

**Recent Trends in Early American Studies:  
A Review Essay**

JULIANA BARR and EDWARD COUNTRYMAN, eds., *Contested Spaces of Early America* (Philadelphia: U of Philadelphia P, 2014), 444 pp.

STACY SCHIFF, *The Witches: Salem, 1692* (London: Weidenfeld & Nicolson, 2015), 512 pp.

BENJAMIN C. RAY, *Satan & Salem: The Witch-Hunt Crisis of 1692* (Charlottesville, VA: U of Virginia P, 2015), 264 pp.

RICHARD C. DE PROSPO, *The Latest Early American Literature* (Newark, NJ: Delaware UP, 2016), 400 pp.

Significant special issues, forums, reappraisals, and review essays on specific fields within Early American studies have appeared over the past decades. Leading journals such as *Early American Literature*, *The New England Quarterly*, and *The William & Mary Quarterly* have published surveys on individual influential studies and topics such as the Salem witchcraft trials, new interpretations of Puritanism and ensuing paradigm shifts, the Black Atlantic, New England Slavery, and Early French America. Review essays touching on early American topics published in *Amerikastudien / American Studies* have tended to focus on long-term perspectives and to include the first half, if not all of the nineteenth century.<sup>1</sup> The drawback of such special-

ist essays on particular research areas is that it is difficult to gain an overall impression of the breadth of the field. In this shifting research landscape, one can detect a bias in European American studies towards research on modernity. The breadth in scope and approach of recent publications suggests that journals aim to reach a broader public. This view is reinforced by the abundance of popular publications on prominent topics such as the Salem witchcraft persecutions and popular personalities such as Benjamin Franklin.<sup>2</sup> While they may serve a significant educational function, such popularizations also tend to promulgate misperceptions. They may do more harm than good for serious specialist research, which is relegated more and more to the margins with regard to its critical reception in the field.

This assessment may be tested in light of four recent publications targeting popular and specialist audiences alike, which are reviewed here with an eye to the general development of the field and the trend towards popularization. And, since such monographs and essay collections continue to be influential in American studies, one should critically assess current tendencies within academic publishing, and determine how they may be improved. Taken together, these reviews make clear that the separation of “early” and “late” modern American Studies in the intellectual biographies and professional careers of many contemporary scholars and publications may be detrimental to the field of American studies as a whole (as are sweeping studies covering long time-frames that take the idea of *longue durée* too lightly). Only a more careful integration of both “early” and “late” and a higher degree of specialization on the central topics of American studies, also from a theological perspective, will enable the field to legitimize its place in academia, while also becoming more critical of its own canonical and categorical fixations and topics. A general tendency towards identifying “early American” with “early US-American” and to not distinguish the latter from pre-US-American in many publications, along with frequent use of the term “early America” for the early Republic

*the American Revolution*,” *The New England Quarterly* 91.1 (2018).

<sup>2</sup> According to WorldCat, more than two-hundred publications bearing “Benjamin Franklin” in the title and more than fifty concerning the Salem witchcraft trials have appeared each year of the past decades.

<sup>1</sup> Alfred Hornung, “American Autobiographies and Autobiography Criticism: Review Essay,” *Amerikastudien / American Studies* 35.3 (1990): 371-407; Waldemar Zacharasiewicz, “Multiplicity of Southern Voices: A Review Essay,” *Amerikastudien / American Studies* 36.1 (1991): 119-42; Charles Cohen, “The Post-Puritan Paradigm of Early American Religious History,” *William & Mary Quarterly* 54 (1998): 695-722; “Forum: Salem Repossessed,” *William & Mary Quarterly* 65 (2008): 391-534; Rochelle Raineri Zuck, “Rethinking the Black Atlantic: Gallows Literature, Slave Narratives, and Visual Culture,” *Early American Literature* 51.3 (2016): 683-96; James F. Brooks, “Continental Shifts,” *William & Mary Quarterly* 74.3 (2017): 533-41; Jared Ross Hardesty, “New Histories of New England Slavery,” *ibid.*: 542-47; Eugene R. H. Tesdahl, “Exchange, Empire, and Indigeneity in French America,” *ibid.*: 548-52; “Commemorating the 50<sup>th</sup> Anniversary of Bernard Baylin’s *Ideological Origins of*

lic (see, for example, the remarks below in the review of Hämäläinen's article in the volume edited by Barr/Countryman) betrays this modern-premodern divide. Thus, while inviting us to rethink European scholars' bias towards modern American studies, these recent publications also draw our attention to the necessity for terminological precision: "early American" should be reserved for pre-Independence inquiries and "early Republic" for the early nineteenth century. Imprecise use of the term "early American" may also reflect publication needs and the structure of the academic market encouraging publications in earlier periods in the wider field of American studies.

