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Looking beyond waves and datasets: “cultures of terrorism” and the future of history in terrorism studies

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ABSTRACT

Frequent observations about the lack of historical work in terrorism studies pose questions about history’s place – and its relevance even – in the field. Why have historians largely failed to engage with terrorism studies? And why have terrorism studies scholars generally failed to engage with history? This article suggests that the dominance of social scientific research methods, the quest for “rules” and models, and the prizing of quantitative data in terrorism studies, combined with historians’ own reluctance to foreground the contemporary relevance of their work, have led to the underrepresentation of history. It calls on historians to demonstrate the value of qualitative research methods to notions of contemporary terrorist violence using a “cultures of terrorism” approach in the context of Critical Terrorism Studies.

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It has long been a truism that history and terrorism studies¹ do not mix. It is more than 40 years since Walter Laqueur published *Terrorism*, the first major English-language synthesis of the subject; by and large it appears that historians have not followed Laqueur’s lead (Laqueur 1977a). In 2019, Bart Schuurman’s review of “trends and gaps” in terrorism studies journals between 2007 and 2016 listed “historical approaches” to terrorism as an “un- or understudied topic” (Schuurman 2019). Brian J. Phillips’s 2023 survey of scholarly articles on terrorism found that between 1970 and 2019, just 2.6% of authors were historians (Phillips 2023). This should give pause for thought given that the phenomenon is broadly recognised to be at least 150 years old; there is much ground that historians could cover. Schuurman and Phillips were not the first scholars to note historians’ apparent lack of engagement with terrorism: in 1983, David C. Rapoport stated that “[t]here is no authoritative history of modern terrorism” (Rapoport 1983, 672). Seventeen years later, Laqueur introduced the 2001 edition of *A History of Terrorism* with the observation that “[t]he history of terrorism goes back a very long time, but the very fact that there is such a history, has frequently been ignored” (Laqueur 2001, xxv).

Social scientists have traditionally dominated terrorism studies. Andrew Silke’s 2004 survey of leading terrorism studies journals during the 1990s found that 48.6% of authors (according to their published biographies) were political scientists, while just 4.2% of authors were historians. Political scientists had authored more than half of all published

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works in these journals, producing 10 times as many outputs as historians (Silke 2004). Historians' contributions were likewise small in the immediate aftermath of 9/11, when publications on terrorism otherwise proliferated. Between 2002 and 2004, less than two percent of articles in the field's leading journals dealt with historical terrorism (Ceci 2016). The surge in publications in the first decade of the twenty-first century did little to change this situation; between 2000 and 2010, the number of historical journal articles on terrorism remained miniscule in comparison with those of political scientists, psychologists, and economists (Phillips 2023). In research terms, the events of 11 September 2001, seemingly left historians unmoved.

In terms of historical work, authors have continued to note a dearth in quantity and, in some cases, quality: in 2004, for example, Rapoport noted that “[n]o *good* history of terrorism exists” (Rapoport 2004, 68n3 [author's italics]). In 2007, Isabelle Duyvestyn lamented that “[t]errorism seems to be a subject with almost no history” and that “[t]he foundations of terrorism studies in historical research are not very deep” (Duyvestyn 2007, 62 and 67). In 2009, Colin Wight concluded that “terrorism research post-9/11 [has] an almost complete lack of historical awareness” (Wight 2009, 103). Rapoport's statement on there being “no good history of terrorism” reappeared in his contribution to Jussi M. Hanhimäki and Bernhard Blumenau's 2013 collection of essays on historical terrorism (Rapoport 2013, 302–4). In 2014, his preface to Richard Bach Jensen's history of counter-terrorism at the turn of the twentieth century claimed that “we know so little about the history of modern terrorism” (Jensen 2014, xiii). In 2019, John A. Lynn III likewise lamented that studies of terrorism “do not provide enough of the actual history” (Lynn III, 12). Dietze's 2021 account of the mid-nineteenth century origins of terrorism purports to treat a subject on which “[g]enuine historical research ... is rare” (Dietze 2021, 12). These examples represent just a handful of comments on the perceived lack of historical work in the field. Meanwhile, as Richard English has noted, academic research centres in the English-speaking world are geared towards the study of contemporary terrorism or associated concepts such as radicalisation (English 2021). It is true that these centres may include historians – for example, the Centre for the Study of Terrorism and Political Violence at the University of St Andrews – but institutional research bodies focused exclusively on the *history* of terrorism do not seem to exist.

Recent state-of-the-field reviews indicate that historians have ostensibly engaged little with the subject. Phillips's data reveals that terrorism is not a frequent subject of research in history journals: during 1970–2019, these publications published just 2.2% (151/6,880) of articles on terrorism. According to Phillips, the most prolific historians of terrorism are Laqueur (twelve articles on terrorism), Jensen (eight articles), and English (four articles). Of these authors, Jensen has contributed most articles to terrorism studies journals (English has contributed one, and Laqueur none) (Phillips 2023). The failure of historians to publish in these journals has diminished history's visibility in the field. The problem for historians lies in the fact that those interested in terrorism may peruse the contents of the latest issue of *Terrorism and Political Violence* or *Critical Studies on Terrorism* but fewer are likely to consult the pages of *Past & Present* or the *Journal of Contemporary History*. That being said, research oriented towards terrorism studies has not ignored historical investigation entirely; there are books that one could locate squarely within the field that do consult historical writing. Marc Sageman's *Turning to Political Violence: The Emergence of Terrorism* (Sageman 2017) is a good

example. In this book, Sageman (who has a PhD in Sociology) delves deep into the history and historiography of terrorism since the French Revolution. There are other works in terrorism studies, produced by non-historians, that draw on historical scholarship (for example Heath-Kelly 2019; Ditrych 2014).

