
REVIEW ARTICLES

*The Thirty Years War and its Place in the General Crisis of the Seventeenth Century**

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I would like to preface this review article with two quotations that represent for me the germ of two fundamentally useful notions which have been developed so successfully by Wallerstein and Braudel with regard to understanding the cohesion and unity between economic and social change in world economies from feudal to capitalist times.¹ Both quotations are from the early 1960s. The first is by Hoszowski, and it contains something of the later notion of the West developing underdevelopment in seventeenth century East Europe. The second is admittedly by Polisenky and from 1971 in the English version, but it refers to Topolski's work from 1962. It contains the germ of the triad — centre, semi-periphery and periphery — so well established by Wallerstein.

Undoubtedly in the XVth and XVIth centuries the possibility of exporting grain abroad encouraged the rapid development of agriculture, activated the money economy, influenced the development of the local markets and raised the standard of living of the gentry class, which in its turn affected the culture and political life of the whole country. These positive effects, however, were counter-

* A review of J. V. POLISENSKY: *War and Society in Europe 1618-1648*, Cambridge University Press, London, 1978.

¹ Cf. IMMANUEL WALLERSTEIN: *The Modern World System. Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century*, New York, 1974; his work includes a massive and useful bibliography, yet it excludes references to Polisenky's scholarship. FERNAND BRAUDEL: *Afterthoughts on Material Civilisation and Capitalism*, Baltimore, 1977; WITOLD KULA: *An Economic Theory of the Feudal System. Towards a Model of the Polish Economy 1500-1800* (1962), London 1976.

balanced by social and political conditions in the country. Having won a dominant position over the other estates, the gentry exploited the possibilities of economic development resulting from their conditions with the European market as a whole in order to set up a system of farming based on serf labour, and to weaken both economically and socially the position of the townspeople, although by doing so they set in motion some factors leading to economic regression. Foreign trade became subordinated to the interests of the feudal class. It had little influence on the growth of a money economy in Poland, and the formation of a home market. It did nothing to awaken the process of capitalisation in the country, and did nothing to develop the middle class. It only enriched and strengthened the burghers of Gdansk and the merchants of Royal Prussia. Thus the influence of the extensive trade of Gdansk on the economy of Poland was one-sided. It strengthened the gentry, and supported only agriculture; that is, it prolonged and maintained the basic foundation of feudalism. The trade of Gdansk was of great importance to the countries of Western Europe... The Gdansk trade encouraged the development of industry in these countries, hastened the process of urbanisation in them, and aided their evolution towards capitalism.²

Topolski stresses rather the disproportions which already existed in social, economic and other spheres within individual countries, and which divided Europe into three camps: firstly, the lands with the greatest economic dynamic where the dissolution of feudal relations had proceeded furthest — England and the Netherlands; secondly, those where neither stagnation nor regression was observable — France, Scandinavia, Germany and the other countries of central and eastern Europe, including Bohemia but not Poland; and finally the areas of regression — Spain, Portugal, Italy and Poland...³

Professor J.V. Polisensky of the Charles University at Prague has done more than possibly anyone to make accessible in western languages his own as well as his Czech colleagues' researches into the history of central Europe in the era of the Thirty Years' War. He is one of the original members of the now classic seventeenth century crisis debate of the 1950s, and on this problem he has published more articles, books and collections of documents in more European languages than anyone. His editorial collective produces the *Documenta Bohemica Bellum Tricennale Illustrantia*. Behind this documentation lies the first exhaustive survey of all archives both public and private that have remained within the present day boundaries of the Czechoslovak state concerning the causes and consequences of the Bohemian Estates' rebellion against the Habsburgs in 1618-20. To English readers Polisensky has achieved two major things. He has put Bohemia and Moravia at the centre of the Thirty Years' War, and he has

² STANISLAW HOSZOWSKI: 'The Polish Trade in the 15th-18th Centuries', *Poland at the XIth International Congress of Historical Sciences in Stockholm*, Warsaw, 1960, pp. 149-50.