While spatial and temporal expansion (following the respective "turns") has left its mark(s) on early American studies during the past decades,<sup>3</sup> future paradigm shifts will be determined by particular religious as much as national agendas, the omnipresence of trans- and postnational trends and claims concerning historiographical veracity notwithstanding. Despite the rise of international historiography—or perhaps as a result of it—old patterns persist, as scholars seek a new self-understanding in their research on early America. The following reviews inquire into the particular—polemical, popular, or scholarly—forms such research assumes, and cumulatively seek to demonstrate the necessity for greater research especially in American Studies in Europe and particularly in Germany. The most curious bias in this regard concerns the treatment of theology. While American scholars look up to Germany with regard to its theology, and theology is seen as an important factor in early American studies, in Germany theology generally plays—despite some significant publications—a surprisingly marginal role in American studies. This survey of recent American publications in the field of early American studies also invites comparison with dominant approaches on both sides of the Atlantic.

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Juliana Barr and Edward Countryman's recent volume on *Contested Spaces of Early America* gathers twelve essays by established European, North, Central and South Ameri-

<sup>3</sup> Michelle Burnham, "Time and Space in American Literary History," *Early American Literature* 39.1 (2004): 133.

can scholars representing a wide array of approaches to the topic of space in the early Americas.<sup>4</sup> Rather than a mere rehashing of the spatial turn in the field of Early American studies, this collection provides cutting-edge research on neglected areas within the field. The editors chose to divide the volume into four sections of varying length, focusing roughly on power, landscapes, resettlements, and memory. Yet, the sections touch upon many other issues, including borderlands, land tenure and land rights, Native Americans, and colonization, and the analyses are carried out within a framework of historical geography and cultural history centered around the eighteenth century. Exemplifying to a certain extent a general recent trend in American studies towards covering broad geographic areas,<sup>5</sup> the volume aims at covering the area "from north of Quebec to south of Buenos Aires and from the Atlantic littoral to the Pacific coast" (1). Thus, it avoids the widespread tendencies of focusing either exclusively on New England traditions or staying within the boundaries of one of the American continents, and instead lends support to the hemispheric paradigm that is gaining traction in early American studies, as well as the transnational approach and the growing importance in studying the history of cartography (2-4).

The editors examine in detail the significance of early modern cartography in their introduction ("Maps and Spaces, Paths to Connect, and Lines to Divide"), and emphasize "the advance of knowledge" (4) in addition to the geopolitical and geographical dimensions of map production. With the help of numerous illustrations, they showcase Native American maps and demonstrate how Native American knowledge gradually came to inform European map-making, pointing out that English maps since the seventeenth century tended to either do away with indigenous place names

<sup>4</sup> The volume is published in cooperation with the William P. Clements Center for Southwest Studies at Southern Methodist University and the McNeil Center for Early American Studies at the University of Pennsylvania. Originally intended as a Festschrift for the retirement of David J. Weber (1940-2010) and conceived with his support, the volume is now dedicated to his memory (1; 23; 427).

<sup>5</sup> Cf. John Corrigan (ed.), *Religion, Space, and the Atlantic World* (Columbia, SC: University of South Carolina Press, 2017).

altogether by replacing them with English ones, or else to place them behind a *frontier* or *wilderness* demarcation (20). Since the editors point to the difference between Spanish, French, and English cartography, it would have been sensible at this point for the sake of advancing the study of cartography to have reflected upon the significance of religion and a potential difference between Catholic and Protestant approaches to map-making and empire-building. In this sense, despite their brilliant and knowledgeable execution of the survey, the introduction already reflects a possible bias of the collection, leading the reader to ask whether its historical focus can do justice to the subject matter. They are certainly right, however, in emphasizing the geopolitical function of map-making. There is thus an effort at recovering Native American map-making from the incursions of the dominant linear narrative of Western proto-nationalist expansion in an attempt at uncovering “layer after layer of historical detritus” (28). They thus inscribe themselves into the growing body of scholarship investigating Native American influence on European settlement, in particular, “how European societies took their form and shape from the Indian spaces they inhabited (and often shared with them),” thus seeing “early America as a single unified space defined by indigenous experiences with colonialism.” Hence, the volume is in search of a “human geography of cultural relations” (23).

