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Yes We Can: Working Together toward a History of Homicide that is Empirically, Mathematically, and Theoretically Sound

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Yes We Can: Working Together toward a History of Homicide

that is Empirically, Mathematically, and Theoretically Sound

Randolph Roth

I would like to thank the Editorial Board of *Crime, History, and Societies* for giving Pieter Spierenburg and me an opportunity to discuss my book, *American Homicide*. As I anticipated, Pieter's essay is an aggressive defense of the "civilization" thesis of Norbert Elias, a thesis which Pieter has advanced in creative ways. At the end of my essay, I will suggest ways in which my findings on the history of homicide are compatible and incompatible with Pieter and Elias's thesis. Before I do, however, I would like to help readers better understand the thesis of *American Homicide* and its statistics.

Spierenburg's critique – like the critique by Jill Lepore, which he paraphrases at length – begins with the claim that *American Homicide* is an "extension" of Gary LaFree's theory of homicide. Obviously I admire LaFree's work, but his theory in *Losing Legitimacy* (1998) about the correlation between murder and trust in government only confirms one aspect of a more comprehensive theory of homicide that I developed independently[1]. The theory of homicide in *American Homicide* is not a theory about legitimacy alone. Low homicide rates have correlated with at least three other variables: the belief that government is stable and that its legal institutions will protect lives and property; patriotism, empathy, and fellow feeling arising from racial, religious or political solidarity; and the belief that the social hierarchy is legitimate and enables men to attain a satisfactory position in society and to command the respect of others without resorting to violence. The theory is thus about nation-building in its broadest sense. "America became homicidal in the mid-nineteenth century because it was the only major Western country that failed at nation-building"[2]. Successful nation-building requires more than legitimate government. It requires comity among elites, strong institutions, security, inspired leadership, and a sense of community that transcends differences over religion, gender, class, race, ethnicity, etc. It is understandable that Spierenburg and Lepore should have focused narrowly on the theme of trust, given the centrality of that issue to public debate in contemporary societies. But in doing so they grossly distort the theory, which is about the homicidal consequences of living in states that to one degree or another failed at nation-building.

Another difference between *Losing Legitimacy* and *American Homicide* is that LaFree's study conflates the causes of different kinds of homicide. The conflation is understandable, because his study relies only on contemporary data. The rates of all kinds of homicide (including family and intimate murders) increased in the 1960s

[1] Roth (2009, p. 16).

[2] Roth (2009, p. 384).

and 1970s in the United States. But the patterns followed by marital and romance murder rates have always been different from the patterns followed by murder rates among unrelated adults, and the two types of murder have different causes. The rise in marital and romance murders in the 1960s and 1970s, like the rise that occurred in the early nineteenth century, had more to do with changes in gender relations and the economic balance of power between men and women than it did with any contemporary political crisis. And murders among adult relatives followed yet another pattern. That is why *American Homicide* reports the rate for each type of homicide separately, rather than "sum" those rates to obtain a total homicide rate, as Spierenburg would prefer. The "total" homicide rate would be a meaningful statistic if the rates of various kinds of homicide were to go up and down together, as the civilization theory in its boldest form predicts. But they do not.

My theory about homicide grew out of evidence I had gathered on northern New England in the late 1980s and early 1990s. When I separated by type the homicides I had found in New Hampshire and Vermont, I discovered that the patterns of homicide made sense in terms of New England's history. Murders of children by adult relatives or caregivers followed a long, smooth curve that was the inverse of the birth-rate : high fertility meant a low child murder rate and low fertility meant a high murder rate[3]. Marital homicides and romance homicides jumped suddenly in the 1830s and 1840s : decades in which jobs opened to women in education and industry, in which self-employment declined for men, and in which the ideal of companionate marriage took hold. Homicides among unrelated adults peaked during periods of political turmoil : the Revolution, the Embargo crisis, and the sectional crisis. It appeared that "state breakdowns and political crises of legitimacy produce surges in nondomestic homicides and that the restoration of order and legitimacy produces declines in such homicides." The same pattern was evident on the national level in the twentieth century, for which comprehensive homicide statistics were available. "The theory can be extended to the twentieth century : the crisis of legitimacy in the 1960s and 1970s (especially in the eyes of African-Americans) may have contributed to soaring homicide rates ; and the establishment of state legitimacy through the New Deal, World War II, and the Cold War may have reduced homicide rates through the 1950s"[4]. The idea that there was a relationship between crime and feelings toward government and society dawned upon a number of scholars independently in these years, including Gary LaFree, criminologist Manuel Eisner, and sociologist Roger Gould[5].