Disciplinary publishing traditions may explain the relative absence of historical articles on terrorism. The monograph is the gold standard for research publications in history. Historians have produced book-length works on terrorism. A cursory glance at the bibliographies of histories of terrorism, from Randall D. Law's updated 2016 textbook (Law 2016) to Crossland's 2023 monograph on nineteenth century terrorism (Crossland 2023), demonstrates that historical work on terrorism does exist. However, relatively few historians take historical terrorism as the *principal* focus of their investigation. They tend instead to orient their work towards the histories of specific periods, nations, or regions, and their work speaks primarily to the corresponding historiography and historians. Terrorism may feature only tangentially: Charles Townshend's works on Ireland between 1885 and 1925 doubtless contain much that is relevant to the history of terrorism, yet this form of violence is not the author's primary concern (Townshend 2014 and 2021). Consequently, if historians *have* engaged with terrorism, they have not engaged in great number with *the field of terrorism studies*, neither publishing in its principal journals nor orienting their work specifically to its research questions (there are exceptions such as Jensen).

Historians' outputs that concern terrorism, or aspects thereof, are simply less visible to those working in terrorism studies. In the case of France, John Merriman has authored a brace of works on turn-of-the-century anarchist terrorism, while Gayle K. Brunelle and Annette Finley-Croswhite have published two books on the "Cagoule", an extreme right-wing terrorist group that operated in the country during the 1930s and 1940s (Brunelle and Finley-Croswhite 2012, 2020; Merriman 2009, 2017). The impact of such works on terrorism studies is limited: neither *Terrorism and Political Violence* nor *Critical Studies on Terrorism* have reviewed these publications. Broader histories may have more influence beyond those centred on national historiographies: among recent works are Jensen's 2014 account of nineteenth- and early-twentieth-century efforts to combat anarchist terrorism, Martin A. Miller's reflection on the origins of terrorism (Miller 2013), Lynn's (2019) survey of the history and forms of terrorism, (Dietze's 2021) analysis of the 'first terrorists' in the 1860s, and James Crossland's (2023) analysis of fear and nineteenth-century terror. Meanwhile, new collections of essays and handbooks have taken historical terrorism as their focus (for example, *The Routledge History of Terrorism* [Jackson 2015]; *The Cambridge History of Terrorism* [Cox 2021]; *A Transnational History of Right-Wing Terrorism* [Johannes and Moritz 2022]). Nonetheless, historians neither authored the 2019 *Oxford Handbook of Terrorism's* chapter on "historical approaches" to terrorism nor the 2018 *Routledge Handbook of Terrorism and Counterterrorism's* chapter on the "history of terrorism".

While it may appear that historians' reluctance to engage with historical terrorism is ebbing, this article contends that hopes for a "historical turn" and an "age of the history of terrorism" (Ceci 2016, 896) in *terrorism studies* are premature. The broader field's long-established approach to the history of the phenomenon – a compartmentalised method inspired by Rapoport's "wave model" – as well as its preference for quantitative research methods more familiar to the social sciences,² have rendered historical work ostensibly

irrelevant. These obstacles may account for historians' relative lack of interest in terrorism studies – and terrorism studies' relative lack of interest in history.

The situation can be rectified. English has explained eloquently the contribution that historians can make to terrorism studies, from their consideration of a variety of aspects of a topic based on a broad range of sources to their appreciation of context that is essential to an understanding of terrorism, both at a given moment in time and over the course of years and even decades (English 2021). English advocates the engagement of historians with contemporary debates about terrorism primarily through public history; that is, with audiences beyond the academy, reachable through the writing of books, and advisory roles concerning memorials, museums and acts of remembrance (English 2021).

I am less concerned than English with historians' direct influence on public debates or policy. Public history and engagement with policy is not for all historians. Constraints may be practical: as a subject, terrorism may be considered to have public appeal but the generally narrow bounds of one's own research may not tempt a readership or publisher; writing for the public requires different skills to writing for the academic market and historians are not as a rule trained to write in such a way. Some scholars may perceive intellectual problems with public history or policy work especially with regard to a topic as sensitive as terrorism: Ann Larabee has already warned about producing academic work that serves state agendas (Larabee 2011). I seek here to offer a means for historians to engage more fully *with the academic field of terrorism studies*, while bringing their work to bear on research into more recent terrorist phenomenon. Building on approaches to the history of violence, as well as pioneering work in Critical Terrorism Studies (CTS), this article proposes a cultural approach to the history of terrorism that draws on the qualitative research skills of historians. This approach to "cultures of terrorism" may allow historians to enter and influence the field because it permits them to demonstrate more fully the relevance of the past to the present.

The article's point of departure is Rapoport's seminal "four waves of modern terrorism" model. This model accounts for the historical development of terrorism from the 1880s to the present. Rapoport first mooted the idea in 2001, elaborated upon it in 2004, and developed a book-length version in 2022. The model has exerted great influence over attitudes and approaches to the past in terrorism studies. It does not, however, encourage engagement *with the past*. In fact, the reverse is true: its (relatively) neat compartmentalisation of historical terrorism into successive yet largely disconnected waves allows the past to remain in the past, without troubling the present. While the model has turned the gaze of terrorism scholars fleetingly to history, its usefulness to the historical study of the phenomenon is uncertain.

The second part of this article looks beyond the wave model at the deeper causes for history's underrepresentation in terrorism studies. A close reading of terrorism studies' scholarship – particularly that which addresses research methods and the state of the field – reveals a tension between dominant social scientific approaches, grounded in quantitative methods, and the methods inherent to historical work, namely the qualitative analysis of data. Schuurman's 2020 survey, for example, posited the use of quantitative and statistical methods as an *objective measure of quality* for terrorism studies research (Schuurman 2020). This judgement echoed Silke's 2004 comment that high-quality terrorism research should rely on, "distinct and quantifiable data", in contrast to "qualitative and journalistic approaches" (Silke 2004, 11–12). New research into historical

terrorism using social scientific methods purports to improve the “sophistication” of the field through the collection of statistical data that will facilitate the “ability to generalize terrorism research across time” (Tschantret 2019, 934). Overall, this situation presents a challenge for the study of historical terrorism, not least because statistical datasets do not exist for the period prior to the 1960s. Moreover, while historians do not reject quantitative research methods *per se*, their reliance on qualitative methods means that the production of these datasets is not a primary concern. It is further unlikely that a historian will study the past to develop a theory that can be generalised beyond a specific context. For the historian, the specificity of a period is essential for understanding behaviour within that period; the search for a “rule” or pattern is not on their agenda. Historians’ work may therefore fall short of the subjective measures of quality that the field’s gatekeepers prize so much.

