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History, Sociology and Dutch Collective Action

Yet Again History and Sociology

'Such predictions,' fulminate Gary Hamilton and John Walton, 'are not merely excessive; rather they are seriously misleading and quite mistaken.'[1] It sounds serious. What predictions do the indignant duo of California sociologists have in mind? That history and sociology will merge in a common enterprise. Hamilton and Walton make their case against such perilous prognostications by describing the actual disciplinary practices of historians and sociologists, in particular the way that historians prefer to ground their inquiries in sources while sociologists prefer to ground them in theories and methods. In doing so, they have unquestionably identified a salient on-the-average difference between the disciplines, one that will not easily disappear. They could even have fortified their case by pointing out that historians' concern for sources correlates with a tendency to define problems within clear limits of time and space, the sociologists' concern for theories and methods with a predilection for identifying problems with structures and processes. If one person tells us she is a specialist in nineteenth-century Dutch history and another that he is a specialist in family structure, we already know a great deal more about differences between their likely ways of working. Despite those obvious disciplinary divergences, however, we have strong reasons for thinking that history and sociology *should* merge, and at least a few weak reasons for thinking that the line between them *will* blur increasingly. They *should* merge because sociology actually behaves, for the most part, as a history of the present, and suffers acutely from its practitioners' unawareness of the historical limits to their observations. What appear to be general theories and methods commonly turn out to apply unambiguously only to the present in some particular part of the world; survey research and

1. Gary G. Hamilton and John Walton, 'History in Sociology,' in Edgar F. Borgatta and Karen S. Cook, ed., *The Future of Sociology* (Newbury Park, California: Sage Publications, 1988), p. 182.

the theory of public opinion provide good examples. A merged discipline would recognize that purportedly universal generalizations about social structures and processes hold within stringent space-time limits: the sphere of contemporary capitalism, the world of twentieth-century North America, or some similar set. It would offer the promise of greatly improved understanding large social processes that take many years to unfold. All this, to be sure, on the premise that social life is to some degree systematic and knowable – a premise that is, in this age of narrative and discourse, increasingly contested. I will adopt that premise without argument, but with a certain amount of trepidation, in these reflections on recent work concerning Dutch collective action, and its connection with my own work on capitalism, state formation, and collective action in Europe as a whole.[2]

Validation of these assumptions will depend on their practical outcomes rather than on my rhetorical skill: Do they eventually produce cumulative, reliable, generalizable knowledge? I think they do. Very likely many of my historical readers, persuaded by recent attempts to recast historical analysis as the construction and glossing of texts, will disagree. In any case, we can agree that sociology and history share the problem. An epistemological showdown is coming – at least I hope so. No reader of current feminist, Marxist, or general historical theory can avoid the problem. Again sociology and history share the concern, and can gain from a merged effort to deal with it.

Whether history and sociology *will* merge is, of course, another matter. Literary historians and historians of science show no signs of bending in the direction of sociology; far from it: They seem to be moving in the opposite direction. Nor do students of status attainment give many indications of historicizing their enterprise, despite the occasional misguided effort to construct American-style origin-destination tables for distant places and times. I must concede to Hamilton and Walton that some parts of history will probably remain irreparably separate for the foreseeable future.

Yet some signs point to the emergence of a greatly enlarged intermediate area which is neither history nor sociology but both together. The first sign is the prospering of formerly interstitial specialties such as economic, demographic, and family history, which pursue regularities in structures and processes within well-defined boundaries of time and space; practitioners continue to debate their proper directions, but no one doubts that they have acquired momentum and partial autonomy as fields of study. The second is the remarkable historicizing of many inquiries within sociology, including the study of work, inequality, and the arts. The third is the emergence of

sociological predispositions in the historical heartland, political history; studies of war, states, revolutions, and international relations have all taken on surprisingly sociological airs, while the students of those subjects within sociology (and, I must allow, anthropology and political science) have led the way to historically-grounded analysis in the discipline.

