CLASSES REREAD

A HEROIC WORK OF EXTRAORDINARY SCHOLARSHIP: ON THE NEW TRANSLATED EDITION OF H. G. ADLER’S THERESIENSTADT OF 1960

BEN BARKOW


More than sixty years after its original publication, H. G. Adler’s Theresienstadt remains indispensable to anyone who has more than a casual interest in what was among the most perverse and strange sites of incarceration in the Nazi empire. Although sadly few people realize it, Adler’s book is also essential reading for anyone engaged in trying to understand the Holocaust. Despite this, for much of its existence it has been unavailable to most people, and between 1960 and 2005 no edition was in print. Until now there has been no edition in English. Those who wanted to own a copy had to scour the second-hand market and pay dearly for the privilege.

The new edition is, to paraphrase Charles Dickens, the best of books, it is the worst of books. To begin with what makes it the best: H. G. Adler is a unique figure among writers on the Holocaust and he wrote a very special book indeed—and one with a complicated past. He had been an inmate in Theresienstadt (Terezín) for thirty-two months before voluntarily accompanying his wife and mother-in-law to Auschwitz, where both were gassed. He was then sent to a sub-camp of Buchenwald in Langenstein, where he was liberated in April 1945. He eventually settled in London and worked as a freelance researcher and writer, producing around two dozen books, covering history, fiction, poetry, and criticism.

Theresienstadt is a work of extraordinary scholarship. Adler carried out immensely detailed research during the 1940s and 1950s, assembling a vast amount of documentation which underpins his work. The riches of his archival researches are so great that a supplementary volume was published to make a selection of them public (never reprinted, never translated). His ability to have done this outside the academy and in a world dominated by the Cold War, closed archives, and no external funding (that I know of) is truly remarkable.

The structure of the book reveals something of Adler’s priorities. It is divided into three parts; History, Sociology, and Psychology. History takes up 150 pages, Sociology 336, and Psychology just 42. Sociology was the key to how he was able to take the raw observations which he had made in the ghetto as a way of coping and surviving it, and hang them on a conceptual framework, to be upholstered with his archival findings. Consistent with his title, we are examining a community. Whether or not this serves as a microcosm or paradigm of modern industrialized, ‘mechanical and materialistic’ society is another issue and in 2018, perhaps no longer the critical one. Adler had grappled with how to approach the writing of the book and found in the sociological thought of figures such as Georg Simmel and Franz Baermann Steiner the perspective that opened up his avenue of approach. The helpful Afterword by Adler’s son, Professor Jeremy Adler (translated from the 2005 German edition by Wallstein\(^2\)), reflects in detail on Adler’s intellectual setting and inspirations.

I would add that for most readers, his theoretical commitments do not matter all that much, since his writing is idiosyncratic and rooted within himself and is not ‘harmed’ by the theories he followed. In a sense, being an ‘amateur’ turned out to be a strength rather than a weakness. Also, his insights and wisdom are greater than the theorists he read in many instances: he is by some length the better and more important writer.

What strikes the modern reader is that Adler is unashamedly the moralist, weighing evidence and bringing in judgements. His lengthy reflections on guilt and Judaism are fascinating. His refusal to put on rose-tinted spectacles when looking at his peers, or to

reduce their behaviour to issues of black and white, contains an important challenge to anyone thinking about Holocaust commemoration and education today. In many cases, this is now based on such radical simplifications that it universalizes, relativizes, and instrumentalizes the Holocaust so completely that the most instrumentalized and politically correct end of Holocaust educational practice is almost as distorting as the soft end of Holocaust denial. Reading Adler is an astringent corrective to this tendency.

But there is a further dimension to the book that deepens it profoundly and gives it a very rare kind of authority—it is as much a survivor’s testimony as it is a scholar’s text. Survivor testimony, these days, is taken as a sort of gold standard of Holocaust education. This is questionable in and of itself, but even if we accept that it is, the failings go further. One of the tragic ways in which we are getting Holocaust education and commemoration so badly wrong is by ignoring the testimonies given in the immediate aftermath of the war (mainly because they are not on film, not in colour and not in high definition, not a hologram). But these early testimonies are among the most vital and significant we have. And while most testimonies are able to describe only what happened in one or two places at one or two moments, Adler’s testimony embraces not just the whole of the Holocaust, but wider human history, as he makes clear in his concluding chapter:

Theresienstadt is part of the history of an empire, and thus part of the simultaneous history of the world. The subjects of the camp’s history could not evade this interrelationship; the course of its history is first of all involved in and then largely determined by the surrounding history (pp. 559–60).

Many of the things Adler describes or explains are only possible because he was there to witness them, or the peer of those who had been. Indeed, his strategy for his own psychological and emotional survival involved turning himself into a detached, yet close observer of the camp/ghetto and its life. Taking all these qualities into account, I cannot think of a more monumental, towering, searing statement of survival and indictment.

I believe that this is a masterpiece and one of the most important books about the Holocaust that has been (and perhaps will ever be)
written. (One of the others would be Adler’s own, utterly ignored, *Der verwaltete Mensch*, a study of the bureaucracy of deportation.\(^3\) I would suggest that Adler’s fictional, poetic, and critical writings, now enjoying rapidly growing admiration and respect, can only be properly understood if *Theresienstadt* and *Der verwaltete Mensch* are taken into account. These books are the intellectual and moral core of his *oeuvre*.

Where does this work sit in the historiography of the Holocaust? *Theresienstadt* belongs to that first generation of studies of the subject that I personally think of as ‘heroic’. These books are now often overlooked, but deserve to be at the heart of every student’s reading list. Its peers include Raul Hilberg’s *The Destruction of the European Jews* (1961), Gerald Reitlinger’s *The Final Solution* (1953), Hannah Arendt’s *Eichmann in Jerusalem* (1963), Elie Wiesel’s *Night* (1960), Eva Reichmann’s *Hostages of Civilisation* (1950), and the edited volume by the Jewish Historical Institute Warsaw *Faschismus – Getto – Massenmord* (1961), among the works of many other authors including Jacob Presser, Reuben Ainsztein, Uwe Adam, Norman Bentwich, Arnold Paucker, and more.

There are shelf-loads of these early accounts and they offer something more than the semi-industrialized outpourings of university departments today. Such books are often not scholarly—or not primarily scholarly—but are informed by a passionate concern that the Holocaust be recognized, remembered, and understood as both an immense crime and a collapse of civilization and morality. The authors were frequently of the generation that lived through the war, if they were not themselves survivors. In our age of relativization, trivialization, and denial—not just of the Holocaust but truth and reality in general—these works, with their unshakeable moral core, are very much worth reconnecting with.

They are, of course, works of their time. They are products of the Cold War, and some are infused with Cold War politics. They are works written during Israel’s youth, and often reflect that fact. They were written despite the fact that the authors had access to a grossly limited set of archives. They are generally not feminist in outlook. But against this they are responses of greater or lesser immediacy. They are not consciously seeking scholarly detachment but are pas-

\(^3\) H. G. Adler, *Der verwaltete Mensch: Studien zur Deportation der Juden aus Deutschland* (Tübingen: Mohr, 1974).
sionate, angry books written to force recognition of crimes and injustices and to awaken the slumbering conscience of a largely indifferent world. Adler should stand at the very heart of this heroic generation. Thanks to the bizarre publishing history of his work, he does not, being thought of as obscure, difficult, and marginal (only the middle one of these terms is true).

Sometimes the heroic generation are criticized for the harshness of their judgements, for instance, relating to the conduct of Jewish Councils. Today’s scholars are more nuanced and tend to make far softer judgments—if they dare to make judgments at all. (They would do well to consult this earlier generation of frequently non-academic writers in order to reflect on their responsibilities as historians, philosophers, social scientists etc. in relation to forming moral judgments.) Yehuda Bauer (perhaps straddling the heroic and subsequent generations), for example, has poignantly used Lawrence Langer’s phrase ‘choiceless choices’ to describe the dilemmas facing Jews and Jewish Councils struggling to cope with Nazi duplicity and hate. There is certainly truth in this but Adler, who is remarkably sensitive to the circumstances under which Jewish Councils labour-ed, judges them harshly nonetheless. His reflections are worth looking at.

In the preface to the second edition he reviews some of the criticisms made of the first edition, and focuses on critics of his portrayal of Jakob Edelstein, the first leader of Theresienstadt’s Jewish Council. Adler was accused of being unduly harsh in his judgements of Edelstein’s actions and decisions. He defends himself by acknowledging Edelstein’s sacrifices and good intentions but goes on:

No, this man does not deserve our hatred and scorn, but when we look back at the impact he had, he also does not merit being turned into a role model or hero. The fact that he did not avoid the unavoidable only fits into the larger picture, but the fact that he did not shy away from what was avoidable tarnishes his memory . . . There [in Theresienstadt] we see Edelstein stoop to new lows (p. xviii).

Most of Adler’s contemporaries judged the Jewish Councils in terms of black and white and found them black. Today’s scholars find them largely white (or at least ‘choiceless’). Adler’s contribution
(just one of many in this book) is to sift through the shades of grey that delineate reality and judge discriminately, but judge. In some areas the leadership was choiceless, in others choices existed. Where choices existed, the leaders deserve to be held accountable for the decisions they took. Adler finds much in their conduct that is weak, dishonest, and occasionally evil. This is the case with Edelstein’s successor Paul Eppstein:

In this connection I would like to mention the case of Vladimir Weiss, a Zionist from Prague who was deported to Auschwitz in September 1943, with his wife and child, because he sent Eppstein a memo on corruption . . . This was not an instance in which Eppstein succumbed to tragic circumstances; these were actions he deliberated over and undertook of his own free will. Something like this cannot be whitewashed . . . (p. xix).

_Theresienstadt_ is unusual in another way. It is, of course, annotated. But not like other books. Its 356 footnotes or endnotes extend to 180 pages, because Adler cites and quotes at length from dozens of sources, includes critical evaluations of them, and tells the stories of their authors. This ‘sourcebook’ aspect of _Theresienstadt_ provides an invaluable trove of documents which illuminate Adler’s argument but are equally important as free-standing contributions to our understanding of events and people. Among those quoted is Philipp Manes, a Jewish businessman who organized lectures, play-readings, and concerts, and wrote an almost 1000-page Chronicle before being sent to Auschwitz (the late Dr Klaus Leist and I edited and translated the manuscript).4 Adler offers this evaluation:

Manes, a man of strict fairness, optimistic spirit, and subtle powers of observation did not succumb, like most of the inmates, to senseless political rumour; however, his outlook nevertheless confirms the tragic attitude of all too many elderly Jews from Germany towards the SS, whose abysmal villainy he did not suspect until the bitter end. Because people often do not believe in the presence of this attitude—this naïveté, which we have repeatedly described . . . (p. 709).

Adler then quotes passages from Manes’s chronicle to illustrate his point, perhaps a little unfairly, since it is quite difficult, if you take the whole Chronicle into account, to work out what Manes’s attitude really was. We will look at translation issues below, but the rendering of ‘Ahnungslosigkeit’ as ‘naïveté’, seems weak. I think ‘cluelessness’ approximates more closely to what I take Adler’s feelings about Manes to have been.

I have sketched out some of the many things that make this ‘the best of books’. It is time now to look at what makes it ‘the worst’. This comes down to set of issues relating to the publisher, the translator, and the editor. Theresienstadt is a large book. But Cambridge University Press have opted to make it a really large book, a peculiar and uncomfortable format that is extremely heavy and sits very uncomfortably in the hand. Reading it involves a considerable amount of weightlifting. Despite this, the binding is that of a cheap paperback. This book will not last if handled regularly, as it should be.

More significant are some editorial decisions I consider to be deeply damaging. Firstly, the glossary. The original has a glossary spanning around thirty pages. This reflects the distinctiveness, and indeed, oddness of the ghetto terminology (not to mention Nazi terminology). I contend that you cannot understand Terezín if you do not grapple with the fact that it was part Czech, part German, and part other, more sparsely represented nationalities. This fact, coupled with the ghetto’s long pre-history, shaped the language and the language in turn shaped the inmates.

Let us look at one central word: Ubikation. This refers to living quarters. Its use was universal. The definition Adler gives is: ‘Czech, “ubikace”, from the Czech and Austrian military terminology meaning quarter.’5 The word thus brings to life Adler’s comments about Theresienstadt as part of a larger history. It has its origins in time when Terezín was an Austro-Hungarian garrison. Readers of the translation will not encounter this key word anywhere in the book and their understanding will be the poorer.

The deletion of the glossary robs readers of a great deal and denies them access to the frequently ironic use of language through which the inmates tried to lubricate the grinding experience of the ghetto. Thus, Mazzeorden (matzo medal) = the yellow star;6 Vitamin B,

5 Adler, Theresienstadt, Wallstein edition (as in n. 2), p. lvi.
6 Ibid. p. xlv.
Vitamin P = B = Beziehungen (connections) P = Protektion;7 Prüser = a vulgar Czech expression meaning something like ‘shit-through’. Adler explains that among the Czech Jews it denoted an illegal activity that had been spotted by the authorities.8

Another, even more significant example, is the term Schleuse. Literally it means sluice (as in in a canal lock, not a funnel, as the translation has it). In Theresienstadt it took on a densely layered set of meanings. Adler’s glossary gives it and its derivatives almost a full page, indicating how much there is to understand about it.9 Adler states that these are the central words of the camp language. Readers of the English edition encounter the word with no explanation and can never understand its full significance. Yet without this word and its derivatives it is not possible to understand the workings of the camp and their impact on the inmates.

Characteristically, Adler classified the words in the glossary: unmarked words were those common in documents and usage; ‘O’ indicated words mostly confined to documents; ‘U’ meant words common in usage but not documents and so forth.

The original book boasted three indexes: persons, places, subjects—spanning some thirty-eight pages. They allow you to navigate through the 926 pages of the Wallstein edition in immense detail to locate whatever you are looking for, and to let happenstance reveal unlooked-for treasures. These indexes have been stripped out of the translation and replaced with a single twenty-eight page index of much lower quality. Look up the name Trostinetz (or Trostenets), to take one example at random: in the original you will find four entries; the translation offers only one. Look up Aachen, Egypt, or Europe, or an important witness such as Gerty Spies, and you will not find anything at all.

The original hardback editions reproduced a plan of the ghetto on the flyleaf, drawn at the time of the ghetto. On the verso was a full page of notes explaining the abbreviations and numbers on the plan. The 2005 Wallstein edition improved on this quite considerably, turning it into a fold-out page at the back of the book with both the plan and the key, so that you can have both before you the whole.

7 Ibid. p. lvii.
8 Ibid. p. xlvi.
time you read. The CUP translation features the plan, much reduced in size so it is very difficult to work with, and missing the key altogether, on the back of the dustjacket. (The 2005 Wallstein edition is the best available, a superb production, adding the essay by Adler’s son, a good size, sewn-in-sections so it lasts forever, and good value—€49 for the paperback when it was in print, although now much more expensive second hand. It is exemplary.)

Finally, and most disappointing, the translation itself. Words were the lifeblood of Adler, he cared about them profoundly and crafted his prose with immense deliberation and attention to detail. This is reflected in the first paragraph of the Preface to the first edition, and it is about words and language:

Although I made an effort to write this book using an untainted German, because of the topic involved—an SS camp set up for Jewish inmates—the text came to reflect and was often subject to the general deterioration of language in the age of mechanical materialism, as well as, in particular, the amorphous, coerced language of the National Socialists and the colloquialisms and written language of Theresienstadt. But the demon that created this camp and left it to vegetate must, certainly, also be conquered linguistically (p. xxiii).

You must take it on trust that, contrary to appearances, the original German of these two sentences is rather beautiful. But it is at least clear that we are dealing with a complex text which reflects and uses four kinds or phases of language. Adler’s intention is to write an unspoiled German. He acknowledges that he cannot achieve this because three things prevent him. Firstly, the general decay of language in an industrial and materialistic age; secondly, the formless yet frantic language of National Socialism (the ideology, not the people who embraced it); and, finally, the extraordinary patois of Terezín. Things are made yet harder because Adler is being a touch disingenuous—his German may be ‘untainted’ or ‘unspoiled’ but that does not mean that it is plain. And it is not—it is dense, allusive, layered, and complex. His book is, aside from being a work of scholarship and testimony, a self-consciously literary work and a work of literature. I do not think Adler would have enjoyed the irony that his effort to conquer linguistically should have been turned into a linguistic quagmire.
While it may not be possible to recreate these linguistic layers and shades in English, the translator and editor should at least be sensitive to them and try to find ways to convey to the reader something of what Adler is doing. A good translation does not offer word-for-word equivalence (which results in nonsense—see below). It must recreate in the rhythms and idioms of the target language something of the music of the original, while also conveying the information in the text. To achieve this with Adler requires artistry as well as scholarship, perhaps backed up by an extensive apparatus of footnotes. The present edition sadly does not achieve this, or even seem to try.

Let us consider the book’s title. Adler’s subtitle is Antlitz einer Zwangsgemeinschaft, which is a brilliant formulation, and ‘Zwangsgemeinschaft’ is a word of his own invention. This is obviously a challenge for a translator. What we are given is The Face of a Coerced Community. I think this is problematic. ‘Face’ is a poor choice for ‘Antlitz’, which is a slightly archaic and poetic word, perhaps best rendered as ‘countenance’ or ‘visage’. Working backwards I would expect ‘face’ to translate back into German as ‘Gesicht’, not ‘Antlitz’. Stylistically ‘countenance’ is the best match for Adler’s original. More significantly, ‘coerced’ also seems wrong. Nazi policy towards Jews after 1941 was characterized by extreme violence and force. ‘Coercion’ commonly implies persuading or bullying people by social pressure, rather than driving them into concentration camps at gunpoint. Coercion perhaps better describes the state of the home front. Those judged by the Nazis to be racially valuable were coerced into complicity. The Jews in Theresienstadt were put there by at gunpoint, and were violated in every way until they either died or were deported to be murdered somewhere else. That is not coercion; that is genocide.

I cannot say what the best translation would have been—I suspect that it should not be a direct translation at all (because the original is pretty much untranslatable). It calls for a creative, artistic intervention to produce something carrying the broad sense but with equal linguistic power. Either that, or something quite unrelated to the original, and a brief explanation of that decision.

The reader of this translation must not expect to experience anything approaching Adler’s elegance and artistry. But can they at least understand what he is saying? Often, yes; frequently, no. On pages 123–4 we read:
To create these paradisiacal conditions, 17,500 people first had to vanish into Auschwitz. Simultaneous with the easing of conditions, partly to the benefit of a minority, partly to the benefit of all who remained, they were produced by a will that was responsible for the gas chambers. That will wore the undeserved and barely camouflaged mask of a benefactor. In this way, developments in Theresienstadt grew into the most gruesome ghost dance in the history of Hitler’s persecution of the Jews.