I knew, however, that it would take more to prove my theory than evidence drawn from the history of Vermont and New Hampshire, which is my area of expertise. Therefore I put my theory at risk against a wider range of evidence. I extended my research to the colonial period, to early modern Europe, and outward to the South, the Midwest, the West, and the urban East. Everywhere I looked, the domestic murder rate for children followed the inverse of the birth rate up to the end of the nineteenth century. Marital and romance homicides increased suddenly in the 1830s and 1840s across the northern United States, and in England and northern France. Everywhere I looked, homicides among unrelated adults correlated with political

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3 Roth (2001a ; 2001b).

4 Roth (1997, pp. 7-8).

5 LaFree (1998) ; Eisner (2001) ; Gould (2003).

events. I conducted "natural experiments" to prove that correlation. I hypothesized, for instance, that the homicide rate would soar during the American Revolution and remain high for decades afterwards in the Georgia-South Carolina backcountry, where the Revolution was a genuine civil war. I also hypothesized that the homicide rate would hold steady or fall in the Shenandoah Valley of Virginia, which enjoyed political stability under patriot control throughout the Revolution, and where support for the war effort and the new federal government was stronger than anywhere else in the South. My research in local archives confirmed these and other hypotheses. And every quantitative measure I could find of changes in people's feelings and beliefs supported the theory. Consider, for instance, the inverse relationship between the homicide rate among unrelated adults in the United States and the proportion of new counties named after national heroes – British heroes in the colonial period and American heroes in the national period. When that proportion was high – an unconscious way of saying that Americans believed in their nation and in each other – the homicide rate among unrelated adults was low. And when that proportion dropped, the homicide rate soared. The turning points in these measures also coincide with the turning points in patriotic feeling that have been identified by humanistic historians such as Lovejoy, Pincus, Colley, and Waldstreicher: powerful evidence for the importance of fellow feeling[6].

The correlation between political stability, legitimacy, fellow feeling, and everyday homicides has been evident at many times and in many places. Every period of political instability in nineteenth-century France – 1830-1831, 1848-1850, 1870-1871 – saw a spike in homicide, not just in places that were engulfed in revolutionary violence, but in places remote from such violence. As Roger Gould discovered, the homicide rate spiked in the same years even on the island of Corsica, where there were no politically motivated homicides, only an increase in feud violence and honor killings. In England, the frustration of the democratic aspirations of working people in the wake of the Napoleonic wars led to a doubling of the homicide rate after the massacre of voting rights demonstrators at Peterloo in 1819 and to a sustained high rate through the years of Chartist agitation. But when the Second Reform Act passed in 1867, enfranchising propertiless household heads in urban areas, the homicide rate fell suddenly by half; and when the Third Reform Act passed in 1884, enfranchising propertiless household heads in rural areas, the homicide rate fell suddenly by half again[7]. Such patterns have appeared time and again in the history of Western nations over the past 450 years, so often that they show the relationship between nation-building and homicide must be causal, even though few homicides are motivated directly by political conflict or political feelings[8].

Spierenburg questions my decision to include political homicides and law enforcement homicides in my study, but I believe it is important to study all kinds of

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6 Lovejoy (1972); Pincus (1998, 2009); Colley (1992); Waldstreicher (1997).

7 Gould (2003, pp. 150-161); Roth (2009, pp. 243-249; 297-299 & 436-437).

8 Spierenburg claims erroneously that I ignore the importance of class and of potentially divisive political movements like socialism. I discuss, for instance, the impact on homicide rates of political polarization between monarchists and leftists in late nineteenth-century France and of the intense political polarization that occurred in many continental European nations following World War I (Roth (2009, pp. 298-299 & 436-437). And class is a major theme in *American Homicide*, as my discussions of indentured servitude, slavery, the decline in self-employment, and the rise of wage work attest.

homicides so we can determine empirically which kinds of homicide are correlated with one another. My statistical tables (as well as the spreadsheets and case notes I have posted on-line) distinguish between politically-motivated homicides, legal interventions, and other kinds of homicide among unrelated adults (feuds, bar fights, etc.). The tables show that the rates for these kinds of homicide go up and down together and that they do not go up and down with the rates for family or intimate homicides.