The essays by Pekka Hämäläinen and Allan Greer in the first part on “Spaces and Power” make overarching claims concerning the spread of power dynamics from the indigenous interior to the continent’s edges. Hämäläinen’s “The Shapes of Power: Indians, Europeans, and North American Worlds from the Seventeenth to the Nineteenth Century” focuses on the Iroquois, Comanche, and Lakota and the transformation of land into a marketplace commodity, while Greer’s “Dispossession in a Commercial Idiom: From Indian Deeds to Land Cession Treaties” examines the “black legend” regarding Spanish practice as being inherently more violent than the English. Hämäläinen seems to second Greer when he points to similarities between English and Spanish American empires in their beginnings when viewed from a Native perspective (39).<sup>6</sup> Both essays in the first part of

<sup>6</sup> On this topic and related work see also René Dietrich’s review essay in *Amerikastudien / American Studies* 62.1 (2017).

the volume highlight the necessity for further research into the differences between Catholic and Protestant empires in the Americas.

In his examination of forms of power, Hämäläinen opens the volume on a cautionary note: historians of early America risk atomizing their subject, that is, ignoring the context for the purpose of avoiding any suspicions of subscribing to an “Anglocentric master story” (32). At the same time, extending the term “early American” to describe events of the nineteenth century risks drawing attention away from the preceding centuries and blurring the—in any case vague—boundaries of the problematic term “early modern.” Hämäläinen ascertains that the current emphasis on localism dominating early American studies functions like a “pointillist painting observed close-up” and that the “fragmentation of early American history reflects the relativist mood of our times” (32-33).

By contrast, Elizabeth Fenn in her opening article of the second part on “Spaces and Landscapes” looks at the Mandan villages on the Missouri River from the point of view of microhistory as a product of local geography, examining their existence, resilience, and persistence in the face of the transformation of their surroundings up to the mid-nineteenth century. In “Colonial Spaces in the Fragmented Communities of Northern New Spain,” Cynthia Radding offers a counter-example in her examination of the co-existence of Spanish and Native societies. Raúl José Mandrini provides a further example from South America, specifically from the area south of Buenos Aires, in “Transformations: The Rio de la Plata During the Bourbon Era.”

The third part on “Spaces and Settlements” opens with “Blurred Borders: North America’s Forgotten Apache Reservations,” in which Matthew Babcock argues that the Apache people were an agricultural and settler nation and that “Indian reservations” arose from negotiation and are therefore best not described as a form of imprisonment. By contrast, enslavement is the topic of Chantal Cramaussel’s essay on the forced transfer of Natives in northern New Spain, which took place even against and after its prohibition in 1542. Her essay draws attention to the need for comparative studies of enslavement, between historical epochs and across cultures. It raises the question at the heart of much contemporary research: To what extent can focusing on single groups or ethnicities ad-

vance slavery studies?<sup>7</sup> She also emphasizes that the colonizers wanted labor rather than land, an observation shaped by recent studies stressing the proto-industrial nature of late early modern slavery.<sup>8</sup> This observation may also contribute to further research in mining history, including the *reales de minas* (207; 423), which proves central to some of the concerns addressed in this volume, especially in Cynthia Redding's contribution (136). Alan Taylor turns his attention to population density, demographics, Spanish, French, British, and US-American colonial geopolitics, and examines different communities of settlers in "Remaking Americans: Louisiana, Upper Canada, and Texas."

The fourth and longest section of the book is dedicated to "Spaces and Memory," opening with Brian DeLay's "Blood Talk: Violence and Belonging in the Navajo-New Mexican Borderland," in which he examines the persistence of different kinds of cultural memory in the Southwest in the nineteenth century. Birgit Brander Rasmussen explores the artistic consciousness of the Kiowa man, (Etahdleuh Doanmoe),<sup>9</sup> reversing the perspective of captivity narratives by depicting an instance from the viewpoint of a male Native. Carrying on this perspective from literary into indigenous art history, Ned Blackhawk examines the Segesser hide paintings from New Mexico, named after Philipp von Segesser von Brunegg (1689-1762), a Jesuit priest who had sent them to his family in Switzerland in 1758, where they remained before being returned to New Mexico in 1986 (277; 280). These hides are a vivid pictorial testimony to

Native Americans being strategic partners on equal footing in early modern warfare in the Americas, demonstrated by the "Segesser II" hide depicting lieutenant general Pedro de Villasur's and his Pueblo auxiliaries' well-known defeat against an alliance of the Plains Indians (Pawnees) and the French in 1720 (280-82). In keeping with the comparative theme of the volume, one could have investigated the Segesser hides in comparison with contemporary European battle tapestries, which the hides may have been modeled after.<sup>10</sup> Such a comparison would have enriched the perspective of art history beyond their production conditions as they were shaped by their more immediate surroundings (294-98).

