

Reviews

Anastasia N. Karakasidou, *Fields of Wheat, Hills of Blood: Passages to Nationhood in Greek Macedonia, 1870–1990*. Chicago and London, The University of Chicago Press, 1997; ISBN 0–226–42493–6 (hbk); xxiii + 334 pp.; £39.95

This is a work on nation-building and the forging of national consciousness at a local level. It examines local notions of identity, their demographic and socio-economic origins, their construction and appropriation in the era of irredentist nationalism in the late nineteenth to early twentieth centuries, and, eventually, their transformation into a single, conceptualized (Greek) identity. Through an anthropological reconstruction of the history of Assiros (formerly Guvenza), a small township (*kinotita*) some 20 km north-east of Thessaloniki, the capital of Greek Macedonia, the author offers an engaging and vivid — if somewhat wordy — account of the nation-building process in the area from the late nineteenth century to date. In particular she seeks to record the ‘protracted process of socio-economic restratification and national homogenization’ (10) which altered the landscape of the area over time. Whatever the ethnic, economic, social and cultural makeup of Guvenza might have been before 1912–1913, following the region’s incorporation into the Greek state the township’s inhabitants gradually become *Greeks*, and ‘loyalty to the new state came to supersede other identities and loyalties’ (236).

Like many recent monographs on modern Greek historical ethnography, which reconstruct the shared experiences of people from below at a local level and their (voluntary and/or compulsory) partaking into nation-wide processes (see, for example, Janet Hart, *New Voices in the Nation: Women and the Greek Resistance, 1941–1964* [New York 1996]), *Fields of Wheat, Hills of Blood* constitutes an evocative attempt to look ‘subconsciously for the basis of [the author’s] own identity’ (xiii). Wanting to understand what made both her father, a Turkish-speaking Christian Orthodox refugee from Asia Minor, and her mother, the daughter of a Greek-speaking Thessaloniki merchant, *Greek*, Karakasidou began a doctorate at Columbia University on the interaction between ‘locals’ and ‘refugees’ in Assiros. Armed with the tools of her training as an anthropologist, she spent a considerable amount of time conducting interviews, recording popularly collected and disseminated oral histories as well as reading the local township archives.

Fields of Wheat, Hills of Blood, which derives from the author's doctoral dissertation, is divided into two parts. Part I examines in great detail the mostly unknown features of the fascinating tapestry that was Ottoman Guvenza. Here the focus is threefold: to unravel the social, economic and family ties which bonded and/or differentiated the Christian (largely Slavic-speaking) population of the township and upon which identities were formed and loyalties forged; to record how Greek and Bulgarian activists and agents sought to fashion their respective national consciousness among the locals; and to assess the effects that such efforts, particularly in the context of the so-called 'Macedonian Question', had on the social fabric of the community. The author's conclusions that the locals' 'distinctions . . . focused not on ethnicity but on socio-economic status', and that 'boundaries were permeable and . . . loyalties often shifted' (136) undermine the widely held view that at the turn of the century ethnicity was a prime marker of distinctiveness and difference among the Christian population of Ottoman Macedonia. (This view was first challenged by Basil C. Gounaris in his thoughtful article 'Social Cleavages and National "Awakening" in Ottoman Macedonia', *East European Quarterly*, Vol. 29 1996, No.4, [originally published in Greek in 1993], which, curiously, is not referred to by Karakasidou.)

Part II concentrates on how the old inhabitants of the township as well as the Greek Orthodox refugees from eastern Thrace who settled in Assiros in the early 1920s were integrated in the culture and polity of the post-1913 Greek state. The author points to the instrumental role played by the *tsorbadjidhes* (local notables) as powerful agents of state policies and state ideology at grass-roots level and as 'brokers of the local population's passage to nationhood' (216). Here, again, the emphasis is largely on socio-economic and cultural factors: the enactment of land reform in the late 1920s, the management of community affairs and finances, the changing character of the local pastoral economy from animal husbandry to stock-breeding, the celebration of rituals of collective identity, etc., and how these have affected the ways the locals conceive themselves as well as their relationships with others. Bringing her analysis up to-date, Karakasidou concludes that through 'new occupations, new channels of exchange, and new avenues of mobility . . . the Assiriotes are intimately immersed in the culture and economy of the Greek nation-state' (217).

Although in examining this protracted process of national homogenization the author makes insightful use of theoretical works on nationalism, ethnicity, nation-building and state-making, a couple of not particularly soundly documented views and oversimplifications,

numerous errors of fact and, more significantly, certain misinterpretations that occur cause grave concern. On page 162 we read that in the inter-war period 'issues of cultural assimilation and national homogenization rose to priority on Greek government agendas'. This may be so, but the evidence which the author offers to substantiate such a view relates primarily to Assiros. In the context of a monograph whose subject matter is the process of nation-building and state-formation, the absence of a detailed discussion of the 'Greek government's priorities' seems rather odd; and it is all the more telling when one considers that national homogenization is a process imposed from above. On the other hand, to contend that Greece 'might still have lost Macedonia when confronted by new Bulgarian aggression' during the Axis occupation of the country, if it had not been 'for the success of Greek nation-building efforts in the region' (200), is to make higher claims for the consequences of the hellenization of Greek Macedonia than would seem warranted by the evidence at hand (or indeed by the geopolitics of the region in the 1940s).

Moreover, when the author writes that the Greco-Turkish war in Asia Minor took place in 1922–1923 (141, 142, 144, 200); that Venizelos presided over the Lausanne Conference (145), and that in June 1935 an attempt was made on his life, following which an unsuccessful coup to topple the Antivenizelist government was staged by Plastiras (172); that the *evzoni* were Greek Independence fighters of 1821 (198); that King Constantine II was expelled from Greece in 1973 (216); that at 'a critical moment in Greece's Asia Minor campaign', Britain moved to support Kemal's government (287); that Metaxas died in 1940 (295); that in October 1940 the Italian government 'sought right of passage for its troops through Greece in order to invade Ethiopia' (302); or that the Greek civil war ended in 1947 (303) she repeatedly demonstrates that she lacks a sound historical background. But the gravity of the issue is thrown in high relief by at least three instances of misinterpretation. Let me briefly look at these.

