



Partecipazione e Conflitto
* *The Open Journal of Sociopolitical Studies*
<http://siba-ese.unisalento.it/index.php/paco>
ISSN: 1972-7623 (print version)
ISSN: 2035-6609 (electronic version)
PACO, Issue 12(2) 2019: 590-592
DOI: 10.1285/i20356609v12i2p590

Published in July 15, 2019

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BOOK REVIEWS

Adria K. Lawrence (2013), *Imperial Rule and the Politics of Nationalism: Anti-Colonial Protest in the French Empire*, Cambridge: Cambridge University Press, 2013. ISBN 9-781-10703709-0 (hardback), £57, ISBN 9-781-10764075-7 (paperback), £20.99, 2013, xx + 298.

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What caused separatist mobilizations in the French colonies of the 20th Century? In *Imperial Rule and the Politics of Nationalism*, the argument is that political decisions made in France were pivotal. The central argument laid out in the book is that nationalist claims developed only after France rejected political demands that had been voiced at an earlier point in time. Whenever requests for political equality were accepted or accommodated in some way, assimilation into France followed, and no subsequent nationalist movement occurred in the colony. The argument stands in contrast to “teleological” explanations of nationalism, which view nationalist mobilization as inevitable. Lawrence's book is in many respects superb, especially the detail of the case studies and the shifts in scale from empirical observations to theoretical propositions. Chapters 1 and 2 present a thorough theoretical and conceptual discussion of political equality and nationalist mobilization and use Morocco and Algeria to differentiate between the two mobilization types. Chapter 3 presents counterfactuals, asking what would have been different if France had granted political equality to Moroccans and Algerians in the 1930s, when the French National Assembly voted to enfranchise Muslims. Chapter

4 introduces different types of disruption to imperial authority, such as the invasion and occupation of the colonial territory, as an eventual trigger for nationalist mobilization. Chapter 5 tests the argument in Spanish and French Morocco, while the last chapter draws broader conclusions for decolonization studies. Lawrence does a great job in zooming in and out of her cases to present the micro- and macro-dynamics of her argument – that will be of inspiration for scholars working in the comparative historical analysis tradition. Despite these merits, however, there are a few shortcomings that deserve attention. In the following, I will raise one (1) theoretical, (2) methodological, and (3) empirical concern.

First, Lawrence's book centers around nationalism and national identity, but she shies away from an in-depth discussion of these concepts and their connection to mobilization. Her research puzzle is explicitly not the emergence of nationalism but nationalist *mobilization*. This distinction is problematic: it is hard to think how a national (group) identity can develop without any mobilization, or how nationalist mobilization can develop without nationalist social identities. In fact, politicized social identity is commonly seen as the single most important determinant of social movement participation. It is not clear to me how nationalist mobilization would not be preceded by the creation (or politicization) of a nationalist identity. Lawrence seems to take nationalist identity as given and as becoming politicized only if elites appeal to it after France rejected claims for political equality. But how was a nationalist identity created in the first place? Second, Lawrence presumes that the political equality movements she is comparing were similar, even though she is not discussing the origin and social composition of those movements. Instead, they are almost exclusively compared in terms of their similar claims (for political equality/independence). However, without showing that those movements were indeed similar in their origin, composition, and societal support, the reader might wonder if her cases were really all that similar.

Another problem that feeds into the same category of (potential) "experimental fallacy" is the non-randomness of the cases where nationalist mobilization does/doesn't occur. Only a few small island colonies, which were geographically and socioeconomically quite similar (and all very different from the negative cases), have been integrated into France. Therefore, her positive cases, which her argument ultimately hinges on, are quite particular in that sense. It would not be surprising if it were not so much the French response to political demands in these cases but other factors, such as the relatively small mobilization potential, that prevented nationalist movements from emerging.

Third, Lawrence presents Senegal as strong quasi-experimental evidence for her argument, but does not discuss all implications the particular case has. In 1848, four de-

partments in Senegal (Drakar, Rufisque; Gorée, and St. Louis), had been granted significantly more political rights than the rest of Senegal. Consistent with Lawrence's argument, these four departments did not have nationalist movements striving for independence, whereas the other part of Senegal had. Lawrence presents the case as strong within-country evidence, also because the French later portrayed the special status given to these four departments as random and a "mistake" (p. 110). However, during WWII, in 1940, the four departments in Senegal lost their special status for six years until independence in 1946. According to Lawrence's theory, we would expect that in those six years, nationalist movements developed in the four departments. If there was evidence that this had happened, the Senegalese case would considerably strengthen the book's argument, but in its absence the case leaves some questions unanswered.

To sum up, this book presents a well-written and well-structured comparative historical analysis, proposing an in large parts compelling argument. The book will prove useful for scholars working on social movements, nationalism, and decolonialization.