In the final chapter, Samuel Truett looks at "The Borderlands and Lost Worlds of Early America," tracing frontier history and cultural memory from the sixteenth century into the age of an emergent tourism industry by surveying major differences and similarities between the North American Northeast and Southwest. Focusing on the eighteenth century, he looks at texts by authors such as Richard Hakluyt (1552-1616), naturalist William Bartram (1739-1828), pioneer Daniel Boone (1734-1820), explorer John Lawson (1674?-1711), physician and collector Hans Sloane (1660-1753), Thomas Jefferson, and the French naturalist and ethnographer Antoine-Simon Le Page du Pratz (1695?-1775). His contribution closes the volume by reminding readers of the necessity of reception history and a perspective of *longue durée* (301) that must accompany the broad geographical scope at focus in these essays; further research needs to bear in mind and reflect these *two* borders of its own disciplinary constraints, while also taking into account the necessity of transcending the overarching historical paradigm across disciplines: historical geography ultimately remains subservient to cultural history, until it opens up to this dimension, too.

Overall, while remaining predominantly historical in its approach, this well-illustrated and well-structured volume draws attention to the necessity of multidisciplinary research from a wide range of perspectives on early American borderlands. It is somewhat curious that, despite the reproduction of various ancient and modern maps, tables, paintings, drawings, prints, and photographs, the vol-

<sup>7</sup> Cf. Edward E. Baptist and Stephanie M.H. Camp, (eds.), *New Studies in the History of American Slavery* (Athens: U of Georgia P, 2006); Jeff Forret and Christine E. Sears, (eds.), *New Directions in Slavery Studies: Commodification, Community, and Comparison* (Baton Rouge: Louisiana UP, 2015).

<sup>8</sup> Trevor Burnard and John Garrigus, *The Plantation Machine: Atlantic Capitalism in French Saint-Domingue and British Jamaica* (Philadelphia: U of Pennsylvania P, 2016) 3; 37; 236; Jackson P. Ward, *European Empires in the American South: Colonial and Environmental Encounters* (Jackson: UP of Mississippi, 2017) 15.

<sup>9</sup> Phillip J. Earenfight (ed.), *A Kiowa's Odyssey: A Sketchbook from Fort Marion* (Seattle: U of Washington P, 2007).

<sup>10</sup> New Mexico History Museum, "Segesser Hide Paintings," 26 Nov. 2017. Web.

ume lacks a list of illustrations. While it is still unclear what new paradigm the perspectives presented in this volume might help usher in, they are all valuable in redefining and differentiating engrained perceptions of the spatial dimension of European settlements of the Americas. In this sense, the book recommends itself to any American studies library and all those interested in redrawing lines in the vast and still largely uncharted territory which is early American studies.

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The witchcraft persecutions in and around Salem are certainly one of the most thoroughly researched and at the same time contested areas in the field, as testified by the abundance of recent publications, both fiction and non-fiction.<sup>11</sup> More research is required on the witchcraft publishing industry at large, as it is embedded in a dense network of interests from memorial culture to the tourism industry, in which scholarly research risks playing an increasingly marginal role. In *The Witches: Salem, 1692*, biographer and journalist Stacy Schiff retells the Salem story in an elegant, gripping, and figurative style—on nearly 500 pages of small print.

Schiff's writing style marks her entire work. One example is her description of how rumor, in addition to what she calls the "Indian" threat, was a central motivating force of the witchcraft accusations: "Rumor was the other nimble traveler [next to the Indians], melting through floorboards and floating through windows, insensible to mud, snow, fatigue, a light-footed fugitive from lumbering truth" (19). Some readers interested in early modern witchcraft persecutions from a scientific point of view may find her poetic style, which emphasizes anaphors, stark contrasts, and film-like descriptions and enumerations to assemble a fact- and plot-driven narrative, to be laying it on a little too thick. While her writing is also backed up by numerous little-known facts, including many facets of the Salem cases in 1692, she makes statements that may make a critical reader cringe. For example, she states that

"Avenging females hovered everywhere in 1692 Salem" (157). Another example is the way she concludes a paragraph on the feats, resilience, and endurance of women as they emerge from captivity narratives (in which they murdered and scalped their "Indian" tormentors): "At a time of shuddering devastation, they stepped in as the dragon slayers" (144). Rather than pointing to Cotton Mather's publications, which contributed to the emancipation of women, she claims that, after 1692, women "went back to being invisible, where they remained, historically speaking, until a different scourge encouraged them to raise their voices, with suffrage and Prohibition" (411). Stylistic issues and the evident preference for dichotomization prompted by popularizing narrative may also contribute to overstatements that misinterpret the role and history of theology—a field that she on the whole ignores in her work. This allows her to make striking statements like the following: "Men blamed their sins for corrupting their souls. Women blamed their souls, which is to say themselves" (144). Stylistic panache is thus favored over scientific rigor.