This is bordering on gobbledygook. Adler is saying that the deportation of 17,500 people to Auschwitz led to conditions easing. In some respects this benefited a minority of the population; in other respects it benefited everyone left behind in the camp. Nevertheless, he continues, this easing of conditions expressed the same intention towards the Jews that was responsible for the gas chambers, albeit thinly disguised as by an ill-fitting mask of benevolence. Thereby developments in Theresienstadt became the eeriest of ghostly dances in the history of the persecution of Jews under Hitler. (If you imagine I have maliciously chosen the one passage that is badly translated, let me assure you, I could produce dozens of similar examples.)

Throughout the book there are formulations that are clumsy, confusing, absurd, or plain wrong. On page 70 Adler quotes from a lengthy document describing Theresienstadt which was written by Otto Zucker, an architect and civil engineer who had served in the First World War and was the deputy of Paul Eppstein. Among other things Zucker describes the functioning of the kitchens and food distribution in the ghetto. In one of these passages the translator offers us: ‘For a normal mass kitchen operation one needs a cooking pot room that corresponds to 1.2 litres per person.’ Cooking pot room? The original German is ‘Kesselraum’, literally, a boiler room, which does not seem to make sense. What I think is being referred to is some measure of capacity relating to the cauldrons used in military and field kitchens to produce hundreds of meals in a short space of time. Zucker specifically refers to military kitchens in this passage. He also refers to the fact that they were able to get new cauldrons, but could not install them owing to the labour shortage.

Overall, the translation reads like a first draft. It would have been a decent first stab and after revision and correction (perhaps two or
three iterations) might have recreated Adler’s prose creditably. But it is so clunky and riddled with errors that it obscures a book that would have been challenging to read even if perfectly translated and edited.

I referred above to the Philipp Manes Chronicle edited by Dr Klaus Leist and myself. In preparing an English language edition of it we confronted similar problems to those facing the translator and editor of *Theresienstadt*: a poor basic translation, lack of support with copy-editing and proof-reading, and immense commercial pressure to hand over a finished manuscript. In our case we worked round the clock for several weeks to fix it, although the result is far from perfect (and our manuscript was a fraction of the length of *Theresienstadt*). I relate this to ensure that the translator and editor do not shoulder the responsibility alone. The publisher must share the blame.

Is it simply too difficult and expensive to produce a good English edition of Adler? Of course it is not. By way of comparison, consider the magnificent *Complete Works of Primo Levi*, published two years ago.\(^\text{10}\) Three volumes, newly translated (and well translated), elegantly produced (still a cheap binding, though) and over 5,500 pages. Price on Amazon: £78.00. *Theresienstadt* has 882 pages; price on Amazon: £77.00.

If *Theresienstadt* had been written recently and published for the first time today, its impact would have been immense, and it would have triggered many a controversy and helped to shape the research agenda for years to come. It would be interpreted, I believe, as a critique of the fields of Holocaust research, commemoration, and education, and be seen to challenge fundamental aspects of how we engage in these activities.

As it is, the book has never been appreciated in the English-speaking world, simply because so few have been able to read it, if they could track down a copy at all. Tragically, what Cambridge University Press, with its associates, the US Holocaust Memorial Museum and the Terezin Publishing Project—the three organizations behind this edition—have produced is a version that perpetuates the confusion about Adler and that, albeit in a new and unexpected way, continues to deny us the chance to hear his true voice. It is a huge missed

opportunity and Holocaust scholarship, education, and commemoration are the losers.

REVIEW ARTICLE

FORGOTTEN, NOT FORGIVEN?
NEW GERMAN-LANGUAGE WORKS ON THE 1918/19
GERMAN REVOLUTION

ALEX BURKHARDT


The centenary of the outbreak of the First World War in 2014 provided the occasion for a veritable glut of new books to commemorate and reassess what George Kennan called the ‘great seminal catastrophe of the twentieth century’.1 The year 2018 offers a similar opportunity for scholars to pen their thoughts on the German Revolution, which brought an end to the war as well as the Hohenzollern monarchy. In contrast to 1914, however, the revolution has tended to slip under the radar of the popular historical imagination. Ian Kershaw is quoted on the back cover of one of the books reviewed here to the effect that ‘today’ the revolution is ‘often forgotten or underestimated’, while a compendium of essays released in 2010 was entitled Die vergessene Revolution (‘The Forgotten Revolution’).2 There is, indeed, little doubt that the revolution does not enjoy the sort of prominence in Germany’s popular historical tradition that the French or Russian revolutions have acquired in their respective nations; nor has it generated the same kind of public interest as, say, the rise of the Nazis or the Holocaust.


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Despite this, the German Revolution was at one time one of the most intensively researched fields in modern German history—and one of the most controversial. The conservative historiography of the 1950s held that the Majority Social Democrats (MSPD) had no option but to forge a deal with imperial elites in order to crush a burgeoning ‘Bolshevik’ revolution centred around the Workers’ and Soldiers’ Councils.³ But in the 1960s and 1970s, scholars such as Eberhard Kolb, Susanne Miller, and Ulrich Kluge dedicated their fledgling careers to challenging this view by arguing that the Council movement carried a ‘democratic potential’ which the Social Democrats failed to exploit because of their excessive fear of Bolshevism and misplaced faith in the military. They thus left untouched ‘anti-democratic’ figures and institutions which would later play a crucial role in delivering the republic over to Hitler.⁴ To some extent, then, the German Revolution of 1918/19 may have been ‘forgotten’, but the Social Democrats’ perceived failure to seize the day and fully democratize Germany in late 1918 was never really ‘forgiven’, at least not by certain sections of the German historical profession. This rather negative evaluation of the SPD’s role in the revolution was taken to an extreme in the German Democratic Republic, where historians depicted the Social Democrats not so much as excessively timid or misguided, but as conscious ‘traitors’ to the tradition of Marxist class politics who showed their true colours during the revolution when they sided with the old regime to become ‘murderers of workers’ (‘Arbeitermörder’).⁵

The legacies of these judgements are very perceptible in two German-language books released in 2017 to mark the revolution’s centenary: 1918: Aufstand für die Freiheit (‘1918: Uprising for Freedom’), by Joachim Käppner, and Die Revolution von 1918/19: Der wahre Beginn unserer Demokratie (‘The Revolution of 1918/19: The Real Beginning of our Democracy’) by Wolfgang Niess. Both books bill themselves as

⁵ A similar argument was also advanced by Sebastian Haffner in his famous polemic, Die deutsche Revolution: 1918/19 (Berne, 1969).
correctives to the revolution’s lack of popular recognition by providing new narrative accounts aimed at mass audiences, and each is written by a trained historian turned journalist (Niess wrote a doctoral thesis on the historiography of the revolution and now works as an editor for SWR Fernsehen, while Käppner, a journalist at the Süddeutsche Zeitung, also holds a Ph.D. in twentieth-century German history).6

In their accounts, both authors focus mainly on those traditionally considered the revolution’s most prominent actors—Social Democrats, the Workers’ and Soldiers’ Councils, the masses who gathered in Berlin and other big cities to demonstrate for peace—while ‘old elites’ in the military, industry, and bureaucracy serve (as per usual) as the villains. Meanwhile, despite becoming more central to the historiography of the revolution since the fall of the Berlin Wall, the Bürgertum, both Protestant and Catholic, is handled quite cursorily by both authors.7 Above all, however, both of these books draw heavily on the radical West German scholarship of the 1960s and 1970s in assessing the revolution’s achievements, failures, and historical significance.

Niess sets the scene with the protests in Berlin that flared up in November 1918 against the Kaiser and the continuation of the war, whereas Käppner opens with a description of the uprising in Kiel. But the second chapters of both books then go back in time to analyse the marginalized condition of the Social Democrats during the Kaiserreich and their conflicted conduct during the war, before providing fluidly written litanies of those events and actors generally considered central to the course of the revolution. These are, briefly, the High Command’s realization during the autumn of 1918 that the war was lost, the resulting ‘revolution from above’ of democratic reforms introduced in October 1918, the spread of the ‘revolution from below’ in the form of Workers’ and Soldiers’ Councils, and the burgeoning conflict between the moderate and radical faces of the


revolution over the new year of 1918/19, which ultimately led to the breakup of the Majority Social Democratic/Independent Social Democratic (USPD) provisional government and the MSPD’s deal with the old elites. The closing chapters of both books depict the growing violence and radicalism of the revolution during the first months of 1919, though Niess’ book also deals with the Kapp Putsch, its aftermath, and the deeply divisive elections of June 1920, while Käppner includes a chapter on the ambivalent impact of the revolution on women (who gained the vote and provided some of the revolution’s leading figures, but who were also generally excluded from the Councils and frequently lost their wartime jobs to returning soldiers.)

The arguments these books present about the revolution would be familiar to anyone conversant with the West German historiography of the 1960s and 1970s. Niess promises to ‘deal with’ several ‘legends’ in his introduction, but these turn out to be the same assumptions that guided much of the conservative historiography of the 1950s, as well as the East German position that the Social Democrats consciously betrayed the revolution. Broadly speaking, in these books, Niess and Käppner provide the same answers to the same set of historical questions addressed half a century ago by historians such as Kolb and Miller; that is, whether or not the Council movement carried a genuine ‘Bolshevik’ threat (it did not), whether or not the Social Democrats were sufficiently aggressive in their attempted democratization of Germany in 1918/19 (they were not), and whether or not this failure to be more reformist paved the way for the Nazis (it did).

All of this, of course, adds up to a version of German history highly redolent of the Sonderweg thesis, according to which the failure of the liberal revolutions of 1848 left Germany with a modern economy and a backward political structure dominated by the old elites, until 1918/19 arrived as a historic chance to thoroughly democratize German society and thereby place the nation on the more agreeably congruent historical trajectory then being traversed by Britain and France. Both Niess and Käppner roll out precisely such a narrative in their chapters on the Social Democrats during the Kaisereich, but there is little mention in either book that, since the late 1980s, this ver-

8 For a useful summation of this literature, see Jürgen Kocka, ‘German History before Hitler: The Debate about the German Sonderweg’, Journal of Contemporary History, 23/1 (1988), 3–16.
sion of German history has been heavily and convincingly called into question, that it now seems clear that both the Kaiserreich and the German Bürgertum were more democratic and ‘advanced’ than the proponents of the Sonderweg theory assumed, and certainly with respect to the supposed bastions of liberal parliamentarism in Britain and France. To be fair, Käppner does fleetingly allude to this position, but he rather breezily dismisses it on the grounds that it constitutes a ‘misjudgement of the power of the old elites’ (p. 49).

With the perceivedly ‘special’ nineteenth century background of the German Revolution in place, both Käppner and Niess then proceed to argue that the SPD failed to enact the necessary democratic reforms in November and December 1918 because of an irrational and exaggerated fear of left-wing, Bolshevik-inspired radicalism and a misguided faith in the trustworthiness of the military. However, the extent to which this fear really was exaggerated remains open to question. The successful radicalization of the revolution under the Bolsheviks in Russia and its horrifying consequences were very much in everyone’s minds in late 1918. That a similar potential for a violent ‘second revolution’ did exist in Germany became abundantly apparent during the course of 1919 and 1920, when some elements of the council movement, disappointed by the SPD’s perceived failure to enact a ‘proper’ revolution, embarked upon a seemingly interminable series of uprisings which punctuated the first five years of the Republic. Some of them involved thousands of armed workers. That these uprisings also came within the context of the Red Army’s highly unsettling progress in Eastern Europe is also significant, and recent research has shown that the Social Democrats, the liberal and conservative media, and much of the German middle classes all feared the apocalyptic possibility of a co-ordinated Bolshevik-style revolution from within and possible Soviet invasion from without.


The importance of the Bolshevik Revolution in framing the MSPD and media’s perceptions of what was happening in Germany is related in Mark Jones, Founding Weimar: Violence and the German Revolution of 1918–1919 (Cambridge, 2016). The author of this Review Article has also written about bürg-
On top of this, Ebert and Scheidemann could have been forgiven for thinking that the war might imminently resume and that the spread of a rather threatening looking Council movement would massively complicate the task of concluding a favourable peace with the Allies.\textsuperscript{11} Only with the benefit of hindsight can we blithely suggest that these fears and concerns were illusory or exaggerated.

In any case, that the principal motivation behind the SPD’s compromise with the old regime was indeed a fear of the revolutionary Left is in itself questionable. During the winter of 1918/19, the Social Democratic government was faced with ostensibly more banal, but potentially more catastrophic, problems even than the threat of a Bolshevik-style uprising, such as the mammoth demobilization which the Allies had demanded be enforced in record time, securing the food supply despite the continuing Allied blockade, reckoning with an imminent housing crisis, and guarding Germany’s eastern borders in the face of possible uprisings and secessions.\textsuperscript{12} Could the government have mastered this incredibly fraught, pressured and uncertain situation, while at the same time ‘thoroughly democratizing’—that is, purging—the military and bureaucracy of those ‘anti-democratic elements’ who had administered both for decades, while reckoning with a revolutionary movement that could conceivably have turned violent? This is highly debatable; at the very least, it would have represented an enormous risk that could have gone badly wrong at immense human cost. As Niess himself points out, Germany was

spared a catastrophic famine during these months, but he does not mention that this was at least partly due to the Social Democratic deal with old elites in the military, bureaucracy, and industry (pp. 188 and 436–7). Käppner, too, fully recognizes the difficulty of the government’s position as dictated by external factors, but he nonetheless describes Ebert’s pact with Groener as the beginning of a ‘nightmare’ for the Republic (p. 220).

Also questionable is the link implied in these books between the SPD’s perceived ‘failure’ to ‘democratically reform’ German society in 1918/19 and Hitler’s so-called ‘seizure of power’ in January 1933. Both books, and the historiographical heritage they tap into, imply a crucial, causal relationship here. As Käppner most forthrightly puts it, the ‘failure’ of the revolution constituted a missed opportunity to remove the ‘old elites’ who ‘delivered the republic over to Hitler’, and thus to ‘prevent Hitler’s tyranny, the war, the road to Auschwitz, and millions of deaths’ (p. 453). Is this a sustainable argument? In the first place, it is rather unfair to depict the ‘elites’ with whom the SPD aligned themselves (especially in the civil service and heavy industry) as homogenously composed of arch-reactionaries bent on the destruction of the republic, as suggested by, for example, the close co-operation between industrialists and trade unionists during the revolution (which Niess acknowledges, but Käppner views with scepticism) or the civil service’s key role in confounding the 1920 Kapp Putsch (which Niess plays down in favour of an unsurprising emphasis on the working-class General Strike).13

However, even if the elites had indeed all been monocle-wearing, moustache-twiddling anti-republicans plotting the enslavement of the working classes and imperialistic wars in dark, smoke-filled rooms, the argument that they were primarily responsible for ‘delivering the republic over to Hitler’ is far-fetched. Such an argument actually conceals a basic, rather Whiggish optimism, that truly democratic societies naturally obviate the potential for a dictatorship such as Hitler’s, and that such dictatorships are, in the final analysis, reactionary constructs battling against democratic forces and the tide of progress. However, anyone who has observed with growing disqui-

13 The burgeoning co-operation between labour and industry, which began during the war itself, as well as the role of the civil service, are the subjects of Conan Fischer, ‘A Very German Revolution?’, German Historical Institute London Bulletin, 28/2 (2006), 6–32.
et political developments in the western world over the last five years surely has good reason to question this rather rosy conception of democracy as something inherently liberal, or the straightforward positing of a battle between (progressive) democratic and (reactionary) undemocratic forces. Indeed, in his introduction, Niess makes mention of the national conservative regimes in Poland and Hungary and implicitly locates them in the camp of ‘anti-democratic’ forces, despite the fact that both currently enjoy resounding popular support and were, in fact, democratically elected.

This (admittedly disturbing) decoupling of the concepts of democracy and liberalism also carries implications for our understanding of Nazism which, though it was, of course, vehemently opposed to a parliamentary system, was also, in the end, the product of a democratic mass society, concerned not with preserving the power of the old elites, but with eliminating, sidelining, or integrating them in the construction of a new type of totalitarian polity based on the putative ‘will of the people’. The unpleasant fact is that the old elites only found themselves in the (from their point of view, rather uncomfortable) position of having to install Hitler as Chancellor because, in free and democratic elections, the NSDAP had become the biggest party in the Reichstag. The Nazis were certainly committed to the destruction of parliamentary democracy, but they also represented what Michael Mann has called the ‘dark side of democracy’, rather than some kind of reactionary force designed to preserve feudal power structures (whose representatives would have much preferred a prolongation of von Papen’s cabinet of barons over Hitler’s disconcertingly rebellious movement.)

This is highly pertinent to the history of the November Revolution because, as Sebastian Haffner pointed out, though Nazism was an avowed enemy of the revolution, it was also the revolution’s off-


spring—a product of the age of democracy and ‘mass society’. The surest proof of this is that, in January 1933, Hitler did not, in fact, recall the Kaiser from his Dutch exile, which rather gives the lie to Niess’s repeated insistence in his conclusion that Hitler’s raison d’être was to ‘roll back’ everything that changed in November 1918. There was a much more complex relationship between the German Revolution and National Socialism than either Niess or Käppner allow for.

Overall, then, these two books provide strikingly similar accounts of the German Revolution based on a tradition of writing which is underpinned by some debatable normative assumptions about the nature of both German history and democracy. To be sure, there are clear differences between the two books: Niess is more optimistic about the achievements of the revolution, emphasizing its positive legacy (the removal of the monarchy, the enfranchisement of women, a genuine parliamentary system) for the later Bundesrepublik, whereas Käppner is more negative, explicitly referring to the revolution as a ‘failure’ and ‘German democracy’ as its ‘big loser’ (p. 19). His version is a more tragic celebration of the courage and sacrifices of those who took to the streets or joined Councils in November and December 1918, but always with a view to the catastrophe that followed after 1933, whereas Niess’s gaze is fixed on the period after 1945.

Nonetheless, whatever their differences, these fluently written and engaging books ultimately convey the same impression as the radical historians of the 1960s and 1970s—that whatever they achieved, and though they were not conscious traitors, the architects of Germany’s forgotten revolution are still in need of forgiveness for their failures and limitations. As Käppner puts it, the Social Democrats ‘gave their best, but their best was not good enough for this revolution, not by a long way’ (p. 461). In fact, however, given everything we have learned about the German Revolution over the last half a century, it would be more reasonable to argue that the revolution actually ‘reflected pretty accurately the reformist potential already present in Wilhelmine society and, beyond that, the more radical ambitions of the democratizing forces in that society’. From the point of view of November 1918, there is nothing to forgive.

