Although the debate about an anti-modernist Sonderweg in German history between 1871 and 1945 has been largely settled among historians,¹ the notion of German exceptionalism remains virulent in the research on German military history.² After the Second World War, British historians identified Prussian militarism and the widespread ‘subject mentality’ (Unterthanenmentalität) in Wilhelmine Germany as key factors for the catastrophic course of German history in the first half of the twentieth century.³ Since the 1960s several generations of German scholars have critically reassessed Prussian-dominated German military culture with the aim of identifying those elements that

³ See e.g. A. J. P. Taylor, The Course of German History: A Survey of the Development of German History since 1815 (London, 1945). During the war Robert Gilbert Vansittart, who was a diplomat in the Foreign Office and a prominent
paved the way for the criminal and genocidal warfare of Germany’s armed forces in the Second World War. In this context, a wide range of aspects has been discussed: the special relationship between military, state, and society, which prevailed not only in Imperial Germany but also in the Weimar Republic; the unbroken power of the old military elites in the Reichswehr after the First World War; and peculiarities both of German mentality and of military strategy. Many researchers argue that the experience of the Great War and the ‘trauma’ of defeat played a crucial role in the radicalization of German military culture. The proponents of the Sonderweg theory claim above all that the Imperial army cultivated traditions which predisposed Germany to ‘absolute destruction’, and that the practices of war in Imperial Germany differed significantly from those in other Western countries. Some researchers, such as Jürgen Zimmerer, see direct continuities between genocidal practices of violence in German colonialism and the Holocaust. However, this master narrative has been repeatedly contested in the past by military historians who emphasized the differences between traditional Prussian-dominated military culture and the military spirit under Nazi rule. Others have focused on ruptures and elements of refusal in German military history as indicators of possible alternative paths of historical development.

This review article will discuss three recent publications on German military history covering the period from 1871 to 1945, which all seek to further define the ‘German way’ of fighting, as Paul Fox describes it (p. 7), and to explain peculiarities of German warfare and practices of violence. Since these publications deal with consecutive historical periods, they open up a long-term perspective, which allows us to draw connections between similar or related phenomena and thus identify continuities and discontinuities in German military history. One proponent of a sharp anti-German line, promoted this view of German history in a series of radio broadcasts. His portrait of German history, which was published in 1941 under the title Black Record: Germans Past and Present, became a huge bestseller. See Jörg Später, Vansittart: Britische Debatten über Deutsche und Nazis 1902–1945 (Göttingen, 2003).


itary culture. What also makes these studies interesting for comparison is their authors’ quite different theoretical and methodological approach to the analysis of the topic. In his inquiry *The Image of the Soldier in German Culture*, Paul Fox analyses the visual representations of Germans at war from 1871 to 1933. Benjamin Ziemann’s study *Violence and the German Soldier in the Great War* closely examines how soldiers experienced violence during the First World War and how it affected post-war society. While the first two studies conceptually focus on one particular aspect of German military culture, Ben Shepherd’s book *Hitler’s Soldiers* aims to present a general and comprehensive analysis of the German army under Nazi rule.

Fox’s study is a substantial contribution to a visual history of the twentieth century and, especially, of modern conflict. Proponents of visual history believe that images produce meaning and thus fundamentally affect reality. It is indeed worthwhile to study visual representations of Germans at war. Illustrations, pictures, and photographs not only reveal a great deal about German military culture, but in the era of mass communication also had enormous influence on the way the recipients of these images perceived and thought about the military and war. One special characteristic of visual representations is their aesthetic dimension. Because of their rapid intelligibility—pictures convey meaning through association—they offer a more direct access to comprehension. Fox analyses a body of over forty representative images of Germans at war, mainly samples from illustrated histories and photobooks. As his main interest, apart from defining the German way of fighting, is describing the relationship between cultural production and military thought, he concentrates on popular visual accounts of war, which were broadly patriotic and depicted idealized forms of soldierly behaviour. In order to further define how Germans fought, he examines how the three components

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7 Horst Bredekamp’s *Bildakttheorie* (‘image act theory’) claims, following the tenets of speech act theory, that images do not only function as representations of reality, but that they have the power to affect the recipient’s thoughts, emotions, and behaviour. See Horst Bredekamp, *Theorie des Bildakts: Frankfurter Adorno-Vorlesungen 2007* (Berlin, 2010).
of fighting power—the moral, the physical, and the conceptual—were related to each other in visual representations of war from 1871 to 1933. For Fox, the most striking element of continuity in the field of popular visual representations of war is the notion of a ‘German way of fighting’ that was ‘characterized by aggressive operations conducted by a coherent force whose collective will to battle in itself advances a compelling claim to moral superiority, even in defeat’ (p. 190).

In the first part of his book, the author describes the visual research methods of his inquiry and extensively reflects on the cultural sources of German military thinking. It would have been more fruitful, though, to integrate these reflections into the empirical part of the study in order to establish a closer link between theory and practice. The chapter ‘Politics of Border-Landscapes’ shows how visual representations of contested, newly conquered, or lost border territories were used to influence the mental map of their viewers. In the months following the armistice, the integrity of Germany’s eastern borders became a prominent theme in visual culture. Faced with the threat of losing territories to Poland, visual representations of German–Polish borderlands aimed to mobilize patriotic sentiments. These propaganda images symbolically underlined Germany’s moral claim to the eastern territories by typically portraying soldiers who conquered or defended the nation’s borderlands as working hand in hand with farmers who cultivated the terrain. The representations of border zones and conflicts drew upon visual traditions that go back as far as the Franco-Prussian war, when the German Empire conquered Alsace-Lorraine. The illustrated histories of the Franco-Prussian war also present soldiers and farmers as equally important for the integration of newly conquered territories into the nation.

Fox bases his analysis of the visual representation of the Great War on two prominent photobooks edited by Franz Schauwecker and Ernst Jünger respectively, which were published around ten years after the armistice. This narrow perspective on nationalist lit-

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8 For example, posters encouraging former soldiers to volunteer for border security operations circulated.

9 See Franz Schauwecker, So war der Krieg: 200 Kampfaufnahmen aus der Front (Berlin, 1927); Ernst Jünger, Das Antlitz des Weltkrieges: Fronterlebnisse deutscher Soldaten (Berlin, 1930).
erature can be criticized as it entirely leaves out republican and left-wing interpretations of the war experience, an omission compounded by Fox’s failure to discuss the political implications of these publications. The aesthetic stylization of the war experience needs to be viewed in the context of the massive political and social transformations of the post-war era. The interpretation and memory of the war experience were major points of contention between the opposing political camps in the Weimar Republic, as Ziemann demonstrates in his study. The photobooks were designed to engage reader-viewers emotionally and to make them identify with the German soldier. Therefore they had a documentary character, focusing on episodic events experienced at the minor unit level or by individuals. Testimony from veterans is included to lend them authenticity.

Fox points out that the visual accounts of the Great War centre around the values associated with soldiers’ experiences at the Front, such as bravery, selflessness, comradeship, and teamwork, which ‘were antithetical to the notion of defeat’ (p. 108). This narrative appealed to many veterans who regarded themselves as moral victors regardless of the outcome of the war, as it emphasized the ‘great deeds and great suffering’ of German front-line soldiers and transformed mass death into a meaningful event. The desire to restore the honour of German soldiers after 1918, however, also fostered tendencies to assign the blame for military defeat to external circumstances, preparing the breeding ground for the exculpatory stab-in-the-back myth. Furthermore, the visual representations of the Great War maintained the illusion of human agency. They frequently incorporated ‘tropes of individual heroism in near-overwhelming circumstances’, underlining the superior moral qualities of German soldiers and their indomitable will to battle, even though in industrialized warfare masses of men and material were far more important than individual bravery (p. 115).

The experience of industrialized warfare deeply affected German military thinking and planning for future wars. Fox discusses the impact of modern military technology on German concepts of war in detail. Although modern weaponry became increasingly important

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in the Franco-Prussian War (1870–1), it played a somewhat more subordinate role in the field of visual representations. During that period, illustrated visual histories foregrounded the outstanding moral qualities of German soldiers rather than their technological excellence. In the course of the First World War, however, the relationship between morale and technology underwent a significant change, especially in the face of defeat. For the first time, technological superiority was acknowledged as a decisive factor in determining victory. Visual accounts of Germans at war now emphasized the importance of modern weaponry to national survival. The tank, which became the icon of military modernity, is presented as the most decisive weapon of the machine age. Still, the ineluctable will to battle is privileged over technological and material aspects of fighting power and praised as the most important feature of German moral superiority. The message conveyed by illustrated histories of the First World War is that situations of technological inferiority give German soldiers the chance to prove their superior moral qualities. The notion that physical and material inferiority could be overcome by such superior qualities would reappear in the Second World War, as Shepherd outlines in his study. Hitler believed that sheer fanaticism and an iron will to victory could stop the enemy. Thus he refused to countenance withdrawal in critical situations, instead issuing orders to stand firm at any cost. The fanatical spirit of National Socialist warfare became especially virulent in the final stages of the Second World War, when soldiers were expected to fight with sheer willpower against well-equipped and highly mechanized Allied forces.

In his study, Shepherd also broaches the relationship between modern military technology, strategic thinking, and warfare. He shows how the German army developed new theories for the deployment of armoured forces in the inter-war period in order to be prepared for future warfare. Having learned the bitter lessons of the First World War, military planners aimed to combine German organizational skills and moral qualities with the destructive potential of mechanized warfare. The Reichswehr and, later on, the Wehrmacht, consistently tested, practised, and refined both German armoured warfare doctrine and the employment of technologies in tactical combination in manoeuvres and war games. In 1935, when the Nazi regime began to rearm Germany, the first three tank divisions ‘designed as self-supporting mini-armies’ were formed, which was a
major development in armoured warfare’ (p. 18). At the heart of German armoured doctrine was the concept of Auftragstaktik (mission-type tactics), which granted independent front-line commanders the flexibility to respond to changing battlefield situations, thus ensuring mobility, manoeuvrability, and the possibility of immediate response. The German doctrine of armoured warfare was not essentially new, but evolved from traditional strategic thinking, which was largely shaped by Prussian concepts of warfare. Here we find a clear connection between the findings of Shepherd and Fox, who characterized the German way of fighting as driven by an aggressive, offensive spirit. Fast and flexible warfare required a quick exchange of information. The German army benefited enormously from the use of highly developed radio communication, unparalleled in other armies. Because of these efforts, the German army gained high proficiency in combined-arms mobile warfare, which contributed largely to its success, as Shepherd points out. In fact, this is a central argument of his book.

Shepherd’s rather traditional study of military history with its emphasis on military thinking, command structures, and campaigns is a skilful synthesis of previous research on the German military in the Second World War. The novelty of his approach lies in his exclusive focus on the army. First, Shepherd stresses ‘how vast, diverse and subject to profound change the army was over the course of the war’ (p. 521). As his study strives to present a comprehensive picture of the army under Nazi rule, it covers the entire span of the war and tackles a wide range of topics. It considers, among other things, the different theatres of war, the social structure and ideological indoctrination of the army, the mentality of the officer corps, the army’s involvement in war crimes and the Holocaust, and the different occupation policies established by the army in the various occupied countries.

The spectacular early successes of the war in particular gave the German army an aura of invincibility. That is why it ‘still enjoys a reputation as the most proficient, effective fighting force to take the field of modern land combat’ (p. xi). Shepherd’s study explains why the army was so successful, but also deconstructs the myth of Germany’s superior fighting power by shedding light on the flaws inherent in the army. It not only reveals the grave mistakes the army made at all levels of warfare, even during the early, victorious campaigns,
but also demonstrates how much the army’s successes depended on the weaknesses and errors of Germany’s opponents. It argues that the German army was not only defeated because of the vast numerical superiority of the Allies. The Allies’ capability to learn from their military failures and to improve at the tactical, operational, and strategic levels was also significant for their triumph over Nazi Germany. After the war, former German army commanders tried to put the blame for the debilitation of the army’s performance on Hitler’s mismanagement of the war efforts.

Yet the army was not a victim of Hitler and the Wehrmacht High Command. As Shepherd demonstrates in his book, high-ranking army commanders made numerous strategic and operational blunders that contributed to Germany’s eventual defeat. He points out how profoundly the army leadership disregarded logistics and intelligence, and discusses economic and organizational weaknesses. The biggest failure of the army leadership, however, lay in its inability to develop a grand strategy regardless of the restrictions imposed by Hitler’s plans. Shepherd underlines that the army’s operational goals exceeded its practical means on the Eastern Front from as early as the summer of 1941, which ‘fatally damaged the German army at the operational level’ (p. xii). Moreover, Shepherd identifies an ‘infernal feedback mechanism’ that led to the army’s ultimate destruction. During the final two years of the war, the ‘army’s moral and military failure reinforced one another’ (p. 536). The desperate military situation reinforced the brutality of the German troops. The excessive brutality, however, hardened the enemy’s will to destroy Nazi Germany and thus contributed further to the army’s military defeat.

Shepherd does not refer to any of the classical theories of Wehrmacht research in order explain why the army maintained the struggle for so long, though it was obvious that the war was effectively lost for Germany. He explains the fact that the army fought almost to the point of self-destruction by the warped sense of honour harboured by many German senior officers, arguing that the unresolved trauma of the Reich’s military collapse in 1918 strengthened the latter’s will to keep fighting to the bitter end this time. This is a valid

11 Other scholars have argued that cohesion among primary groups, Nazi indoctrination, the Nazi terror apparatus, the Hitler myth, comradeship, and the German sense of duty were the main reasons why the German army did not disintegrate until May 1945.
argument. The experience of Germany’s humiliating defeat in the First World War certainly influenced senior officers’ behaviour in the final phase of the Second World War. Shepherd’s argument that a number of senior military leaders continued the fight to expunge the dishonour they felt in the wake of the July 1944 attempt on Hitler’s life, which was itself carried out by a group of high-ranking Wehrmacht officers, however, is not at all convincing. He argues further that soldiers of all ranks simply continued to fight out of sheer pragmatism, as it was their best option for survival in the desperate situation of spring 1945. But he also stresses that fear of the vengeful Red Army invading the Reich was one of the most important motives for sustaining the fight in the last months of the war.

Shepherd adheres to the well-known thesis of a ‘partial identity of goals’ shared by the military elites and the Nazi regime, first put forward by Manfred Messerschmidt: ‘the leadership of the German army willingly entered into a Faustian bargain with Adolf Hitler that provided the opportunity and means to meet its highest aspirations’ (p. 521).12 He recognizes several important areas of overlap between the officer corps’ worldview and Nazi thinking: racial contempt for Slavs, deep-rooted antisemitic resentments, pronounced anti-Bolshevism, and ‘the technocratic ruthlessness that had characterized the officer corps since the First World War’ (p. 55). This is a major reason, he argues, why so many soldiers participated in war crimes and the Holocaust on the Eastern Front, or at least tolerated them. Already during the Polish campaign, it became clear that the face of warfare in the East was to differ significantly from that on the Western Front.

As we learn from Fox’s study, a broad spectrum of the German public after the First World War believed that Germans had a moral claim to contested German–Polish borderlands. Thus it is not surprising that the invasion of Poland in 1939 was broadly accepted by German soldiers, who regarded it as fulfilment of revisionist claims to former German territories lost under the terms of the Treaty of Versailles. The Polish campaign must, however, also be understood within the broader historical context of a German ‘drive to the East’ (Drang nach Osten). In the nineteenth century German nationalists demanded an expansion into Slavic lands as they were convinced that Germans had a civilizing mission there. They legitimized their

demands by reference to the eastern colonization of Germanic peoples in the high and late Middle Ages. In his study, Ziemann also demonstrates that the ‘German soldiers’ willingness to fight on the Eastern Front’ during the First World War ‘was directed not simply against Russia’s military, but also its entire culture and society’ (p. 59). The Nazi Lebensraum concept, which aimed for the extermination and resettlement of Slavic populations and the ‘Germanization’ of former Slavic territories, could build upon the vision of a German ‘drive to East’. In fact, it was only a small step from considering the Slavs culturally inferior to the concept of racial inferiority as propagated by Nazi ideology.

It is a well-known fact that the army committed war crimes on an institutional level, as did individual officers and soldiers of all ranks. Shepherd does not produce a single explanation of why individual units or soldiers participated in these crimes. He also stresses the blatant differences with regard to involvement in war crimes. Occupation units were, for example, much more complicit in war crime than front-line units. In this context, several factors played a role: one’s age, the unit one belonged to, the level of ideological indoctrination, and numerous situational factors such as time and place. Both Ziemann and Shepherd show that stereotypes of cultural or racial inferiority were important factors enhancing violence. Yet they also highlight the complex interactions between ideological indoctrination, racial contempt, and circumstances. By comparing the anti-partisan struggle on different fronts, Shepherd illustrates how ideological indoctrination, situational factors, and feelings such as fear, anger, or desire for revenge contributed to the escalation of violence against partisans and civilians suspected of supporting the partisan movement. He shows that Nazi racial ideology played a significant role in this regard. While the army exhibited restraint in partisan warfare on the Western Front, it practised brutal violence in anti-partisan operations in Greece, the Balkans, and on the Eastern Front. Shepherd argues that the German military had a ‘pronounced hatred for irregular warfare’, which had its roots in the Franco-Prussian War of 1870–1 and first manifested itself in the brutal reaction to alleged franc-tireur attacks in Belgium and France in 1914 (p. 290). The question of whether an organized franc-tireur movement existed remains a highly controversial issue to the present day. John Horne and Alan Kramer claim that the presumed sniper attacks were just a product of self-
German atrocities of 1914 play a central role in the debate about the brutalization of warfare, since they are often considered as the starting point for the escalation of German violence against civilians in the first half of the twentieth century.

The question that arises is this: does the ruthless brutality with which the military responded to irregular warfare constitute a line of continuity in German military culture? Ziemann and Shepherd seek to explain the dynamics of wartime violence and identify the reasons for the escalation of brutality in both the First World War and the Second World War. They show that in both wars similar factors led to the outbreak of irregular violence and that similar arguments were employed to justify severely disproportionate reprisals and other harsh measures against the civilian population, for instance, the necessity of war or security reasons. However, similarities should not be confused with continuities. The war the German army unleashed against Soviet Russia in 1941 was *sui generis*. Nazi warfare, characterized by the readiness to commit massive war crimes and genocide, differed fundamentally from the war in 1914.14

Ziemann’s study is an impressive example of how to write modern military history. His argumentation is clear and concise, and at the same time he provides a vivid description of the manifold experiences of violence during the First World War in numerous biographical case studies. Ziemann explores different types of violence practised by the German army and studies the soldiers’ motives for perpetrating violence, but also for refusing to take part in it. Since violence is a core wartime experience, the subject is highly relevant. Though the main focus of the study is the Western Front, it also sheds light on significant differences between the Eastern Front and the induced paranoia. In 2017 the art historian Ulrich Keller published a book in which he opposed this widely accepted thesis and presented evidence indicating that an organized guerrilla movement existed. For example, he referred to German military hospital records from 1914 saying that German soldiers had wounds that could not have been inflicted by regular army weapons. See John Horne and Alan Kramer, *German Atrocities 1914: A History of Denial* (New Haven, 2001); Ulrich Keller, *Schuldfragen: Belgischer Untergrundkrieg und deutsche Vergeltung im August 1914* (Paderborn, 2017).

Western Front. The First World War is understood as a ‘laboratory of violence’, since all armies experimented with new forms of the latter.

Ziemann devotes one chapter of his study to Ernst Jünger’s reception of violence. Since Jünger’s depiction of trench warfare has already been well researched, Ziemann attempts to provide a fresh perspective on the subject, but it would have been more interesting if he had chosen a less well-known author. Ziemann is especially interested in Jünger’s authentic experience of violence in front-line combat. Thus he analyses his original war diaries, rather than the literary, stylized version of his war accounts, which were first published in 1920 under the title *In Stahlgewittern* (*Storm of Steel*). Jünger’s war diaries show that the killing of the enemy followed a complex set of rules and that artillery dominated the battlefields of the First World War. Trench warfare often condemned the soldiers to passivity. The ‘man against man’ fight Jünger had dearly longed for was the exception rather than the rule. Jünger’s attitude towards violence, as displayed in his war diaries, was typical of a conservative front-line officer at that time. Throughout the war, he was guided by the ethos of the Prussian officer corps, striving to be a worthy representative of the German officers’ caste. Thus even after four years of exposure to brutalizing violence on the front-line, his behaviour was primarily determined by traditional soldierly values, such as honour and comradeship.

