

David Lewis Tengwall, *The Portuguese Revolution (1640-1668). A European War of Freedom and Independence.* Lewiston, The Edwin Mellen Press, 2010, 482 pp. ISBN: 13: 978-0-7734-3614-5.

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Prudent readers know they should be wary of the first impression that may be created by a book's title. First because the author of a work is not always responsible for choosing the title—the policy of publishing houses normally manages to impose itself—and second, and most importantly, because even when authors are free to choose their own title, there is always some doubt as to whether they have successfully managed, with just a few magic words, to convey the dense and complex message that a book contains.

I believe that the work by David L. Tengwall, Professor and Chair of History and Political Science at Anne Arundel Community College in Arnold (Maryland, United States), fits into the second hypothesis. On innumerable occasions from the beginning to the end of the book, Tengwall repeatedly uses the enticing words that are found in the title: “revolution” (and “revolutionary”), “freedom” and “independence”—to which we might also add the word “nationalism”. So, there is no room for any confusion, since the reader is clearly informed about the thesis that the author defends: that the separation of the crown of Portugal from the Spanish Monarchy, achieved between 1640 and 1668, amounted to a revolution that can be considered equivalent to others that took place in seventeenth-century Europe, insofar as that process represented a radical change, not only because of the recovery of independence on the part of the Portuguese, but also because of the internal transformation that Portugal experienced with the new dynasty of the House of Bragança. The triumph of this revolution, which until today had remained hidden from the world of historiography, consisted of installing in Portugal a “limited monarchy” (143) of a participatory and constitutional nature: “Portugal emerged,” Tengwall concludes, “as a truly modern European state” (391).

The author is also clear about the theoretical bases (the readings and models) that led him to defend this thesis. However, the few authors that he quotes in the introduction in order to justify his concept of “revolution” and then apply it to the Portuguese

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revolution of 1640 cannot be taken very seriously, not because these names are unrepresentative in the world of political science, but because their claims relate to the contemporary age.

The effect that is caused is thus an unacceptable conceptual anachronism. We must therefore search in Tengwall's own professional career for the reasons why he is drawn to the model of Revolution. Nonetheless, this alternative procedure is difficult to perform because of his somewhat limited theoretical production, even despite his being a senior researcher: we know about his PhD thesis (completed in 1978), three articles and various papers presented at conferences that have not yet been published. The best clue would undoubtedly be his thesis, supervised by Francis Dutra—who wrote the preface to this book—a professor at California University and a specialist in the Portuguese Military Orders. The thesis had the title of *The Portuguese Military in the Seventeenth Century*, but it was never published, which makes any value judgment rather difficult.

Nonetheless, because of the date when it was completed—1978—we can form an idea as to the main historiographic currents on which he based his writing at that time, and which continue to inform his view of the problems: the theory of the general crisis of the seventeenth century, which was formed in the 1940s, but continued to be adhered to in the 1970s, and the model of a *military revolution*, which appeared in the 1950s and still shows signs of good health even today.

In this way, it can be understood that Tengwall's work is not, as one might think, a provocation in the light of the enormous developments that took place in studies of Modern Portugal after 1980, when the author finished his thesis, but simply an antiquated and rather unprofessional book that one reads with a mixture of surprise and irritation. The surprise is caused by the concepts on which he bases his interpretation of the “restoration”, some of which have already been pointed out: “nationalism”, “national revolution”, “national spirit”—deriving from *volkegeist*, a term that the author quotes literally in order to apply it to the Portuguese of the sixteenth and seventeenth centuries (8 and 200)—“revolutionary leader” (used to refer to the king Dom João IV, 107), “military revolution”—which, in turn, would have given rise to a rather eye-catching “Portuguese Revolutionary Army”—or “freedom”, a word that, when used by an American professor of Political Science might be expected to appear in a specific context and not be bandied around with such a generic meaning.

The specialist reader immediately knows that Portugal in 1640 was different from the United States in 1776, but, once again, the dangers of anachronism dominate the book,

beginning with its actual title. Because, through its association with the term “independence”, the word “freedom” leads to the serious risk of our interpreting a rupture from the seventeenth century, influenced by the political culture of the Baroque, as a colonial rebellion at the end of the 18th century, which was largely the fruit of the Enlightenment. The problem lies in the fact that this seems to be Tengwall’s intention, no matter how frequently he places the terms between inverted commas. Unfortunately, in historiography, punctuation marks are not sufficient to characterize scientific concepts; on the contrary, these require a complementary intellectual effort that one does not find in this book.