17 This is the central argument of Fritzsche, Germans into Nazis.
ALEX BURKHARDT was awarded his Ph.D. in June 2017 by the University of St Andrews. The resulting thesis, ‘Democrats into Nazis: The Radicalisation of the Bürgertum in Hof-an-der-Saale, 1918–1924’, examines the changing political culture of the middle classes in a single German town during the first half-decade of the Weimar Republic. He is currently preparing the manuscript for publication.
BOOK REVIEWS

PETER H. WILSON, The Holy Roman Empire: A Thousand Years of Europe’s History (London: Allen Lane, 2016), lxvi + 942 pp. ISBN 978 1 846 14318 2. £35.00

This history of the Holy Roman Empire by the historian Peter H. Wilson, an Oxford professor since 2015, is a substantial work which addresses both a specialist academic readership and a wider interested public, as the cover makes clear. It depicts a sun breaking through the clouds, used as a symbol of the true faith and later to represent the spread of the Enlightenment. Above it, in a deliberate clash of styles, rides a medieval knight, carrying a yellow double-headed eagle standard, who could have sprung from a popular history website. Most consumers of such new media, however, would find themselves struggling with the breadth and complexity of this book, and the variety of perspectives that it offers.

There is no equivalent history of the Holy Roman Empire from around 800 in recent German historiography. The concept of a ‘thousand year Reich’ is so ideologically tainted that it is almost unimaginable that such a history could be written in Germany by one author. And any history of the Holy Roman Empire written in Germany would, rather, start with the Ottonian line. In addition, the division between the Middle Ages and the early modern period is still very clearly defined in German historiography. Thus we have numerous overviews of the history of the early modern empire—Peter Wilson has also written one (published in 1999), although it is by no means as extensive as that by his colleague, Joachim Whaley, Germany and the Holy Roman Empire (1493–1806).1 But the difference between the medieval and the early modern empire is also strongly marked in Wilson’s new work, reflected in the profound processes of change dating from around 1500 to which he frequently refers, and in the way in which he presents his subject.

Trans. Angela Davies, GHIL.

Both works share a perspective on the history of the empire, but also on German historiography, which is to some degree that of an outsider. Both are very comprehensive. Whaley devotes almost 1,500 pages to the early modern empire, while Wilson writes almost 950 pages on the history of the thousand year Reich. For a reader interested in the early modern period it would be highly instructive to read them alongside each other. Whaley writes the history of an empire becoming ever more ‘German’. Wilson, by contrast, takes a decidedly European approach in which the Holy Roman Empire as the ‘Heart of Europe’ (thus the more appropriate title of the US edition) is presented as a far-flung feudal association (Herrschaftsverband) whose longevity was the result of its flexibility, and without which the history of Europe would be unimaginable. While the structure of Whaley’s volumes, which are divided into many chronological chapters and sub-chapters, makes them seem rather Germanic, Wilson’s table of contents comprises just eighteen words. They could not—seemingly—be more different. In fact, however, Wilson’s book, much like its subject matter, is characterized by a tendency towards particularization and fragmentation, which makes it possible to grasp the main outlines of the interpretation by skimming the text. A quick scan reinforces the impression that despite all conceptual and interpretative differences, the two works share a tendency: they convey a genuinely positive view of the Holy Roman Empire which clearly differs from the older, negative evaluations that still dominate the popular discourse.

Wilson’s book is divided into four parts, each of which is subdivided into three chapters. Some of the terms used as headings are instantly comprehensible, such as ‘Kingship’, ‘Territory’, and ‘Dynasty’ in Part III; others less so, for example, ‘Lands’, ‘Identities’, and ‘Nation’ in Part II. The arrangement of the book reveals the shaping influence of the historian. To write a book with four parts, each with three chapters plus an introduction and a conclusion (‘Afterlife’), is an aesthetic decision. Many people in the late Middle Ages and the early modern period would have associated this structure with Christian symbols with a claim to universal truth.

The book, however, does not follow a ‘holistic’ approach, which would make little sense, and is, in any case, unachievable. Rather, it has a clear orientation towards structural history with a genuinely enlightening aim. The author analyses; he does not narrate. His aim
is not to entertain, but to explain how the empire functioned (well, as a rule, on the basis of consensus-oriented political decision-making processes), why it existed for so long (because of its political and cultural diversity and the associated high potential for inclusion), and what impact it had on the history of Germany and Europe and assessments of them after it came to an end. Rightly, Wilson repeatedly warns against the unreflected use of modern concepts, such as ‘federalism’, in relation to the Old Reich (pp. 8–9). Given the extremely sophisticated level of argument and the stupendous knowledge of the literature displayed here, the discussion of the term *Imperium* (pp. 4–5) is a little disappointing. This could have been important because this term is also misused today for political agitation. It becomes clear, however, that a historiography which took the creation of nation-states as its criterion of historical progress misjudged the empire in evaluating it according to its degree (or lack) of state-formation, or refuting the idea that it was a state at all. The *Sonderweg* thesis is also disposed of in passing, likewise the idea that imperial rule fundamentally declined in the empire from the late Middle Ages.

In Part I (‘Ideal’), which deals with the intellectual and symbolic foundations of the empire, the author emphasizes the significance of the medieval ideal of the rule of two swords, indicated by the symbolic act of Charlemagne’s founding of the empire. Wilson also points to the conflicts which subsequently arose in the relationship between the papacy and the empire as a result of the role of the popes as dispensers of the imperial dignity. The imperial church system established by Charlemagne remained effective as something unique in European history. It was an essential prop of imperial rule and, given the current lack of separation between church and state in Germany, had long-term consequences. The idea of the emperor as the defender of Christianity was fundamentally challenged in the sixteenth century by the religious schism within the empire, although the Reformation and confessionalization are discussed mainly in terms of their political and constitutional consequences. The concept of sovereignty can only conditionally be applied to the first 700 years, which is why it is not introduced in its quality as ‘fragmented and shared’ (p. 171) until the end of Part I. In this context Wilson illustrates how the emperor, other European monarchs, and the empire as an ‘international actor’ (p. 171) had different patterns of behaviour.
and forms of legitimizing foreign policy. These were based on a variety of concepts, as becomes clear if we look at important adversaries such as the French kings or the Ottoman sultans. Wilson particularly emphasizes the role of the Holy Roman Empire, neither expansive nor hegemonic, in securing the peace, although individual actors within the empire certainly pursued hegemonic and expansive interests.

Part II (‘Belonging’) looks at forms and ideas of belonging in a dominion that Wilson describes as a ‘patchwork of lands and people’ (p. 179) without a clear centre. He assumes that the empire’s subjects—to the extent that these are visible beyond the elites—had overlapping, not contradictory, notions of the nation and fatherland. In the concept of ‘multilayered identities’ (p. 252), however, this reviewer misses religious identities, which played an important part in the early modern period, but also earlier, for example, among the Hussites, in combination with the idea of the nation. According to Wilson, the idea of ‘teutsche Freiheit’ (pp. 264–5), limited to the privileged estates, stabilized the empire, among other things because it was strengthened by imperial institutions whose institutionalization, in turn, it advanced. Wilson also regards the innovative potential of the empire as identity-creating based on the structural peculiarities of its constitution. This applies, for example, to its unique variety of periodical and non-periodical print media, and to the higher education sector with its many universities. Wilson sees the main reason for these developments as the high degree of territorial fragmentation and the forced processes of state-building at territorial level (pp. 276–7). Largely because the empire lacked any central representation in the form of architecture, Wilson attributes an important representative function to Reichspublizistik, which, apart from some well-known critical voices, was largely positive (p. 280).

In Part III (‘Governance’), the longest because of the overall importance of the constitutional perspective, Wilson analyses the empire’s important institutions of governance. The main thrust of his argument is revealed in the heading ‘Governance not Government’ (p. 295), which emphasizes the significance of consensual processes of negotiation between those with power in the empire. The emperor did not rule—if he tried, he generally failed—but relied on the consent of other bearers of power in the empire. Wilson emphasizes the transition from a dynastic succession to an imperial electoral constitution and the processes of internal territorialization and differentia-
tion of the feudal system as essential. He rightly sees the processes of reforming the empire around 1500 as crucial. These included the Hoftag gradually giving way to the Reichstag, and the concentration of power and political influence in the empire as reflected in the system of Curia and the distribution of votes in the Reichstag. The idea of a dualism of emperor and empire is rejected; rather, Wilson argues, the dynastically oriented policy of important imperial princes resulted in centrifugal developments. This caused a discrepancy, culminating in the eighteenth century, between the constitutional status of certain imperial Estates and their material power basis, which had clearly grown. A stabilizing effect on the imperial system is attributed to the Peace of Westphalia and the Immerwährender Reichstag as organs of governance. While the perception of the Peace of Westphalia as a ‘catastrophe’ in German history is pretty much out of date now, the pendulum can occasionally swing too far in the other direction.

The title of Part IV (‘Society’) implies a social history of the Holy Roman Empire, something that could hardly be written in the context of this book, as the author himself concedes. The social and economic life of the empire’s subjects was determined primarily by local and increasingly territorial factors, but hardly imperial ones. Here it is striking that medieval developments are often presented in terms of emperors and their dynasties, which is by no means always convincing. Does it really make sense to speak of Carolingian or Staufer society? Wilson emphasizes the variety of local rights and privileges that existed for certain social groups, which he sees as a root of the imperial constitution. This, he says, was primarily directed towards the preservation of traditional structures. The increasing differentiation of society, accompanied by a variety of co-operative and hierarchical forms of organization and rule, is presented as characteristic of the period since the late Middle Ages. Beyond this, an important aspect is the development of urban societies. Processes of oligarchization have been observed in the emergent urban bourgeoisie since the late Middle Ages. Another important aspect is the development of early modern territorial states with central institutions which aimed to assert their rule by imposing norms of behaviour and sanctions. One focus, therefore, is on justice as a consensus-oriented instrument for regulating conflict, as the empire had many legal norms and authorities for the administration of justice. In line with recent research, the
author draws a (somewhat too) positive picture of imperial justice as an authority for keeping the peace, which also represented the interests of underprivileged actors, although with limited success.

The final chapter of Part IV looks at the end of the Holy Roman Empire, which is attributed both to the dynastic ambitions of its leading princes and to the empire’s supposed inability to be reformed, although this has been questioned by recent research. Wilson also emphasizes a change in practices of political communication, which proved to be increasingly incompatible with the empire in its capacity as an association formed by ties of personal allegiance (Personenverband) and an instrument of rule designed to preserve long-standing privileges.

A conclusion looks at the afterlife of the empire in the nineteenth and twentieth centuries. It begins by relativizing the view that the empire disappeared without a trace, which, despite modern research, is still prevalent. After all, many social, political, and legal structures survived the end of the empire. Wilson suggests that it would have ended later without Napoleon, but still during the first half of the nineteenth century. Thereafter the author works through the positive or negative connotations of attempts to update our views of the empire; for example, there were the romanticized constructions of a brilliant empire and a supposedly harmonious medieval society dating from the nineteenth century, and the Third Reich’s rather ambiguous way of dealing with this historical legacy, which was mostly mined for propaganda purposes only by individual actors such as Heinrich Himmler. Current references by politicians to the Holy Roman Empire as a model for European unity, or Charlemagne as its founder are rightly rejected. If the author assumed, however, that the Euroscepticism of Europeans is less than their dissatisfaction with their own governments, the referendum decision in favour of Brexit has taught him better.

The volume has a number of colour illustrations aimed at an anglophone market whose members are likely to be less familiar with the motifs, and a glossary that sensibly does not translate untranslatable German terms. In addition, there is a chronology of important events between the third century and 1806 which cannot necessarily be understood from reading the systematically organized main text. The maps are helpful, although looking at the map of Charlemagne’s empire, one wonders whether this form of representation does not
obscure rather than illuminate the character of the empire as a loose *Personenverband* and a construct of historiography. But these are minor details. Wilson’s *Holy Roman Empire* is an outstanding book: it displays the author’s complete mastery of the huge amount of material and research literature presented; it is original in structure with its pithy central ideas; and it is courageous in its educational impetus, as the author does not hesitate to express his own political opinions. One of the book’s main aims is to show why studying the Holy Roman Empire can offer illuminating perspectives on present-day politics for citizens of the EU (and for those who will soon no longer be). Anyone who is unable to understand this after reading Wilson’s book is beyond help.

HARRIET RUDOLPH is Professor of Early Modern History at the University of Regensburg. She specializes in European political cultures, the history of diplomacy, and material cultures. Her publications include *Eine gelinde Regierungsart: Peinliche Strafjustiz im geistlichen Territorium. Das Hochstift Osnabrück, 1716–1803* (2001); *Das Reich als Ereignis: Formen und Funktionen der Herrschaftsinszenierung bei Kaiserauftritten, 1558–1618* (2011); and (ed. with Astrid von Schlachta) *Reichstadt – Reich – Europa: Neue Perspektiven auf den Immerwährenden Reichstag zu Regensburg (1663–1806)* (2015).
These two books, both from the stable of C. H. Beck and aimed at lay readers, were published to coincide with the 950th anniversary of the Battle of Hastings. Unsurprisingly, this anniversary precipitated commemorations in England too, including the long-awaited publication of David Bates’s biography of William the Conqueror.1 The annual Battle Conference, peripatetic since 2009, returned to Battle itself to mark the occasion. A major conference was also held in Oxford under the title ‘Conquest: 1016, 1066’. This conference, the proceedings of which we await with anticipation, makes explicit the extent to which modern scholarship understands England’s position on the eve of the Conquest as being firmly anchored within the North Sea world. Dominik Waßenhoven, whose scholarly publications include a book on Scandinavians in Europe in the high Middle Ages, and Jörg Peltzer, who declares that ‘one cannot narrate the story of 1066 without 1016’, are both well aware of this trend.2 They embed their own narratives of the Conquest within the wider political context of north-western Europe and both present the Conquest and the events surrounding it with a nuance that is not always found in British popular histories of 1066.

Waßenhoven’s book is part of the Beck ‘Wissen’ series (analogous to the Oxford ‘Very Short Introductions’) and although he manages to pack a great deal into a slender tome, inevitably a narrative of events dominates, with over two-thirds of the book devoted to this delineation. With a book over three times the length, Peltzer has the opportunity to be rather more expansive. His is an altogether more ambitious volume, and while the narrative of events is also prominent, Peltzer is able to broaden the study to draw a vivid picture of the experience of secular and ecclesiastical elites in the eleventh century. In this undertaking the influence of his recent work on aristo-

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1 David Bates, William the Conqueror (New Haven, 2016).
ocratic rank is evident. While both aim at a lay readership these books appeal to different audiences; this is reflected in the inclusion of extensive footnotes and a comprehensive bibliography in Peltzer’s volume. Waßenhoven, by contrast, provides only a short bibliography.

The starting point of both books is the medieval sources. Both authors begin their introductions by familiarizing the reader with relevant chronicles and texts, including the different versions of the Anglo-Saxon Chronicle, works of later chroniclers such as Orderic Vitalis, and celebratory texts, such as the Gesta Guillelmi of William the Conqueror’s chaplain, William of Poitiers. Attention is also drawn to two famous and unique sources: the Domesday Book and the Bayeux Tapestry. The embroidery provides both cover images, almost mandatory on books about the Conquest, and a number of further illustrations. This initial focus on the diverse contemporary sources is indicative of the fact that throughout these books both authors are careful to present the competing descriptions and interpretations of the events as found in the medieval sources. The utilization of chronicles, annals, vitae, poetry, and the embroidery itself, to weave a nuanced account of events is a strength these books have in common. Their final chapters, on the consequences of the Conquest, also share much common ground. However, in the intervening chapters the volumes diverge. While the format of Waßenhoven’s book requires the taking of a rather direct path, Peltzer provides a more scenic itinerary.

Waßenhoven opens his first chapter, on Anglo-Saxons, Norsemen, and Normans, with a description of the St Brice’s Day massacre of 1002. He uses this attention-grabbing event as a stepping stone to take the reader both back in time, to the first Viking incursions of the late eighth century, and forwards in time to the reign of Cnut. In doing so he elegantly explains the complexity of interactions between

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3 In addition to his monograph on the counts palatine of the Rhine, two sets of conference proceedings have been published in English: Jörg Peltzer, Der Rang der Pfalzgrafen bei Rhein: Die Gestaltung der politisch-sozialen Ordnung des Reichs im 13. und 14. Jahrhundert (Ostfildern, 2013); Thorsten Huthwelker, Jörg Peltzer, and Maximilian Wemhorier (eds.), Princely Rank in late Medieval Europe: Trodden Paths and Promising Avenues (Ostfildern, 2011); Jörg Peltzer, Rank and Order: The Formation of Aristocratic Elites in Western and Central Europe, 500–1500 (Ostfildern, 2015).
Anglo-Saxons and Norsemen, emphasizing that the Scandinavians did not merely raid and depart, but settled too, leading to the development of manifold Anglo-Scandinavian identities. He then briefly outlines Edward the Confessor’s upbringing at the Norman court and the manoeuvrings of the Godwin family during his reign as king, before concluding the chapter with the Northumbrian rebellion, as a result of which Harold’s brother Tostig was deposed as earl and exiled to Flanders. Scene set and leading actors introduced, Waßenhoven then moves headlong into the action scenes of 1066 itself.

Peltzer’s book is, by comparison, a slow-burner, with seven chapters devoted to setting the scene before the battles commence. Like Waßenhoven, he opens by examining the connections between England and Scandinavia and the Anglo-Saxons and the Normans. Having situated the Anglo-Saxon kingdom in its North Sea context, Peltzer then describes the social and political structures within the realm. A discussion of Edward the Confessor’s coronation at Easter 1043 illustrates both the international prestige of the English monarch and his pre-eminent position within his own kingdom. Peltzer devotes several pages to explaining the composition of secular and ecclesiastical elites in the Anglo-Saxon kingdom and concludes the chapter with a final section on fighting and military organization. In a memorable allusion to Tolkien, he tells the reader that Anglo-Saxon England was no peaceful hobbit Shire but instead a land characterized by weapons rather than ploughs.

Three biographical chapters provide information about the backgrounds of the main protagonists of 1066. In chapter three Harold Godwinson is presented as a slick political operator with military credentials confirmed by his successful Welsh campaigns. In the following chapter, Peltzer draws a vivid portrait of the life of William of Normandy, from his precarious minority to the duke’s consolidation of his position as the most powerful figure in north-western France. A short chapter on Harold’s excursion to Normandy offers a reading of events grounded firmly in German historiography and as such is of particular interest to an English audience. Peltzer views the events through the lens of symbolic communication, which enables a sophisticated reading of conflicting Norman and English reports of the affair. Further chapters follow covering relations within the Godwin family, particularly Harold and Tostig’s tumultuous fraternal relationship, and describing William’s preparations for the inva-
sion of England. In a final chapter before the three battles of 1066 take centre stage, Peltzer provides a fascinating narrative of the life of Harald Hardrada, whose exploits in Kiev and Constantinople extend the story well beyond the grey skies of the English Channel and the North Sea.