Historical Studies of Conflict and Collective Action

Consider the historical study of conflict and collective action, which has played a significant part in renewing political history. Beginning with the 'history from below' of radical historians such as Albert Soboul, Richard Cobb, George Rudé, and Eric Hobsbawm, that study has become a small historical industry. It extends into the analysis of revolution, rebellion, resistance, and everyday life. The study of conflict and collective action centers on the ways in which people pool their efforts to make claims on each other. 'Claims' include all communications that call for changes in the social relations of parties other than the claimants themselves, or call for maintenance of those social relations in the face of challenges from others; claims involve such verbs as demand, petition, entreat, attack, condemn, support, and cheer. Three very general questions guide the study of conflict and collective action:

1. Under what conditions do people make claims together?
2. Under what conditions *don't* they make claims?
3. To what extent do major social changes produce standard alterations in claim-making?

Although plenty of people are pursuing these questions within a strictly contemporary context, historical analyses and historical evidence have added substantially to our understanding of their proper answers. Historical analyses matter because the conditions under which people act collectively, or fail to do so, change significantly over time and space as a function of the organization of political power and the structure of economic life. Historical evidence matters because the changes in question extend over significant blocks of time; to capture their full sweep, we must reach back into history.

The junction of historical with social-scientific analyses of popular collective action that formed in the 1960s enriched work on both sides of the divide. At first it made sense to organize the work as a contest between 'breakdown' theories and theories of 'solidarity,' as George Rudé did in his eloquent empirical attack on conceptions of the irrational crowd, and I did repeatedly in my writings of the 1960s and 1970s. Breakdown theories treated the collective action of strikes, demonstrations, attacks on tax collectors, or religious movements as forms of disorder, deviations from well- regulated behavior.

Rapid social change, ran the typical argument, introduced conflicting norms and weakened social controls, with the result that disoriented individuals killed themselves, committed violent crimes, went mad, joined weird sects, or formed revolutionary crowds.

Solidarity theories contradicted theories of breakdown in almost every particular. They drew their initial inspiration from the classic Marxist portrait of a working class, maturing with experience, that acquired the capacity to act collectively as it developed shared consciousness, internal connections, and mutual commitments; thus the women's movement, the civil rights movement, and movements for civic reform experienced setbacks from time to time, but over the long run progressed to ever-greater solidarity and capacity to act; individual actions such as strikes and demonstrations simply expressed that underlying solidarity. However much solidarity theorists distanced themselves from this model of a unitary *Sozialbewegung*, they agreed that collective action rested on prior group ties, shared grievances and aspirations, cumulated experience, and the routines of everyday life; such views committed them to combating any idea of 'the crowd' as a collection of segmented, demented individuals cast off from normal social existence.

The confrontation between theories of breakdown and solidarity enlivened debates about collective action in the 1960s and 1970s, although the selective use of cases and evidence on both sides meant that researchers rarely organized fair applications of two competing arguments to the same events, processes, and populations. Instead, breakdown theories began to fade as a consequence of vigorous criticism and an accumulation of studies organized around the theme of solidarity. At the same time, solidarity theories became more nuanced and specific. By now, breakdown models of collective action have almost vanished; analysts divide instead over the extent to which the logic of individual rationality accounts for collective action, the places of ideology and identity-formation in different varieties of collective action, and the connection between routine small-scale struggles and episodic, large-scale making of claims. Classic social movement theory is now fragmented, although the social movement itself—the emergence of sustained challenges to established authorities in the name of aggrieved populations – is very much alive.

Aside from the crowding out of breakdown theories, perhaps the greatest accomplishments of the last two decades' work on popular collective action have been a) to identify the organizational bases of claim-making and b) to show that (far from being the normal response of deprived, aggrieved, or angry populations) collective action is problematic and costly. 'Organizational' does not necessarily mean 'associational'; people often build their claims on existing ties of kinship, religion, neighborhood, and work without creating special-purpose associations to sustain collective action. Informal ties built up in the everyday contacts of a local market, a workshop, or a parish have often formed the bases of concerted action, in the absence of any special-

purpose organizations. Indeed, the rise of specialized associations as the base of collective action sets off a distinctive historical stage, one that occurred in much of Europe during the nineteenth century. The organizational bases of popular collective action varied and changed with the organizational structure of routine social life.

