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Staying on the Straight and Narrow
Recent Books on Violence, Crime, and the Question of Order in Nineteenth-Century Urban America

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See table of contents

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Staying on the Straight and Narrow:
Recent Books on Violence, Crime, and the Question of Order in Nineteenth-Century Urban America

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John K. Alexander, *Render Them Submissive: Responses to Poverty in Philadelphia, 1760-1800* (Amherst: University of Massachusetts Press 1980).

Gunter Barth, *City People: The Rise of Modern City Culture in Nineteenth-Century America* (New York: Oxford 1980).

Paul Boyer, *Urban Masses and Moral Order in America, 1820-1920* (Cambridge, Ma.: Harvard University Press 1978).

Theodore Hershberg, editor, *Philadelphia: Work, Space, Family and Group Experience in the 19th Century* (Toronto: Oxford University Press 1981).

Jame E. Inciardi and Charles E. Faupel, *History and Crime: Implications for Criminal Justice Policy* (Beverly Hills: Sage 1980).

David R. Johnson, *Policing the Urban Underworld: The Impact of Crime on the Development of the American Police, 1800-1887* (Philadelphia: Temple University Press 1979).

Roger Lane, *Violent Death in the City: Suicide, Accident and Murder in 19th Century Philadelphia* (Cambridge, Ma.: Harvard University Press 1979).

John C. Schneider, *Detroit and the Problem of Order, 1830-1880: A Geography of Crime, Riot, and Policing* (Lincoln: University of Nebraska Press 1980).

CANADIAN HISTORIOGRAPHY IS about to cross another set of trans-Atlantic wakes raised by British and American social historians. To date, international debates over the theories and substance of emergent fields like family, labour, and urban history have enriched the body of Canadian literature. Foreign scholars — if they wished — now could find in a few recent Canadian monographs and journals respectable accounts of the issues and

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methods that have interested them. Very soon there will be a comparable flurry of publication in relation to the criminal justice system including analyses of violence and crime. Douglas Hay has been examining the operation of British criminal justice in the old province of Quebec; Robert Fraser has begun to code the records of the Assizes of Upper Canada. More localized studies on the Gore District of Upper Canada, Victorian Toronto, and the Eastern Judicial District of Manitoba are nearing completion.[1] This is not meant to be a comprehensive list, for the aim of the current review is to consider the methods, insights, and controversies arising from recent American books. Since the criminal justice system of the United Kingdom and some of the British theoretical statements are especially relevant, a few British titles will be mentioned in passing.

Before sorting out current debates, it is worth reporting that one former tempest has been stilled. None of the current books takes a position against quantification, although a few years ago the arch-skeptic of criminal data, J.J. Tobias, was very much a part of any historiographic review on the subject of crime.[2] His legitimate concerns about the problems of working with serial information collected in the nineteenth century have been absorbed by historians whose very mastery of understanding the enforcement system and court procedures enables them to demonstrate the merit of serial information as indirect indices of crimes or violence. The best works employing machine-readable records (police arrest sheets, grand jury indictments, coroners' reports, court records, jail ledgers) display a thorough knowledge of each collecting agency. David Philips' *Crime and Authority in Victorian England* (London 1977) actually contains more accurate reflections on the processes of the criminal justice system than Tobias' traditional work. In a sense, the proponents of quantification have pushed further than their critics in seeking to reconstruct the operations of constables, courts, and jails. This important maturation and acquisition of skills has been promoted by one of the most statistically sophisticated researchers, namely Eric Monkkonen. His *The Dangerous Class: Crime and Poverty in Columbus Ohio, 1860-1885* (Cambridge 1975) was unparallelled in its application of statistical techniques. Nonetheless, in a recent article in *History and Crime: Implications for Criminal Justice Policy*, he has admitted that "given our data limitations and problematic models, the most useful approach for the next decade of research will be in understanding the behaviour, roles, and relationships of organizations." Both the attention to the precise meaning of source materials and an interest in criminal justice organizations have been realized already in several studies on areas in England. For readers interested in savouring deft monographs that have the additional feature of describing the foundations of Canadian criminal justice organizations, I recommend Barbara Hanawalt, *Crime and Conflict in English Communities, 1300-1348* (Cambridge 1979) and David Philips, *Crime and Authority In Victorian England* (London 1977).

To read in the history of disorder, crime, and criminal justice is to witness the influence of wider contrasts between American and British historiography. The latter has a finely-honed tradition of legal

[1] See for example John Fierheller, "Approaches to the Study of Urban Crime: A Review Article," *Urban History Review*, 8 (October 1979); John Weaver, "Crime, Violence, and Immorality in a Pre-Industrial Society: Unlawful Acts and Law Enforcement in the Gore District of Upper Canada from Settlement to the Railway Era," Paper Given at the Canadian Historical Association, Annual Meeting 1981; G.A. Friesen and D. N. Sprague, "Application of Modernization Theory to Crime Data in the Old World and the New," *ibid.*, Gerald Wood, Principal Investigator, Federal Corrections History Projects, Solicitor General's Office, has prepared a list of scholars working on Canadian criminal justice history.

