GLOBAL PATHS TO MODERNITY*

THIS IS AN EXCEPTIONALLY rich book, and a short discussion must be very selective. The following comments focus on the importance of Bayly's work for historical sociologists in general and approaches to modernity in particular. It seems appropriate to begin with a title that encapsulates a distinctive view. As in some other interpretations, a long 19th century provides the chronological framework for a great transformation; the first date, less precisely defined than the second, indicates that the story goes back before 1789, but the proximity in time suggests a direct background rather than a long-term build-up; finally, the emphasis is on the formation of a world, and thus on a global dynamic.

Bayly contrasts his approach with a traditional tendency to "see the 19th century as an age of relative stability between the cataclysms of 1789 and 1917" (p. 147). It may be added that "relative stability" has often been equated with more or less logical progress from one historical turning-point to another. Although the Marxist account of capitalist development and class struggle was – for much of the 20th century – the most influential version of that view, the same landmarks can be adapted to other perspectives. One line of interpretation portrayed 1917 as the triumph of a "totalitarian democracy" that had already been present on the scene in 1789; for others, the two breakthroughs were key episodes in an ongoing revolutionary process that did not lend itself to definitive theorizing of the kind proposed by orthodox readings of 1917. As the legacy of 1917 decomposed and the meaning of 1789 entered a new round of dispute, all historiographical models centred on the two dates lost ground. Bayly draws on critiques developed by a wide range of authors and integrates them into a narrative that can perhaps be seen as the most accomplished alternative picture of the long 19th century now available. His comments on the French and Russian revolution sum up the results of recent scholarship: "1917 should be seen as a sudden rupture in a pattern of gradually increasing and effective governance, not as it once was, as the culmination of inexorable social conflicts" (p. 266); as for 1789, revisionist historians have focused on a structural crisis of the monarchic state, leading to a breakdown of its *modus vivendi* with society, and the complex alignment of forces on both sides is now too well understood for the outcome to be seen as "the triumph of the bourgeoisie or of middle-class, market-oriented virtues" (p. 287). In both cases, the critical junctures and temporary reversals of state formation were linked to global constellations: hence the choice of 1914 rather than 1917 as the terminal point, and the reference to world events preceding 1789.

The formerly prevalent view of the two social revolutions and their epoch-making impact drew much of its strength from association with a

* About C. A. BAYLY, *The Birth of the Modern World, 1780-1914* (Oxford, Blackwell, 2004).

Johann P. ARNASON, Emeritus Professor of Sociolgy, La Trobe University, Melbourne [J.Arnason@latrobe.edu.au].
Arch. europ. sociol., XLVI, 3 (2005), 529-540—0003-9756/05/0000-812$07.50 per art + $0.10 per page©2005 *A.E.S.*

third major historical break: the industrial revolution. This link was particularly central to Marxist thought, but widely accepted in other circles as well. More recently, economic historians have been revising established views of the industrial revolution, and therefore also of its social consequences. Bayly discusses this question at considerable length and pulls together arguments otherwise presented in separate contexts. Drawing on the work of Kenneth Pomeranz and others, he argues that Britain crossed the critical threshold of industrialization later than historians once believed, and its global ramifications were even more delayed: "Industrialization itself only seems to have become a critical impetus to change after 1850" (p. 473). Its socio-political impact was not only very uneven, but also fundamentally ambiguous. Industrial transformations were not necessarily spearheaded by a triumphant *bourgeoisie*, nor were they conducive to a unified working-class consciousness with revolutionary aspirations. "Industrialization widely came to the aid of kings, priests and aristocrats" (p. 169): a whole chapter of the book (pp. 395-431) is devoted to the "reconstitution of social hierarchies" in the second half of the 19th century. And although new patterns of social conflicts and new forms of political action developed in industrial cities, directions and results varied widely.

Assumed linear connections between industrialization, social conflict and revolution were essential to theories which explained the trajectories of modern societies in terms of an internal logic; conversely, approaches that allow for multiple and contingent paths will be more sensitive to the global context and the local twists of its dynamics. Bayly's analysis of globalization is one of the most original parts of his argument; here I can only note a few key points. He rejects the all too common misconception of globalization as an unprecedented recent macro-historical leap, but he avoids the opposite error of diluting it into a continuous process. His model of successive stages begins with "archaic globalization" (pp. 41-44). This term refers to patterns of expansion and interdependence that can be traced back to the early Eurasian network of civilizations; Bayly singles out three main factors that shaped this stage of global interaction. Ideas and operative models of universal kingship prompted expansion as well as a search for rare resources and prestigious objects. Cosmic religions (a broader concept than world religions in the strict sense) inspired pilgrimages and wanderings, but also – in some cases – more warlike enterprises. Finally, biomedical beliefs placed a high value upon exotic products, and thus reinforced the far-flung trading networks that were also dependent on the two other factors.

Archaic globalization did not simply disappear when world history entered a new stage: well-established networks persisted both inside and alongside new ones, and early European expansion is best understood as a particularly sustained push for insertion into archaic networks, rather than a radical innovation. But as this process gathered momentum, it gave rise to more novel trends which – for Bayly – mark the onset of early modern globalization. The growth of inter-regional (and more particularly Atlantic) trade, closely linked to mercantilist state power and overseas imperial expansion, changed the course of world history. Although the most decisive inputs came from Western Europe, the resultant developments unfolded in a worldwide arena and in conjunction with shifts occurring simultaneously in different settings, but accelerated and brought into contact by quickening global interactions. The final phase of what Bayly calls the "great domestication", the displacement of nomadism by settled agrarian populations, coincided with the early modern stage of globalization and affected many parts of the world in varying ways. More importantly, Bayly generalizes the concept of the "industrious revolution", first coined by economic historians of Europe as a replacement for the less adequate term "proto-industrialization", and shows that it can be applied to a wide variety of changes in Afro-Asian societies. Industrious revolutions involved reorganizations and improvements without major technological breakthroughs, led to more effective division of labour as well as more intensive commercial exchanges, and were in the most important cases linked to worldwide diffusion of tropical products. It would, however, be misleading to identify them with advances of small-scale production: as Bayly notes, the slave economies across the Atlantic, based on "a flexible, financially sophisticated, consumer-oriented, technologically innovative form of human beastliness" (p. 41), must be ranked among the most significant cases in point. Industrious revolutions and expanding peasant economies shaped the global environment in which industrialization was to score its first successes. The slave plantations were an integral part of the North Atlantic economic world on the eve of the industrial revolution, and although statistic criteria are disputed, some economic historians stress their strategic importance; at the same time, Bayly notes that "Britain at the point of 'takeoff' of industrialization had the largest tributary peasantry in the world, in Highland Scotland, Ireland, India and Africa" (p. 418).

However, these historical and structural connections between economic regimes are not the main themes highlighted in Bayly's genealogy of globalization. He rejects all versions of economic determinism, and his analysis of 18th century developments underlines the formative role of political as well as – to a lesser extent – ideological factors. The key phase of the transition from early modern to advanced modern globalization was, first and foremost, a time of massive political changes, crises and conflicts that transformed the pre-industrial world and imposed specific conditions on the emerging industrial one. In fact, Bayly's account of the long-drawn-out "world crisis" that engulfed states and empires between 1720 and 1820 is so convincing that it raises questions about the relevance of 1780 as a watershed. At the most basic level, his interpretation assumes that imperial overstretch – in the sense of a tension between ambitions and resources – was a ubiquitous factor, latent in some cases but more visible in others, and that it was aggravated by new military techniques, new openings for expansion, and more acute inter-imperial competition within and beyond the

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

Eurasian macro-region. Because of growing interdependence, global dynamics could affect particular regions in ways that then led to worldwide repercussions. One of the more unexpected results of Bayly's globalistic approach is a plausible case for seeing the intertwined collapses of the Safavid and Mughal empires after 1720 as beginnings of a much broader and more long-drawn-out process. The historical forces at work shifted to a new arena and a higher level with the mid-century conflict (the first veritable world war) between major European powers; this triggered a "European military-fiscal crisis'' (p. 93) which in turn paved the way for the French Revolution and a last round of the British-French struggle for hegemony.

Bayly devotes less space to the French Revolution than standard histories of the period tend to do, and his line of argument diverges markedly from the most influential ones. In his view, the most salient and durable result of the worldwide revolutionary crisis around 1800 was a "moral rearmament of the state''. (The early history of the United States is noted as a major exception, but the American pattern of institutionalized anti-statism was not, in the long run, immune to the trends that prevailed more easily in Europe.) There is much to be said for this view. Upgraded or reconstructed states profited from accelerated growth of knowledge, adapted revolutionary methods of mobilization to their own purposes, and took advantage of new legitimizing devices linked to the idea of the people. State-strengthening efforts, prefigured by successive revolutionary leaderships in France, went much further under the imperial regime that inherited their legacy, and continued along various lines in post-revolutionary conditions. But there was another side to the revolutionary transitions from 18^{th} to 19^{th} century forms of statehood. Bayly stresses the role of "cultures of opposition'' in the downfall of old regimes, and he notes contrasts and parallels between modes of critical thought within major Old World civilizations. He has less to say on the epoch-making mutation of critical discourse that occurred in conjunction with the French Revolution. The title of the subsection that deals with ascendant states also refers to "ideological origins of the Modern Left'' (p. 106), but this theme is marginal to the main argument. There is no discussion of the Jacobin moment; in view of the interest that historical sociologists have recently taken in Jacobinism (seen as an ideological matrix that lends itself to divergent political uses), this seems a significant omission. The "converging revolutions'' around 1800 did not only shift the global balance of power in favour of Europe and its overseas offshoots; they also gave birth to ideas and projects that were – over a longer period – to become essential sources of inspiration for non-European "cultures of opposition'' and their attempts to resist European domination.

The last point should be linked to the broader issue of European exceptionalism. Bayly's general statements on this question are more balanced and judicious than most other contributions to the current debate. He admits exceptional features of the European trajectory, but cautions against the

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

search for unique and decisive causes. "Europe's temporary and qualified exceptionalism was to be found not in one factor, but in an unpredictable accumulation of many characteristics seen separately in other parts of the world'' (p. 71). During a crucial historical period, Europeans and North Americans developed "more effective tools for accumulating money, power and knowledge'' (p. 71) than societies in other parts of the world. It is also true that within this combination, some factors seem to have acquired a dynamism unmatched in other cases: this applies to the "particular buoyancy of the European idea of knowledge and its material rewards'' (p. 80), but also to innovative relationships between war and finance. In short, Europe's competitive advantage was based on intricate and changing mixtures of inventive and destructive capacities. This approach is a welcome relief from the now fashionable Euro-bashing which masquerades as a critique of Eurocentrism; but some specific formulations appear to concede too much. When Bayly suggests that the 18th century intellectual achievements of "most world civilizations'' (p. 291) stand comparison with European scientific rationalism, and refers to Islamic thinkers laying "much stress on the rational sciences'' as well as Chinese ones making "a concerted move toward empirical observation of man and nature'' (p. 291), objections are in order. Interest in the "rational sciences'' did not loom large in the context of 18th century Islam – it was, in world-historical perspective, overshadowed by a much more momentous resurgence of Islamic revivalism. As for 18th century Chinese "empiricism'', Bayly notes elsewhere – in agreement with sympathetic historians of Chinese thought – that it was of a very specific and restrictive kind: it had to operate within a framework that justified only reinvention of classical ideas (p. 484). A critical spirit was at work, but within the tradition and without ever raising radical questions about its foundations. There was certainly no wholesale disestablishment of tradition in 18th century Europe, but there were much more acute tensions and overt challenges. At several points in the book, Bayly insists – with convincing reasons – that science, in the double sense of a coherent body of knowledge and a distinctive mode of discourse, crystallized at a later date than historians have often assumed. This does not, however, alter the fact that its cognitive building blocks and the cultural interpretations that served to tie them together were in the making for a longer time. Bayly's perspective on the history of ideas does not do justice to the interplay of scientific practices and broader intellectual orientations; he treats positivism – a persistent and adaptable intellectual current – separately from the scientific progress with which it tried to identify its philosophical message, defines it in a narrow sense, and limits it to a small intellectual niche (p. 308).

So far I have discussed some core aspects of the background to Bayly's global history; it is now time for a brief bird's eye view of the global dynamics as such. Bayly's central thesis, reiterated in different contexts throughout the book, is that growing uniformity of ideas, practices and power structures was accompanied by growing internal complexity of the societies caught up in the homogenizing process. In the concluding chapter, this perspective is summed up in terms of "contested uniformity and universal complexity'' (p. 478). Uniform patterns were, at the end of the long 19th century, more visibly confronted with resistant forces than before; the trend towards greater complexity had, however unevenly, transformed the whole spectrum of human societies and was reinforced by the very factors – not least the pressures of interstate and inter-imperial competition – that imposed uniformity. To get a grip on Bayly's argument, it may be suggested that he is bringing together – in an original way and from a historian's point of view – two sets of questions which sociological analysis has tended to keep apart. Higher levels and new forms of complexity have been of particular interest to those who work with functionalist models, especially when functional analysis is combined with an evolutionist perspective; the growth of global uniformity, although often taken for granted, has received less attention, but the most concrete analyses have linked the issue to state formation and interstate competition.

Bayly's account of 19th century transformations gives pride of place to the state. His multi-dimensional conception of globalization excludes any notions of a prime mover, but within that framework, there is no doubt that the state stands out as the most central agency (that fits the emphasis on an 18th century head start of political globalization) and the most effective homogenizing force. By contrast, capitalism – famously described by Max Weber as the most fateful force of modern life, and seen by many others as the levelling machine par excellence – is much less clearly defined and less systematically integrated into the story. Bayly's concluding reflections refer to "powerful economic convergences'' created by industrialization (p. 473) and a "growing uniformity of social processes'', inherent in a world economy (p. 481). When it comes to details, he pinpoints the workings of capitalist principles and mechanisms in various contexts, and he identifies the "triumvirate of royalty, capital and land'' (p. 476) as a common denominator of European power structures; but the conflict-ridden combination of capital and industry is never examined in a way comparable to the analyses of relationships between state and society. To note this is neither to suggest that a focus on capitalism would *ipso facto* provide a better key to global history – Bayly is right to reject the "conceptual dustbins'' (p. 475) of world system theory – nor to belittle the lessons that can be learnt from Bayly's survey of modern states in the making.

The underlying theoretical perspective is specified in opposition to Marxist views as well as to more recent visions of an autonomous and invariably triumphant state. On the positive side, Bayly's stance has obvious affinities with the historical sociologists who have tried to reconstruct and compare processes of state formation (Norbert Elias and Michael Mann are the outstanding examples). His line of inquiry amplifies the agenda of comparative research in several noteworthy ways (in part because the main part of his narrative begins roughly where the most seminal sociological work on state formation, Elias's *Civilizing Process*, came to an end). One of the most useful analytical innovations is a distinction between different forms of "statishness", with correspondingly specific dynamics of state formation (pp. 254-260). Centralized state building was more characteristic of continental Europe than of Britain or the United States, but in the latter cases, the diffusion of power amongst local elites and institutions may be seen as a distinctive form of "governmentality" and a basis for alternative paths of state formation. A very different situation prevailed in colonies where corporate bodies had absorbed or introduced the elementary forms of statehood. In some non-Western societies, religious institutions were autonomous enough to operate as more or less activist counter-states; this was the case in Muslim countries (although not everywhere to the same extent) and some Buddhist ones. In such conditions, the diffusion of European state-building models and techniques did not proceed in the same way as where they were imposed on tribal polities or incipient state structures. One could add to this list the specific problems of premodern empires in transformation, often rooted in traditions which obstructed the progress of governmentality but at the same time forced to adapt to a world dominated by more innovative rivals. In any case, Bayly's typology of institutional patterns and geographical sectors should be taken on board by all those interested in the historical sociology of state formation; the same applies to his analyses of the redefinitions of justice involved in state building, the dialectic of controls over and obligations to society, as well as various other aspects of a complex argument.

The other part of the picture, growing internal complexity, seems more diffuse and Bayly's treatment of it less directly related to sociological schools of thought. He refers to the complex division of labour that characterizes a global industrial economy; the progress of professionalization in many areas of social life; last but not least, the growing diversity of social forces entering a field previously dominated by intertwined economic, political and cultural elites, and of the strategies developed by elites, counter-elites and popular movements in an environment that offered new options to actors at all levels of de-stabilized social hierarchies. His account of these developments is convincing, and there is no reason to disagree with the argument that links worldwide homogenizing trends to growing social differentiation. But it is still possible to raise questions about the range of variations – regional, structural and historical – in the relationship between the two aspects of modern globalization. To do so is to enter the debate on diverse paths to and patterns of modernity – "multiple modernities", to use the term most popular among participants. Apart from a brief reference at the beginning, the book does not touch upon this problematic. The author who did most to initiate the debate in the 1980s and 1990s, S.N. Eisenstadt, is mentioned once – with reference to a book published in 1966, and closer in spirit to classical modernization theory than to the new ground explored in his later writings.