As historians of the Conquest well know, extracting an uncontested and straightforward narrative of the events of 1066, and particularly the Battle of Hastings, from the myriad competing and conflicting contemporary accounts is no easy task. Both Peltzer and Waßenhoven provide lucidly written accounts of the battles of Fulford Gate, Stamford Bridge, and Hastings, while gently leading the reader through the problems and possibilities of the various medieval witnesses to these events. Peltzer carefully constructs his account of the Battle of Stamford Bridge on the shifting sands of the medieval texts, from the cursory report of the Anglo-Saxon Chronicle, through the more decorative accounts of Anglo-Norman authors, to the later Scandinavian sagas, whose accounts most probably reflect the realities of warfare around 1200 rather than those of 1066 itself. In his account of the Battle of Hastings, Waßenhoven likewise informs the reader of divergent details found in different sources. In some instances, he goes beyond merely noting such disparities and considers the purpose of the inclusion of details within the texts themselves. This is the case, for example, where he discusses the unlikely assertion found in both William of Poitiers and the Carmen de Hastingae Proelio that William and Harold exchanged numerous messengers on the eve of the battle. Clear and comprehensive analysis of the medieval sources is a strength of both Peltzer’s and Waßenhoven’s narrative of the key events of 1066.

Both authors draw attention to William’s coronation at Westminster on Christmas Day 1066. Peltzer devotes a short chapter to this event pointing to the importance of this multilingual inauguration, with its imperial pretentions, to consolidating William’s new and elevated position as a ruler over multiple peoples. That the use of more than one language at the acclamation precipitated panic and violence, when the guards stationed outside Westminster Abbey mistook the shouts as signs of trouble, is indicative of the fact that William’s coronation did not mark the conclusion of his conquest. Accordingly, resistance to the Norman monarch’s rule is discussed by both Peltzer and Waßenhoven. Waßenhoven focuses on the
decade after the Conquest, in which uprisings broke out in the Welsh borders, Devon, Northumbria, and East Anglia, where resistance centred on Ely. In his discussion of the figure of Hereward, who escaped from Ely before William crushed the rebels gathered there, Waßenhoven once again brings the medieval sources into focus. His chapter on resistance to William’s rule concludes with the 1076 rebellion ‘of the three earls’. That the earls in question were an Englishman, an Anglo-Breton, and a Norman shows that this uprising was no simple rejection of Norman rule. Indeed, as Waßenhoven points out, there was never any co-ordinated national Anglo-Saxon resistance, but a series of regional uprisings that remained isolated thanks in no small part to William’s swift actions. Peltzer’s coverage continues until William’s death in 1087, which enables him to pay increased attention to the continuing Danish threat and to that great monument of Norman rule: the Domesday Book. Waßenhoven briefly discusses this survey in a concise and well-constructed chapter entitled ‘Legitimation and Interpretation’, in which he also tackles Norman justifications of the Conquest.

The final chapters of both books are devoted to discussion of the consequences of the Conquest. The authors are in almost complete agreement on the main outcomes, as is reflected in the similarity of their chapter titles and subtitles. Both Peltzer and Waßenhoven consider the effect on land and people, particularly the almost complete eclipsing of the Anglo-Saxon elite, both lay and ecclesiastical, by newcomers from Normandy. They also outline the architectural and linguistic effects of the Conquest epitomized by the building of castles and cathedrals and the dominance of French as the language of the royal court. Waßenhoven points to the difference in the linguistic origins of words used to describe animals and their culinary manifestations (cow/beef; sheep/mutton; etc.) to draw a distinction between Anglo-Saxon producers and Norman consumers. This is an indication that for those below the level of the elite, life continued more or less as usual after the initial shocks of 1066 had subsided. Peltzer argues that the Conquest, for the most part, accelerated changes rather than initiating them—church reform, chivalry, and Romanesque architecture had already begun to cross the English Channel before 1066.

In his introduction Peltzer explains to his intended German lay audience that everyone who grows up in England grows up with
1066. He draws a comparison with German knowledge of the events of 1077, when Henry IV sought the forgiveness of Pope Gregory VII at Canossa. In German scholarship ‘Canossa’, while the dramatic meeting itself still excites attention, has come to stand for a whole process of change across the second half of the eleventh century. Both Peltzer and Waßenhoven present the events of 1066 as momentous in themselves, but also as indicative of broader currents of change sweeping across the continent. Hastings and Canossa are thus not merely events of national interest, but of European importance. This reviewer grew up with 1066, a momentous year in ‘Our Island Story’, whose European significance was rarely alluded to. These two books, written by historians who grew up outside this insular tradition, present 1066 as of intrinsic interest and relevance to their continental lay audience. While Waßenhoven provides a whistle-stop tour and Peltzer a more leisurely exploration, both authors deploy their knowledge of medieval and modern historiographies to guide the reader through the events, interpretations, and consequences of the Conquest with clarity and verve.


JOHANNA DALE joined UCL History as a British Academy Postdoctoral Fellow in September 2016. She has previously worked in the Department of German at the University of Cambridge and in the Department of History at the University of Heidelberg. She is interested in the intersection of political and cultural history in the medieval period, with a particular focus on British and German-speaking lands. Her Ph.D. comprised a comparative study of the influence of coronation liturgy on images of kingship in England, France and the Empire, c.1050–c.1250.
Throughout the tenth century the Babylonian Talmud was introduced, step by step, into Europe. Soon several centres of rabbinical learning emerged at a number of places in Europe. With R. Shlomo ben Yitzhaq (better known as Rashi, c.1040–1105) the process of textualizing rabbinical knowledge reached its first climax and the Talmud became the standard written reference work for Jewish learning and jurisdiction. Soon copies of the Talmud with Rashi’s comments were circulated. The next generation of scholars, the so-called Tosafists, added their own explanations to the Talmud and to Rashi’s comments. One of the last authorities of these Tosafists was Rabbi Meir ben Baruch of Rothenburg (also known as MaHaRaM). R. Meir was born around or after 1220 in Worms, which at that time was still one of the centres of Jewish learning in Ashkenas (that is, the areas of German-speaking Judaism). In his writings he mentions twelve of his ancestors who were also rabbis. R. Meir studied Talmudic law and philosophy in the tradition of Moses Maimonides in Würzburg, Mainz, and Paris. It is quite likely that R. Meir was an eyewitness to the burning of the Talmud in Paris in 1242; at least he wrote a poem (piyyut) about this incident which, to the present day, forms part of the synagogue liturgy of the Tenth Av (the date on which the two Temples were destroyed in 587 BCE and 70 CE). In it he expressed grief for the loss and raised the question of God’s will. After his return to Germany R. Meir founded his own school at Rothenburg ob der Tauber. He became one of the leading rabbinical authorities of his time and his comments on some Talmudic treatises became part of the Tosafists’ comments on the Talmud. He also wrote more than 1,500 halakhic responsa (juridical decisions) which he or his students collected and transmitted as one collection. In 1286 R. Meir fled from Germany, but was arrested and imprisoned by the German king Rudolf I. On 27 April 1293 he died as the king’s prisoner in Wasserburg am Inn. After payment of a hefty ransom in 1307, his bones were transferred to Mainz, where his tombstone is still visible today.

The author of the present study, which was submitted as a Ph.D. thesis at Tel Aviv University and originally written in Hebrew, is less
interested in biographical information than in R. Meir’s teachings
and the underlying theo-philosophical system, or, to be more precise,
the political theory connected with R. Meir’s legal theory (p. 19). He
therefore proceeds in three steps. After introducing the historical
context of R. Meir’s teachings and identifying his authentic writings,
he scrutinizes his political theory, which comprises two major aspects.
First, he demonstrates that for R. Meir the Jewish congregation was a
community of individual partnership, but also a sacred community
(qahal qaddosh). Second, he expounds R. Meir’s theological under-
standing of that community: every violation of a law regarding the
community is also a violation of God’s unity. Although the research
hypothesis is not new and has been mentioned by several scholars of
R. Meir, Lifshitz provides a careful and in-depth analysis. His book
will be the standard reference for R. Meir’s political philosophy.

This notwithstanding, some points of criticism have to be men-
tioned. First, in the Hebrew version Lifshitz used the vocalized text
of the earlier editions as well as of the manuscripts. The English
translation, however, does not quote the Hebrew texts. Confusingly
for the reader, information concerning the Hebrew used in the origi-
nal version is left in the translated text (pp. 22–3). Here and in simi-
lar cases more careful editing would have been desirable. This also
applies to the transcription of Hebrew words and terms, for example,
‘agadah’ (p. 23) vs. ‘aggada’ (p. 24), ‘agudah ehat’ (throughout the book)
instead of the correct ‘agguda ehad’, and so on. Second, not all titles
mentioned in the footnotes appear in the bibliography (for example,
p. 15 n. 34). The bibliography itself contains many mistakes. For exam-
ple, the primary sources are divided into Hebrew and English
sources, but the English sources are not, in fact, originally written in
English, but English translations of Greek sources (p. 251). Among the
Hebrew secondary literature, translations from the French and from
the English are mentioned (for example, Graetz and Le Goff). Among
the English secondary literature, German titles are listed (Germania
Judaica; Güdemann; Zimmels), although there is a separate heading
for German secondary sources. Throughout the text and the bibliog-
raphy German place names in particular, but also book titles and
names of authors are given incorrectly (for example, on p. 36 ‘Bohn’
and ‘Meersburg’ instead of ‘Bonn’ and ‘Merseburg’). Careful copy-
editing by the publisher would have been indispensable.
GÖRGE K. HASSELHOFF is a Privatdozent at the Technische Universität Dortmund. He is working on Christian–Jewish relations throughout the ages, focusing mainly on the Middle Ages and on Latin translations of Jewish writings (Talmud, Rashi, Maimonides, etc.). Recent publications include Ramon Martí’s ‘Pugio fidei’: Texts and Studies (ed. with Alexander Fidora, 2017) and Religio licita? Rom und die Juden (ed. with Meret Strothmann, 2017).
MATTHIAS POHLIG, Marlboroughs Geheimnis: Strukturen und Funktionen der Informationsgewinnung im Spanischen Erbfolgekrieg, Externa: Geschichte der Außenbeziehungen in neuen Perspektiven, 10 (Cologne: Böhlau Verlag, 2016), 457 pp. ISBN 978 3 412 50550 9. €60.00 (hardcover)

A reader who only casually looks at this volume in a bookshop might well mistake both its importance and its subject. One could mistakenly assume that the issue at hand is an investigation into a well-worn topic in British political history. Both the opening and closing sections of Matthias Pohlig’s book about ‘Marlborough’s Secret’ refer to the events during the autumn of 1711 that culminated with the dismissal of John Churchill, first Duke of Marlborough, from his high military, diplomatic, political, and Court positions. At the time, Marlborough was one of the best-known individuals in the kingdom as the most senior military officer of the nation. He was simultaneously Captain-General of British forces on the Continent and allied forces in the southern Netherlands, the British ambassador to the Dutch Republic, as well as close personal adviser to Queen Anne and a member of her cabinet council. At that time a Parliamentary Commission was investigating charges that Marlborough had improperly used public funds intended for feeding allied troops and had diverted them for his personal use.

Some historians and biographers have studied this political controversy, but that is not the subject of this book. The relevance of the 1711 event is not the charges against Marlborough, but his defence when he explained that these funds were used to gather intelligence. Pohlig has examined the wider subject that Marlborough raised in his defence and has used it as the focal point to make insightful use of Marlborough’s massive collection of Blenheim Papers at the British Library. He has also used complementary manuscript materials held at the National Archives in Kew and the British Postal Museum and Archive in London to document a new, innovative, scholarly understanding of the structure and function of information gathering during the War of the Spanish Succession. Even more, Pohlig brings to his study an in-depth understanding of military intelligence, information-gathering, and information exchange that makes this book of great significance to the general history and practice of such matters.
The book is a revised version of Pohlig’s 2015 *Habilitationsschrift* at Westfälische Wilhelms-Universität in Münster. The author has divided the book into seven parts. The first defines the book’s themes, concepts, and methodology. The second provides an excellent summary of the background to the war and to Marlborough as well as to British domestic and foreign politics and the machinery of British governmental organization and decision-making. This discussion is very firmly grounded in an impressively thorough grasp of the scholarly literature in several languages. The final sections of the book contain the list of archival sources and the extensive bibliography in part six as well as an index of personal names in part seven. The heart of the book lies in the 214 pages devoted to the structure of information-gathering in part three and the seventy pages in part four on the functions of information. A five-page general conclusion to the entire study follows in part five.

Pohlig’s thorough study of the structure of information-gathering demonstrates the wide range of sources that Marlborough used. He begins his examination of this topic with a discussion of the general conditions, including the financing involved, the importance of reading and writing letters in this period, the international postal system and packet boats from England to the Continent, and the use of maps. With government organization, one sees Marlborough’s relationship with the secretaries of state and other ambassadors, as well as the use of spies, interception of postal letters, and the deciphering efforts of the Post Office. He then turns to focus on two individuals, John Mackay and John Fonseca, who undertook espionage initiatives for the secretaries of state. Complementary to them were the reports from diplomats and military leaders. Marlborough also dealt in what Pohlig terms ‘the grey zone’ of formal diplomacy through his contacts with foreign officials such as Anthony Heinsius in the Dutch Republic, Colonel F. W. Grumbkow in Brandenburg-Prussia, and the Hanoverian secretary Jean de Robethon. A further, but subsidiary and often marginal, complement to these sources of information was the newspaper press and the handwritten news letters. While often having a distinct propaganda value, the print media spread information relatively quickly and provided notice of trends that could be confirmed later through private correspondence.

Turning to the subject of the functions of information, Pohlig first summarizes the theoretical aspects of the issue. He then goes on to
discuss the disputed role of information in the decision-making process, and also examines the function of information in patronage and representations of competence and legitimacy. Pohlig concludes that information-gathering was a central element in the political, diplomatic, and military practice of the early 1700s, showing that the War of the Spanish Succession was, in some respects, an information war. The emphasis on information-gathering led to the creation of specific types of organizations, infrastructures, and networks. At the same time, Marlborough’s information system was not independent and private, but was closely tied to and interwoven with the machinery of the English government. The role of information in decision-making involved the minimization of uncertainty as well as contributing a means of supporting the competence of key leaders.

Pohlig concludes his important contribution to the scholarship on the subject with the observation that Marlborough associated the various methods of information-gathering with the different requirements of his formal and informal positions, regarding both the British government and the Grand Alliance. It involved an understanding of his character combined with an understanding of the governmental process in which he was involved. In these ways, Pohlig’s book makes a highly significant scholarly contribution to understanding Marlborough and his times as well as providing an excellent case study of the structure and functions of information-gathering.

JOHN B. HATTENDORF is Ernest J. King Professor Emeritus of Maritime History at the US Naval War College, where he held that chair from 1984 to 2016. Additionally, he served as chairman of the College’s Maritime History Department and director of the Naval War College Museum from 2003 to 2016. The recipient of many awards, he is the author or editor of more than forty books, including the Oxford Encyclopedia of Maritime History (2007). In addition to his work in maritime history, Hattendorf is the author of English Grand Strategy in the War of the Spanish Succession (1987) and the editor-in-chief of Marlborough: Soldier and Statesman (2012).
This review looks at the first two volumes of Mark Hewitson’s projected trilogy, which deal with the violence of war in Germany from 1792 to 1888. The third volume has not yet been published, but has already been announced under the title *The Violence of War: Germany, 1888–1968*. The trilogy argues that wars and the violence associated with them played a crucial part in transforming German states and societies in modern history. Although peacetime clearly outweighed wartime in the period covered, warlike conflicts in the German lands in the nineteenth and twentieth centuries generally had a wide-ranging and long-term impact, and often represented a turning point for Europe as a whole. The attitudes of soldiers and civilians towards violence and war as shaped by the violence of war at that time, as well as contemporary concepts of war influenced by these attitudes, Hewitson argues, were directly connected to Germany’s changing readiness to go to war and the policies this inspired.

The first volume, *Absolute War: Violence and Mass Warfare in the German Lands, 1792–1820*, investigates the impact of the Revolutionary and Napoleonic Wars on the millions of people directly affected in the German lands. Hewitson covers both Prussia and Austria, as well as the middling and small German states. His analysis is based on a large variety of published sources (neither volume draws on archival documents), including letters, diaries, memoirs, official correspondence, press reports, pamphlets, essays, plays, and cartoons.

Hewitson uses the term ‘absolute war’ with reference to Clausewitz and in contrast to ‘total war’, which he reserves for the twentieth century. According to Hewitson, the term ‘absolute war’ indicates that conscription, which was practised by all German armies during the Napoleonic Wars, made these wars into ‘people’s wars’. Conscription, which resulted in 60 per cent of men in the period 1813
to 1816 being called up—an even higher percentage than at the beginning of the First World War, when the figure was 55 per cent—he suggests, turned war into a mass experience. Added to this was that many German states were theatres of war, which meant that large numbers of civilians shared the experiences of violence, killing, and dying associated with war. Another factor in intensifying the war experience, Hewitson suggests, was that survival rates during the Napoleonic Wars were similar to those in the First World War, clearly lower than in the other nineteenth-century conflicts. The direct involvement of a majority of the population, he argues, means that the period was essentially shaped by war. Hewitson rejects the view put forward by Ute Planert, Leighton James, and others that the military conflicts of the Napoleonic era were essentially a continuation of early modern warfare and the state-building wars of the time as they had little national charge and the practices of war and their consequences had not fundamentally changed. On the contrary, Hewitson suggests that in the period from 1792 to 1815 the majority of German states underwent a military revolution.