[2] Fierheller, "Approaches," 106.

history as well as a rich body of labour history that poses ideological and theoretic questions. As well, local history has an established acceptance. The American historians of law and order seem strongly attached to explaining routes to the present, analyses of how policing "went wrong," or to probing the myths of American promise by raising questions like the following: Did the American Revolution lift oppression or engender fears that resulted in measures of social control? Has America become more or less violent? Are racism and blocked access to white opportunities fundamental to the history of the Republic, heightening domestic and community tensions? Before reviewing these American queries in depth, the issue of quantification requires summary. On method, though not on historiographic context, British and American scholars almost converge. No serious researcher on either side of the Atlantic now would dismiss quantification or, on the other extreme, claim that their data base captures the real dimensions of crime. Nor would they propose that most crimes are absolute and measurable across time, thereby providing a true gauge of social pathology. Much crime has been and is unrecorded (larceny, rape); much is socially defined or inspired (vagrancy, automotive manslaughter). Curiously enough, the wise concessions made to the understanding of source materials have not quite shaken the claim made by some American researchers that studies of the past will produce policy options for the present. In sum, the social-science conception of history hangs on in spite of the questions being raised about the problematic nature of time-series analysis. Because so much of the United States research evolved from the circumstances of urban disorders and "great society" programmes of the 1960s there remains an intellectual and funding-supported strain in American writings that contrasts with British work.

Applied history is very much an American notion; it is the dominant concern in the volume of essays edited by Inciardi and Faupel. Several of the people associated with the Presidential Task Force, *Violence in America: Historical and Comparative Perspective* (1969) appear as contributors to *History and Crime*, a collection of essays derived from a 1979 workshop sponsored by the National Institute of Law Enforcement and Criminal Justice. The Institute is a branch of the U.S. Department of Justice. One of the editors (Inciardi) was the Associate Director of Research at the New York State Narcotic Addiction Control Commission. The introduction is sprinkled with statements that portray the historians' craft as a junior, untried, but promising partner in the bold ventures of social engineering: "What is history and what use might it be for the study and understanding of contemporary social phenomena?" Sponsors of the workshop believed that "Historical research adheres to the canons of scientific inquiry."(11, 16) Given this apologetic introduction, completely detached from the independent and sophisticated controversies that introduce so many British works, is there much hope for the contents? In fact, the thirteen essays are pleasantly mixed. It is entirely true that the editors hope that historical research into persistent forms of criminal behaviour will suggest new methods of control. It is also true that several authors have smuggled in some radical ideas, such as Mary Gibson's argument for the decriminalization of prostitution. The editors are distinctly uncomfortable with her conclusion but tolerantly admire her "clear presentation of the issues and problems". (24) Several articles pertain to crime in Europe or in the twentieth century. This review will focus on only those that treat nineteenth-century America. Ted Gurr, a veteran of the 1969 Task Force volume, synthesizes the historical experience of nineteenth-century America in terms of modernization theory, but the article is really too diffuse to categorize. A more substantial article, for those seeking an inside assessment of

American research and an agenda for future work, is provided by Erik Monkkonen. His article, "The Quantitative Historical Study of Crime and Criminal Justice," touches base with various modes of analysis, sources and their limitations, and theoretical models. Monkkonen can find good points among most of the latter, but hs settles in favour of social control. Along with Mark Haller's conclusion that a number of "illegal enterprises" should be decriminalized and David Rothman's criticism of "The Progressive Tradition in Prison Reform," his ideological preference further points out that misgivings about the volume are not entirely warranted. A book should not be judged by its introduction, but it is important to establish the auspices for its publication if only to observe that a particular institutional group has been endeavouring to organize American scholarship. It will be interesting to follow the course of research funding on the study of crime in history under the Reagan Administration.