³ J. V. POLISENSKY: *The Thirty Years' War*, London, 1971, p. 6. J. TOPOLSKI: 'La régression économique en Pologne', *Acta Poloniae Historica*, 7, 1962, pp. 28-49, developed into the concept of a stalled economic crisis of the seventeenth century in Europe.

explored the impact among members of the ruling classes of the reporting of newsworthy events showing how this influenced their further diplomacy and warfare in the policies and politics of courts, administrations, nobilities and city councils of the day. Polisensky has sought to link the history of crisis with the history of events by reconstructing the atmosphere in which the important decision-making processes of the times took place from the records often of private correspondence and of *fiscalia* deposited by those in power as landlords, officers and overlords. He has based himself firmly on the archives of Czechoslovakia and has combined it with the most comprehensive critique of the east and west European literature on the Thirty Years' War that is currently available. The fruits of this are presented to English readers in two books: *The Thirty Years' War* (Batsford, 1971) and *War and Society in Europe 1618-1648* (C.U.P., 1978). Yet the results of his work are extraordinarily difficult to assess. Why should this be the case? His latest book highlights the problem.

War and Society in Europe contains three basic types of historical writing awkwardly knitted together, as the author admits, chapter by chapter. They are archive surveys, the state of knowledge from the monographic literature, and finally his own new studies in depth, carefully chosen to encourage further ways of examining specific issues in order to increase our understanding of the interconnectedness of government, governed, economy and society in this period of general crisis. Above all he seeks closer examination of the European links between leading nobles, administrators, officers and agents creating an informed, active public opinion at the time. The nobility as a class are given great prominence, with Bohemia and Moravia consciously at the cross-roads between the new northwest European commercial, manufacturing system of the Dutch, soon to be taken over by the British, and the older Spanish Habsburg feudal autocracy of the south and centre of Europe, which was to be redeveloped by the absolutist French. What is missing here is an exploration of the links between Polisensky's system and those of Braudel's Mediterranean world generally, and especially of Immanuel Wallerstein: *The Modern World System* (I, 1974). Indeed, the great weakness of Polisensky's general framework is that it is not up-to-date. Although he has made good use of the work of the older generation, his peers ranging from Christopher Hill to John Elliott, he has not given enough space to the work respectively of Wallerstein and of Geoffrey Parker. Perhaps the manuscript in its present printed version has been with the publishers for some years before being published.

To take Wallerstein first. Basing himself on Braudel and the 'Annales' school of French social history, combined with the work of the Polish Marxist school of economic history, Wallerstein has applied the notions of 'centre', 'semi-periphery' and 'periphery' in explaining the development of the home and overseas European economy in the sixteenth and early seventeenth century.⁴

⁴ Most succinctly in I. WALLERSTEIN: 'The rise and future demise of the world capitalist system', *Comparative Studies in History and Society*, 16, 1974, pp. 387-415; and

As the 'centre' shifted from the Mediterranean towards the North Sea, so it brought with it readjustments to the west-central European 'semi-periphery' and to the eastcentral European 'periphery'. This might explain why it was economically expedient for the Dutch not to encourage the further development of the Bohemian economy especially in textiles and manufactures, since it would rival their own activities in markets of mutual access by sea or river such as those along the Vistula, Elbe, Oder and Baltic, and also set up unwelcome rivalry for raw materials coming the other way. To let the Habsburgs destroy the efficient estate management, urbanisation and putative industrial production taking place on the lands of leading members of the Bohemian Estates might well have suited Amsterdam economically even if it did not satisfy the Hague in terms of its religious and military-naval problems of defence and aggression with the Spaniards nearer at home in Brussels. For, to apply Wallerstein's generalisations, one might say that between 1620-50 Bohemia and Moravia joined the Austrian Habsburg world of 'semi-periphery' and 'periphery', producing by means of extensive, low-technology agriculture, the foods and raw materials needed for the more highly developed 'centre' in the west. The 'ideological' split between protestant commercial northwest and catholic feudal south and east-centre would then be seen to have been no 'split' at all but rather an abandonment consciously of rebel protestant Bohemian nobles and, more crucially, an abandonment unconsciously of the progressive nobles', burghers' and peasants' political and economic situation in Bohemia and Moravia as elsewhere in east-central Europe. For west Europe needed the 'colonial' produce of its immediate hinterlands in east-central Europe, obtained by financing the existing aristocratic-feudal order there. Why should the Dutch and British therefore have risked a major conflict to stop the refeudalisation of Bohemia by the Habsburgs both *before* and after 1620, where it was in their own immediate economic interest to let it happen?