Next, I would like to speak about the statistics in *American Homicide*. Spierenburg goes to great lengths to try to discredit my statistics, because he recognizes the threat they pose to the civilization thesis. He objects no less than three times to the finding that the northern United States and the mountain South had a remarkably low homicide rate between the War of 1812 and the Mexican War – a finding that nullifies his argument that the United States is comparatively homicidal today because it was a "premature" democracy[9]. He objects twice to the finding that the homicide rate was moderate to low throughout British North America from end of the Glorious Revolution to the end of the Seven Years War, which also nullifies his argument. These findings, however, are not mine alone[10]. Everyday homicides were rare in these periods, and political and communitarian violence (except for a brief period in the mid-1830s) was seldom lethal. Spierenburg's version of the civilization thesis cannot entertain the possibility that a society in which the state did not enjoy a "monopoly of force" and in which the majority of citizens were armed had a low level of interpersonal violence. But in fact, homicide rates in America were low relative to rates in Europe and Canada from the mid-1690s through the early 1840s, except during the years of the revolutionary crisis (1765-1789) and in the post-revolutionary slave South.

My colleagues and I arrived at that conclusion by studying multiple sources and by embracing the mathematical tools of modern demography, epidemiology, and criminology : matching-list (or capture-recapture) analysis, sampling theory, disaggregated rates, and age-specific rates. Spierenburg and Robert Dykstra, who he quotes extensively, are having none of it, but their criticisms have no merit. I do not have space to respond to their criticisms in full, so I hope readers will refer to the essays by Eric Monkkonen, Douglas Eckberg, Michael Maltz, Cornelia Hughes Dayton, Kenneth Wheeler, James Denham, and myself, in which we present our methods and respond to Dykstra's misguided attacks[11]. I believe, however, as did the leader of our working group, the late Eric Monkkonen, that it is possible to put the study of homicide on a sound basis, mathematically and empirically.

I will take up Spierenburg's criticisms in turn and in so doing outline the methods my colleagues and I use :

1. **Use of multiple sources**: There is only one way to obtain reliable homicide estimates, and that is "to review every scrap of paper on criminal matters in every courthouse, every article in every issue of a number of local newspapers, every entry

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9 Spierenburg (2006).

10 Gilje (1996) ; Lane (1997).

11 Monkkonen (2001) ; Eckberg (2001) ; Denham, Roth (2007) ; Roth (2001a ; 2007) ; Roth *et al*. (2008, 2011).

in the death records, and every local history based on lost sources, local tradition, or oral testimony"[12]. For every state or county I studied personally, I examined every source I could find, including journals and diaries for places and periods when newspapers were not available; and I kept a careful record of which sources were available for each suspected homicide. Full bibliographical references are available in the case notes I posted on the Historical Violence Database. In addition, each source is coded at the head of the notes for each case ("INQ" for inquest, "VIT" for vital record, etc.). Spierenburg's claim that I do not identify the sources for each case is not accurate.

I also assess the reliability of the evidence for each incident – whether it was "certain" a homicide occurred, "probable," or "uncertain". That assessment is coded on the notes for each case and in the statistical spreadsheets posted online, so other scholars can calculate the homicide rate using various criteria for including or excluding cases. I decided in the end to exclude cases I considered "uncertain." The number of uncertain cases was small, however, and their inclusion, I discovered, would have had no significant impact on homicide rates.

Body inspection reports are not, as Spierenburg claims, inherently more complete or reliable than indictments or other sources. They must be evaluated on a case-by-case basis. My case notes include testimony at coroner's inquests, entries from diaries, texts of newspaper reports, etc., so scholars can make their own evaluations, rather than be bound by mine.

2. **Homicide estimates using matching-list (or capture-recapture) mathematics**: Spierenburg objects to the way that Monkkonen, Eckberg, and Roth[13] use matching-list mathematics to estimate the degree to which the surviving sources understate the number of homicides that came to the attention of the authorities or the public. The technique, however, is standard and widely used among demographers, epidemiologists, and criminologists. It has been used successfully to estimate death rates for young children in Egypt[14] HIV-infection rates among drug addicts in Thailand[15], and other vital or epidemiological rates in societies that lack effective means of registration or reporting[16]. It was created to account for data that are missing because of record loss or recording failures – problems every historian of violence faces. The method depends on creating at least two lists of homicides that are largely independent statistically, because the degree of overlap between the two lists determines the degree to which the surviving sources understate the number of homicides. A high degree of overlap means that most homicides probably appear in the surviving record; a low degree of overlap means that many probably do not.

The best way to ensure the statistical independence of the lists, as Eckberg[17] demonstrates, is to compile one list from legal sources and another from non-legal sources. Legal sources are not statistically independent from one another, so they belong on the same list. For instance, if a coroner's inquest is available, there is

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12 Roth (2009, pp. xi-xii).