Nor can one blame the work’s failings on the choice of models already mentioned—the general crisis of the seventeenth century and the military revolution. Some of the points of view stated in these models may have been surpassed in recent times, but there is no doubt that both models have evolved in their respective research fields with quite remarkable results. What is surprising about Tengwall is his continuing loyalty to the first manifestations of these currents of thought, as well as the rigidity with which he incorporates his knowledge in the field of Contemporary Political Science, which, if considered as a comparative reference for the modern age, could also have helped him to become aware of the great conceptual distance that exists between the present-day world and the Portugal of the Bragança dynasty. Thus, the problem is not only, nor even mainly, that of the method chosen to define a “revolution”, but instead that he did not even accept that this word might have had other possible meanings in the seventeenth century.

The first aspect is a serious one and immediately directs our attention to the introduction, where the author tries to convince us that the four necessary requirements for cataloguing an event such as a revolution—the existence of a stable society and economy, the outbreak of occasional deep crises, the ineptitude of the government and the alienation of the “intelligentsia” (4–8)—were the predominant fare in Portugal on the eve of 1640. But the second aspect is even more alarming. And it is particularly surprising in the sense that the use of the word “revolution” in the contemporary documents at the time of the Restoration invites us to investigate what our ancestors actually meant by it. If he had followed this path, the author would inevitably have discovered another type of “revolution” (which, in my view, would have been much more interesting), such as the one that Olivares tried to bring to bear in Portugal with his reforms. In fact, “the heroes of 1640” were so precisely because they attained their objective: to prevent the revolutionary policy of the Count-Duke from achieving its goal. But Tengwall seems to reject everything

that does not fit into his single class of “revolution”, such as, for example, when he refuses to see the Portuguese 1640 as a *coup d'état* promoted by an elite that had no real effects on the country—or, in other words, it did not provoke a revolution. Tengwall’s lack of interest in its complexity—and his reluctance to describe the Restoration as (shall we say?) a *conservative* or traditionalist movement—inevitably filters through to the reader, who finds himself faced with a closed and biased account, running from the very first chapter in the book to the twelfth and last one, without finding any room for stimulus or for other hypotheses.

This determinism is what causes our initial feeling of surprise to change to one of general irritation. The lack of *possible* unforced models for analyzing a European revolution in the seventeenth century represents only the first warning that there are other distortions affecting the proposed chronology, discourse categories, sources, bibliography, absences and writing.

Thus, although the years that appear in the title refer to the period of 1640-1668, the book does, however, begin in 1578 with the dynastic crisis that was opened up by the death of the king Dom Sebastião, after which we are presented with the reigns of Filipe II and Filipe III (without any mention of the twelve-year truce of 1609, of capital importance for the presentation of his reign) and Filipe IV, until we arrive at 1 December 1640 (“The Spanish Captivity”).

This beginning is correct and, furthermore, necessary since the political process that historiography knows as the “Portugal of the Filipes” forms a *continuum* without whose horizon any argument loses its coherence. But, seen from this perspective, it isn’t possible to close this process with the signing of the Peace Treaty in 1668. Without paying at least minimum attention to the experiences of the generation that came after this treaty, one runs the risk of considering the end of the war as the end of the Restoration, when, in reality, the two processes were different, although they were related. Undoubtedly, the war ended in February 1668, but the Restoration—i.e. the consolidation of the new political regime installed in 1640—did not.

Tengwall, who could have made use of more colorful categories in order to improve his argument sadly did not do so. On the contrary, what he gives us is a dual combat between “Spain” and the “Spanish” and “Portugal” and the “Portuguese”, and, even worse, he does not distinguish “nation” and “love for one’s country”—which were typical cultural and emotional concepts of the seventeenth century—from the “nationalism” of the nineteenth and twentieth centuries—which is a political theory.

Because at this moment we know that the cultural differences between peoples and the identities *as a nation* that these differences generated—such as the ones that naturally existed between Castilians and Portuguese—did not necessarily lead people to consider belonging to the Habsburg monarchy as a tragedy (as a “captivity” from which it was important to free oneself). At least not everyone thought in this way. The author presents the Portuguese nation without the slightest chink in its armor, as a bloc, especially in the countless pages devoted to military operations—where his training as a military historian is clearly shown in the comfort that he feels whenever he is explaining tactics and battles.