It might be objected that the whole problematic of "multiple modernities" has more to do with the 20th century than with the long 19th one: after 1914, the historical contingency of established models was more manifest than before, alternative projects more relevant and the idea of one main pattern (Parsons) more problematic. The idea of "multiple modernities" is to a great extent based on a rediscovery of 20th century experiences, in explicit opposition to the simplifying and idealizing constructs of classical modernization theory. It does not seem illegitimate to extend this re-sensitizing perspective into a more distant past, beginning with the long 19th century; neither a deterministic nor a teleological vision of history is needed to justify the search for antecedents or prefigurations of a later diversifying turn. With this in mind, a few words should be said about Bayly's discussions of three subjects – nationalism, religion and political ideologies – that have some bearing on the more general theme of plural modernities. They are, in their specific ways, related to the cultural components that analysts of "multiple modernities" have invoked as differentiating factors.

Bayly refers to nationalism and empire as "among the few thoroughly 'theorized' historical subjects" (p. 199). This is an overstatement, particularly with regard to empire (which is in fact a theoretically underdeveloped topic); but here our main concern is with nationalism. Although it has caused lively controversies, the debate has on the whole been characterized by short memories and unmerited success of simplistic ideas. Several innovations in recent scholarship have, however, broadened the frame of reference and laid the groundwork for more adequate interpretations. Non-European forms of nationhood and nationalism have been studied much more extensively than before; long-term processes of nation formation, more or less closely linked to the trajectories of state formation, are now increasingly seen as a key theme for comparative analysis (in the European context, but arguably not only there, genealogies of this kind go back to the Middle Ages); the invidious dichotomy of civic/Western and ethnic/Eastern nationalism, often presupposed in earlier discussions, has given way to more nuanced typologies.

Bayly responds most directly to the first line of inquiry. Against diffusionist theories, he stresses the indigenous roots and autonomous dynamics of Afro-Asian nationalisms; but the emphasis is squarely on parallel developments in European and non-European settings. The issue of different types of national identity and nationalism is not raised with regard to their impact on overall patterns of modernity (late 19th century Japan already exemplified such connections). The importance of long-term links between state and nation formation is noted, but with some skepticism about the stronger claims in that vein; Bayly does nevertheless make a valid point when he argues that the analysts of nationalism have not paid enough attention to armed conflict and its consequences (including the historical breaks it can cause). At the same time, he makes unnecessary concessions to the modernist orthodoxy. For example, it is definitely not true that Ernest

Gellner's theory of nationalism "works best for the central and eastern European societies which were to the forefront of his mind'' (p. 303): it is a commonplace among Gellner's critics that his model fails in Poland and Hungary, and reflects at best a very partial grasp of the Czech case. As for typological approaches, Bayly distinguishes two ends of the spectrum: states and nations created together out of "old patriotisms'', and nationalisms created by states (p. 207). This is of course a less prejudiced starting-point than the civic/ethnic stereotype – for one thing, the distinction is value-neutral. But the only intermediate case mentioned in this context is the multi-ethnic imperial formation, and here the focus is on common problems of European and non-European empires. There is no reference to the different (and unequally developed) paths of imperial modernization that were to ramify into major historical divergences in the 20th century. On the other hand, it should be acknowledged that Bayly puts forward a convincing interpretation of the relationship between nationalism and imperialism on the European side: not that the latter was a natural expression of the former, but the combination of an increasingly assertive nationalism with imperial power structures constituted a general precondition for other factors coming into play. Even here, though, the mutations of nationalism are not given their due. The "integral nationalism'' of the late 19th and early 20th century, aiming at nothing less than a reconfiguration of modern societies and now recognized by historians as a fountainhead of fascist projects, does not enter into the picture.

The chapter on "Empires of religion'' (pp. 325-361) is one of the most challenging parts of the book. The once widely assumed identity of modernization and secularization has come in for criticism from many quarters, but this is surely one of the most sustained and conclusive statements on the subject. Briefly, Bayly's thesis is that the expansion and internal strengthening of world religions was "as important as, if not more important than, the rise of nationalism and liberalism'' (pp. 363-64); that this resurgence occurred within different traditions and civilizational complexes, not necessarily in ways that advantaged the religion of the ascendant Western powers; and that the strategies of reform, aiming at "the reformulation of doctrine and authority'' (p. 325), were fundamentally similar. A universal effort to rationalize religious beliefs and institutions – often in a formal rather than substantive sense – was part and parcel of the global shift towards uniformity. Bayly describes these transformations of religious life in convincing detail. But our understanding of them will depend on basic assumptions about the meaning and history of religions. Repeated asides point to unsettled questions in this regard. According to Bayly, "the 19th century saw the triumphant reemergence and expansion of 'religion' in the sense in which we now use the term'' (p. 325); Confucianism, despite obvious parallels with religious traditions elsewhere, "was not really a religion'' (p. 341); as for Hinduism, "it is doubtful if a religion existed in this conventional sense'' (p. 336). Further discussion of the conceptual issues thus indicated would have led to closer engagement with a particular school of thought: the Durkheimian branch of the sociological tradition. More than any other classical sociologist, Durkheim adumbrated a theoretical framework for a socio-cultural history of religion. In particular, he distinguished two dimensions of change, both of them crucial to the transformations of modern societies: religion retreats from its traditional role as a "meta-institution", a comprehensive framework of social life, and secular institutions assert their autonomy, but at the same time, religious meaning – in the sense of sacred status – is projected onto new values and practices. This double-edged transformation of religion, briefly but unambiguously outlined in Durkheim's work, has attracted the interest of contemporary scholars such as Marcel Gauchet and Charles Taylor. Bayly's account of 19th century religions, "partly expelled from the workings of politics and the state" (p. 338) but seeking compensation in new spheres and transforming themselves in the process, converges with their analyses and adds some very important insights to them. These affinities with Durkheimian and post-Durkheimian thought remain latent, and there is no hint of the multiple developments that could be envisaged within that framework (in terms of different combinations of the two trends). When Bayly discusses the reconstituted "empires of religion", he rightly stresses their indigenous cultural resources; but the 19th century bridges between older legacies and later divergences would merit more attention. The 18th century Islamic revival runs parallel to the European innovations that were largely channelled into nationalism and state-building, and it "may yet outlast them" (p. 77). This very reference to Western political history may serve as a reminder of basic contrasts: the Islamic mode of reconstitution has not drawn the same kind of dividing line between religious belief and socio-political regulation as the European one, and although the difference has not crystallized into a distinctive Islamic pattern of modernity, the ongoing and internally contested search for such a model has been a significant historical force. The Confucian record differs from comparable cases in that this tradition suffered – at the end of the period covered by Bayly's book – a wholesale institutional breakdown. The long-established Confucian civilizational paradigm collapsed and could not be rebuilt. In this institutional vacuum, ideas and images drawn from the Confucian tradition – and from the broader cultural complex over which it had held sway – could still influence 20th century Chinese history in elusive but momentous ways. Finally, the 19th century redefinitions of Hinduism were contested, and increasingly so during the last decades of the period; it was of major importance for the formation of Indian modernity that attempts to channel a revitalized tradition towards nationalism were overshadowed by strategies that combined a multi-traditional vision of India with liberal models of political organization.

Durkheim's programme for a sociology of religion included keys to the phenomena that came to be known as "secular religions". For Durkheim, the most prominent secular mutants of the sacred had to do with foundational values of democracy, but the much more detailed later analyses centred on anti-democratic ideologies and utopias. Although this whole field of inquiry is very much a 20th century issue, a history of the preceding period bound to raise questions about sources and potentials; in particular, the socialist tradition calls for closer scrutiny from this angle. Bayly's comments on socialism (pp. 308-312) highlight its complicated relationship to traditional religions and their various ways of adapting to a new world. In his view, the socialist movement – including its professedly Marxist and therefore "scientific'' branches – owed much of its appeal to an open or tacit alignment with older religious visions of reform and a better future. This claim is grounded on solid evidence; but much less is said about the novel and specific message of socialism as a secular religion, based on the promise of a second modernity that would grow out of the conflict-ridden progress of a really existing one. The vision of a second modernity was deconstructed by classical sociology at the end of the period discussed here (Durkheim, Simmel and Weber did it in different ways). The sequel to that exemplifies the non-synchronicity of intellectual and political history: at the very moment when the socialist project of a second modernity had been subjected to decisive criticism, it mutated into a rationale for an alternative modernity alongside the really existing one. That part of the story goes beyond Bayly's brief. But the achievements of classical sociology – and their *fin-de-siècle* intellectual contest – should perhaps have been acknowledged. If the "language of symbols in Western art'' (p. 389) rates a mention, the same ought to apply to the language of concepts in Western thought.

The question of socialism as an imagined second modernity brings us back to the more general problem of defining modernity: a world may have been made between 1780 and 1914, but why call it modern? To conclude this discussion, let us return to Bayly's introductory remarks. Here he takes a clear and well-grounded position; it is less clear whether its implications are fully articulated throughout the book. He "accepts the idea that an essential part of being modern is thinking you are modern'' (p. 10), i.e. perceiving the present as a break with the past and envisioning the future as open to further movement in that direction. This basic cultural orientation translates into more specific institutional dynamics, most importantly those of the capitalist economy and the bureaucratic nation-state. Their triumphs and transformations unfold on a global scale, and that adds another aspect to the modern condition: it becomes a "*process* of emulation and borrowing'' (p. 10), an interplay of unequal development and aspirations to match or even outdo the most advanced pioneers of the process. In this global field, the West was at first "both an *exemplar* and a *controller* of modernity'' (p. 12). Other exemplars and controllers emerged later. But to what extent did they represent or presage new possibilities and paradigms? Was "Japan's partially self-fashioned modernity'' (p. 12) to some degree a self-defining one, and thus a precursor of further differentiation? More generally speaking, the link between the cultural premises and the institutional forms of modernity is flexible enough to leave a large space for mutually contested translations, critical responses to them, and attempts to transcend a given configuration of projects and counter-projects. Bayly signals some of these developments in the introduction (including rejections of modernity by thinkers fully abreast of the modern experience), but his reflections do not lead to any sustained exploration of the them in the main parts of the text.

The present review is written from the viewpoint of a historical sociologist, and therefore focused on issues central to debates in that field. But the above criticisms and reservations are not meant to detract from the merits of a book that can teach historical sociologists much more than they can criticize. And because of limited space, I have left out many valuable parts of the argument. To mention a few topics that would deserve more comment: the analysis of the mid-century global conjuncture of revolutions and civil wars (pp. 125-169) and the chapter on the destruction of native peoples (pp. 432-450) break new ground; so does the concluding section on the "great acceleration'' that took a "liberal civilization born of the compromise between revolution and hierarchy'' (p. 467) off the rails in 1914. These contributions, together with others mentioned above, constitute one of the most insightful and substantial responses to the "continuing riddle of the modern'' (p. 9).

Johann P. Arnason

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

PAUL STARR'S *The Creation of the Media*, a path-breaking book on the scale of his earlier *The Social Transformation of American Medicine*, offers a powerful response to a uniquely American "problem". That problem is the mythology that has come to surround the First Amendment to the US Constitution which specifies that "Congress shall make no law... abridging the freedom of speech, or of the press". Many Americans, not least of all journalists, seem to believe that other than by keeping its hands off, government has had nothing to do with the considerable success of the US press. The *Creation of the Media* provides substantial evidence against this first prejudice (that the state has done nothing positive), while staunchly and less convincingly defending the second (that the US is the best).

Starr is not the first scholar to insist on the political shaping of the media, as his ample footnotes attest. Isolated from one another, these specific case studies might be dismissed as just occasional lapses from a *laissez-faire* tendency. Simply through its encyclopedic breadth, *The Creation of the Media* makes it irrefutably clear that political intervention was the rule, not the exception – even in America! This, of course, is a hopeful finding, because it reminds us that media are the products of human agency, not the inevitable result of economic or technological forces.

Embedded within Starr's layered historical narrative is a theoretically-sophisticated causal model of media development – what one might term historically-contingent political culture. Challenging the technological determinists, Starr shows that technological development was almost always anticipated and guided by political "constitutive" choices. These choices, Starr suggests, concern three areas: legal and normative rules (access to information, privacy, intellectual property, free expression), specific design of media networks and industries, and broad institutions "related to the creation of intangible and human capital – that is, education, research, and innovation" (p. 5).

Against economic arguments that politics serve merely as a vehicle for the implementation of underlying commercial interests, *The Creation of the Media* demonstrates that democratic ideals and values also guided policy choices. By stressing historical contingency and political struggle, however, Starr also distinguishes his approach from more static "policy paradigms" used by Frank Dobbin and other new institutionalists to explain enduring cross-national differences in industrial policies. For Starr, no outcome is ever inevitable, and "no single idea, interest, or condition explains the distinctive path taken by communications in America" (see pp. 14, 436). Starr is at his best when he describes the complex interplay of contingent historical factors

* About Paul STARR, *The Creation of the Media: political origins of modern communications* (New York, Basic Books, 2004).

Rodney BENSON, New York University [rodney.benson@nyu.edu].
Arch. europ. sociol., XLVI, 3 (2005), 541-544—0003-9756/05/0000-813$07.50 per art + $0.10 per page©2005 *A.E.S.*

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

that explain why media technologies developed one way rather than another. For instance, the privatization of the telegraph, which set the pattern for the telephone, radio, and television industries, was far from pre-ordained. During the early 1840s, the editor of the *New York Herald* expressed the common view, also held by inventor Samuel Morse, that the "government must be impelled to take hold'' of the telegraph as part of the Post Office (pp. 163-164). Yet the US government never did take hold, and the explanation lies not in some timeless policy paradigm, but in the contingent confluence of multiple factors – political (increasing North-South conflict, and the 1844 election of a Democratic president opposed to a policy perceived to aid the industrialized North), economic (depression, the financial default of several government-funded railroad and canal projects, and thus a greater skepticism toward "internal development'), and business strategy (an overly timid initial development that failed to generate impressive returns).

In sum, relative to Europe, America's "distinctive path in communications'' has consisted of three elements: first, earlier and more rapid development of media systems; second, broader geographical extension (into rural areas, as well as cities) and popular accessibility (not oriented only toward elites); and third, a higher level of technological innovation (p. 227). The dependent variable in Starr's analysis, however, is not always clear. As an "engine of wealth and power creation'', he insists, the "American framework of communications'' has no equal (p. 3). Indeed, there would be little argument that the most powerful global "Media are American'', to borrow Jeremy Tunstall's memorable phrase. More problematically, though, Starr also wants to argue for American superiority in the democratic qualities of its mediated public sphere.

Starr's American triumphalism holds up best through the first half of the 19th century. Ironically, the First Amendment was the least of the American press's early supports; with only minor exceptions, the Supreme Court "did not uphold a single claim based on the First Amendment until after World War I'' (p. 81). The American advantage lay more in geography and overall political structure. Even if the government had wanted to censor political dissent, the sheer size of the new nation, further fragmented by a federal system of governance, made it nearly impossible. Perceiving their young republic as having a greater need for information flow than social control, American political leaders did far more than their European counterparts to pro-actively promote communication. For example, "while the Europeans taxed publications, the United States subsidized the growth of independent newspapers through cheap postal rates'' (p. 16). By 1850, through its education, tax, intellectual property, and postal policies, the American government helped assure a higher rate of literacy (with the exception of Sweden), more affordable access to a wider range of books and newspapers, greater protections of citizen privacy, and greater transparency in governmental policy-making and administration than existed anywhere in Europe (p. 105).

Starr's tale of American superiority begins to break down in the late 19th century. Although political censorship laws were decidedly more strict in France (and to a lesser extent England) through most of the 19^{th} century, from the 1880s into the 1930s, it was the United States which led the way in sweeping censorship of morals and manners. Starr also shows that America's early unregulated private telegraph (Western Union) and news service (Associated Press) monopolies offered few if any democratic advantages over their nationalized counterparts in Europe. In Britain, a single nationalized postal and telegraph system facilitated the rise of multiple, ideologically competing news services (p. 179); in contrast, the Associated Press and Western Union often abused their monopoly power to monitor private telegraphic communications, and selectively present or suppress news, in one case to assure election of the Republican presidential candidate over his Democratic rival (pp. 186-187). Throughout most of the 20^{th} century, one would be hard pressed to argue that political dissent, especially from the left, has been better protected, let alone promoted, in the US than in Western Europe. And in the current era of corporate media consolidation, American press law, with its focus on governmental abuses, has often been tragically ill-equipped to counter overarching business power.