1. A section of the book's introduction, entitled 'Looking-glass histories. Borders and scholars' begins with a long quote which the author subsequently refers to as an example of 'the rhetoric of primordial, essential characteristics of one nation over another' (27). The quote reads, partly, as follows: 'The Slavs have light-colored hair and blood; they have blue eyes. The Greeks have dark hair, fair color, and sparkling eyes. The Slavs are broad faced, fat, and somehow unrefined. The Greeks are flexible, clever, and fun-loving. . . . The Greek is active, fast in perception, capable, eloquent in speech, qualified in many tasks, and impatient for innovation. The Slav is

slow, and passive, and his interest is hardly triggered by anything which does not concern him immediately' (14). Citing an article in the Greek journal *Makedonika* (Vol. 2 [1941–1952], 329), Karakasidou wrongly attributes the quote to Theodoros Manousis, a native of Siatista in western Greek Macedonia and professor of history at the University of Athens in the 1840s and 1850s, instead of crediting it to the well known hellenist and professor at Harvard, Cornelius Felton (*Greece Ancient and Modern*, Boston 1867, Vol. 2, 313), as the author of the *Makedonika* piece clearly shows.

2. Maintaining that 'some Greek scholars have gone to considerable lengths to argue that there has never been a non-Greek Macedonian ethnic or national group' in Greek Macedonia (15), Karakasidou informs us in an endnote (264) that the historian and political scientist George Mavrogordatos is among the few scholars who have recognized the existence of a 'Slavo-Macedonian' ethnic group. She then goes on to say that in one of his articles Mavrogordatos has claimed that 'Jews, Turks and Slavo-Macedonians constituted the majority of the electorate in the national Greek elections of 1915 and 1929 [sic], and their votes played a key role in those results'. However, what Mavrogordatos writes ('The Schism as a Crisis of National Completion', in D. Tsoulos, ed., *Hellenism-Hellenikotita*, Athens 1983, 71 [in Greek]) is that in the elections of 1915 and 1920 the compact vote of these ethnic groups 'stifled the voice of the Greek majority in Macedonia', rather than the whole of Greece, as Karakasidou's wording seems to suggest.

3. On page 141 the author writes that in his *Short History of Modern Greece* (Cambridge 1979, 112) Richard Clogg has argued that 'by the time of the Greco-Turkish Asia Minor War of 1922–23 [sic], Venizelos had been "prepared to cede newly won territory in Macedonia to the Bulgarians, if necessary" in order to acquire parts of Asia Minor'. However, what Clogg in fact writes is that Venizelos had proposed the cession of eastern Greek Macedonia to Bulgaria in early 1915, so that the latter did not enter the war on the side of the Central Powers, in exchange for a recognition on the part of the Entente Powers of Greek claims over the Smyrna region. Not only has Karakasidou got the date wrong, but implicitly has attributed this error of fact to Clogg.

Perhaps what really casts strong doubts on the merits of the author's knowledge and use of the relevant historiographical output and her treatment of historical sources is the following bibliographical entry which appears on page 318: 'Tounda-Fergadi, Areti. 1986. *Greek-Bulgarian Minorities: Protocol of Citizen Kalvov*. Thessaloniki: Institute for Balkan Studies, No. 201. [in Greek]'. What is the unsus-

pecting reader to make of it? That sometime (the dates 1924–1925 which appear on the cover of the book are not included in the author's entry) a protocol was signed, entitled 'Citizen Kalvov', which pertained to the issue of Greek-Bulgarian minorities. The rendering into English of the word 'Politis' as a noun, rather as the surname of the Greek diplomat and representative at the League of Nations Nikolaos Politis, would seem to suggest that the author has not consulted Tounda-Fergadi's work. And if this the case, then why include it in the bibliography?

The above should not be regarded as merely pedantic or nit-picking criticisms. They raise serious concerns about the author's treatment of sources (written as well as oral), especially when juxtaposed with the statement that 'the anthropological reconstruction of local history presented in this work differs from the ways in which local respondents conceptualized their own historical past', and her frank admission that 'part of the problem lay, no doubt, in the different language that anthropologists often use to present their findings' (both on page 231). One can only hope that this truism has not been employed in the reading of the local township archives ('an important source of primary data on local history', xv), although given the author's predilection for excoriating 'national historiography [and its] long-standing bias in favor of written data' (32), such hopes are somewhat dented.

One also hopes that Professor Karakasidou's monograph will form the basis for a series of informed theoretical and empirical discussions and debates about the wider issues it raises, in terms of content as well as methodological approach. For, if nothing else, it illustrates the long path that both historians and anthropologists have to traverse before they can reach common ground, particularly as regards methodological issues and the treatment of historical sources. In her preface, the author mentions that she 'lived between history and anthropology' (xv). Although as a historian I believe that her attempt to expose 'ethnographic island(s) to the crosscurrents of history' (ibid.) has been undermined by an exclusively anthropological 'reading' of history, which in turn raises serious concerns about the soundness of her findings, *Fields of Wheat, Hills of Blood* constitutes a stimulating and engaging account of nation-building in Greek Macedonia.

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Reply to Carabott

Philip Carabott has graciously given my book the careful scrutiny that only a historian could offer, and I am very grateful to him for pointing out some errors, typos, and omissions. Yet while any oversight on my part is regrettable, I hardly feel that the points he raises detract significantly from the soundness of my general argument, theoretical reasoning, or empirical documentation, as he suggests.