Historians still disagree about the extent to which prisoners were killed on the battlefield after surrender in the Great War. While some scholars assume that this was a frequent phenomenon, Alan Kramer claims that prisoner killing ‘was the exception to the general rule’. The analysis of Jünger’s war diaries confirms Kramer’s theory as it reveals that surrendering soldiers were generally treated well, even though prisoner killings occurred from time to time in the heat of battle. Ziemann therefore concludes that the majority of the soldiers on the Western Front regarded the enemy as fellow human beings,

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which marks a decisive difference from the Wehrmacht’s war of annihilation against Soviet Russia. Ziemann claims further that Jünger’s alter ego in the war diaries is ‘neither a protofascist fighting machine nor a prophet of the amalgamation of man and military machine’ (p. 90). This is a strong indicator that Jünger did not undergo a process of political radicalization until after the war. It appears that the experience of defeat and revolution had a great influence on both his political thinking and his attitude towards violence. The considerable revisions made to his published war accounts in the course of the 1920s also support this hypothesis. The later versions of In Stahlge-wittern display a stronger nationalist colouring and a more aggressive idealization of combat.

A large section of Ziemann’s study deals with the refusal of violence in the German army during the First World War. Ziemann suggests that deserters ‘were in many respects completely ordinary soldiers’, who lacked a common social or generational background (p. 119). In many cases, desertions were caused by shortages of supply, poor quality of food, or an easy opportunity arising. Only a minority deserted for political reasons. Nevertheless, desertion rates were especially high among national minorities in Imperial Germany (such as soldiers from Alsace-Lorraine and Poles). All deserters were, in a certain sense, outsiders. Desertion was not a group phenomenon; most deserters planned their escape from the front-line service alone. Only in the final phase of the war did deserters ‘turn from outsiders into a mainstream current’ (Fox, p. 120). Military historians unanimously agree that during the summer of 1918 morale among German field soldiers on the Western Front drastically deteriorated. This progressive erosion of morale caused soldiers to desert or absent themselves from their units, surrender to the enemy, or search for other ways of evading front-line service. After the failure of the last German offensive on the Western Front in July, ‘the army began deflating like a pricked balloon, new arrivals being vastly outnumbered by those leaving’.17 The gravity of the situation can be appreciated by the fact that the Supreme Army Command and the field authorities issued increasingly harsh and desperate directives to stop this ongoing process of disintegration.

17 David Stevenson, With our Backs to the Wall: Victory and Defeat in 1918 (Cambridge, 2011), 288.
There remain, however, two controversial questions: when and why did the German army actually collapse? In 1986 the military historian Wilhelm Deist put forward the thesis that a hidden military strike took place among German soldiers in the final months of the First World War.\(^\text{18}\) The hidden strike is not to be understood as an organized strike, but as a mass movement of soldiers fleeing the front on their own initiative to save their lives in the face of looming defeat. Deist claims that this covert mass flight was responsible for the Reich’s defeat, thus providing a fundamentally new interpretation of Imperial German history. According to his theory the Reich was not overwhelmed by superior enemy forces, nor did it fall because the soldiers were ‘stabbed in the back’ by internal enemies. Rather, it simply collapsed militarily from within. In his 2008 comparative study on the fighting morale of German and British troops, the historian Alexander Watson claims, however, that ‘shirking’ was not a mass phenomenon on the Western Front.\(^\text{19}\) Instead, he points out that large numbers of German soldiers were taken prisoner after giving themselves up voluntarily in 1918. He argues further that the mass surrender of German soldiers could not be interpreted as a symptom of a dramatic decline in fighting morale. The soldiers were strongly encouraged to lay down their weapons by Allied propaganda, and junior officers led their men to surrender in an orderly fashion. Watson therefore concludes that the collapse of the German field army was not the result of a hidden strike, but ‘foremost that of an ordered surrender’.\(^\text{20}\)

Ziemann rejects Watson’s thesis of an ‘ordered surrender’ as an explanation for the army’s disintegration in the summer of 1918. His criticism is directed not only at the weak empirical evidence—in Ziemann’s opinion Watson only presents a single detailed piece of evidence to support his narrative—but also at the historical and political implications of Watson’s theory. By emphasizing that German soldiers were obeying orders until the end of the war, and only went


\(^{19}\) See Alexander Watson, *Enduring the Great War: Combat, Morale and Collapse in the German and British Armies, 1914–1918* (Cambridge, 2008).

\(^{20}\) Ibid. 123.
into captivity in large numbers led by junior officers, his theory affirms a prominent part of the ‘stab-in-the-back myth’, which says that the field army never participated in a revolutionary mass movement.

Ziemann supports and empirically substantiates Deist’s thesis of a hidden military strike based on materials from the Bavarian War Archives in Munich. In his study, he reconstructs the various practices of evading front-line service in the summer of 1918 in order to obtain a more precise picture of this mass refusal of violence, which therefore represents an important event in German military history. The largest group of soldiers moving around in the rear area were probably soldiers who were sick or wounded but still able to walk. Besides this group, a rapidly growing number of men absconded from troop trains carrying them from Germany to the front. The Bavarian sources provide evidence that allow us to quantify the extent of the covert strike. Ziemann estimates that an absolute minimum of 185,000 soldiers left their units to move rearwards in the summer and autumn of 1918.\(^{21}\) He adds that the number of men reported sick—more than 900,000 men between August and the armistice—far exceeded the numbers of wounded, killed in action, or taken prisoner.

Ziemann agrees with Watson and others ‘that a truly comprehensive disintegration of command authority’ on the front did not take place until October 1918, when the German government asked for an immediate armistice on the basis of Wilson’s Fourteen Points.\(^{22}\) But he also emphasizes that the military apparatus had been steadily eroding since June, which was why the army collapsed as quickly as it did. The ‘barbarization of military justice’ under Nazi rule was an obvious reaction to the hidden military strike (Shepherd, p. 384). In order to prevent this disintegration of the army from within repeating itself, the Nazis established a harsh military justice system to maintain morale and discipline. Desertion, for example, was punishable by death. As German military fortunes began to wane, military courts imposed particularly severe punishments that were intended to terrify soldiers into obedience.

\(^{21}\) Wilhelm Deist arrived at an estimate of between 750,000 and one million based on Erich Volkmann’s report for the Reichstag from 1929.

\(^{22}\) Christoph Jahr, Gewöhnliche Soldaten: Desertion und Deserteure im deutschen und britischen Heer 1914–1918 (Göttingen, 1998), 166.
The question of whether the war experience had brutalizing effects on German society is the subject of continuing controversy among scholars. It poses, however, several theoretical and methodological challenges: how can one measure the degree of brutalization of a society? Is it possible to establish direct causality between the violence experienced in war and different types of brutal behaviour that occurred in the Weimar Republic? How can one reason exclude others? How can we distinguish whether aggressive behaviour has its origin in wartime or post-war experiences? Ziemann and others differentiate analytically between the brutalization of soldiers who actively participated in the war and ‘violence-affirming interpretations, images and myths’ which circulated in the post-war era and were also adopted by those who had not actively fought in the war themselves (Ziemann, p. 166).

In his ground-breaking study, Fallen Soldiers, George Mosse claimed that the experience of industrialized warfare led to a brutalization of European societies after 1918. For Mosse, brutalization was not only a ‘process that penetrated most aspects of German political life’, but also ‘an attitude of mind derived from the war and the acceptance of war itself’. The brutalizing effect of the war experience manifested itself, according to Mosse, above all in an appetite for violence, an emphasis on aggressive masculinity, and the militarization of society. There is, however, a powerful counter-argument to Mosse’s brutalization theory. Not all European countries showed violent tendencies after 1918; instead, the losers of the Great War tended to be especially subject to this development. Thus Robert Gerwarth argues in his book The Vanquished that it was not the experience of violence during the Great War that led to a brutalization of German society, but rather the experience of defeat and revolution. He describes the period between 1918 and 1923 as the ‘long end of the First World War’, since many European countries faced enduring conflicts. Contrary to all hopes and expectations, the peacetime order established at Versailles did not end violence across Europe. Those countries which were on the losing side in the Great War—the

23 See Mosse, Fallen Soldiers.
24 Ibid. 159, 161.
Habsburg, Romanov, Hohenzollern, and Ottoman empires and their successor states—were shaken by three types of conflict: inter-state conflicts, civil wars, and political or social revolutions. In Germany, violence did not vanish after 1918, but became an essential part of the political culture in the inter-war period. For example, the founding of the Weimar Republic was accompanied by a wave of violence that in some places even reached civil war-like conditions. Gerwarth is right, of course, in pointing out that the immediate post-war years have received too little attention so far in the context of explaining the destructive path of German history in the first half of the twentieth century. It is true that many Germans became habituated to paramilitary violence and radicalized themselves in the political and social turmoil of the post-war era, as Ziemann showed for the case of Ernst Jünger.

Both Ziemann and Gerwarth strive for a more differentiated analysis of the impact of violence on post-war societies by adopting a transnational perspective on this issue. They seek to determine the decisive factors for the escalation of violence in the twentieth century in order to gain a better understanding of the phenomenon of violence per se and general mechanisms of brutalization. Ziemann also argues that it was not the war experience itself that had brutalizing effects on post-war European societies. The crucial factor was how societies dealt with the war experience after the violence ended. He claims that the effective containment of post-war violence depended largely on whether societies found a common frame of reference for interpreting and remembering the war experience. In this regard, he recognizes significant differences between the British, French, and German situations. While German post-war society lacked an overall frame of reference that could have had the potential to unify different social classes and political parties, the British and French nations arrived at a consensual interpretation of the war experience transcending heterogeneous social milieux and political camps.

The British nation successfully revived the unifying self-idealization of itself as a ‘peaceable kingdom’ promoting the values of civility and peaceableness. In France, veterans’ associations played an important role in shaping a common national framework for remembering the Great War. Their willingness to co-operate regardless of

their diverging political orientations—in 1927 they even founded an umbrella organization—conveyed a sense of unity and thereby had remarkable stabilizing effects on the political culture of the Third Republic. The British and French examples show that the existence of a collectively shared interpretation of the war experience made it easier for the veterans to re-adapt to civil society and distance themselves from the violence experienced prior to 1918.

In Germany, by contrast, the debate on the interpretation and memory of the war experience was deeply polarized, since the Weimar Republic ‘lacked a liberal culture that could have functioned as a shared point of reference’ (Ziemann, p. 164). Thus the conflict between the opposing political camps over the interpretation of the war escalated. The creation of the stab-in-the-back myth accusing socialists, Jews, and women of having stolen victory from the undefeated army is an important example of the nationalists’ efforts to gain discursive hegemony over the interpretation of the war experience. In the wake of defeat, the nationalist camp underwent a process of radicalization. A considerable number of veterans joined right-wing veterans’ groups, such as the Stahlhelm (Steel Helmet), which pursued an aggressively revisionist policy. The nationalist-conservative spectrum did not reject violence, but glorified it as a legitimate political tool. The enthusiasm for military traditions and practices remained unbroken in large parts of German society.

With regard to the brutalization of soldiers who actively participated in the First World War, Ziemann concludes, primarily based on previous research, that ‘all in all, it appears unlikely that the experience of war resulted in extensive brutalization among German soldiers’ (p. 169). This claim raises strong doubts, mainly because it is not empirically substantiated. In his study, Ziemann himself presents evidence indicating that the war experience had brutalizing effects on front-line soldiers. He discusses the violent excesses of the ‘Black and Tans’ against civilians in the Irish War of Independence. Among those were many veterans of the Great War. Thus, ‘the example of the Black and Tans shows that the aggression built up during the war had the potential to develop and intensify further’ (Ziemann, pp. 160–1). Moreover, one could argue that German veterans had indeed adopted ‘wartime attitudes, which persisted into the post-war period’, such as the preference for violent solutions to conflicts.27

27 Mosse, Fallen Soldiers, 161.
In the last section of his book, Ziemann focuses on those political and social forces in the Weimar Republic which rejected militarism and violence. He presents the interesting biographical case study of Hermann Schützinger, a former career officer in the Bavarian army, who served in a people’s militia immediately after the war and later joined the Reichswehr, but eventually converted to pacifism and was involved in the No More War Movement. Schützinger’s delayed rejection of violence is just one of many examples. Hundreds of thousands of former soldiers distanced themselves from the violence of war, gathering in pacifist veterans’ associations such as the Reichsbanner Schwarz-Rot-Gold, or supported their pacifist aims. Ziemann also sheds light on two anti-militarist novels which were widely read in the early Weimar Republic but are little known today. Ziemann’s decision to focus on the pacifist delegitimization of violence after 1918 can be criticized as being too one-sided. Thus he does not give enough weight to the mystically charged glorification and heroic stylization of the war experience within the nationalist-right spectrum, which largely influenced the attitude of many Germans, especially the younger generations, towards violence in the inter-war period.

The ‘German way of fighting’ was shaped by ideologies and military doctrines, the experiences of Germans in armed conflicts, and how these military conflicts were collectively commemorated after the violence ended. The studies reviewed in this article reflect all of these aspects that have contributed to German military culture. They examine the origins of, and changes within, military culture and seek to trace similarities, continuities, and ruptures within German military history. It is not possible, however, to verify or falsify the Sonderweg thesis in the field of military history by focusing on German history alone. There is undoubtedly a great need for further transnational comparative studies in order to highlight transnational similarities and national idiosyncrasies, and thereby assess peculiarities of German military culture and practices of violence.

28 Wilhelm Appens, Charleville: Dunkle Punkte aus dem Etappenleben and Heinrich Wandt, Etappe Gent, which were published shortly after the end of the First World War.
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CLASSICS REREAD

THE GUERRILLA HISTORIAN: ERIC J. HOBSBAWM AND THE LONG NINETEENTH CENTURY

ANETTE SCHLIMM


For a long time the long nineteenth century was not a fashionable period in German-language historiography. Just a few years ago David Blackbourn published an essay with the provocative title ‘Honey, I Shrunk German History’, in which he expressed his dismay that modern German history seems to consist solely of the twentieth century.¹ Since then, however, much has happened. Even apart from global history, which for a while seemed to have an exclusive claim on the nineteenth century, more historians are turning to the latter in order to develop innovative approaches and find inspiration for methodological discussions.² Thus it is timely that the publisher WBG Theiss has re-issued a German translation of an authoritative overview of the history of the ‘long’ nineteenth century, namely, Eric J. Hobsbawm’s monumental three-volume work, first published in English between 1962 and 1987. This is the first time Hobsbawm’s

Trans. Angela Davies (GHIL).

three volumes have been published in German as a complete work, bound and in a slipcase. Hobsbawm’s world historical overview of the nineteenth century — like his extensive oeuvre in general — is rightly regarded as a milestone of twentieth-century historiography, although, or perhaps because, Hobsbawm’s work was methodologically and politically contentious and controversial. Yet even historians who by no means see themselves as political or methodological Marxists, praise the character, wit, and wisdom of Hobsbawm’s works. In the many newspaper articles that were published on his death in 2012, his syntheses of the nineteenth century, and of the nineteenth and twentieth centuries, were universally praised, and described as a ‘great tetralogy’, a ‘history of the modern world’, an ‘economic history of the rise

3 The publisher, however, has missed an opportunity to produce a proper new edition. This one does not contain new translations, but simply reprints the old ones, including all the original errors. Richard J. Evans’s Foreword in the first volume could also have done with some proof-reading. Moreover, this edition is rather utilitarian. It lacks illustrations (which can be found in other editions), and the production, on the whole, looks cheap. The fact that pages were bound in the wrong order in the first instalment of volume one, and that the publisher simply ignored this, despite all queries, contributes to the impression that this edition has been produced just to generate sales. This work deserves better.


of industrial capitalism’, and a ‘history of the capitalist world from 1789 to 1991’.

The sheer size of the work is impressive, comprising, as it does, 1,300 closely printed pages in the German version. In addition, the narrative moves lightly from one topic and space to another, from Japan to Latin America, and from the history of opera to the bandits and social rebels who are stock characters in Hobsbawm’s work. In many of the tributes paid to him, Hobsbawm’s international, world historical approach was related to his biography. Born in 1917 in Alexandria to an Austrian mother and a British father, himself the son of a Polish Jewish immigrant, Hobsbawm grew up between the wars in Vienna and Berlin. When the Nazis came to power, he went to London—and stayed there. During his academic career Hobsbawm cultivated contacts with foreign colleagues and spent long periods of time in North and South America in particular. Most of his life, however, was spent in Britain and he became one of the most important twentieth-century British historians, while also being part of the networked and cosmopolitan world of the twentieth century.

As a Communist and Marxist historian, Hobsbawm was—in neutral terms—an exception, politically and intellectually; he and his work have always been controversial. He was loyal to the British Communist Party for a very long time, and he was strongly criticized for his ‘defense of Stalinism’ after the downfall of the Soviet Union.

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10 On this see also his autobiography, Eric J. Hobsbawm, Interesting Times: A Twentieth-Century Life (London, 2002).
He was also methodologically committed to Marxism, for example, as a co-founder of the Communist Party Historians’ Group and of *Past and Present*, an important leftist journal advocating innovation. He was a force driving research on the history of work and the working classes, and on other non-bourgeois groups in modern society. He shaped not only British but also German historiography, going far beyond Marxism. Even if the three big syntheses on the nineteenth century plus the fourth volume, published later, on the ‘short twentieth century’ (*Age of Extremes*, 1994) are not Hobsbawm’s only influential works, this article takes the recent re-issuing of the trilogy as providing an opportunity to re-read them. It will closely examine the creative element in producing the syntheses, discuss methodological problems, and clarify why the work should be considered less as introductory reading than as a classic that has itself become historicized as an interpretation of the nineteenth century from the viewpoint of the ‘age of extremes’.

*A Guerrilla Historian of the ‘Long Nineteenth Century’*

Hobsbawm’s impact was less in conceptual work than in identifying themes and specific individual theses. He once characterized himself as a ‘guerrilla historian . . . who does not so much march on his target behind the artillery fire of the archives, as shoot at it from the side, out of the bushes, with the Kalashnikov of ideas’. What he thought was important was ‘to bring new perspectives into old discussions, and perhaps to open new areas, by taking new approaches’.12 This method can be discerned in the trilogy under discussion here, which is not a synthesis in the sense of capping off the debate, but rather a synthesizing attempt that is intended to raise questions rather than to answer them once and for all.

The strength of the books lies more in identifying and presenting individual thematic emphases than in drawing up a methodological roadmap. The development of theories was not Hobsbawm’s

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strength, and it is in vain that we seek theory-rich concepts in his work. This also applies to the concept of the ‘long nineteenth century’, but it does not in any way detract from the great influence it exerted. And just because Hobsbawm did not explicitly define and problematize the term does not mean that he did not work with a specific historicity. Periodization—historians should always remember—is a powerful interpretative tool. It organizes history and underlines the significance of certain events, while other developments are subordinated to them as being less important.

The idea of a ‘long nineteenth century’ extending from the French Revolution to the First World War has become highly influential, appearing in the titles of professorships, examination subjects, and university courses. Hobsbawm was surely not its only inventor, but he was instrumental in popularizing this temporal division, which in the German-language area was mainly disseminated by Hans-Ulrich Wehler’s publications. Yet Hobsbawm’s three volumes about the nineteenth century were not designed as a trilogy from the start. Hobsbawm himself once pointed out that it was more of a coincidence than a systematic work in three parts. It was only while working on the second volume, *Age of Capital*, that it became clear to

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13 Eric J. Hobsbawm, ‘Peasants and Politics’, *Journal of Peasant Studies*, 1 (1973), 3–22, at 3: ‘It may well be a very complex matter for a zoologist to define a horse, but this does not normally mean that there is any real difficulty about recognizing one. I shall therefore assume that most of us most of the time know what the words “peasants” and “politics” refer to.’