The final part of this article asks how historians can make a meaningful impact upon terrorism studies without sacrificing either their discipline-specific skills and preferences or the complexities and nuances of the past. To do so requires the historian to demonstrate the relevance of their work to scholars of contemporary terrorism using qualitative techniques. Historians can achieve this through investigating historical “cultures of terrorism”. Cultures of terrorism are the frameworks of values and qualities that informed – and inform – common beliefs about the nature, operation, and goals of terrorism and its perpetrators. This approach draws on work in CTS, namely that of Joseba Zulaika and William Douglass, and Richard Jackson, which recognises that “terrorism” is a social construction that draws its meaning from “cultural premises and discursive strategies”, rather than from “reality” (Zulaika and Douglass 1996, 6). Historians, in examining past cultures of terrorism, can reveal the long-term roots of contemporary understandings of the phenomenon. They can trace the discursive evolution of these cultures from their origin to the present. They can thus demonstrate the historical continuities and adaptations inherent to our own perceptions of terrorism and its perpetrators, identify recurring themes and actors within the discourses of terrorism, and render us more sensitive to contemporary and future applications of the label, and the agendas behind this labelling process in the present.

Rapoport’s ‘four waves of modern terrorism’ model

Rapoport’s four waves model is the best-known framework for understanding the history of terrorism (Parker and Sitter 2016; Rapoport 2001, 2004).³ The model locates the origins of modern terrorism in 1880s Russia. Rapoport divides the subsequent 140 years into four waves of terror. He defines a wave as a “cycle of activity in a given time period”, with each cycle “characterized by expansion and contraction phases”, growing, swelling, and dissipating, like a wave (Rapoport 2004, 47). Each wave has a “common predominant energy” and it draws its name from this “energy”: the anarchist wave (1880s–1920s); the anti-colonial wave (1920s–1960s); the New Left wave (1960s–1980s) and the religious wave (1979–present) (Rapoport 2004, 47). A wave lasts for approximately 40 years or the lifespan of a generation, coming to an end when the “dreams inspiring parents lose attractiveness for children” (Rapoport 2004, 48). The model allowed Rapoport to predict that the religiously-inspired terrorism of the 9/11 attackers would end in about 2025.

The influence of Rapoport's model is striking. Firstly, critics have sought to modify the model but not to refute its basic premises. Most often, scholars propose the inclusion of an extra wave or waves, for example, that of a right-wing wave (Auger 2020; Collins 2021; Hart 2021). On the other hand, Dietze proposes the insertion of both an "abolitionist wave" and a "national-democratic wave" in the mid-nineteenth century (Dietze 2021, 211). Secondly, authors have used the model to predict the beginning of future waves of terrorism. Recent acts of extreme right-wing terrorism, as well as the approach of 2025 (the predicted end of the fourth wave), have led some to suggest that a new wave is in the offing (Collins 2021). Finally, the wave model has influenced historians who have adapted their own work accordingly, even if they do not follow Rapoport's model per se. Thus, Lynn identifies three waves of "radical terrorism" (defined as violence perpetrated by revolutionary sub-state groups), during 1848–1920, 1945–1980, and 1980–present (Lynn 2019, 13). Lynn broadly agrees with Rapoport's definition of a wave; this leads him to dismiss entirely the period between 1920 and 1945, when expressions of terrorism were "essentially self-contained, [and] lacking the international or transnational interrelationships characteristic of a 'wave'" (Lynn 2019, 14). He therefore addresses neither extreme right-wing terrorism between the wars nor resistance or partisan movements during the Second World War. The omission of the latter is perplexing given that Lynn defines terrorism as a low intensity form of warfare in pursuit of political goals (Lynn 2019, 15–17). It is worth noting here that Rapoport likewise ignores interwar fascist and wartime resistance terrorism.

Rapoport's detractors have questioned his sense of history, proposing alternative methods for categorising outbreaks of terrorism (Parker and Sitter 2016). Dietze points out that the wave model (as presented in the author's shorter works) lacks analysis of specific historical situations or events, a fact that renders the theory plausible but not valid (Dietze 2021, 12). While Rapoport's 2022 book affords him the opportunity to engage more deeply with history, his wave model still does not deal well with ostensibly anomalous historical evidence. Rapoport does allow for some deviation from each wave's predominant energy: nationalist organisations, he explains, were present in all waves. Consequently, "[e]ach wave's name reflects its dominant but not its only feature" (Rapoport 2004, 47). Nonetheless, Rapoport can be remarkably forthright in his assertions: "All groups in the Second Wave [1920s–1960s] were nationalist but we named the wave 'Anti-Colonial' because colonial powers were the enemy – an enemy that had become ambivalent about retaining their colonial status" (Rapoport 2013, 283). He has little to say on the most significant example of 1930s European terrorism: the 1934 assassination of King Alexander I of Yugoslavia by the Croatian Ustashe, which led to the first international convention on counterterrorism. This act of violence sits uncomfortably in the "anti-colonial" wave, as defined by Rapoport. Certainly, we could at a stretch classify the killing as "anti-colonial": the Ustashe sought independence for the Croat people from what it described as "foreign" Serbian subjugation within the state of Yugoslavia. However, Belgrade was far from ambivalent about retaining control of Croatia. Furthermore, during the 1940s, violence against Occupying powers in the Second World War could also count as a form of anti-colonial terrorism. Yet Rapoport's anti-colonial wave concerns only opposition to the European control of *overseas* territories, mainly after 1945.