The other major line of inquiry that Polisensky starts but does not fully explore is the reconstruction of life within occupation, community, family and region at actual points of 'high' and 'low' crisis, although the book is bursting with useful archive information for just such a purpose. This approach has been heralded for the military system by Geoffrey Parker: *The Army of Flanders and the Spanish Road* (C.U.P., 1972). We would expect Polisensky

four vital articles by Marian Malowist, in *Past and Present*, No. 13, 1958, pp. 26-39; *Economic History Review*, Second Series, 12/ii, 1959, pp. 177-89; *Economic History Review*, Second Series, 19/i, 1966, pp. 15-28; but above all his 'A certain trade technique in the Baltic Countries in the 15th-17th centuries', *Poland at the XIth International Congress of Historical Sciences in Stockholm*, Warsaw, 1960, pp. 103-116. This deals with Hanseatic and Dutch financed buying up of Polish Vistula grain and timber in advance via the Gdansk middlemen and the Polish gentry which led to price rises at home and abroad, with increased serfdom and impoverishment of towns and crafts inside Poland: a classic case of the West developing underdevelopment in the East.

and his researchers to do the same for the armies of central Europe. The links between archive positivism and economic and social anthropology must be established more firmly by historians in the next few years. Are the rent-rolls and military papers of men like Wallenstein and Piccolomini, which Polisensky lists as now accessible in the Czech archives (even giving a good map of their present location), going to be used to reconstruct the relationship between ordinary people living through the crisis of the Thirty Years' War in their homes and cottages in town, suburb, market and village, and the military system that readapted their daily routine to the new physical and social needs of survival? For example, the methods of Alan Macfarlane: *Reconstructing historical communities* (C.U.P., 1977) need to be applied to settlements in Friedland before, during and after the period of Wallenstein, 1625-34, using the lists of archives given by Polisensky. For the generalisations to be gleaned from the micro-analysis of historical anthropology are the way towards opening a long overdue dialogue between Rankean archival positivism and the sociology of everyday life. The causes and consequences of the Thirty Years' War in Bohemia would provide the ideal place for such studies, precisely because of Polisensky's pioneering work, collecting up the material evidence and presenting it to a wide academic public. What is still missing is a sensible framework, structure, language and grid of question to organise and deepen our knowledge of the people who lived in that time and space as persons and parts of groups, which will be so seminal to an understanding of behaviour in terms of ideology, economy, morality and society.

What are the great insights in Polisensky's book? They are nearly all elaborations from his earlier work published in English in 1971, and by far the best analysis to appear on the Thirty Years' War. The new book must be read in conjunction with it. No one should expect to grasp at once what Polisensky's overall view is, since it is almost as complex as the subject matter itself. We are dealing with a highly original, individualistic scholar who honestly takes the reader into his confidence by laying bare the materials and methods behind each step in his own thought process in the production of his work. This method can be alienating unless the reader is fully conscious at all times of the importance of subject and author. But it is disappointing that Polisensky seems not to be more appreciative of the Braudel school, although it might help to explain his admiration for academics like Mousnier, Trevor-Roper and Namier.

Is Polisensky placing the revolt of the Bohemian Estates on a par with the earlier Dutch revolt and the later English parliamentary revolution? He affirms that the Bohemian uprising was squashed between

'a politico-military conflict between two models of politics and society, represented respectively by Spain and the United Netherlands' (p. 77).

Quite so. But the real problem is surely to rediscuss this in terms of the political language of the period, thereby reassessing what was meant by religious

war and by all forms of propaganda and rebellion at the time. Polisensky is of the generation that perhaps too readily secularizes just because the people making the decisions took them in a pragmatic and Machiavellian manner. The more that occurred, the more religion and propaganda were needed to raise morale at home, denigrate enemies and rivals and coax and coerce subjects into action. Not without good reason does the modern world of news, newspapers and public opinion originate in the internecine European conflicts of the late sixteenth and early seventeenth centuries. Polisensky should perhaps modify the non-religious approach to understanding the Thirty Years' War in the light of what was *believed*. The study of popular religious and propagandistic media, including pulpit, sermon and confessional in the era of the Thirty Years' War as part of the mainstream of the general history of the period is long overdue. It should prevent one from being thrown back merely into the diplomatic-military and regional-nationalistic events and sentiments of the period. The thesis-antithesis of marxist and *annaliste* or Braudellian structures will broaden this traditional history-making by using the evidence sensibly and sceptically to qualify and quantify the popular and high culture, the propaganda — from whom to whom? and back again — and the mentalities within social-psychological and economic-geographical time and space of the people who lived through the period. This does not mean, for example, producing a history of the Thirty Years' War without Wallenstein, nor indeed yet another exclusive biography of the great man.