Such investigations as Victoria Bonnell's comparison of workers' politics in Moscow and St. Petersburg or Edward Walsh's study of citizen mobilization after the Three Mile Island nuclear breakdown exemplify the sophistication of recent thinking on the subject. Through close studies of the organization of production, the character of trade unions, and the texture of social life among artisans and industrial workers in the two Russian metropolises, Bonnell effectively combats the portrayal of workers' revolutionary action as an outcome either of the dislocation experienced by peasant migrants to the city or, contrariwise, of the transplantation of peasant life and mentality into the city. Walsh combined interviews, on-the-spot observation, and files of published material in an analysis in showing how pre-existing interpersonal networks shaped the patterns of activism and coalition-formation in citizen response to a serious, dramatic threat of nuclear contamination. Although both of these studies emphasize solidarity more than breakdown, neither of them fits neatly into the old categories.

As relatively coherent contemporary schools of thought, we might distinguish three main brands of theorizing about collective action, pivoting on *mentalities*, *individual action*, and *collective action*. Especially with respect to so-called New Social Movements – environmental, feminist, homosexual-rights, peace, and related campaigns – analysts of mentalities see collective action as a creative process in which people simultaneously act on shared conceptions of the world and create new identities and beliefs. Analysts of individual action see shared claim-making as an aggregation of individual choices and preferences. Analysts of collective action treat it instead as an outcome of prior group experience. The three headings correspond to the fundamental split in the social sciences and history, among those who 1) take the coherent society as a starting point for analysis, 2) treat the individual as the fundamental social unit, 3) begin with social relations, and derive both individuals and complex social structures from them.

Repertoires of Collective Action

My own work falls chiefly into category 3. I deny the existence of the vague entities called 'societies,' and decry the stringent limits of individualism as an approach to social analysis. In studying the transformations of social life

wrought by the development of capitalism and the formation of national states, I have often examined the processes in their own terms. But I have also examined changing repertoires of collective action in Europe and America, following the history of collective action in itself and as a function of major structural changes. Detailed catalogs of events in which people made visible claims – strikes, violent encounters, contentious gatherings, and others – have constituted the core of my evidence in that regard. The studies of collective action have alternated between two different kinds of analysis: attempting to account for the general evolution of some form of collective claim-making such as the strike; tracing the impact of particular structural changes such as the formation of national states on popular collective action.

A good deal of my recent effort pivots on the idea of *repertoires* of collective action. The forms of collective action vary as a function of the surrounding social structure and the previous history of conflict among particular sets of actors. Take the case of industrial conflict. Strikes, in the sense of concerted withdrawals of labor from production coupled with demands for changes in the conditions of production, occur chiefly where labor has proletarianized, capital has concentrated, and workers labor in close communication with each other. But the particular forms of strikes, as Michelle Perrot has shown for nineteenth-century France, grow out of struggles among workers, capitalists, and agents of the state; as a result, each country accumulates its own special laws, police practices, bureaucracies, worker routines, union techniques, and employer strategies around the fact of the strike. That accumulation then shapes the incidence, timing, frequency, character, and outcome of strike activity.

What is true of strikes holds generally for all forms of conflict. As compared to the forms of conflict that are theoretically possible, any pair of actors that engage in sustained conflict tend to employ an extremely limited set of routines, adopting the same ones over and over again with minor variation. In contemporary capitalist states, concerted conflicts between employers and workers take the forms of strikes, lockouts, committees, demonstrations, demands for state intervention, sabotage, and very little else.

In eighteenth-century European countries, on the other hand, masters and workers confronted each other in shaming ceremonies (such as parading a strikebreaker on a donkey), and what the English called a *turnout*: the gathering of dissatisfied workers of a particular town in a protected location, their marching from shop to shop in their trade calling for workers to join them, a new workers' assembly, collective framing of demands, sending of delegations to masters and local authorities, counter-assemblies of masters (and sometimes of local authorities), negotiations, refusals to work during the negotiations, eventual agreements, both on a community-wide level and shop by shop. That the turnout did not always follow this exact sequence – that, for example, individual employers sometimes locked out all their workers,

and refused to hire them back – confirms that it was not an empty ritual, but an actual means by which antagonists worked out conflicts.

In western countries, since World War II a form of conflict that rarely occurred earlier became quite common: one group seizes a symbolically-important space, person, or object, and holds it hostage while bargaining with another group. Aircraft hijackings fit this pattern, as do factory occupations and sit-downs in offices or public squares. Hostage-taking has an ancient history in war, and demonstrations have gravitated to major public spaces for more than a century, but as a deliberate tactic this seizure-and-bargaining constitutes a new departure. It overlaps with the tactics of that heterogeneous array of conflicts authorities call 'terrorism'.