Starr captures well the complexity of communications policy, and how positive outcomes are as often as not the unintended results of political choices. However, one consistent prescriptive lesson he draws – the virtue of decentralization over centralization – is debatable. Certainly, decentralization may be one factor that allows a media sector to resist government efforts to control it, as when the centralized US movie industry became a much easier target of censorship than the more dispersed book, magazine, and newspaper publishing houses. But the link between decentralization and other democratic media virtues are not so clear cut. A fragmented media system is also one in which voices are dispersed rather than joined in debate, and lacking debate, these voices may become homogeneous echoes of one another rather than distinct ideological alternatives – as is the case with the almost interchangeable monopoly newspapers, chain-owned or not, that now dominate most American metropolitan regions. If centralization increases the threat of government control, it also seems to intensify democratic political life. Even today, there is probably more genuine intellectual diversity and lively debate in the concentrated media and intellectual milieus of Paris or London than across the wide stretch of the American continent.

The Creation of the Media leaves off exactly at the historical moment – the post-World War II era – when America's democratic advantage becomes least obvious. Since the 1950s, European public service broadcasters and politically-engaged national newspapers have often contributed to broader citizen participation and more reasoned, critical public discourse than one finds in the United States. In recent years, C. Edwin Baker and other US legal scholars have called attention to the negative "externalities" produced by America's hyper-commercialized media system, and have called for targeted government intervention to promote speech that is being silenced by market mechanisms. In failing to cite this literature, in his frequent praises of the virtues of advertising funding, and in his dismissal of the Frankfurt School's critique of consumer culture, Starr implies a normative preference against non-commercial alternatives, when in fact, these are also part of America's historical legacy and are perhaps needed now more than ever.

While Starr the historian reminds us that things might have turned out differently for America, Starr the comparativist seems just as content that they did not. *The Creation of the Media* sometimes reads like the account of a tourist who goes abroad only to confirm her existing prejudices about the advantages of home. Nevertheless, Starr has presented a provocative thesis about American-European differences that calls for further testing. This future research should build on Starr's strong finding of cross-national media policy differences to increase our understanding of their complex links to media content and form, as well as the contours of democratic political life.

RODNEY BENSON

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

ETHNIC MIGRATION IN THE LIBERAL STATE *

COVERING A RANGE of "liberal states", representing the variety of immigration politics found in the Western world, Christian Joppke's new book charts what he calls the contemporary "de-ethnicisation" and "re-ethnicisation" of the criteria used by such states to select or refuse incoming "ethnic" migration – either of co-ethnics returning to their "homeland", or of foreigners (typically non-Western) seeking entry to a Western country. Joppke's distinctive, wilful, often truculent voice, amidst the boom in scholarship on immigration, citizenship and the nation state in recent years, is always an essential one. He is perhaps the scholar who has done the most in his previous work to shape coherent analytical debates out of a very uneven patchwork of historical, sociological and political studies. Readers familiar with these debates will be impressed that he has managed to make another, highly original cut into this now widely covered area of research; another intervention that will infuriate some, and provoke others to think again and anew about citizenship and nationhood in the modern world.

Many otherwise liberal states have in the past century offered preferences towards "ethnic migrants" on the basis of historical or cultural affinities to the receiving countries. Classic examples are the former "whites only" policies in Australia; preferences for *aussiedler* in Germany, or Latin American return migrants in Spain; or the openness of the Israeli state to Jewish settlers after World War Two. These "selection by origin" policies represent a longer term continuity with historical nation-building processes, that have often depended on anchoring "ethnic" and "cultural" roots alongside modernist, universalist democratic norms. Joppke thus sets out to analyse and contrast three types of receiving states, that each represent distinctive "constellations" in immigration policy: "settler" states, such as Australia and the USA; "postcolonial" states, such as France and Britain in the North West of Europe, and Spain and Portugal in the South West; and "diaspora" states – the most familiar examples of "ethnic migration" – such as post-war Germany and Israel, who have had proactive policies favouring ethnic in-migration. He charts the different trajectories across these states, that have led on the whole to the strong de-ethnicisation of immigration policies. One by one, such liberal states have given up their ethnic preferences, in favour of human rights influenced norms that recognise no discrimination. The book builds on each national case, towards a powerful comparison of the de-ethnicisation of the German nation state post-1989, with the continued maintenance of ethnic Jewish preference on immigration in Israel.

Joppke's technique is "comparativist" in the interpretative/literary sense: juxtaposing political stories, recounting case law, teasing out context-

* About Christian JOPPKE, *Selecting by Origin: Ethnic Migration in the Liberal State* (Cambridge, MA, Harvard University Press, 2005).

Adrian FAVELL, Associate Professor, Dept of Sociology, University of California, Los Angeles [afavell@soc.ucla.edu].
Arch. europ. sociol., XLVI, 3 (2005), 545-551—0003-9756/05/0000-814$07.50 per art + $0.10 per page©2005 *A.E.S.*

ual variation, but proposing only the most tentative explanations. The methodology is the familiar technique Joppke has used to effect in his previous works: a heavily theorised historical reconstruction, built on political sources, legal texts and a vast array of secondary literature. The book confirms a long-standing liberal trend in the de-ethnicisation of the nation, boldly stating that, on immigration and citizenship, liberal states are beginning to resemble one another the world over. Western nations are converging on universal rights-based principles, that are likely to see "culturalist" nation state-based thinking giving way in the future to states "merely" managing rational supply and demand led processes. He also points to exceptions to this trend: counter-currents of re-ethnicisation, such as the bolstering of *jus sanguinis* for long distant emigrants from France and Italy, or wrangles over co-ethnic external minorities in Central Europe. Yet even these cases are nevertheless circumscribed by his strikingly optimistic, modernist account of the triumph of liberalism. Doubtless, he would tell us to look beyond the apparently growing nativism – not to say ugly racism – of much recent anti-immigration politics in Europe: whether in "hyper-liberal" states like the Netherlands or Denmark, or more familiarly ethno-cultural states, such as Haider's Austria, or Berlusconi's Italy. Joppke's view, rather, is the *longue durée* of modern liberal norms, and their functional triumph. Crucial to this argument is the stress he makes on the difference between the contemporary liberal state, hemmed in by universalistic norms and international rights and standards, and the classic nation-building nation state of the late 19th and early 20th century, that was so much more aggressive and coercive in its cultural construction of national society and bounded territories.

It is hard not to be swept along by this brilliantly argued and constructed text. Joppke is a German who writes much better English prose than most native speakers – whether the dry monotony of much North American sociology, or the jargon-ridden pretence of most British social theory. It is a good thing that, on the whole, *Selecting by Origin* is a rather less truculent, rather more considered text than some of his past polemics. It will however certainly still sustain his bruising reputation for refusing the lazy pieties of academic correctness, and for constantly skewering taken-for-granted ideas in the literature. Joppke is unparalleled in terms of his ability to spot historical paradoxes, and in wielding an analytical blade in the most complex and knotty of political histories. He finds fresh things to say on immigration in the USA and Australia, and offers a valuable introduction to Spain and Portugal, countries that are rarely studied comparatively; and if the sections on Britain and France are rather too familiar retreads of debates on nationality law, they do nevertheless play a crucial architectural role in the text. Most of the critical attention, however, will likely focus on the chapter on Germany and Israel, a true *tour de force* in which he presents a provocative, unflinching juxtaposition of positive discimination in the immigration histories of these these two tragically intertwined nations. Just broaching this raw and emotive subject matter requires courage, but Joppke does not hesitate to draw a contrasting portrait between the eventual emergence of liberal norms in "ethno-cultural'' Germany after the "normalisation'' of 1989, with the still fierce Israeli reliance on illiberal ethnic nation-building in the territorially unstable Middle East. Joppke will win no friends amongst the pro-Israeli lobby in American academia, but this chapter is the book's most important contribution.

Joppke's treatment of the empirical cases is powerful and plausible throughout, despite the obvious limitation of only focusing on textual and discursive materials, most of it secondary. In terms of the theoretical infrastructure surrounding these case studies, however, the verdict is likely to be far more mixed. Insofar as we can accept his choice of methodology and self-limiting empirical focus – on the political/legal debates, discourses and texts surrounding immigration politics – Joppke certainly can be said to excel in the Habermasian game of sociological analysis of political argument. But nearly two decades after the first works by Rogers Brubaker that lauched this kind of political sociology of immigration, it is no longer clear that this is really where the sociological substance is or should be in works on the subject.

Yes, there is a sociological theory of ethnicity to be found here – the conventional Weberian frame, nicely laid out – and theories of nationhood are gestured towards (Smith for the 19th century, rather more Gellnerian for the present). All are lightly worn and not really developed or tested in any way. And these sociological touches end strictly at this broad-brush, macro-historical level. Dodging the issue in the preface, the text determinedly refuses to tell us anything at all about actual migration phenomena – that is, the human reality that ends up raw material for the political rhetoric here diagnosed. Disappointingly, then, this is yet another book about the *politics* of immigration – how it is talked about, legislated and publicly framed – that has little or nothing to say about migration as such, let alone migrants as real living people. Thus, we get next to no data, numbers or even a qualitative sense about migrants or migration trends for any of the cases presented. A few tables, or some some short vignettes of immigration/emigration in context, would have helped ground the material more. Even more remote is any sense of the sociological processes *from below* that migrants must presumably embody, in order to fulfill (if they indeed do) the political rhetoric *from above* that frames these micro-level processes. Politicians and academics talk about immigration in terms of sweeping macro-generalisations about "citizenship'', "nationhood'', "assimilation'', "integration'', etc., but all of these processes have to be lived out by real flesh and blood individuals, somewhere and somehow. Migrants may get characterised by their host polities as "ethnic'' (in either a positive or negative sense), but there is little or no trace here of how "ethnicity'' or "nationhood'' gets constructed from below in actual social interactions. Ethnicity as a concept, if it has any sociological value – Brubaker has recently argued in this journal that it often does not – is not an objective property of individuals, attributed to them as part of groups. It must be the product of observable interactions between natives and newcomers, or between immigrant generations; it is not just invented by political discourse from above. This is an interdisciplinary text, and it might reasonably be claimed that legal scholars or political scientists would never even think of getting down to this kind of nitty-gritty level. But Joppke is a self-described sociologist, so it is disappointing that he thus consistently reifies – in the way political theorists also do – the kinds of categories of understanding that sociologists are best equipped to re-examine and deconstruct.

Hence, the first chapter, which lays out his theoretical contribution, is really a dialogue with political theory and political philosophers, rather than with comparative historical sociology, as it should be. Joppke clearly sees himself more as contributing to the grand comparative historical sociology of as the nation state – the company of Michael Mann, Rogers Brubaker, Yasemin Soysal, John Torpey – but the authors that draw most sustained attention are the usual political theory suspects: Will Kymlicka, Michael Walzer, Joseph Carens, David Miller. There is, certainly, much of interest in their work, as a form of analytical classification and clarification of normative argument. But Joppke's target is the patently flimsy sociological assumptions that structure their work. His acid dissection of what is wrong with contemporary political theory, here, is quite breathtaking – nobody will ever be able to get away again with the feeble "ethnic/civic'' distinction in talking about immigration and citizenship – but the more difficult historical explanatory questions get eschewed in favour of these normative, analytical debates.

In the end, it is this lack of a micro-sociological touch that leads to one set of conclusions – the argument about dominant liberal trends in the de-ethnicisation of the nation state – where objections can be most obviously raised. At some level of political rhetoric – particularly in the airy, self-image of democracy, rights and freedom that accompany the politics of the Western world – there are certainly de-ethnicising currents at work. But can we be so confident that this is working out just as philosophers and politicians say it is, at the level of lived experience? Joppke argues that, unlike the coercive socialisation pressures of assimilation faced by immigrants encountering 19th and early 20th century nation-building, "the nation-building capacity and ambition of the contemporary state has greatly diminished in the past hundred years, at least in the North Atlantic zone'' (p. 237). Immigrants are no longer forced to be "culturally'' assimilated. They only need "integrate'' voluntarily into the procedural minimum – adhering to democratic values, and following the democratic rules (Joppke suggests that "to integrate'', unlike "to assimilate'', is an intransitive verb, hence implies migrant agency). This minimalist integration into "American'', "Dutch'' or "British''society reveals these proud nation states to be merely "particularisms [which are] only different names for the same liberal-democratic creed'' (p. 239).

Although rooted in a grand theoretical frame that makes sense on some level, this sweeping pronouncement is divorced from sociological sensibility about either the dynamics of inclusion/exclusion, as they play out on the ground, or the successful reproduction of these nations, and their "national" distinctiveness, through contemporary processes of national-societal "integration" (if we must continue to use this concept in its dubious ordinary language usage). Does the third generation West Indian Londoner, really resemble interchangeably the third generation Surinamese in Amsterdam, or the third generation Los Angeleno Korean? Each are products, according to Joppke, of the same "universal, nationally anonymous creed of the liberal state" (p. 239). Maybe yes, on the level of formal rights and citizenship, but surely not, at the level of culture, practice, habitus, or self-identity. What is remarkable in each case, in fact, is how thoroughly nationalised each has become in their behavioural patterns, in the face of all the universalist expectations that globalisation and human rights might bring. Something has happened here: a nation in each case has successfully reproduced itself, through socialising those most vulnerable to coercive societal pressures; those most likely (on "ethnic" grounds) not to be received, accepted, tolerated, or thought to "belong", by the receiving host population.

The degree to which this classic nation-building nationalisation succeeds with immigrants is precisely something which varies across national contexts. America, the Netherlands and Britain – "liberal states" all – are also very successfully cohesive, culturally distinct, national societies, who still score remarkable socialisation successes with their immigrant populations. The banal nationalism that gets reproduced daily by their "philosophies of integration", is such that it is invisible to many who live there. Americans think, "We are the World!"; Britons, that they are a truly "multi-ethnic", "postcolonial" exception in an ethno-cultural Europe; the Dutch that they are uniquely open minded and "tolerant". All are nationalist discourses, that underscore how effectively these societies still function as bounded, distinct societal units. A more fragmented society like Belgium, with a weaker sense of the nation, might actually be a better example of how immigrants can integrate functionally to a more generic form of European modernity – that is essentially only to be found in the de-nationalised spaces of major cities. Immigrants in Brussels thus are, in fact, the only true "Belgians" in Belgium; a truly thin, procedural identity, given that all other citizens think of themselves, first and foremost, as Flemish or Walloon.

Another argument Joppke uses to sustain his peculiar vision of a de-ethnicising modernity, is to contrast the wholesale round up of ethnic Japanese after Pearl Harbour, with the "rather subdued" treatment of Muslims in the US, post 9/11 (p. 230). Here, an archetypal Joppke reversal of polite liberal academic wisdom is pushed to the point of absurdity. It takes a true optimist to brush aside the manifold ways in which Bush's America has imposed illiberal nation-defending (and nation-building) measures in the last couple of years, both internally and externally; just as it does to ignore the ways in which 9/11 revealed just how massively nationalistic the American public in fact is. America, thankfully, is not the world. And, in its ruthless pursuit of anything it conceives as "un-American", in the name of "universal" democratic values, it only reveals how effective it is as a culturally particularistic nation state. Joppke may believe America to be the "heartland of liberalism", but the "Hobbesian" US (a blatant contradiction here) is also still the most effective nation-building assimilation machine on the planet. It would have been wise to leave out these cheap, journalistic references to 9/11; and it may have been too, to rethink some of these more outrageous statements about the demise of the nation state, in the face of triumphant universal norms. Some of this all sounds too much like the kind of pious cant chanted by American presidents at their most high handed. Here, Joppke's sometimes overly clever analysis collapses right into the paradoxes he seeks to expose.

Joppke is led to these unsustainable conclusions by his grand theoretical palette, a theory of modernity that mixes Luhmann and Parsons, even Simmel at one point, with a delicate touch. Only occasionally does he stretch his theoretical legs, but this is in fact a cardinal strength of all his work: which is theoretically rich, yet discreetly so, built on a mound of secondary reading and attention to detail. Joppke argues that the nation state is an anomaly in the accelerated differentiation processes of modernity, that have led to the breakdown of so many pre-modern structures, and the emergence of the modern, human rights protected "individual". This liberal arrow, shot into history by modernity's individualising and globalising processes, has now, he argues, come to circumscribe and almost triumph over all counter currents of particularism and re-ethnicisation attempted in the western world. Liberalism, for Joppke – curiously, for something so identifiable with the histories of specific nation states, in a few specific parts of the developed world – is here portrayed as a kind of universalising force outside nationhood (a "view from nowhere", political philosophers would call it); a functional force of universal reason for planetary enlightenment.