Dr Carabott is critical of the material I use to substantiate my findings, arguing that it relates primarily to the village of Assiros. Although I agree that a detailed discussion of Greek government priorities would provide a helpful context for a study of nation-building and national homogenization, I have undertaken this task elsewhere. This particular study was framed by a concern for what anthropology might contribute to our understanding of such processes, and in such an effort local-level data are indispensable. Dr Carabott does not address that material at all, but instead questions the quality of my study and its validity by highlighting issues of contention relating to national events.

He charges that I 'repeatedly' demonstrate a lack of 'sound historical background,' pointing to what he considers several grave errors. He argues, for example, that I incorrectly attribute the end of the Greek civil war to the year 1947 (303, n. 21). That date is wrong, but it is a typographical error. Ironically, while he portrays this as an indication of my faulty sense of history, on the very same page, only four lines above this typo, in footnote 19 (as well as at two other places in the main text itself, 203 and 205), I clearly identify the end of the civil war as 1949.

There is, of course, no excuse for faulty proof-reading, a transgression that all too many of us are guilty of at one time or another. Indeed, Dr Carabott himself is not immune to such errors. He claims, for example, that on p. 198 (Ch. 7) I incorrectly identify the *evzoni* as War of Independence Fighters. In fact, there is no mention on p. 198 of either *evzoni* or Independence Fighters. Rather, I discuss the two on p. 199 — not to claim, as Dr Carabott asserts, that the *evzoni* are or were Greek Fighters, but rather to note that on national holidays school boys dress up as Greek War of Independence Fighters and they are called *evzoni*. Although a more judicious choice of wording may have averted such misunderstanding, this does not detract from the fact that, much earlier (Ch. 4, n. 28), I provide a clear definition of *evzones* as soldiers wearing white skirts.

Although he does not elaborate, Dr Carabott criticizes my reference to the Greco-Turkish War in Asia Minor, implying again that I have my dates (1922–3) wrong. Nevertheless, when most Greeks

talk about the 'Asia Minor Catastrophe' they generally refer to 'The Twenty-two' (*to ikosi dhio*) i.e., the time when the violence and suffering inflicted upon the civilian Christian Orthodox population peaked. If diplomatic historians mark the official start of that conflict in July 1921, with the short-lived Greek military offensive in Anatolia (that had stalled by September), rather than in 1922 with the Turkish counter-offensive (that followed eleven months of uneasy peace), then it is indeed important to set the record straight. But in terms of popular consciousness, a major focus of my book, the war is often ascribed the dates 1922–3. Why is it that most Greeks do not mention or remember the first year of the war? Instead of asking this question Dr Carabott maintains that my use of the dates is simply an erroneous statement of fact. His assertion, however, further misconstrues the broader (and more important) point worthy of further research.

Also without elaboration, Dr Carabott questions the notion that Britain moved to support the Kemal government at a key juncture in the Asia Minor war. Although he calls me to task for such an assertion, the citation he provides to my book (287) is actually a foot-noted reference (Ch. 5, n. 6) to Jelavich. It is Jelavich who argued that Greece 'appealed in vain' for British assistance in August 1922 as the Turkish counter-offensive gained ground. Greek troops had been ferried to Asia Minor a few years earlier by British (as well as Italian and American) warships, as the Allies had hoped to use Greek forces to impose a peace settlement on the Ottoman empire during World War I. The 1921 Greek offensive in Anatolia had been undertaken with Allied approval (or at least so Greece thought), yet by 1922 most of the so-called Great Powers had come to accept Kemal's authority. British forces in the region took care not to engage Turkish troops in 1922 as the latter crossed the Straits into Thrace, and Britain was directly involved in the armistice negotiations between Greece and Turkey, negotiations that helped to legitimize Kemal's new government in Ankara. If Dr Carabott is privy to information that refutes or significantly alters Jelavich's historical chronology or her interpretation of history, I would most certainly welcome his substantive input. But it is unclear why he holds me responsible for what he seems to believe is a lack of 'sound historical background' on the part of Jelavich.

Dr Carabott points to several other examples of what he regards as faulty scholarship on my part, leaving to readers the task of seeking truth from facts. He quite correctly questions the popular belief that Metaxas died soon after 'Ohi Day', 28 October 1940 (295, n. 14); Metaxas, in fact, lived for almost another three months before he died in January 1941. Ohi Day, which became a major national holi-

day in Greece, commemorated Metaxas's rejection of Mussolini's ultimatum (302, n. 11), which led to war between Italy and Greece. Many Greeks, including villagers in Assiros, believe that the ultimatum included a demand for free passage of Italian troops through Greece to reach Ethiopia, which Mussolini had attacked in 1935. Dr Carabott also rejects my contention that King Constantine was expelled from Greece in 1973. The dictator Papadopoulos implicated the King in a failed naval coup in May 1973. While the King was already living in exile in Rome, Papadopoulos declared him deposed in June of the same year, and sponsored a public referendum in July to establish a parliamentary republic. In retrospect, I no doubt could have been more judicious in my use of words, paying particular attention to distinguish popularly-held perceptions of national events from historical realities. But does this detract significantly from the central tenets of my arguments or the conclusions of my study?