In this book, not one single word is devoted to the attractive phenomenon of exile that was widely practiced by a very important sector of the aristocracy and the Portuguese nobility in Madrid during the Restoration. Nor is there any mention of possible shared identities—the Portuguese of the seventeenth century might feel a certain revulsion for the Castilians, but they could also proudly consider themselves to be *Spanish*, i.e. sons of Hispania. In the case of “Spain”, it is even worse, since its presence in the work is really an absence, amounting to nothing more than a fuzzy caricature without any distinguishing features, being just something to be roughly described as an imperial power without any other plan than to dominate the periphery. Given that Tengwall accepts the point of view of the *Restorers* of 1640, he does not bother to waste any time exploring the nature of that highly complex Hispanic monarchy, nor to reflect on the constitutional culture on which it was based, and in which the Crown of Portugal—and not Portugal—represented just one of the pieces, alongside others that were similarly filled with tradition (and conflict). We can therefore deduce that the anachronistic model of the “nation-state” followed by Tengwall was the reason why the Habsburg monarchy was reduced to a similarly anachronistic “Spain”, in the same way that this premise allows us to understand why the author erased Overseas Portugal from his physical and conceptual map; except for a few accidental references to Brazil, neither this territory nor the *State of India*, nor any other point of the Portuguese Empire appear in the book. The consequences that arise from this unfortunate amputation are quite evident.

Taking into account the rigidity of the author’s thesis, his method and the categories that he uses, it is easy to understand that there is little to be gained by referring here to the sources and bibliography on which Tengwall constructed his argument. Naturally, there is abundant information of a documentary and bibliographical nature (the latter group of data being much more sizeable than the former), but one is left with the impression that these references were used with the aim of demonstrating erudition rather

than coherence. The fact that the author did not consult any Spanish archive is a negative sign that needs no further comment. I draw attention to the fact that, in the prologue to the book, Dutra quite rightly criticizes the abusive way in which historians dealing with the 1640 revolution tend to quote the Count of Ericeira's *Portugal Restaurado*, particularly because the reader is shortly to discover that Tengwall is no exception. Some footnotes are so disproportionately long that they interrupt the rhythm of our reading. On other occasions, the references tend to pile up without any critical explanation being provided. This leads to another of the more serious problems that affect *The Portuguese Revolution*: sometimes we don't know whether we are reading a monograph on the Restoration or a History textbook on the sixteenth and seventeenth centuries. This strange and uncomfortable mixture of a monograph and textbook directs the reader's attention towards subordinate analyses, and, in many cases, takes him on an exhausting journey into the world of uncertain erudition—I say “uncertain” because I have the impression that Tengwall does not have a very solid grasp of Portuguese, and, even more seriously, he quotes works without having read or completely understood them.

This is not the first time—nor will it be the last—that a historian has confused the narrative method with the chance to tell us everything that he knows. Narrating history consists in explaining a complex process with a chronological order, but also stopping at opportune moments in order to make analyses. Although this is a technique, there is no denying that it also requires a certain amount of *art*, in other words making endless revisions and corrections before considering a text to be definitive.

Tengwall does not seem to have devoted to his work the amount of time that such a crucial theme of European modern history required, resulting in some lamentable errors caused by carelessness.

There are abundant mistakes to be noted in the works quoted in the notes and in the final bibliography. The numbering of the footnotes between pages 36 and 45 is wrong. The map of the provinces of Portugal in the seventeenth century is repeated (it appears twice, on page 183 and on page 238). Like most Portuguese and Iberian kings, Dom João IV was not, strictly speaking, “crowned”, but instead acclaimed (136–40). Edgar Prestage was not a “noted Portuguese historian” (141), but a British one—although he did live in Portugal and was certainly a well-known author. Finally, Dom Luis Méndez de Haro, Filipe IV's favorite, who replaced Olivares, was never called “de la Paz” (338). These are all slips (and there are many more) that could easily have been avoided. If I highlight them here, it's

not because they are exceptional in the editorial world, but because the price of this book—over 100 dollars—converts them into a lack of consideration for the reader.

The appearance of a text in English on the Portuguese Restoration should always be a reason for rejoicing among the specialists writing on the subject. Not only does it help to spread interest in this fascinating chapter in European history, but it also makes it possible to disseminate the results obtained in this historiographic area among the widest possible audience. However, sadly, what *The Portuguese Revolution* gives rise to is a feeling of great concern, because of the serious insufficiencies that have been pointed out, and above all because of how unrepresentative this book is of the progress that has been made in relation to the *Portugal of the Habsburgs* in the last two decades.

I am seriously afraid that the appearance of this book in English will not help to win over new fans to the scientific study of the Restoration, but that it may in fact help us to lose them. This is a dreadful pity, because (and in this regard I am in full agreement with Tengwall) Portugal deserves to occupy a prominent place in comparative historiography in general, and in European historiography in particular, and it is disheartening to notice more and more that this has not happened.

But I am also convinced that a work such as *The Portuguese Revolution* will not contribute towards Portugal being taken into greater account in the circuits of European and world historiography; on the contrary, this is one of the ways in which it will be expelled from them.