Clandestine attacks on authorities by illegal groups have occurred for millennia. In that general sense, terrorism is nothing new. The novelty of recent terrorism, as Donatella della Porta indicates, lies in the frequent choice of symbolically, rather than materially, critical targets, the search for psychological effect, the matching of the message to the target, and the direction of many attacks against persons who do not have the power to respond to the perpetrators' demands. These tactics, when successful, simultaneously confirm the existence of the activist group, publicize their claims, demonstrate the vulnerability of the authorities, and hold third parties hostage to the response of authorities. Gradually they, like other forms of conflict, crystallize into recognizable routines.

One name for the array of means for conflict employed by any pair (or larger set) of actors is its *repertoire*. The theatrical metaphor conveys a sense of a limited number of relatively distinct routines calling for interaction among allies and enemies that the participants deploy according to negotiated rules, that are more or less familiar to all the participants, that vary from one performance to the next, and that those involved seek to manipulate to their own advantage. Innovation in the forms of conflict does, of course, occur, but it occurs chiefly at the periphery of forms that already have established places in the repertoire. Within conflict repertoires, moments of bursting creativity such as July 1789 or May 1968 are extremely rare. Thus nineteenth-century British political activists created the demonstration step by step as a set of variations on the public meeting, the petition march, and the delegation, all of which had some standing in the British repertoires of the later eighteenth century. By the later 1830s, the mass demonstration had become a standard tactic of groups seeking concessions from authorities.

The advantages of this innovation in the shadow of existing repertoires are obvious: participants in the new form have relatively little to learn, and to the extent that the repertoire has acquired *de facto* or even *de jure* legitimacy, opponents have more difficulty invoking legal and moral sanctions against innovations that seem to fall within its perimeter. Its disadvantage follows almost as a corollary: to check the innovation, opponents can use means similar

to those they have already employed in counteracting its predecessors. As a result, innovators in conflict constantly have to weigh the advantages of familiarity and legality against the undoubted advantages of surprise.

Collective-action repertoires vary according to the structure and history of the social relations in which they are embedded. Take the case of the social movement: the sustained challenge to authorities in the name of a population that lacks advantages many other populations enjoy. Some time between 1780 and 1880 the apparatus of the social movement as we know it today took shape in most western countries. It includes named associations, public meetings, announced programs, statements, slogans, marches, petitions, and recognized speakers for both sides. On the side of the authorities, it also includes standard routines for policing, spying, containing, hearing demands, and negotiating. Just as demonstrations often generate counterdemonstrations, social movements often generate countermovements representing parties whose interests the movements threaten. The social movement grew up in an environment in which states were expanding, capital was concentrating, special-purpose associations were proliferating, national police forces were differentiating from armies, and the right of assembly was expanding; it drew its form from these conditions, and helped modify them in turn.

Analysts of social movements have had an unfortunate tendency to write of them as if they were groups, when they actually consist of sustained interactions between challengers and authorities. Only rarely does a single, unified group make the challenge. Much more often, movement organizers spend much of their energy patching together coalitions, inventing grouplike names for those coalitions, suppressing rivals or inappropriate allies, and disciplining participants to maintain the illusion of a united front. The whole apparatus bears a remarkable resemblance to that of electoral campaigns. Nor is the resemblance fortuitous: the social movement came to thrive as a standard form of conflict when the expansion of suffrage gave political leverage to anyone who could provide public proof that large numbers of people supported a particular person, demand, or program. Since the nineteenth century, the social movement has occupied a large place in the conflict repertoires of most western countries. One of the major efforts in my own work has consisted of tracing the emergence of the social movement, and the transformation of its character and use, in France and Great Britain. Critics of this work have sometimes claimed that it neglects culture, ideology, and belief. They are partly right: I shun self-consciously cultural explanations of collective action for the tautologies they frequently contain, and doubt that variations in ideology suffice to account for the incidence of claim-making. Shared beliefs and social routines, however, pervade my analyses, in the guise of group identities, estimates of the likely outcomes of different courses of action, accumulated repertoires of contention, definitions of the situation, and conceptions of justice and injustice. All these cultural elements articu-

late closely with social ties – negative and positive – and thereby constrain popular collective action.