Dr Carabott highlights the 'gravity' of what he considers my lack of 'sound historical background' by pointing to three instances of 'misinterpretation'. For example, he faults me for claiming that Venizelos was prepared to cede Macedonia by the time of the Asia Minor War, and accuses me of implicitly attributing this error to Clogg. He asserts that, to the contrary, Clogg argued that Venizelos was ready to give eastern Greek Macedonia to Bulgaria as early as 1915. That may have indeed been the case, but if so I believe this strengthens — rather than detracts from — my argument. If Greece's leaders were prepared to cede the territory only three years after its incorporation into the Greek state, does this not cast further doubt on present-day assertions that Macedonia has always been Greek? It seems all the more apparent that Greek leaders of the time regarded the region of Macedonia as a spoil of war that could be bargained away, rather than as an inalienable and central part of the Greek nation-state. (And *à propos* of Venizelos, I might add parenthetically that he might not have literally 'presided', but he did indeed help to negotiate the Treaty of Lausanne in 1923 [at least according to Clogg]; he was a signatory to the convention on the exchange of populations; and an attempt was indeed made on his life in June 1933, following a failed coup by Plastiras in which many had suspected Venizelos of complicity.)

As for the work of Mavrogordatos, Dr Carabott and I simply seem to read that source differently. I stand by my assertion that Mavrogordatos recognized the existence of the Slavs of Macedonia as an interest group, at least in relation to party politics and local and national elections. In fact, the quotation Dr Carabott provides from Mavrogordatos (that the vote of Jews, Turks, and Slavo-

Macedonians 'stifled the voice of the Greek majority in Macedonia') seems to verify — rather than refute — the claim that Slavs did have an influence in the electoral process and its outcome.

Dr Carabott has my very sincere thanks for correcting the misattribution of the Felton quote to Manousis. The passage in question does indeed come from Felton; it is an excerpt from a longer quote (running to two-and-a-half pages) in a *Makedonika* article about Manousis and his attack on Fallmeyer. I failed to record the footnoted citation to Felton, and I will most certainly correct this oversight in a future edition or translation of my book. Dr Carabott is also correct to point out the inappropriate translation of a proper noun in one of the references in my book's bibliography. With regard to the work by Tounda-Fergadi, 'Politis' is obviously a name rather than a common noun. Dr Carabott cites this particular example as one that 'really casts strong doubts on the merits of [my] knowledge and use . . . and treatment of historical sources'. Considering his strong language, I find it difficult to understand why he has opted not to mention the fact that in the main text of the book itself, where the Tounda-Fergadi reference is footnoted (on p. 133), I refer to the Politis-Kalfoff protocol, clearly using the name Politis in its proper form.

I agree that a citation of Gounaris's work on social cleavages in Ottoman Macedonia would no doubt have made my book richer in bibliographic coverage, and Dr Carabott is justifiably critical of this omission. However, he is incorrect to credit Gounaris with first challenging the view that ethnicity was held as a primary marker of distinctiveness in turn-of-the-century Macedonia. On the contrary, Vermeulen (an anthropologist) questioned such notions in a 1984 article ('Greek Cultural Dominance among the Orthodox Population of Macedonia during the Last Period of the Ottoman Rule') on socio-economic status and the ethnic division of labour in Macedonia (published roughly a decade before Gounaris's study).

Dr Carabott claims that these are not 'pedantic or nit-picking criticisms', but rather 'raise serious concerns' about my treatment of sources, 'written as well as oral'. He builds upon these points to question the validity of my 'reading of local township archives'. This is, perhaps, the most serious issue he raises, since local archives provided the most important single source of documentation for the processes I reconstruct in my book. In this sense, it is truly regrettable that he has chosen to question my use and interpretation of local archives indirectly, through largely rhetorical devices, rather than through substantive engagement and original reinterpretation. Nevertheless, I am grateful to him for giving my book a close critical reading, and for his assistance in checking references, correcting

errors, and keeping the record straight. In the end, I leave it to readers to judge whether a typographical error in a footnote, a mis-translated word in the bibliography, or a couple of points of semantic contention support his opinion that my book presents ‘not particularly soundly documented views and oversimplifications, numerous errors of fact and . . . misinterpretations’ that ‘cause grave concern’.

As a recognized scholar of Greek national history, Dr Carabott seems to be disappointed with my attempt to live ‘between history and anthropology’, arguing that my ‘exclusively anthropological “reading” of history’ undermines my study and the soundness of my findings. He laments that anthropologists and historians are still a long way off from reaching common ground, particularly on ‘methodological issues’ (which he does not address at all) and use of historical sources. Indeed, Bernard Cohn has eloquently addressed the myriad problems that often confront scholars from these respective disciplines in their mutual discourse. A great deal of misunderstanding still derives from the different details that anthropologists and historians are trained to observe, emphasize, and document. For me, history is more than the canonization or recitation of dates; it entails reconstructing the processual transformation of society, consciousness, and identity. Periodizations of time based largely or solely on national history impede our understanding of history as it is experienced by people at the local level. Yet I believe that such a common project is indeed possible, and it was toward such an end that I was working. Rather than diligently guarding the borders between our cognate disciplines, I would suggest that there is more to be gained by crossing such boundaries.

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Jeremy Black, *America or Europe? British Foreign Policy, 1739–1763*, London, UCL Press, 1998; ISBN 1–85728–185–3; £45

At a time when so much research is focused on Britain’s eighteenth-century imperial commitments and connections, it is refreshing to see a book that redirects academic attention to Britain’s relations with Europe. Jeremy Black’s *America or Europe?* not only debunks several longstanding yet pervasive myths about the role played by the colonies in eighteenth-century British foreign policy, but constitutes a long overdue reassessment of British diplomacy in the 1740s and 1750s.

British diplomatic and imperial history has long been dominated by a Whig agenda depicting the eighteenth century as a colossal contest for imperial and commercial colonial supremacy between England and France. This, beginning with the War of the Austrian Succession, supposedly culminated with the Napoleonic Wars that finalized a redirection of Britain's commerce away from Europe and towards the wider world. Pervasive though Francophobia was in the political culture of Hanoverian Britain, Black demonstrates not only that colonial and commercial considerations played little part in Anglo-French relations during the early to mid-eighteenth century, but that British official views of international relations remained governed by a European mindset up to the mid-1750s.