13 Monkkonen (2001); Eckberg (2001); Roth (2001a).

14 Becker *et al*. (1996).

15 Mastro *et al*. (1994).

16 E.g., Crimmins (1980); Hook, Regal (1995).

17 Eckberg (2001).

roughly a two-thirds chance that there will also be an indictment; and if there is an indictment, it is almost certain there will be an inquest, if the inquests survived. Experience has shown that legal and non-legal sources are largely independent statistically: an appearance in a newspaper, diary, or local history is no guarantee that a homicide will show up in the legal record, or vice versa. Our decision to make lists from legal and non-legal records is not arbitrary, as Spierenburg claims, but mathematically and empirically sound: the accepted method among statisticians. That is why the sources for each homicide I studied are coded on the statistical spreadsheets available on the Historical Violence Database as legal, non-legal, or both.

Thus, if legal and non-legal sources are available, we can estimate the number of homicides that came to the attention of the public. The list from legal records (inquests, case files, docket books, minute books, and prison records) and the list from non-legal records (newspapers, diaries, oral tradition recorded in early town histories, etc.) are matched to determine the number of homicides that appeared on both lists (C), on the list from legal records only (N_L), and on the list from non-legal records only (N_{N-L}). Following the method of Chandra Sekar and Deming[18], as adapted by Eckberg, the proportion of homicides missed by both lists (X) can be estimated thus:

$$X = (N_L * N_{N-L}) / C$$

The result of that equation can then be used to estimate the number of publicly recognized homicides (N) that occurred: the sum of the number found only in legal records (N_L), the number found only in non-legal records (N_{N-L}), the number found in both kinds of records (C), and the number missed by both lists (X).

$$N = N_L + N_{N-L} + C + X$$

The method is robust. It does not require that the evidence from which the lists are drawn be comprehensive or complete, so long as the loss of records and the omissions of record keepers are largely random.

Matching-list mathematics does not, as Spierenburg claims, inflate the number of homicides that occurred the farther we go back in time (although such inflation would support Spierenburg's thesis, not mine). The estimate of the proportion of homicides that appear in the surviving sources goes up and down with the history of record loss and recording failures. Spierenburg also objects to the fact that non-legal sources outnumber legal sources in the seventeenth century, but that is simply because a large proportion of perpetrators escaped prosecution and because some legal records have not survived. These are precisely the circumstances in which matching-list mathematics is useful.

New England records provide a good example of what matching-list mathematics can do. The records of early New England are rich, because the legal records are largely intact for each colony and because many settlers wrote histories or kept journals or diaries before the advent of newspapers in the eighteenth century. Thus, for most of the colonial and revolutionary period, the estimated proportion of homicides that appear in the surviving record in New England is high. But that proportion was only 55 percent in the 1650s and 1660s – the decades for which the court records of

[18] Sekar, Deming (1949); Eckberg (2001).

Massachusetts, the most populous colony, are largely lost – and 79 percent during the American Revolution, when courts met less regularly, local newspapers occasionally suspended publication, and perpetrators had a greater chance of escaping prosecution.

Changes in homicide rates rarely coincided with changes in record loss or recording failures; and in no instance were the changes in record loss or recording failures large enough to cause a major change in homicide rates. The pattern of homicide in early New England became clear once the number of homicides was adjusted to reflect record loss and recording failures. The homicide rate for every five- or six-year period between 1637 and 1675 ranged between 7 and 9 per 100,000 adults per year; from 1676 to 1692 between 4 and 5 per 100,000, and from 1693 to 1769 between 1 to 2 per 100,000. Without matching-list mathematics and multiple sources, that pattern would have been obscured, and the abruptness with which homicide rates fell in 1675-1676 and 1692-1693 would have gone unnoticed.