Ironically, given Joppke's trenchant critique in the past of Yasemin Soysal, this John Meyer-like macro-sociology of modernity (Meyer barely appears in the book), strikes this reader as strangely counter to much of his previous work. It leads to his peculiarly innocent view of the universalist, liberal US; and the overly happy view of global migrants pushing the functional de-ethnicisation of nations. One might mischievously turn one of Joppke's old anti-Soysal jibes against him here. It is easy to imagine migrants as globally individualised free agents, who feel little socialisation pressure from the liberal (nation) states that receive them, when one is a frequent flying, transnational professional oneself. But this kind of privilege is – as the heavily coercive experience of integration and assimilation that most sociology of immigration suggests – dramatically stratified by class and social power. Any sociologically sensitive account is likely to find that those at the bottom end of the pile are *much* more constrained by the coercive

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

attention of their host nations, than those more powerful citizens who wear their human rights-based identities so lightly. Power is missing from Joppke's optimistic world view of liberalism triumphant; the power exerted at the micro-level on individuals by the very same "liberal'' macro-structures of law, state and government, that philosophers so love to idealise.

In the end, for his view to stand, the crucial test would be to compare with the cases here, socialisation pressures on immigrants in those states *outside* of the self-selecting liberal club that Joppke, by a circular logic, defines as the universe of "liberal states''. Is the world really divided into modern "liberal states''... and the rest? Let us see whether those in the club are really any less coercive or controlling of individuals – or whether we have just been more effectively socialised ("forced'', in Rousseau's terms) to feel "free''. Lacking this test is a major design flaw. Israel is in a sense a limit case, as he argues, that might be of use; but this is an awkwardly unique case. It would have been so much more interesting to look at other cases of non-Western modernity: Japan, Singapore, India, China, even Egypt or Iraq, to see whether Westernising (or is it just all-American?) forces of modernity and individualisation are really so bound up together as he claims. As well as confirming that these are indeed *not* all good members of the club, we might also begin to see how the modernity of modern western states can take on a distinctly illiberal aspect too.

One feels ultimately that the "liberal state'' in Joppke's work is an unreal, philosopher's oxymoron, not a credible sociological/historical concept; an idealised end point of modern development, purged of much darker, national-societal contexts – the historical murk, out of which any actual examples of "progressive'' liberalism or democracy have emerged. Michael Mann, in a recent book, calls this oversight the "dark side of democracy'': the uncomfortably twinned closeness of high minded liberal rights and freedoms, with the nationalist low road to ethnic cleansing. It might be thought surprising that Joppke should fall into this kind of theoretical position – given his trenchant reputation for skewering the most comfortable liberal arguments and assumptions in the past. But the theoretical dilemma he falls into, is in fact an archetypal problem in most of the over-idealised liberal political philosophy that has so dominated political theory in recent years. It is also, it must be said, the characteristic of all thinking coming out of the Habermasian school; an intellectual origin Christian Joppke often successfully masks in his work, but never quite transcends.

Adrian Favell

DURKHEIM ET L'ÉPISTÉMOLOGIE DES PRATIQUES SOCIALES *

S'INTÉRESSANT à un ouvrage phare de la sociologie du début du XX[e] siècle, *Les Formes élémentaires de la vie religieuse*, Anne Warfield Rawls nous propose, dans son ouvrage *Epistemology and Practice (Durkheim's The Elementary Forms of Religious Life)*, de relire l'œuvre de Durkheim sous un nouveau jour. Celui-ci n'est plus présenté comme le chef de file d'une pensée à la fois idéaliste et moralisante, mais comme le premier sociologue à avoir fourni une véritable épistémologie à la discipline. Affirmation pour le moins déroutante, Durkheim étant bien souvent considéré certes comme l'un des « pères fondateurs » de la sociologie, mais surtout comme un penseur, l'auteur le dit, « naïf », pratiquant une sociologie dont on ne retient plus que les apports méthodologiques, au travers de règles d'étude contraignantes au sens tellement dilué qu'il frôlerait le simplisme...

A. W. Rawls affirme que l'ensemble des travaux durkheimiens se structure autour d'une seule et même logique, visant à appréhender les mécanismes à l'œuvre au sein de toutes les relations sociales. À travers une analyse détaillée des religions primitives des Aborigènes d'Australie, *Les Formes élémentaires de la vie religieuse* constitueraient le lieu privilégié de l'élaboration de cette structure logique. Celle-ci se retrouverait donc plus ou moins implicitement dans toute la pensée durkheimienne, et aurait pour intérêt majeur d'établir, selon l'auteur, les bases de l'unique alternative valide proposée à ce jour au problème philosophique de l'origine des catégories de l'entendement, « dilemme » entre apriorisme et empirisme symbolisé par les figures respectives de Kant et Hume – dont sont minutieusement retracées les approches.

Ce sont les implications sociologiques de ce dilemme qui ont intéressé Durkheim : les caractéristiques de l'homme qui le distinguent de l'animal, ses capacités intellectuelles en tête – « *the categories of understanding* » – sont-elles innées comme le pensent les kantiens, ou construites à partir de ses perceptions sensorielles, comme l'affirment les tenants d'Hume ? Durkheim propose une troisième voie : ces deux approches pèchent selon lui par l'individualisme qui les sous-tend – « *both have in common the assumption that an epistemological argument must begin with individual perception and explain how individuals come to share knowledge in common* » (p. 62). Il affirme au contraire que pour répondre à la question de l'origine de l'homme doué de raison, il faut s'intéresser avant toutes choses non pas aux croyances, idées et représentations des individus, ni à leurs impressions sensorielles, mais aux pratiques partagées par tous les membres d'un groupe donné. En effet, elles seules peuvent révéler l'origine des catégories de l'entendement, de la raison humaine, qui ne se réalisent pleinement que dans le collectif, le social. Les pratiques sociales apparaissent pour Durkheim comme le point de

* Au sujet d'Anne Warfield RAWLS, *Epistemology and Practice, Durkheim's* The Elementary Forms of Religious Life (Cambridge University Press, 2004).

Sandrine ALLIGIER-OTANIAN, ATER Université de Provence, LAMES (Laboratoire Méditerranéen de Sociologie), Aix en Provence [sandrine.otanian@hotmail.fr].
Arch. europ. sociol., XLVI, 3 (2005), 552-558—0003-9756/05/0000-815$07.50 per art + $0.10 per page©2005 *A.E.S.*

départ de la construction de la société, et comme le facteur du maintien de sa cohésion.

C'est selon A. W. Rawls à cette démonstration que s'attachent *Les Formes élémentaires* : on y trouverait la justification de la priorité accordée par Durkheim, dans l'ensemble de son œuvre, à l'étude des pratiques sur celles des croyances. Cette thèse nous paraît d'autant plus pertinente qu'elle sait éviter la caricature : s'il refuse de faire découler les catégories de la connaissance, de la seule généralisation de l'expérience individuelle (empirisme), ou exclusivement des caractéristiques inhérentes à la nature humaine (apriorisme), il n'est cependant pas question pour Durkheim de « tuer » Kant, ni Hume ! Il concède ainsi au premier que ces catégories sont nécessaires et non contingentes, présentes *a priori* en chaque homme. Mais comment en acquérir la pleine maîtrise, comment les faire s'épanouir en toute conscience ? Voilà toute la question de Durkheim. De même, il reconnaît avec Hume que ces catégories sont construites par chacun *via* son expérience. Mais comment celle-ci fait-elle sens ? Selon Durkheim, en étant commune avec celles des autres membres du groupe, à travers le partage d'un certain nombre de pratiques : ainsi, c'est seulement dans et par le social que l'individu « naturel » peut éveiller en lui ses potentialités d'homme doué de raison. Voilà selon nous la force de la troisième voie durkheimienne mise en évidence par A. W. Rawls, levant les limites de l'empirisme et de l'apriorisme à travers l'importance cruciale accordée aux pratiques sociales dans la construction de l'homme.

L'auteur parle alors de « démonstration épistémologique » – « *a careful empirical elaboration of Durkheim's epistemological claims* » (pp. 7-8). On peut regretter qu'elle ne prenne pas la peine de définir plus précisément ce qu'elle entend par « épistémologie de la pratique » ; il est en effet difficile de cerner la dimension proprement épistémologique des explications durkheimiennes sur l'importance des pratiques sociales. Si la visée de l'auteur des *Formes élémentaires* était proprement épistémologique (1), ne chercherait-il pas avant tout à discuter de la portée scientifique de son analyse des pratiques ? Or son propos semble moins focalisé sur des questions de scientificité qu'orienté vers un unique but : mettre en évidence le fondement social de la pleine expression de la raison humaine – ce que montre d'ailleurs fort bien A. W. Rawls. À ce titre, les explications durkheimiennes nous paraissent plutôt relever de l'anthropologie philosophique que de l'épistémologie. Néanmoins, le titre de l'ouvrage semble pouvoir se justifier autrement : la réflexion épistémologique présentée est en effet moins celle de Durkheim que celle de A. W. Rawls sur les choix d'étude durkheimiens. *Les Formes élémentaires* ont ainsi pour l'auteur une forte dimension « épistémologique » dans la mesure où elle y voit une grille de lecture de toute la science de Durkheim. Dans cette même optique peuvent être qualifiées d' « épistémo-

(1) On adoptera ici la définition suivante de l'épistémologie : « discipline qui prend la connaissance scientifique pour objet » (Larousse, Dictionnaire encyclopédique, 1997).

logiques » les affirmations qui ont pour fonction de justifier la validité des choix effectués par Durkheim dans la mise en œuvre de sa démarche scientifique : choix de ses objets d'étude, de ses méthodes, tous sous-tendus par la priorité accordée aux pratiques sur les croyances, qui rend valide toute étude voulant appréhender empiriquement la question de la nature sociale de l'humain. Néanmoins, l'essentiel nous semble ailleurs que dans la (re)découverte éventuelle d'une épistémologie propre à la pensée de Durkheim : celui-ci aurait voulu avant tout démontrer l'origine sociale de l'épanouissement des catégories de l'entendement humain. Mettre en exergue cette finalité transversale à toute l'œuvre durkheimienne, c'est là selon nous le véritable point fort de la démonstration de l'auteur – contrairement à ce que pourrait laisser penser le titre.

Plus précisément, ces catégories sont élaborées à partir des pratiques rituelles qui, au sein des religions tant primitives que modernes, induisent le sens du sacré chez chaque participant, désormais lié aux autres par la force morale issue du partage des émotions religieuses. Les pratiques rituelles sont les premières structurantes dans la constitution d'une société. Afin d'avoir toujours à l'esprit l'extraordinaire force sociale qu'elles ont fait naître, sont créés des symboles (les totems dans les religions primitives), qui permettent d'évoquer – au sens étymologique du terme : *e-vocare*, faire venir – ces moments sacrés, de se les remémorer toujours. Autour de ces pratiques et des symboles qui leur sont associés émergeront ensuite des représentations collectives, ensembles de mythes et de croyances qui dans l'optique de A. W. Rawls sont envisagés comme des « compte-rendus » (« *accounts* ») des moments sacrés : le raisonnement prend ici un tour inattendu, quasi ethnométhodologique, pour le moins surprenant dans une analyse de la pensée durkheimienne, mais qui s'avère fort pertinent au vu de l'importance cruciale accordée à ces « compte-rendus »... C'est en effet parce que ces mythes et croyances ont le même sens pour l'ensemble du groupe que vont se mettre en place des schémas mentaux fixes, communs à tous. Ils permettront d'élaborer des raisonnements expliquant en retour ces mythes et croyances : naîtra ainsi l'homme social, doué de la capacité de raison.

Les pratiques sont premières, les croyances secondes. Une telle affirmation constituait un paradoxe à l'époque de Durkheim ; aujourd'hui encore, nombre de lecteurs auraient tendance à voir la religion avant tout comme un système de croyances (essentiellement en une déité), déterminant un ensemble de pratiques rituelles. Durkheim affirmait l'inverse et condamnait les penseurs de son temps qui dénigraient l'importance des pratiques. Une telle position lui était nécessaire dans la mesure où il souhaitait apporter la preuve empirique des causes sociales du développement de la raison humaine. Comme le reconnaît A. W. Rawls, cette ambition paraît emphatique au tout début des *Formes élémentaires*. Durkheim deviendra plus humble dans le cours de sa réflexion. C'est également un des chevaux de bataille de l'auteur que de restituer leur juste place aux affirmations de l'introduction et de la conclusion, trop souvent considérées comme prégnantes par rapport au reste du texte. Ainsi, si Durkheim parle longuement de la religion en introduction, il ne faut pas y voir pour autant le principal objet du livre, qui se focalise en réalité sur les pratiques sociales pouvant engendrer la force morale nécessaire à la pleine exploitation par l'homme, de ses capacités de raisonnement ; il insistera tout particulièrement sur la catégorie de la causalité (la capacité à établir des liens de cause à effet entre les phénomènes), qui se révèlera elle aussi dans le social. A. W. Rawls prend ainsi le temps de nous faire relire l'œuvre chapitre par chapitre, afin d'en faire émerger cet essentiel qui selon elle a le plus souvent échappé aux exégètes : l'idée que *Les Formes élémentaires* s'intéressent moins à la religion en soi que comme fondement des pratiques permettant de créer, entre des individus isolés, la cohésion nécessaire à leur réalisation en tant qu'êtres sociaux, doués de raison.

Durkheim envisage plus précisément le cas de la religiosité primitive, vecteur privilégié, selon lui, de la révélation de ces catégories de l'entendement : en effet, religion et pratique rituelle pure y sont pour ainsi dire synonymes. À ce titre, *Les Formes élémentaires* constituent un véritable plaidoyer pour la validité des religions « archaïques » comme objets d'étude du religieux à part entière. Il est également bon de trouver dans l'argumentation de l'auteur un rappel des critiques sévères de Durkheim à l'égard de l'ethnocentrisme évolutionniste de nombre de penseurs de son temps. Là encore, A. W. Rawls insiste pour que le lecteur retienne non pas que Durkheim fut un bon analyste des religions, mais que cherchant l'origine de la raison humaine, il a trouvé ses racines dans le sacré suscité par les pratiques rituelles collectives autour de symboles. De même, elle restitue à la notion d'*homo duplex* son sens originel, fondé non pas sur une opposition radicale, mais plutôt sur un lien dialectique entre les dimensions individuelle et sociale de l'homme, à la fois séparées et absolument nécessaires l'une à l'autre, se réconciliant au moment où est ressentie cette émotion propre à l'accession au sacré. Ce sont les pratiques rituelles (notamment sacrificielles) qui permettent aux Aborigènes d'Australie de dépasser leur individualité pour atteindre cet état de société décrit par Rousseau, et repris dans ses grandes lignes par Durkheim, au sein duquel l'homme accède enfin à la pleine maîtrise de ses catégories de l'entendement - « *because the categories* [of understanding] *are empirically valid and socially caused, there is a separation, but no divorce, between natural reality and social reality* » (p. 107). Ainsi, rappelle A. W. Rawls, ce n'est pas l'*homo duplex* qui a donné naissance à la distinction sacré/profane, mais l'inverse. Cette dichotomie est pour Durkheim la première de toutes. Elle seule permet, au travers de l'expérience rituelle partagée, de faire émerger chez tous les participants une force morale extra-ordinaire qui transforme chacun d'eux en « humain » : l'individu sent alors une part de lui capable de toucher le sacré, tout en restant ancré dans le profane de par sa nature biologique.

Sur ce point seulement la portée épistémologique de la position durkheimienne nous apparaît plus clairement : *Les Formes élémentaires* proposeraient une « épistémologie » au sens où l'ouvrage chercherait à fournir une explication scientifique au dualisme humain au travers d'une analyse empirique de la distinction, première et universelle, entre le profane et le sacré, afin de mettre en évidence ses implications sociales ou, mieux encore, afin de démontrer qu'elle seule est révélatrice de la dimension sociale de l'homme.