The union of the British and Hanoverian crowns in 1714 had created a joint Anglo-German state whose policy would be dictated by the defensive needs of Hanover up to 1760. While these, in addition to Britain's lack of territorial ambitions in Europe, dictated the conduct of a reactive and non-aggressive policy devoted to the maintenance of a territorial and political status quo, the extent to which British diplomats and statesmen regarded a 'classic' balance of power as a European ideal is questionable. The terminology of balance often reflected no more in contemporary international relations than an awareness of mutual participation in a states system subject to common rules of conduct and did not commit any state to active intervention for the purpose of preserving a theoretical equilibrium.

Black is much more successful in assessing the impact of short-term foreign and domestic factors upon the formulation of British foreign policy during the 1740s and 1750s. The roles played by parliament, the crown and the king's ministers are analysed perceptively by Black, from which British high political opinion emerges as anti-Hanoverian, but not anti-European. A blue water and explicitly imperial identity for Britain was conceived by the elder Pitt and captured the public and political imagination at a time when the 1756 Diplomatic Revolution was requiring Britain to rethink its traditional European policy. Whatever a Patriot press might print during the 1750s and 1760s, blue water imperialism, as conceived by Pitt, did not require Britain to abandon its ties to Europe. It suggested, not that an empire could be won on the battlefields of Germany, but that Britain's European status could be preserved by war in America.

This research raises interesting suggestions about the development of British national identity in the later eighteenth century. Black suggests that blue water imperial isolationism and the conceptualization of France as Britain's premier commercial maritime enemy

were specific products of later eighteenth-century public debate. Even in 1763, statesmen and politicians did not believe that a global maritime empire was Britain's manifest destiny. The accession of George III, which freed Britain from any direct connection to Hanover, was a necessary precondition of a foreign policy in which purely British interests would predominate, as was the death of public men whose perceptions of Britain's identity had been shaped by the international repercussions of the Glorious Revolution and the Hanoverian accession. Britain became less European after 1760 but this was not preordained by the diplomacy of the early to mid-eighteenth century.

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Marie-Hélène Huet, *Mourning Glory: The Will of the French Revolution*, Philadelphia, University of Pennsylvania Press, 1997; ISBN 0-8122-1617-2; viii + 223pp; £16.50

Historians have always, it seems, been fascinated by the role of violence in Revolutionary France and have been at odds, from their manifold political and historiographical stances, to explain the death of idealism, the gradual slide from the Declaration of the Rights of Man and the ideal of citizenship to the grotesque parody of liberty and equality that found its place in Robespierre's speeches in justification of the Great Terror. Michelet shared that passion in the nineteenth century, just as François Furet, Keith Baker, Branislaw Baczko and others have done in our own times. Marie-Hélène Huet counts herself among those who believe that violence runs deep in the revolutionary project and that the philosophical ideas that underpinned the reformers of 1789 also bear a heavy responsibility for the bloodlust of 1794. In this series of more or less discrete chapters, several of which emerge out of previously published essays, she makes her own stylish and at times idiosyncratic contribution to this debate, bringing to the problem the methodology of the New Cultural History and seeking to explain the intolerance of the Revolutionaries in cultural terms, as part of a process of mourning for a lost sublime.

The theme which holds these essays together she defines as 'the will of the French Revolution', explaining that revolutionary will implies premeditation, not just 'resolve', but 'a reasoned declaration

of intent and purpose'. Terror in this context means a form of philosophical collapse in the course of which 'representation in all its forms came under scrutiny'; and Robespierre, the dominant figure of her Revolution, becomes its arch-priest. Language plays a powerful part in her argument, since she remains loyal to the postmodern view that literary sources should be used like any other text and that politics is largely fashioned by its discourses. In the same tradition she calls upon key figures from other disciplines to explain and order revolutionary history, most notably, and perhaps predictably, Freud and Foucault. Thus she attempts to explain Saint-Just's espousal of Terror by referring to his own domestic tensions and his relations with his father. And in discussing the execution of Louis XVI she endorses Jacques André's somewhat contorted Freudian reading. For Huet, as for Freud, the Revolution can be read as an event which replays the origins of society, and in this context André insists that the fundamental social bond was based, not on the father's murder (as Lynn Hunt has proposed) but on brotherly love. Or, to quote André, as Huet does with fulsome reverence, 'From love addressed to the father to reciprocal attachment, fraternal homosexuality and the state of society develop through the same impulse' (166).

It is a moot point whether insights of this kind really do much to advance our understanding of Revolutionary France, however elegantly they are presented. Marie-Hélène Huet is a literary specialist — she is Professor of French at Michigan — and she tempts us with insights drawn from literary theory, which many cultural historians will doubtless see as brilliant and telling. She certainly provides some dazzling images, some arresting analogies, and some memorable passages of description. And her subject is less an understanding of the Revolution itself than of the interplay between history and memory, between the living and the dead. Here discourses attain a great symbolic importance, and she makes considerable play of the idea that the Robespierist Revolution was suspicious not only of cults and theatricality, but also of words and the verbal images they incarnate. Key words conveyed hidden meanings, which in the context of Robespierre's insistence on openness and the general Revolutionary pathology of conspiracy meant that words themselves could be suspect. Virtue, she suggests, was what would elevate discourse beyond all representation, which may explain Robespierre's evident distrust of words and wordsmiths, his concern to find truth where language had failed. Here again Huet is tempted to speculate. 'Quel est le remède de tous ces maux?', enquired Robespierre of the failure of words to distinguish the truthful and virtuous. Noting that the spoken word *maux* would be indistinguishable from *mots* to Robespierre's audience, she speculates that the usage was deliberate

and the confusion pregnant with significance (54). It is a clever idea, though it remains unprovable, and I confess that I find it difficult to imagine Robespierre regaling the Convention with carefully crafted puns. Like André's image of a subsumed homosexuality in the act of killing the King, it seems over-contrived and far removed from the immediate historical context. Indeed, like various other ideas cleverly analysed in these pages, it might seem to fit better into the genre of *belles-lettres* than of history, though to admit that is merely to betray this reviewer's own concept of revolution and approach to politics.