14 Charles S. Maier, ‘Consigning the Twentieth Century to History: Alternative Narratives for the Modern Era’, *American Historical Review*, 105/3 (2000), 807–31, at 809. Maier also rejects the idea that centuries can represent meaningful periodizations, and criticizes the enthusiasm, widespread among historians, for creating ‘short’ and ‘long’ centuries in order to force historiographical contexts into the Procrustean bed of secular structures. In doing so, he refers explicitly to Hobsbawm. Ibid. 813. See also the fundamental work by Reinhart Koselleck, ‘Geschichte, Geschichten und formale Zeitstrukturen’, in id., *Vergangene Zukunft: Zur Semantik geschichtlicher Zeiten* (Frankfurt/Main, 1979), 130–43, at 131–2.

15 On this see Jürgen Kocka’s historiographical derivation in *Das lange 19. Jahrhundert* (Stuttgart, 2001), 34.

him ‘that I had let myself in for a great analytical synthesis of the history of the nineteenth century’. Not until the third volume did he explicitly discuss what held the entire nineteenth century together, between the Anglo-French, industrial-political dual revolution at one end, and the outbreak of the First World War at the other. And only in *Age of Extremes* did he succinctly sum up the nineteenth century in order to draw a contrast with the twentieth century:

This [Western] civilization [of the nineteenth century] was capitalist in its economy; liberal in its legal and constitutional structure; bourgeois in the image of its characteristic hegemonic class; glorying in the advance of science, knowledge and education, material and moral progress; and profoundly convinced of the centrality of Europe, birthplace of the revolutions of the sciences, arts, politics and industry, whose economy had penetrated, and whose soldiers had conquered and subjugated most of the world; whose populations had grown until (including the vast and growing outflow of European emigrants and their descendants) they had risen to form a third of the human race; and whose major states constituted the system of world politics (AoEX, 6).

Yet was it really the aggregation of capitalism and liberalism, a bourgeois age, and progress in knowledge and culture that turned this period of 125 years into a single era? Hobsbawm’s narrative is more sophisticated than this because the unifying bond is to be found at the meta level: the years between the ‘age of revolution’ and the beginning of the First World War were held together by the histori-

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19 Although the period being treated is given in the title as 1789 to 1848, Hobsbawm’s ‘age of revolution’ starts not in 1789, but with the beginning of the Industrial Revolution, which he places in the 1780s.
The revolutionary changes of the nineteenth century are captured in several cross-cutting chapters which present a panorama of the world (or Europe) at particular points in time, so that differences are clearly visible. The world in the 1780s, which Hobsbawm’s trilogy begins by describing, ‘was at once much smaller and much larger than ours’ (AoR, 7). It was predominantly rural and characterized by feudal power structures and absolute rulers. From an economic point of view, sluggish agriculture and active trade were more or less unconnected, and there were other power centres in the world apart from north-western Europe (AoR, 7–26). The world on the centenary of the revolution, by contrast, was quite different. It was divided into two parts: ‘a smaller part in which “progress” was indigenous and another much larger part in which it came as a foreign conqueror, assisted by minorities of local collaborators’ (Aoe, 31). The ‘progressive’ part of the world was characterized by industry and the idea of political modernity—and the notion that progress was ‘possible and desirable’—and that it was already happening (Aoe, 31).

Progress was not continuous or purposeful, but displayed a specific temporal structure that matches the narrative structure of the three volumes. The first volume marches to the drum beat of revolutionary change at the beginning of the century. The second volume stands for the establishment and stabilization of the capitalist order, a phase during which the characteristics of the century that had first been hinted at were developed in an ideal-typical form. Bourgeois society stabilized itself and its characteristic features emerged into view: the capitalist mode of production spread to all corners of the earth and sectors of industry; and social inequality became entrenched. The third volume, finally, deals with the growing paradox of the century. The further the nineteenth century progressed, the stronger did its contradictions become. According to Hobsbawm, the imperial age was both a golden age and a time of crisis beneath the surface. The next revolution, the big turning point of the age, was imminent.

Not only experts will recognize in this structure the grand narrative of Marxism, despite Hobsbawm’s heterodoxy. This historical dramaturgy is specific to Hobsbawm’s ‘long nineteenth century’ and distinguishes his account (and interpretation!) from so many others who have adopted the same term. This is not apparent at first glance.
because unlike other authors, Hobsbawm nowhere explicitly lays out the structure of his books. His trilogy is not preaced by theoretical instructions for use, as is, for example, Hans-Ulrich Wehler’s *Deutsche Gesellschaftsgeschichte*. Hobsbawm neither explained his theoretical assumptions, nor discussed his claims to offer an explanation. His roadmap for the three volumes remained unspoken. This may be because Hobsbawm saw the books as popular accounts, but it also reflects his restrained critical attitude towards larger theoretical discussions. Thus the supposedly indeterminate nature of the ‘long nineteenth century’ might be precisely what contributed to its success. The term is memorable and makes sense immediately, referring as it does to undisputed turning points in the history of events. Meanwhile, Hobsbawm’s underlying interpretations remain hidden until one is familiar with the whole work. His trilogy on the nineteenth century is therefore almost an anachronism, as the overall shape of the work cannot be appreciated by those who read only chapters and excerpts. The re-issuing of the trilogy in German is therefore a wonderful opportunity to (re)read the whole work and to engage with Hobsbawm’s interpretation.

*Capitalism and Social Struggles*

The great pacesetter of Hobsbawm’s nineteenth century, and its most important outcome, was capitalism. Its emergence plays a prominent part in his argument. For Hobsbawm, capitalism was the basis of a process of global transformation in the long nineteenth century. Yet he was not an orthodox Marxist in the sense of dismissing everything beyond the transformation of the mode of production as mere superstructure. His description of the emergence, stabilization, and maturity of bourgeois capitalism was socially comprehensive and not limited purely to the mode of production. In addition, he refrained from identifying unambiguous causalities in favour of lively portrayals. In his account, it remains unclear whether the economy caused, influenced, or accelerated the other changes. Apart from these subtleties,

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however, Hobsbawm attached particular importance to the economic side of history. But it was not only for chronological reasons that he began his account with a chapter on the Industrial Revolution in England. Only by analysing this, he wrote, could we ‘understand the impersonal groundswell of history on which the more obvious men and events of our period were borne; the uneven complexity of its rhythm’ (AoR, 28).

Hobsbawm did not present economic transformations purely at the level of quantification, even though the relevant chapters do not lack figures and statistics. In the chapter on the boom of the 1850s (AoC, 29–47), for example, he repeatedly emphasizes enormous growth rates: British cotton exports doubled; the total capital of the Prussian joint-stock companies jumped from 45 to 114.5 million Taler (without counting the railway companies); the steam power used by German fixed engines grew from 40,000 to 900,000 horsepower. These figures are impressive, and they are intended to show how radically the economy changed within a short period of time. But Hobsbawm’s account of the capitalist economy is not limited to quantitative growth. It also shows the enormous diversity of this development, which means that he switches back and forth between different standards of investigation and between different themes. Sometimes he examines the history of financial problems, the state administration, and the influence of politics and world fairs, but he repeatedly looks at the worlds of experience of the historical actors, at their hopes and fears. The economic development that forms the backbone of his great narrative therefore represented not just a huge structural change, but one that included all areas of social and human activity. Thus the period of the great boom was not only one in which ‘the world became capitalist and a significant minority of “developed” countries became industrial economies’ (AoC, 27), but also the one in which contemporaries developed new ideas: ‘the model of economic growth, political development, intellectual progress and cultural achievement’ (AoC, 47).

It is hardly possible to distinguish between structural factors and critical events in Hobsbawm’s account, because they were so closely intertwined. He linked the levels by giving the views of contemporaries a great deal of space in his account (and argument). According to Hobsbawm, historical change took place neither only in the big cabinets nor exclusively behind the backs of contemporaries. He
declared the historical actors and their expectations, in all their plural- 
ity, to be an important factor of history:

Bourgeois expected an era of endless improvement, material, 
intellectual and moral, through liberal progress; proletarians, 
or those who saw themselves as speaking for them, expected it 
through revolution. But both expected it. And both expected it, 
not through some historic automatism, but through effort and 
struggle (AoE, 339).

Hobsbawm gave these struggles a corresponding amount of space 
in his account. On the one hand, for methodological reasons he saw 
these struggles as a condensation of otherwise possibly invisible 
developments;21 on the other, he could use them to demonstrate that 
historical change could not be understood teleologically and as 
something uninterrupted, but only as eruptive, revolutionary, and 
unsettling. His ‘long nineteenth century’ began with a dual revolu-
tion,22 which resulted in further unrest, revolts, and revolutions until 
1848. For Hobsbawm, however, the significance of the 1848 revolu-
tion lay not only in the event itself, and certainly not in its successes, 
for 1848 was the classic failed revolution. What was crucial was a 
mediated effect: ‘In a sense it was the paradigm of the kind of “world 
revolution” of which rebels were henceforth to dream, and which at 
rare moments, such as in the aftermath of great wars, they thought 
they could recognize’ (AoC, 10) The revolution of 1848 influenced the 
years that followed through the hope and fear of its return (AoC, 10).

21 ‘Moreover, certain important problems cannot be studied at all except in 
and through such moments of eruption, which do not merely bring into the 
open so much that is normally latent, but also concentrate and magnify phe-
nomena for the benefit of the student, while—not the least of their advan-
tages—normally multiplying our documentation about them.’ Eric J. Hobs-
bawm, ‘From Social History to the History of Society’, Daedalus, 100 (1971), 
20–45, at 39.

22 Hans-Ulrich Wehler adopted this idea, but applied it to German history. 
In order to be able to speak of a dual revolution in this case, the mid nine-
teenth-century industrial ‘take-off’, which is still the ‘great boom’ in Hobs-
bawm (AoC, 29–47), has to be linked with the revolution ‘from above’, that 
is, the foundation of Imperial Germany. See Wehler, Deutsche Gesellschafts-
geschichte, vol. iii: Von der ‘Deutschen Doppelrevolution’ bis zum Beginn des Ersten 
Economic change on the one hand; permanent social struggle on the other. But this does not complete the panorama of the transformative period in Hobsbawm’s Europe. Chapters on the history of ideas alternate with detailed descriptions of the ‘agrarian problem’, and scholarship and the arts had a firm place. The emergence of capitalism as a unifying thread and the variety of topics did not contradict each other, but together formed the ‘compass’ of his work. The thematic and geographical variety displayed by this trilogy is exemplary and striking. It testifies not only to the author’s broad interests and amazing erudition, but also to the outstanding complexity of his historical narrative.

Women in History and other Gaps

However interesting this interpretation of the nineteenth century is, it differs greatly from more recent approaches to the historiography of (not only) the nineteenth century. It would be futile to discuss all the points on which historians today see things differently from Hobsbawm. Only a few methodological aspects will be addressed here by way of example, namely, his blind spot for gender history, his conception of world rather than global history, and his concept of capitalism.

I am not the first historian to notice that while Hobsbawm included in his account many subjects that at the time were untypical—for example, bandits—he showed a remarkable lack of interest in women in history. In the reprint of his well-known essay ‘From Social History to the History of Society’ in 1997, he noted ‘with embarrassed astonishment that it contained no reference at all to women’s history’. His description of the French Revolution in the first volume of his trilogy makes practically no references to the role of women in the revolution, if we discount the derogatory remarks. The third volume, however,

25 Marie-Antoinette remains nameless, but Hobsbawm describes her as a ‘chicken-brained and irresponsible woman’ (AoR, 61), and he points out that while the women among the Girondists were well known, they were ‘politi-
devotes a chapter to the issue of gender. But here Hobsbawm explains that ultimately, the vast majority of women had no part in the history of the nineteenth century. He points out that in the Western world only the middle- and upper-class women had been integrated into the dynamics of the nineteenth century, while ‘[i]n the condition of the great majority of the world’s women . . . there was as yet no change whatever’ (AoE, 193). Although he provides references to the masculinization of the economy and politics (AoE, 200), gender relations do not provide a cross-cutting category for Hobsbawm’s history of the nineteenth century. Admittedly, gender history only developed as a part of social history in the period during which Hobsbawm was writing, but this shows why the work feels as though it comes from a different age, and sometimes seems rather old-fashioned.

Something similar applies to Hobsbawm’s conception of world history. Even if Jürgen Kocka writes, in an appreciation of Hobsbawm’s oeuvre, that he was ‘uniquely’ ahead of the trend for global history as he always argued in a global historical way, Hobsbawm’s approach to writing a world-spanning history of the nineteenth century is outdated in the twenty-first century. In the nineteenth century Hobsbawm’s world had a clear-cut centre, in (north-western) Europe. Even if he switches effortlessly between the various continents in his account, for Hobsbawm the driving forces in the nineteenth century emanated from Europe and radiated out into the world. This Eurocentrism was not a mistake, or unconscious, but central to an approach that saw the English-French dual revolution as a

cally negligible but romantic’ (AoR, 68). In the third volume, in the chapter on the ‘new woman’, however, there are references to the important role that women played in the French Revolution (AoE, 200).

historical force for the whole world.\textsuperscript{28} Actors in the non-European regions of the world come off badly in Hobsbawm’s books. Moreover, a world orientation towards a European–American centre is not an outcome of the nineteenth century, but its starting point (AoR, 1–2). This clearly distinguishes Hobsbawm’s approach to world history from the global history narratives that have had such great success in recent years.\textsuperscript{29}

Hobsbawm’s interpretation of the nineteenth century places modern capitalism at the centre of all considerations. Surprisingly, however, his work has found little resonance in the present renaissance of research on capitalism. We find occasional references to some of Hobsbawm’s chapters,\textsuperscript{30} but the debate about an analytical concept of capitalism is conducted with hardly any recourse to his work. This is clearly not because Hobsbawm did not explicitly problematize his concept of capitalism; others do not do this either, but are nevertheless recognized and discussed at great length in the research on capitalism.\textsuperscript{31} This indeterminacy alone is therefore probably not the reason why Hobsbawm’s trilogy on the long nineteenth century has been practically ignored by research on capitalism. The problem is more likely to be that Hobsbawm largely equated ‘capitalism’ with ‘industrialism’, which makes the concept of capitalism unnecessarily undifferentiated. Precisely this equation is being discussed and rejected in more recent approaches to research on capitalism.\textsuperscript{32}

These points alone show that Hobsbawm’s account of the nineteenth century is not only old, but in some respects outdated, and cannot contribute much to the current research discussions. This is not just to do with fashions and conventions, but also with the fact that in recent years and decades perspectives have been developed.

\textsuperscript{31} On this criticism see e.g. Peer Vries, ‘Cotton, Capitalism, and Coercion: Some Comments on Sven Beckert’s Empire of Cotton’, \textit{Journal of World History}, 28 (2017), 131–40.
that have contributed significantly to the complexity of our view of the nineteenth century. We cannot fall behind these. Hobsbawm’s work therefore no longer seems to me to be suitable as introductory reading for the history of the nineteenth century.33

_Literature rather than Introduction_

As literature, however, I thoroughly recommend Hobsbawm’s history of the ‘long nineteenth century’. In a lecture Hobsbawm himself once said: ‘Obsolescence is the inevitable fate of historians; the only ones who survive it—a very rare occurrence—are those who were also significant writers: a Gibbon, a Macaulay, a Michelet. But today we have no control over who becomes a member of this tiny group. Only the future can decide that.’34 I would include Hobsbawm in this group, not only because his books are beautifully written (a judgement tempered somewhat by the rather pedestrian German edition) and skilfully draw upon varied topics, regions, and analytical levels, not only because breaking up the large narrative through the inclusion of smaller stories, biographies, and details makes possible an exciting journey through the world of the nineteenth century (with all the limitations described). The position of the writer in these volumes is a particularly interesting aspect for the reader. Hobsbawm as the author wrote himself into all of them, so that not only is his incomparable hand recognizable everywhere, but also his specific perspective, his viewpoint (what Chladenius called a _Sehepunkt_). From the perspective of the early twenty-first century in particular, the volumes under review here can be read and experienced as products of the twentieth century, as the observations and interpretations of someone who stood firmly, with both feet, in the ‘age of extremes’.

Hobsbawm began to reflect upon this himself, and in _The Age of Empire_ he made it explicit. He begins the volume by describing how

33 In his review of Hobsbawm’s biography Niall Ferguson called the three volumes the ‘best starting point I know for anyone who wishes to begin studying modern history’. Niall Ferguson, ‘What a Swell Party it was . . . for him: Niall Ferguson Reviews Interesting Times by Eric Hobsbawm’, _The Telegraph_, 22 Sept. 2002, online at <https://www.telegraph.co.uk/culture/4728809/What-a-swell-party-it-was-.--.for-him.html>, accessed 27 Apr. 2019.
34 Hobsbawm, ‘Geschichtswissenschaft’, 78.
his parents met in Alexandria before the First World War, illustrating how strongly the imperial age was intertwined with his own biography. According to Hobsbawm, the imperial age was a ‘twilight zone between history and memory; between the past as a generalized record which is open to relatively dispassionate inspection and the past as a remembered part of, or background to, one’s own life’ (AoE, 3). For Hobsbawm’s own present, this imperial age was still tangible: ‘The world we live in is still very largely a world made by men and women who grew up in the period with which this volume deals, or in its immediate shadow’ (AoE, 3). We could say that Hobsbawm conceived the imperial age as contemporary history in the sense of Hans Rothfels’ ‘age of living witnesses’ (‘Epoke de Mitlebenden’), or at least as a period in which the ‘scholarly and the existential, archive and personal memory’ overlapped, rubbed up against each other, supplemented, or contradicted each other (AoE, 3–5).

Even if he explicitly made this twilight zone only one feature of the ‘imperial age’, approaches to it can also be seen in the other volumes. For Hobsbawm, his own present began in the nineteenth century, when it was formed. And where this history at first sight seemed alien, it could be made comprehensible by finding parallels with times one had lived through, for example, by frequent comparisons with the history of the Spanish Civil War. But this familiarity between the nineteenth and twentieth centuries which Hobsbawm evokes has the opposite effect on today’s readers. Not only the ‘long nineteenth century’, but also twentieth century that is so closely intertwined with it, seem far away. The distance between our present on the one hand, and both narrated and narrative time constantly grows. That is the special charm of reading these books.

Although Hobsbawm’s trilogy is not an introduction to nineteenth-century history, it is a true classic, even if, in its own way, it has become a source in its own right.

35 See e.g. the parallels he draws between Spain in the 1930s and France in 1794: ‘The process which, during the Spanish Civil War of 1936–9, strengthened Communists at the expense of Anarchists, strengthened Jacobins of Saint-Just’s stamp at the expense of Sansculottes of Hébert’s’ (AoR, 71).
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BOOK REVIEWS


These conference proceedings are based on papers held at an international conference from 5 to 8 July 2014 at the Fries Museum, Leeuwarden, Netherlands. The conference, ‘Across the North Sea: North Sea Connections from AD 400 into the Viking Age. Second Interdisciplinary Symposium on Runes and Related Topics in Frisia’, was organized by John Hines, Nelleke IJssenagger, Tim Pestell, Tineke Looijenga, Gaby Waxenberger, Kerstin Kazazzi, and Han Nijdam. To mark the twentieth anniversary of the ‘First International Symposium on Frisian Runes and Neighbouring Traditions’ that took place in 1994, also at the Fries Museum, the organizers opened up the conference to a broader approach, providing a newly interdisciplinary perspective.

As a result, the thirteen essays included in the book come from a variety of research fields, but all of them touch upon aspects of early medieval Frisian identities and cultures and the maritime networks involving Frisian people at this period. Particular weight is given to the interactions of the Frisians with their immediate neighbours. The editors, John Hines and Nelleke IJssenagger, introduce the book with a general overview of the topic and of Frisian history between the fifth century and the Viking era, explaining the book’s theoretical approach to concepts of ethnic groups and the historical development of the Frisians in the Early Middle Ages, before examining how Frisians viewed and described ‘outsiders’ along with the interactions that took place across their geographical settlement area.