L'intérêt de l'analyse de A. W. Rawls est double : elle propose tout d'abord une lecture extrêmement détaillée des *Formes élémentaires de la vie religieuse* sans pour autant jamais tomber dans la paraphrase, démontant les moindres rouages du raisonnement durkheimien, reprenant chacun des concepts phares de sa sociologie (« effervescence collective », « distinction sacré-profane », « personnalité », « émotion », « rite », « symbole totémique », etc.). Ce qui l'amène, et c'est là sa seconde réussite, à proposer une exégèse d'un nouveau genre, en raisonnant en amont de tout ce qui a pu être dit et écrit sur *Les Formes élémentaires* jusqu'à présent, puisqu'il s'agit pour elle d'y mettre au jour les fondations non pas d'une théorie, ni même d'une méthodologie, mais de la structure (épistémo)logique qui les sous-tend. Il est évident que le but de l'auteur n'est pas de proposer une lecture « décalée » pour simplement faire dans l'originalité : A. W. Rawls ne semble pas concevoir une bonne compréhension de la pensée durkheimienne sans une attention première à sa conception générale de l'homme, être à la fois animal et social. Pour elle, Durkheim fut un sociologue largement incompris ; paradoxe, on a fait de lui le « père fondateur » d'une sociologie qu'il aurait en grande partie reniée, notamment dans ses classifications trop arbitraires. On comprend ainsi que parler de « holisme durkheimien » n'a pas grand sens si l'on entend seulement par holisme l'idée d'une société monstrueuse imposant sa ligne directrice à des hommes objets, pantins de la structure dans laquelle ils évoluent. Selon Durkheim, au contraire l'individualité n'est pas annihilée par la société mais bien sublimée dans l'accession de tous, c'est-à-dire de chacun, au sacré ! A. W. Rawls va même jusqu'à avancer que c'est de la mauvaise interprétation de l'ensemble de l'œuvre durkheimienne que découlent les distinctions universitaires faciles, quasi simplistes, entre « individualisme » et « holisme » méthodologiques, entre analyses « micro » et « macro » sociologiques. À mille lieues de ces préoccupations peu fécondes, le but – la vocation – de la sociologie doit être d'abord et avant tout l'étude empirique des pratiques des hommes, qui font d'eux des êtres sociaux. Voilà la seule sociologie qui pourrait se réclamer fidèlement de Durkheim.

Notons que l'emploi du terme « vocation » n'est pas sans rappeler les propos wéberiens sur la vocation de la science en général – et des sciences humaines en particulier – dans *Le Savant et le Politique*. Ne peut-on pas rapprocher le « devoir de clarté » du sociologue wéberien, cherchant à décrire la réalité de la façon la plus minutieuse et la plus objective possible, de la vocation sociologique de Durkheim, tellement soucieux lui aussi d'éclairer les hommes sur la nature de leurs relations et l'état de leur société ? Certes, les deux penseurs sont éloignés l'un de l'autre, tant sur les plans théorique que méthodologique. On pourra cependant regretter que A. W. Rawls, qui dénonce elle-même les distinctions universitaires caricaturales, les oppose si violemment : Durkheim s'intéresserait avant tout aux pratiques, Weber n'en aurait que pour les concepts ; elle en tient pour preuve son utilisation récurrente de l'idéal-type. Or l'œuvre wébérienne n'est-elle pas tout autant un plaidoyer pour l'étude des pratiques sociales, les types idéaux n'étant que des abstractions utilisées à des fins méthodologiques, détours conceptuels destinés à mieux appréhender, au final, encore et toujours la réalité la plus concrète qui soit : les pratiques (2) ? Il nous semble ainsi manquer à sa critique comparative une argumentation approfondie de la conception wébérienne de la religion, qu'elle affirme pourtant être catégoriquement opposée à celle de Durkheim – un système de croyances *versus* un système de pratiques. Une telle analyse aurait sans doute été plus constructive que l'évocation des traditionnelles pensées « ennemies » de Durkheim que sont les théories animistes ou naturalistes dont les failles, connues et reconnues, n'ont plus besoin d'être rappelées.

Reste que, fort heureusement, l'exégèse de A. W. Rawls ne tourne pas au panégyrique : l'auteur ne cesse de pointer du doigt les points obscurs de la démonstration durkheimienne ; elle lui reproche notamment de ne se préoccuper véritablement d'épistémologie que dans sa conclusion, contrairement à ce que laissaient entendre les promesses de l'introduction. De plus, si Durkheim met au centre de sa pensée les pratiques, plus particulièrement les rites et non les croyances, ce sont pourtant ces dernières qui sont envisagées les premières, objet du livre II des *Formes élémentaires*, alors qu'il faut attendre le livre III pour que soient abordés les rites, ce qui prête à confusion quant à leur importance respective. D'autant que les croyances bénéficient d'une analyse poussée, alors que selon A. W. Rawls son argumentation sur les rites se noie sous un flot de descriptions fastidieuses. Durkheim serait ainsi en partie responsable du fait que ses exégètes n'aient pas ou peu vu la portée épistémologique de son étude, en raison du caractère souvent confus de la présentation de ses arguments – mais non de ses arguments eux-mêmes !

A. W. Rawls donne un livre à la fois très conventionnel dans sa forme, puisqu'il suit pas à pas le cheminement des *Formes élémentaires de la vie religieuse*, mais également novateur dans son approche d'une œuvre sur laquelle on aurait pu prétentieusement penser que tout avait été dit. On retiendra tout particulièrement le chapitre II « Durkheim's dualism : an anti-Kantian, anti-Rationalist position », qui réhabilite magistralement l'*homo duplex* durkheimien en cessant d'en faire un schizophrène pour affirmer la possible harmonie entre ses deux pôles (pré-rationnel animal et rationnel social), ainsi que la conclusion, qui fait toucher du doigt l'actualité

(2) On renverra le lecteur à l'*Introduction* de *L'éthique économique des religions mondiales*, in *Sociologie des Religions*, recueil de textes de Weber réunis et traduits par J.-P. Grossein (nrf éd. Gallimard, 1996).

de la réflexion durkheimienne : la survie d'un groupe d'individus en tant que société dépend de la capacité de ses membres à transcender leur individualité ; la clé de cette survie réside dans des pratiques sociales communes susceptibles de dégager de l'émotion, d'inciter à la communication, c'est-à-dire à la création du lien social. En ces temps de relativisme ambiant, le partage de pratiques semble être la condition *sine qua non* au partage de représentations, de croyances, de valeurs, qui structurent notre entendement. Ce dernier est donc nécessairement socialement construit. La preuve en est que dérives de la raison et délitement du lien social vont généralement de pair. Les choix durkeimiens se trouvent aujourd'hui plus que justifiés, et la démonstration de A. W. Rawls montre là toute sa pertinence.

Sandrine Alligier-Otanian

NEOLIBERAL HEGEMONY *

MOST WRITERS closely associate the rise and the (alleged) subsequent decline of neoliberalism with the (mis)fortunes of the Thatcher and Reagan administrations in the UK and the US, respectively. If you were ever suspicious about such a narrow political interpretation of neoliberalism and wondered what exactly "neoliberalism'' is, where it came from, and how it more or less succeeded in replacing social liberalism as the hegemonic paradigm of contemporary capitalism since the late 1970s, you can now find the answers in Bernhard Walpen's fine study of the Mont Pèlerin Society. This parsimoniously organized network of intellectuals was originally founded in 1947 in the small village in Switzerland whose name it carries by Friedrich August von Hayek, Albert Hunold, Milton Friedman, Karl Popper, William Rappard, Ludwig von Mises, Lionel Robbins, Wilhelm Röpke and others. Originally a group of 39 scholars, mainly from Europe and the US, the neoliberal "international academy'' (in Hayek's imagination) now boasts a membership past and present of 1,000 (current members number 500). Walpen's archival research for the book included the compilation of a database including all the members, and the book documents Walpen's study of the lives and works of about half of them in astonishing detail.

The book first traces the pre-history of neoliberalism beginning in the 1920s and dates its premature birth to the *Colloque Walter Lippman* held in Paris in 1938. At this conference convened to discuss Lippman's book "The Good Society'', some of the later Mont Pèlerin Society members met and agreed to develop a *positive* liberal program. The need for such a project was urgently felt, both to overcome the mortal crisis of *laissez-faire* liberalism, and to combat the agonies resulting from what the group termed "collectivism'' (including Socialism, Nazism, and social liberalism). The deliberations turned to the question of what to call this new liberal philosophy, and thus the term neoliberalism was coined. Early efforts to promote the neoliberal vision, via an international think tank with branches in several countries, were disrupted by the Second World War. Walpen documents the revivial of the Mont Pèlerin Society (MPS) following the war, and we learn how attempts to integrate the Mont Pèlerin group into the parallel formation of the Liberal (Party) International were rejected in order to avoid intellectual and political compromises. Hayek's vision for the future of neoliberal hegemony did not include such party political affiliations and their intellectual entanglements, as implied by the famous dedication of his 1944 book Road to Serfdom implied: to "socialists in all parties''.

* About Bernhard WALPEN, *Die offenen Feinde und ihre Gesellschaft. Eine hegemonietheoretische Studie zur Mont Pèlerin Society* (Hamburg, VSA Verlag, Schriften zur Geschichte und Kritik der politischen Ökonomie 1, 2004).

Dieter PLEHWE, Social Science Center Berlin [plehwe@wz-berlin.de].
Arch. europ. sociol., XLVI, 3 (2005), 559-562—0003-9756/05/0000-816$07.50 per art + $0.10 per page©2005 *A.E.S.*

Walpen traces the history of this unique organization in four chapters dating from the 1950s to the present. The Society was almost doomed to collapse during the 1950s when some key founding members including Albert Hunold, a Swiss industrialist, and German-Swiss ordoliberal economist Wilhelm Röpke attempted to politicize the activities of the group. This faction held that the imminent danger posed by socialism called for a more vocal and public strategy on the part of the MPS. Hayek, Fritz Machlup, Bruno Leoni and others succeeded in preventing this shift in tactics and forcing the contenders out. Their view was that the group should concentrate on its ultimate mission: winning the long term intellectual battle necessary to secure of neoliberal hegemony. They argued that MPS members should avoid the potential divisiveness of politics in favour of strengthening the intellectual competencies and capacities of its members, who would in turn be better able to influence discourses and promote the neoliberal vision in decentralized ways.

The design of the Mont Pèlerin Society was simple and straightforward: apart from economists, the largest group of members, Hayek and others invited academics from many other disciplines including political science, sociology, history, philosophy, and theology. In addition, they carefully selected practical men working in corporations, the media, and the state (including Ludwig Erhard and Luigi Einaudi as prominent political leaders) to match the perceived intellectual strength of the Left. Based on six founding principles (e.g. asking for the redefinition of the role of the state, not for its destruction) which can be regarded as the smallest common denominator of MPS neoliberals, the new organization began holding yearly (later bi-yearly) global conferences. When the Society grew more rapidly, regional conferences were organized to shorten the gaps between the international meetings. Both through the conferences and the countless research and publication projects of members, the Mont Pèlerin Society thereby enabled its members to develop an interdisciplinary discourse covering about every important philosophical, disciplinary, and political question of post-war capitalism and socialism.

The neoliberal knowledge and expertise they gained through membership in this network prepared many members to actively and effectively intervene in the public sphere. While the Society successfully shielded its internal conversations (the public only learnt about meetings from a select group of journalists who are members including Henry Hazlitt, John Davenport, Gerhard Schwarz etc.), its members started a second highly innovative initiative: the founding of *partisan* think tanks. Walpen's research shows that more than 100 think tanks exist around the globe which have been founded by, or with the help of, MPS members. While the Foundation of Economic Education and other US think tanks were already in existence (Henry Hazlitt and Ludwig von Mises are among MPS members with a base at the FEE), Hayek convinced Anthony Fisher to fund the London based Institute of Economic Affairs as a "model'' for the sort of strategic

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

neoliberal knowledge organization he envisioned. Target groups such as students and journalists were singled out to be bombarded with swiftly written, typically short publications, many of which were based on the scholarly work of MPS members. Early on marketing was acknowledged to be as important as content, and the techniques were further refined. Today, many neoliberal think tanks can easily produce neoliberal material ready-made for publication in newspapers, radio and television. The Heritage Foundation of course refined this approach for politicians, developing very short briefing papers on a variety of issues that are distributed to Congress members (often "just in time", i.e. while they are on their way to vote on the issue).

Anthony Fisher subsequently helped to set up many more think tanks, e.g. the Manhattan Institute in New York, and Hernando de Soto's Institute for Liberty and Democracy in Lima, Peru. Since the 1980s, the Atlas Economic Research Foundation based in the US serves to coordinate neoliberal think tank activities around the globe, much like the Mont Pèlerin Society serves to further develop and coordinate the global network of neoliberal intellectuals.

For lack of a better term, one can understand the group as a transnational meta-discourse community or *Weltanschauungsgemeinschaft*, a comprehensive "thinking collective" developing a specific "thinking style" (to use categories developed by sociologist of knowledge Ludwig Fleck). Its recognition as a "private (knowledge) authority" both in domestic and international relations has so far escaped scholarly scrutiny, not least due to the hidden links between MPS scholars, corporate, business and media leaders, and between MPS members and partisan think tanks. (On this note, and as a gesture towards the transparency that is still lacking in the world of organized neoliberalism, I should acknowledge my close collaborative relationship with Walpen; we have co-authored a number of articles and edited the forthcoming Routledge book "Neoliberal Hegemony: A Global Critique".)

Due to Bernhard Walpen's long-term effort (a good decade went into the research and writing), scholars interested in neoliberalism and non-state actors can find a solid fundament in this book for further studies in many issue areas and discourses, e.g. the rise of monetarism (Milton Friedman, Alan Walters and several corporate and central bankers are or have been members of MPS), or the neoliberal counter-revolution in development economics (involving MPS members Peter Bauer, Herbert Frankel, Depaak Lal and many other key actors). Bernhard Walpen can further aid such urgently needed research tasks by better specifying the links between MPS members and think tanks in case of a second printing. Apart from this omission, the reader has only herself to blame if she does not find all of the wealth of information contained in 300 pages and 80 pages of endnotes, many of which cover important theoretical and empirical ground that is sadly missing in most critiques of neoliberalism. Walpen's book is by far the best example of how a neo-gramscian perspective can enhance our capacity to understand the promotion of neoliberal globalization and the globalization of neoliberalism. A remaining critique: the book is written in German. Thus we are left with MPS member Max Hartwell's "insider" history published in 1995 as the work on MPS that is accessible to the wider English language audience, and Richard Cockett's (1995) "Thinking the Unthinkable", which covers some ground mainly with regard to the UK's Thatcher revolution. If a neoliberal scholar of Walpen's capacity had written such a major work, its translation and publication in major countries of the world would be expediently secured, given the determination, spread and scope of neoliberal partisan science networks – we can only hope for the quick discovery of Walpen's book by a major international academic press. As long as Walpen's book is not translated I have only one piece of advice: learn German.

DIETER PLEHWE

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

EXIL ET DÉSASSIMILATION*

PIERRE BIRNBAUM s'est fait connaître dès les années 1970 par plusieurs livres de sociologie politique qui conjuguaient une bonne connaissance des grands politologues américains et des résultats d'enquêtes sur le pouvoir et les élites. En 1992, la publication des *Fous de la république ; Les Juifs d'État en France* (1) marquait un changement de centre d'intérêt qui s'est confirmé avec les études suivantes consacrées à la socio-histoire des juifs français.

Géographie de l'espoir est beaucoup plus ambitieux. « Une longue histoire se termine probablement, celle de la rencontre entre les Juifs et les Lumières conçue sur le seul mode universaliste et ancrée dans une vision exigeante de l'assimilation régénératrice ». Ces lignes prises dans la longue introduction, intitulée « Vers une contre-histoire », font comprendre que l'auteur est à la recherche des historicités juives, essentiellement d'origine est-européenne, occultées ou refoulées par l'émancipation et le parti-pris fréquent de nombre d'intellectuels juifs en Occident, pour l'assimilation. Plus simplement dit, Pierre Birnbaum entend s'inscrire dans le courant – assez fort aux États-Unis – de ceux qui ont mauvaise conscience devant l'assimilation, qui n'y croient plus, ne la valorisent plus ou, encore, la refusent. Cependant, et c'est la grande ambiguïté du livre, s'il parle peu en son nom, préférant laisser s'exprimer les voix qu'il a choisi de présenter, le commentaire n'est pas neutre. Dès l'introduction, tous les historiens, sociologues, anthropologues, linguistes juifs qui n'ont pas étudié les juifs sont comme cloués au pilori et l'on trouve aussi bien le Français Marc Bloch, résistant de la première heure, que l'Allemand nationaliste Ernst Kantorowicz et une longue liste aux États-Unis. Au nom de quoi pareille condamnation peut-elle être formulée ? La réponse n'est jamais clairement donnée.