3. **Homicide counts from single sources**: Sometimes we must rely on a single source, such as indictments or inquests, to arrive at a minimum count of the number of homicides that occurred, or we must rely on the work of scholars who consulted only a single source. In every instance, *American Homicide* states whether a rate was derived from matching lists, from a count from multiple sources, or from a count from a single source; and in no instance did it misrepresent an indictment rate as a homicide rate to create a false impression of a low homicide rate. As I state clearly in the appendices, I found through experience, as Roger Lane did before me, that indictment rates generally go up and down with the actual homicide rate and that it is a good rule of thumb to assume that homicide rate was at least 50 percent higher than the indictment rate. That is why *American Homicide* assumes that the homicide rate was at a least 50 percent higher than the indictment rate, if only indictment rates are available[19]. I made a similar calculation to determine the difference in England and Wales between the number of homicides known to the police (a statistic first available in the late nineteenth century) and the number of persons jailed for homicide (a statistic available for the entire nineteenth century). The mean and median difference between the two rates was 33 percent from 1868 to 1914, with a range from 10 percent to 50 percent: near symmetry. I therefore multiplied the homicide committal rate from 1810 to 1867 by 1.33 to estimate the number of homicides known to the police for the earlier period[20]. I would have preferred to have had more than indictment statistics for colonial Pennsylvania and police statistics for nineteenth century Great Britain, but I always used multipliers – explicitly and transparently – to estimate the probable homicide rate from such statistics. Spierenburg's claim that I used indictment rates from colonial Pennsylvania to deflate the homicide rate in eighteenth-century America is just not true.

4. **Age-specific homicide rates**: Spierenburg and Dykstra claim that I "inflated" the homicide rates in *American Homicide* by reporting rates per 100,000 adults per year. Homicide rates for adults are generally higher than homicide rates for entire populations, since children are generally murdered at far lower rates than adults. But

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19 Roth (2009, p. 492).

20 Roth (2009, p. 543 n.135).

it is vital to use adult rates, because jurisdictions varied widely in their willingness to report homicides of children and because the proportion of children in the population varied widely from place to place: from only 5 to 10 percent in cattle and mining towns to 50 percent on newly settled agricultural frontiers. If adults were murdered at ten times the rate that children were – a plausible figure – the total homicide rate for cattle and mining towns would appear to be nearly twice as high as the rate on newly settled agricultural frontiers, even if adults and children were murdered respectively at the same rates in both communities. That is why criminologists and epidemiologists calculate age-specific and age-adjusted rates as a matter of course[21]. Historians must do the same when they focus on violence among adults. As long as we compare adult rates to other adult rates, there is no inflation, only greater comparability[22].

5. **Representativeness and reliability of homicide rates**: I agree with Spierenburg that we must consider the population totals of the jurisdictions we have studied to determine whether they are of sufficient size to allow us to make statistical interferences about jurisdictions with similar qualities that we have not yet studied[23]. But Spierenburg goes farther. He claims, citing Robert Dykstra as an authority, that homicide rates for places with small populations are inherently unreliable, so they cannot be used to make inferences about rates in particular regions or types of communities. Dykstra's assertion, however, is completely unfounded[24]. Homicide rates in places with small populations are unstable from year to year, because one or two homicides can cause a substantial change in an annual homicide rate. But homicide rates for places with small populations stabilize if we study those places for a sufficient number of years, because mathematically, the stability of homicide rates does not depend on the size of the population but on the average number of persons in the population *times* the number of years of exposure. That is why homicide rates in *American Homicide* are reported for spans of years during which the homicide rate was fairly steady, rather than for individual years.

Consider the four mountain counties in *American Homicide*. Together they had a small average adult population – roughly 14,000 – but the number of adult years-of-exposure in those counties from the end of the Civil War to 1900 was 456,048 (the data for Taney County, Missouri, are available only through 1886). If these counties in Missouri, Georgia, Kentucky, and Tennessee were representative of

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21 The Bureau of Justice Statistics, Department of Justice, supports an on-line database that allows researchers to calculate age-specific homicide rates in the United States from 1976 to the present [http://bjs.ojp.usdoj.gov/dataonline/Search/Homicide/State/StatebyState.cfm]. The Centers for Disease Control supports a similar database called "CDC Wonder," which allows researchers to calculate age-specific mortality rates in the United States for various causes of death, including homicide, from 1979 to the present [http://wonder.cdc.gov/mortSQL.html].

22 Roth, Maltz & Eckberg (2011).

23 I apologize that we have not yet had time to post population figures on the Historical Violence Database, but the basic population statistics that are necessary to analyze the data from *American Homicide* are available online through the Millennium Edition of the *Historical Statistics of the United States* from Cambridge University Press, the Inter-University Consortium for Political and Social Research at the University of Michigan, and the Integrated Public Use Microdata Series at the University of Minnesota.

24 Roth, Maltz, Eckberg (2011).

counties that experienced political strife during the Civil War – and there is every reason to think they were – and if we assume that their inhabitants were a representative (if not strictly random) sample of the inhabitants of such counties in the South, we can use the laws of probability to develop a rough confidence interval for the homicide rate in *all* politically-divided mountain counties.