The model reveals itself to be rather inflexible. Evidence that does not conform is ignored or explained away. Rapoport is a hostage to this thinking: he expresses

scepticism that extreme right-wing attacks since 2010 amount to the beginning of a fifth wave because in previous waves of terrorist violence, activists have been young (mainly in their twenties), whereas recent right-wing attackers are generally older. He further adds that these recent attacks have “emerged during the Fourth Wave’s last decade, a process that has never produced a wave before” (Rapoport 2022, 305). The implications are that all waves must follow the same pattern and that a new wave can begin only after 2025 - because that is what the model states. Twenty years after its first elaboration, the model still dictates the relevance of the evidence, rather than vice versa.

Regarding terrorism studies’ relationship with history, Rapoport’s model does not encourage engagement with the past. In fact, the notion of waves suggests a discontinuous history of terrorism that has little bearing on the present. Certainly, the theory gives the *impression* of continuity between contemporary terrorism and its antecedents, not least because it seems that something called “terrorism” has existed for more than a century. Furthermore, Rapoport explains that in each period terrorists aimed to bring about “revolution”. He hints, too, at some other aspects of continuity: the end of a wave generally overlapped with the beginning of its successor; tactics could persist across waves, for example, the preference for assassinations seen in the anarchist and New Left waves. Rapoport further allows for the fact that groups, such as the IRA, could persist beyond the lifetime of a wave. At first glance, it looks as if the study of contemporary terrorism can therefore draw lessons from the history of the subject.

Beneath the surface, discontinuity is the order of the day: as Rapoport explains, discontinuity is evident in the language used by terrorists, their strategies, their choice of weapon, and their targets. Rapoport is frank about this in his 2022 book: “[e]ach wave had distinctive features and employed different tactics” (Rapoport 2022, 4); “[e]ach wave had its own geography” (Rapoport 2022, 4); “[e]ach wave had its own special tactics” (Rapoport 2022, 273). In essence, the model compartmentalises the history of terrorism, with little seeming relationship between waves. Karen Rasler and William R. Thompson note this feature, too: “The central motivation for terrorism in each wave is distinctive, as are the tactics that are most likely to be employed. . . Each wave is likely to play itself out and to be replaced by a new wave of terrorism that is centred on a motivation which is as difficult to predict as the timing of the next upsurge” (Rasler and William 2011, 17). The character of one wave therefore gives little clue to that of the next. Steven M. Radil and Jaume Castan Pinos likewise interpret Rapoport’s model as one that identifies “unique characteristics regarding the dominant motivations and tactics groups used” in each wave (Radil and Pinos 2022, 312). Historians Heinz-Gerhard Haupt and Klaus Weinbauer draw similar conclusions: “there is no continuous or unitary history of terrorism. The different forms of terrorism . . . had little in common with each other, and owed little to the inspiration of preceding examples” (Haupt and Klaus 2011, 208).

The problem for the historian seeking to make their work applicable to terrorism studies is that if terrorist waves are generally self-contained phenomena then their contemporary relevance is severely diminished. Rapoport’s approach is perhaps unsurprising in a field whose “presentist” bias is frequently noted, and best encapsulated in the post-9/11 “New Terrorism” thesis. Indeed, historians of terrorism can find themselves beholden to a perceived irrelevance to the contemporary world: Hanhimäki and Blumenau prefaced their 2013 collection of essays with the plea that, in spite of the

changing nature of terrorism, “it is still possible to learn from past experiences, even though they are not directly applicable to the present” (Hanhimäki and Blumenau 2013, n. p.). The book appeared in a series edited by Rapoport.

In 2007, Duyvestyn’s call for a better incorporation of historical approaches into terrorism studies contained an implicit attack on models such as Rapoport’s. She maligned the social scientific “quest to uncover general laws about human behaviour” and dismissed their “rough descriptions of time periods and typologies” (Duyvestyn 2007, 54). Such approaches could also lead to a “macro-historical perspective on terrorism” that involved generalisation and compartmentalisation (Duyvestyn 2007, 55–6). Rapoport’s “four waves of modern terrorism” is one such model. It has erected a barrier between the present and the past and allowed – or prompted – terrorism studies’ disengagement from history. As Duyvestyn observed, the origins of the model – and model-making in general – lie in a broader difference of approach between history and the social sciences. This difference has further complicated and obstructed the relationship between history and terrorism studies. This article now turns to this clash of research cultures.

History and terrorism studies: a clash of research cultures

As the quotations that opened this article indicate, for nearly 40 years scholars have noted the paucity of historical approaches to terrorism. Concomitantly, reviews of the field have mentioned a disproportionate focus on recent manifestations of terrorism: Schuurman (2019), for example, observed that the “field’s almost singular focus on [contemporary] jihadism remains in place” (Schuurman 2019, 13). Terrorism studies’ research priorities, extrapolated from Alex P. Schmid and James J. Forest’s two lists of “un- and under-researched” subjects, likewise tend towards the concerns of the present. The first list, authored by Schmid in 2011, was intended to offer graduate students ideas for their research. It listed 50 subjects, from the role of new technologies in terrorism, to recent (mainly Islamic) forms of terrorist violence, and the counter-terrorist efforts of contemporary governmental and non-governmental entities. Explicitly historical topics did not feature (Schmid 2011). The second list, authored by Schmid and Forest in 2018, offered an even better indication of a presentist bias. To compile the list, Schmid and Forest consulted a broad range of interested parties: “members of the Editorial Board and Advisory Board of *Perspectives on Terrorism* - and a few other colleagues from the scholarly, policy and practitioner communities” (only one of whom was trained as a - historian).⁴ In the updated list of 150 subjects, only one entry made particular reference to history (“Learning from the past? The role of historical analogies in counter-terrorism”). This suggestion implied that the past was useful to the extent that it could be used in the service of the present (Schmid and Forest 2018, 74). We cannot say that terrorism studies is entirely uninterested in history and much historical content in the field goes “unnoticed”. Chapters in the 2018 *Routledge Handbook of Terrorism and Counterterrorism* devoted to terrorist groups and national case studies contain a good deal of historical context.