Yet my schemes sometimes strike critics as too rationalistic. Rudolf Dekker, reviewing the general history of Dutch collective violence, complains that conceptions 2 (individual action) and 3 (collective action) – and particularly my version of conception 3 – emphasize rationality too strongly. In rebuttal, he asserts that what he calls 'riots' do call up distinctive collective mentalities, which encourage people to acts that in other circumstances they themselves would judge irrational. He and I apparently agree on some points, and disagree on others. We agree that people in crowds often feel exaltation, anger, fear, or other strong emotions, and act on those emotions; the questions are whether such emotions run stronger or different in crowds than outside them, and whether they explain the general character of crowd action. We agree that people in crowds often behave in distinctive fashion, wreaking revenge or meting out justice collectively in ways that they would not dare to undertake individually; the question is whether a group mind, or something of the sort, explains the distinctive behavior. Recent work on the microdynamics of collective action suggest that objective changes resulting from the presence of others, communications processes leading to alterations in shared definitions of the situation, and the importation of social relations from outside the gathering account for the main deviations from individual rationality that appear in crowd setting.

The issue of rationality is a red herring. We who polemicized in the 1960s against views of collective action as 'irrational' and therefore worthy of repression no doubt bear some of the blame for letting that scarlet fish swim into current discussions. Eager to promote a hearing for antiwar activists, civil rights protesters, student rebels, and other makers of unconventional claims, we stressed the continuities between conventional politics and disruptive collective action. That was right, and helped reorient theories of social movements and collective action. But it also introduced an image of cool individual calculation, tight tactical coordination, and strict sobriety: the logic of algebra or investment decisions. We would have been better off pointing out that collective encounters have strong rules and regularities in the same manner that jazz, conversation, and football matches do; in none of the three cases does the underlying order exclude passion, rage, inspiration, or mayhem. In none of the three cases need we invoke a group mind to explain the underlying order. That does not mean, however, that existing models of

collective action explain everything well. In his study of popular collective action in the province of Groningen, Homme Wedman identifies one of the limits of conventional models, including my own: that in postulating the prior existence of shared preferences, interests, identities, and organization, they obscure the way that collective action itself forms groups and their orientations. He is right; the difficulty haunts theoretical traditions beginning with

societies, individuals, and social relations alike. The problem is to find an effective theoretical alternative to the *a priori* postulation of interests or the derivation of their changes from a second set of processes that are at least partly independent of collective action. Here historical work can contribute mightily to theoretical development, since the observation of long, continuous streams of organization and action makes it possible to examine models in which, with appropriate lags, collective action and social structure influence each other successively.

Dutch Collective Action

How might we historicize the study of Dutch collective action, and sociologize the history of Dutch collective action? The fundamental task consists of placing particular struggles in the frame of large, powerful social changes. Whatever other changes we consider, for the last few hundred years the transformations of states and the development of capitalism must figure very actively in any such analysis. We can take them singly, asking both how the growth of large, active, durable, centralized, heavily armed states affected claim-making and how the appearance of capital-concentrated firms, a highly commercialized economy, and the class divisions peculiar to capitalism affected it. Better yet, we can ask how the two sets of changes interacted. In my own analyses, I have found it useful to conceive of a two dimensional variation defined by a) the degree of concentration of means of coercion, which is the sphere of the state, b) the degree of concentration of capital, which is the sphere of cities, merchants, and manufacturers: Thus we can distinguish a coercion- intensive path (high concentration of coercion, low concentration of capital), a capital-intensive path (low concentration of coercion, high concentration of capital), and an intermediate path of capitalized coercion (greater equality between concentrations of coercion and capital).

With respect to state formation, municipal autonomy, and military organization, the paths marked out very different experiences in Europe. In the coercion- intensive category we find Russia and Hungary, with their reliance on co-opted landlords for state administration, their subordination of cities, and their creation of a bulky central apparatus. On the capital-intensive side we find Genoa and the Dutch Republic, where capitalists and municipalities dominated the state, the state disaggregated in times of peace, and little durable bureaucracy formed at a national level. In between we find the capitalized-coercion trajectory exemplified by France and England, where the agents of capital and of coercion balanced each other, and collaborated (however reluctantly) in the work of creating the large national armed forces, centralized

state structures, and extensive fiscal apparatuses of the national state. Eventually the military superiority of the national state made it the predominant type in all parts of Europe, regardless of the paths by which different regions arrived there. But on the way citizens or subjects in the coercion-intensive, capital- intensive, and capitalized-coercion zones experienced very different relations to powerholders, and even at the end the previous trajectory left an important residue in such areas as municipal privileges, uniformity of governmental structure, and differential class power.