Ce livre, fort érudit avec cent pages de notes comportant des éclaircissements de prix, présente d'abondantes citations et nombre de mises au point ; il dispensera d'en lire beaucoup d'autres. Dire qu'il soit pleinement convaincant est plus délicat, même si, sur un tel sujet, quiconque prend la plume doit se méfier de sa partialité engagée. Le parcours proposé est élégamment distribué en huit chapitres, tous construits autour d'une grande figure, dont trois au moins, et c'est une des étrangetés du livre, ont clairement exprimé leur refus d'entrer dans la question et les doutes chers à Birnbaum. Le premier est Marx confronté d'abord, et pour l'anecdote, à Heinrich Graetz, auteur de la première et monumentale *Histoire du peuple juif* (en douze volumes), un des maîtres de la Wissenschaft des Judentums qui, pénétrés de l'esprit des Lumières, entendaient contribuer aux sciences phi-

* À propos de Pierre BIRNBAUM, *Géographie de l'espoir. L'exil, les Lumières, la désassimilation* (Paris, Gallimard, 2004).

(1) Paru chez Fayard en 1992 pour l'édition originale, puis réédité en poche Seuil en 1998.

Jacques LAUTMAN, Professeur émérite, Maison méditerranéenne des sciences de l'homme, Aix-en-Provence [lautman@mmsh.univ-aix.fr].
Arch. europ. sociol., XLVI, 3 (2005), 563-567—0003-9756/05/0000-817$07.50 per art + $0.10 per page©2005 *A.E.S.*

lologiques et historiques allemandes en produisant sur les œuvres de culture juive un savoir indépendant de tout engagement, de toute connotation religieuse. Au demeurant on sait que, pour Marx, la question juive ne devrait plus se poser dans l'avenir et que son antisémitisme, qui n'est pas exterminateur, a en fait deux dimensions. La première est l'agacement sardonique à l'encontre de rites et de pratiques que tous les juifs émancipés jugeaient dès 1830 archaïques, dépouillés de sens ou ridiculement particularistes. La deuxième est liée à la place des banquiers juifs dans le premier grand capitalisme européen.

Si Marx est traité avec sympathie, il n'en va pas de même des deux Français Durkheim (chapitre II) et Aron (chapitre IV). L'auteur n'a de cesse de s'étonner, comme il l'a déjà fait dans l'introduction, qu'ils n'aient pas pris pour champ d'observation le monde juif. Pourtant, honnête, il rappelle bien qu'aucun des deux ne fut un juif honteux et il apporte à propos de Durkheim quelques éléments qui ne sont pas très connus, ainsi que cet incroyant ferme faisait le voyage d'Épinal chaque automne pour vivre avec sa famille les jours de Yom Kippour et de Roch Hachana ou, encore, son action pendant la première guerre mondiale en faveur des juifs russes récemment arrivés en France et maltraités dans les rangs de la Légion étrangère où ils se sont engagés répondant à l'appel : « Frères, c'est le moment de payer notre tribut de reconnaissance au pays où nous avons trouvé l'affranchissement moral et le bien-être matériel ». Théoricien de l'universalisme républicain, il n'en garde pas moins le souci d'une mémoire juive et d'une cohésion dont les liens de famille entretenus sont les vecteurs. Raymond Aron a toujours été très explicite, se présentant comme Français juif dé-judaïsé, appartenant à une famille au patriotisme intransigeant. Quelques points dans le chapitre sont à relever, à commencer par l'étonnante lettre privée de Claude Lévi-Strauss très hostile au livre *De Gaule, Israël et les Juifs* et écrivant : « cela sentait le complot, voire la trahison. Devant les positions prises par certains juifs français en faveur d'Israël, j'ai eu honte ». Aron est plus complexe ; tout en mettant sa citoyenneté française au-dessus de tout, il reconnaît une « dilection particulière » à l'endroit d'Israël qui ne sera en aucun cas une double allégeance même si, dans une conférence de 1974 donnée à la *New School for social research*, il plaide pour l'avènement en France en particulier d'une conception de la citoyenneté plus ouverte, autorisant le développement d'un certain multiculturalisme. Sans le nazisme et la Shoah, il est probable qu'il aurait moins éprouvé la nécessité d'affirmer cette solidarité juive.

Georg Simmel a eu à souffrir toute sa vie d'un antisémitisme sûr de soi de la bonne société allemande, sur lequel le nazisme fera fonds et qui était largement endossé dans le monde universitaire. Brillant et salué comme tel de son vivant, il n'est devenu un grand auteur que bien après sa mort, aux États-Unis, grâce à Louis Wirth, au milieu juif de l'université de Chicago et à Lewis Coser. Il est le théoricien de la figure de l'étranger, de sa position privilégiée pour les rôles d'intermédiaire, de passeur et il en a une vue plutôt optimiste, même si, quand il donne pour exemple le cas des juifs européens, il ne sous-estime pas les souffrances liées à la marginalité indépassable. Fortement opposé au sionisme, il est convaincu que l'avenir des juifs est en Europe, dans une dissolution mêlée qui assurera une judaïsation de la société, parfaitement compatible avec un antisémitisme persistant. La suite du chapitre est davantage un passage en revue des interactionnistes juifs américains influencés par Simmel pour constater, une fois de plus et avec tristesse, qu'ils ne se sont guère intéressés à la diversité des mondes juifs.

Hannah Arendt et Sir Isaïah Berlin (chapitres V et VI) donnent déjà plus de satisfactions à Birnbaum. Par-delà de profondes différences, la première, sioniste sous le nazisme mais plus que réservée vis-à-vis d'Israël, et le second, actif et efficace soutien du sionisme avant et après la création de l'État d'Israël, affirment tous deux qu'au travers du processus de libération/émancipation des juifs qu'ils valorisent et dont ils se savent bénéficiaires, des pertes, culturelles irrémédiables sont survenues et imposent un devoir d'effort de mémoire, quels que soient les engagements du présent. L'examen des positions d'Arendt est mené en contrepoint avec celui des écrits de cette Rahel Levi, qui a assez fasciné Arendt pour qu'elle lui consacre une biographie. Dé-judaïsée, Rahel, devenue par mariage Varnhagen von Ense, tient un salon littéraire à Berlin, vers 1800, mais découvre avec douleur les impasses de l'assimilation et la pauvreté de sa culture juive au point de conclure que, juive bien sûr, elle n'est plus rien. Arendt, tout aussi consciente de l'antisémitisme toujours menaçant, affirme un choix clair en faveur de l'humanisme universaliste, mais n'en proclame pas moins le devoir pour les juifs d'entretenir une culture historique, fondement d'un minimum d'identité et considère avec quelque nostalgie l'époque où, sans État, le peuple juif était comme en dehors de l'histoire. Pour Birnbaum, la clé de ces contradictions serait à chercher dans le vague du contenu de sens qu'elle donne au judaïsme. On peut, tout autant, juger que ces positions se situent sur des plans différents, la réflexion de philosophie morale conduisant à l'engagement universaliste ne supprimant ni le devoir de solidarité, ni le sentiment douloureux d'une rupture qu'il fallait assumer en pleine connaissance des pertes liées.

Isaïah Berlin, sans aller jusqu'à la double allégeance, est un précurseur du communautarisme. Il nous est présenté à partir de son ouvrage *The Roots of romantism,* comme influencé par Herder, théoricien des nationalismes fondés sur les communautés vécues de langue, de culture et de pratiques qu'il est difficile de ne pas considérer comme opposé sinon à la Déclaration des droits de l'Homme, du moins à la conception française de l'État-Nation. Le débat vif évoqué avec l'anthropologue Ernest Gellner et le sociologue Steven Lukes, tous deux juifs, l'un Allemand de Berlin, l'autre Anglais, mais sans concession quant à leur attachement aux Lumières et à l'émancipation des juifs, ne laisse aucun doute. Sur ces sujets, Sir Isaïah, malgré ses protestations rationalistes, affirme un engagement émotionnel qui le conduit à faire fi de la cohérence ordinairement attendue d'un philosophe. Il est du côté de ceux qui refusent le nivellement par l'uniformité, qui valorisent la chaleur communautaire ce qui, soit dit au passage, explique son hostilité à cette Arendt si froide devant Israël.

Qu'il soit révéré par Charles Taylor et par Michael Walzer n'étonne pas, mais on voit se préciser les connotations à la fois romantiques et contre-révolutionnaires. En revanche, il n'est pas aussi assuré que l'imputation qui lui est faite de relativisme soit fondée. Rapprocher des textes qui ne se situent pas sur le même plan est toujours hasardeux. Quant à l'État d'Israël, il le voit, à raison, devenir un état normal, la crainte étant qu'il ne bascule du côté des nationalismes exacerbés.

Les auteurs retenus pour les deux derniers chapitres, Michael Walzer et Yosef Yerusalmi sont plus clairement des théoriciens du refus de l'assimilation. L'itinéraire de Walzer est parallèle à celui de Birnbaum, puisque, après *Spheres of justice*, il s'est rapproché des *jewish studies* en étudiant le rapport au pouvoir des juifs depuis l'exode. Il a produit une théorie très intéressante qui distingue l'*Exode*, expérience abondamment réitérée dans l'histoire par les peuples les plus divers, de l'*exil*, condition spécifique des juifs depuis la perte de la souveraineté politique. Privé d'État, le peuple juif a, d'une part, inventé des stratégies d'accommodement qui ont permis le maintien et la transmission de l'identité, d'autre part, créé des institutions communautaires assurant dans plusieurs contextes historiques et géographiques des zones d'autonomie. Aman, le ministre d'Assuérus à Babylone, serait le premier des antisémites, quand il fait reproche aux juifs de garder leurs traditions. Au livre d'Esther on lit en effet : « ces gens ont des lois qui diffèrent de celles de toute autre nation... Il n'est pas de ton intérêt de roi de les conserver ». La vie en exil ne sera possible que si la loi appliquée aux étrangers répond à des critères universalistes. « Une seule Loi régira l'indigène et l'étranger demeurant au milieu de vous » (*Exode* XII, 49). C'est ainsi que l'exil à Babylone est un moment fondateur de l'universalisme qui est d'abord celui des opprimés.

Yosef Yerusalmi, grand historien des marranes et qui succède à Salo Baron, n'a jamais cessé d'être un bon juif et il intéresse Birnbaum entre autres motifs parce qu'il a fort bien montré pourquoi et comment les communautés juives méfiantes ont cherché à se mettre sous la protection des tenants du pouvoir. Un joli exemple permet de comprendre : Lisbonne 1516 ; des dominicains excitent le peuple contre les juifs ; un massacre s'ensuit ; le consistoire écrit au bon Roi pour l'assurer, contre toute évidence, que bien sûr ni les bourgeois ni les autorités de la ville n'y ont pris part... et pour demander la protection du « bon Roi Manuel ». À défaut d'acceptation horizontale, les segments de la diaspora ont cherché, et assez souvent obtenu, une sorte d'intégration verticale, avec le phénomène bien connu des juifs de Cour, ainsi cet Isaac Cardoso, médecin honoré et respecté qui finit par fuir la Cour de Lisbonne pour se terrer dans le ghetto de Gênes et y vivre la vie d'un pauvre homme pieux. Ainsi Yerusalmi saisit, mieux que quiconque, en quoi, après deux millénaires de diaspora, l'extermination des juifs par la décision d'un État a été un phénomène sans précédent et ne saurait être comparée à aucune autre persécution, pas même celles de l'Inquisition qui n'avaient pas le monopole de la violence légitime. Que la Shoah ait suffit pour que les juifs ne puissent plus ne pas douter de la protection des États. Pour lourde que soit cette conclusion, elle ne conduit pas nécessairement à faire aussi peu de cas de la contribution à l'Universel que Yerusalmi et Birnbaum ne semblent s'y laisser aller parlant de stéréotype.

Au terme d'une lecture attachante, voire plus, la brève conclusion ne répond pas vraiment aux questions que le lecteur peut se poser quant aux leçons à tirer du livre. En allant d'auteurs non croyants, largement dé-judaïsés, assimilés (en Allemagne, en France ou aux États-Unis), ayant vécu ou, du moins, ayant été formés avant le nazisme, à d'autres, plus jeunes qui ont des attitudes plus complexes et qui font retour à la religion de leurs pères, mais n'ont pas choisi de vivre en Israël, il montre assurément la vigueur renouvelée d'un judaïsme culturel et religieux susceptible d'entraîner l'adhésion d'intellectuels qui, probablement, seraient, une génération plus tôt, restés en dehors. En cela il met l'accent sur une face peu connue de l'histoire juive récente, mais le choix des auteurs est marqué de beaucoup de singularités. Ces cas sont exemplaires, forcent le respect. On se réjouit qu'ils soient ; ils n'indiquent pas une tendance lourde. Au fond, l'auteur semble presque regretter d'être bénéficiaire d'une assimilation réussie en France. De là cette critique répétée contre ces intellectuels juifs qui n'ont pas choisi d'étudier les juifs. Implicitement, il leur reproche de ne pas avoir été assez juifs militants, d'avoir pensé que, juifs, ils ne devaient pas s'adonner dans leurs travaux à un particularisme juif. Soit, mais au nom de quoi ce reproche se justifierait-il ? Comme Pierre Birnbaum n'entre guère dans une analyse des apports culturels juifs à la culture contemporaine, sa position se réduit presque à une affirmation de devoir identitaire fondé, comme Arendt le souhaite, sur un savoir anthropologique et historique, philosophique peut-être, encore qu'il ait évité de traiter de Lévinas, dont l'œuvre aurait pu nourrir son propos.

Jacques Lautman

FASCISTS*

MICHAEL MANN examines with admirable thoroughness the literature concerning the social composition of fascist militants and voters. His ambition reaches further, however, to nothing less than a new general explanation of fascism's rise (later stages are treated in a sequel, *The Dark Side of Democracy: Explaining Ethnic Cleansing*, Cambridge, 2005).

As in all his extensive output as a comparative historical sociologist, Mann rejects any "one-dimensional model" (p. 118). He works within the framework of his multivolume *The Sources of Social Power* (Cambridge 1986 ; 1993), of which *Fascists* was first intended to be a part before metastasizing into a separate book. Mann argues that fascism was successful because it offered plausible solutions to simultaneous crises in all four of the sources of social power: ideological, economic, military, and political.

Mann usefully situates fascism within a family of authoritarian, nationalist and statist movements that surged in central, eastern and southern Europe between the two world wars. Fascism "piggy-backed" (p. 358) on that surge. It differed from the others by its mass base and by proclivities toward paramilitarism, expansionism, enemy-cleansing, and a "youthful blend of moralizing and violence" (p. 90). This approach simplifies the explicandum: why did authoritarians in some regions of interwar Europe but not others take these extra radicalizing steps? And it makes possible a terse definition: "fascism is the pursuit of transcendent and cleansing nation-statism through paramilitarism" (p. 13).

Mann is particularly eager to demonstrate the insufficiency of class and materialist interpretations alone. Fascists' beliefs, he insists, must be taken seriously and not dismissed as a surrogate for something else. Beyond that, beliefs must be rooted in organizations and institutions in order to influence action. A fascist might be simultaneously a policeman, an observant Protestant, a war veteran, a member of a white-collar union, a graduate of a nationalist school, a young "macho" male, and a native of a disputed border province. Each of these qualities could influence his political identity, and only the study of "fascist careers... in action" (p. 3) can sort them in order of priority.

This seems eminently sensible, and one can only wish that Mann had sometimes descended from his usual aggregates to trace more particular fascist itineraries. A model work in this genre, missing from Mann's encyclopedic bibliography, is Philippe Burrin's *La dérive fasciste* (Paris, Le Seuil, 1986), a masterful analysis of the steps by which Marcel Déat, Jacques Doriot, and Gaston Bergery chose certain options and closed off others in their journey from Left to Right.

* About Michael MANN, *Fascists* (Cambridge, Cambridge University Press, 2004).

Robert O. PAXTON, Professor Emeritus of History, Columbia, University, New York [ropl@columbia.edu].
Arch. europ. sociol., XLVI, 3 (2005), 568-571—0003-9756/05/0000-818$07.50 per art + $0.10 per page©2005 *A.E.S.*

Mann shows (in accord with recent scholarship) that fascist claims to transcend class divisions were justified. No other parties, except some Catholic parties, recruited as broadly, and indeed this was part of fascism's appeal.