Of course, for historians, the past is more than just the context to the present; it is the object of study. In this regard, the extent to which terrorism studies scholars’ interest in history extends to more than a mere “glance over the shoulder” is questionable (Cox 2021, 575). A case in point: in the *Routledge Handbook of Terrorism and Counterterrorism*, editor

Silke pointed out that Leonard Weinberg's chapter on historical terrorism was "rightly the longest in the volume" (Silke 2019, 6). In 20 pages of text, Weinberg devoted just over four pages to terrorism before 1945 (Weinberg 2019). The chapters that covered the historical context to different terrorist episodes (mentioned above) rarely looked back further than 1945.

If the presentist focus of much terrorism studies' research suggests a lack of interest in history, we should ask whether historians themselves are uninterested in terrorism studies. Why have historians not been moved to study terrorism in greater numbers? In 2011, Larabee cited the political sensitivity of the topic in her warning that historians should not enter lightly into the area for, in "developing a historiography of terrorism" without the necessary precision over "definitions, classifications, and origins", they risked serving as the accomplices of domestic counterterrorist forces (Larabee 2011). Perhaps for this reason, historians who have tackled historical terrorism have rarely proffered conclusions applicable beyond their period or context (Gage 2011, 83).

I suggest that the principal reason for the relative absence of history from terrorism studies is a fundamental difference in research methods between social scientists – who lead the field – and historians (or humanities scholars more broadly). Approaches even differ over the fundamentals of research. Take sources: for a historian, a primary source is a piece of "evidence contemporary with the event or thought to which it refers" (Tosh 2010, 91). Historians base their research on these sources that are "closest in time and place to the events in question ... [because] [t]he historian is often as much interested in what contemporaries *thought* was happening as in what actually happened" (Tosh 2010, 91–2). When terrorism studies scholars bemoan a lack of primary research in the literature, the historian may assume that this means a lack of engagement with evidence. Not so: Schuurman states that "newspaper articles are considered a secondary source of information about terrorism and terrorists, but become a primary source when the research focuses on how media reports on terrorism" (Schuurman 2020, 1015). Furthermore, he expresses considerable scepticism about the use of newspapers to gather information on terrorism and terrorists (Silke shared this scepticism in 2004).

Schuurman's comments prompt two objections from this historian. Firstly, historical newspapers are *always* "primary" sources, regardless of the use to which they are put. Secondly, while caution is advisable when approaching press sources, this does not mean that their information is inaccurate and therefore useless. Journalists can have access to informers and insiders who provide information from within terrorist groups and the police. Study of the historical press in comparison with the files of police investigations can reveal that, amongst some speculation and fabrication, newspapers could be surprisingly accurate. Trivial though this example may be, this very basic difference in the understanding and labelling of sources points to a problematic incompatibility between historical and social scientific approaches to terrorism.

This incompatibility is in evidence in two further areas: i) the search for rules or generalisable theories about terrorism; ii) approaches to data gathering. Firstly, rules. Social scientist Dipak Gupta has succinctly summarised this tension between discipline-specific approaches: The primary difference between history and social science lies in the former's reluctance to develop a generalised theory, while the latter clamours for common rules that bind human behaviour over time and geographic space. As a result, while historians work in the archives unearthing the sequence of

events that led to a certain outcome, social scientists generally pursue data-driven analyses (Gupta 2011, 96). The scepticism of historians is tangible: Dietze claims that the “general purpose of the social sciences is to arrive at universally valid statements that hold true independently of time and space” and for this reason the “social sciences have no single comprehensive approach that explains all the questions that are important in a historical analysis of terrorism’s origins” (Dietze 2021, 21). Beverly Gage concurs: “historians, as a group, have little patience for general theories”; they are thus content to keep the social scientific terrorism studies “at arm’s length” (Gage 2011, 79–90).

Secondly, data gathering. The social scientific control of the field has seen a growing emphasis on quantitative research methods and statistical data collection as the benchmark for terrorism research. As mentioned above Silke’s 2004 review measured the quality of research according to its use of “distinct and quantifiable data” (Silke 2004, 11–12). He found much work lacking in this respect, bemoaning the “heavy reliance on qualitative and journalistic approaches” to terrorism which, though “quite good in describing the broader context of terrorism”, lacked the hard data to provide real insight (Silke 2004, 11–12). He took a negative view, too, of the use of the “archival record” (Silke 2001, 4; 2004, 11–12). If Silke did not mention historians, he was describing their methods and approaches. Silke restated his criticism of qualitative methods in 2008 (Silke 2008). Lisa Stampnitzky’s review of the field in 2011 noted that the terrorism studies research community perceived a lack of statistical data to be a problem, in addition to the failure of many academics to conduct fieldwork: one scholar interviewed stated, “there was a whole generation of terrorism scholars that never got out in the field that did all of their research from the faculty lounges or university libraries” (Stampnitzky 2011, 9). The inability of historians to do fieldwork does not need explanation.

For researchers such as Silke, statistical analysis seems to offer a form of clarity that cannot emerge from qualitative approaches: Since the 1950s, the social science disciplines have experienced a rapid increase in the use of statistics. People are extremely complex, and their behaviour and thoughts are the result of a confusing interaction of emotions, motivations, learned behaviours and genetically determined traits. Consequently, social science researchers typically have to work with very “noisy” data where there are potentially a vast number of factors exerting an influence on any one behaviour, event or trend. Statistical analysis has emerged as a way for researchers to determine which factors genuinely are important and which are not. Descriptive statistics enable the researcher to summarise and organise data in an effective and meaningful way (Silke 2001, 9). The reductivism of this approach is anathema to historians for whom the “noise” of the data is of primary interest. As Laqueur stated in 1977: “[a] quantitative index cannot possibly reflect the qualitative aspect of human frustration; discontent and relative deprivation defy ‘objective’ measurement” (Laqueur 1977b, 10–11). The context in which historical terrorism operated is, for the historian, of vital importance to an understanding of its operation.