Monkkonen's discussion of theory affords an appropriate point to begin with the serious review of works in hand. Vague, global, and woolly (a combination that defines a tautology), modernization theory is rejected by Monkkonen without much of an explanation as to what modernization theory meant or means. The neglect is understandable, for modernization no longer has shape. Other theories are presented. According to Monkkonen, urbanization has its own life as a model even though it can be subsumed as part of a modernization model. This somewhat independent urbanization model alleges that city life created anomie and that condition in turn precipitated crime. Most models or theories can be inverted and for years Roger Lane has been dedicated to upending this one. He has been so successful that Monkkonen should have mentioned Lane's hypothesis as the truly operational one. In effect, Lane has maintained that the emergence of the nineteenth-century industrial city created daily patterns of activity that reduced *individual level violence*. This particular line of thought is important enough to deserve later elaboration. Industrialization also has been employed as an explanatory mechanism for fluctuations in the rates of or the composition of crime. That is to say, the notion of a transition from a pre-industrial to an industrial city is supposed to have been accompanied by an escalation of thefts, a reflection of altered property relations. Once again industrialization is a theory often rolled into the modernization hypothesis, but it presents Monkkonen and Lane with a strawman for their community-based appraisals. Monkkonen has found no evidence to support the notion that industrialization had any influence on criminal patterns around Columbus, an admittedly small centre. Finally, Monkkonen's article considers the social control theory and adduces many advantages. Primarily, it brings attention to the criminal justice system and its activities as part of society's formal control efforts. In turn, the social control approach reinforces the growing art of understanding the institutions and thereby the data that they accidentally collected for historians.

Social control has its critics. Liberal historians of reform movements and historians of ideas cringe from it, believing that the theory represents a cynical reductionism that disputes the existence of altruistic and self-sacrificing commitments to charity, social work, educational reform, housing improvement, temperance, and religion. Anthony Donajgrodzki, in the introduction to *Social Control in Nineteenth Century Britain* (London 1977), defends social control against this concern.

> To identify social control processes in religion, social work, leisure or even the agitation for public health, is not to assert that the control element within each is necessarily its main characteristic, still less its only meaning. Parents undertake control functions with respect to their children, and vice versa, but it would be a cynic indeed who saw this as the true and only mean-

ing of family life. Second, it is important for its use in the study of inter-class relations to recognize that the concept suggests that controllers *and the controlled are, as it were, trained to* their roles. . . . Social control seeks to place it [humanitarian zeal] in a context, not to explain it away.(15)

Exponents of social control also must cover their left flank, since Marxist historian Gareth Stedman-Jones has rejected the theory because he felt it necessitated the acceptance of "a consensual social model . . . and . . . is associated with a description of the transition from agrarian society to capitalism which denies the permanency of class struggle."[3] Proponents certainly acknowledge social control's origins within a conservative sociological tradition. At some points in its history it was not applied critically; rather it was used to bring attention to a set of direct and indirect means to defend a social *status quo*. The family tree, however, is not everything; the theory's recent proponents have found radical employment for it. That is certainly the case with John K. Alexander's *Render Them Submissive*.

Ostensibly Alexander's account of the Revolution's impact on the poor in Philadelphia examines how the poor lived and how the wider society perceived them. The scale and conditions of poverty are succinctly presented. From relief rolls, excluding recipients of aid from churches and private charitites as well as those who received no relief at all, it appears that between 10 and 15 per cent of the population of Philadelphia could be defined as poor. The American Revolution appears to have done nothing to improve the condition of the minority. Wealth became more highly skewed over time and underemployment remained a chronic problem. Possibly the Revolution altered the tradition of deference. In fact, in the eyes of the affluent, the Revolution had caused the poor to behave less as they were expected to act. Enter new or strengthened forms of social control in the aftermath of riots and strikes in 1779 and a mob of discharged soldiers in 1793! The instruments of social control embraced homilies or ideology, the criminal code, a reform of relief practices, and the activities of educational reformers. The more affluent citizens, we are told, turned a pre-Revolution trickle of general comments on the happy prospects for the industrious poor and the lamentable burdens of the rich into a torrent. Alexander does stretch credulity in claiming a leap in the number of recitations of the nameless griefs and toils of the rich. The other ingredients of social control have a more tangible basis for comparison over time. The criminal law was reformed in the wake of the revolution by limiting the use of capital and corporal punishment. On the other hand, another measure (1790) eliminated the possibility of an early release for those who had behaved well in jail. Also, Philadelphia gained the authority to sweep the "vulgar riffraff off the streets." In the area of poor relief, the post-Revolutionary charities placed more emphasis than previously on the habits of the poor which, it was believed, should and could be reformed. Whereas Philadelphians had manifested little enthusiasm for educating the poor until 1785, there was an explosion of interest afterward. Some education reformers may have imbibed the Enlightment view on human progress, but Alexander disputes their importance and finds a far larger group consisting of tough-minded pessimists. This element felt that education should train the poor to accept an inferior station in life. In terms of the debates on social control, Alexander uses the theory to challenge Raymond Mohl's stress on a humanitarian impetus in poor relief; his argument also rolls back the explanatory significance of social control from the first half of the nineteenth century where it had been placed by historians of education and of the criminal justice system.

Paul Boyer's *Urban Masses and Moral*

[3] A.P. Donajgrodski, ed. *Social Control in Nineteenth Century Britain* (Totowa, N.J.: Croom Helm 1977), 10.