To estimate a 95 percent confidence interval, we must estimate the lower and upper bounds for the number of homicides that occurred:

$$\text{homicides} +/- (1.96 \sqrt{\text{homicides}})$$

Here "homicides " stands for the number of homicides in the four mountain counties in the South that have been studied (151); and "1.96" stands for the t-value used to calculate a 95 percent confidence interval if "n" is large, as it is in this case.

$$151 +/- (1.96 \sqrt{151})$$

$$151 +/- 24.1$$

Divide the lower and upper bounds of homicides (126.9 and 175.1, respectively) by the number of adult years-of-exposure (456,048) and multiply by 100,000, and the 95 percent confidence interval is between 27.8 and 38.4 per 100,000 adults per year: a very high range. The probability that the homicide rate in mountain towns in the postbellum South was low or moderate by the standard of most nineteenth-century Western societies – less than 10 per 100,000 adults per year – is vanishingly small.

The idea that one can conclude anything from one hundred and fifty-one homicides in four counties with a combined annual average adult population of 14,000 is hard for non-mathematicians to accept. That is why Dykstra's criticisms have gained such credence in the profession at large: not many historians are at ease with statistics. Spierenburg seized upon the mountain counties in my study because they comprise by far the smallest "sample" of any type of jurisdiction discussed in *American Homicide*. He was certain he had found an error in my work that could discredit the whole. But if the four counties are representative, as they almost surely are, those numbers are large enough to set plausible bounds on the homicide rate in *all* politically-divided mountain counties in the South[25].

Of course, the average populations of the cities and counties studied elsewhere in *American Homicide* were far larger than those for the four Southern mountain counties. Consider the rate that Spierenburg considers the greatest threat: the low homicide rate in the northern United States from the end of the War of 1812 to the Mexican War. The number of adult-years-at-risk in that calculation – based on thirty counties in New England and the Midwest plus New York City – is 17 million. And that low rate – based on multiple sources and matching-list analysis – is consistent with the indictment rates available for other jurisdictions in the North, including Boston, Philadelphia, and Cincinnati. The chance, statistically, that the homicide rate in the North will prove to be moderate or high once all the data are collected is nil.

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[25] Roth, Maltz, Eckberg (2011).

6. **Measures of feelings and beliefs**: Spierenburg takes me to task for relying on political and cultural historians to gauge the feelings and beliefs of people in the past, even though his work (and Elias's) relies on the same non-quantitative research. I acknowledge in the preface to *American Homicide* that we as yet have few quantitative measures of feelings and beliefs, and Spierenburg has every right to disagree with me when I say that I think the work of political and cultural historians sheds more light on the feelings and beliefs of the people they study than any opinion poll could. But I used every quantitative measure I could find of feelings toward nationality, marriage, and romance, and those measures support both the humanistic scholarship on identity and intimate relationships and the theory of homicide in *American Homicide*. And Americans left a lengthy record of their feelings and beliefs in their votes. Spierenburg mocks my interpretation of the impact of the Mexican War and the Kansas-Nebraska Act on the feelings and beliefs of Americans, implying that I distorted the historical record to fit the homicide rates I discovered. Nothing could be further from the truth. Historians have long known that the Mexican War and the Kansas-Nebraska Act were divisive, except in slave counties in the Deep South, where whites rallied to support slavery and where proto-nationalist feelings would soon blossom into support for the nascent Confederacy. There is no mystery, in my opinion, why homicide rates, like feelings toward the nation and the federal government, followed different paths in the North, the Upper South, and the Lower South in the late 1840s and 1850s.

Since the publication of *American Homicide* I have been working, as I promised, to find new measures of feelings and beliefs; and in every instance, these measures have confirmed the intuition and insights of the humanistic historians on whom I relied. I plan to publish an essay on these measures soon, but consider, for a moment, the measures that were made available last year when scholars at Harvard University teamed with Google to create "Google Labs Books Ngram Viewer" – an interface that allows researchers to trace the incidence of words or phrases in books that Google has scanned into its database from 1500 to the present[26]. *American Homicide* argues, following the work of Kathleen Brown and others, that there was little sense of fellow feeling among European colonists until the late seventeenth century, because of deep divisions over class, religion, and morality; and that those divisions were overcome in the late seventeenth as colonists began to identify with each other on the basis of race with the advance of racial slavery. Ngrams shows that Brown was right: the words that she says were used to signify class hatred – "rogue" and "whore" – were commonplace in American publications in the mid-seventeenth century, but they disappeared in the late seventeenth century as the words "slave" and "negro" rose to prominence and the homicide rate among unrelated colonists crashed. *American Homicid*e also argues, following Frederickson and McPherson[27], that divisions over slavery undermined fellow feeling in the late 1840s and intensified hatred of African Americans. They too were right: the word used to signify racial hatred – "nigger" – came to the fore as the nation divided during the Mexican War, peaked during the Civil War and Reconstruction, declined in the late 1870s and 1880s, and returned with a vengeance in the 1890s – a perfect map of the ups and