This critical observation is not, however, the only cautionary note. The popularizing narrative also exhibits problems with regard to its content in its partial distance to contemporary historiography on early modern witchcraft, especially when it touches upon issues beyond New England, ignoring closely related disciplines like angelology and demonology. When dealing with the question of possession, for example, one might have wished for a more nuanced treatment of the topic: rather than opposing bewitchment with possession to describe the first instances of witchcraft in the Parris household (25), it would have been worth considering the different forms of early modern possession such as demonic versus spirit possession. In the fifth chapter entitled "The Wizard," she claims that by May 1692 the girls who made accusations "served as traditional witch-finders," which is inaccurate (162). Similarly, she maintains that Cotton Mather "seems to have embroidered" on court reports in his *Wonders of the Invisible World* demonology without acknowledging that this publication was the last early modern demonology. Instead she sees it as an apologia, judging it without respect to its genre, hence ahistorically (346-47).<sup>12</sup>

<sup>11</sup> Cf. Richard Hite, *In the Shadow of Salem: The Andover Witch Hunt of 1692* (Yardley: Westholme Publishing, 2018) and Hester Fox, *The Witch of Willow Hall* (Toronto: Harlequin Enterprises, 2018), to name but two examples.

<sup>12</sup> Cotton Mather, *The Wonders of the Invisible World* (Boston 1692/93).

Thus, ultimately, Schiff offers one more retelling of the Salem trials “on the ground,” writing a meticulous history of events that does not do justice, however, to the imaginative, intellectual, and theological context of witchcraft *at the time of the trials*. Even if the events in Salem are important for her narrative, the early modern witchcraft persecutions are too complicated for such a plot-driven retelling. Her work may be seen as a prime example of the dilemma surrounding scholarship on early modern witchcraft: the compelling and shocking nature of such stories stifle any attempt to unravel them in an objective analysis. Using the sources to create a gripping, if truthful narrative and analyzing them in order to further our understanding of the phenomena are two very different things. In this light, Schiff’s work may invite us to rethink the role of narrative history in scientific analysis.

These details nonetheless point to further directions in research. Schiff’s observation that many daughters of ministers were accused corroborates the theological issues since such accusations targeted ministerial families (201). The “not so much unclear as perforated” line between Catholicism and Protestantism, when it came to dealing with folk magic, points to the need for further research in relation to the “Catholicization” of Protestantism reflected in the Salem events (291-92). On the whole, despite the pitfalls of overly narrative approaches, the great merit of this account is that it makes a wealth of detail easily accessible, thus deterring over-simplifying, dichotomizing, or monocausal explanations.

Both books draw our attention to a curious lacuna in contemporary research on Salem witchcraft: while the transnational paradigm and hemispheric studies have broken up previous entrenchments and fixations on regional and local history, the Salem story still gets told for more parochial purposes, curiously uninformed by the research landscape surrounding it. It may console us that if Salem remains at the heart of US-American national self-understanding, research on it may be a truthful indicator of the seriousness with which present paradigms in the humanities are being implemented. As long as they do not touch research on Salem, they may not have been meant that seriously after all.

Benjamin C. Ray’s *Satan & Salem: The Witch-Hunt Crisis of 1692*, by contrast, emerges from the editorial project of the Salem Witch Trials Documentary Archive

and the new print edition *Records of the Salem Witch-Hunt* (ix; 9).<sup>13</sup> In his introduction, Ray weaves together current research on the Salem witchcraft persecutions. Overall, his book may be seen as a refutation of the thesis advanced in 1974 by Paul Boyer and Stephen Nissenbaum that the Salem persecutions were caused by property disputes and factional strife.<sup>14</sup> Ray even produces a map to depict the distribution of the accusers and the accused in Salem village (189). He reiterates why the persecutions cannot be accounted for by pointing to the threat of Native American military strifes<sup>15</sup> or by appealing to biomedical and rationalist explanations, from the start making his readers aware of the necessity of a multi-causal approach (5-6). Ray elegantly retraces the course of events, correcting emphasis and details along the way and generally updating it from the vantage point of current research on early modern witchcraft.

While he acknowledges in the first chapter the (political) relevance of changes to church polity and the ministry in Salem village preceding the persecutions, as well as the significance of the two Protestant sacraments and their accessibility to the people, he does not reflect on the theological or political implications of these facts. Given the central importance of Salem village minister Samuel Parris for the trials,<sup>16</sup> it is astonishing that Ray does not further engage with theological questions.<sup>17</sup> This raises the question to what degree the larger socio-political constellation and theological developments around Salem should be considered. One established fact

<sup>13</sup> *Records of the Salem Witch-Hunt*, ed. Bernard Rosenthal (Cambridge: Cambridge UP, 2009); *Salem Witch Trials Documentary Archive* 29 Nov. 2017 <salem.lib.virginia.edu>. 30 Nov. 2017. Web.