Egge Knol and Nelleke IJssenagger discuss how Frisians adapted in different ways to the different landscapes they inhabited, as the geography of Frisia varied noticeably between the west and the north east. Adaptation strategies included highly specialized economic

Trans. Emily Richards (GHIL).
activities, but also specialized settlement types, the ‘terpens’ and ‘wharves’ or ‘dwelling mounds’. Noting the necessity of distinguishing ‘Friesland’ as a geographic region from ‘the Frisians’ as an ethnic category, the authors discuss Pliny the Elder’s early mention of certain inhabitants of the North Sea coast. The fact that Pliny never names these inhabitants, the authors argue, shows how difficult it is to differentiate between the Frisians and their immediate neighbours, the Chauks, a differentiation not always possible even from archaeological findings. What the material does demonstrate is a break that seems to have occurred in settlement patterns in the fourth century AD, although the eastern part of Frisia shows greater continuity while the break becomes more apparent towards the west. There is evidence of resettlement from the fifth century on, although the authors state merely that these new settlers came ‘from various places along the North Sea coast’. A more specific description would have been useful here.

In the course of the early medieval period, smaller regions, from this time on identifiable by name, began to emerge with their own leaders and leading families. At the same time, the transition from the Merovingian to the Carolingian period sees a notable increase in archaeological evidence. Finally, the authors turn to the Viking era, which in the Netherlands even now is still not recognized as a historical period in its own right. Our primary source of information from this time are Latin documents, but there are not many Viking finds available, which in part is due to a lack of a national centre for recording archaeological finds in the Netherlands.

Similarities between Frisian and English cultures are under-researched, as John Hines notes in his contribution on the ‘Anglo-Frisian Question’. Yet, as Hines goes on to show on the basis of evidence from language, runology, archaeology, and history, such a cultural affinity becomes obvious if we look at the early medieval period. As early as the seventeenth century, the philologist Franciscus Junius the Younger developed the theory of Anglo-Frisian as an independent branch of the Germanic language family, based on the similarity between Old/Middle English and Old Frisian. The theory was taken up by Vlitius in the seventeenth century and Halbertsma in the nineteenth, and in 1981 Hans Frede Nielsen demonstrated its probability through comparing the similarity between Old English and Old Frisian with the related, but more distant Scandinavian and
Continental Germanic languages. These observations are confirmed by, for instance, similar developments of Old English *fuþark* and Old Frisian *fuþorc*. In the field of runology, these are sometimes considered to be connected, even though there are also contrary opinions and alternative models. Hines presents further examples from the field of archaeology, based on the exploration of *sceattas*, and from the field of history, based on a broader analysis of sources in English–Frisian historiography and literature.

Possible linguistic contact between Frisians, Celts, and Romans is discussed by Peter Schrijver in a consideration of two different models of language distribution and the early history of the Frisian languages in the region between the Rhine and the Ems from Roman times until the Early Middle Ages. Looking at the possibility of linguistic contact in this region and Celtic influences on Frisian, he compares the Celtic vowel system—represented by British Celtic and Northern Gallic—with early medieval Frisian. He argues that the land between the Rhine and the Ems was inhabited in Roman times by a Celtic-speaking people that eventually began to speak a Germanic language. As a result, early medieval Frisian was characterized by the introduction of a Celtic accent. Schrijver’s contribution ends with a hypothesis, so that it would have been helpful to know more about the discussion that took place at the conference on this subject.

Menno Dijkstra and Jan de Koning present a general overview of their research in connection with the Frisia Project on the settlement history of West Frisia and the Fryslân province, an area that they term ‘Middle Frisia’. They note that there is no early medieval settlement legend from which we could deduce more about the conditions of the time, and no coherent historical record of the make-up of the population along what is now the Dutch coast. However, archaeological evidence shows that the population had diminished between the third and fifth centuries possibly to between 10 per cent and almost 0 per cent of what it had been during the Roman period, and did not rise again until the second half of the fifth century AD, perhaps in part as a result of increased immigration from neighbouring regions. Archaeological finds also reveal cultural exchange with Franks, Anglo-Saxons, and Old Saxons as well as clear evidence of intra-Frisian communication, for example, relating to burials and the emergence of elite networks or miniature kingdoms. The two authors
conclude that in contrast to Frisia and Groningen, for example, the western coastal region was not occupied by Anglo-Saxon groups but was more significantly influenced by the Frankish hinterland and the Rhine–Maas region.

Johan Nicolay uses examples of gold and silver jewellery to sketch five phases in the development of elite societies and networks on the southern North Sea coast. From 390 to 500, Saxon, Anglian (from Anglia, that is, the peninsula in what is now Schleswig-Holstein, Germany), and perhaps Scandinavian people passing through this landscape on their way to Britain influenced the style of this period in the Frisian territories. This initial phase was followed from 475 to 550 by a period in which Roman gold, which had probably come to Northern Europe in the form of tribute, was made into regional jewellery and adornments that expressed a new confidence on the part of local powers. From the late fifth century and throughout the sixth, however, local kingdoms on the Elbe, the Dutch coast, and even as far north as Kent and Norfolk used ornaments from southern Scandinavia to represent their status. Regional elites in particular, who developed their own, localized adornment styles, are typical for the period from around 590 to 630/40, although their local character also reveals a clear affinity with the Scandinavian and Frankish world. The final phase (600–700) was dominated by Byzantine-inspired jewellery that became the new form of status symbol among the North Sea elites.

Referring to current discussions on a possible ‘Anglo-Frisian unity’, Gaby Waxenberger begins by examining difficulties in comparing the rune corpus. While Pre-old English and Pre-old Frisian were contemporaneous (both prevalent from around 410 to 610/50), Old English (650–1100) and Old Frisian (1200–1500) are chronologically distant from each other. The author goes on to show the challenges presented by the Frisian rune corpus and describes, using a variety of rune inscriptions and the internal development of fuþorc (so called to distinguish it from the otherwise pan-Germanic fuþark), in both languages and their similarities.

Arjen Versloot and Elżbieta Adamczyk examine the spread of linguistic innovation by looking at the main geographical factors that contributed to changes in Old Saxon as a result of North Sea Germanic linguistic influences. In what is now Belgium and the Netherlands, North Sea Germanic (Ingwaonic) spread inland only to a distance of
50 km, but because of a geographical bulge this meant that it penetrated to Eastphalian Old Saxony. Significant factors affecting its distribution were often geographical; rivers, in particular, served as axes for transport and communication just as natural barriers tended to prevent the latter. On the basis of linguistic features found in written sources and place names, the authors have cautiously mapped the areas in which North Sea Germanic may have influenced Old Saxon. An appendix discusses the linguistic characteristics typical of North Sea Germanic.

Iris Aufderhaar shows that archaeology supports the theory of a complex system of central locations and shipping hubs, and of hinterland connections between the Elbe and the Weser, from as early as the first century to as late as around AD 500. Rivers, of course, were a means of communication and transport from late antiquity and the migration period onwards, and the Elbe–Weser triangle formed an important connection between Scandinavia, the Rhine region, and south and east England. Sievern and Gudendorf, in particular, due to their geographical position near the coast, were at the centre of infrastructure networks between the land and the sea.

With a focus on the Flandrian coast, Pieterjan Deckers examines two categories of material culture: the main pottery type and house-building. Based on archaeological findings in these two areas, he develops the theories of John Hines and Malcom Ross in arguing that innovations in pottery and house-building tend to take place at the same time as linguistic developments. Deckers identifies a ‘correspondence with developments in material cultures’ in a period of Ingwaonic convergence (koinéization) which began in the fifth/sixth century and ended in the eighth. However, convergence speeds in these three different forms of cultural expression (language, house-building, pottery) are different; language and house-building assimilated only imperfectly until the eighth century, while pottery shows a generally similar convergence ‘already in the course of the 6th century’ (p. 183). From the eighth century, various forms of houses are replaced by more homogenous traditions, while organic-tempered pottery has been completely replaced by sand-tempered pottery by the mid eighth century. Unfortunately, it is difficult for a non-specialist reader to follow the argument of this otherwise very interesting contribution, as many of the specialist terms used are not explained.
Tim Pestell has chosen East Anglia (the region bearing this name today in the east of England) as the focus of his overview of connections between Britain and inhabitants of the southern North Sea coast in the early medieval period. He begins by noting that early medieval scribes such as Procopius and Bede record political exchanges and other forms of contact between inhabitants on both sides of the North Sea. Citing coin hoards, brooches, belts, belt buckles, and pottery, he shows that such historical reports of intercultural exchange and trade are reflected in archaeological findings between the seventh and ninth century. He is particularly interested in Frisian trade patterns that brought Continental goods to England, especially as these can also be geographically determined on the basis of where Anglo-Frisian sceattas coins have been found.

The next contribution discusses the nature of wergeld payments in relation to death and injury, and how such payments were based on a concept of honour. Comparing the Kentish law of King Ethelbert with the Lex Frisionum and later developments of its tradition in Old Frisian legislation of the thirteenth to the fifteenth centuries, Han Nijdam shows how written law aimed to prevent blood feuds through substituting a system of financial compensation. Following an overview of how such legislation was structured and the history of research in this field, the author analyses wergeld compensation rates in terms of their technical legal names, the assigning of different values to different parts of the body, and the mechanisms for determining value set out in the texts. In summary, he argues that Kentish and Frisian legislation developed separately but that both may have a basis in codes and practices found across wide areas of the Germanic world. However, he does not fully reject the possibility of an Anglo-Frisian or Ingwaonic sphere of influence. Finally, he lobbies for more in-depth research on wergeld rates in their universal and individual significance as a part of Germanic culture.

The question of communication networks along the nordic arc—that is, the trading region between the North Sea and Russia—is the focus of the essay by Christiane Zimmermann and Hauke Jöns. As historical records have nothing to say on this point, we can generally only speculate on how trade in this region took place. For the eighth and ninth centuries we can assume that the different Germanic dialects in northern Europe were still similar enough for speakers to understand each other without the need for a lingua franca or inter-
preters. This implies that the region was a milieu characterized by different languages that nevertheless were mutually understood, whether in written or in spoken form, along with two contemporaneous rune corpora (fuþark and fuþorc) that were used by a variety of speakers in this period. Considering whether Reric/Groß Strömken-dorf formed a hub for trade between the North Sea and the Baltic regions, the authors use archaeological findings to demonstrate the cultural exchange that took place here and its unique role among trading places on the Baltic coast. Against this backdrop, the authors focus more closely on a comb inscribed with runes from around 770 from the town’s artisan quarter, analysing its inscription and attempting to translate it. They find that the runes cannot be assigned to any particular corpus of the time, but are closest to what are known as ‘Scandinavian inscriptions on the Continent’. This means that we now have archaeological evidence of an artefact made in the North Sea region, inscribed with runes from the early Scandinavian Viking period, and finally making its way to the Baltic region.

As these conference proceedings impressively show, international and interdisciplinary approaches can lead to valuable findings in Frisian research. We possess relatively few written records for early medieval Frisians, and what we do have is written entirely from an external (and usually one-sided) point of view. This makes an interdisciplinary approach essential if we are to research further aspects of Frisian history and culture. All too often, such research tends to be limited by today’s geographical and linguistic borders, as Oebele Vries already noted some years ago in in the Handbook of Frisian Studies / Handbuch des Friesischen (2013). This is also recognized by John Hines in the current book. International and interdisciplinary conferences, such as the conference that formed the basis for this book, help to fill this gap and allow us to look more closely into Frisian lives and the Frisian environment beyond the written sources. Researchers in this area would do well to extend such an approach to the end of the Early Middle Ages, as research on important Frisian developments—such as the settlement of North Frisia and the expansion of Frisian trade—is at present patchy and unsatisfactory. However, this collection of essays on the Frisians and their neighbours in the North Sea region now offers an excellent foundation for research on subsequent periods from a variety of perspectives and disciplines.
JENS BOYE VOLQUARTZ is writing his dissertation on late medieval castles in North Frisia and Dithmarschen (title: ‘Im Spannungsfeld zwischen herrschaftlichem Zugriff und bäuerlicher Selbstbestimmung: Spätmittelalterliche Burgen in Dithmarschen und Nordfriesland’) at the Department of Regional History with a Focus on the History of Schleswig-Holstein, Kiel University. In addition to this, one of his fields of research is Frisian Studies in the Early Middle Ages, and his publications on this topic include *Friesische Händler am Oberrhein im Frühmittelalter* (2017) and *Der fränkisch-friesische Konflikt (690–734) aus Sicht mittelalterlicher Quellen* (2018).

It is clear that this work is the product of many years of intensive research. The author, a recognized expert on the history of early printing in Germany, and currently Curator of Rare Books at Princeton University Library, began to compile it in 2011 during his tenure as Curator of Special Collections at the Bridwell Library in Dallas, where he was inspired by the latter’s collection of Gutenberg Bible fragments. The book ambitiously sets out to reconstruct the ownership history of all the extant copies and fragments of the first printed Bible, insofar as this is at all possible. Essentially, the book represents a kind of census of all complete and partial copies of the Gutenberg Bible—including those which we only know of thanks to references in earlier literature—and updates our knowledge of them, something that is only achievable through very extensive research in the history of the book and the history of collections.

The book’s central idea is to establish when and how any given copy came to the notice of researchers, that is, when it was ‘discovered’ by historians or other academic disciplines, beginning with the very first description of it or publication about it, whether or not this was generally known to have happened. Marshall White lists twenty-four copies of the Gutenberg Bible known to have existed in the eighteenth century (pp. 179–80), plus a further three of which the present locations are unknown; nobody at the time was aware of the existence of all twenty-seven. Jeremias Jacob Oberlin, in 1801, listed only six known copies (p. 184). However, in describing the individual copies, the author is not merely interested in their discovery; he also provides the entire history of their ownership and their eventual fate, in every conceivable detail. Careful attention is devoted to concepts familiar to students of the history of the book, such as settings, completeness, illumination, and other features specific to the individual copies. The section titles always include the book’s current location and the year in which the book became known. This helps with identification, but does not tell us anything about when and in what context each discovery took place. Neither can this part of the book offer a conclusive narrative, as numerous similar and overlap-
ping situations arose in the previous and later history of each copy (see p. 84). Thankfully, however, the author provides a regular summary of what was known at the time about all the copies and their travels and locations, and Part III also helps to clarify various histories for the reader. In addition, each copy of the Bible is identified by its listing in the four most important censuses (De Ricci, Schwenke, Hubay, Needham).

Chapter 1, ‘Editio princeps’, summarizes the current state of knowledge about the beginnings of print and the Gutenberg Bible, providing readers with the essential foundation they need to understand the rest of the book. The author first provides a very readable history of printing in South-East Asia, which shows how printing with movable types gradually replaced the more traditional block printing in the region in the late Middle Ages, and had become completely established in Korea around twenty years before Gutenberg (p. 20). The author then sets out the sources on early printing in Europe up until the beginning of the eighteenth century, which tell us more about Gutenberg and his Bible. Among other examples, Marshall White highlights a letter written by Enea Silvio Piccolomino in 1455, which discusses Bibles on offer in Frankfurt in 1454, and the ‘Helmasperger Instrument’ as by far the most detailed sources that we possess on Gutenberg’s and Fust’s collaboration on the ‘work of the books’. Our most important source for the rediscovery of the Gutenberg Bible is the Cronica von der hilliger stat van Coellen, printed in 1499, which not only names Gutenberg as the inventor of book-printing, but also states that the first book he printed was a Bible ‘in grosser Schrift’ (‘with a large letterform’ in White’s translation).

Although Gutenberg’s printing press was sporadically mentioned by contemporaries, by the end of the sixteenth century he had been more or less forgotten. One historical narrative named Johann Fust as printing’s inventor, and fatally, this version of the facts came to the attention of Erasmus. Other narratives naming Haarlem’s Laurens Janszoon Coster and Strasburg’s Johann Mentelin respectively fell out of favour comparatively quickly. But although Gutenberg played little part in seventeenth-century historical narrative, his Bible played even less, and by 1700 he could have been described as a ‘printer without a book’.

The chapter ‘The Work of the Books’ discusses the physical evidence for the extant printed works. Beginning with minor early print-
ed works which used more primitive DK typefaces (The Book of the Sibyls, the earliest known edition of the Ars minor of Aelius Donatus, and the Turkish Calendar) and early examples of the B42 typeface (the Donatus edition in the Scheide Library), along with indulgences that used these typefaces for their headings from 1454 to 1455, the author underlines the importance of the 42-line Gutenberg Bible as the first major printing project in Europe. At the same time, he notes the problematic nature of traditional nomenclature, as this copy, which has no indication of publisher, place, or date, is not the only 42-line Latin Bible, and the nature and extent of Fust’s contribution is uncertain. With great care and in great detail, White collates the newest research on the production process as evidenced by various settings, watermarks, and analysis of ink batches. As a result, we know that production was carried out using four sets of compositors, that the print run was increased by around a third during production, and that the sale and distribution of all the copies after 1455 took some time. The author also sets out Needham’s and Agüera y Arcas’s findings on typeface manufacture without reusable matrices. These findings relativize Gutenberg’s status as the inventor of a printing process that would hold good for centuries, and raise new questions. A reproduction of an image from a thirteenth-century Paris manuscript shows how the layout of the Gutenberg Bible was still largely based on manuscript traditions. The chapter ends with a description of the ‘ideal copy’ of the printed first edition.

In ‘A Book Without a History’, the author shows how the Gutenberg Bible, despite its initial commercial success and its immense influence on Bible philology, was gradually replaced in the course of the fifteenth century by more practical and cheaper versions. Up until the early eighteenth century, copies of the Gutenberg Bible were even used as binder’s waste. Occasional mentions of it as the first printed book do not include references to actual copies. The history of its rediscovery, however, does reveal some important milestones; Bernhard von Mallinckrodt assessed the sources in 1640, and Johann David Köhler’s work of 1741 represents an important first step in salvaging Gutenberg’s reputation. Identifying copies on the basis of the sources, however, continued to be a major problem, and none of the numerous eighteenth-century researchers cited by White succeeded in achieving this.

It is here that Part II takes up the story, with a description of each
known copy in the chronological order of discovery. This second part of the book is structured in three chapters, each of which covers a century. ‘The First Fruits of the Eighteenth Century’ is, appropriately, by far the longest. In 1997 Ilona Hubay showed that fourteen complete copies of the Gutenberg Bible were known in the eighteenth century; Marshall White manages to bring the total up to twenty-five, of which fifteen were first mentioned in printed publications, nine in library catalogues or correspondence, and two are only known through contemporary witnesses. The first description is that of the copy held in the Staatsbibliothek Berlin, which was described as the first printed book as early as 1700 in a note made in the copy by librarian Christoph Hendreich. Unfortunately, Hendreich never came to enjoy the reputation of being the man who discovered the first printed Bible; this accolade was undeservedly given to the librarian Guillaume-François De Bure thanks to his rather vague description of the copy held by the Bibliothèque Mazarine in 1762. As a result, the Gutenberg Bible was often referred to in the eighteenth century as the Mazarine Bible.

In 1745 a description of a Gutenberg Bible said to be held by the Aschaffenburg Hofbibliothek crops up in a previously overlooked letter written by one Samuel Engel, a librarian at Berne, in the course of a discussion on the invention of printing. The year 1765 sees the first appearance of a Gutenberg Bible in an auction catalogue belonging to Pierre-Ignace-Eloi Favier. Thanks to the rediscovery of a description contained in the catalogue, this copy has now been identified beyond all possible doubt. But despite the fact that since the second half of the eighteenth century no other copy of the Gutenberg Bible has been lost or destroyed, the whereabouts of this particular copy remains a mystery.