On the whole, capital-intensive state formation concentrated in the relatively urban band extending from Northern Italy to Flanders and the North Sea. In those territories, sovereignty long remained fragmented, but when rulers or local oligarchies raised military forces they could borrow from their own capitalists and impose customs or excise on commercialized economies – at the price of considerable concessions to capitalists and to citizens' rights. Dutch experience illustrates the situation very well. From the revolt against Spain to the Napoleonic occupation, the Dutch state followed a capital-intensive, low-coercion itinerary. As Rudolf Dekker points out, the particular features of its domestic politics resulted to an important degree from its economic and political relations with the rest of the world. As an intensely commercialized economy, heavily involved in maritime trade, the Republic responded to the interests of its merchants and bankers as few other European states did. A mighty seapower and builder of a commercial empire, the Dutch state developed an exceptional sensitivity to conditions of war and peace around the globe. Yet, like other states in the capital-intensive track, the Dutch state remained fragmented, left great powers to the provinces and, especially, their capital cities, built little centralized bureaucracy, disaggregated with relative ease in times of peace, while drawing substantial revenues from its monetized economy and loans from its affluent burghers with relative ease.

These features of the Dutch state and economy marked the character of Dutch collective action. Dekker reminds us that in the Dutch Republic the beginnings of wars – quintessentially international events – typically sparked internal struggles over revenues and war powers. Maarten Prak points out that the *ends* of Dutch wars, for similar reasons, also incited more than their share of political conflict; the return to a peacetime footing, especially after a losing war, made state authorities vulnerable while reviving issues of prerogative and priority. Prak also identifies an important difference between France and the Dutch Republic: the much greater prominence of burgher-based movements for oligarchical privilege in Holland and its neighbors. He exaggerates the difference – during the Fronde, for example, struggles over municipal privilege aligned many cities against the Crown. Nevertheless, the general point is correct, and telling: In France, on its state-capital trajectory, kings from Henry IV to Louis XIV worked successfully to contain and

co-opt the once-great power of municipal oligarchies, and eventually made a Dutch-style municipal republicanism almost inconceivable. (Only *almost* inconceivable, since in the nineteenth century a new radical variant appeared in the Paris Commune and similar movements.) For much of Dutch history, municipal and national politics intertwined so tightly as to make distinguishing them nearly an idle exercise.

The Napoleonic conquest and subsequent reorganization brought the Northern Netherlands much closer to the model of the national state; in their dismantlement of the Napoleonic Empire, the great European powers created relatively centralized monarchies where none had previously existed. The Kingdom of the Netherlands (whose internal structure owed a considerable debt to the French model) came to life. Johan Frieswijk's analysis of landless laborers' collective action illustrates how that nationalization brought the Kingdom's conflicts into conformity with those of its neighbors. True, Frieswijk's portrayal of the first phase of landless laborers' actions as 'spontaneous' raises a whole series of professional doubts; the adjective has practically disappeared from standard accounts of popular collective action. Nevertheless, the overall trends of nationalization, unionization, and politicization Frieswijk identifies apply to the labor movements of adjacent national states, and for much the same reasons. Homme Wedman provides evidence of a similar reorientation of popular collective action in the province of Groningen.

Still – perish the thought! – the Netherlands did not become indistinguishable from its neighbors Great Britain, France, Belgium, and Germany. Homme Wedman provides an interesting indication of the difference from France in his observation that Dutch police archives are much thinner; residues of decentralization and municipal autonomy remained in the very system of policing. Throughout Europe, in fact, the structure and content of police archives (not to mention of police forces themselves) vary in strong relation to the overall organization of relationships between citizens and their states. Through their detailed studies of particular series of struggles, through their attempts to connect those struggles with large social transformations, and through their sensitivity to the analytic problems in making those connections, Dutch social historians are obviously helping to shape an intermediate discipline that is both history and social science. That disagreements should arise in the process is normal; our task is not to avoid confrontation among incompatible views, but to make confrontation fruitful. The practical work of Dutch historians and their foreign colleagues provides the most eloquent reply to the complaint that sociology and history are irreconcilable enterprises.

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