<sup>14</sup> Cf. Paul Boyer and Stephen Nissenbaum, *Salem Possessed: The Social Origins of Witchcraft* (Cambridge: Harvard UP, 1974).

<sup>15</sup> Mary Beth Norton, *In the Devil’s Snare: The Witchcraft Crisis of 1692* (New York: Knopf, 2002).

<sup>16</sup> Cf. Samuel Parris, James F. Cooper, Kenneth P Minkema (eds.), *The Sermon Notebook of Samuel Parris, 1689-1694* (Boston: Colonial Society of Massachusetts, 1993).

<sup>17</sup> As is only rarely done, cf. Walter Stephens, *Demon Lovers: Witchcraft, Sex, and the Crisis of Belief* (Chicago: U of Chicago P, 2002).

that goes entirely unmentioned in both books on Salem is the lateness of the Salem persecutions within the framework of the phenomenon of early modern *European* witchcraft persecutions to which they belong. Given the fact that European witchcraft persecutions were concerned with centralization, one should ask how this series of events in Massachusetts reshaped the position of the colony with regard to a centralized government and the development of the legal system. The legal aspects of the trials, which have been emphasized as an important factor, also need closer scrutiny in the New England context. This holds especially if one regards—as Ray does—Tituba's confession, which “played a pivotal role at the beginning of the proceedings,” as a “collaborative creation” in which the legal authorities took active part (39-40).

On the whole, the Salem witchcraft trials may serve as the best reminder of the limits of historiography: the impulse towards linear narrative that they invite, as these two publications demonstrate, may be counterproductive by concealing the reasons behind the persecution. While reception histories and studies of popular culture continue the narrative approach the persecutions themselves first fostered, we should be aware of the complexities facing research that takes a more structural and multidisciplinary approach, which integrates more fully the medical, theological, legal, political, and folkloristic aspects of the topic. For such research, these volumes and the sources they describe may serve as a helpful preparation. But they, too, will need to be approached critically: both books repeat the lore of Cotton Mather's presence on horseback at the execution of George Burroughs, an episode derived only from Robert Calef's later polemic, a source that neither author evaluates critically (Ray, 142; Schiff 300). In another instance, Schiff misguidedly contrasts the printing success of the Mathers with the censoring of New England's first newspaper, published by Benjamin Harris, without mentioning the most recent and important research on this topic and that Cotton Mather may have had a hand in the writing and publishing of this very newspaper, *Publick Occurrences Both Foreign and Domestick* (340-41).<sup>18</sup>

<sup>18</sup> William D. Sloan, “Chaos, Polemics, and America's First Newspaper,” *Journalism & Mass Communication Quarterly* 70.3 (1993): 666-68. doi:10.1177/107769909307000317.

One minor quibble remains: the term “witch-hunt” that pervades both books, even making it into the title of Ray's and the recent edition of the *Records of the Salem Witchcraft Trials*, is inadequate.<sup>19</sup> It stems from the rationalistic paradigm and should be replaced by “witchcraft persecutions.” Though it may seem to be a minor point in relation to such a contentious and complex topic, terminological precision is all the more important. In such cases, one would always do well to distinguish research from popularizing endeavors.

There is good reason to regard this critique as more than mere quibbling. It is partly through engagements with Salem that the term “witch-hunt” has become common currency, recalling the way Arthur Miller used it as a parable for the communist “witch-hunt” in the 1950s. Like many of his predecessors,<sup>20</sup> Ray uses the term to advise his readers that the “witch-hunt” is “a sociopolitical phenomenon that periodically erupts at both the local and national level in our society” (14). This,

<sup>19</sup> One should also reflect upon the adequacy of the frequently used term of a (social) “epidemic” in relation to the witchcraft persecutions and its potential for distorting the historical facts (Schiff 310; Ray 183-87, 231 following Malcolm Gladwell, *The Tipping Point: How Little Things Can Make a Big Difference* [Boston, MA: Little, Brown, 2000] 11). If one wishes to assume such a broader historical perspective, it may be essential to not seek the origins of escalation in social phenomena as Ray does, but instead the workings of “persistent” social escalation as the source of “sensational” escalation. Such an approach also risks favoring the historical events over their reception history, thus falling short of achieving historiographical depth. More importantly, however, as Ray's list of explanatory factors makes clear, it favors persons and (local) geography over ideas and (trans-geographical) structures. Future research that does not focus exclusively on juridical and theological developments may demonstrate that the Salem witchcraft persecutions were anything but a local or regional phenomenon. In this sense Ray's renewed attention to the religious side of the persecutions (188-89) and Schiff's observations on the broader context in her two final chapters point in the right direction.