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[26] The Ngram Viewer is at [http://ngrams.googlelabs.com/]. On the interface and database, see Michel *et al*. (2011).

[27] Frederickson (1971); McPherson (1988).

downs of the homicide rate among unrelated adults in the United States, particularly homicides among whites and of blacks by whites.

What can we say then about the civilization thesis, once we put the history of homicide on a firm footing, mathematically and empirically ? Much of what we can say is positive, as I noted in *American Homicide* and its on-line supplement. Spierenburg, Stephen Mennell, and Elias are right : state formation plays a role. Without political stability, homicide rates can soar into the tens or hundreds per 100,000 per year. But Elias's theory[28] must be modified, I believe, to incorporate the scholarship on nation-building that has emerged since World War II, which sees nation-building as a far more complicated process than establishing a "monopoly" of violence and taxation. Strong nations (or strong multinational or multiethnic alliances) do not have to have strong central governments to deter homicide if they establish legitimate institutions, create a sense of common interest and purpose, and help people feel secure. This means, of course, that failures at nation-building will send homicide rates soaring in the future, just as they have in the past – a conclusion that conflicts with the meta-story of the civilization theory, that interpersonal violence will decline over the long term as states become more powerful and as citizens are disarmed. But such a revision of the civilization thesis will help scholars understand why homicide rates soared in central Europe at the end of World War I and again at the end of World War II, just as they did in the United States in the mid-nineteenth century, in France during the Revolution, and in medieval Europe after the Black Death. And such a revision is consistent with Elias's broader theory, which emphasizes the importance of "human emotions"[29]. Feelings play a crucial role in the success or failure of nation-building. Spierenburg, Mennell, and Elias's theory would be strengthened if those feelings were taken into account.

The problem with Spierenburg and Mennell's interpretation of violence in the United States is empirical as well as theoretical. Consider, for instance, the low rates of interpersonal homicide, collective violence, and vigilantism that prevailed from the Glorious Revolution to the end of the Seven Years War. Spierenburg and Mennell discount these rates – which were first described in Brown, Gilje, and Lane[30] – because they define colonial America as a "weak" state that had not yet established a monopoly of violence. It relied on state militias and had a citizenry that "did not have sufficient time to become accustomed to being disarmed"[31]. According to their theory, colonial America had to have a high level of interpersonal violence, much of it gun violence.

Most historians, however, would characterize the seventy years from 1693 to 1763 as a period of successful state formation in colonial America, when the colonies were integrated successfully into the powerful British state. The colonies enjoyed remarkable political stability and a near-complete absence of political rebellions. The colonies' struggle for "co-possession" of central authority (Spierenburg's term)[32] was successful, as the imperial government acknowledged – however

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28 Mennell (2007, pp. 11-18).

29 Spierenburg (2006, p. 105 n.6).

30 Brown (1975) ; Gilje (1996) ; Lane (1997).

31 Spierenburg (2006) ; Mennell (2007, pp. 1 & 122-157).

32 Spierenburg (2006, p. 109).

grudgingly – the rights and powers of colonial assemblies. Colonial militias, which enrolled up to a third of the adult male population in times of crisis, fought alongside regular British troops or, in their stead, in pursuit of British war aims. Colonial governments financed improvements in education, finance, communications, and transportation, and collected revenues effectively. These successes came at a time when over half of all households owned guns, which were used for hunting, sport, vermin control, and defense. And even though guns were everywhere, they were used in only a tenth of all homicides, domestic and non-domestic[33]. Spierenburg and Mennell could defend the core of their "civilization" thesis by acknowledging that nation-building is possible without a monopoly of violence. But they decline to do, because they are wedded to Elias's restrictive state-formation theory and because they believe erroneously that guns, not the failure of nation-building in the mid-nineteenth century, are largely responsible for American violence today.