Order is something of an idiosyncratic study, fascinating and probably coherent in terms of its own broadly defined area of interest. Placed alongside a work like Alexander's, however, it appears amorphous. This is not simply owing to its lack of a community base, although the unspecified range of what is virtually the history of an idea — the proposition expounded by many reformers and intellectuals that urban life is an aberration — is a contributing factor. Chronology also poses a problem because Boyer covers men and movements from 1820 to 1920. Finally, the peculiar and unfocused nature of the book derives from or is symptomatic of a proclamation about its terms of reference in relation to what it is not. The "focus in this book *is not* on efforts to constrain the urban populace by force of law." Likewise he claims he wants to avoid "a sociological analysis of the many subtle and largely unconscious ways by which all social groups... shape those who fall within their orbits."(viii) To him, the phrases "social control" and "moral control" are terms used "nearly interchangeably." This does not place readers any closer to a real thesis. He appears to revert to a conservative conception of social control, evading the ideological sting evident in Alexander's application of that term to the symbolic time and place for the founding of the Republic. Boyer's expressed interest, to comprehend reformers who were aiming at bringing adherence to a general standard of *right conduct upon which an enduring urban moral order could be built*, shades toward a very moderate position. Nonetheless, the index entries for "social control" lead to knowledgeable discussions about the history and value of the theory.

One trait of works that touch on social control is their reconstruction of a vision of crisis that worked on the attitudes of an urban elite or a particular group of zealous reformers. This is an essential feature of the social control argument not only because there must be a perceived or real challenge of deviance to provoke a response, but also because the timing of a social control measure or campaign must be explained. To account for the flourishing of an evangelical spirit with its auxiliaries of Sunday Schools and the pamphlets of the American Tract Society, Boyer recounts the facts and the mythology of urban decadence in the 1820s and 1830s. To many concerned contemporaries, the sheer growth of cities, the conflict-laden mingling of immigrants with natives, the presumed despoilment of rural migrants in the big city, and a greater and more boisterous public participation in local politics were signals that public opinion had to be marshalled against mounting disorder and corruption. Boyer enlivens his account with the biographic sketches of leading moral-uplift proponents like Lyman Beecher and his society for the suppression of vice and promotion of good morals (the Good Morals Society) or of the Canadian theology student John R. McDowall whose study on prostitution in New York was the famed *Magdalen Report* (1831). His descriptions of classic tracts like "The Dairyman's Daughter" present both irresistible reading and serious analysis of the efforts of moral reformers to propagate in the city the values and restraints alleged to have existed in villages. By the mid-century, Boyer believes that the cutting edge of the moral reform and uplift movement was slipping away from an evangelical base and becoming more secular in aims and leadership. The Children's Aid Society and the YMCA exemplify this transition and once again the biographical profiles are accomplished and fresh. The account of Children's-Aid-Society founder Charles Loring Brace is noteworthy. *The Urban Masses and Moral Order* includes the Progressive reform era and then closes in the 1920s because Boyer argues that from that point forward the city had achieved legitimacy in American thought and increasingly attention was being given to the value of heterogeneity found among the urban masses.

Just as it is possible to dispute Boyer's choice of 1820 (Alexander makes the case for the 1790s), one can criticize the conclusion and the selection of 1920. Perhaps a rightward turn in American politics and the force of the Moral Majority have now tapped the protean rural and moral uplift attitudes that were supposed to have been abandoned.

David R. Johnson's *Policing the Urban Underworld* probes the most direct form of social control. Rarely mentioning the term and making few forays into the theoretical place of his work, Johnson nonetheless charts the significant interaction between agents and objects of social control. From the inception of policing, criminals and constables became entangled in ironic relationships. Like Boyer, Johnson seems unequipped to carry his fine observations to a pointed conclusion, missing an opportunity for a provocative statement on a repercussion of social control. Instead of deviance leading to social control, he might have expressed the idea that social control can lead to deviance. The evidence abounds. Johnson comes so close so often that the want of an explicit theory is pardonable. Moreover, he succeeds admirably in preventing a review of police activities from becoming crudely attached to the direction of an urban elite. For example, it is not the instigators of social control — the police reformers and worried merchants — that provided the police with their actual tactics of violence or criminal fraternization. With no clear direction from police-force founders as to how to perform the job, patrolmen learned by street experience and developed a street lore. Much of the quickly-gained knowledge recommended preemptive force. By the 1850s, the carrying of firearms was becoming common. In order to capture thieves, constables had to consort with them and that raised a host of problems. Did the public want crime suppressed or property recovered? The skill of burglars led to the hiring of detectives who were, at least in the early years, more entrepreneurs than public servants. Recent scandals among Miami and New York detectives raise the unsettling prospect that a similar potential survives. As a final thought about the outbreak of novel forms of deviance and violence stemming from the formation of police forces, Johnson observes the hardening of criminal organization and skills in reaction to legal intervention. One wonders what historical lessons the earnest social scientists at the National Institute of Law Enforcement and Criminal Justice would derive from this altogether engaging book?