There is likely another agenda at work here: quantitative data can feed into policy work that seeks a solution to immediate threats. Phillips has demonstrated the reactive nature of terrorism studies – “Terrorism research seems to *mostly* follow global terrorism patterns” (Phillips 2023, 413) – because state and security agency funding increases and decreases accordingly. Terrorism studies researchers seem concerned with the “predictive

value” of data and its potential to forecast “future events” (Silke 2001, 2). The rewards for contemporary-focused data-driven research that can inform policy and even foretell future attacks are clear; those for qualitative methods applied to history are less so.

Schuurman’s 2020 article revealed a similar preference for research that generated quantitative data. The author described the increasing adoption of such methods as an “improvement” and a “positive change” in terrorism studies, and an indication of growing scientific rigour. Yet the continuing relative lack of quantitative research work was an “area of concern” (Schuurman 2020). There are grounds to criticise Schuurman’s survey of quantitative research in terrorism studies. In compiling his data, Schuurman considered *any* use of statistics in a publication as an indication of quantitative research: “authors who created an extensive dataset and carried out complicated statistical analyses [were] grouped together with authors who provide a relatively straightforward overview of the frequency with which a particular search-term appears on Google” (Schuurman 2020, 1016). He explained that “[t]his was done to avoid making subjective judgements on what primary data or which types of statistical analyses were ‘good’ or ‘extensive’ enough; a process bound to introduce considerable bias into the results” (Schuurman 2020, 1016). Without assessing the value of this data or the relative merits of its interpretation, Schuurman decided that its simple inclusion was a marker of “quality”.

Historians do not reject quantitative analysis, yet they ask questions of this evidence, as they would any type of source, not least those regarding the circumstances surrounding the collection and compilation of such data. CTS scholars have drawn attention to such questions with regard to terrorism databases and datasets, notably the conditions according to which an act of violence was determined to be “terrorist” and therefore included in the data. Simply noting that the use of statistics is a marker of quality and progress is not satisfactory. One need not look far to find examples of the problematic use of statistical data on terrorism, especially when applied to the past. Rasler and Thompson, for example, use data from the International Terrorism Attributes of Terrorist Events (ITERATE) source to “prove” that Rapoport’s four waves model is valid. Given that ITERATE data does not cover the period prior to 1968 (encompassing the entirety of Rapoport’s first and second waves, and the majority of the third), it is difficult to see how Rasler and Thompson arrived at this conclusion (Rasler and William 2011, 25).

A recent attempt to compile a dataset on historical terrorism lends further weight to the above criticisms. Joshua Tschantret has generated a dataset on historical terrorism during the 1860–1969. Tschantret considers that terrorism studies is “becoming more methodologically sophisticated as scholars move from descriptive case study to quantitative analysis” (Tschantret 2019, 933). However, “data limitations” – notably the paucity of data on terrorism between 1860 and 1969 – have impeded the “ability to generalize terrorism research across time” (Tschantret 2019, 934). To address this situation, Tschantret conducted a close reading of “books and articles on historical terrorism, political violence, and contentious politics to identify terrorist groups and the city-years in which they were formed”, looking specifically for groups that committed assassinations and bombings’ (Tschantret 2023, 15–17). He (and his co-authors) defined terrorism as “clandestine political violence by nonstate actors that do not control territory” and who attacked “civilians or symbolic targets” (Tschantret, Yang, and Nam 2021, 665). Tschantret used this data to claim that terrorist groups were most common in cities

where civic associations flourished over time. “Associationism” provided a means for like-minded people to meet, share a sense of grievance, and hone political campaigns; it was the “intervening variable between modernization and terrorism” (Tschandret 2023, 9). Tschandret thus deduced that terrorists must prioritise “social solidarity” and comradeship over political aims (Tschandret 2023, 6).

While Tschandret’s compilation of data on historical terrorism is commendable, and his conclusions thought-provoking, for historians they are deeply problematic. Firstly, there is no allowance for the fact that the data – found in secondary sources, that is, those sources one or more steps removed from the event or period – has already gone through a process of selection and interpretation before its selection for inclusion in the dataset. The *dataset* may be original, but his *data* is not. The historian’s focus on the provenance of a primary source, the biases inherent to its construction, and the processes of interpretation and representation to which such sources are subjected in the secondary literature limit the usefulness of third-party quantitative data such as this, and its application to the past.

Secondly, the interpretation of the data conflates correlation with causation. To determine whether terrorists were most likely to belong to civic associations, and therefore make a connection between associationism and terrorism, would require an analysis of multiple historical case studies – an “ideographic” approach that Tschandret rejects. Without this, however, one might suggest a link between, say, terrorists and kitchen sinks, because most terrorists had one.

Thirdly, it is not clear how Tschandret deals with instances of transnational terrorism. For example, if a group committed an attack in a country other than its own, what is the relationship between associationism in this country and the terrorist group? How, for example, did the level of associationism in 1930s France relate to the assassination of the French President by a Russian immigrant in 1932?

Finally, the dataset is divorced entirely from historical context (CTS scholars have recognised the danger of the decontextualisation of data on “terrorist” violence included in databases [Jackson et al. 2011]). Take the example of France. Between 1860 and 1969, five different political regimes governed this country, from the imperial autocracy of the Second Empire, through the parliamentary Third and Fourth Republics, to the presidential regime of the Fifth Republic, not to mention the wartime Vichy dictatorship that ruled in collaboration with the Nazi Occupier. The impact of these political regimes on associationism and its apparent links to terrorism does not concern Tschandret. Furthermore, the place of the Second World War in the dataset is worth mentioning. The data on the “temporal distribution of terrorist groups” shows that terrorism declined during the conflict, with no terrorist groups founded in the latter years of the war (Tschandret, Yang, and Nam 2021, 668). Yet wartime resistance groups surely conformed to this definition of terrorism (“clandestine political violence by nonstate actors that do not control territory” and who attacked civilians or symbolic targets’) (Tschandret, Yang, and Nam 2021, 665). Their absence from the dataset is likely due to the fact that historians do not readily classify such groups as terrorists; once again, we see the impact of the data collection method on the results.