In the 1780s and especially the 1790s discoveries of ‘new’ Gutenberg Bibles increased. This was partly a result of the rise of a new kind of collector who, for the first time, viewed early prints as historically significant artefacts rather than simply the physical vehicles for texts. But it was also thanks to the Napoleonic Wars, whose effects on the wider population were beginning to make themselves felt, and to the first confiscations of church property. The Gutenberg Bible held by the John Rylands Library in Manchester, for example, was looted from the Augustinian house in Colmar by French soldiers in 1790, while that held by the Austrian National Library was original-
ly owned by the Maria-Steinach convent at Meran, which was dissolved in 1782.

Again and again, the records mention two Benedictines who acted as agents in the sale or disposal of books: Jean-Baptiste Maugerard of Erfurt and Alexander Horn of Regensburg. Their most important customers were British bibliophiles such as George John, 2nd Earl of Spencer, whose library was famous, and as early as the eighteenth century five Gutenberg Bibles made their way to England, while one was sold to a Scot, David Steuart. In France, one of the central actors in this drama was the Archbishop of Toulouse, Étienne Charles de Loménie de Brienne, the owner of three copies in the 1780s. Marshall White has found a rich source of evidence in the correspondence of English book agents; thanks to a letter from Alexander Horn to the Earl of Spencer dating from 1798, for example, he is able to trace the journey of the Rebdorf copy to the Morgan Library in New York.

But despite these many discoveries, most scholars in the eighteenth century were not willing to accept the 42-line Bible as Gutenberg’s first printed edition, not even when Georg Wilhelm Zapf discovered inscriptions in the hand of a Mainz priest dating from 1456 in the Bibliothèque Nationale’s copy in 1789; and, indeed, Zapf himself, notwithstanding his own discovery, continued to support the theory that Pfister’s 36-line Bible was the first letterpress print edition.

The next chapter, ‘The Long Harvest of the Nineteenth Century’, reveals that a further nineteen complete copies became known in this period. Revolutionary looting, changed borders, and confiscations of church property led to an unprecedented relocation of collections. Around a third of the copies held by religious houses, for example, became available on the newly flourishing antiquaries market. Marshall White devotes particular attention to the copy held by the University of Texas in Austin, whose first recorded owner, James Perry, in 1821 memorably compared the art of printing with the goddess Minerva, who was born already perfect. In terms of its illuminations and binding, this particular copy is the most interesting among those still extant. Its provenance, probably ‘s-Hertogenbosch, can be derived from a complex synthesis of the established evidence. The copy known as Trier II, now widely dispersed, initially came to light in the form of fragments in 1812 near Trier, although large sections of the quires are now held in Mons and Bloomington, Indiana.
Although the Gutenberg Bible became enormously collectable in the nineteenth century, other incunabula were still selling for even higher prices. It was not until 1890 that Karl Dziatzko proved beyond all possible doubt that the 42-line Bible was earlier than the 36-line version, although the 1892 discovery of the Book of the Sibyls from the early 1450s relativized the status of the Gutenberg Bible somewhat.

‘The Last Gleanings of the Twentieth Century’ mainly turns on the question of why the five complete copies discovered in that century remained unknown for so long. However, the nine newly discovered fragments are of even greater importance; they include a number of leaves glued together as pasteboard. Around sixty vellum leaves of a copy from the monastery at Vadstena were discovered in 1906 in Stockholm, where they were being used as archival wrappers. Of the complete copies, two are in Spain (Burgos and Seville respectively), where they seem to have been present from an early date. The author notes without providing further historical context that these, along with a further copy that was sometimes recorded in Spain, are illuminated in Flemish style. In Germany, three important discoveries were made in the twentieth century: one in 1911 in the library of the Graf von Solms-Laubach; one in 1975 in the parish house at Immenhausen; and one in 1996 in the archive of Sankt Marien, Rendsburg.

Finally, in the epilogue to this section, ‘Fertile Ground for the Twenty-First Century’, the author concludes that it is highly unlikely that any further complete copies will be found, but adds that continual advances in manuscript research and digitization are likely to result in the discovery of more fragments in the future. So far, the only new discovery of the twenty-first century is of a vellum leaf from a manuscript held by the Augsburg Staats- und Stadtbibliothek, found there in 2017. The epilogue also includes a summary of the early dissemination of the Gutenberg Bible, mainly based on copy specifics but considerably supported and enriched by our knowledge of the sites where fragments have been discovered. Early owners were mainly church libraries and religious houses, and records also bear witness to wealthy individual donors and book printers who used them as copy-text. Benedictines were by far the most important owners, with at least eight copies held at various Benedictine abbeys, while four can definitely be assigned to Carthusian monasteries. Taken in conjunction with details of their illumination, we can make
out two dominant directions for distribution of the Gutenberg Bibles. A large number seem to have been brought to Erfurt, Leipzig, and Lübeck, where they were first illuminated and then sold. Others made their way northwards down the river Rhine and thence in some cases to Flanders (three copies) and England (two copies).

Part III of the book is a census of the Gutenberg Bibles that summarizes all information on the copies themselves and their ownership history. In structure, the author follows Needham’s 1985 census, which has the particular advantage of not depending on localization of copies (as the place where the copy is held need not be connected with the place where it was made) (see pp. 294 ff.). He first lists the vellum copies, then the paper ones, while the copies within each category are listed in order of how far they reproduce the first setting.

Fragments are listed from No. 50 onwards and are named in each case after their first known place of provenance and ordered according to the number of leaves in each case. Unfortunately, the identification of copies in the census is made more difficult by the fact that the author does not provide his own census number in the narrative of Part II. This makes it difficult to find copies in the census coming from Part II. But going in the other direction, when coming from the census, it is easier to find copies in Part II according to the year of discovery. Descriptions of the complete copies are generally rather brief, including the shelfmark, the numbers of the most important censuses, the exact dimensions of the block, any missing leaves, the numbers of the quires in the second setting, along with the first traceable provenance, and, conspicuously visible, the date of discovery. The copy held by the University Library at Mons counts as a complete copy, despite the fact that 557 of its leaves are dispersed, as do the ‘Noble Fragments’ from Mannheim. In the case of the Mons copy, the author provides an impressive list of all known sets, while the various locations of the ‘Noble Fragments’ remain undisclosed.

The author provides extensive information on the fragments. Each fragment is identified by the leaf number, its folio number, and the biblical text it contains. For many of the fragments, the author also provides details of its discovery and ownership history along with any relevant literature, which adds a good deal of value to this part of the census. Following the census itself, there is a list of the additional recorded surviving copies/fragments for which evidence exists, but whose current location is unknown, numbered from A to
W. Three additional notional copies are listed as X, Y, and Z respectively; these were used by printers of later Bibles in Strasbourg and Bamberg as copy-texts. Another list records four ‘Accessory Fragments’ which, to judge by the way in which they are manufactured, cannot have been part of a complete Bible and must be interpreted as either trial print runs or as supplements to incomplete Bibles. Each of these is discussed in detail. The census concludes with a list of over 50 ‘Doubtful Copies and Ghosts’. This list includes all the copies which appear in the records but the mention of which is probably or certainly based on misinformation. This excludes the many supposed identifications before 1890 which actually referred to the 36-line Bible. Each entry is considered in detail, while the author has wisely chosen not to include the relevant secondary literature in the main bibliography. The book is completed by a short index of people, places, and things along with a longer provenance index.

The author presents us with an impressive collection of material that demonstrates his excellent knowledge of current and past Gutenberg research. Much of this consists of assessments of printed material that in part is very difficult to find (for example, auction catalogues, early library catalogues, correspondence, travel journals), and sometimes he also refers to handwritten sources. Again and again, he manages to discover something new. Once the history of the extant copies is known in detail, it is possible to assign copies mentioned in the records to those we know of, thereby using the exclusion principle. Sometimes this requires some extremely complicated detective work, for example, if we look at how George III acquired the copy now in the British Library from Alexander Horn (pp. 169–70).

Overall, the book provides a sweeping panorama of the history of a book and all its copies in a way that has probably never been done for any other printed work. Researchers will be especially grateful for the comprehensive data on ownership history, which, one imagines, could easily form the basis for a digitized database or, for example, the digital mapping of known distribution routes. The book will also be extremely valuable as a work of reference when identifying and placing fragments and records in relation to the Gutenberg Bible, and for future research on the history of collecting and the reception of early printed texts since the eighteenth century. Thanks to its wealth of illustrations, which provide images of numerous leaves and manuscripts along with the most important other sources for a history of
typography, the book is not only visually appealing but also very suitable for teaching at university level. The book could also inspire an ongoing search for new evidence of individual copies; for example, research on scholarly publications, unpublished papers, and records could develop discussions that Marshall White has initiated. To take just one example, the Göttingen Gutenberg Bible appears in a 1588 inventory compiled by the chancery clerk Eberhard Eggelinck for the library of Duke Julius in Wolfenbüttel: ‘Biblia latina, pars prima et secunda, vff Pergamein gedruckt In dem aller ersten vnd eltesten gedruck, Da die Druckerey erst angefangen In folio vnd in bretern mit gelem semischen leder vbertzogen, vnd mit measings Puckeln vnd Clausuren beschlagen gebunden’ (Latin Bible, first and second part, printed on vellum in the very first and oldest edition, when printing had just started. In folio, bound with boards in yellow soft leather and furnished with brass studs and clasps).1 While the entry itself may tell us little that we did not already know about the provenance of this particular copy, it does provide spectacular early evidence that the 42-line Bible was regarded as the earliest printed version of the Bible, and as at least one of the oldest works to be created in letterpress print at all. It appears that German princely courts of the sixteenth century may have possessed a level of bibliographic expertise that to date has gone unrecognized.


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A vital part of the agenda promoted by Barbara Stollberg-Rilinger’s research group ‘Vormoderne Verfahren’ (‘Pre-Modern Procedures’) at the University of Münster has been its focus not so much on norms, but rather on practices; not so much on authoritative statutes, but on the authority of procedures. Procedures tend to develop logics of their own, establishing their authority and binding force by using certain forms. André Krischer’s *Habilitation* thesis is very much indebted to this approach, setting out to achieve nothing less than a cultural history of the English lawsuit from the mid sixteenth to the mid nineteenth century.

The history of the English lawsuit is especially interesting because in contrast to continental practices, disputes in English courts were conducted publicly and orally. In fact, the public and oral nature of the trial by jury was seen by contemporaries as the main advantage of English law in comparison with its continental counterparts. As a result, the power of procedures was more evident, and it was also more important to achieve the acceptance of court rulings by all those affected by them. Thus the premiss of the study is that the binding force and legitimacy of a verdict is based more on the authority of form than on the plausibility of the evidence presented at the trial itself; Krischer’s aim is therefore not only to analyse the proceedings themselves, but to include perceptions and debates concerning prominent trials. Often, trials were reported and discussed in print; even the statements by the defence and the last words of the convicted before their execution sometimes appeared in printed media.

The study concentrates on lawsuits concerned with high treason. Legally defined for the first time in the fourteenth century, high treason has always represented a special case in jurisdiction, but also a site of experimentation and a model for lawsuits in general. Since lawsuits dealing with this crime are mostly well documented, it makes sense to study the evolution of legal proceedings for high treason in England.

In an introductory chapter, Krischer explains the development of certain parts of the trial, such as the indictment, the defence, the gath-
The English Lawsuit

ering of evidence, and the verdict of the jury, and the role of all these in the overall proceedings. The indictment was the basis for the whole trial, because everything that followed was aimed at demonstrating (or contesting) its truth. Thus the wording of the indictment was an important as well as controversial step in the whole process.

The main body of the book is based on a broad variety of sources concerning thirty cases of high treason, and unfolds roughly along chronological lines. Beginning in the Elizabethan era, juridical proceedings were performances of authority, in the course of which conflicts were enacted, verbalized, and made determinable. The trials were carefully staged orchestrations of impartial justice, even when—for example, in the case of Elizabethan trials against alleged Catholic conspirators—the principle of ‘innocent until proven guilty’ did not apply in practice. Because of the ‘paranoid style’ (p. 97) that tended to characterize legal proceedings in the context of conspiracy theories, such proceedings did not aim primarily to establish guilt or innocence. Instead, they were built entirely on a presumption of guilt. It was important, however, for the courts to avoid the impression that the alleged conspirators were being tried because of their Catholic faith. The carefully worded indictments therefore focused on the accusation of conspiracy against the Queen. Sometimes the accused tried to negate the authority of the court or the judges, sometimes they declared that the jury was prejudiced. But even in contesting claims of authority or impartiality, they played a part in the staging of the trial. It was decisive for the legitimacy of the proceedings that the court could force a certain role on all participants and thereby make them take part in a script with a specific outcome, that is, a verdict. Although this verdict was often a foregone conclusion, the symbolic expression of fairness played an important role in establishing the legitimacy of the proceedings even in the Elizabethan trials against alleged Catholic conspirators.

For the English Revolution and the Interregnum, only one case is analysed in detail, that of John Lilburne. The (admittedly well researched) trial of Charles I and other legal cases during the Commonwealth and Protectorate are omitted. The book continues with the post-Restoration era after 1660. The proceedings against the regicides and during the Popish Plot of 1678 represented a particularly important step in the development of legal proceedings. Although the actors still retained some room for manoeuvre, the script for interac-
tions during trial became more formalized over time. Whereas the earlier trials were performed as rhetorical contests, they now became laboratories or experimental settings aimed at discovering the truth. Krischer characterizes this process as a change from a regime of persuasion to a regime based on formalized procedures of examining witnesses with the aim of reconstructing the course of events. For Krischer, this represents a new link between changed orders of knowledge and the expression of power, which he interprets against the background of the rise of natural philosophy and science. From now on, proceedings relied on witnesses to construct and produce the facts on which the jury based its verdict. The more the proceedings appeared to be governed by standardized rules, the more they were accepted as fair trials. The debate about fairness reached a new stage after the trials against the Whig leaders in 1683 (Rye House Plot), which after the Glorious Revolution led to some reforms, especially the right to an advocate at the trial who was permitted to speak on behalf of the defendant. Viewed by contemporaries as well as legal historians as a step forward for fair procedure, Krischer points out that this reform also strengthened the legitimacy of the judicial process.

Starting with the trial of George Lord Gordon in 1781, Krischer identifies the rise of a third ‘regime’ that would come to dominate legal proceedings in the subsequent seventy years. He argues that under this new regime, the ‘programmatic structure’ of proceedings became conditional. That is, it was characterized by steps that progressively built upon each other, where each step within the procedure was dependent on the outcome of the one before it. In addition, it was characterized by the increasing immunity of the individual procedural steps, which could not be questioned and formed the basis for the jury’s verdict. Krischer underlines the self-referentiality of the proceedings, which led to a high degree of acceptance.

It is one of the many strengths of this study that it deals openly with the teleological traps of a longue durée perspective. Krischer has resisted the temptation to frame his study as a pre-history of modern jurisdiction, which would have had the effect of making modern proceedings appear the inevitable outcome of all the foregoing historical developments. At the same time, it addresses change; not change in the sense of progress or an inevitable evolution to modernity, but simply in the sense that things became different from what they were.
before. The author describes this development as a form of differentiation (*Ausdifferenzierung*), a concept loosely borrowed from Niklas Luhmann. What is sometimes missing in Krischer’s treatment of his subject, however, is the contextualization of the lawsuits, which were, of course, part of wider political conflicts. Although Krischer seeks to include public debates, his focus is on the perceptions of and judgments on lawsuits, rather than the broader political setting of a conflict. However, even without these contexts the book is a good 720 pages long; it would be excessive to demand the inclusion of further aspects.

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The question of Britain’s relationship with Europe—its place in it, or position separate from it—has been discussed by historians since long before the current Brexit debates. Only two years before the 2016 referendum, the tercentenary of the accession of the House of Hanover to the British throne in 1714 provided a special opportunity (as well as funding) to re-assess Britain as a European, indeed Continental power. Johanna Oehler’s book is part of a range of publications, exhibitions, and conferences associated with this anniversary.

The University of Göttingen is not only in the title of this book, but also in its DNA, as this is the revised version of Johanna Oehler’s Göttingen Ph.D. dissertation. It was written in the context of the graduate research group ‘The Personal Union of Great Britain and Hanover, 1714–1837’, which produced a number of excellent studies on the political and cultural impact of the 123 years of Personal Union between Britain and Hanover. Moving away from an older approach that considered Anglo-German relations in that period mainly in terms of German anglophilia and Britain’s employment of German mercenaries, this fresh approach instead highlights the interdependencies between Britain and the Continent in the eighteenth and nineteenth centuries.

The focus of Oehler’s book is thus firmly on British students in Germany. It analyses the 237 British students who attended the Hanoverian Landesuniversität Göttingen between its foundation in the 1730s and the occupation of Hanover by Napoleonic troops in 1806—the largest group of non-German students at the university. Oehler analyses the social composition of this group, their motivations and experiences, and the contacts and networks they built up, and focuses in particular on their role as agents of cultural transfer between the German and the British parts of the Personal Union. However, Oehler achieves much more, as she provides an important addition to the history of British universities in the eighteenth and nineteenth centuries, highlighting not only that the republic of letters was a supranational space of scholarly communication, but also that
Hanover should be integrated into a history of British higher education.

Founded in 1734, and opened ceremonially three years later, Göttingen developed into one of the best attended universities in mainland Europe within a few decades. Entirely in line with established scholarship, Oehler points out that unlike medieval universities and the many universities set up following the Reformation, Göttingen university was run by the state, independent of clerical influence, and open to students of all denominations. A typical product of the Enlightenment, Göttingen taught ‘useful’ subjects in applied settings, integrating clinical teaching into medicine and making statistics and diplomatic practices part of the law curriculum. From the start, the Hanoverian government, hoping to attract wealthy students, in good mercantilist fashion, marketed Göttingen as a place of polished student culture, where future diplomats and courtiers, judges and doctors could gain the expertise as well as manners they needed to succeed in the monde. The ceremonial visit of founding monarch and namesake George II in 1748 was not least designed as a clever marketing ploy to promote the king’s new foundation, supported by an English-language prospectus. The Faculty of Philosophy offered a wide range of modern languages as well as tutoring in fencing and dancing. Within a short time, Göttingen was equipped with a top-range library, botanical garden, and chemical laboratories, and in 1751 an Academy of Sciences was added to the portfolio.

Apart from a comprehensive introduction, and a very useful appendix, which includes a list of all British students at Göttingen before 1806, the book is structured into five parts, analysing the entire group of British students in their socio-cultural context before focusing in more detail on aristocratic and non-noble students, discussing Göttingen’s role as a centre of European scholarship, and situating Göttingen in its north German academic context.

Combining quantitative analysis and a qualitative approach, Oehler provides a ‘collective biography’ of British students at Göttingen, which allows her to profile the entire group of British students over a longer period while considering the specificities of individual biographies. Some of these are explored in detail, and Oehler shrewdly avoids retelling only the better-known stories of Samuel Taylor Coleridge, or the three younger sons of George III. Instead, she highlights the German experiences of John Murray (later 3rd
Duke of Atholl) and the Scottish physician Andrew Duncan, for example. British students at Göttingen are thus shown to be a diverse group, who came from a variety of social backgrounds and pursued quite different interests. One thing, however, they had all in common: not only aristocrats, but all British students at Göttingen were part of well-established networks in Britain, and many had academic or aristocratic contacts in Hanover or other parts of the Continent.

This was particularly true for the significant group of aristocratic students who dominated the British contingent in the 1750s. For these students, Göttingen merely represented one stage of their Grand Tour. Deliberately avoiding the acquisition of a rather embarrassing academic degree, they complemented their networking at German courts with studies in history and Roman law as well as learning German phrases that might help them in diplomatic postings. The proximity of Hanover offered access to the elector–king’s summer court at Herrenhausen, and the sons of Jacobites, such as John Murray, and disgraced courtiers, such as James Brydges (later 3rd Duke of Chandos) used their stint at Göttingen to re-establish family links with the Hanoverian monarchy. George III famously never visited his German dominions, but sent his three younger sons to study at Göttingen in the 1770s, as well as supporting the university and its professors in a number of ways, and inviting Göttingen professors to his court. Spending some time at Göttingen thus remained attractive for British courtiers who wished to endear themselves with the king.