John Schneider's monograph about the origins of policing in Detroit enters the social control literature with an original insight, claiming to help explain the timing for the establishment of urban police forces. In common with Alexander and to a lesser degree with Boyer, he explores the perceptions held by an urban elite with respect to social order. He attempts to establish precisely what triggered fears and decisively persuaded reluctant municipal governments to adopt a paramilitary form of social control. In part, his work challenges the usual social-control approaches because he feels that these neglect a critical mechanism that brings a city elite into action against a perceived erosion of social discipline. According to Schneider, a fear of the working class or of ethnic rioting did not arouse the Detroit authorities to take immediate steps. A casual labourer posed no threat if he had a family and lived in a single family dwelling; in Detroit there was no inclination to think of immigrants *per se* as a source of disorder. "Police reform, therefore, cannot be reduced to any simple class, ethnic, or political ideological interpretation."(85) For Schneider, the essential catalyst can be isolated in the impact of changing land use upon the perceptions of influential city residents. This introduction of social geography is a salutory innovation. Far too often labour or social historians neglect the spatial dimension, although the conditions of mixed or seg-

regated residential space have obvious implications for the study of class conflict or alternately the embourgeoisement of labour. Schneider uses geography as an aid to and not a replacement for the social control perspective.

> Clearly, police reform was not unrelated to the wider concern for social control that had roots in traditional American ideology, in ethnic heterogeneity, in early industrialization, or in the reform spirit of the age, and which found expression in everything from educational innovation to the rise of the Republican party. One cannot ignore, however, that changing spatial patterns in the growing cities of the nineteenth century complicated, even undermined, traditional methods of social control.(86)

The emergence of low entertainment, vice, and bachelor sub-culture areas close to the central business district and wealthy residential quarters was bothersome. Crime waves — burglaries and muggings in the central business district — prompted the hiring of mercenary police forces to patrol the business core. Still, these private patrols did not strike at the low life districts through which many affluent citizens passed on their journey between home and office. An 1863 riot that took place much closer to truly valuable property than riots in the 1850s was a decisive episode in bringing the threat of disorder to the doorsteps of the city's prominent businessmen and professionals. The spatial aspect is carried further, for Schneider evalutes the activities of Detroit's police force in terms of urban space. The police department functioned as a service to elite interests downtown. The problem with the spatial hypothesis is that it may not apply in other urban centres. Detroit's delay in forming a police department does not seem typical. Hamilton, Canada West, was forming a police department a decade before Detroit. Moreover, Hamilton had about half the population of Detroit and was far less spatially developed when it dramatically expanded its constabulary. Schneider plays down ethnic tensions and upper-class fears of working-class action, but in Hamilton, a police establishment grew when the city's elite became unnerved by the famine-Irish immigration and striking railway navvies. In Detroit, the contact of the wealthy with vice areas conceivably heightened the anxiety of a civic elite, but the threshold of reticence about policing was crossed in different ways in other cities.

Having formulated a less than universal explanation for the timing of social control by policing, Schneider concludes more conventionally by considering the geography of crime. In the late nineteenth century, the criminals of Detroit victimized their own neighbours or invaded nearby commercial or upper-class districts. With the development of streetcar suburbs, the more affluent residents began to put greater space between themselves and the enclaves of disorder. Schneider proposes another consequence of social space. Streetcars conveyed immigrant whites away from contact with blacks (there had been vicious racial rioting in 1863) and away from the low entertainment areas. Distance minimized certain provocative contacts and, where these contacts remained, a permanent police patrol reduced the incidence of crime and ruffianism. These observations blend into the subjects of the next three books, namely the processes of urban life which allegedly acculturated city dwellers so that the end of the nineteenth century can be considered a less violent period than the mid-century. In these works, full play is given to variations on the urbanization and industrialization models.

Roger Lane's *Violent Death in the City* expands upon his now classic "Urbanization and Criminal Violence in Nineteenth-Century Massachusetts."[4] He continues to challenge a popularly held notion that

[4] Roger Lane, "Urbanization and Criminal Violence in the Nineteenth Century: Massachusetts as a Test Case," *Violence in America: Historical and Comparatives*, ed. by Hugh Davis Graham and Ted Robert Gurr (New York: New American Library 1969).