In support of his views Hewitson outlines in detail the new forms of violence that developed during the conflicts. Warfare, he claims, had changed fundamentally since the eighteenth century as a result of conscription, quicker movement, and the greater weight given to battles. He also describes comprehensively how the war experiences of violence, killing, and dying, shared by many soldiers and civilians, influenced broader debates about military conflict, armies, and military policy during both wartime and peacetime. He argues that the Napoleonic Wars had changed the parameters within which the Germans saw military conflict, as the images of war in the public discourse responded to contemporary experiences of war. Hewitson points out that this insight is often dismissed in the research as self-evident, but suggests that its consequences have not yet been adequately considered. Fear, terror, and disgust were the formative emotions during war. Despite their instinctive rejection, however, the majority of people had got used to their circumstances. Their ongoing experience of war meant that people’s hopes of a lasting peace had dwindled. As a result, ‘people’s war’ was now seen as a dangerous but necessary means by which sovereign states conducted policy. Consequently, attempts to restructure armies into mass armies were widely accepted in almost all German states.
Hewitson divides the volume *Absolute War* into five chapters. Chapter one looks at developments from cabinet warfare to mass armies. He takes a critical look at the concepts of ‘total war’ and ‘military revolution’, and investigates the impact of the *levée en masse* and compulsory military service on the conduct of war in terms of tactics and strategy, and its ideological transformation. Chapter two analyses public opinion about the war and contemporary justifications. Hewitson cites philosophers, artists, and journalists, and considers to what extent the people at the time participated in the public discourse. Chapter three looks at the role played by violence in civilian life during the period under investigation. Here Hewitson highlights the significance of the internal pacification of the German states since the eighteenth century which, by the beginning of the nineteenth, had clearly altered attitudes to pain, injury, and death. Chapter four looks at the lifeworlds of soldiers during the Revolutionary and Napoleonic Wars, drawing primarily on soldiers’ memoirs. Hewitson stresses the escalation of violence during the Napoleonic Wars, which was also reflected in ego documents. After 1805 in particular, soldiers’ war reports were characterized less by heroism than by mourning for the suffering of war. The final, fifth chapter looks at the culture of remembering the Revolutionary and Napoleonic Wars in the period of peace that followed up to 1820, at the ideas and patterns of interpretation that were predominant at the time, and at connections with the existing post-war order.

Hewitson’s study of the impact of the Napoleonic Wars on the German states convincingly presents the experience of combat and conscription as the central parameter in the thinking of contemporaries. Drawing on many examples, he demonstrates that the threat of violence in war and the high human and financial costs of war between mass armies was of central importance in contemporary thought. The connection between warfare and patriotism and nationalism, by contrast, was only of secondary importance for contemporaries.

In his second volume, *The People’s War: Histories of Violence in the German Lands, 1820–1888*, Hewitson presents a longitudinal study of how warfare was perceived in the German lands during the nineteenth century. His starting point is the argument that as the most violent military conflicts in European history up to that point, the Napoleonic Wars had a long-term impact on individuals and on
political developments that has so far been underestimated and hardly researched.

The most remarkable thing about the military conflicts and international crises since 1848, he suggests, is that conscripted soldiers and volunteers were always extremely willing to go to war, regardless of whether the conflict was seen as part of the national movement or not. Hewitson explains this by pointing out that the aims or origins of a conflict were not a decisive factor in shaping soldiers’ ideas of a legitimate war. As a result, the role of nationalism in maintaining morale among the soldiers was limited. Rather, war and the army were generally accepted by wider society as integral, legitimate components of policy as conducted by sovereign states. Contemporaries accepted the necessity of conscription and ‘people’s wars’ as a political means, and obediently performed their military service—a notion that had become established during the Napoleonic Wars, as Hewitson shows in the first volume of his trilogy. The social acceptance of war and the army in the nineteenth century represented a fundamental change by comparison with the eighteenth century, when resistance to military recruitment and mobilization had been much more common. This insight has not, as such, been discussed in the historiography so far.

Hewitson investigates German attitudes to the violence and suffering of war in the wake of the mass warfare of the years from 1792 to 1815, and the ideas and expectations of future military conflicts derived from them. He takes a long overview of the years from 1820 to 1888, which were marked by long periods of peace, and shorter phases of warfare. He justifies this by pointing out that only by looking at several generations is it possible to work out how the contemporary image of warfare changed as the result of the interplay and transformation of memory, emotions, experience, politics, institutions, events, and media.

In this case Hewitson’s sources are contemporary press reports, war literature, paintings, lithographs, and photographs. Drawing on them, he asks how ministers, journalists, academics, artists, and ordinary people in the German states imagined war during the nineteenth century. Hewitson evaluates contemporary memoirs of the Revolutionary and Napoleonic Wars and relates them to press reports about wars that took place elsewhere in Europe and overseas, to political debates about compulsory military service and military
reforms in the German states, to experiences of revolution and counter-revolution, and to individuals’ everyday experiences of violence and death. In addition, he analyses the reactions of soldiers and civilians to military conflicts in which their own states were involved during the period of investigation. It becomes clear that for most of the population of the German states, memories of past wars were also connected with expectations, hopes, and fears relating to the future.

Hewitson divides his volume *The People’s War* into two sections, entitled ‘The Romance of War, 1820–64’ and ‘The Horror of War, 1864–88’. Both sections contain five chapters. He explains the division into two by pointing out that the wars between 1805 and 1815 had a fundamentally different impact from those between 1864 and 1871. After 1815 a romantic, heroic narrative of the Napoleonic Wars established itself. It was spread by the authorities and the press, and also shaped ideas about a future war right up to the wars of unification. This did not have much to do with German nationalism, or the connection between warfare and nationalism, although research has so far concentrated on this connection. Rather, it can be attributed to the fact that in the years after 1815 the liberal myth of the ‘wars of liberation’ was widely accepted by the reading public. This myth overlaid painful memories of the war partly because after 1815, as a result of censorship, the testimonies of veterans which focused on the sufferings of war were less visible in public than the later testimonies of the veterans of the wars of unification. The late publication of most war memoirs also played a part here, as did the fact that many soldiers did not return from the war of 1812, which meant that they could not shape the discourse of memory.

The section entitled ‘The Romance of War, 1820–64’ starts with a chapter on contemporary narratives about military conflicts. The next chapter deals with daily life in the German armies in peacetime. In chapter three Hewitson analyses the significance of violence in the lives of civilians at the time, while in chapter four he investigates acts of violence and military operations connected with revolution and counter-revolution. In the final chapter of this section Hewitson examines war reports about the military conflicts during the period 1820 to 1864.

The second section, entitled ‘The Horror of War, 1864–88’, points to the fact that the romantic images of war dominant before 1864 had little in common with the brutal realities of war during the wars of
unification. In addition, while civilian life was increasingly pacified, military technologies of killing were becoming ever more destructive. This contrast reached a peak during the wars of unification. After they had experienced modern war in practice, many soldiers were clearly much less willing to continue fighting, or to go to war in future. While the press continued to disseminate heroic images of battles along with national justifications of the war, veterans left testimonies in which the horror of war was a leitmotiv and romantic notions of war were rejected. Unlike in the years after the Napoleonic Wars, in the decades after the Franco-German war of 1870–71, the reports and memoirs of veterans in which fear, disgust, sorrow, and grief were openly expressed as the formative experiences of war were juxtaposed with the narrative of a heroic national struggle, which dominated the press. This meant that in Imperial Germany, post-war discussions about the military conflicts were characterized by controversial interpretations and consisted of contradictory expectations of a future war. Thus until 1914, ambivalent ideas of war dominated Imperial Germany, uniting hopes of a national confrontation with scepticism about romantic notions of war.

Hewitson begins the second section with chapter six, in which he investigates political mobilization and public campaigns during the wars of unification. Chapter seven looks at contemporary justifications of military conflicts and wartime violence. Chapters eight and nine each deal with the experiences of war and violence in a specific armed conflict, the wars of 1866 and 1870–71 respectively. The final chapter turns to the culture of remembrance relating to the wars of unification, and the violence perpetrated at the time.

These two volumes are clearly structured, fluently written, and open up a broad panorama of contemporary German views on violence and warfare. In addition to historical analysis, Hewitson provides a treatise on classical and current theories of violence, critically reviewing the ideas of sociologists, philosophers, psychologists, and historians. Beyond this, the volumes provide a good survey of the older and more recent historiographical debates on the wars of liberation, the meaning of the concept of the citizen–soldier, and military and political continuities and discontinuities from the eighteenth to the nineteenth century. In all this, Hewitson reveals the potential inherent in approaching a general history of Germany via the history of wartime violence. As far as developing his argument goes,
Hewitson is less interested in regional differences than in presenting an overall picture which reveals large developments and major changes. This means that at times, regional differences in the German states do not get as much attention as they perhaps deserve. But Hewitson makes up for this by presenting clear opinions which any future studies on the subject will have to address.

An index of names, places, and subjects completes each volume. To conclude on a positive note, it should be said that instead of a simple bibliography, *The People’s War* contains an annotated and thematic review of current literature, which provides easy access to the state of international research. We can look forward to the third volume in the trilogy.

GUNDULA GAHLEN is working on the project ‘Mentally Traumatized Officers of World War One in Germany’ at the Friedrich-Meinecke-Institut, Freie Universität Berlin. She is the author of *Das bayrische Offizierskorps 1815–1866* (2011).

This concise work draws attention to something that has long been overlooked in historical studies. After the Congress of Vienna, European politics were dominated by the major powers. Yet the smaller states in particular continued to stake their own claims to political agency in a variety of ways, while counting on public support to achieve their goals. This deserves a detailed analysis, and perhaps the most fruitful approach is by way of the middling and smaller-sized states of the German Confederation, who themselves were undeniably actors on the European stage.

A number of recent studies have emphasized the important role of this ‘Third Germany’ in creating the German nation-state in the nineteenth century. In focusing on the active nation-building policies of the German middling states, such studies swim against a teleological tide of thought which long held that only Prussia was capable of heeding the call to a nation-building destiny. They thus form the inspiration for this book about the Kingdom of Württemberg, which sets out to reveal the ‘process of nation-building in action’ (p. 2), or, more precisely, the role played by the Kingdom of Württemberg in the creation of a German nation-state, something that has been under-estimated in the past.

The book’s succinct account of this process may perhaps be best understood as a prologue to future studies, rather than as setting a benchmark in terms of method or content. If we measure the work against its own aspirations—that is, to explore how new identities were created through an analysis focused on social movements or ‘masses of the public’ (p. 6), rather than on individuals—then it is bound to fall short. One immediately obvious problem lies in the author’s selection of source materials, which consist primarily of accounts (including some from a British perspective) by leading statesmen and diplomats, while findings from the highly diverse national movements are almost entirely ignored. Primary sources include government records from Berlin, Darmstadt, Karlsruhe, London, and, especially, Stuttgart; over thirty contemporary news-

Trans. Emily Richards (GHIL).
papers from various states; a variety of contemporary speeches, letters, and diaries; and, finally, all manner of relevant printed materials. As a result, it is impossible to carry out a systematic interrogation of the historical material within this study’s narrow confines. Quotations from the sources are used primarily to illustrate a collection of insights gained from secondary literature, which themselves form the basic premise for the author’s argument.

The book is structured chronologically. Beginning at the end of the eighteenth century, it summarizes some of the well-known ideas about nationhood circulating towards the end of the ancien régime, for example, those explored by Moser, Schiller, or Goethe. However, it ignores another star of Weimar whose thoughts on this topic were probably even more influential—Herder, with whom originated the powerful concept of a *Kulturnation*, a nation defined by its culture. From this starting point, the book’s focus quickly turns to Napoleonic Württemberg and the successful defence of its sovereignty orchestrated by Friedrich II/I as duke, elector, and finally king of an immensely strengthened middling state, a success attributed by the author to Friedrich’s artistry in balancing national and state interests (p. 32).

The next chapter, on the consolidation of the Württemberg state after Napoleon’s defeat, is devoted to the Congress of Vienna (along with the cautionary tale of Saxony’s near-total destruction) and the contemporaneous constitutional battles of 1815 to 1819. The south German regionalism of the time is seen by the author as arising out of attempts at economic modernization, and he views the creation of the South German Customs Union (*Süddeutscher Zollverein*) as a significant milestone towards the Third Germany. Württemberg’s provocative reactions to Metternich’s policies, such as its reluctant implementation of the Carlsbad Decrees or its appointment of arch-liberal Wangenheim as its representative at the Frankfurt National Assembly, reflected its political ambitions within the German Confederation. Such enterprises sometimes failed, but had long-term effects, even beyond Württemberg, which here is intended to represent a ‘microcosm for these national ideas’ (p. 48).

The nation-building project, something originally confined to an elite, slowly began to gain considerable popularity, for example, in the form of the Schiller Festivals held on the anniversary of the poet’s death. But this appears only as a marginal point at the beginning of
the next chapter, which sketches the various attempts, from the Congress of Vienna to the 1848 revolution, to create a German nation. Württemberg experienced opposition, for example from List, Pfizer, Mohl, and Römer; but Württemberg also practised opposition, for example to the Carlsbad Decrees (p. 57). On the basis of these examples, the author argues that a greater German solution was by no means the only option. Even Metternich attempted at one point to take on the role of advocate for the middling states, if only in order to avert the creation of a unified German nation at the last minute. When such plans were made, it was always with an eye to international relations; fear of renewed conflict with France was particularly intense in the early 1830s and again during the 1840s. Austria was so slow to react to this situation that it was inevitable that Württemberg’s gaze should instead fall upon Berlin; not because of any natural political affinity, but out of calculated political necessity. After all, Württemberg’s main objective was an agreement with the other German middling states, whose military resources were limited.

This is also considered in the section of the book dealing with the revolutionary years of 1848/49, which starts by looking at why Württemberg itself never experienced any radical uprising. The reason, according to the author, was that Wilhelm I was careful to instil in his people his own anxiety about the dangers that came from abroad, rather than within, insisting that a national political solution was needed that had to include Prussia and Austria (p. 76). Any opposition to this on the home front was nipped in the bud by the appointment of Friedrich Römer, while the king himself was active as one of the pre-eminent supporters of unification. Curiously, the author pays no particular attention to the fact that Württemberg was the first state to recognize the Frankfurt Constitution, all the more so since Stuttgart—albeit only for a short time—provided a home for the Paulskirche rump parliament, where those convicted of high treason in other German Confederation states were able to continue their nation-building efforts. Considering the implications of this, one can imagine that if things had turned out differently, Wilhelm I might have gone down in history as midwife to the nation.

The next section, examining the upheavals of the 1850s, shows how nation-building continued to play a prominent part in Württemberg politics. It begins with the failure of the Erfurt Union, a plan for unification initiated by Prussia, but sadly fails to enlighten the
reader as to Wilhelm I’s attitude to this project for north German regional unification in opposition to the German Confederation (p. 87). This is a pity, as even during the Hesse-Kassel crisis the middling state of Württemberg continued to vaunt itself as the protector of middling state interests (p. 89) and, thanks to its constitution, as the model for a liberal and constitutional Third Germany. In this respect, it was not dissimilar to Bavaria, which liked to see itself as the most progressive state in this regard (and it would have been helpful if the book had included a short discussion of the rivalry between Bavaria and Württemberg; this could have shed light on their competing claims to be the leading state when it came to German unification). Württemberg now even began to present itself as a player on the world stage, bringing its own banner to the presentation of the German Customs Union (Zollverein) at the London World Fair in 1851 to lend visibility to its international ambitions (pp. 93–4). Furthermore, the author argues, Wilhelm I himself, aware of his own increasing age, and conscious of the fact that time was moving on—and with only a low opinion of his son, the heir to the throne—decided to make one last stand as a pioneer of national unification. But his hopes of using the Italian war of unification and, specifically, French opposition to Austria, to this end, were, of course, in vain; the idea of a Third Germany collapsed once again in 1859 (p. 101).

Nonetheless, it was precisely the middling states that took further steps along the path to German nationhood in the years before 1866, at the same time as industrialization and mechanization were now profiting from the Zollverein. Yet during this period it also became apparent that economic modernization had its limitations, as did the ability of the state to defend itself. These concerns, among others, led to the Wurzburg Conferences organized by the middling states. However, these were initiated neither by Württemberg nor by Bavaria, but by Saxony—another rival for the title of most important middling power—in the person of its foreign minister Friedrich von Beust. Plans to reform the confederal army became plans to reform the confederation itself, but without any tangible outcome; any potential reforms were blocked by the deadlock caused by Saxony’s, Bavaria’s and Württemberg’s competing claims to leadership (p. 113).

Other groups and associations that played a significant early role in the creation of the German idea, such as the League of Singers, itself active in Württemberg, have thus far gone without mention.
But the book does now turn its attention to the German National Association. This movement had successes, for example, in Esslingen, and it was tolerated, perhaps even regarded with sympathy, by the government (p. 116), in spite of its failure to abide by the Carlsbad Decrees. The discussion of the Union sets the stage for two new protagonists—Bismarck, the Prussian minister president, and Karl von Varnbüler, Württemberg’s foreign minister who, despite his initial opposition to Bismarck’s national policies, turned out to be just as flexible, if not as cunning, a politician as Bismarck himself. Varnbüler had not yet given up hope of the Third Germany, believing that this could only be achieved by reining in Prussia’s hegemonic ambitions and supporting Austria in the Schleswig-Holstein crisis. This was the reason for the alliance formed in the German Civil War of 1866, which, however, ended in unexpected defeat (p. 124).

The next section of the book describes the forming of the German nation state between 1866 and 1871, showing that Württemberg’s ultimately pro-Austrian policy was the result of sophisticated calculation (p. 127). The author sees evidence for this in Württemberg’s ‘ineffecual and lethargic’ (p. 128) participation in the Prussian-German War, and its negotiations to create a south German alliance in (mistaken) anticipation of a military stalemate between Prussia and Austria (p. 136). But the creation of the North German Confederation meant that the Third Germany was now reduced to a south German rump, with leadership now falling to Bavaria. Eventually Württemberg, too, gave in to the irresistible pull of unification—which need not necessarily be ascribed to any ‘backwardness’ of the south (p. 139)—partly also because of its vulnerability in relation to France. As a result, it moved closer towards Prussia, not least by entering into a ‘Schutz und Trutz’ (Protection and Defence) alliance with Prussia in 1866.

The last chapter offers a brief perspective on Württemberg’s continued existence after unification, both as a state in Imperial Germany with numerous special prerogatives, and as an idea. Many Württembergers celebrated themselves as victors rather than losers in the unification process (p. 151), even though one may justifiably doubt whether they were really prepared to replace their Württemberg identity with a German one. Perhaps too much attention is paid here to the scandal surrounding King Karl I’s lover, Charles Woodcock; for the purposes of better understanding Würt-
temberg’s pride in its identity, it might have been more instructive to consider how its people viewed themselves (King Wilhelm II, known for walking his dogs himself, was a far more popular figure in Württemberg in the end than his namesake, the German Emperor).

The conclusion reiterates the warning not to succumb to teleology, and emphasizes the significance of Württemberg in the creation of a German nation-state. Here, Württemberg appears more as an agent of change than as its victim. According to the author, King Wilhelm I in particular was keen to make a virtue out of necessity by attempting to overcome class ‘inertia’ through close co-operation with Württemberg’s neighbouring German states (p. 157; although this is a verdict that itself appears all too teleological). Sometimes he was successful, especially if we consider the customs unions; sometimes less so, if we think of the failure of a Third German confederation. The conclusion argues further that the failure of the 1848 revolution may have put an end to the liberal nature of the German unification project, but not to the project itself; Württemberg continued to be involved in numerous efforts to unite the middling states, in some ways even providing intellectual inspiration. In brief, Württemberg’s intention was to be actively engaged in the formation of Germany and, like other south German states, it probably played a ‘far more active role in the unification of Germany than has previously been allowed’ (p. 160).

