectory is surprising (I might even say, shocking), particularly for a scholar of his brilliance. He mentions biographies by Bruce Gordon and Yves Krumenacker and Matthew Tuininga's 2017 monograph on Calvin's political thought and discusses specifically their unfamiliarity with an article by Alain Dufour—on which more momentarily. He then briefly discusses “the most prominent contemporary writer who self-identifies as a Calvinist, Marilynne Robinson.” (p. 3) Incidentally, the fact that Robinson self-identifies as a Calvinist seems an odd thing to raise when discussing her scholarship. But significantly, there is no mention of J.T. McNeill, Marc-Edouard Chenevière, Josef Bohatec, Quentin Skinner, Michael Walzer, Heiko Oberman, Carlos Eire, Ralph Hancock, Harro Höpfl, John Witte, Jr. or several other scholars who will be discussed in the next two paragraphs.

Second, he embraces erroneous views. He seems convinced that all Calvin scholars believe Calvin to have been wholly opposed to active resistance. Benedict summarizes the views of Marilynne Robinson to this effect and seems to feel his case is closed. This is far from true. One work which Benedict (curiously) *does* mention late in the volume but which *does not* seem

to have forced him to rethink his assessment of scholarship is a chapter by Willem Nijenhuis published in 1994 which argues that Calvin wrestled in the last years of his life with the question of active resistance engaged in by individuals. And there are other studies, which Benedict does not mention, which set down positions similar to, and even stronger than, that of Nijenhuis. Among these are the following: in 1998, Max Engammare explored the possible links between Calvin and the monarchomachs. Robert Kingdon published two papers, in 2000 and 2006, which seek to identify the moment of Calvin's conversion to supporting active resistance, which Kingdon argues can be dated quite precisely. And in a Brill companion on the Reformation (2009), David Whitford argued that Calvin's additions to the last chapter of the 1559 *Institutio* represent a defense of the rights of individuals to rise up against civil authority.

The upshot of this unfamiliarity with Calvin scholarship, third, is that Benedict unfortunately works towards conclusions ignorant of the fact that they have already been demonstrated. In *John Calvin as Sixteenth-Century Prophet* (Oxford, 2014), I argued that Calvin, working with Theodore Beza, François Hotman, and other Genevans, was seeking, from at least 1556, to instigate an uprising led by a prince of the blood which aimed to place him on the throne or, at least, to remove the Guises and place Reformed councillors around the king. (I also summarized my argument in a 2015 book chapter in a festschrift published by Bloomsbury). I argued that scholars have downplayed Calvin's interest in the Amboise conspiracy and have ignored the findings of Alain Dufour on the affaire de Maligny (see Alain Dufour's 1963 article, "L'affaire de Maligny (Lyon, 4-5 septembre 1560) vue à travers la correspondance de Calvin et de Bèze," *Cahiers d'Histoire* 8 (1963), pp. 269-280 in which Dufour convincingly shows that Calvin and Beza involved themselves in a coup plot). I showed that Calvin and other Genevans engaged in a number of activities expressly designed to work towards this aim. This included training ministers and sending them into France to serve Huguenot churches with the specific aim of readying French believers for the time when a prince of the blood had been found; urging Antoine of Navarre, (who *was* a prince of the blood) to insert himself into the struggle towards freeing France from Catholic tyranny, and other actions. Following the death of Henry II in July 1559, they redouble their efforts to remove the new king, fifteen-year-old Francis II, leading to their involvement in the Maligny plot and eventually the first French civil war. Benedict's approach to these matters is more exploratory. Nonetheless, he sets out a case which he believes is novel. Benedict does not go as far as I do on explicitly aligning Calvin with the first war of religion. Thus, Benedict may well have sought to push back somewhat against these conclusions. But he was unaware of them.

The impact of these oversights is felt throughout the work. Benedict treats his own findings concerning Calvin as if they served to push open a door which was nudged by Dufour in 1963 (and perhaps before that by Amédée Roget in the late nineteenth century (specifically 1870-1883)) but has effectively remained closed to modern Reformation research due to its ignorance of these older writings. Had Benedict known Calvin scholarship better, he would have known this was not true. As stated earlier, the work Benedict has done on the plots themselves is absolutely outstanding. Thus, there is much to appreciate in this monograph. But as a study which aims to reassess Calvin, Beza, and the Genevans, the work lacks the depth needed to make it a useful piece of scholarship.

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