<sup>20</sup> Cf. Robert Rapley, *Witch Hunts: From Salem to Guantanamo Bay* (Montreal, QC: McGill-Queen's UP, 2007).

at least to the present reader, suggests an essential similarity between this early modern phenomenon and modern ones, a tendency that may also be observed in Schiff's account. Downplaying the historical difference between early modernity and modernity in this way actually goes against the attention to detail that characterizes both publications on Salem. The use of this term, and the insistence on both the universality of the Salem phenomenon and its historical minutiae, create an ambiguity that future research will have to address if it intends to avoid rehashing the present state of our—frankly still quite limited—understanding of the Salem witchcraft persecutions.

Perhaps what is most striking in these two works on the Salem witchcraft persecutions is the fact that the persecutions themselves are still approached primarily from a historical perspective, while other disciplines (such as political science or theology) serve, at best, an ancillary function. While historiographical discourse acknowledges the necessity of multi-causal explanations, it has come short of cashing in on this insight by engaging in or demanding a more thoroughly multidisciplinary approach to early modern witchcraft persecutions. Also within the field of historiography itself, early modern witchcraft persecutions should be seen in comparison with other developments in which its crucial elements (such as centralization and renegotiations and redistributions of power in its wake) were manifest.<sup>21</sup>

One should also think of these two books in the context of recent research on early modern witchcraft as a whole. It is striking that North American research and European research on early modern witchcraft are not in sufficient dialogue; the limited number of scientific publications in languages other than English in the Anglo-American world seems to hinder research. Despite the fact that the context of Increase and Cotton Mather's thought and publishing is utterly European, both books on Salem reviewed here clearly emphasize North American scholarship on early modern witchcraft. The field will likely benefit from more comparative engagements with the important differences between early modern witchcraft

in continental Europe, England, Scotland and New England; this may, in turn, help to overcome instrumentalizations of early modern witchcraft phenomena within national and counter-narratives. For this kind of research, the attention to local detail presented by both of these publications—if in very different ways and for very different audiences and purposes—offers the necessary basis.

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Richard C. De Prosop, by contrast, remains skeptical about the state of early American studies. The latest eloquent installment of his research—gathered in a volume entitled *The Latest Early American Literature*—summarizes and elaborates his criticism of the field in a series of trenchant essays, some of which had been previously published but which he significantly updated for this anthological summation of his work, which touches upon a wide spectrum of important aspects of the field. His meta-critical and meta-historical perspective is direly needed to subvert, question, and extend the self-assurances not only of early American Studies, but of historiography as a whole. Whiggism prospers. At least this is, in short, his contention, the implications of which he illustrates in great detail in his treatment of research in early American history and literature.

Regardless of how one responds to his rather critical position, he identifies a difficult issue that defines large parts of American studies either implicitly or explicitly, namely, the persistence of nationalist attitudes in contemporary historiography (23). He is also to be commended for consistently arguing from a meta-historical perspective, aiming to rectify the frequent methodological and theoretical deficiencies of contemporary American studies. On the whole, it is remarkable that such a meta-historical perspective is more present in contemporary history. With its focus firmly on early American studies, the work is all the more valuable, since the field has been somewhat neglected in the U.S. and even more so in Europe, as he points out in the previously published third chapter, "Marginalizing Early American Literature."<sup>22</sup> In that context he accuses many literary, philosophical, and his-

<sup>21</sup> Cf. Eric H. Ash, *The Draining of the Fens: Projectors, Popular Politics, and State Building in Early Modern England*. Johns Hopkins Studies in the History of Technology (Baltimore, MD: Johns Hopkins UP, 2017).

<sup>22</sup> Richard De Prosop, "Marginalizing Early American Literature," *New Literary History* 23 (1992): 233-65.

torical treatments of early America of “presentism” (as does his ninth chapter “Before/Beyond Multiculturalism”, 168); the third chapter reiterates the state of methodological criticism of early American studies of the late 1980s, which was, according to De Prospro, a period of persistent condescension toward early American literature (38; 43), and the moment when several attempts at taking stock of the field were published.<sup>23</sup> In this sense, his study also raises the question of whether another moment of taking stock of the field has come, or whether too much of it is still subject to the “Neoromantic” biases De Prospro had already diagnosed in the 1990s (36).