After the Seven Years War, however, a new pattern emerged that put Göttingen firmly on the map of British scholarship. From the 1770s the number of British ‘middle-class’ students rose steadily, and by the later eighteenth century the sons of merchants, bankers, and doctors enrolled in Göttingen’s Faculty of Philosophy to prepare for a career in business, politics, or the military. Mirroring the pattern of the Grand Tour on a lesser social level, they focused on acquiring a wide range of useful knowledge, but unlike the earlier generation of aristocratic visitors, these students were supervised by a new and dedicated generation of Göttingen professors (above all the renowned physicist, declared anglophile and wit, Georg Christoph Lichtenberg), who had their own networks in British academia as well as at the royal court. Yet, as Oehler points out, it was ultimately
a relatively small number of individual intermediaries who built up contacts and maintained networks across the Channel. Apart from Lichtenberg, Ludwig von Schrader was crucial here. As a secretary to Frederick, Prince of Wales, and a correspondent of Göttingen’s star professor, Albrecht von Haller, von Schrader inspired several members of the court to send their sons to Göttingen.

In addition, Göttingen became a prominent address for students of medicine, most of whom chose the Hanoverian university as a medical finishing school after obtaining degrees in Edinburgh, Glasgow, or London. John Sibthorp and Edward Ash received travel grants, while others, such as Andrew Duncan, came at their father’s expense. In contrast to their well-off peers from aristocratic or business backgrounds, these young medics and natural scientists did not mix with courtiers and aristocrats, but focused on Göttingen’s excellent libraries and top-notch research facilities, as well as on building up contacts in the world of scholarship the former group shunned. Here Oehler convincingly argues that it is difficult to distinguish these often ‘mature’ students from travelling scholars, who also visited Germany’s leading university in large numbers. Göttingen’s success was certainly rooted in the fact that this cosmopolitan university managed to cater for quite different groups of non-German students.

The last chapter contextualizes Göttingen within the wider academic world by comparing the Hanoverian university with a rival institution. The Collegium Carolinum in the nearby Duchy of Brunswick had been established in the 1740s as a novel type of college of further education catering for the rich, and was the second most popular German destination for British students, even topping Göttingen for a number of years. Oehler’s chapter on this is one of the best recent accounts of this important educational institution, which has received far too little scholarly attention. Her comparison, moreover, highlights the underlying prerequisites for all British student trips to eighteenth-century Germany. The central criterion was the existence of dynastic links to London (George III’s sister was married to Duke Carl Wilhelm Ferdinand of Brunswick); the institutions were run by decidedly anglophile academics who kept up close relations with British colleagues; they promoted an attractive set of applied science subjects allowing British students to prepare for a non-academic career; and they were decidedly ‘elegant’ institutions designed to attract wealthy students looking for an education in manners as
well as subjects. Needless to say, both Brunswick and Hanover were Protestant. The success of both Göttingen and Brunswick was also due to the failures of Oxford and Cambridge, which experienced their ‘Great Depression’ (Lawrence Stone) in the eighteenth century, as despite the scholarly merits of individual dons, their curricula failed to take on board new subjects such as the natural sciences.

The role Göttingen played as the most important destination for British students outside the British Isles ended when Napoleon’s occupation of Hanover after 1803 and the Continental blockade of 1806—designed to eject Britain from European politics and trade—interrupted the close exchanges between Britain and Hanover. After Napoleon’s defeat, British students returned almost immediately, but Oehler points out that the particular constellation of the later eighteenth century could not be revived. The broad education sought by eighteenth-century students was now increasingly being replaced by Fachstudien (specialist subjects), and the new Prussian universities of Berlin and Bonn immediately became attractive for foreign students after an entire generation of Göttingen’s star professors (from Lichtenberg to Christian Gottlob Heyne) had died by 1812. It was not the end of the Anglo-Hanoverian Personal Union in 1837 that put an end to Göttingen’s particular role for British higher education, but Napoleon’s enforced Brexit of 1806.

Apart from university archives in Germany and Britain, Oehler has unearthed a large number of correspondences, diaries, and memoirs, and has followed the traces of Göttingen students to local archives in Chester and Cornwall, and the Huntington Library in California. Her book thus brings the personal union to life as a space of communication that was not restricted to the few courtiers and diplomats maintaining political relations between London and Hanover, but affected the careers and lives of lawyers and bankers, officers and medics from London to Scotland and Ireland. How much did all this, then, contribute to cultural transfer? Here Oehler provides a nuanced discussion, admitting that the ‘impact’ of Göttingen is hard to measure. While some British aristocrats worked hard to shield themselves from immersion in a German student experience, others built up long-lasting contacts with German scholars, in particular, in the natural sciences. This becomes especially clear in the ego documents, which discuss Göttingen lectures as well as practical questions of everyday life in a foreign country.
The comprehensive list of all British students in the appendix is particularly useful as Oehler for the first time manages to identify the actual length of time British students spent at Göttingen. In the eighteenth century, after all, not all visiting students matriculated properly—some wanted to save the fees involved, while travelling aristocrats sometimes did not bother with the formalities of academic life.

Oehler’s book makes an important contribution to scholarship on eighteenth-century universities and higher education in Britain and Germany, the Personal Union, and Anglo-German relations. It is well written and makes a potentially dry topic interesting and vivid. The number of errors (George III was George II’s grandson, not his son, p. 127, and in chapter VI, the dukes of Brunswick are confusingly called ‘Fürsten’) is negligible, and Oehler’s command of secondary literature on the monde of eighteenth-century Britain and Germany is impressive.

This book is part of a veritable wave of publications and events reconsidering the Personal Union published in the context of the 2014 anniversary, which all seemed to put the old view of Britain’s ‘splendid isolation’ in the eighteenth and early nineteenth centuries to bed for good. With hindsight, however, this new consensus itself needs to be re-evaluated (and not just because of the 2016 referendum): German universities and museums, as well as the GHIL, were much keener to remember the advent of the House of Hanover to the British throne than British institutions (not to mention the media), which tended to focus more on the anniversary of the outbreak of the First World War—a very different chapter in the history of Anglo-German relations.

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This book could not have found a better cover image than that of a destroyed memory—a monument that only exists as a photograph. The monument itself, a statue of the Prussian Prime Minister Otto Theodor von Manteuffel, was melted down for the armour of the First World War. Like his monument, Manteuffel’s biography has fallen into oblivion, and now only a few specialists in Prussian history know him as an exponent of post-revolutionary reaction politics. His name is associated with repression, censorship, and surveillance of the domestic and foreign opposition, especially of the ‘forty-eighters’, the revolutionaries of 1848 who had remained in the country.

Anna Ross wants to prove this stereotype wrong and devotes herself unbiasedly to the challenges that a Prussian politician had to overcome in dealing with the revolution of 1848–49 and its consequences. She has consulted the relevant Prussian archival sources for the central, provincial, and local authorities; estates; files of the royal house; and relevant newspapers and magazines. From the secondary literature, she has taken into account international and especially German-language research, and it is surprising how richly Prussian history has been researched, even for the alleged decade of reaction that lay in the slipstream between revolution and the foundation of the German Reich. Nevertheless, the author manages an approach that brings a breath of fresh air to the discussion of the post-revolutionary decade.

Although Manteuffel is the focus of the study, Ross is not writing a biography, but an inside view of the Prussian Ministry of State. It is important to her to emphasize that the ministry did not undergo an involuntary modernization, as was previously thought, but played an active role. She is interested in specific policies and thus makes it clear how undifferentiated the approach to the notion of ‘conservative’ politics has been so far. The concept of the conservative is differentiated in an almost exciting way. She destroys the image of a monolithic Prussian domestic policy after the revolution, showing both the counterforces (above all, the ultraconservatives represented in the camarilla), and the resistance and stubbornness of King Frederick
William IV. In sharp contrast to Prussia’s ultraconservatives (Leopold and Ernst Ludwig von Gerlach) Manteuffel showed a new realism, more pragmatic and deviating from the doctrinaire approaches of the ultraconservatives.

The author is also concerned to refute the idea of a Prussian special path (Sonderweg). She does this by comparing the policies of Austria and the German middle states in the same period, recognizing many parallels with the bureaucratic modernization of the Habsburg Monarchy after 1850 in the Bach era. Ross also observes a comparable flexibility of conservative governments in many German states, following recent studies embedded in and across Europe more broadly and with recourse to the latest research.

Ross recognizes parallels with previous state-building projects in the Napoleonic era, illuminating threads of continuity that are too often overlooked in histories of the nineteenth century. However, she goes even further and puts forward the bold thesis that ‘the post-revolutionary moment should be recognized as a second Reform Era, essential to the formation of the modern Prussian state’ (p. 18). The policy under Manteuffel was the logical continuation of the Prussian reforms of Stein and Hardenberg, breaking down noble prerogatives. Manteuffel sought to remove the feudal intermediaries, and, in cities, dissolved the authority of the guilds, creating a growth-oriented capitalist economy; he also wanted to complete the process of peasant liberation. Then as now it was the bureaucracies that carried out these reforms. The bureaucracy promoted a course of moderate reform between revolution and total restoration.

However, the decisive and strongest argument for the continuation of the Stein–Hardenberg reforms can be found in the political will to seek compromise with the civil constitutional movement, which the reformers had failed to do at the time. Brandenburg and Manteuffel were able to convince the king, or at least to force him to acknowledge, that constitutionalism was an urgent necessity and the only tool by which anarchy, terrorism, and the Jacobinism of the French Revolution could be overcome.

In detail, Ross is interested in the state-building projects and initiatives to strengthen contact between state bureaucrats and the Prussian population. She examines policies in various fields: judicial and penal institutions, agriculture, industry and communications, and knowledge management, made possible by the statistical knowl-
edge being produced by the Prussian Central Statistical Office. She recognizes a consistent will to modernize the state and centralize its resources, not least through investment in infrastructure, for example, new railway tracks and telegraph networks. Commercial affairs changed decisively, facilitating free trade, introducing an array of measures to protect impoverished craftsmen against hardship, promoting the mining industry, and precipitating the introduction of commercial investment banks.

The Municipal Ordinance of 1850 was a logical continuation of Stein’s reforming work, now without drawing a distinction between region or city and the countryside. Here the police assumed an important function because they were not only an instrument of repression, but part of the old welfare police in Berlin. This is shown by the introduction of the Police Construction Ordinance (Baupolizeiordnung) in 1853, tightening building regulations, preventing fire, and affirming a growing state interest in public health. Demonstrating a strengthening of official activity commonly associated with self-governing communities, the police played a role in reforming urban life, especially in Berlin under Hinckeldey.

The Criminal Code of 1851, however, reveals the inherent ambivalence of Brandenburg’s and Manteuffel’s initiative. Superficially, one might imagine that it was created as a counter-revolutionary measure. Ross shows, however, that it helped to constitute a second Reform Era, believing that procedural, rather than substantive reform was the best way to establish the rule of law, securing codes for trial procedures, laws for court organization, and laws governing the organization of the private legal profession; in other words, it was a significant contribution to the formation of the modern Rechtsstaat in Prussia, continuing the liberal fiscal policies of the revolutionary years.

Ross does not conceal the restrictions imposed by changes in electoral law, the tightening of criminal law, and the manipulation of freedom of the press and of associations. Manorial estates remained exempt from taxation. The government took many measures to control and manipulate the press. This was done by press offices, promoting newspapers with subsidies for government-friendly broadsheets. Here modernization initially went hand in hand with manipulation and monitoring. Nevertheless, public opinion played a larger role than had previously been acknowledged; not least the family
papers played a large part in this, keeping national questions alive throughout the 1850s. The management of the press and the sharing of government-friendly data continued after 1858, especially under Bismarck.

Nevertheless, the study presents a convincing argument that the policy of the Manteuffel era was formative for the development of the Prussian state, a second Reform Era. Those who deal with the New Era that began with the Prince Regent Wilhelm must now reconsider whether there really was so much that was ‘new’ about it. What was new was clearly not so much the implementation of specific policies underpinned by an effective bureaucracy—a form of government that was already seeking legitimacy through participation, and which continued under Bismarck. Wilhelm, however, set a qualitatively new accent when he proclaimed to the State Ministry on 8 November 1858 that Prussia must achieve moral conquests in Germany (‘In Deutschland muss Preußen moralische Eroberungen machen’). The Manteuffel era, on the other hand, was introverted, focused on stabilizing and developing the state.

This new interpretation of the post-revolutionary era is supported by evidence that Brandenburg and (after the latter’s death in 1850) Manteuffel were firmly convinced that popular participation and representative assemblies had become a necessary part of modern politics. It is a merit of this study that it also highlights the resistance to this strategy and so clearly shows that the prime ministers were pursuing an approach of reform, which Bismarck was later able to follow up. However, I would not go so far as to claim (like the historian Daniel Ziblatt, with whom the author sympathizes) ‘that the successful emergence of “conservative political parties that originated representing old regime elites” was the essential shaper in democratization in the nineteenth century’ (p. 14).

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Anyone working today on what is often described as ‘the new monarchical history’ tends to find themselves looked at askance. At one conference in Germany where I spoke on the subject, a member of the audience wanted to know why any historian would be interested in monarchies and their concomitant governance structures, given the utter irrelevance of these to most modern societies. Unlike my own inadequate response at the time, the book presently under review provides an eloquent answer to this important and interesting question.

This outstanding monograph, which represents Torsten Riotte’s Habilitationsschrift, takes a new view of exiled monarchies as political and dynastic institutions that can offer an alternative perspective on state-building in the nineteenth century. Riotte, senior lecturer and researcher in contemporary history at the Goethe University, Frankfurt am Main, examines, for example, how monarchs and their families were treated by the governments of the countries where they eventually established their homes in exile. He argues that this can offer substantial insights into contemporary legal theory and practice, which were by no means always identical. When it came to property rights, for example, legal rulings tended to take into account the high social status of these dynasties.

The methodological approach is effective and useful on several levels. The author views dynasties as an essential component of modern constitutional and absolutist government systems, whose political and socio-cultural function can only be understood and explained in the context of other institutions. In other words, monarchies did not exist in a vacuum but, as Martin Kirsch has shown, played an active role in shaping events and decision-making within political systems.¹ Riotte’s book therefore does not simply examine the rela-

tionship between exiled monarchs and their host countries. He also addresses the gap left by deposed monarchs ‘back home’, which legitimistic parties aimed to compensate with political activity and by encouraging a continuing sentimental attachment to the dynasty itself. Both perspectives locate monarchies in their respective socio-political contexts, considering how the relationship with exiled dynasties and their supporters was negotiated within the latter. Thanks to this approach, Riotte fully succeeds in his stated intention to write an ‘entangled history of state-building and legitimism’ (p. 45).

A second valuable feature of the book is its international comparative approach, in a research field that even today tends to be defined by national case studies. Riotte compares the exile of the Hanoverians after 1866 with that of the Bourbons, exiled in the wake of the July Revolution in 1830. Both exiled dynasties took refuge in the Austrian Empire and spent decades in their host country, thus allowing for direct comparisons. The international comparative approach is reflected in the author’s ambitious research methodology; the source materials for this study came in part from the often closely guarded family archives of the dynasties as well as from archives in Germany, France, the United Kingdom, Austria, and Italy.

As case studies, the book focuses on the Comte de Chambord and Duke Ernst August of Hanover, both the heads of their respective dynasties and thus representative of two generations of ‘absent monarchs’ as Riotte puts it. The biographical comparison between Chambord, the older of the two men, who became head of the exiled House of Bourbon in 1844, and Duke Ernst August, head of the Hanoverian dynasty from 1878, shows its usefulness particularly in the section which discusses citizenship rights. While the deposed French monarchy retained full rights to extraterritoriality after 1830, Riotte shows that just one generation later, a special legal status for exiled families could no longer be taken for granted. In discussing the citizenship rights of the Welfs, Riotte considers the case of the Bourbons (p. 122) and shows through this direct comparison how dramatically the rules of the game for deposed monarchs exiled to Austria changed in the second half of the nineteenth century.

The history of dynasties in exile is often seen as just one element of the general collapse of monarchical structures in the nineteenth century. In this familiar narrative, the revolutions and government crises of the nineteenth century made the cracks in a hopelessly outdated
system ever more obvious, until the First World War brought about its final and total downfall. Riotte’s work, like other current research projects on the modern history of the monarchy, pays greater attention to the opportunities monarchs and their families found to actively shape their lives in exile. In the long term, the field can only profit from such alternative approaches and interpretations, which do not set out to understand the history of the monarchy from a teleological perspective, but attempt to understand how dynasties were embedded in contemporary contexts. For example, Riotte highlights the importance of financial autonomy for the Bourbon and Welf families in exile. No longer entitled to funding from the civil lists, and with their assets limited or confiscated, exiled monarchs often plunged into new roles as modern businessmen and cunning investors on the financial markets (pp. 125–6), acquiring property and speculating on the stock exchange. This makes a refreshing change from the traditional image of the exiled monarch, often depicted as a tired and sad old man who, far from the public eye, drags out his empty and hopeless days alone and yearning for past glories.

At the same time, Riotte notes that the dynasties of the upper nobility represented a very specific ‘club’ that thought and lived in traditional socio-cultural categories. Unlike earlier works on monarchies in exile, Riotte’s book is not particularly interested in the restoration question or the ‘desire to go home’ (p. 37). Instead, he focuses on the presence of the dynasties in their host countries and on the strategies they used for communication and to obtain financial security. Both the Bourbon and the Welf dynasties made every effort to finely tune their survival strategies to ensure that other (ruling) noble families would identify with them; just because a dynasty was in exile did not mean that it ceased to be part of the European and global dynastic system. Instead, membership of this particular club determined their strategies: ‘the way in which monarchs acted in exile was not governed by their wish for political restoration but by their view of themselves as members of the highest echelons of the nobility’ (p. 133).

A further innovation of Riotte’s book is the author’s introduction of the concept of ‘dynastic survival’. This describes ‘the tension between the politicization of the person and the attempt of the monarch in exile to control and limit this process in such a way that other members of the nobility (particularly those of their host coun-
try) would not be able to find reasons to fault them’ (p. 51). Riotte sees ‘dynastic survival’ as a third category to complement that of the personal and the political, which on their own cannot adequately describe the highly nuanced experience of nineteenth-century monarchs in exile.

Whoever writes or reads about monarchies knows how blurred the line between private and political actions can be. This applies as much to internal actions within the dynastic structure as to external scenarios where the monarchy performs a representative function. The ‘Black Spider Memo’ scandal in the UK in 2015 epitomizes this difficulty. If the British heir to the throne, Charles, Prince of Wales, writes letters to ministers of Her Majesty’s Government which are not intended for publication, is he writing as a private individual, or does the office of the Prince of Wales always silently accompany his actions, making them de facto political? Even if we use the term ‘political’ in its broadest sense—which, if we are cultural historians, we will tend to do—this does not make it easier to differentiate. In the case of the Black Spider Memos, even a ten-year legal dispute was not able to resolve this question conclusively!

To my mind, the concept of ‘dynastic survival’ is a useful new tool in the methodological toolbox for historians of the monarchy, as it highlights a certain tension which has not yet been adequately addressed. Whatever monarchs from dynastic families did in exile—whether it was receiving legitimistic visitors with the greatest of ceremony or financing a revolutionary army in the hope of regaining their lost kingdom—they did it as representatives of an exclusive social class that had its own forms of communication and specific privileges. Monarchs and their families in exile took care not to lose the support and good opinion of this exclusive club, and thus it was dynastic survival that determined the strategies available to them for the political restoration of the Crown.