This is by no means a new insight. But it is an appropriate summary of a succinct book, whose valuable contribution to the transfer of knowledge across different historiographies should be acknowledged. There are no comparable studies in English on the subject of Württemberg, nor of any other middling states. The author’s work of translation—in the broadest sense—may reveal some minor errors, but this has no direct effect on his argument. But it does show that not enough care has been taken. Siebenpfeiffer’s Der Bote aus dem Westen is listed both in the end notes (p. 172, n. 63) and the bibliography (p. 192) as Die Bote aus Westen; the jacket shows a photograph of a steam locomotive in front of Rosenstein Castle with date given as ‘c.1840’ although the railway line in question was not opened until 1845, as stated correctly in the text on p. 68; on the same page, however, the song cited as ‘Auf der Schwab’shen Eisbahn’ (sic!) is also mistakenly dated to this period, an absolute impossibility.

Gaps in the bibliography are problematic for researchers. Those with some knowledge of the subject will wonder why Dieter Lange-
wiesche’s important studies have not been included; the same goes for the works of Wolfram Siemann (for example, *Vom Staatenbund zum Nationalstaat. Deutschland 1806–1871*) or Harm-Hinrich Brandt (*Deutsche Geschichte 1850–1870*). More recent specialist studies (such as Jonas Flöter’s *Beust und die Reform des Deutschen Bundes 1850–1866* or the collection of essays *Der Wiener Frieden 1864*, edited by Oliver Auge, Ulrich Lappenküper, and Ulf Morgenstern) are missing. Even the *Handbuch der baden-württembergischen Geschichte* seems irrelevant for the purpose of his study as far as the author is concerned. As a result, the book is unlikely to make an impact on German research. Whether it can provide any new impetus for future English-language research also seems doubtful. There is, however, good reason to be grateful to the author of this book for at least giving the Third Germany the prominence it deserves.

GEORG ECKERT is a Research Fellow in Early Modern History at the University of Wuppertal. His publications include ‘*True, Noble, Christian Freethinking*’: *Leben und Werk Andrew Michael Ramsays (1686–1743)* (2009) and *Zeitgeist auf Ordnungssuche: Die Begründung des Königreiches Württemberg 1797–1819* (2016).

This comparative study, which considers the military discourse on the war of the future in the period from 1880 to 1914, clearly draws its inspiration from an earlier volume, also edited by Stig Förster, which examined the debate over future war in the inter-war period.1 Those who profited from the great breadth of coverage of the latter volume, which examined the debate on future war in no fewer than seven countries (Italy, France, Britain, Belgium, Germany, Switzerland, and the United States), may be slightly disappointed with the scope of this long-awaited follow-up study. As the editor explains, the original intention was to consider five countries (Britain, Germany, France, Russia, and Austria-Hungary), but the chapters on Russia and Austria-Hungary were not completed (pp. 12–13). Thus, this book considers only Germany, France, and Britain, albeit in longer chapters than in the precursor volume.

The basic premise of this study is essentially the same, namely, that it is profitable to consider the published discussion about the war of the future in military periodicals, since these were an important part of a wider debate. While this editorial concept worked extremely well the first time round, mainly because of the opportunity to compare seven different national discourse cultures via specialist military journals, in this case the perspective is somewhat narrower. Still, with this reservation in mind, this book does nonetheless repay reading. The three lengthy studies come quite close to being stand-alone, individual monographs: Markus Pöhlmann examines German military journals;2 Adrian E. Wettstein considers French

1 Stig Förster (ed.), An der Schwelle zum Totalen Krieg: Die militärische Debatte über den Krieg der Zukunft 1919–1939 (Paderborn, 2002). This volume also drew on the momentum generated by a series of conferences on the theme of ‘total war’.
journals; while Andreas Rose concludes with his analysis of British military journals.  

As this study considers the military debate about the war of the future in north-western Europe rather than in the broader international framework which was pursued in the 2002 volume, one might have expected the source material to extend beyond simply the journals consulted. Pöhlmann and Wettstein generally keep their gaze directed towards the journals as their principal source, although Pöhlmann includes the civilian journal Deutsche Revue in his analysis, while Rose makes rather more use of British quarterlies. This difference raises an interesting question as to the value of considering military journals as the main source for each study. Pöhlmann makes the most convincing case for the usefulness of examining the development of military thought, as revealed in the journals, over a period of several decades. The founding of several new military periodicals before 1914 marks, itself, the emergence of a process of professionalization. He argues, further, that the focus in the research literature on popular and more sensational authors has distorted some of the claims made about military writing in the pre-war period. This is an important point, and one which demonstrates the value of the book as a whole. Indeed, German military historians appear to have shown more interest than their British and French counterparts in military journals as a source to be analysed in its own right. That British military historians appear less interested in the Quellengattung of military journals is surprising, given the strong interest among literature specialists in both Britain and the United States in nineteenth-century periodicals. 

4 Andreas Rose, ‘“Readiness or Ruin”: Der “Große Krieg” in den britischen Militärzeitschriften (1880–1914)’, ibid. 245–389. 
6 See here the seminal article by Margaret Beetham, ‘Towards a Theory of the Periodical as a Publishing Genre’, in Laurel Brake, Aled Jones, and Lionel
If one takes all three studies together, there are some significant differences in approach, however, which means that it is difficult to draw definitive conclusions. In fact, in terms of methodology, it is never really explained why the project begins in the year 1880 rather than 1871. It is even noted by Wettstein that the journal *Revue Militaire des Armées Etrangères* was founded in 1871 as a direct answer to the French defeat (p. 143), highlighting the apparently random choice of year in which the analysis begins. While the decade following the Franco-Prussian War was without doubt dominated by assessments of that war rather than reflections on the future, a clearer explanation either by each author, or in the editor’s introduction, would have helped the reader in understanding the reason for the choice of 1880 rather than 1871. Indeed, because of the absence of the two other planned chapters, the reader is left in the dark as to whether the volume is intended to communicate any broader conclusions, or whether it has simply to be accepted as offering three stand-alone studies.

Nonetheless, these reservations aside, the book does succeed in raising an important question, namely, to what extent these three nations provide an indication of a definite trend in Western Europe towards increasing military professionalization. In fact, this can be seen in each of the three countries, with each one displaying a developmental upward curve, occurring in more or less the same time period. If this is, indeed, the case, it would imply that multiple factors contributed to this, rather than one side taking the lead, and so providing a stimulus to the other two nations. It is certainly true, as each chapter demonstrates, that two wars loomed large in each of the national military debates before 1914: the Boer War (1899–1902) and the Russo-Japanese War (1904–1905). Moreover, other subject matter—such as infantry tactics, new technology, and the future role of cavalry—can be seen to have received almost equal attention in the military journals of Germany, France, and Britain. Likewise, each national debate as reflected in the pages of the military journals took note of the debates in the other two nations; and, as Wettstein’s chapter reveals, two French journals were devoted to examining develop-
ments in foreign armies. As such, then, the volume does demonstrate that a form of international epistemic community existed before 1914 among Europe’s three most advanced military powers. It would, of course, have been interesting to have discovered whether this international debate included other nations’ armed forces.

Another important observation to be distilled from all three contributions, although the point is argued most cogently by Pöhlmann, is that between 1880 and July 1914 naval and military writers should be located historically in a multi-faceted and complex inter-relationship with the print media, thereby rendering redundant the ‘vulgar militarism theory, which restricts itself to manipulative media strategies’, as Pöhlmann puts it (p. 28). Still, while each author does make clear the rich contours of the military journals published at the time, there are two aspects of the publication activities of officers which arguably could have been better woven into the respective analyses. First, military debates did not take place solely within the pages of military journals since, frequently, important controversies resulted from book publications. Second, military authors did not restrict themselves simply to military journals: quarterly journals and other highbrow periodical literature often contained articles on military subject matter, which was just as likely to have been read as contributions to the military journals. While Rose is more willing than the other two contributors to engage with these other publications (although, as already mentioned, Pöhlmann does include the civilian Deutsche Revue in his chapter), this may be a reflection of their broader function within the intellectual life of Britain in the three and a half decades under consideration. While reviews of specific books in the military journals do attract the attention of each author, there could perhaps have been more scope in the volume as a whole to consider the other literary locations in which the military debates were pursued. Or, even more, for some collaboration between the three authors, perhaps in a conclusion, to offer interpretations as to possible differences between each of the national discourses.

The authors could be forgiven, of course, for countering that the broader societal debate has already been assessed for Britain, France, Germany, and the United States in Jörn Leonhard’s imposing study, Bellizismus und Nation, published a decade ago. It would have been

possible, though, to have related each of the national debates more closely to some of the major military theoretical achievements in each country. Pöhlmann in his study does mention some of the most important writers in Germany in setting the broader context for the debates of the time. Among the authors were Colmar von der Goltz, Friedrich von Bernhardi, and Alfred Graf von Schlieffen, whose writing is arguably essential for any understanding of the military ‘discourse landscape’ of the time. While the publications of some of the most important British theorists—names such as G. F. R. Henderson, Julian Corbett, Spenser Wilkinson, and Charles Callwell spring to mind—are referred to at least obliquely in Rose’s chapter, since their books were reviewed and their ideas debated in the pages of the journals, their appearance in the analysis is rather incidental as the result of his clear focus on the journals.

In sum, these chapters taken together open up new possibilities for research and historical debate rather than providing any final verdicts on the military debates before the outbreak of the First World War. One of the issues which still requires investigation, and is not really addressed in this work, is whether or not British military theorists led the way in Europe by 1914. The extent of major theoretical works by British authors cannot be denied, with the publication of important works by Henderson, Corbett, Wilkinson, Callwell, and others before the outbreak of war. When it came to the most significant German theorists—von der Goltz, von Bernhardi, Schlieffen—they built their thinking on the strong traditions of the Prussian General Staff. While these intellectual traditions are reflected in many of the articles published in German military journals, the really interesting question is whether British military writing was influenced more by the largely civilian character of the British amateur military tradition, or whether by 1914 it had been decisively shaped by either German or French military culture. But to answer this question, each of the three authors would have needed to have cross-referenced their co-authors’ work more closely and to have widened their source base to include more of the civilian literature, the quarterlies, and other non-military periodicals.

This final observation should not be taken, however, as a fundamental criticism of either the point of departure of the book, or the quality of each of the three chapters, since they all contain useful details and trenchant observations. What this volume reflects is a sig-
significant segment of the broad picture of the military debates of the three decades or so before the outbreak of the Great War. But it remains for others to expose those dimensions which still lie in the dark to the light of historical investigation.

ALARIC SEARLE is currently Distinguished Visiting Professor, College of History, Nankai University, Tianjin, People’s Republic of China. Among his most recent publications are an edited collection of documents, prepared for the Army Records Society and entitled *The Military Papers and Correspondence of Major-General J. F. C. Fuller (1916–1933)* (2017), and *Armoured Warfare: A Military, Political and Global History* (2017). He is also Professor of Modern European History at the University of Salford, Greater Manchester.
Das verkehrte England is the first book by Zentrum für Antisemitismusforschung and Museum Pankow scholar, Maren Jung-Diestelmeier. Keeping the author’s professional affiliations in mind is important, because a thorough and ongoing appreciation of the interplay between visual culture and material culture (epitomized by these institutions) is key to the success of her fascinating book.

The picture postcard—a still-new medium in the period under investigation (1899 to 1918)—is yet another of those neglected sources of Anglo-German relations that have seen such fruitful investigation in recent years.1 And it is a source that (as Jung-Diestelmeier makes clear) is ignored at its peril. For, even more so than the political cartoons and caricatures of Kladderadatsch, Lustige Blätter, or Simplicissimus, picture postcards standardized and disseminated, to an enormous extent, the stereotypical image of Britain in Germany (and beyond) in a critical phase of Anglo-German relations. German printers not only commissioned and produced anti-British images for a mass market, but the individuals comprising that market responded by purchasing and utilizing such postcards in order to communicate with one another on a regular basis. In Germany (as in Britain and France, and elsewhere), postcards also became collectors’ items, and scrapbooks and folios were soon to be found in many middle-class drawing-rooms and libraries across Europe and beyond. Produced in a number of regional contexts (Jung-Diestelmeier’s sample includes cards published in Leipzig, Munich, Saarbrücken, Dresden, Strasbourg, and Tübingen, as well as Berlin), these artefacts saw a convergence of imagery for what was fast becoming a national market, rather than a city- or Land-focused one. Such postcards should there-

fore be considered as amongst the most remarkable ways that nationalism was harnessed by print capitalism (in Benedict Anderson’s notable formulation) and therefore an effective means of perpetuating and building ‘banal nationalism’ (in Michael Billig’s famous phrase) as the German nation moved towards its first great moment of crisis. As the author shows, this began in peacetime, in a period of growing ambivalence, but sharpened and became a major underpinning of the German auto-image (as well as the image of the enemy) during the Great War of 1914 to 1918.

Following on from a detailed theoretical and methodological section, Jung-Diestelmeier structures the main body of the book into three sections of analysis, grouped around distinct periods of development of German images of Britain. Throughout, one finds good holistic analysis both of the content of the imagery (for example, the importance of John Bull and the British lion, as well as Queen Victoria and King Edward VII, as representative figures), as well as the mass media context for such images (including background information on the different publishing houses, the key cartoonists involved, and the development of the picture postcard as a cultural phenomenon as well as an inexpensive and practical means of mass communication).

The book’s first section (1899–1905) shows how the image of Britain was central to German constructions of alterity as well as identity. Key moments—of collaboration over the Boxer Rebellion in China (1900); and tension over the Anglo-Boer War (1899–1902)—combined with matters in which the two empires were not directly involved (the Russo-Japanese War, 1904–5), saw the production and dissemination of imagery that used Britain as a model for world power, but also provided Germany with a contrast. The second section (1904–14) shows how there was a decline in specifically anti-British imagery in postcards, but that this was continued in the dedicated satirical press to a great degree; and, of course, the cartoons from the satirical press were often, themselves, reproduced as postcards. Jung-Diestelmeier notes how a profusion of suffragist-themed postcards indicates the persistence of an indirect kind of *Schadenfreude*.

The rationale behind the third period of development is self-evident (1914–18): this was the time when laughing at the enemy; demonizing him as an underhanded and dishonourable foe; and sharing

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his misfortunes (a large number of postcards depicted captured British POWs) was part of a national war effort that relied on an effective postal system between front line and home front. The imagery explored therein is both familiar and startlingly new. Triumphant images of Zeppelins evading the searchlights of London and effective uses of photography from the front have probably never before seen the light of day (in a scholarly sense).

Jung-Diestelmeier’s source base is extremely impressive. Archives from across Germany (public and private) have been mined to reveal as much as possible. The secondary literature is multilingual (with almost all of the major works in English standing alongside the considerable literature available in German). It is particularly gratifying to see reference being made to one of the earliest serious engagements with postcards as a historical source: that by Ian McDonald. His 1990 book, *The Boer War in Postcards*, did not have the immediate impact on scholarship that it deserved.

The publishers, Wallstein, have produced the work in two separate volumes (*Bildband* and *Textband*), to allow readers to peruse the source material more readily while making their way through the analysis proper. In this they follow some good, established precedents, and avoid the problems faced by others in requiring the reader constantly to flip backwards and forwards between visual material and analysis. Although the postcards are reproduced in colour in the *Bildband* (and plenty of bibliographical material is provided throughout), the size of the reproductions is rather frustratingly small. One wonders whether reproducing the images at as close to their original size as possible might have improved the appreciation of these as documents.

*Das verkehrte England* is a valuable addition to the literature on this key period of Anglo-German relations, as well as, of course, to the literature on the development of German national identity. Well written and clearly structured, it is certainly a work that achieves its stated goals, and provides a model for other studies of its kind. It is also

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the first volume in a new series, Studien zu Ressentiments in Geschichte und Gegenwart (produced by Jung-Diestelmeier’s key affiliate institution, the Zentrum für Antisemitismusforschung). If the standard of this, first study is anything to go by, then scholars will have much to look forward to from the series.

RICHARD SCULLY is Associate Professor in Modern European History at the University of New England (Armidale, Australia). He is the author of British Images of Germany: Admiration, Antagonism & Ambivalence, 1860–1914 (London, 2012), and has published widely on the history of political cartoons.

Pierpaolo Barbieri, executive director of Niall Ferguson’s Green-mantle advisory firm, has not written ‘a book about Spain’, as he himself states in the introduction. ‘Rather it is a story of political economy and war in the tumultuous 1930s’ (p. 2). In *Hitler’s Shadow Empire* he traces the creation of an informal Nazi empire back to Germany’s intervention in the Spanish Civil War and the trade benefits Germany was able to obtain in exchange for military support and munitions. At the centre of this study is Hjalmar Schacht, the temporarily ubiquitous German ‘economic dictator’, as the *Financial Times* called the Reichsbank president and minister of economic affairs in 1934.

The Spanish conflict turned out to be the only proxy war of the 1930s in which communism, fascism/National Socialism, and liberalism were engaged in an internationalized civil war—the third party only in Spain, as the liberal democracies preferred non-intervention. Barbieri eloquently describes the Spaniards’ path to civil war and intertwines the Spanish, German, and Italian parts of the story. His essentially chronological analysis is combined with anecdotal miniatures.

Readers who do not know much about Spain in the 1920s and 1930s will profit from the first half of the book about the path to civil war and intervention. Barbieri examines the internal conflict in Spain; the problems the Spanish question posed for the already precarious international system; and, finally, Schacht’s economic concepts and German economic policy up to the mid 1930s. It is only in the middle of the book that Barbieri announces his leitmotiv (pp. 133–4) and brings together ‘the two hitherto distinct narratives of this study: Spain’s path to civil war and Nazi Germany’s road to recovery under the stewardship of Germany’s “economic dictator”’ (p. 138).

The sections about the political economy of Schacht’s informal empire are the most interesting but also most debatable parts of the monograph. The Reichsbank president’s biographer, Christopher Kopper, had already invoked the idea of ‘informal empire’ in the context of Schacht’s plea for colonial expansion,¹ a rather fruitless thread.

Barbieri’s merit is to extend this idea to trade relations with Spain.

Based on a thorough interpretation of Schacht’s doctoral thesis, neglected by earlier scholars, Barbieri’s study shows how Schacht’s early academic work and experiences during the First World War shaped his political thinking. Schacht’s ‘imperial, mercantilistic strategy’ (p. 168) involved disengaging from Anglo-American trade partners and reorienting Germany’s attention towards the Balkans, Latin America, and, finally, Spain. With these partners, the political and economic asymmetry was reversed in favour of Germany, so that better terms of trade were possible. Drawing on various sources, especially private conversations and public interventions during the 1920s and 1930s, Barbieri reconstructs Schacht’s economic and political position, summarized as Weltpolitik: centralized economic decision-making and state intervention at home up to the point of micro-management; colonial expansion; and a preference for trade with countries on which Germany could impose one-sided terms of trade. Barbieri seems undecided, however, whether to describe these ideas as ‘re-emerging mercantilism’ (p. 117), ‘neomercantilist’ (p. 133), or ‘pseudo-mercantilist’ (p. 91).