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Lucy Riall, *Sicily and the Unification of Italy: Liberal Policy and Local Power, 1859–1866*, Oxford, Clarendon Press, 1998; ISBN 0–19–820680–1; xi + 252 pp.; £35

Lucy Riall has written a valuable study of nineteenth-century Italy, and has done so in a manner which will make it useful to all with an interest in the development of the state in modern Europe. *Sicily and the Unification of Italy* deserves an audience well beyond Italianists. The major criticism of the book is that it could have been longer and more comprehensive, for Riall's archival work and breadth of interest merit more space than was allowed her. To reach a wider readership, Riall introduces the book — and each chapter — with highly competent mises-en-scène based on a deep knowledge of the historiography, which is only to be expected, in the light of her previous book on the Risorgimento. In this way, Sicily in the period is fixed firmly in the political landscape of early nineteenth-century Italy, and Riall's handling of the contrasts with Albertine Piedmont are particularly prescient.

The political and historiographical context is not quite matched by the space Riall devotes to Sicilian geography or culture, however, and those who know her work will realize this is due to lack of space alone. More detailed handling of the conflicts within the rural economy of the interior and a fuller account of local politics in the Restoration period would enhance the core of her research. Riall refers frequently to problems with the Sicilian judiciary, but its character and composition remain unexplored here. That said, Riall has done the period a great service by bringing the annexation of Sicily back to basics, and centring her work on the problem of law

and order, the foundation without which all political initiative is rendered meaningless. From this starting point, Riall opens the way to a much-needed discussion of why the Piedmontese failed to make of law and order the tool that rallied local élites to them in Sicily, in stark contrast to their success elsewhere in Italy in the period of unification. Her thesis, that they failed to fill the power vacuum created by the earlier Bourbon reforms, is surely the most convincing and best supported case for the nature of the 'Sicilian problem' yet to emerge. Riall presents a convincing case that Sicily did, indeed, experience modernizing change — and that this occurred under the Bourbons — but that it had disjointed consequences, which the Piedmontese found impossible to understand, never mind to correct.

There is a lingering Whiggishness about some of Riall's conclusions, nonetheless, particularly her belief that the Garibaldini offered both the Piedmontese and Sicily a way out of the impasse, when so much of her evidence points in the other direction. Gramsci is astutely cited and deployed, yet the lasting impression of Riall's research points more to the relevance of the Sicilian theoretician who is never named, Gaetano Mosca. Nevertheless, this monograph marks the arrival of a fine historian. Finally, Oxford have not done a fine author good service, with so many slips in proof and copy editing, minor blemishes on a fine book.

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Henrik Meinander and J.A. Mangan, eds, *The Nordic World: Sport in Society*, London, Frank Cass Publishers, 1999; ISBN 0-7146-4825-6 (hbk), 0-7146-4391-2 (pbk); 213 pp.; £27.50, \$39.50

In his prologue to this volume of essays, Henrik Meinander identifies each chapter by a key word. Mats Hellspång's theme in dealing with sport in Swedish agrarian society before the nineteenth century is 'time'; 'border' is the key word of Jørn Hansen's chapter on politics and gymnastics in Schleswig-Holstein. Meinander's own contribution looks at the rhetoric of sport in the early twentieth century ('word'); the other key words include 'people' (Matti Goksøyr on nationalism and sport in interwar Norway), 'gender' (Else Trangbæk on femininity and sport), 'war' (Erkki Vasara on maintaining a military capability through sport), and 'Olympic' (Per Jørgensen on the early Olympic movement and the Nordic nations). However, there are two other key words that recur throughout the

volume: 'organization' (taken up by Johan Norberg in his study of the relationship between sports organizations and the state) and 'control', which is the impression left with the reader of the ways in and for which sport has been organized in Scandinavia. The picture that emerges here of the Nordic peoples at play is that they take it extremely seriously. Sport is always for a purpose, whether it be to ensure that soldiers can ski or shoot well, or simply to make the participants into good modern citizens. The 'modernity' of organized sport is frequently emphasized. In the opening essay, Mats Hellspong argues that there was a sharp break with tradition in Sweden, where traditional games survived only on the margins, until rescued and preserved by the local folklore movement, as on the island of Gotland. Hellspong offers no obvious explanation of *why* the playing of traditional games died away so rapidly, or why Swedes embraced so wholeheartedly sports which had no predecessors in traditional culture, and which were 'newly cosmopolitan and unambiguously modern' (23), though he does offer some clues as to why this may have been so. The efforts of Captain Viktor Balck, 'the father of Swedish sport' as he is described, to modernize and standardize traditional games in the 1880s, were clearly designed to make them acceptable to a new sports-playing urban public, which wanted competition and results instead of a bit of harmless amusement in moments of snatched leisure. Rural pastimes and games were often condemned by churchmen and other figures in authority as childish or 'uncivilized'. The village swing, on which adolescent boys and girls would spend their Sunday evenings 'singing indecent songs and disturbing the sleep of law-abiding citizens', in the words of one Finnish newspaper (*Sanomia Turusta*, 8 August 1882), was one of several traditional pastimes that were labelled as uncouth. As James Obelkevich pointed out in his study of rural South Lindsey in the Victorian era, the farm labourers themselves began to accept the distinction now being made between childhood and adulthood and to withdraw from playing games on the grounds that they were childish (James Obelkevich, *Religion and Rural Society: South Lindsey 1825-1875* [Oxford 1976], 100). It is hard to believe that shifting attitudes on behaviour and leisure did not play a significant part in the decline of traditional rural games in northern Europe as well.