By concentrating so hard on the Bohemian question Polisensky is in danger of losing sight of the main problem, that the causes of the Thirty Years' War lie primarily in the religious dissent of central Europe generally, and as it worked itself out in the federal German politics of the Holy Roman Empire specifically since the sixteenth century. Bohemia was the crown jewel and prize of the dynastic and religious rivalries within the Holy Roman Empire as a whole, and this went back to at least the reign of Charles IV in the mid and later fourteenth century. Once the civil war of 1618 was under way, wider European issues began increasingly to assert themselves over and above the merely 'German' and dynastic-territorial federal political consensus. That does not mean to say that Bohemia was 'German' any more than that the Thirty Years' War was solely 'Bohemian' or Czech. A glance at the historical map will show crudely that the major, recurring field of hostilities was the Holy Roman Empire, that is the lands of the crowns of Germany and of Bohemia in equal measure. And the constitutional point of 1648 was that Westphalia recreated the federal system of the Holy Roman Empire as it had evolved under the Habsburgs with rescripts and executive ordinances since the later fifteenth century as regards law, order, economy and finance and defence. That does not mean to say that the immediately recognisable causes of the Thirty Years' War were to be found anywhere else than in the policies of all the territories comprising the 'German' federal system of empire generally, and in the politics of Bohemia

specifically — led and misled by the Habsburgs of the day. A concern with the importance of the parts played by other regions of Europe in escalating this conflict has to take the primacy of 'German' federal-dynastic politics into account as the basis of activity at least up to the peace of Prague in 1635. If the Thirty Years' War eventually caused an *ad hoc* European balance of power after 1648, then it was something that had first and foremost to be worked out from *within* the brotherhood of territories that existed *within* the Holy Roman Empire, however weak its federal system of politics at the centre. For it was precisely that looseness at the centre which was of greatest advantage in leading to the wider development of international diplomacy and concord in the seventeenth century world at large. Furthermore, exhaustive examination of one important part of this loose, federal empire can not preclude similar examination of all the other parts. Polisensky only begins to lead the way. Bohemia may indeed have been the tail wagging the dog in 1618, but the whole animal was the Holy Roman Empire.

The most useful parts of *War and Society in Europe* include the very succinct narrative of the Bohemian War, 1618-20 (pp. 53-65), followed by an explanation of the extraordinary Danish campaign in Silesia and Moravia, 1626-7, when it was hoped to attack the Viennese Habsburgs from the Slovak-Transylvanian bases of Bethlen Gabor (pp. 105-121). There follows what is now the liveliest available thumbnail sketch of Wallenstein (pp. 130-154). It ends in highly emotive writing especially as regards Wallenstein's treason and the role of the Habsburgs in Bohemian economy and society. In all this concentration on Bohemia the great importance of the Spaniards is stressed in a new way. Unfortunately the equal importance of the Bavarians, Tilly and the Catholic League is still not given the coverage that it deserves. Chapter 6 is entitled 'The Swedish-French Period, 1635-43' (note the incorrect running heads on even page numbers 156 to 178), but in fact the substantive subsection deals neither with the Swedes nor the French (here the author no doubt *intends* to jolt the reader) but with the attitudes in the 1630s and '40s of men like Piccolomini's English royalist agents to the deepening parliamentary crisis derived from the failures of Stuart foreign policy in the 1620s (pp. 163-179). Polisensky demonstrates the importance of exile and the cross currents of counter-exile on a European scale by contrasting Bohemian protestants in England with English royalists in continental Europe. The question of 'Exile' is a much neglected field in the study of early modern history. It could well be the clue to understanding the psychological factors that always elude the cruder tax-register and rent-roll centred approaches to the damage, population loss and war-horrors of the more traditional demographic, economic and social historians. But the possibilities of this remain implicit only. They are not fully stated in this chapter. It shows that every twist and turn in Polisensky's controversial writings must be the subject of careful discussion among early modernists.