The Spanish endeavour and its subsequent quid pro quo (Spanish raw materials for munitions and overt military intervention) fitted well into these concepts. German–Spanish trade soon reached new dimensions, as did Spanish debt to Germany. Political, military, and trade relations between Germany and Spain were clearly not a ‘relationship of equals’ (p. 149). As Barbieri shows, ideology was at most a secondary factor. This applied to Hitler’s decision to support the Spanish army’s rebellion, and, in particular, to back the Franco faction, which was not self-evident. Barbieri gives a precise and dense account of the decision-making process on 25 July 1936 in Bayreuth. The economic dimensions of this decision were clear. To organize the clearing of German–Spanish trade, two monopoly companies were founded at short notice, HISMA in Spain (replaced in 1939 by SOFINDUS) and ROWAK in Germany. They worked under the supervision of the Foreign Organization branch of the Nazi Party (NSDAP/AO), thus indirectly in the realm of Schacht’s most important opponent, Hermann Göring.

Barbieri integrates these events into a larger evaluation of Nazi foreign policy in the mid 1930s, drawing a conclusion by analogy that
does not convince this reviewer. Hitler’s interest in the economic exploitation of Spain does not mean that Schachtian Weltpolitik was ever a viable alternative to the Lebensraum objective. By contrasting the Reichsbank president’s pragmatism on the one hand with seemingly surreal Lebensraum goals on the other, Barbieri implies a false opposition. Hitler had a functionalist view of economy and trade, which were not an end in themselves. All sources, from Mein Kampf to the speeches of 1 February 1933 and 5 November 1937, show that Hitler believed neither in economic imperialism and colonies nor in trade or an export economy. Although Barbieri postulates a reading of the 1937 Hossbach Memorandum ‘through a different lens’ (pp. 185–6), he is not, ultimately, able to give an alternative interpretation.

Barbieri accurately describes the ‘policy struggle’ (p. 131) in 1936 which pitted Schacht against Göring. Schacht’s search for an ‘exit strategy’ (p. 157) from the misguided growth and armament overproduction of the first years of Nazi rule did not accord with the plans of Hitler and those around him. They did not share Schacht’s diagnosis (too much armament) and would not have prescribed the same remedy (an economic slowdown). In this situation, Schacht was ‘sidelined in Berlin as Hitler drifted [sic!] towards a wider war’ (p. 9), an expression that, once again, gives a rather curious interpretation of Hitler’s agenda. On the whole, Barbieri overstates Schacht’s influence outside the economic sphere and especially on questions of principle, such as the long-term goals of foreign policy and war.

The chronological relationship of this policy struggle to the Spanish endeavour remains vague. Curiously enough, the Vierjahresplan memorandum was drafted under Göring’s auspices at around the same time as Hitler decided in favour of intervention. This said, interpreting the following exploitation of Spain as an example of Schachtian informal empire is questionable. Barbieri himself states that even after Schacht was ousted, German–Spanish trade continued as before. He points out that Spain remained crucial for German imports even after 1939, as the ‘formal’ empire took shape. Against this background, strong ex post facto assumptions postulating a conditional nexus and a sequence instead of simultaneity are not convincing: ‘Crafting the exploitative, genocidal, and ultimately ephemeral empire for which we remember the Nazis required [sic!] burying [sic!] the informal one on Iberian soil’ (p. 245).
Contrary to what is suggested in the introduction, Barbieri does not present a ‘useful counterfactual’ by describing an ‘Empire that could have been’ (p. 3). He gives an explicit narration of an allegedly functional Schachtian ‘informal empire’ that was torpedoed by ignorant Nazis and their power games. In order to prove its supposed ‘effectiveness’ (p. 12), in his final chapters Barbieri contrasts the German experience with the return on investment of the Italian intervention in Spain and with later forms of German occupation in Europe. The outcome for Mussolini was modest, although the Italian investment of money, men, and matériel doubled the German effort. Barbieri argues convincingly that the Duce’s focus on ideological friendship, propaganda, and Italian splendour prevented his officials from collecting trade-offs similar to those obtained by their German partner and competitor. The second comparison is less convincing. In the ninth chapter (‘Formal Empire’), Barbieri contents himself with a survey of the standard literature on different annexed and occupied countries. The supposed superiority of informal rule, deemed more ‘sustainable’ (p. 13) than the overt exploitation of eastern Europe depends mainly on the fact that the German occupation regime after 1939 was short-lived.

The alleged effectiveness and sustainability of ‘informal empire’ can also be questioned by looking at the Spanish case itself. Barbieri himself concedes that Germany could not maintain political, military, and economic pressure in the long term. For some years German trade eclipsed the traditionally dominant Anglo-French trade with Spain, and HISMA/SOFINDUS even tried to perpetuate the situation by converting the growing Spanish debt into direct investments, especially in mines in Spain and Morocco. But the ‘informal empire’, in fact, was limited in time, scale, and scope, as Franco increasingly played for time instead of satisfying German demands. From the beginning of the Second World War he had more room for manoeuvre and gained autonomy from his former allies, whereas Germany’s dependence on Spanish resources grew. Germany’s economic ‘penetration of Spain’ (p. 147) relied on a contingent historical situation and thus remained incomplete.

This reviewer would have liked to know more about the HISMA/ROWAK complex. How did the clearing of German-Spanish trade work exactly? Who was involved? How was business done on the ground? How did the flow of information between German indus-
try, German economic bureaucracy, HISMA/ROWAK, and, finally, their Spanish counterparts work? As Barbieri privileges a top-down approach, the answers unfortunately remain abstract, although HISMA/ROWAK is the cornerstone of both Spain’s integration into the German economic system and Barbieri’s argument.

The bibliography is extensive, as the author is well-versed in the English-, Italian-, and Spanish-language research. But a run-through of the footnotes shows that the German literature is sometimes quite outdated and cited in a rather unspecific way (e.g. ch. 6, nn. 44, 49, 52, 61, 68, 77, 81). Barbieri has used material from all the relevant archives in France, Germany, Italy, the UK, and the USA, but he privileges published sources in his annotations, especially in the case of German sources. These observations may explain his debatable evaluation of the German policy discussion and his all too easy refutation of old-style research which saw German foreign policy ‘on an “inexorable” road to war’ (p. 3) and interpreted the quest for Lebensraum as a ‘preordained path of Nazi rule’ (p. 131). Some metaphors fit in with Barbieri’s ‘empire’ terminology but seem misleading to this reviewer, for example, calling the German intervention a ‘fully fledged colonial endeavor’ (p. 134) or naming HISMA’s managing director Johannes Bernhardt an ‘effective viceroy’ (p. 188).

The appendix of ‘economic data’ contains seven charts which are either of minor relevance to Barbieri’s argument (for example, German hyperinflation 1922–6, German unemployment rates 1921–39, and so forth) or give only overall trends in Spanish imports and exports. Figure A.7 is particularly intriguing, as Barbieri illustrates the ratio of ‘Cumulative German and Italian Intervention Costs’ (33 and 67 per cent respectively) by presenting the two figures in a pie chart: 1,932 as against 3,914 million pesetas.

Barbieri is not the first scholar to integrate discussions on German and Italian intervention with the question of Nazi economic and trade policy, but he does so in a sophisticated and readable way that will probably reach a wider audience than earlier literature. The book, already translated into Italian (2015, Mondadori), presents a good overview of German and Italian intervention in the Spanish

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2 Unfortunately the bibliography was only available online on the website advertising the book and has since disappeared: <http://www.hitlersshadowempire.com/bibliography/>, accessed 1 Aug. 2016.
Civil War embedded in the international context. Emphasizing Schacht’s role is important but, in the end, the argument by analogy (Schacht’s programmatic texts correspond to forms of organizing German–Spanish trade) does not hold. Instead of opposing two mutually exclusive models of imperialism, the author might have found it more interesting to think of them as complementary forms of exploitation and exercising power. Finally, comparing the Nazi idea of European hegemony and empire with the status and the underlying idea of the European Union, its common market and common currency (pp. 246–7) is, at best, a distortion.

JÜRGEN FINGER is a postdoctoral researcher at the German Historical Institute in Paris. His books include, with Sven Keller and Andreas Wirsching, Dr. Oetker und der Nationalsozialismus: Geschichte eines Familienunternehmens 1933–1945 (2013).
For decades the Holocaust has been considered one of the seminal events of the twentieth century and has thus attracted great scholarly interest. In the book under review here, Tim Cole takes ‘a fresh look at a familiar event’ by analysing its spatiality (p. 6). In doing so, he takes his place in a steadily growing body of research on the spatial dimensions of history. In the book the Holocaust is understood as ‘a place-making event that created new places . . . or reworked more familiar places’ (p. 2). The places researched are (reflecting the order of the chapters in the book), the ghetto; the forest; the camp; places of refuge such as attics and cellars; the river; and the road. As the singulars imply, Cole refers only to one specific place in each chapter, for example, the Warsaw ghetto, the Danube river, or the Bergen-Belsen concentration camp. Instead of touching on every chapter in detail, I will here discuss the three main research focuses which run throughout the book.

The book first suggests that the Holocaust cannot be considered as a monolithic event, but must be seen as a dynamic and multi-faceted one. As the timeline shows, between 1939 and 1945 there were different intensities and ways of killing. Thus from a spatial perspective it is possible to identify ‘different genocides . . . in different places’ in Europe (p. 2). As the author points out, during the death marches on the German roads in 1945, there was a recurrence of face-to-face killings of individual prisoners, behaviour which had also characterized the early phase of the Holocaust (p. 186). Using Timothy Snyder’s concept of ‘bloodlands’, Cole states that the events of 1944–5 extended this zone as far as central Europe. This is a crucial distinction which leads to the idea that what is usually labelled as the ‘Holocaust’ should be seen as multiple and highly dynamic ‘Holocausts’, as the most recent research has suggested.

Second, and inextricably linked to this premise, Cole places the violent and highly dynamic spaces of the Holocaust at centre stage. Nearly all the chapters highlight the geography, boundaries, and social topography of the places being researched. Thus the spatial and social dynamics of the Holocaust become apparent. Jews trying to escape regularly chose the less mature parts of the eastern European forests as hideouts because hardly anybody went there to
collect firewood, and so they were less likely to be discovered. The need to survive in the wilderness meant that people who were hunters or craftsmen were quite quickly allowed to join one of the emerging fugitive groups living in the woods. What mattered for survival was what counted, and these were skills that were not prestigious in peacetime civilian societies. The author also rightly explains that nature matters in history because it affected the living conditions for those fleeing as well as in the camps.

All of this is interesting, but it raises a number of further questions. While Cole considers spaces such as ghettos, roads, and forests as given entities, it is left open how they came into being. It is almost as if the author creates them in order to systematize his topic, as Snyder did with ‘bloodlands’. We learn a great deal about how the Jews behaved and thus shaped the ‘Holocaust landscapes’, but remarkably little about how they perceived them. Their conceptions of woods and trains—spaces that have a history of their own, if we think of romanticism and modernism—remain a fruitful field for research. Ghettos also were not first invented in 1939, but have a vividly remembered history. The cultural history of these spaces was reflected upon by contemporaries, but Cole rarely touches on these aspects.

Third, the book considers the Jews as ‘victims’ of the Holocaust, but at the same time points out their ‘agency’ in terms of survival and coping strategies (p. 7). In analysing their experiences the author relies heavily on survivors’ accounts and oral histories of the Holocaust. These sources have many limitations for historical analysis, and these pitfalls are discussed in the book. Nonetheless, in some passages it would have been helpful if the author had included more figures and findings from the latest Holocaust research instead of drawing solely on eyewitness accounts and late oral history interviews (for example, on p. 121). This would have made it possible to evaluate more accurately whether the individual experiences so vividly developed in many chapters of the book can be generalized. More than once Cole leaves this question unanswered and uses phrases such as ‘oftentimes’ instead.

In considering the topics treated in this well-written book, this reviewer has many further questions. What about the place-making of other groups involved in the Holocaust, most notably the Germans, of course. Some of them drew up camp blueprints, ordered
the escaping Jews to be chased, or were involved in the Holocaust in many other ways, sometimes even as helpers in times of need. Other groups also made up the ‘Holocaust landscapes’. On the part of those who were persecuted, we can mention the Romani people, homosexuals, ‘asocial’ groups as defined by the Nazis, those considered racially inferior, criminals, and the politically suspect. But local populations also played a part. These questions show that a spatial analysis of ostensibly well-known histories can have inspiring results, and Cole’s book provides ample evidence of this.

CHRISTOPH NÜBEL is a historian and a staff member of the Center for Military History and Social Sciences of the German Armed Forces (Zentrum für Militärgeschichte und Sozialwissenschaften der Bundeswehr) in Potsdam. His many publications include Die Mobili- sierung der Kriegsgesellschaft: Propaganda und Alltag im Ersten Weltkrieg in Münster (2008); Durchhalten und Überleben an der Westfront: Raum und Körper im Ersten Weltkrieg (2014); and Krise ohne Ende? Kriegserwar- tungen und Kriegsbereitschaft in Europa vor dem Ersten Weltkrieg (2014).
The Saarland, which lies in south-western Germany on the border with France, is one of the most interesting territories for an investigation of transnational entanglements and confrontations in Europe. Under various different sovereignties for centuries until the Versailles Treaty specified that, as the Saargebiet, it was to be administered by the League of Nations for fifteen years (1920–35), with France in a strong position, it can be seen as an early precursor of possible territorial Europeanization. In the plebiscite held, as prescribed at Versailles, in 1935 to determine the future of the Saar Territory or Territoire de la Sarre, an overwhelming majority voted in favour of joining Germany, although this had been under National Socialist rule since 1933. From 1935 to the present day the area has been known as the Saarland. In 1945 it first formed part of the French zone of occupation in Germany and then, in 1947–8, it was placed under the supervision of a High Commissioner, separate from Germany. In an economic and currency union with France, its semi-autonomous political status was unclear.

When European integration received a strong boost in 1950 with the plan put forward by Robert Schuman, the French foreign minister, for the creation of the European Coal and Steel Community, the Saar was soon a central source of conflict between France and the Federal Republic of Germany, founded in 1949. From 1952 this unresolved problem increasingly blocked progress towards European integration. Only when the French and West German governments agreed in Paris, in October 1954, to hold another plebiscite on the Saar in 1955, did European integration take off again. It led, surprisingly quickly, to the Treaty of Rome creating the European Economic Community.

In 1954–5 the Saarland was to receive a European statute to establish its independence, but in October 1955 a two-thirds majority voted to reject this. Within hours, France accepted that politically, this represented a vote for integration with West Germany, although this choice was not, officially, on the ballot paper. Politically, the

Trans. Angela Davies (GHIL).
Saarland joined West Germany in 1957, followed by integration in economic and social policy, providing an exciting example of how to deal with problems of integrating very different economic systems. Thus the Saarland played a key role in the reconstruction phase after the Second World War, both as a blocking force and in a positive, constructive sense. To the present day, it remains institutionally and politically the most international of Germany’s federal states, right down to the fabric of its everyday life, with a special emphasis on Franco-German co-operation in a European perspective.

In a detailed synthesis, based largely on the evaluation of a wide range of documents from France and the Saarland, Bronson Long traces the complex development of the Saarland in a number of political areas from 1945 to 1957 for anglophone readers; the last large synthesis in English, by Jacques Freymond, dates from 1960. Long’s core thesis runs through the whole book and is explored in great detail: ‘a near obsession with de-Prussianizing the Saar drove the actions of French officials. As the war had destroyed Prussia, France’s Saar policy was thus backward-looking in nature from the very beginning’ (p. 27). ‘French officials did not comprehend how deeply traumatic Germany’s defeat was for both Germans and Saarlanders alike’ (p. 19) and ‘formulated many policies on the basis of what amounted to outdated ideas about Germany’ (p. 23). ‘What French officials failed to comprehend was that if Germany had needed “de-Prussianizing,” the war itself had largely accomplished the task’ (p. 22). Here Long rightly stresses a misapprehension that was widely held in Europe for a long time.

The author first outlines France’s general plans for the Saar and, in line with recent research, claims that France’s goal was not an annexation of the Saar, although this was widely demanded by the general public. Gilbert Grandval, who had been a leading member of the French resistance to German occupation during the war and was Military Governor in 1945, High Commissioner from 1947, and French ambassador to the Saar from 1952, thought that the Saar should become independent; he often spoke of Luxembourg as a model. Long considers that Grandval’s aim was an independent Saarland nation. In Paris, by contrast, while different positions were taken on individual points, the main interest was in economically exploiting the region. Significant sections of French private industry, especially in the mining sector, however, were less than enthusiastic.
because they feared competition from the strong industry of the Saar. After the establishment of the Federal Republic of Germany in 1949, the Saar became a key problem in Federal Chancellor Konrad Adenauer’s policy for France and for Europe, and led to sharp domestic political clashes connected with the general problem of reunification. Since the end of the war it had become increasingly clear that independence for the Saar was politically feasible only as part of a Europeanization of the country. This gave rise to an impressive review of earlier steps towards Europeanization in local political practice.

Long gives a broad account of France’s cultural achievements. An early cultural agreement between France and the Saarland, among other things gave the young people of the Saarland chances to travel and take part in exchange trips as far afield as the USA. Nothing comparable could, at that time, be offered by any other part of the former Reich. Guided by his main question concerning presumed nation-building in the Saar, Long pays particular attention to the churches, sport, the school system, which was rebuilt immediately after 1945, and the establishment in 1947–8 of a new university which was successfully oriented towards Europe and could boast an international teaching staff. He rightly sees these developments in the areas he reviews as thoroughly positive. As an example, we need mention only the more than 10 million new school textbooks that had been produced by 1948 for the whole of the French occupation zone, far more than in the British or American zones. Because of its potential for generating mass enthusiasm, sport (mainly football) drew the special attention of the High Commissioner, and thus also of the author. The Saarland’s strong sports clubs aspired to enter the Fédération Française de Football but, after long internal debate, were rejected on grounds of nationality as well as competition. At the 1952 Olympics in Helsinki, the Saarland was represented by its own team. Finally, in the 1950s, they turned to the Federal Republic of Germany and in 1952 were runners-up in the national league, earning the enthusiastic support of the Saarlanders. Grandval, sharply critical of Paris, saw football as one reason for France’s progressive loss of prestige in the Saar, and recent research supports this view.