urban growth and a mounting degree of violence are inexorably related. For Lane, urbanization had a civilizing influence that lessened violence over the long term. From the standpoint of collective or mob violence, the small cities of the earlier nineteenth century were, he proposes, more riotous than the metropolitan centres at the end of the century. The measurement of such a phenomenon, however, as Lane must recognize, is futile. So, in order to fortify his central argument about cities, he has shifted from collective violence to individual level violence. To do this, he set aside the many possible categories of collective violence and the numerous types of crime that characterized his Massachusetts' study. Instead, he concentrated on the bad endings that overcame some Philadelphians: suicide, homicide, and accident. The process of collecting serial data was often flawed, but Lane claims that the records of mortality were superior to those of criminality. All violent deaths of adults were officially registered in some fashion. The fact that forensic science was inexact made it impossible to categorize all recorded instances of violent death. This apparent limitation becomes an advantage in Lane's hands because from it he can allege that early reporting of violent death underestimated the real level of homicides. The situation was worse than partial data indicates. As a seminal North American monograph based on coroners' reports, *Violent Death*'s observations should stand for sometime — at least until other cities are studied or Lane's avowedly simple descriptive statistics are challenged by an adept social scientist. Until the inevitable methodological challenges, a few of his basic observations can pass for fact. Concerning death by accident, he discovered a sharply rising trend attributable principally to new ways of making a living in factories and near railways. This rise came in spite of what Lane sees as an increasingly careful and more sober populace. There are other anomalies to explain. The officially recorded rate of homicide in the modern city is much higher than for the city of Philadelphia in the previous century. Lane hurdles this barrier by noting that relative to the total number of killings, city streets have become safer since more killings are now committed in the home. Besides, Philadelphia's murderers have included a large proportion of highly transient individuals who Lane argues cannot be considered urbanized. Finally he stresses the underestimation of homicide in the early period; drunks found dead in an alley died officially from misadventure or a clinical cause such as a fractured skull, although they might have been beaten to death. Death by drowning could be the official cause of another mortality, although a prior act of violence had precipitated the drowning. The rivers were popular places for the disposal of bodies. By using illustrative examples, Lane establishes a menacing portrait of Philadelphia at mid-century; there was a Dickensian world of unfortunate country girls, and depressed souls who killed themselves by gun, poison (laudanum was gentle and popular), gas or slashing. Footpads, street gangs, and barroom brawlers prowled the streets of the city of brotherly love.

One corollary to Lane's thesis concentrates on a suicide-murder ratio. Based on an established theory drawn from sociology, it argues that the two forms of violent death are inversely related. As suicides increased among whites, murders declined. Lane proposes that the shift followed from different forms of economic organization. The industrial city demanded control and regulation. Furthermore, it socialized the workforce until labourers became "their own slave drivers." Factory and bureaucracy imposed norms that internalized expressions of aggression. With a spontaneous impulsive recklessness reduced, homicides fell but suicides increased due to internalized aggression. The immediate reaction from conflict-oriented labour historians to this argument, smacking of hegemony, can be imagined. But it must be recalled that Lane

has deliberately set aside collective conflict and dwells solely upon individual-level violence and, concerning these, David Philips' conclusions about crime and violence in the Black Country of England are quite pertinent here. According to Philips, there were no "visible indications of social purpose, still less of the individualistic waging of class war behind most normal criminal acts." Is it not conceivable that for white residents, urban life as process both reduced individual-level violence and simultaneously fostered confrontation between special purpose associations such as unions and employer groups?

The critique of the hypothesis of urban anomie and the forceful counter claim of an urban culture providing non-violent norms, becomes the subject for extended treatment in Gunter Barth's *City People*. Barth has set out to demonstrate how these norms grew by a process in which living in the city became an education in itself. A number of urban institutions are selected to exemplify the overt and subliminal functioning of normative contagion; the apartment house, the department store, baseball, the metropolitan press, and vaudeville. The book's sequence of unconventional insights is too extensive and sustained to summarize, but easy to criticize. A few observations illustrate the technique. Cheap urban newspapers in America were not instruments of ideology as in Europe where the press had a refined ideological flavour. Instead, urban dwellers in the United States found in human interest stories and advertisements information about behaviour and styles that indicated how they should act in the urban procession that most had just recently joined. The baseball park combined a setting reminiscent of a country fair with the importance of restraints and rules. Furthermore, the magnetism of the ball park pulled together crowds of strangers who succumbed to a startlingly intense sensation of community. Vaudeville also fused pleasure and instruction. Signs informed audiences how to behave and bouncers gave rough lessons in etiquette to those who ignored the posted rules of the house. Managers stepped on stage to lecture the throngs. Barth certainly draws different conclusions about the composition and lessons of popular culture than do most labour historians. In fact, he virtually articulates a rejoinder to the school of class conflict in culture. While it is thus an important speculative work, it plays with ideas and is not firmly planted in social history. Instead of an investigation of just one of his chosen institutions in a given place where his conclusions might be tested against polarizing and factionalizing incidents, Barth compresses pithy observations drawn from across the American urban system. In fact, his broadest purpose is to argue that through the railway, telegraph, and metropolitan newspapers, the culture of the city became the culture of America. Barth's eclecticism and far-reaching conclusion are bound to delight and to infuriate. Urban historians will miss the sense of place. Labour historians will question the supposed receptivity of the working class to the etiquette apparently imposed at some important types of public assembly. Moreover, to write about department stores and not about the persistence of markets and street vendors raises a tip of doubt concerning the class composition of Barth's urban dwellers who were being brought into an alleged mainstream of urban life. Historians must take seriously what once was dismissed as too mundane: dwellings, shopping patterns, sports, and entertainment. It will be a long time, however, before the role of leisure in either fostering community or class loyalty can be sorted out. If and when the dust settles, *City People* is not likely to be remembered so much as a valued work of reference as a provocation on the trail to more complete understanding.