Chapter 7 poses similar problems. Captioned 'War, revolution, peace

negotiations 1643-50', one would expect a survey that included the recent German work on the Peace of Westphalia and its diplomatic sources for indeed the only recent rivals to the *Documenta Bohemica Bellum Tricennale Illustrantia* (Prague, 1971ff) are the *Acta Pacis Westphalicae* (Münster, 1962ff). Instead, Polisensky chooses to examine the Masaniello rebellion and its aftermath in Naples, 1647-8, as described in eye-witness correspondence now in the Czech archives which kept 'Bohemian' noble families with Italian interests in the picture, above all the Piccolomini. It is a brilliant sketch, cutting right into our 'triad' of basic wars, basic rebellions, and basic empires. For Polisensky reconstructs the problem of what did people know, how did they come to find out, and how did the way in which they were told influence the action they took? This approach distils living, human history from the inanimate archives. Now, where do the *basic wars* — Spain and the Netherlands, France and Spain, The Holy Roman Empire and the Turks, Sweden and Poland, Sweden and Denmark, England and Holland — the *basic rebellions* — the Dutch revolt, the Bohemian Estates' and Palatinate affairs, the English parliament and republic, the Catalan, Portuguese and Neapolitan threats to the cohesion of Spanish Habsburg dynastic rule —; and the *basic empires* — declining Spain, rising France, Sweden, Britain, retrenching Holland, Denmark, Poland, and consolidating Austria — fit into the notions of exile and action for survival, personal gain and any collective progress of the period that Henry Kamen aptly called *The Iron Century*? What explains the insecurity and waywardness of the era as shown for example in the career of Arnim and Anhalt, or Mother Courage and Simplicius Simplicissimus? It is vividly outlined by Polisensky (pp. 190, 192) when eye-witness Filomarino writes on 12 July 1647 that the Tribune of the People of Naples, fish-porter Masaniello, had —

such authority and power to command and at the same time commands such respect and obedience that the whole city is under his spell. His orders are carried out by his followers with all precision and thoroughness. In a word, he has become king in this city, and moreover the most glorious one that ever ruled in the world. He wore nothing more than a shirt and trousers of white linen; in the manner of fishermen he went barefoot and bareheaded' (note the biblical undertone of this)

and when only *four days* later the mood has changed so drastically that, according once more to Filomarino, Masaniello had —

'lost his former quality of deliberation, his judgement and his temperance, and has acquired such a self-willed daring and tyranny that nobody could be sure of him, neither the people nor his followers; therefore this morning he was killed and his head cut off'.

Of course it was not quite that simple, and the moral economy of the Nea-

politan crowd has still to be worked into this description, as have also the variables which may determine seventeenth century thresholds of violence.⁵

Finally, Chapter 9 examines the changes that took place in the Bohemian nobility through the system of new creations, *incolat* and political confiscation practised by the Habsburgs at one time or another in the century from the reigns of Ferdinand I to Ferdinand III. Here we are told that

“ ‘ The Thirty Years’ War meant a ‘renewal’ for the nobility, chiefly German, which was of significance for all of eastern Europe in the seventeenth and eighteenth centuries. But to what extent did the ‘old nobility’ survive the Thirty Years’ War and how large was the role of the ‘new nobility’ in this period which was certainly one of ‘refeudalisation’? ” (p. 203)

The links with the Wallerstein thesis should now be taken up. Perhaps the entrepreneurs of the more rapidly developing west, based on Amsterdam, London and Paris were happy with this feudal reaction in east-central Europe. Indeed, they may have inexorably brought it about. Why should the west have supported economically advanced rivals such as the protestant noble and burgher entrepreneurs of Bohemia before 1620 in a merchantilist era when this could not have been seen as good for business? Polisensky shows that the ‘new nobility’ in post-1620 Bohemia were precisely those who ran the largest *latifundia*, a system of socially regressive, low-yield, bulk agricultural production for export at the expense of the consumer potential of the majority of the native inhabitants, who were now ‘enserfed’ more than ever before. The tragedy of the Thirty Years’ War is that it caused the colonialisation of east-central Europe by a west European imperialism that was a mixture of both commercial capitalistic and aristocratic-feudal elements. Polisensky makes the reader think for himself.

⁵ Cf. LOUISE TILLY: ‘The Food Riot as a Form of Political Conflict in France’, *Journal of Interdisciplinary History*, 2, 1971-2, pp. 23-57.