With this concept, therefore, Torsten Riotte’s study has given current research on the history of the monarchy new impetus and a new direction along with helpful and well-founded methodical approaches that are certain to inspire further case studies on this topic.
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SEBASTIAN GOTTSCHALK, *Kolonialismus und Islam: Deutsche und britische Herrschaft in Westafrika* (1900–1914), Globalgeschichte, 27 (Frankfurt: Campus Verlag, 2017), 324 pp. ISBN 978 3 593 50676 0. €43.00

In the first half of the twentieth century more than half of the world’s Muslims lived under British imperial rule. This connection between British colonialism and Islam—especially in the case of India and the Middle East—has been the focus of much research. But as Sebastian Gottschalk points out, relatively little attention has been paid to the relationship between European colonialism and Islam in sub-Saharan Africa. In his case study, the author focuses on the Sokoto Caliphate, a powerful independent entity in West Africa for much of the nineteenth century, which was subjugated to British and German rule. By comparing and contrasting the developments in Northern Nigeria and Kamerun, and embedding colonial practices on the ground within Western academic and political discourses on Islam, Gottschalk aims to contribute to this growing field of research.

The study is based on files from the British and German colonial offices (national archives in London and Berlin) and the German administration in Kamerun (microfilm copies from the national archive in Yaoundé). Unfortunately, Gottschalk was unable to access the files of the British administration in Nigeria. In addition, he draws on the private papers of British colonials (held in the former Rhodes House Library, Oxford) and publications by influential imperialists and scholars (for example, Frederick Lugard, Kurt Strümpell, Paul Marty and Carl Heinrich Becker). This source selection allows an insight into the ideas and expectations, as well as the decision-making processes, of Europeans colonial actors. But, as in so many studies on the subject, the material makes it difficult to create a balanced picture that also includes an African perspective, something Gottschalk himself acknowledges and aims to correct by a thorough re-reading of the sources. While the author rightly points out that missionary activity in the area discussed was limited, mission societies had an enormous influence on the debates on Islam. Had at least some missionaries’ voices from Nigeria and Kamerun been incorporated, a more vibrant picture of local conditions and issues could have been created.

The book is firmly rooted in a cultural approach to colonial history and in the first content chapter provides an overview of the cur-
rent state of research on Said’s Orientalism and the debates on Islam in nineteenth-century Europe. The most relevant aspect is the paradoxical attitude of regarding Islam as ‘backward’ compared to Western Christianity yet, as a monotheistic religion, more ‘civilized’ than other non-Christian belief systems. This made a formal racialized inferiority difficult to justify. Hence colonials stressed that African Muslims only ‘superficially’ embraced and understood Islam, and thus could justifiably be colonized. Knowledge of sub-Saharan Islamic societies was primarily shaped by travellers to the region, whose publications influenced decision-makers in London and Berlin. This focus on British and German discourses is somewhat lopsided: in France, with its long and intensive engagement with African Islam, colonial knowledge and practices developed differently from how they developed in Germany, and were especially distinct from British colonialism, which was predominantly concerned with Islam in India. Although Gottschalk acknowledges and references recent French scholarship on the topic, he unfortunately does not use French primary sources, which would have allowed for a more balanced assessment of this ‘European’ discourse.

In chapter three Gottschalk explores how this ‘Orientalist’ knowledge of Islam influenced colonial conquest and subsequent rule in West Africa. Both Britain and Germany made the pragmatic decision to follow a pattern of ‘indirect rule’ hoping to avoid costly military campaigns. The ‘men on the spot’ regarded the Muslim rulers they encountered as men they could do business with. However, arrogance and ignorance led to miscommunication and misunderstandings, which turned the assertion of European power into a much bloodier conflict than anticipated or militarily necessary. Yet the public narrative of these campaigns omitted any details of this violence and instead centred around the reciprocal respect shown between Britons/Germans and local Muslims, which differed significantly from the image of colonial wars against other Africans.

After the initial rush to bring as much territory under their control as possible, Britain and Germany ceased to act as colonial competitors and began co-operating in the area as amicably as they did in other parts of Africa. Practices of ‘indirect rule’, which at the time were theorized and celebrated as a particular British form of colonial rule, were embraced by the German administration in North Kamerun even more thoroughly than by their British counterparts in
Northern Nigeria, and many pre-conquest structures, such as the Islamic court system, were kept intact. These observations and inspirations taken from across the border are fascinating, but they are not unusual. Taking a wider perspective on ‘indirect rule’ and the use of comparisons, for example, of German rule in Rwanda, would have made clearer how far European attitudes towards Islam, combined with remoteness and lack of resources and manpower, made a less direct power structure necessary.

The challenges to European rule in Africa are explored in chapter four in the form of the Mahdi Movement in the Anglo-Egyptian Sudan in 1906–7. Here, several uprisings against colonial rule were swiftly crushed by British forces, but left colonial authorities in North Africa concerned about the repercussions for their territories. German and British administrators in North Kamerun and Northern Nigeria readjusted their views on African Muslims, distinguishing now between useful ‘false’ Muslims and dangerous ‘real’ but fanatical ones, with the latter located outside the colony. This outside world is discussed again in chapter five, where Gottschalk explores the impact of the haj on West Africa. Like all colonial rulers, the British in Northern Nigeria and the Germans in North Kamerun disliked and distrusted the uncontrolled movement of colonial subjects, and were often unsure how to handle pilgrims, especially upon their return from Mecca. Reflecting their imperial global perspective, British concerns centred around revolutionary, anti-colonial ideas or, worse, agitators who might aim to infiltrate local society. German worries were more concerned with the spread of diseases and the local disruption of trade and tax.

The book’s final chapter is concerned with the impact of experiences and practices of colonial rule on the metropole. In London, events and conditions in Northern Nigeria were too peripheral and too unimportant to change the general perspective on Islam or on Muslims in the British Empire more specifically. The dominance of India and Egypt in academic, political, and public discourse allowed little space for readjustments to include Islam in Africa. In Berlin, however, theorists and practitioners concerned with Germany’s colonial ambitions and scholars of Islam were grateful for unique and first-hand accounts from West Africa which were incorporated into academic research and teaching. As already noted, the closing chapter—and the entire book—would have benefited from the inclusion of
a French perspective on Islam and Africa. France not only ruled over most African Muslims but also had a long and deep colonial engagement with West Africa in particular. Gottschalk does acknowledge the importance of French scholars, but fails to show the influence these had on British and German discourses.

A further point of critique is the book’s misleading subtitle: ‘German and British rule in West Africa, 1900–1914’. This undersells the book’s geographic scope and timeframe. As mentioned above, Gottschalk links events in Northern Nigeria and Kamerun to wider developments in the ‘Sudan’ and the haj pilgrimage to Mecca and, impressively, is able to transcend a purely regional history. Likewise, at many points the book discusses events after 1914 as well as long-term developments into the 1920s and 1930s. Showing these long-term developments and continuities before and after the First World War is, of course, welcome. However, it is rather puzzling that the important shift in Europe’s relationship with Islam in the course of the First World War is not discussed. Not only did Muslims fight on the side of both Germany and Britain, but West Africa became a theatre of war itself. The collapse of German colonial rule and the transfer of power from Germany to Britain in the Cameroons, in particular, were undoubtedly significant for understanding subsequent events.

While certain arguments in the book would merit further engagement and exploration, Gottschalk’s work nonetheless represents an important addition to the growing corpus of comparative colonial studies. Readers will be grateful for the book’s clear structure, but will also miss an index.

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As this monograph is about emotions, the best place to start is with Harold Bloom’s *The Anxiety of Influence* (1973). In this work, the Yale literary theorist argued that poets feared their poetry could be derivative of another poet’s verse, the impact of which would diminish the quality and especially individuality of their own art. An earlier review of Jensen’s *Wie die Couch nach Kalkutta kam* written by Andreas Mayer, one of the leading historians of psychoanalysis, touches on this anxiety of influence. Mayer suggests that Jensen ignores or remains silent about key contributions to the ‘transnational’ history of psychoanalysis and that, in doing so, the author’s claims to originality should be questioned.

Jensen’s excellent book invites this kind of criticism. Unusually for a book based on a Habilitation thesis, *Wie die Couch nach Kalkutta kam* endeavours to appeal to both academic and non-academic audiences. For scholars beholden to academic conventions such an approach might irritate; for readers keen to gain a panoramic view of psychoanalysis in the early twentieth century this ambition succeeds on many fronts. Divided into four sections—insti-tutions, treatments, emotions, politics—the work address several questions. How did psychoanalysis manifest itself outside Vienna and Zurich, and especially in places far removed from the personal interactions that characterized Freudianism in Central Europe, Britain, and the United States? Can we speak of a psychoanalytical ‘travelling culture’ beyond the Freudian Atlantic? How was psychoanalysis applied in Berlin, London, and Calcutta, and how did these uses affect practitioners and patients alike? What were the political dimensions of psychoanalysis? And, finally, to what extent can psychoanalysis be described as a technique that elicits or even produces emotions?

All four sections detail the emergence, co-production, and reception of psychoanalysis since 1900. As Jensen helpfully reminds his readers, the fact that, with few exceptions, early psychoanalysis was shunned by the establishment facilitated its internationalization, as

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its exponents felt compelled to move beyond the confines of academic psychiatry and psychology. Although (erstwhile) colonialism, particularly in the British and Spanish cases, had some effect on the dissemination of psychoanalysis, its reception did not depend on the straightforward ‘influence’ of Western on non-Western ideas. On the contrary, the first and most serious group to discuss psychoanalysis in Calcutta, for example, did so not so differently from the way in which many Germans or Britons first came in touch with Freudian concepts, namely, as part of a circle of artists, writers, and academics who congregated to have tea, play chess, and consider the latest and most fashionable ideas. Some of the guests would later join the Indian Psychoanalytical Society, which was founded in 1922 as the first non-Western psychoanalytical association. Again, one of Jensen’s strengths is to defamiliarize the reader of some well-worn assumptions, especially regarding the ‘European’ complexion of the Freudian project.

Wie die Couch nach Calcutta kam traces the way in which, once established, psychoanalysis spread in a given setting. A substantial number of women, for whom academia’s doors remained closed and for whom developmental psychology became ever more interesting, joined the movement. Early Freudians relied on private offices; as the membership of societies expanded, institutes (to codify the means by which psychoanalysis would be taught and exercised) and clinics (to treat the less affluent) were founded. Other forms of dissemination and appropriation included trips to famous authorities in the field, the translation of seminal texts, and the publication of journals. Much of this is well known, as is the fact that psychoanalysts continued to practice eclectic forms of therapy, shunning neither suggestion nor hypnosis, in spite of Freud’s disapproval of these methods. Even so, the book relates these facts in fascinating detail, demonstrating the hold of hypnotherapy amongst analysts the world over as well as the continued relevance of cathartic techniques for ‘dissidents’ such as Sándor Ferenczi and Otto Rank. Jensen’s discussion of psychoanalytical treatments, moreover, opens up new vistas of how the circulation of new ideas entailed the invention of new traditions. While patients regularly lay on a couch, ideally allowing ‘regression’ to occur and transference to ensue, in Girindrasekhar Bose’s Calcutta practice the deck chair replaced the couch and the darkened room evoked the hypnotic backdrops of another era. Freud’s famous divan, covered
with oriental rugs and surrounded by heavy furniture, was hardly the norm, as modernist furniture entered the practices of analysts in Berlin and elsewhere.

The book’s main thesis, however, centres on emotions, and how emotions were induced (and intended to be induced) as part of psychoanalytical treatments. Jensen maintains that Freud’s innovation did not lie in interpreting dreams or unearthing the unconscious or discovering transference, but rather in producing affects and utilizing these for the benefit of the patient. Jensen refers to *Emotionstechnik* in this connection, whereby the cool, detached analyst encounters hot emotions in the shape of love, hate, longing, fear, and resentment. Re-enacting relationships of the past, the patient learns that these projections need not be repeated in the future, once their sources are acknowledged and their effects subdued. Again, most psychoanalysts and historians of psychoanalysis would recognize this story. Where the author differs, however, is, first, in his emphasis on the artificial production of new emotions (therapeutic emotions, as it were), which, in their artificiality, can be manipulated and controlled, but which may also turn into ‘real’ emotions in the wake of analysis, when patients continue to feel attached to their analysts; and, second, in his insistence that we use a more neutral vocabulary whenever examining the goings-on between analyst and analysand from a historical point of view.

Unlike Mayer, I find this perspective important, as it offers a more inclusive language that allows for comparative lines of enquiry as opposed to the boundary marking that comes with jargon or a preference for a history of science approach. But Mayer does have a point when he wonders about some of the author’s claims to originality. As much as *Wie die Couch nach Kalkutta kam* has a broad audience in mind (and therefore avoids, rightly so, too great an emphasis on historiographical debates), Jensen might have responded, however briefly, to the works of several authors whose interpretations prefigured or indeed resemble some of his findings. Two examples should suffice: to write about *Selbsttechnologie*, but not to engage with Eli Zaretsky’s thesis on Freud’s contribution to a culture of individuality and authenticity; or to speak of early psychoanalysis’s gendered thinking, especially regarding the rationality/emotionality binary, without taking into account Freud’s abhorrence of unity, symbiosis, and fusion, as documented by Joel Whitebook, do signify an anxiety
of influence. That said, anyone interested in the emergence and diffusion of psychoanalysis in the first half of the twentieth century must read Jensen’s book.

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The National Socialist past of many (West) German journalists went unnoticed, or at least unmentioned, in the Federal Republic until as late as the 1980s—a period when most, but by no means all, of the journalists in question had already passed beyond the reach of any worldly authority. One of the most important actors involved in bringing this aspect of Germany’s past to light was Otto Köhler. Köhler was not a historian; he was a journalist and author whose magisterial work *Wir Schreibmaschinentäter: Journalisten unter Hitler und danach* (1989) was built on a vast body of research. In the same year, Norbert Frei and Johannes Schmitz published their small study, *Journalismus im Dritten Reich*, which was followed in 1995 by Peter Köpf’s *Schreiben nach jeder Richtung: Goebbels-Propagandisten in der westdeutschen Nachkriegspresse*. In impressively researched detail, Köpf’s important book provided evidence, listed according to federal state and publication, of the many personal continuities in journalism post-1945.

Apart from a few studies, however, little has been said since then about the services rendered to the Third Reich by one of the most important and influential intellectual elites of the modern era. This need not surprise us given the continuing lack of interest shown by historians in the research potential of media and journalism, and the discipline’s failure to grasp that the media are a key source for nineteenth- and twentieth-century history. Even in the few German universities that still take media and communication studies seriously (most have either cut back or, as in Göttingen, completely withdrawn their courses in this area), there are almost no qualitative research specialists working in the field of media and journalism history. This general lack of research in the field is the only thing that can explain why Volker R. Berghahn’s study in journalistic history has been able to find a home with such a prestigious publisher as Princeton University Press, even though it overlooks innumerable facts, ignores or

* Trans. Emily Richards (GHIL).
fails to notice important connections, and interprets history in a way that can only be called distorted.

Yet the book is concerned with only three protagonists: Paul Sethe, Hans Zehrer, and Marion Gräfin Dönhoff. To lump these three very different actors together under the heading of ‘Generation of 32’ (p. 217) is bizarre, a reductio ad absurdum of an otherwise potentially useful generation-based perspective on history. The disadvantages of such an approach, however, become only too clear when Berghahn generously grants all three of his protagonists a retrospective free pardon for their lives and activities in Nazi Germany: ‘Sethe, Zehrer and Dönhoff (though she was not yet a journalist) continued to keep their distance from the regime thereafter (1933)’ (p. 2). He is able to achieve this misinterpretation only through reliance on a concept that is itself questionable, the notorious idea of an ‘inner emigration’. According to Berghahn, his three chief actors all ‘went into inner emigration’. But this is not all; he goes as far as to claim that the Nazi regime posed a threat to each of them personally (‘Having lived, often quite dangerously, under the Hitler dictatorship to the bitter end’, p. 3). Happily, in the author’s view, they were ultimately spared to take up their fight for a new democracy after 1945: ‘They wanted to restore precisely those moral and ethical axioms that Hitler had so totally demolished’ (p. 3). Berghahn’s justification for this highly debatable interpretation of his protagonists’ careers is once again the concept of an inner emigration, which he defines as ‘best viewed as a spectrum along which they moved from a limited involvement with the regime to survive economically while continuing to reject Nazism, at the one end, to increasingly passive resistance and ultimately active participation in anti-Nazi movements, at the other end’ (p. 5).

It would seem that Berghahn originally intended to base his study on a much wider sample of journalists, publishers, and writers. I deduce this from his rather odd detour (pp. 11–24) into the lives of another mismatched trio (Ernst Jünger, Margret Boveri, und Henri Nannen). But even these shady figures of journalism cannot provide any convincing reason why we should believe in Berghahn’s ‘Generation of 32’; and none of them was an inner emigrant. Instead, their writings either helped the regime take power (in Jünger’s case) or successfully to hold onto it (Boveri/Nannen).

I do not want to discuss Marion Gräfin Dönhoff (referred to in the book frequently—and for a critical study, inappropriately—as
‘Marion’) in any great detail. Unlike Sethe and Zehrer, who died in 1966 and 1967 respectively, ‘the Countess’ (die Gräfin), as she was known in Germany, continued to be active in the world of journalism until 2002. During the Nazi period, she was able to continue living the life of a Prussian aristocrat, travelling widely and internationally, a life which was only briefly interrupted by a Gestapo interrogation after the Stauffenberg assassination attempt of 20 July 1944. All her life, she mourned the demise of Prussia, and especially in her later years became one of the keenest defenders of ‘the Prussian way of life’ and ‘Prussian values’—something that in itself would provide material for critical studies.

To return to Paul Sethe and Hans Zehrer, however: it is impossible to explain how Berghahn has arrived at his view of them as ‘inner emigrants’. He may have overlooked Peter Köpf’s work; he does cite, perhaps out of a sense of obligation, Otto Köhler’s book, although he clearly has not understood it. The facts that I present in the following are based on these two standard works of reference; they are facts, however, which inexplicably appear in Berghahn’s book only as marginal notes, if at all, or where they do appear in more detail, have been glossed over.

Until 1943 Paul Sethe was the political editor for the Frankfurter Zeitung (FZ). After the latter closed down in 1943, he moved to the Völkischer Beobachter (VB). After 1945 he spoke of having been ‘conscripted’ (‘dienstverpflichtet’) to work for the organ of the NSDAP. But even while ‘se’ (Sethe’s moniker at the FZ) was working for the ‘bürgerlich’ (that is, Nazified) FZ, his work consistently reflected the values of National Socialism. For the Easter 1942 edition, for example, ‘se’ wrote:

Today, Germans find themselves engaged in a struggle that has often been compared with the struggle of Frederick the Great. And rightly. At that time, it was no mere question of winning or losing, but of victory or the utter collapse of our culture—and today, similarly, it is a question of victory or utter destruction . . . But today, we are no longer merely fighting to ensure the continued existence of the state as we know it. Instead, we fight for a victory that will permit us finally to achieve our highest endeavours.
After the move to the *Völkischer Beobachter*, the tone of Sethe’s calls-to-arms based on supposed historical fact became even more extreme. But Berghahn, having issued his free pardon to Sethe, prefers to remain blind to the latter’s involvement in the Nazi regime: ‘Yet Sethe’s military journalism may also have been a protection, because it enabled him to avoid direct political comment’ (p. 47). An attempt to separate the military from the political, especially in the years of the Third Reich, demonstrates either naivety or a determination to gloss over the facts at any price. Indeed, Berghahn seems to be entirely unaware of Sethe’s later polemics in the *Völkischer Beobachter*: ‘It is likely that this kind of reporting extolling military successes continued up to the autumn of 1941’ (p. 47), while proffering extraordinary explanations for the closure of the *FZ* in August 1943: ‘Influencing international opinion was no longer important’ (p. 51), as ‘total war’ had made this unnecessary. But in fact, from this period on the Nazi regime actually ratcheted up its external propaganda, for example, in its anti-Bolshevist campaigns directed at Europe and the West generally. And it continued to have access to, and exert its influence on, the international public sphere until April 1945, not least thanks to its secret deal with Associated Press (AP).