As Thomas Childers, Detlef Mühlberger and others have already shown, fascist workers were not uncommon. In Hungary and Romania, where a socialist option hardly existed, fascism was authentically proletarian. Even in northwestern Europe significant numbers of workers joined fascist parties or voted for them, though somewhat below their proportion in the general population. Proletarians (along with students) actually dominated paramilitaries, even in northwestern Europe. Even Marx knew that objective class position ("class in itself') is a poor predictor of behavior. Those workers deeply rooted in "worker ghettos'' were relatively immune to fascist appeals, as were Catholics well-immersed in diocesan life (though Catholics were later disproportionately numerous among the "willing executioners''). That socialization strongly affects recruitment is now universally admitted, and Mann may be battering down an open door here.

He is more original on the middle class. Fascism was predominantly middle class, but so were all non-Marxist parties, as Mann notes refreshingly. This commonplace remark thus carries less weight than used to be thought. He differentiates fruitfully among strands of the middle class. He locates one fascist "core constituency'' in those who worked for the state. Civil servants, teachers, agents of law and order were attracted by nationalist statism. Fascist cadres, moreover, as distinct from the street fighters, were generally well-educated and upwardly mobile, rarely the kind of misfits and marginal people that the old impressionistic works often identified as fascist.

Categorization problems always plague this sort of argument. Did teachers become fascist in the same way as other civil servants like policemen? Again, we need – and can not get at this level of abstraction – more particular fascist itineraries: the successive steps by which a potential fascist made the choices that led to fascism.

More unconventionally, Mann identifies as another "core constituency'' those sectors of the middle class he considers distant from the front lines of class warfare: artisans, shopkeepers, lesser functionaries and so on. He gives this group a curiously negative definition. Those most directly involved in class warfare tended to be either pre-empted by membership in the proletarian counter-society (workers) or suspicious of fascists' radical rhetoric (businessmen). The left-overs, it seems, were available for fascism.

At work here is Mann's reluctance to attribute as much explanatory power to material interest as most authors do. He is surely right to challenge what may be only a residual Marxist convention. Moreover, he does recognize the preeminent role of class conflict in inescapable cases: the Po Valley landowners' resort to Mussolini's Blackshirts to combat agrarian socialism;

the clash between socialist Vienna and the peasant hinterland of Austria. But can we follow him when he finds "no overall relationship between economic cycles and authoritarian surges in the interwar period'' (p. 57)?

Mann has relied here on a simultaneity test. Similarly he asks us to believe that "Hitler's coup in 1933 was surely too late to be directly attributed to defeat in the First World War'' (p. 66). Or that the fear of revolution played little role in generating support for Hitler and Mussolini, since revolution became unlikely (as we know now) after 1920. He is mystified by the panicky over-reaction of many European elites, but what counts is perception of crises and obsessive recollection of them, and these, alas, are refractory to the sociologist's quantifications.

Mann's first two chapters are necessarily somewhat abstract, as he tests the utility of numerous variables linked to the rise of fascism. Since he explicitly rejects multivariate analysis here, in the absence of a statistically significant number of cases, the argument seems reduced to testing one variable at a time.

The book picks up steam with five chapters devoted in a more narrative style to national case histories: Italy, Germany, Austria, Hungary, Romania, and Spain. Now we can begin to see how the factors "intertwine'' (a favorite word), and under what circumstances authoritarians may turn to fascism. We see clearly how profoundly the various fascisms differed. This reader found the discussions of Spain, where Mann spent a year, and Romania particularly informative. In both instances authoritarians actually held fascists at bay.

Despite his usual meticulousness about terminology, Mann has succumbed to one convention. He refers throughout to fascist "*coups*'', even while admitting quite explicitly that "there was no Nazi *coup*'' (p. 200) and in Italy at most a "half-*coup*''. This may have happened because Mann's narrative remains too uniformly general. Some crucial turning points where powerful individuals determined the outcome remain off stage: King Victor Emmanuel III's capitulation to the bluff of the so-called March on Rome; the tortuous efforts of conservative political leaders to coopt Hitler's mass following for their own purposes. One recalls yet again the famous remark in Angelo Tasca's 1927 classic on the rise of Italian Fascism that to understand fascism one must write its history.

Mann laments that he lacks the representational skills to "plot fascist movements (each one obviously unique) amid a five-dimensionsional space'' (p. 17). If one defines the fifth dimension as time, the matter becomes simpler. Mann could have dealt more systematically with changes over time. He neglects to specify even the elementary distinction between fascist movements out of power and fascist regimes, although of course he often notes these differences in concrete instances.

As for the future, Mann believes that similar movements of expansionist, cleansing nation-statism are possible (for example, in Russia), but they would not call themselves fascist. One hopes he is not too optimistic.

Michael Mann is by nature a poser of questions, a challenger of convention, and a taker of risks. Sometimes this leads him onto thin ice and into contradictory statements and idiosyncratic positions. It also gives his account of fascism an uncommon vivacity and interest, and leaves the rest of us some unsettled issues to resolve. He has enriched but not exhausted the subject of fascism.

ROBERT O. PAXTON

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

TERRORISME ET MISSIONS SUICIDE*

VOICI UN OUVRAGE académique dont les articles tranchent avec les travaux journalistiques qui envahissent le marché du terrorisme. Dans cet ouvrage, plusieurs thèmes sont traités exhaustivement. Le premier est la notion même de *kamikaze* dont on se sert souvent sans en connaître le contexte historique et l'ampleur. Peter Hill parle des Kamikazes japonais entre 1943 et 1945 et montre comment quelques 3000 pilotes, de l'armée de l'air et des forces navales japonaises se firent tuer en jetant leur avion sur les vaisseaux alliés. L'auteur montre fort bien le contexte religieux japonais, les modèles de suicide honorable que charriait la culture, l'éducation militaire qui leur a été dispensée, les données chiffrées sur les formes d'attaque et leur efficacité sur le plan militaire. On apprend en particulier comment on sélectionnait les candidats au kamikaze, quels étaient leur entraînement et leur encadrement idéologique, leur motivation et les derniers jours qu'ils vivaient.

Stephen Hopgood analyse les Tigres Tamoul dont on sait qu'un nombre fort important a accepté de mourir en martyr au service de la cause nationale dans la guerre civile au Sri Lanka. Tout comme l'étude précédente, le contexte historique, le mode de recrutement, les motivations, les types d'action, leur nombre et le mode de sélection des candidats ainsi qu'une appréciation de leur efficacité politique et militaire sont analysés avec vigueur. L'auteur souligne comment le sentiment d'appartenance à une élite motive les candidats. La religion ne semble pas jouer un rôle déterminant pour le kamikaze et l'auteur pense que ce n'est pas dans la religion, même l'islam, qu'il faut chercher les raisons essentielles de l'implication des candidats pour la mort dans ce type de mission « suicidaire » (*suicide missions*).

Luca Ricolfi analyse le cas palestinien de 1981 à 2003. Il procède comme dans les deux autres cas, à l'analyse de l'ampleur du phénomène que ce soit dans la période « libanaise », Intifada 1, Période d'Oslo, puis l'Intifada post incident al Aqsa. Il dégage les motivations, l'efficacité supputée de ces actes, la représentation des autres Palestiniens de ces missions « suicidaires » ainsi que les causes historiques et politiques.

Stephen Holmes consacre son chapitre à Al-Qaeda jusqu'au 11 septembre 2001. Ici aussi, l'analyse est exemplaire pour sa rigueur, le traitement des données quantitatives, les personnes impliquées, avec une section spéciale pour Mohamed Atta. Il montre en particulier comment tous les membres de l'équipe étaient issus des classes moyennes et les raisons de leur implication dans les « missions suicides ». Il dégage l'arrière-plan psychologique des auteurs de cette mission meurtrière et souligne qu'ils se considéraient comme des « soldats », proposant une notion comme « auto-martyre » (*self-*

* A propos de Diego GAMBETTA, ed., *Making sense of Suicide Missions* (Oxford University Press, 2005).

Farhad KHOSROKHAVAR, École des Hautes Etudes en Sciences Sociales, Cadis [Farhad.Cavard@ehess.fr].
Arch. europ. sociol., XLVI, 3 (2005), 572-574—0003-9756/05/0000-819$07.50 per art + $0.10 per page©2005 *A.E.S.*

martyrdom) située entre le martyre et le suicide. L'auteur pense que la logique de ces acteurs relevait plutôt d'une « éthique martiale » que d'une « éthique religieuse ». Il souligne en particulier que les personnes recrutées par Al Qaeda pour accomplir les missions du 11 septembre combinaient une grande rancœur contre l'Occident avec un sentiment de mépris de soi et de reproche à soi. L'analyse tente de dégager le sens de la peur de mourir et son dépassement dans ce cas précis, en référence au Coran notamment. L'analyse porte aussi sur les concepteurs et les dirigeants d'Al Qaeda qui ont conçu l'opération. Selon l'auteur, ce n'est pas l'islam ni une version de fondamentalisme islamique qui ont motivé ces attaques, mais le reproche fait à l'Occident d'agresser les musulmans. Il ne s'agirait pas de vouloir convertir l'Occident à l'islam ni de mener une guerre de religion, mais d'un moyen politique de riposter à l'Amérique. La religion, en l'occurrence, ne joue pas le rôle fondamental, elle est mise au service de griefs et de revendications séculiers.

Michael Biggs traite de l'immolation de soi entre 1963 et 2002, remontant au modèle d'origine au Vietnam en 1963, puis, passant en revue les autres pays où des actes d'auto-immolation se sont produits, que ce soit en Inde, en Corée du Sud, aux Etats-Unis ou ailleurs. Il compare l'efficacité de ce type d'action avec les « suicide missions » et procède, lui aussi, à une analyse systématique des motivations pour ce genre d'action.

Stahis N. Kalyvas et Ignacio Sanchez-Cuena analysent des missions où l'on tue l'autre sans se faire tuer. C'est notamment le modèle adopté par *Irish Republican Army* en Irlande du Nord ou encore, ETA en Espagne, les Brigades Rouges en Italie, *Baader Meinhof* en Allemagne, le Sentier Lumineux au Pérou et d'autres groupes armés. La comparaison avec les autres formes d'activisme déjà citées est intéressante, que ce soit en termes de motivation ou d'efficacité escomptée. L'auteur souligne que c'est la répression politique et la privation économique qui sont à l'origine de ce type d'acte et non la religion en soi, même si des ressources religieuses peuvent être mobilisées à cette fin.

L'article de Jon Elster résume bien les termes du débat autour des « suicide missions » mais son schéma explicatif tourne court et en un sens, renvoie à l'impossibilité de trouver une unité dans la diversité de ces différents modèles de conduite extrême qui ont pour visée la dénonciation ou la protestation. Il présente des notions comme « quasi-croyances » (*quasi-beliefs*) ou « *quasi-motivations* » qui sont entre une logique de motivation dictant une conduite et d'autres facteurs intervenant dans la décision ultime d'aller vers la mort.

Enfin, Diego Gambetta tente de présenter une vision unifiée dans la diversité des cas analysés. Il dégage cinq traits fondamentaux : mises à part les auto-immolations, les autres formes de « *suicide missions* » se font avec l'aide d'une organisation. En second lieu, différents types d'organisations armées se prêtent à ce type d'activisme, allant des armées régulières aux milices nationalistes (Tigres Tamouls) aux organisations politico-militaires comme le Hezbollah libanais ou le PKK au Kurdistan. En troisième lieu, en règle générale, au sein de ces organisations, les missions kamikaze forment une partie de leurs activités. La quatrième caractéristique est que ces organisations sont celles qui n'ont d'enracinement dans aucune communauté ou bien, celles dont les communautés de soutien les approuvent dans leur extrémisme (ceci rend plutôt triviale la première affirmation qui serait intéressante). Le cinquième trait général est que les missions kamikazes sont le fait de groupes en position de faiblesse par rapport au groupe adverse. L'auteur souligne en passant que les attaques de cette nature se font dans la perspective d'un fort impact politique. Enfin, il souligne que ces types d'activité ne peuvent perdurer qu'en opposition aux régimes démocratiques.

L'ensemble de l'ouvrage, on s'en rend bien compte, est fort sérieusement construit. Les analyses sont fort bien renseignées et fournissent des données quantitatives souvent fiables. L'une des faiblesses de l'ouvrage est néanmoins son « provincialisme anglo-saxon ». De la riche littérature en français, en allemand et en d'autres langues européennes fort peu est signalé. Les recherches de l'auteur de ces lignes sur le martyre en Iran et en Europe et Al Qaeda dans une perspective comparatiste, celles de Jean-François Legrain, Pénelope Larzillière, Agnès Pavlowsky et bien d'autres sur les kamikazes palestiniens, celles de Joseph Allagha sur le cas libanais ou celles de Gilles Kepel sur les kamikazes égyptiens, sont totalement ignorées. Un autre problème de ce travail est la faible exploitation des données disponibles sur la subjectivité des martyrs (interviews par exemple, qui existent en nombre suffisant).

Les auteurs ont souvent raison de dénoncer le culturalisme qui consiste à identifier dans la religion (en l'occurrence, l'islam) le motif essentiel des « suicide missions », il n'en demeure pas moins qu'entre le culturalisme absolu en la matière (attribution de la cause à la religion ou à la culture) et une attitude objectiviste tout aussi absolue (la religion ou la culture ne jouent pas de rôle, des motivations politiques ou économiques étant les seules pertinentes), il y a beaucoup de combinaisons possibles et c'est dans le cumul des deux facteurs et de leurs nuances que réside la capacité effective de proposer des analyses « fines ». Malgré ces défauts mineurs, l'ouvrage est indispensable comme source de renseignement et d'analyse pour les chercheurs dans ce domaine.

Farhad Khosrokhavar

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

AFTER SLUMBER: THE SOCIOLOGICAL STUDY OF SLEEP*

WHILE THERE IS no shortage of articles on sleep in newspapers and popular magazines, the state of non-waking has remained conspicuously absent from sociological debate so far. In spite of occasional illuminations about sleep, e.g. as a tension release phenomenon – Parsons (1), and a few isolated papers: Aubert/White (2), Schwartz (3), Taylor (4) – a monograph on the subject was missing and, indeed, to be missed: one can hardly avoid the thought that a sociology that is almost completely silent about one-third of human life is quite limited in its ability to comment on the other two-thirds.

In his new book, the University of Warwick's medical sociologist Simon J. Williams comes to our rescue. It is a risky operation, though, on which he invites his readers to join him. "Ventures" are promised, and even more so such that lead "into the (un)known", in brief nothing for the feebleminded. Williams delivers on his promise admirably, yet it is also clear that this first exploration will need several follow-ups in order to put sleep fully and sustainably on the sociological agenda and thus fulfil the author's aspiration. This aspiration is justified by the strong aspects of this text: the historical setting and contextualisation of sleep within systematic terms, the diagnosis of what Williams calls the "sleepicisation" of society, his notion of a sleep role and the emphasis on the liminality of sleep, the sensitivity for the development of a veritable capitalist sleep industry in our times, and an outlook on the possible future of sociological as well as interdisciplinary sleep research. This review will highlight these strengths first, and then address those points that should appear on the sociological sleep agenda for adjournment.

First of all, the broad historical perspective on sleep, which is laid out in the two beginning chapters and forms an undercurrent to the rest of the book, deserves some praise. The accounts of thought about sleep from the ancients on up to modern sleep science and medicine or even the history of beds present different ways of *how* sleep has been socially constructed, in philosophy, science, and literature. Williams translates the historical references on sleep and rest, which he has unearthed, into systematic terms of the sociology of the body. Thus, the disciples sleeping in the Garden of Gethsemane represent biblical bodies. Norbert Elias's theory of the civilising process is invoked to supply us with the notion of civilised bodies,

* About Simon J. WILLIAMS, *Sleep and Society: Sociological Ventures into the (Un)known* (London/New York, Routledge, 2005).

(1) Talcott PARSONS, *The Social System* (London, Routledge and Kegan Paul, 1951).

(2) Vilhelm AUBERT and Harrison WHITE, "Sleep: A Sociological Interpretation" [I and II], *Acta Sociologica* (vol. 4, fasc. 3, 1959, pp. 46-54).

(3) Barry SCHWARTZ, "Notes on the Sociology of Sleep", *The Sociological Quarterly. Journal of the Midwest Sociological Society*, (vol. 11 (4), 1970, pp. 485-499).

(4) Brian TAYLOR, "Unconsciousness and Society: The Sociology of Sleep", *International Journal of Politics, Culture and Society* (vol. 6 (3), 1993, pp. 463-471).