Of the sports considered in this book, those generally deemed by the outside world to be peculiarly 'Nordic' receive surprisingly little attention. Goksøyr has more to say about football than skating, or skiing, which is accorded the status of a specifically Norwegian 'national' sport in passing, without any closer examination of what that might mean. Finnish military officials in the early 1920s clearly did not believe theirs was a skiing nation, though they worked very

hard to make it into a major sport by organizing what had presumably been hitherto little more than a means of getting about in wintertime. Hellspong notes that, although skiing was known and practised in prehistoric times, the first evidence of skiing competitions in Sweden dates only from the second half of the nineteenth century. He is probably right in seeing competitions in pre-industrial agrarian society as spontaneous rather than planned, and in this sense, skiing as a sport had to be 'invented', or at least given standards and regulations; it is a pity nevertheless that none of the other contributors to this book bothered to take up the story of *how* Nordic skiing was given its modern form. Certain sports found favour more than others amongst the Nordic public and participants. Else Trangbæk offers some pertinent observations of why gymnastics was deemed especially suitable for women, and she attributes the successes of the Danish women's handball team in the European championships of 1995 and 1997, and in the Olympics of 1996, to the long-established tradition of women gymnasts playing handball in the summer. Erkki Vasara suggests that a lack of suitable and accessible sports grounds was a major reason why skiing was a more popular activity than athletics amongst the Finnish Home Guard (to an English reader, a somewhat misleading translation of the Finnish *Suojeluskunta*, more commonly translated as Civil Guard), though track and field athletics was and remains a huge favourite with the public, thanks largely to the early success of Finnish runners in the international arena.

This is inevitably a bit of a mixed bag, ranging over several centuries as well as a variety of situations, and a non-specialist reader might well find the political or social background rather confusing at times. It is hard to place J.A. Mangan's tailpiece, which deals mainly with English public school ethics, other than as a subtle comment on the differences between a multilayered culture and the rather blander and less diverse strands of Nordic society, and as a rather avuncular exhortation to the contributors to extend their enquiries; but it is cheering to see a series editor so engaged. The general purpose of the volume, in Meinander's words, 'is to show why sport came to play such an important part in the twentieth century in Nordic society' (5). The contributors have something useful and interesting to say about the way in which sport has been organized and used for national and nationalist purposes; they are less convincing on why sport (or certain kinds of sporting activities) has had such an enormous and abiding appeal.

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Mark Harrison, ed., *The Economics of World War II: Six Great Powers in International Comparison*, Cambridge, Cambridge University Press, 1998; ISBN 0-521-62046-5; xxi + 307 pp.; £35

What wins wars, in this instance World War II? Conversely, wherein lie the roots of defeat? Even more puzzling, what process transformed apparent 'losers' into 'winners' in the postwar period? There is a mountainous literature in multiple languages to account for victory or defeat, usually cast in terms of deserved military triumph or undeserved nemesis, great climactic writing, vast encyclopedic compendia, recitations of 'official history', oceans of documentation. Dr Mark Harrison takes as his point of departure that we have passed beyond the 'postwar period'; the true 'defining moment' of the present era is not 1945 but 1991, the end of the Cold War. Understanding 'World War II as it really was' facilitates 'learning to see today as it really is'. Additionally, contemporary economic problems have revived interest in postwar reconstruction, while for the economist wartime experience is intrinsically interesting 'in terms of government versus the market'. To this end the wartime economic experience of six great powers, the United Kingdom, the United States, Germany, Italy, Japan and the Soviet Union are scrutinized and compared in great detail each by an expert author, each exploiting a treasure trove of statistical data.

This *embarras de richesse* serves not only the economist and the economic historian but also any historian of World War II searching for war 'as it really was', ably assisted by Dr Harrison's preliminary 'Overview'. Until 1942 the Axis powers, 'despite significant economic inferiority', appeared to be on the verge of complete success, the decisive victory which finally eluded them due to military failure rather than economic weakness. But in a second phase, beginning in 1942, 'economic fundamentals reasserted themselves', superior military qualities yielding to superior GDP and population numbers, in which respect the Allies were the chief beneficiaries of globalization of the war, once America mobilized and the Soviet war economy stabilized. Not that quality was totally subservient to quantity. Qualitative factors continued to exercise a 'major influence', principally German and Japanese use of their military resources to hold off defeat for so long, but quantity was indubitably of the essence in Allied strategy.

The exploitation of mobilization resources is rigorously investigated with respect to the six warring nations, demonstrating in detail how 'economic fundamentals' operated and how 'the war was won' from that particular perspective, but what is quite startling is the inference that there are 'benefits to defeat in war'. Though the

authors of the study of *Great Britain* at war are sceptical of this idea, the cases of Italy and Japan, not excluding Germany, command close attention. From Professor Vera Zamagni's study it might be argued that Italy was a special case, with the lowest proportion of expended GDP of all combatants. Thanks to limited losses, there was rapid recovery to prewar levels, while the Marshall Plan facilitated economic expansion, the switch into metallurgy, engineering, chemicals and fresh export markets. In Japan militarization altered the industrial structure towards heavy engineering and chemicals, creating a more experienced work force. Wartime mobilization and wartime destruction changed the structure of the prewar economy, while occupation policies, emancipation of the labour movement, and land reform all shaped the postwar Japanese economy. An understanding of the German war economy and the attendant enhancement of the 'development potential of the industrial economy' is essential to explaining *Wirtschaftswunder*.