The evidence is less kind to Elias's theory about the relationship between cultural change and interpersonal violence. Elias, in Mennell's words, "linked changes in people's everyday behavior, in the codes of manners they followed and in their typical feelings and emotions, to the formation of states with relatively effective monopolies of violence"[34]. Over time, "social pressure towards self-constraint" tilted the balance "in favour of more demanding social standards of habitual self-control" and encouraged people to feel shame, embarrassment, disgust, or repugnance when those standards were violated[35]. *American Homicide* notes, however, that the diffusion of refined manners and the rhetoric of self-mastery and restraint, which Elias considered both causes and consequences of the civilizing process, had little impact on the level of violence. These cultural changes took hold in colonial America in the mid-eighteenth century, after the homicide rate had already fallen, and they failed to restrain interpersonal violence when the political order lost legitimacy and destabilized in the late 1760s and early 1770s. In the early eighteenth century, words like "polite," "manners," and "refinement" assumed a more prominent place in British and American print, according to Google Ngrams. Colonists turned increasingly to civil courts to resolve disputes over slander, trespass, assault (which included threats and verbal abuse), and assault and battery (which included physical violence). Changes in genteel manners fostered an appreciation of men who used wit rather than force to triumph over adversaries in politics and in personal disputes. Humor, self-deprecation, control of one's temper, and attentiveness to the feelings of others became the hallmarks of the gentleman. Politics became less combative. Good manners, conversational skills, and a sharp pen were vital to success in a political world that revolved around coffeehouses, soirees, contested elections, and newspapers. There was less fighting and more satirical writing and repartee, skills that were valued not only by the gentry, but by farmers, artisans, and laborers[36].

The movement toward restraint and refinement was evident as well in quantifiable changes in daily habits and material culture. Between the 1720s and the 1760s, colonists of all classes gradually stopped throwing their garbage into their yards and began to bury it neatly in deep, square garbage pits ; and they began to purchase

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33 Roth (2002, 2009, p. 82).

34 Mennell (2007, pp. 1 & 6-11).

35 Mennell (2007, pp. 1 & 6-11).

36 Roth (2009, pp. 88-90).

chamber pots for the disposal of human waste. They moved away from the practice of eating with spoons out of communal trenchers and drinking out of communal cups, and embraced the modern practice of eating with forks and using individualized plates and cups. They ate fewer stews and potages, and more triparte meals with discrete portions of meat, starch, and vegetables. Archaeologist James Deetz associates these changes with a movement toward "order, control, and balance" and with a movement away from the "medieval" emphasis on community toward a modern emphasis on the individual, which he associates with the adoption of the cultural style of the Georgian-era British elite[37]. Elias's theory would associate these changes with a decrease in interpersonal violence, caused by the increase in self-monitoring and restraint. But as noted, these changes followed the sudden decrease in violence in the late seventeenth century, and they were powerless to prevent an increase in violence during the revolutionary period. Improvements in manners may have been a consequence of successful nation-building and a decline in interpersonal violence, not a cause.

Finally, the evidence does not support Spierenburg's contention that the survival of a "culture of honor" among African Americans and Southern whites is responsible for American violence[38]. Elias's civilization theory hypothesizes that over time, such cultures, which encourage men to brook no insult and to defend their reputations at all costs, will give way to cultures of restraint, which encourage men to believe in their worth regardless of public opinion. There were relatively few honor killings, however, by blacks or Southern whites in the mid-eighteenth century, compared to the nineteenth century, so it is difficult to portray the "culture of honor" or the violence associated with it as a culture that would disappear gradually as the civilizing process advanced. I believe, as did Gould[39], that a readiness to commit violence in defense of personal honor is a hallmark not of a culture, but of a status hierarchy that has lost its stability and legitimacy, or has not yet gained it. Southern whites and blacks became more violent in the decades after the Revolution when their society, based on distinctions of caste and class, was destabilized by democratic ideas and aspirations. Southern blacks became more violent in the 1890s when they were suddenly pushed below the status of their forebears by disfranchisement and legal segregation. Cultures of honor appear in such violent times, but they do not cause violence; they facilitate it.

So where do we stand? Pieter, Stephen, and I will surely debate these matters for the rest of our lives. But to progress, we need precise measures of the rates of various kinds of violence and of the social and cultural changes that may correlate with those rates, so we can test our theories and develop better ones. That will require the use of multiple sources, proper mathematical tools, and the findings of humanistic historians of politics, culture, and society. That is a challenge we can meet.

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37 Deetz (1996, pp. 63, 86 & 185).

38 Spierenburg (2006, pp. 110-113).

39 Gould (2003).

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