He thereby touches upon questions not only of research, but also of education and dissemination of knowledge through canonization; he indicates a long-term perspective for early American studies that highlights the significance of scholarly re-editions and reflects on the currently increasing efforts at reshaping the field through shifts of emphasis in anthologies, biographies, and textbooks. William C. Spengemann’s meta-historical criticism serves here as the major inspiration and source of De Prospro’s own reflections, evident in his fourth chapter, “The Tain(e) of/in Spengemann’s Mirror.” This chapter raises the question whether his engagement with philosophy instead of theology is the right avenue to solve the problems he identifies. Nonetheless, his thoughtful, if brief, engagement with Spengemann’s work is certainly impressive, and so is his reassessment of American literary history from the perspective of early American studies. His project as a whole engages with the nineteenth-century historical progressivism still pervasive in American studies and historiography despite the inroads of social and cultural history, multiculturalism, and cultural relativism. In an erudite examination of the methodological premises of a wide range of publications, too numerous to be mentioned even in passing here, he demonstrates the

pervasiveness with which the consideration of early American studies is still relegated to the status of a prehistory of a later flourishing. As his careful analysis demonstrates, despite the shifts that have swept through the landscape of American studies since the cultural turn, the methodological deficiencies of progressivist assumptions abide. His criticism is hence in line with the many voices that see a persistence of nationalist paradigms behind the mask of multiculturalism, transnationalism, and the cultural turn; yet his focus is early American studies, and his goal is to revive a philology that keeps methodologically unsound historiography and superficially historical literary studies in check. This is a bold and demanding project, articulated by a voice which looks to improve the quality of research by resettling its biases.

His work expresses a dissatisfaction with the insularism of American exceptionalism that still colors the field. One may doubt whether his polemic style helps his project, though it certainly does help to focus his critique. In “Romancing the Medieval,” his second chapter, he seems to have set himself a daunting task: identifying general tendencies of modern interpretations of the medieval; it is not always entirely clear how his reassessment of nineteenth- and early twentieth-century critical concerns relates to present research on early American studies. In criticizing both the partiality of empiricism especially in early American studies (15-17) and the “‘myth-and-symbol’ theorists,” for which Perry Miller is considered the archetype, he points to the problems with dialectics as an explanatory model for our (post-)modern relation towards the medieval. His astute criticism of early American studies as practiced in the U.S. raises the question whether similar tendencies may be observed in many other “national” or “proto-national” historiographies. He points out that the relationship between different historical facts is semiotic, not causal, and that most historical misconceptions are based on this faulty premise. De Prospro’s main target seems to be the difference between the pre-modern and the modern, since he believes that continuities and not ruptures (20) are the appropriate historical explanatory models. He calls for a fundamental rethinking of established historiographical premises. In this process, early American studies assume the special function of examining cultures that developed during the presumed turn to mo-

<sup>23</sup> David D. Hall, “On Common Ground: The Coherence of American Puritan Studies,” *William & Mary Quarterly* 44 (1987): 305-51; Philip Gura, “The Study of Colonial American Literature, 1966-1987: A Vade Mecum,” *William & Mary Quarterly* 45 (1988): 305-51, with answers by Norman Grabo, David Levin, and Larzer Ziff; reprinted in *ibid*, *The Crossroads of American History and Literature* (1996): 12-54.

dernity, which one sees in his ninth chapter on Paine and Sieyès. He believes the failure of much of early American studies is indicative of the difficulties with conceptions of the premodern per se (21-22).

De Prosopo also vents his dissatisfaction with present-day research in American literature at large. He refers to “some very prominent scholars of American literature who have detected that American ethnocentrism carries with it an (un)theoretical dimension that is disabling American literary scholarship” (48). His perceptive engagements with “hyper-canonical” authors like Alvar Núñez Cabeza de Vaca, Mary Rowlandson, William Bartram, Charles Brockden Brown, Thomas Paine, Emmanuel Joseph Sieyès, and Thomas Jefferson, Poe and Twain raises the question why De Prosopo does not simply focus on areas he claims are neglected or on creative methodological innovation instead of criticizing the current research; instead of critique, he could further the field constructively by directing his attention elsewhere—on the other hand, his work may be a necessary precondition for new directions of research by identifying current weaknesses.

His work is evidence of a wide range of experience and a long career in scholarship, and also urges us to rethink the choice of research topics more generally: eliminating biases in research also calls for methodological shifts. A further merit of this book is that it makes us more aware of the intricate link between our choice of research topics and the methods we use. He is spot-on in targeting the self-affirming tendentiousness of methodologically biased research.

This thought-provoking and comprehensive book, which constitutes a critical and unbiased take on a field mired in self-complacency, merits close attention by anyone wanting to reconsider not only early American cultural history. It reminds its readers to reflect on the larger preconceptions behind historiographical discourses and to methodologically rethink the significance attributed to the various fields of literature, philosophy, historiography, and theology in engaging with early American studies. If it achieves this goal, it will be an exemplary case of polemic historiography turned useful.

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