These studies by Professor Zamagni, Professor Akira Hara and Professor Werner Abelshauser respectively bring a very new perspective to 'defeat'. Dr Mark Harrison's study of the Soviet Union reverses the argument, demonstrating how a 'victor' became a 'loser'. This is a formidable investigation of the Soviet war effort, which illustrates the high mobilization capacity of the Soviet economic system and also the powerfully entrenched 'military-industrial complex'. Given the latter circumstance it would seem that militarization of the Soviet postwar economy was inevitable. Dr Harrison argues convincingly that this was not so. Social change induced by the war unquestionably changed attitudes, raising questions including renewing military competition with wartime allies, but this was a mood lacking overt public expression, emerging only under Khrushchev in the guise of 'peaceful coexistence'. By this time, however, whatever disposition had existed after 1945 was much enfeebled; effective socialist reform could not compete with the power of the military-industrial élite, legitimized by 'victory' and committed to the continuation of militarized economy. That élite may have won the war but learned the wrong lessons from it, 'losing the peace' by failing to adapt, thereby condemning the Soviet system to ultimate decay.

This is a very impressive, challenging volume, seriously questioning ageing orthodoxies associated with World War II. It presents unequivocal proof that World War II economic experience cannot be fully understood without reference to the prewar period, nor the transformations of the 'postwar period' without considering the impact of war upon the nominal victor or supposedly vanquished. Professor Hugh Rockoff, surveying American experience, argues

that the real legacy of the war was less a material tally, more 'intellectual, institutional', a new macroeconomic regime which profoundly influenced employment and inflation for decades. Besides maximum guns, marginal butter, wartime economic workings provided jam for the morrow. We might then say we were all finally winners.

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Hans-Joachim Torke and John-Paul Himka, eds, *German-Ukrainian Relations in Historical Perspective*, Alberta, Canadian Institute of Ukrainian Studies Press, 1994; 0-920862-91-8; 239 pp.

This edited collection is made up of fourteen papers which were originally presented to a conference backed by Stiftung Volkswagen and held as long ago as 1986. The gathering was initiated and organized by the Canadian Institute of Ukrainian Studies. The time span covered in the volume is from the early nineteenth century to the 1990s. The latter date reflects an updating of the original conference contributions. The weight of the articles certainly falls to the years of the Second World War. Ihor Kamenetsy analyses German colonization plans both after 1941 and between 1914 and 1918. Ralf Bartoleit deals with trends in agriculture during 1941 and 1942. Wolfdieter Bihl discusses Ukrainians who fought for the 14th Armed Grenadier Division of the SS. Peter Potichnyi traces the relations between the Ukrainian Insurgent Army and the German authorities. In an all too brief piece Taras Hunczak discusses the place of Ukrainian nationalist sentiment in the organizations inspired by Stepan Bandera.

Two contributions are included that appeal to more contemporary interests. John Armstrong poses a highly germane question: 'Ukraine: Colony or Partner?' He concludes that the Ukraine must try to engage in international relations, especially with Berlin and Moscow, as an equal partner. This should be the best way to retain the loyalty of the territory's ethnically mixed citizenry. Certainly the view has a ring of truth: it would be counter-productive either to build too close an alliance with Germany only to alienate ethnic Russians, or to maintain an attitude of subservience to Moscow which would disillusion Ukrainians. Equally perceptive are Armstrong's comments about the limits on international influence

which lie at present within the capabilities of a reunified Germany. Recognizing the nation's heavy domestic commitments, he believes Germany's restricted capacity for power projection presents the Ukraine with advantages. As he puts it, 'Ukraine obtains its share of German aid without incurring severe risks of German dominance' (196). The present situation is, he continues, tailor made for both the government in Kiev and private Ukrainian organizations to draw on the widest possible extent of advice and support from Western Europe and North America, so avoiding the emergence of dependence on any single power. Yaroslav Bilinsky provides a piece entitled 'Germany, Western Europe, and Ukraine after World War II'. The end point of his discussion is less 'upbeat' than that of Armstrong. He recognizes in particular that there have to be concerns about the implementation of economic reforms in the Ukraine as well as about the capacity of the state to provide political stability. Still, he believes that 'at least the foundation for a solid relationship with united Germany has been laid, quickly and skilfully' (222).

Particularly strong contributions deal with Germany's policy towards the Ukraine during the First World War. Returning to Kamenetsky's piece on colonization, he observes that in 1918 the German Foreign Office believed there were 2 million ethnic Germans living in the lands of the former tsarist empire, owning around 10 million ha of land. Of these, 700,000 were believed to control 4.5 million ha in the Ukraine. No surprise, then, that it occurred to the German High Command, in the form of Erich von Ludendorff, to try to unite this resource. He developed the plan to settle Ukrainian Germans together in the Crimea and along adjacent strips of coastal territory. Although the Foreign Office opposed the plan as at odds with the desired end of establishing an independent Ukrainian state, Ludendorff's ideas lived on through National Socialists such as Alfred Rosenberg. During the Second World War, General Plan East was devised which once again saw the Ukraine designated as a settlement zone. Here, then, we have a clear example of history repeating itself in the form of tragedy. Peter Borowsky provides an interesting article entitled 'Germany's Ukrainian Policy during World War I and the Revolution of 1918-19'. He argues persuasively that the German Foreign Office was always pursuing more than just a short-term policy of immediate exploitation. The goal actually was to separate the area from Russia as a means to weakening the latter, to anchor German influence in the Ukraine's economic sphere and thereafter to establish the area as a bridgehead for the economic penetration of the remains of the Greater Russian empire.

Other essays deal with the history of German colonists in the Ukraine in the years before 1914, German culture in the nationalist

awakening of Western Ukraine in 1848 and relations between Germany and the Soviet Ukraine from 1920 to 1939. They are uniformly worth reading. All in all this is a remarkably stimulating volume which should be appreciated by anyone wanting to find out about the general topic of Germany and Eastern Europe. The contributors deserve congratulations.

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*School of Slavonic and East European Studies,
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