Historians face a problem if the use of quantitative methods and datasets are largely considered the touchstone of terrorism research. To be clear, historians do not reject such methods and advances in technology have lent themselves to the construction of

datasets of historical information. Nonetheless, there is reluctance to move away from a primary focus on text and narrative that produces context-specific conclusions (Morris 2022, 161). Additionally, the development of generalisable rules or models is not the historians' concern (English 2021, 22). Conversely, to conceive of the use of statistical analyses as a sign of "progress" in terrorism studies is to close avenues of investigation into periods for which quantitative data is either lacking or unsuitable. Erica Chenoweth has called for a more qualitative approach to complement the wealth of quantitative studies: "I believe that the terrorism field would benefit from returning to more in-depth historical and ethnographic work – not because this approach is superior but because the terrorism field, by relying exclusively on quantitative data, may be too distant from the politics or terrorism itself for the field to produce new and meaningful insights" (Chenoweth 2013, 374). Historians must be proactive in effecting this action. They must make a case for the relevance of their qualitative research to the present and, by extension, the field of terrorism studies. The final section of this article explores how they may do this.

The future of history in terrorism studies

Historians can operate comfortably in the field of "orthodox" terrorism studies, where the focus is on measuring terrorist phenomena, seeking to understand why it emerges, how it functions, and what explains its disappearance or retreat (as described in Heath-Kelly 2019, 227). The wealth of sources available, from government documents to the memoirs and diaries of historical players – politicians, police leaders, and former terrorists themselves – as well as the files of police investigations, including confessions, explanations, and justifications delivered in public and in private, not to mention fictional portrayals on the page and screen, permit the examination of terrorist episodes from a variety of angles. Our distance from the past facilitates a comprehensive approach that can encompass the beginning, middle, and end of a terrorist group in a way that is not possible for scholars studying ongoing threats. Analysis of terrorists' own pronouncements, manifestos, and handbooks, is also possible, allowing us to appreciate how actors themselves considered their actions, not least with regard to longer histories of violence and possible historical role models. Historians thus have much to contribute on the "mechanics" – the who, what, when, where, why, and how – of historical terrorist violence. If it is true that the two predominant questions in the field are currently "why do people become involved in terrorism?" and "why do they cease involvement?," historians can contribute meaningfully (Frumkin, Morrison, and Silke 2023, 7).

The study of historical terrorism on its own terms (as set out above) can raise questions of contemporary significance; it risks leaving the historian with little to say beyond "this has happened before". A recent (2022) volume on right-wing terrorism offers an example of this intellectual cul-de-sac: the editors write in the introduction that the essays within the volume, "show that transnational and global right-wing terrorism is not a new phenomenon" and that, given the strength of historical right-wing terrorism, "the resurgence of new networks of right-wing terrorists throughout the world should concern us" (Dafinger and Florin 2022, 12). Of course, this statement must be set in the context of the broader apparent ignorance of the extreme right in terrorism studies (Ravndal and Bjørge 2018). Yet it does not provide compelling evidence for the relevance of the historical

studies in the collection to the study of contemporary terrorism. However worthwhile historical inquiry, the orthodox approach to historical terrorism risks leaving the past in the past.

History need not be hobbled by its prizing of context and nuance or its aversion to providing straightforward answers for the present. But if historians are to carve out a place in terrorism studies – and receive the recognition of terrorism scholars beyond their discipline – they must work to make their research relevant to contemporary concerns. Indeed, according to Jeffrey D. Simon, the perceived inability of history to answer questions about modern terrorism is to blame for its seeming irrelevance (Simon 2011, 44). To strive to answer these questions does not mean abandoning history. Nonetheless, to avoid falling into obscurity, historians must recognise the agendas that dominate the field and render these agendas sensitive to the value of historical study.⁵

To this end, historians may take two approaches. Firstly, they can engage with theoretical literature, often developed through the analysis of contemporary terrorist threats, and apply it to, or test it against, historical case studies. In doing so, they may approach the subject matter by a novel means or draw conclusions that prompt adaptations or adjustments to accepted theories. This approach requires immersion in current scholarship on terrorist violence as well as more traditional historical studies. It also requires authors to be more strident in their assertions about the applicability of their investigations to contemporary terrorist threats.

Secondly, historians can use qualitative approaches to terrorism to examine how past societies understood the phenomenon, how these understandings shaped behaviour, and how modern ideas emerged from these understandings through a process of long-term evolution. This method not only reveals the roots – or genealogies – of recent ideas but also renders the researcher sensitive to constructions of terrorism in their own society, and the agendas they serve. I call this approach “cultures of terrorism”. I have defined cultures of terrorism as the “frameworks of values and qualities that [inform] common beliefs about the nature, operation, and goals of terrorism and its perpetrators. ‘Terrorism’ is therefore understood ... not only as a ‘brute fact’ (Jackson 2011, 117), but also as a political and cultural construction of violence composed from a variety of discourses and deployed in particular circumstances by commentators, witnesses, and perpetrators” (Millington 2023). This approach recognises that “terrorism” is a volatile concept that draws its meaning from contextually-specific “discursive ecosystems” (Livesey 2021, 475). To label an act of violence, a group, or an individual “terrorist” relied (and relies still) on this framework of layered ideas, values, and meanings, which itself drew on and adapted other available discourses (such as those concerning race, immigration, gender, and so on). Ultimately, rather than an accumulation of scientific knowledge, they resemble more the “received wisdom” about, or “common sense” view of, terrorism at a particular historical moment.