In fact, Sethe’s seamless move from the *FZ* to the *VB* in 1943, like the careers of other National Socialist journalists (*Schriftleiter*), reveals the true nature of the latter in the Third Reich; they were propaganda specialists in the service of the state. Journalists belonged to an intellectual *nomenclatura*, providing an intellectual foundation for the regime’s work based on their own intellectual capital. The famous ‘Zwischen den Zeilen’ (‘Between the Lines’) column in the *FZ* was no exception; it was a cool calculation, intended to throw sand in the eyes of observers abroad and soothe critical voices at home even as the regime was coming to an end. And journalists enjoyed countless privileges. Men, in particular, benefited from one literally vital advantage; they were deemed to be in a ‘reserved occupation’, in contrast to their contemporaries sent to the front as cannon fodder. When we consider Sethe’s position at the time—an official in the NS propaganda machine, relatively remote from the dangers of the war—it is not only historically inaccurate to speak of an ‘inner emigration’, it is illogical. And to describe Hans Zehrer, whom Hellmut von Gerlach, writing in *Die Weltbühne* in 1932, once called ‘the Duce of *Die Tat’s* inner circle’, as an inner emigrant is not only historically
inaccurate, it is highly problematic, recalling Achim Mohler’s post-1945 project of creating a (morally purged) ‘Conservative Revolution’ intended to be clearly distinct from fascism.

It is true that in the 1920s Zehrer was employed at the liberal Vossische Zeitung, but he then took over Die Tat as its managing editor. Hans Paul Brunzel, who in 1952 submitted a dissertation about Die Tat and its inner circle, called the newspaper ‘an attack on the Weimar constitution in print form’. It embraced a hyper-nationalistic, anti-liberal ideology that supported the ideas of a Volksgemeinschaft and a form of German isolationism based on a rejection of the West. Writing in the Weltbühne on 22 November 1932 Carl von Ossietzky described Zehrer’s inner circle at Die Tat as ‘out-Hitlering Hitler’. Unfortunately for Zehrer, however, he had simply put his money on the wrong political horses when he chose to support Kurt von Schleicher and Gregor Strasser. His attempts to get on the right side of the winning National Socialist faction after the elections of 30 January 1933 failed. The Tägliche Rundschau, another newspaper taken on by Zehrer in August 1932, published a paean to Germany’s new chancellor in March 1933 (‘The destiny of Germany today has a name: Adolf Hitler’), but even this was in vain, as was his polemic against a Jewish ‘golden Internationale’ in April 1933 and his boasts that the new regime would ‘eliminate Jewish influence from key national positions’. When these attempts to curry favour proved unsuccessful, Zehrer retired to Sylt for five years, but remained in contact with like-minded colleagues such as Ferdinand Fried (aka Friedrich Zimmermann), whose career, like that of many others, flourished in the Third Reich. And when Zehrer later took over Die Welt, he took Fried with him. For on Sylt, Zehrer became acquainted with Axel Springer, who took Zehrer under his wing after 1945 and entrusted him with influential positions in his West German media empire.

In Berghahn’s narrative, however, Zehrer is said to have gone underground in a ‘hovel’ on Sylt, ‘where he survived on a meagre [sic] budget until 1945’ (p. 9). But Zehrer had not been forbidden to write, nor was he subject to any other restrictions imposed by the Nazi regime. It is simply not possible to describe his situation as one of ‘inner emigration’. In 1939 Zehrer was accepted as a member of the Reichsschriftumskammer, the Reich Chamber of Culture, a government agency established by Goebbels to control German art and culture in
conformity with NS ideals. He also became an editor at the Stalling publishing house in Berlin, the home of military journal *Die Wehr* and books about the Condor Legion, where he was soon promoted to managing director.

Rather than an ‘inner emigrant’ Zehrer can be far more accurately described as one of those who ‘paved the way for National Socialism’ (‘ein Wegbereiter des Nazionalsozialismus’), as he was in fact described by Rudolf Küstermeier, a concentration camp survivor who was editor-in-chief of the Springer newspaper *Die Welt*, in a letter written in 1958. The ‘whitewashing’ of Zehrer after 1945 on the part of the Protestant church in Germany and its bishop Hans Lilje is an area which has not yet been researched in as much detail as one could wish, including from a modern church history perspective.

Sadly, what Volker Berghahn has produced can only be termed a misleading study. He references the existing literature only in part and his reading of it is fragmentary. He seems to be unaware of the most recent research publications, for example, Dirk van Laak and Dirk Rose’s conference proceedings *Schreibtischtäter: Begriff – Geschichte – Typologie* (2018). Sethe and Zehrer may both, after 1945, have seen their work as a ‘quest’ to help with the ‘moral reconstruction’ of post-Nazi Germany (p. 5). But their quest was certainly not for a liberal, democratic Germany in the sense that we understand it today. Such a Germany exists in spite of, not because of Nazi pen-pushing perpetrators like Sethe and Zehrer.

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Overviews of post-1945 German history are now so numerous that it can be extremely difficult, especially for students, to know where to start. The resources available include large-scale syntheses of twentieth-century German history, handbooks, and general overviews that are mostly intended for a student audience. This book belongs to the latter category.

It tells the story of both German states from the perspective of two US authors who have taught and studied twentieth-century German history, and is primarily aimed at students at Anglo-American universities, as can be seen from the exclusively English-language literature recommendations included at the end of each of the book’s thirteen chapters. Primary sources are supplied in the form of both images and texts (with primary texts framed within the main text to make it clear that this is what they are); these add the attraction of first-hand accounts to the narrative and could also be used as case studies for student seminars.

After a short summary of National Socialist society and a discussion of various interpretations of the Nazi era by contemporary German historians, the authors have chosen the unconditional surrender of the ‘German Reich’ as the starting point for their narrative. The main body of the book covers the history of West Germany (FRG) and East Germany (GDR) respectively as well as the shared history of the new Federal Republic of Germany from 1989/90 to 2017. The authors’ very knowledgeable depiction is mainly focused on political history, but also considers social and cultural aspects.

The overview is structured chronologically in three periods, starting with 1945–1970 (‘Dividing Germany’), before moving on to the period 1969–1992 (‘New Beginnings’) and finishing with the ‘Berlin Republic 1990–2017’. The authors have embedded the developments and events that form the basis for this chronological structure in their historical context, thereby justifying their choice of these particular dates positively; this means that their historiography, at least in the

Trans. Emily Richards (GHIL).
main body of the text, is not marred by an artificial reliance on years and decades simply for the sake of it.

But which history of Germany are the authors interested in? Do they offer their readers the post-1945 narrative of (West) German success that has come to hold the status almost of an axiomatic truth, or have they decided to do things differently? And how do they integrate the history of the GDR? Both these questions lead us to historiographical debates that show no signs of fading away. It would be interesting to know, for example, how this overview fits in with the recent critique by Frank Biess and Astrid Eckert of the ‘success story’ that has in the past been typical for West German historiography.¹ As Biess and Eckert both teach in the USA, published their critique in English, and seem to be unaware of similar older, German-language criticism of such ‘success narratives’, it is natural to ask whether the two current authors, whose work seems to be mainly based on English-language secondary sources, bring an equally critical approach to German historiography. But they do not. Caldwell and Hanshew are happy to reproduce the success narrative, at least for West Germany, for example, in such phrases as ‘the importance of the economic miracle to the short- and long-term success of the FRG’ (p. 73).

Besides this, however, they go so far as to view this narrative as continuing unbroken in the period after 1989/90, especially when it comes to Merkel’s chancellorship. Both authors seem to be enthusiastic fans of the current German chancellor, the only one out of all Germany’s post-war leaders to be given her own section within the book. Caldwell and Hanshew foreground her scientific expertise during the Fukushima catastrophe (p. 309) and support her actions during the refugee crisis of 2015 (pp. 341–2). That Caldwell and Hanshew believe the success narrative to be constitutive for Germany even today can be seen in the three ‘basic principles’ which they claim lie at the heart of German politics: first, the rejection of racism and the defence of the rule of law in the face of growing right-wing populist and nationalist movements; second, Germany’s refusal to expand its military capacity and reluctance to endorse or engage in international military interventions; and, finally, Germany’s central position within the European Union (pp. 347–8). Just as Biess, in his highly

praised monograph *Republik der Angst*, cannot overcome the power of the success narrative. Caldwell and Hanshew are ultimately unable to do this either, thereby acknowledging just how axiomatic it has become in German historiography.

This is understandable in the context of an introductory overview of post-1945 German history. However, it is still surprising that they should adopt this position so unconditionally given that in the book’s introduction, they explicitly identify the works of Heinrich August Winkler and Ulrich Herbert as success narratives and ask how it might be possible to write German history differently (pp. 13–14).

On the other hand, in contrast to many overviews, the GDR is here no mere ‘footnote to history’ as Hans-Ulrich Wehler once described it. The authors dedicate almost as much space to East German history as to West Germany, although this should not be taken to mean that they see the history of the two Germanies exclusively as an entangled one. Instead, they maintain a balance between the USSR’s influence (which should not be over-estimated) on East German politics and society, and the attempts of each side to differentiate itself from the other during the Cold War while, at the same time, each sought to instrumentalize the other for its own purposes. Sensibly, the authors are also careful to emphasize the differences between dictatorship and democracy; but unfortunately they restrict this explanation to an extremely short sub-section and only as a postscript to a lengthy description of political and social changes in the GDR in the 1970s. If they had chosen instead to foreground their explanation as an introduction to chapter 6 (‘New Social Republics, East and West’) this would have provided the reader with an important analytical tool for assessing these respective forms of society.

In fact, the authors generally situate their work within the current research landscape implicitly rather than explicitly – at least for West Germany. This is usual for overviews, yet it is conspicuous that they reference other research perspectives more explicitly when it comes to the GDR while omitting them almost entirely for the former FRG. It might have been worth considering introducing each chapter with an overview of current positions in research so that students are not only given a chance to become familiar with the content of contem-

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porary historiography but also with different approaches on the part of historians. Space could easily have been freed up for this by sacrificing some of the less relevant images in the book, for example the half-page depiction of Nina Hagen on p. 138.

But despite these critiques, the book’s use as an introduction to post-1945 German history outweighs its faults—and not only for students from English-speaking countries.

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Throughout her long career as a leading politician, beginning in the early 1990s, the current German Chancellor, Angela Merkel, has rarely reflected on her situation as a woman in politics, publicly at least. Yet she did so briefly in her 2019 Harvard Commencement Speech, where she highlighted that she was the first woman in German history to become head of government;¹ an expression, perhaps, of late-term liberties in office. From a scholarly angle, Joyce M. Mushaben, Professor of Global Studies and the Curators’ Distinguished Professor of Comparative Politics and Gender Studies at the University of Missouri-St Louis, investigates, among other things, Merkel’s unique status as the first female German Chancellor in her most recent book. This is a not a biography but a well-written and well-informed study of Merkel’s domestic and international policies during her first three terms in office, combining policy analysis and its context, that is, Merkel’s personal values along with national traditions and beliefs.

There are several persistent clichés attached to Merkel’s career, the most common being that as a woman raised in the former German Democratic Republic (GDR), she is incapable of leadership. Mushaben mentions it, but does not stop at this superficial observation. Instead, she uses it as a starting point for her study, analysing the related stereotypes and prejudices as symptoms of the power strategies and media campaigns Merkel has been involved in. She asks the right question, which is ‘how could someone so successful be accused of “not leading”?‘ (p. 6). Instead of reinforcing these stereotypes of a female politician from the GDR—still viewed differently—as other allegedly scholarly accounts have done in the past,² Mushaben sets out to analyse the political use of these stereotypes and the means they employed. In this way, she contributes to an intersectional gender analysis of contemporary politics and, to a less-

² Gertrud Höhler, Die Patin: Wie Angela Merkel Deutschland umbaut (Zurich, 2012).
er extent, to a history of political thought and values in Germany since unification through the lens of the political career of a central individual, a ‘one-woman laboratory’ (p. 2) as Mushaben calls it. By taking this approach, the author broadens our understanding of current German debate and Merkel’s policies alike.

Having said this, it is a pity that Mushaben walks right into another trap, which has to do with a second cliché regularly attached to Merkel’s way of doing politics: the widely held view that, having trained as a scientist in her first career, she must therefore apply a physicist’s approach to politics. Mushaben misses the chance to deconstruct this notion in the same way as the gender cliché. Instead, she falls back on it as a valid explanation time and again.

Mushaben has nevertheless written an intelligent book which offers a number of original and thought-provoking interpretations. We do not have to agree with all of them, but they have the potential to stimulate further debate. That is why it is worth citing some examples here, in the order in which Mushaben presents them.

The book consists of three parts, each of which contains several chapters. The first part (‘The Personal is the Political’) investigates the ways in which Merkel’s personal life has affected her policies, and how far politics in turn has influenced her as an individual. Mushaben addresses Merkel’s physical and political makeover on her way to the top and her change of attitude towards political reform; how she deals with East and West German identities; and morality and historical responsibility in her relationship with Israel. Mushaben’s observations include the fact that before her first term as Chancellor, Merkel got a ‘new haircut, highlights, make-up, and pastels’ (p. 33), paradoxically, in order to have ‘a gender-neutral campaign’ (p. 34) then planned for her. Also, she makes the point that ‘women’s strong presence in the Red–Green government (50 percent in 2004) granted Merkel a legitimacy in the Grand Coalition she might not otherwise have enjoyed’ (p. 37). In terms of German identity, Mushaben believes that it was not Merkel’s grasp of Western power politics that helped her; rather, ‘her ability to effect a gradual reconciliation of conflicting identities owes more to her pragmatic Protestant upbringing’ (p. 60). Merkel has, as Mushaben notes, helped West Germans in particular ‘to see that differences are normal, non-antagonistic, necessary, and even welcome in a pluralist democracy’ (p. 77). At the same time, she makes the interesting observation that
‘Merkel never criticized the Westerners’ erroneous belief that they alone financed reconstruction’ after unification (p. 68). Merkel also achieved progress on gender issues: ‘earlier hostility toward the patriarchal state is fading insofar as Merkel’s reforms have produced real changes in the rights and responsibilities of men, for example, paternity leave’ (p. 71). And for relations with Israel, Mushaben offers a very interesting comparison between Merkel and the former Green Minister for Foreign Affairs, Joschka Fischer (pp. 97–104), before highlighting that ‘Merkel has already received more awards for her commitment to Jewish culture and the Israeli state than any of her predecessors’ (p. 97). It is part of Merkel’s policy for Israel, Mushaben believes, that she combines ‘unambiguous acknowledgement of historical responsibility’ with ‘recognition of German suffering’ (p. 105). Merkel also ‘walk[s] a fine line between trade relations and historical responsibility’ (p. 107).

Part two (‘From Understudy to Leading Lady: Angela Merkel on the Global Stage’) investigates international policies, that is, in this case, Merkel’s handling of Russia and the Euro crisis. Mushaben highlights that Merkel ‘recognizes that German responsibility for securing the peace now extends beyond regional boundaries’ (p. 127). However, she also finds that Merkel, born ‘after the Korean War and forced to turn eastward as a GDR citizen . . . never developed the emotional tie to France evinced by her predecessors, nor did she inherit an intuitive understanding of the European Community’ (p. 126). Merkel’s foreign policy ‘supports Germany’s traditional multilateralism and its culture of restraint but she also enjoys a reputation across Europe as an honest broker, to a degree not seen among her predecessors’ (p. 150). A reason for this might be that ‘Merkel searches for flexible win–win options’ (p. 151) and that negation for her ‘is not a matter of hard or soft power; it is merely a rational approach to meliorating complex problems’ (p. 155). Mushaben is much more critical, even disappointed, when it comes to the Euro crisis, as she explains:

this chancellor has pursued a pragmatic, progressive approach to women’s employment in her own country by leveraging EU policies in relation to work–family reconciliation. She even chose an iconic female figure, the Swabian housewife, to convey the need for personal economic responsibility during the
first stage of the European crisis. She demonstrated no visible interest in upholding broader EU gender equality mandates in relation to the austerity programs that followed, however (p. 188).

The difference between domestic and foreign policy becomes obvious: ‘Once the Euro crisis had crested, Merkel nonetheless overrode opponents back home, introducing a three-pillar 30 percent quota for women in corporate and public service management’ (p. 197). Regarding EU institutions Mushaben’s judgement is also definite:

As the [financial] crisis unfolded, it became clear that Europe not only had a deficit/debt problem but also deeper governance problems. Merkel came to prefer a ‘stability union’ resting on a common approach to financial policy, fiscal policy, and economic policy, along with greater ‘democratic authority’. The paradox is that she has advanced the first three by undercutting the fourth (p. 198).

Part three (‘“Method Merkel” and the Push for Domestic Reform’), finally, turns back to domestic issues such as the national energy turnaround and refugee policies. Mushaben makes the point that it was Merkel’s ‘“leadator” stance on climate-change mitigation’ that ‘brought new intensity to EU efforts regarding CO₂ reductions and RE development’ (p. 238). She highlights that Merkel ‘made climate change a personal priority’ (p. 241), partly because as ‘a politician in a democratic society, she quickly learned that mass protests and public opinion matter’ (p. 242). In terms of migration, Mushaben sees Merkel as a Chancellor who ‘has demonstrated her strongest leadership abilities in the very arena that triggered the most vociferous opposition within her own party’ (p. 283). Again, it was her ‘quick mastery of EU processes [that] . . . accorded her unprecedented influence over policy-framing at both the national and the supranational level’ (p. 251). Mushaben also credits Merkel with a modernization of the substance of migration policy: ‘Merkel’s stress on inclusiveness, transparency, and self-accountability means that integration failure can no longer be randomly attributed to ethnic groups’ (p. 265).

All the empirical chapters start with several sections of contextual information, which are well-chosen and helpful for understanding
Mushaben’s argument. However, they tend to overshadow the discussion of Merkel’s actions. This is because the background is not always directly connected with Merkel’s actions, yet in more than one case takes up a significant part of the individual chapters. A prime example of this is the chapter on foreign relations with Russia (ch. 4), where two-thirds of the chapter explains the background for Mushaben’s reading. This is unbalanced, even for an international audience.

Mushaben presents an account focusing on policy outcomes, which are correlated specifically with Merkel as a female leader and her GDR background. This is convincing and makes a valuable contribution, as we lack such an account so far, even in German. What is not at the centre of the discussion, however, is the way Merkel reaches her decisions, that is, how she governs at the micro level of everyday power politics. Mushaben, of course, hints at this when she mentions other players and the strategies of other political forces and rivals, such as the rejection by Conservatives in her own party of the guideline powers (Richtlinienkompetenz) encoded in the German Basic Law. Mushaben also occasionally mentions Merkel’s techniques of decision-making. But all of these aspects are generally only mentioned, not analysed. As a result, the mechanics of Merkel’s power to a large extent remain in the dark. Mushaben tells us why the Chancellor favours a particular decision, and informs us of the outcome, with a very knowledgeable reading of how both ends are linked. What is lacking, however, is what comes in between, that is, how Merkel conducts politics and deploys power strategies.

How important, for example, is her tactic of delaying decisions, her decidedly non-confrontational style of arguing, and her tendency to keep quiet about her political opponents? How important is her way of not explaining her politics in securing her power? What about her approach of delegating the process of finding compromises to commissions and round tables seen, to mention just one of many cases, during the energy turn-around after the Fukushima meltdown; her collective response to international crises (for example, including the French president in the talks about Ukraine); or her attempt to globalize European politics by bringing in the World Monetary Fund to deal with Greece, or delegating the implementation of Germany’s immigration policy to the EU deal with Turkey and African states? How exactly does she deal with opponents in her cabinet and else-
where, and what are her strategies for forcing her opinions through? What about her tendency to isolate herself from the lowland of politics, representing herself as a President-Chancellor, and her use of internal communications and press conferences? Or her willingness to become acquainted with all the details of specific questions and solutions, her ability to control emotions, her capacity to build alliances across traditional party lines, and her ability to resist stress under pressure and in times of national and international crisis?

These are the sorts of questions that remain open, even after Mushaben’s intriguing book. There are two brilliant accounts, written by journalists, which deal with some of them, taking the debt crisis and what has become known as the ‘refugee crisis’ as case studies. But a comprehensive, scholarly investigation of Merkel’s power dynamics, her negotiating tactics, and her strategies for remaining in power is still to come.


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