When Roger Lane joined the club of historians who have tried to make Philadelphia into an historical laboratory, he got access to the research compiled by the

Philadelphia Social History Project (PSHP). From one of its associates, he secured "arcane statistical data." The PSHP was and is a unique beacon for urban and social historians, not for just the able few who seem so attached to the city's past and the broader meaning of that past to America.[5] Among other results, the PSHP produced conclusions overlapping with Lane's views on the civilizing context of the industrial city, a rising black homicide rate, and the unique status of urban black families. These concerns constitute the burden of the collection of essays resulting from one of the most massive social history projects ever undertaken. Theodore Hershberg's edited collection, *Philadelphia: Work, Space, Family, and Group Experience in the 19th Century*, brings together a decade of publications stemming from the PSHP. Such a project will probably never be seen again. Appendix II recounts the collection and integration of the data files. A few examples of the machine-readable data give an indication of the scope and labour of the decade-long project: variables for each of 500,000 individuals, 144,000 families, 29,000 firms recorded on manuscript census schedules, 20,500 individuals who died between 1 July 1880 and 30 June 1881, and 750 voluntary associations. Experts from many fields were introduced to the data bank so that a truly interdisciplinary urban history would result. Given the mass of data and the expertise of various writing teams, only an exceptional polymath or a panel of experts could hope to review critically such specialized undertakings as the econometric models for assessing industrial efficiency, the multiple classification analysis of residential mobility, or the probit regression analysis of families. Most articles adhere to a formula: the statement of a problem in terms of the historiography, a discussion of data and methods of data manipulation, observations, and conclusions. Therefore, if a reader assumes the legitimacy of the methods and the accuracy of the machine-readable records, and if the formulation of equations is not pursued to the point of distraction (and migraine), the articles have a compelling lucidity. What is more, the volume benefits from an overarching design revealed in Hershberg's enormously important introduction. In blunt and personal terms, the purpose of the volume is to overthrow the social mobility approach to social history closely associated with the chronicler of American opportunity, Stephan Thernstrom. Thernstrom has denied the significance of urban environment and concentrated on an atomistic account of social and geographic mobility. *The Other Bostonians* followed various groups of people up and down (usually up) an occupational scale as if they were so many balls in a bingo machine. Thernstrom described an almost open system. In his estimation, the few social barriers were, or should have been, spurs to overachievement. Hershberg now has reaffirmed the integrity of urban history and the existence of real obstacles to social progress. He wanted to look at the city as a process where there are real mechanisms for livelihood and progress as well as real mechanisms for their denial.

Before considering the connection between Lane and Hershberg, the contents of several articles deserve synopsis. The selection is both personal and related to the thrust of this review; the 13 articles present such a uniform standard of high quality scholarship that there is no feasible selection by order of merit. Bruce Laurie and Mark Schmitz organize a considerable amount of detail of certain interest to labour historians. "Manufacture and Productivity: The Making of an Industrial

[5] For a consideration of the scholarly works with a critical bite that take Philadelphia as a subject, see Allen F. Davis and Mark H. Haller, *The Peoples of Philadelphia: A History of Ethnic Groups and Lower Class Life, 1790-1940* (Philadelphia: Temple University Press 1973); Sam Bass Warner Jr., *The Private City: Philadelphia in the Three Periods of its Growth* (Philadelphia: University of Pennsylvania Press 1968).

Base, Philadelphia, 1850-1800," one of the few articles not previously published, discusses the "islands of hand techniques in a sea of modernity." What is challenging is their contention that increased scale and mechanization did not necessarily yield improved returns for industrial capitalists. "Artisan shops and sweatshops — work settings without the technological variability of factories and manufacturers — were nearly as efficient as small factories."(86) In their study of the journey to work, Theodore Hershberg, Harold E. Cox, Dale Light Jr., and Richard R. Greenfield also have discovered a paradox.