Long also pays special attention to the churches. Within the Catholic Church, France wanted to create a new, unified bishopric of the Saar but failed because of the opposition of the Vatican and the the bishops of Speyer and Trier, both of whom had responsibility for
parts of the Saar. On the Protestant side, attempts to bring together the church in the Rhineland, which was more critical of French policy, and that in the Palatinate also failed because of theological differences (Lutheran versus United) that were largely incomprehensible to the Governor, and political opposition. Here, too, it appears that all the important issues relating to the Saar were closely entangled with the area’s general but unclear political status, and with its international position.

At political level, Long pursues the meandering developments in the status of the Saar, which remained largely undefined while granting France extensive opportunities for control. The many differences between the various authorities in Paris and, until 1949, at the High Command in Baden-Baden are described in broad brush strokes; in reality, they were even more complicated. Regarding the constitution of 1947, it is surprising that Long does not mention that Grandval, on his own initiative, had the people of the Saar draw up a constitution in line with German tradition, although Michel Debré, head of the Saar Department in the French Foreign Office and an opponent of too much independence for the Saar, had explicitly prohibited this. He had ordered the adoption of a completely different model with a weak government and weak parliament. Grandval’s action meant that when the Saarland was incorporated into the Federal Republic of Germany in 1957, the constitution of 1947—allegedly imposed by France—could largely be adopted, once the provisions for economic and monetary union with France had been deleted. Long cites colourful examples to demonstrate how little Debré knew about conditions in Germany and the Saarland. Grandval sought permanent confrontation with Foreign Minister Robert Schuman, who had already rejected the introduction of the franc in the Saar. For Schuman, European integration and co-operation with the Federal Republic of Germany soon gained priority over autonomy for the Saar as championed by Grandval. Europeanization, however, brought a dilemma for Paris: the aim of autonomy for the Saar seemed to be achievable only through Europeanization, but this would indirectly strengthen the Federal Republic within Europe (pp. 187 ff.).

Long provides a differentiated analysis of the politics of the pro-autonomy governments of Minister President Johannes Hoffmann, who was initially highly popular, but soon became a controversial figure. On the basis of his experiences of National Socialism and exile
from 1935, Hoffmann had grave doubts about the capacity of the Germans for democracy, agreeing with Grandval on this. Out of this grew a policy of control that was designed specifically for the Saarland, and which gradually alienated ever larger sections of the population from his government which, until about 1951–2, had enjoyed broad political support given its close ties with France. This shift was caused not least by government surveillance, motivated by this mistrust, of political organizations that were critical of autonomy, something that was incompatible with the goal of democratization.

By 1954 the majority of the population seemed to endorse a Europeanization of the Saar. Its rejection in the referendum of 23 October 1955 is explained by Long as a result of the control policies of the Hoffmann period. He also sees it as a vote against France because of its contradictory policies: in its colonial empire France acted against its own aims of democratization and in Europe, the European Defence Community proposed by France was rejected by its own parliament, while at almost the same time, a European statute was proposed for the Saar. The author gives a lively account of the complicated and soon acrimonious debates within the Saarland in the summer of 1955, which led to the rejection and end of French supremacy and integration into the Federal Republic in the period 1957 to 1959.

This book is based on a wide range of official documents from party and private archives in France, Germany, and Switzerland (Foundation Jean Monnet). Especially interesting is the additional information gleaned from some of the private archives which were not available to earlier researchers (Bidault, Debré). The sources have been thoroughly evaluated and the majority of references is to archival material. A carefully prepared index facilitates the use of this book.

Long’s main argument provides ample material for discussion. To the extent that he draws general conclusions about the whole of French occupation policy and policy for Germany from his work on the Saar, some of their basic outlines are not convincing. This is unfortunate, given the thorough work he has done in the archives. Our knowledge of French politics has moved on considerably since the 1980s, and it would have been good to hear his possible counter-arguments, all the more so as much of the archival material he uses has already been comprehensively evaluated in recent decades. Long quotes some of the many more specialized publications on the Saar and argues carefully and in great detail, whether he agrees with them
or comes to different conclusions. But apart from a few aspects of European policy, the basic research on the main lines of the European post-war order and French policy is only marginal to his analysis, and he therefore hardly engages with it discursively. He thus still takes positions, for example, based on the subjective political perceptions of contemporaries, or the partially erroneous interpretations of French policy by senior American decision-makers, such as the harsh but charismatic Deputy Commander in Chief, Lucius D. Clay, that have entered the research and been confused with France’s actual positions. In the French files, as well as in the American files published since the 1960s, the necessary differentiations are clear. To take just one central example: the argument that France had pursued a policy of obstruction in the Allied Control Council and thus prevented the central economic administrations for the whole of Germany from working is inaccurate, as we have known for thirty years. France had an existential interest in German economic unity. But it demanded that the political decentralization of Germany as prescribed in the Potsdam Agreement should be implemented, and thus that ‘German central agencies’ (p. 32) should not be set up under German leadership, but that ‘central agencies’ should be created under Allied leadership—‘bureaux alliés’, ‘allied agencies’, in the terminology that Clay impatiently rejected as too complicated.

Still the most thorough work on French policy for Germany in the early post-war period, that by Dietmar Hüser, is occasionally mentioned by Long. But he does not discuss its basic findings, which contradict his main arguments. From 1945 France had pursued a ‘dual policy for Germany’: it may have used sharp rhetoric in public and towards Allied partners (as analysed by Long); but at the same time, in practice it followed a policy that was in many respects highly constructive, and whose core goals (decentralization and access to German raw materials in the mining sector) were realized in 1949. Hüser’s work and the monumental volume by Armin Heinen on the Saarland (both published in 1996) offer a wealth of analyses even of the extremely complicated French decision-making processes, and they could have served Long well.

A number of errors that are seemingly small, but relevant to the interpretation, arise from the basic problem of contextualization. Under the League of Nations mandate the Saar region was not ‘given to France’ after 1920 (‘Versailles . . . gave it to France’, p. 2), but
France had a strong position within the international Governing Commission and as a consequence of French ownership of the mining industry in the internationalized territory. ‘Saargebiet’ was its official designation, not just what it was ‘often simply called’ (p. 35). The situation after 1945 was fundamentally different, and especially in Paris they learned from previous experience: France no longer had ‘ownership’ of the Saar pits as after 1920; Long mistakenly equates it with the sequester administration (pp. 41, 236, among others), but this was a decision not about ownership, but about administration and use; here the internal French fronts were again highly complicated and changeable. The author repeatedly confuses the *Arbeitskammer* (Saar Chamber of Labor) with the *Einheitsgewerkschaft* (unitary unions), a consolidation of all the trade unions in the Western zones. The *Arbeitskammer*, by contrast, was an original and important institution that existed only in the Saar and in Bremen and in which, completely against French tradition, unions and employers were represented on equal terms; it was not until the Saar’s integration into the Federal Republic that it became a chamber of employees, which it remains to the present day. Nor did the USA put an end to the dismantling of industry in May 1946 (p. 30)—in individual cases, this carried on until 1951—but it stopped supplying reparations goods from its own zone to the Soviet Union.

These and other seeming details are probably connected with the author’s main thesis in that they may contribute to a circular argument, for the strong focus on ‘nation-building’ in the Saarland allows the long-term successes of French policy to fade or disappear. In the area of culture, for example, which is Long’s special interest, many further activities in museums, in modern music and art, art colleges, the media, theatre, film, and so on are overshadowed as a result of this approach. But they could have drawn attention to the fact that France’s engagement in the Saar in these and many other fields was by no means a failure but, on the contrary, highly successful. Long repeatedly emphasizes that in 1945 France had revisited its aims of 1919, but this is not true. On the contrary, reflections on the failure of the Versailles peace order not only in the Saar, but in the whole zone of occupation since 1945 led to a variety of initiatives in a new policy designated ‘democratization’, which prepared the ground for future reconciliation after the establishment of the Federal Republic. At the same time, it was was often the act—recognized as such—of focusing
on old stereotypes that, in part conceptually but especially in practice, contributed to an attempt to change the allegedly aggressive ‘German soul’. Long even refutes his own thesis that the majority vote against European status for the Saar had been a vote ‘against France’ when, on the final pages of his book, he describes the quick resumption of co-operation after 1955 and the consolidation of the French institutions built up in the Saarland in the decade after the war. Given that many of the factors described by Long clearly explain the result of the vote, it seems rather surprising that, in 1955, one-third of the votes nonetheless went to the vaguely formulated European Statute. This outcome reflects how strong the Saar’s European orientation and its connections with France remained. The majority of the population saw the political future as lying in integration into the Federal Republic of Germany. But to the present day, the Saarland is the German Federal Land with the closest cultural, social, and institutional connections with France.

Such comments underline that Long’s work stimulates very interesting discussions. His concisely written book provides information about many areas during these post-war years on the Saar, which were so exciting precisely because local, regional, bi-national, and international levels were always closely intertwined.

American policymakers generally tend to avoid speaking of military occupation, preferring instead to use the language of liberation or regime change to describe the activities of their military abroad. When they do talk of occupation, they usually like to present the United States as an exceptionally able and altruistic occupier. In such discourses, the American occupations of Germany and Japan after the Second World War have often featured prominently as particularly successful examples that demonstrate the American capacity to effect democratic transformation through benign military government. Such familiar narratives acquired a special urgency during the occupation of Iraq in 2003, when the Bush administration often referred to the occupations of Germany and Japan as model occupations to be emulated in the Middle East. As Susan Carruthers recounts in this fascinating book, it is therefore not surprising that a New York University law professor tasked with helping draft Iraq’s novel constitution should have observed on his flight to Baghdad that seemingly everyone was reading books about the post-war occupation of Japan.

One would have wished that those very same passengers had been able to read Carruthers’s lucid comparative anatomy of the US occupation of Germany and Japan, which forcefully demolishes the self-congratulatory and strikingly persistent myths surrounding the American experience of post-war occupation. Written against the grain of much recent commentary and political science writing on occupation that rather hopelessly seeks to unearth the magic formula that makes for successful occupations, her carefully researched book is a stringent reminder that simplistic accounts of ‘good occupations’ cannot be accepted at face value. Conversely, as this book well demonstrates, turning occupation into a success story, ‘like so much else in the postwar world . . . required radical reconstruction’. That benevolence and success finally emerged as the key ingredients of the occupations’ master narrative was not self-evident. Rather, as Carruthers convincingly argues, it took ‘time and toil to smooth the rough edges of lived experience into the sleek veneer of national legend’ (p. 312).
Drawing primarily on a large number of letters, diaries, and memoirs written by both ‘ordinary’ and more high-ranking servicemen and women, Carruthers tells her story through sources that have hitherto featured less prominently in the historiography of the post-war occupations. In an inversion of Wolfgang Schivelbusch’s influential concept of ‘cultures of defeat’ that revolved around the experiences of the occupied population, she focuses on the impact of occupation on the occupiers themselves, exploring the often ignored ‘psychology and phenomenology of victory’ (p. 10) by putting the subjectivities of Americans centre stage. This is therefore not a history of the high politics of occupation, but an account of how occupation was experienced by its practitioners and how those very experiences were transformed and pressed into a narrative that rendered occupation something virtuous. The result is a novel, and in many respects provocative, reading of the two major US post-war occupations emphasizing above all the fractiousness and moral messiness of the whole endeavour.

To describe the ‘transformation act’ (p. 5) that produced the ‘good occupation’, Carruthers takes as her point of departure the training imparted to occupation personnel by the School of Military Government at the University of Virginia in Charlottesville. The School, Carruthers argues, faced a constant lack of top-down political direction, leading to the transmission of a narrow conception of occupation to its students. Recruits were instructed to carry out administrative oversight over pre-existing local governing structures and taught that occupation amounted to a form of ‘“government” without politics’ (p. 23). At the same time, the Charlottesville faculty tried to invent a virtuous tradition of military government that dissociated occupation from imperialism. In doing so, they construed a lineage of military government that affirmed that Americans were particularly apt in governing territories overseas because they respected pre-existing customs and structures. More significantly, however, the USA had its own history of occupation closer to home to contend with, namely the occupation of the South after the American Civil War. As Carruthers shows in a highly innovative section, memories of the occupation of the South and tales about the presumably tyrannous rule of the North and its disrespect for local customs were very vivid among those involved in preparing for post-war occupation. This produced an emphasis on leaving ‘indigenous traditions intact’
that owed much to an ‘extrapolation from racially overburdened domestic history’ and less to ‘cultural sensitivity’ (p. 27).

A frequent point of reference in Carruthers’ story is John Hersey’s 1944 novel *A Bell for Adano*, which was highly influential in propagating notions of benign American occupation and fittingly won the Pulitzer prize on VE day, setting the template for future accounts of the ‘good occupation’. Set in occupied Sicily, the novel’s hero Major Joppolo embodies the honourable and sympathetic American officer who, against a backdrop of post-war privation, works in the interest of the local community and in doing so imparts the virtues of democracy while respecting the way of life of the Sicilian population. As Carruthers well demonstrates, however, this idealized image contrasted markedly with practices of American officials on the ground who often used racially inflected discourses to describe Sicilians as backward people who did not conform to their idea of Europeanness. Abuses and rapes were widespread. Similarly, occupation officials did not see their job as that of teaching the lofty ideals of democracy, but thought instead they simply had to get basic services and institutions running again. *A Bell for Adano* thus stands throughout the book as an image for how the tale of the good American occupation was spun almost in real time while the multiple US occupations were unfolding.

In two tandem chapters on the end of the war and beginning of occupation in Germany and Japan, Carruthers shows how Americans experienced, enforced, and performed victory. She describes how some GIs were markedly uncomfortable with their role as conquerors, while others enjoyed the many luxuries that were now afforded to them by being part of an occupying power. Looting and theft were widespread. In Japan, the Americans performed a carefully staged surrender ceremony to impress on the Japanese the totality of defeat. While the American authorities construed the occupation of Japan from the outset as particularly virtuous through heavy censorship of the US and Japanese press, the initial period of occupation was marked by extreme levels of looting, rape, and vandalism. The occupation of Okinawa resulted in mass population movement, the burning of villages, and the concomitant destruction of much of the pre-existing local culture. In contrast to Germany, American soldiers also experienced Japan as a culturally and ethnically unfamiliar space. Many a GI turned into ‘an amateur ethnographer, attuned to mark-
ers of difference’ (p. 106), with the ‘Japanese brain’ (p. 103) targeted as the enemy that had to be defeated during the occupation. Similarly, as Carruthers shows, influenced by racializing and orientalizing discourses, occupation personnel often described both Japanese culture and Japanese bodies as inferior, with a frequent trope being the lack of cleanliness and the general backwardness of the country.

In fact, one of the main recurring themes throughout Carruthers’s story is that occupation was, in many respects, a bodily experience. Human excrement, odours, and intimate contact with bodies both dead and living filled many pages of contemporary letters and diaries. One of the main loci of such experiences were female bodies. This is, of course, familiar territory for historians of mid twentieth-century occupations, but Carruthers’s chapter on the multiple iterations of ‘fraternization’, and the various changes to official policy intended to keep it in check, shows how romantic and sexual relations between the occupiers and the occupied had significant repercussions at home, threatening the positive image the USA wanted to project about their occupations, with the high venereal disease (VD) rate remaining a constant sting. In response, German ‘Fräuleins’ were quickly construed in the US press as ‘hyperideologized, hyperfertile, predatory’ females (p. 126), and the role of coercion involved in some such encounters remained mostly in the dark. While non-fraternization rules were, to a large extent, intended to transmit to Germans the idea of collective guilt, Carruthers argues that orders issued in Okinawa to American troops to stay away from relations with the local population were grounded in racial concepts that relied on an ‘American identity of pristine whiteness’ (p. 133).

Human bodies are also at the core of Carruthers’ discussion of displaced persons (DPs), which brings out the abusive treatment and racial hierarchies applied to different groups of DPs. In Germany, many American members of the occupation saw survivors of concentration and extermination camps as ‘abstractly deserving but personally repellent’ (p. 162), and many contemporary accounts dwell on the filth and excrement surrounding DPs. This produced a dehumanizing portrayal of DPs, and in particular of Jewish survivors, an attitude exacerbated by the antisemitism that was widespread amongst American personnel. Carruthers argues that American encounters with DPs enduring dismal living conditions led to the paradoxical situation that many occupation officials had more sym-
pathy for their former German foes than for those who had suffered persecution or had been subjected to forced labour. In Japan, a similar racial hierarchy was in place, with the Japanese at the top and Korean DPs at the bottom, the latter being stigmatized as either communists or criminals. In both Germany and Japan, the occupiers consequently vilified specific groups of DPs for crimes such as theft and black market activities that were also carried out by the occupiers and the majority population.

While occupation work was posthumously beatified as a particularly worthwhile endeavour, Carruthers describes how a large number of occupation officials and soldiers regarded their jobs as particularly unrewarding, boring, and distasteful. Widespread demoralization led to large-scale protests in early 1946 in both Asia and in Europe, with soldiers demonstrating for their quick demobilization. In the USA, American wives and their children mobilized under the flag of ‘Bring Back Daddy’, invoking the restoration of family life, the threat of increasing divorce rates, and looming sexual impropriety as compelling reasons for a fast demobilization. The USA responded to this crisis in morale with what Carruthers fittingly describes as the domestication of occupation, flooding American troops with consumer goods, leisure activities, and travel opportunities. They also set up an almost entirely isolated world of American clubs and residential spaces, which commentators at the time criticized as an imperial practice of social segregation, captured in the striking formula of ‘Hans Crowism’. Yet even the arrival of American families in April 1946 did not, in Carruthers’s interpretation, contribute significantly to raising morale and morals, with the incidence of VD, alcoholism, and pilfering remaining high.

Carruthers debunks the various myths surrounding the inherent goodness of the post-war occupations with much brio and erudition, deploying her trademark evocative prose style to give colour to the bleak post-war world. The occupations that she describes do not fit neatly into the familiar story of democratization and re-education, but are stories of moral ambiguity that find their point of gravity around fraternization, violence, corruption, looting, and racism. Some may argue, of course, that historians should move beyond assessing whether an occupation was a good or bad thing, and concentrate on exploring how and in what ways American military occupation transformed the societies under their rule. Others may con-
tend that despite the multiple negative, and indeed often brutal by-products as well as the very top-down, conservative inflection of post-war ‘democratization’, the occupations did ultimately produce significant institutional and political changes after many years of authoritarian rule. Yet Carruthers’s book is essentially a study of the occupiers, not of the occupation’s socio-political impact on the occupied, and here her contribution is genuinely illuminating, for it brings to light the gulf between the often morally muddled experiences of occupation personnel and the crude representation of occupation as a virtuous project pervading public discourse today. In doing so, she has set the ground for a more extensive investigation of the construction of the memory of post-war occupation, a subject which has hitherto escaped historians, but which they would be well advised to take seriously in the future.