A cultural approach requires a focus on the discourse of terrorism in the past, as found in the press and media, as well as in government and police documents, and fictional and cultural works. Richard Jackson’s analysis of the discourse of Islamic terrorism is instructive in this respect. Jackson explains that terrorism discourse amounts to “the terms, assumptions, labels, categories and narratives used to describe and explain terrorism” (Jackson 2007, 394). It is a socially-constructed discourse that contains “unacknowledged assumptions and embedded political-cultural narratives” and is used “to promote a number of

discrete political projects and [to] reify a particular kind of political and social order” (Jackson 2007, 395; 425). This latter observation means that the process of labelling violence as “terrorist” is a political act. The work of Joseba Zulaika and William Douglass focuses on this labelling process; it offers a further point of reference for the historian. Zulaika and Douglass, like Jackson, recognise that the discourse of terrorism is constitutive, rather than reflective, of “reality”. To label an act as terrorist relies on “cultural premises and discursive strategies” that give the word power and meaning (Zulaika and Douglass 1996, 6).

The work of Jackson, Zulaika, and Douglass, is circumscribed in its focus on contemporary terrorism: Jackson examines sources on Islamic terrorism between 2001 and 2006; Zulaika and Douglass claim that their conclusions are applicable only to the period since the 1970s when “[t]he present media paradigm for treating terrorism emerged” (Zulaika and Douglass 1996, 7). John Carter Wood’s research on broader cultures of historical violence may suggest longer-term roots to the contemporary discourse of terrorism (though Wood does not address terrorism itself). Wood proposes that humans understand violence through culture, which he defines as “historically accumulated collections of beliefs and practices which are socially produced and aimed at meeting psychological needs”; culture provides frameworks through which humans act (Wood 2007, 81–2). Culture is a product of the human mind and because (according to Wood), “mental mechanisms” are not infinite, it is possible to see “cross-cultural regularities in human social behaviour” (Wood 2007, 81). Similarities in the interpretation and management of these logics can also span chronological periods. One can thus compare attitudes to, and representations, of plebeian knife fighting in nineteenth-century Greece and political street fighting in interwar France (Gallant 2000; Millington 2014).

The work of Wood, when read in conjunction with Jackson, Zulaika, and Douglass, raises the possibility that understandings of terrorism – or cultures of terrorism – may also cut across geographical and chronological boundaries. These discourses do not remain fixed because they form through interaction with events and developing political and cultural contexts. Cultures of terrorism are not therefore disconnected from “real events” because these events can bring about changes or adaptations in discourse. Importantly, this approach does not require a definition of terrorism; we are less beholden to arriving at a specific definition when what matters is how historical actors used the word.

How does this approach render history relevant to scholars of contemporary terrorism? Historians investigating cultures of terrorism can trace the development of these cultures over time, across terrorist episodes, right up to the present. They may discern features of a national – or international – culture that persist across periods, even if the ephemeral content of these cultures shifts. The emphasis is on adaptation over time rather than caesuras. Zulaika and Douglass are correct to identify that after the 1970s, the media routinely applied the terrorist label to the “Other” in American society (Zulaika and Douglass 1996, 13). This “Other” was central to historical cultures of terrorism, too, even if the character fulfilling this role – the anarchist, the colonial rebel, the wartime resister – changed. These new “facts” tend to be overlaid onto existing cultures, or the cultures adapt themselves accordingly, without moving far from their origins. We can thus draw broad comparisons between Theodore Roosevelt’s 3 December 1901 address to Congress in the aftermath of President William McKinley’s assassination, and George W. Bush’s 20 September 2001 address in the aftermath of 9/11.

By looking at “what terrorism *was* in the past”, and “what terrorism *is* in the present”, historical cultures of terrorism allow us to trace the durable aspects of contemporary constructions, locating their origins and determining moments of solidification and adaptation. They allow us to perceive contemporaneous features of our own cultures of terrorism and how these are adapted to suit new political, cultural, and social circumstances. They free historical work from its silo by placing the emphasis on *continuity* while remaining sensitive to change over time. Finally, they encourage the critique of contemporary labels and the labelling process itself and inure the historian against serving state policy agendas (against which Larabee cautioned historians [Larabee 2011, 106–10]).

In conclusion, CTS is the natural home for the historian who seeks *not only* to understand terrorism in the past *but also* to make their research relevant to the present. Significantly, the historian who takes a cultural approach to the subject can move beyond period-specific “facts” to explore the long-term roots of “terrorism” and the “terrorist” as “cultural constructs” and “social facts” (Jackson 2015, 488; Heath-Kelly, 2019, 227). The history of these constructs and labels points to a *continuity* across different forms of terrorism while taking into account evolution and development. For terrorism studies researchers, rather than consigning past forms of terrorism to their own historical containers, a cultural approach suggests that notions of terrorism today have evolved into their contemporary form over a long period of time that merits further engagement.

Notes

1. When I refer to “terrorism studies” in this article, I include both “orthodox” and “Critical” terrorism studies, unless stated otherwise.
2. I define the social sciences to include international relations, political science, sociology, and psychology <https://acss.org.uk/what-is-social-science/>
3. Laqueur had used the term “wave” in 1979 to describe the “ups and downs” of historical terrorism, though his conception of the wave depended more on the terrorists’ choice of weapon than their political goal.
4. The historian was Richard English. The other people consulted were: Shazad Ali, Christine Boelema Robertus, Boaz Ganor, Paul Gill, Rohan Gunaratna, Berto Jongman, Daniel Koehler, Gary LaFree, Clark McCauley, John F. Morrison, Kumar Ramakrishna, Bart Schuurman, Ryan Scrivens, Michael S. Stohl, Judith Tinnis, Ahmet Yayla, and Aaron Zelin.
5. “The essential point of historical inquiry is not to intervene in contemporary debates or to learn lessons, since that historians learn to denounce as ‘presentism’ in their first days of graduate school. Yet too rigid an adherence to such strictures can lead to obscurantism and disengagement, as if historians have nothing to say about the day’s most pressing social questions” (Gage 2011, 94).

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