> On the one hand, we envision stable neighbourhoods, strong friendships, and kin networks and limited exposure to the world beyond with its different ethnic and religious groups, culture, and behaviour; on the other, we see changes as the hallmark of the society with continuous uprooting and relocation, limited opportunity to develop enduring ties to kin, friends, and fellow laborers, constant exposure to new people, values and ideas, and different ways of life.(166-7)

The resolution of firm networks and continuous uprooting can be found in the collection of reasonably self-contained neighbourhoods that resembled one another. "The cultural milieu and the organization of life, in other words, might have remained pretty much what it had been before the move to a new job and neighbourhood."(167) This is full of implications for labour historians should they include spatial factors in their considerations of class solidarity. Incidently, it picks up on the arguments of Lane and Barth insofar as it explains adjustment to the churning society of the cities. Did the familiarity and social ties awaiting a newcomer to an urban neighbourhood contribute to diminution of random violence?

Whatever the particular ramifications of familiar and convenient neighbourhood conditions for white labour — native born or immigrant — the galaxy of relationships including family, work group, and residential space were different for Philadelphia blacks. In a brilliant article in an outstanding collection, Claudia Goldin reconstructs family strategy and the family economy; she brings to life the precise distinctions that had caused W.E.B. Dubois to explain the absence of child labour among blacks as "due to restricted opportunity; there is really very little that Negro children may do."(298) Hershberg, in a well-known article of his own, adds "job discriminating . . . was complete and growing. . . . All social indicators — race riots, population decrease, disenfranchisement, residential segregation, per capita wealth, ownership of real property, family structure, and occupational opportunities — pointed toward socioeconomic deterioration within Philadelphia's antebellum black community."(376) Is racism the counterpoint to white urban adjustment with its bonus of declining personal violence? Let us allow Roger Lane to stray out of the nineteenth century and have the last word. "The urban black population . . . frozen out of bureaucratic and factory employment, developed unique patterns of violence as a result, with a continually rising homicide rate."(*History and Crime*, 107)

The books concerned about order in American cities fix on unique conditions and questions that scrape the marrow of liberal American mythology and assumptions of progress. At first glance, because of a different bundle of national issues and claims of a distinctly Canadian social philosophy, the American studies do not seem entirely appropriate as background to Canadian research. Additionally, Canadians have no high homicide rate to explain or to explain away. Racism, though not missing from the Canadian record, has been limited through circumscribed opportunity. Nonetheless, the works under review do speak to Canadian historians. For example, did early Canadian settlements share a continental propensity for urban-frontier-style violence? If so, is Lane's hypothesis of declining violence and his linking of this phenomenon with urban growth applicable to Canada? An

affirmation would deliver a hard empirical blow to sponsors of an alleged Tory element in the makeup of Canadian "decency," for it would make an orderly society the product of socio-economic forces rather than philosophical "fragments" or systems of government. What Michael Cross and Ruth Bleasdale have written about Upper Canada/Canada West recommends a serious assessment of the applicability of Lane's thesis.[6] Moreover, the books by Boyer, Johnson, and Schneider treat institutions with Canadian counterparts. In fact — to once again beard the proponents of the Tory mythology — they demonstrate that Canadians have not been alone in believing that the state should provide moral direction for the society it governed. Fifteen years ago, David Potter wrote in *Canada Views the United States* that "Americans have countenanced not only extremes of freedom and permissiveness, but even violence, corruption, license, and social deviance in preference to a public authority strong enough to control them." The outdated claim rings hollow; perhaps its Canadian counterpart, proposing a northern emphasis on "discipline, order, responsibility, obedience, even inhibition"(129) does also.

[6] Michael Cross, "Stony Monday, 1849: The Rebellion Losses Riots in Bytown," *Ontario History*, 63 (1971); "The Shiners' War: Social Violence in the Ottawa Valley in the 1830s," *Canadian Historical Review*, 54 (1973); Ruth Bleasdale, "Class Conflict on the Canals of Upper Canada in the 1840s," *Labour/Le Travailleur*, (1981).

Canadian Historical Association Conference, 1983

The 1983 Canadian Historical Association Conference will be held in Vancouver at the University of British Columbia. The Programme Committee invites proposals for papers and sessions from members, other historians, and members of related professions. Three theme areas have been selected for special emphasis at the conference: the history of the family, Canadian historical geography, and early modern European social history. In addition joint sessions with the Canadian Political Science Association and the Canadian Society for the History of Medicine are being planned. As usual the conference will also present papers in Canadian, American, European, Asian, African and other historical fields. Those wishing to place proposals before the committee are asked to write the chairman of the appropriate co-ordinator. The committee consists of: Peter Ward (History — UBC), chairman; Robin Fisher (History — SFU); Yvan Lamond (Centre d'Études canadiennes françaises — McGill), co-ordinator for Francophone historians; Douglas Owram (History — Alberta), co-ordinator for proposals from CHA member sub-groups; Joy Parr (History — Queen's), co-ordinator for family history; Allan Smith (History — UBC); Donald Sutherland (History — Brock), co-ordinator for European social history; and John Warkentin (Geography — York), co-ordinator for Canadian historical geography.

Deadline for submissions: 15 September, 1982.