Anja FINGER, University of Erfurt, Germany [anja.finger@stud.uni-erfurt.de].
Arch. europ. sociol., XLVI, 3 (2005), 575-578—0003-9756/05/0000-820$07.50 per art + $0.10 per page©2005 *A.E.S.*

whose sleep has become privatised. Michel Foucault, notably the one of *Discipline and Punish*, and Max Weber come into play to examine the sleep habits of disciplined and ascetic bodies. The historical studies Williams draws on, such as A. Roger Ekirch's unique one about *Pre-Industrial Slumber in the British* Isles (5), illustrate the contingency of sleep arrangements. This study contends that early modern sleep was a far cry from contemporary musings about an idyllic past, but often disrupted, non-restorative, and generally segmented: after a period of first sleep, an interval of waking would be followed by a second sleep. Victorian discourses about over-crowding and poverty dealt with sleep, often with a moral/istic undertone. Sleep figured as an issue of social class in the "spatialisation of dormant (working-class) bodies" (p. 65).

Is there a chronic sleep deprivation in contemporary Western society? The pros and cons of this timely debate are thoughtfully balanced, using statistics from the USA and the UK. Williams does not put forward a decision for either side; for him it suffices that sleep "is seen to be the 'casualty' of profound social, economic and technological change over the past century or so" (p. 105). Different institutions all have their own ways of dealing with sleep: day-care and pre-school centres, prisons, hospitals, and nursing homes are screened for typical sleeping arrangements. The sleep of the homeless and their deviant sleep roles encourage critical self-reflection: "Sleep, in short, may well be embedded in all our lives, but it is only the favoured or fortunate among us whose embodiment is truly embedded, whilst we sleep, night or day" (p. 133). Williams outlines the concepts of both medicalisation, which turns moral into medical issues, and healthicisation, which turns health-related into moral issues, and proposes an alternative diagnosis: the *Sleepicisation* of society. By this he means "a heightened social awareness and cultural sensitivity to sleep-related matters, which itself, in part, is a response to or rebuttal of, the 'incessant'/'poorly' slept society" (p. 165).

Echoing Talcott Parsons's *Sick Role*, Williams sketches out a *Sleep Role* and the specific rights and responsibilities it involves. The role occupants are unaware of their role occupancy, which makes the sleep role untypical compared to other roles. Sleep also comes to be seen as "an embodied non-experience which is *liminal* in at least two principal ways: first because it is inferred rather than directly experienced; second, because it is neither an entirely voluntary or involuntary, purposive or non-purposive phenomenon" (p. 96).

Williams's final verdict on the business aspects of sleep and the emergence of a "sleep industry" is rather laconic: "Whether it is sleep or wakefulness, indeed, capitalism profits" (p. 160). Sleepicisation involves a translation of social and medical problems into sleep-related ones and does not even spare sociology. Of this last point Williams finds himself to "no doubt

(5) A. Roger Ekirch, "Sleep We Have Lost: Preindustrial Slumber in the British Isles", *American Historical Review* (Apr. 2000, pp. 343-387).

stand accused'' (*Ibid.*). After all, there is no doubt in the reviewer's mind that the author wants to be taken to account. Therefore one has to refuse the reluctant invitation made at the very end, "something to sleep on perhaps?'' (*Ibid.*). Particularly so, since Williams provides us with some pointers as to how the topic of sleep is to be tackled in the future. He rejects a playing out of sleeping against waking life and sees the two as a continuum. Sociological sleep research should not contribute to the fragmentation of sociology but have an impact on the variety of its concerns. When time would be ripe for interdisciplinary investigations into sleep, "embodiment'' may be used to integrate the different perspectives, because it has the advantage of being anti-reductionist, the author asserts. This seems to be a well-balanced proposal; whether those working on sleep-related matters will take it on board, the future shall tell.

Ventures involve the possibility of loss as well as the chance for profit. The sociological ventures of Williams's book do indeed offer such a chance, metaphorically speaking as the author does – but does he need to borrow his metaphor from the world of business, finance and the very capitalism he rightly, if shyly, criticises for profiting from the sleep industry? An impressive panoply of perspectives and issues related to sleep is presented, and the concept of the sleep role as a re-application of social theory is no mean feat. Yet, as is only to be expected given an intellectual product of this scope, some parts invite further critical comment, too.

In the *Introduction*, three different levels of analysis are distinguished – individual/(non)experiential, social/interactional and societal/institutional levels – around which certain specific problems as well as key concepts are grouped respectively, followed by examples for research perspectives (p. 5). As far as the structure of the text is concerned, one wonders where these different levels of analysis have ended up. They are mentioned only briefly in the conclusions, while they are not, as structuring components, explicitly used in the main chapters. A more prominent role for these differentiations might have strengthened the systematic backbone of the book.

Sociologists who try to introduce new subject matters are often hard-pressed for definitions. Williams has chosen Dement and Vaughan's definition of sleep with its two criteria: first, sleep erects a perceptual wall between sleeper and external world, second, it is reversible (6). This definition originated in medical sleep research. Maybe it is too early to propose a genuinely sociological definition of sleep. Or one could argue against the urge for definitions on principle. When pondering the question of a definition of society, Adorno quoted Nietzsche: "Only that which has no history is definable'' (7); that sleep indeed does have a history is definitely one of the results the reader of Williams's book will bear in mind.

(6) William C. Dement and Christopher Vaughan, *The Promise of Sleep: The Scientific Connection between Health, Happiness, and a Good Night's Sleep* (London, Pan Books, [1999] 2001).

(7) Theodor W. Adorno, *Introduction to Sociology*, edited by Christoph Goedde and translated by Edmund Jephcott (Cambridge, Polity Press, 2000).

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

While the sleepicisation thesis as well as the notions of the sleep role and the liminality of sleep provoke further thought as theoretical applications and revisions, some of Williams's concerns with embodiment and the lifeworld are less easy to grasp, particularly when one thinks of the wide range of readers the book is intended for. Williams approvingly introduces Drew Leder's (8) concept of the absent body. According to this, sleepiness is a mode of bodily "dys-appearance", sleep involves "depth disappearance" and a severance from waking. Even though the sleeping body is indirectly accessible, sleep entails an absence from oneself and others. Williams then turns to Marcel Proust's *Swann's Way* – the first volume of the famous *Recherche* with its multiple appearances of sleep, of falling asleep as well as being asleep – and concludes: "A phenomenology of sleep, in short, Proustian or otherwise, helps us recover or reclaim, if not reconceptualize, our cyclical (ad)ventures into this great 'abyss of oblivion' or 'not-being'. 'To be or not to be?': that indeed is the question" (p. 73). Here it is again, the leitmotif of adventures and ventures, even enriched by some Hamletian dramatics. However, when descending from these literary heights, one might feel a bit uncertain as to how this phenomenological project might be continued by or linked to a more focused, sociological one.

In view of the variety of historical material discussed, individual specialists are probably going to miss some details in whatever happens to be their area of expertise. This is due to the survey character of this text, which prepares the ground for future investigations. In this vein, one might go on e.g. to look for alternatives to phenomenological approaches relying on a philosophy of consciousness, or to scrutinise possible links between biblical and civilised bodies. Do the latter ones really represent just another starting point than the former ones, as Williams suggests? Or is there a way in which biblical and civilised bodies have been connected historically? How are the concepts of civilised and disciplined/ascetic bodies systematically related to one another? Further inquiry into these matters is required and in fact called for: the conceptual triad of sleep-related bodies, which Williams has mapped out, opens up new ways of combining systematic with historical concerns from a perspective of embodiment.

The sociological debate on sleep has been opened, and anyone wishing to take part in it is well advised to consult Williams's text. Finally, the book would not be fully done justice to, if the bibliography was not mentioned: being a rich source, it excels both as a complement to an equally rich text and as a travel companion for all those ready to wander off into the land of sleep sociologically.

Anja Finger

(8) Drew Leder, *The Absent Body* (Chicago, IL., University of Chicago Press, 1990).

DILEMMAS AND CONTRADICTIONS OF POWER*

One does not expect any of Gianfranco Poggi's books to be conventional, and this book does not disappoint that expectation. Individual, even idiosyncratic, it is inspired by writings and traditions remote from, or at least ignored by, most contemporary discussions of power in the social sciences. Thus Poggi seeks to rescue the work of such writers as Herbert Rosinski, whose *Power and Human Destiny* (1965) is a principal inspiration of the present book. He also makes extensive reference to the neglected (at least in the English-speaking world) thinking of Arnold Gehlen.

Following Rosinski's "anthropological'' approach, Poggi sees power in the broadest sense as "the distinctive human ability to make a difference to natural circumstances of the species'' (p. 8). Applied to the social realm – the realm of necessary inequality – this means "the ability to make a difference to the making of differences'' (p. 8). By exercising power over you I employ my ability to make a difference to control the like ability that you (equally) possess to make a difference (e.g. by enslaving you). This gives rise to power relations between people, with all that that implies by way of the characteristic "dilemmas and contradictions'' of power: self-interest, force, oppression, subversion, resistance, inertia, etc.

A particularly distinctive property of power, Poggi points out, is its "potentiality'': its relation – uniquely in the human case – to the future. "We can think of power (both power in general and social power in particular) as a way of confronting and controlling the inexorable sense of contingency and insecurity generated by our awareness of the future'' (p. 11). As Hobbes says, we are the only animals that can feel tomorrow's hunger today. While this rightly suggests – following the Parsonian tradition – that power has a positive valency, an enabling quality, it can equally fortify negative conceptions of power, since my power to control the future may be at your expense, your potentially equal capacity to do the same. My security is purchased at the cost of yours.

A final preliminary involves a brief discussion of one of the most influential concepts of power in the current literature, that associated (implicitly) with Steven Lukes and (explicitly) with Michel Foucault. In this view power operates at its most effective when largely unsuspected and so uncriticized and unchallenged. Power is inherent in the workings of apparently unproblematical, routine, structures which nevertheless incorporate and promote the interests of particular groups. This is power as ideology or as "knowldege'' or "discourse''. Poggi is aware of the persuasiveness of this view but he objects on the grounds that it removes the concept of "agency'' from power. By focusing on "the unperturbed continuation of established

* About Gianfranco Pogghi, *Forms of Power* (Cambridge, Polity Press, 2001).

Krishan Kumar, William R. Kenan, Jr., Professor of Sociology University of Virginia [kk2d@virginia.edu].
Arch. europ. sociol., XLVI, 3 (2005), 579-581—0003-9756/05/0000-821$07.50 per art + $0.10 per page©2005 *A.E.S.*

arrangements'' such a concept is incapable of accounting for the frequent disruptions and discontinuities of power relationships.

Poggi does not much like definitions – perhaps in the spirit of Weber's "definitions should come, if at all, at the end of the inquiry rather than at the beginning'' – but he "reluctantly'' offers one, again very much in the Weberian spirit which emphasizes power not as a property, a thing possessed, but as a relationship:

> Social power relations exist wherever some human subjects (individual or collective) are able to lay routine, enforceable boundaries upon the activities of other human subjects (individual or collective), in so far as that ability rests on the former subjects control over resources allowing them, if they so choose, to deprive the latter subjects of salient human values. The chief among such values are bodily integrity; freedom from restraint, danger or pain; reliable access to nourishment, shelter or other primary material goods; the enjoyment of a degree of assurance of one's worth and significance. (p. 14)

It is clear that with this we are back on fairly familiar territory; and, though one might regret that from certain points of view, it certainly adds to the usefulness of this volume, in that it can be fitted without too much difficulty into existing debates and discussions of power. Thus Poggi is, by his own admission, fairly close to Michael Mann's – and of course Weber's – typology of power, in distinguishing political, economic, and "normative/ideological'' forms of power. This gives rise to the familiar trinitarian system of social stratification by class, status group, and "party'' (or some other political organization whose principal resource is the power to coerce). Poggi, like Mann, also considers military power as a possible independent form of power. But while he admits that the military can on occasion define an autonomous will and interest, he rightly argues that, *pace* Mann, "organized violence'' should not be seen as an independent source or form of power but rather as a dimension, perhaps the most significant dimension, of political power.

But if the skeletal structure of Poggi's account is fairly familiar, his manner of fleshing it out is anything but. He does not engage much with the conventional scholarly literature on power but instead relies on his own wide-ranging knowledge of history, literature, law, philosophy – and the human condition as such. Thus one of the most agreeable aspects of this book is his use of numerous proverbs, in all the European languages, to express or illustrate various aspects of power – e.g. the cynically German "*gegen Demokraten helfen nur Soldaten*'' – against democrats only soldiers are of use. He draws liberally on Dante, Shakespeare, the Bible and the classics of Greek and Latin literature. Throughout also there are references to many French, German and Italian writers, past and present, who are largely unfamiliar to English-speaking students and scholars but who have many interesting things to say about power. There is, also, a continual and studied use of irony – perhaps the stylistic hallmark of this book – as in his extensive discussion of the alleged trade-off between political inequality and material benefits in the modern constitutional state. Altogether this is a most refreshing and engaging approach, and is sure to make the book popular with students of politics and sociology.

Of the many topics discussed in this very comprehensive survey, I single out three to which Poggi gives unusual attention and whose importance he brings out very clearly. He is very good on the question of the justification of power, a topic that is particularly well covered in the extensive discussion of the role of intellectuals, especially creative intellectuals torn between being court jesters and critics. He takes seriously – drawing especially on Hans Popitz – the whole process of the institutionalization and differentiation of power, an approach often dismissed simply as an aspect of Parsonian functionalism. And he emphasizes, more clearly than I have seen in any other discussion, the "ethical tension" between political power (the *ur*-form of power, as it were) and the presumption of natural equality on which it is based. All power relations, Poggi shows, assume people who share the ability – in principle equally – to think, act and communicate. Political relationships are "artificially established between parties who necessarily recognize each other as equal, otherwise one of them could not command and the other obey" (p. 49). The asymmetry of power relations is based on this underlying symmetry. Hence the inherent and permanent instability of all power relations, hence the overriding need for justification and ideologies of legitimation. The Weberian inspiration is acknowledged; but here as elsewhere in this stimulating and highly readable book it is the creative elaboration of the master's ideas that is the real achievement.

Krishan Kumar

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

CLARISSA RILE HAYWARD

SPACE AND THE STATE IN THE TIME OF GLOBAL CAPITAL*

HOW SHOULD we understand the role of the state in processes of global restructuring? Is it the case, as some have suggested, that technological advances in communication and travel; the globalization of production, investment, and trade; and the growth of transnational corporations and supranational governance institutions render nation-states increasingly powerless, increasingly politically insignificant? What about local places, such as the great cities and the metropolitan regions that figured so prominently in earlier phases of capitalist development: are these, too, declining in significance, as place-less global space erodes the materiality, the territoriality of social, political, and economic life? These are the principal questions Neil Brenner asks in *New State Spaces: Urban Governance and the Rescaling of Statehood*. The latter two he answers with a resounding "no". Brenner's central claim is that, although nation states have undergone significant restructuring in the face of globalizing trends, it is wrongheaded to suggest that they have ceased to matter. Instead, the *ways* states matter – the ways they exert influence in social, political, and economic life – have changed.

Focusing on postwar Western Europe, Brenner makes that case that there is an unavoidable, a significant, and a dynamic spatial dimension to state power. Both the ways states structure their own institutions and practices, and also the ways they exercise power over non-state actors, shape and are shaped by the physical spaces in and through which they govern. States organize themselves territorially. They define borders and boundaries. They demarcate spatially distinct political jurisdictions. States adopt strategies and policies, as well, (investment strategies, for instance, housing policies) in ways that are influenced by space and that create differential spatial effects.

These and similar spatial politics, Brenner's claim is, have shifted dramatically in the face of globalizing pressures. The past several decades have witnessed what the author terms a "rescaling" of state power: a change in the hierarchical relation of levels of government to one another, as governance grows increasingly decentralized. In addition, they have witnessed what, in his terms, constitutes a major shift in "state spatial strategies": a change in the aims or the ends of spatial politics, from promoting redistribution across places with a view to alleviating place-based inequalities within the national territory, to investing disproportionately in already-advantaged major cities and metropolitan areas with a view to promoting global economic competitiveness. These shifts, Brenner suggests, undermine what he calls "socio-spatial justice". They erode the relative equality and the social cohesion that characterized the Fordist-Keynesian era. What is more, he claims that

* About Neil Brenner, *New State Spaces Urban Governance and the Rescaling of Statehood* (New York, Oxford University Press, 2004).

Clarissa RILE HAYWARD, Department of Political Science, Ohio State University [hayward@polisci.sbs.ohio-state.edu].
Arch. europ. sociol., XLVI, 3 (2005), 582-586—0003-9756/05/0000-822$07.50 per art + $0.10 per page©2005 *A.E.S.*

nation-states have been complicit in this shift. "[T]he state is not a helpless victim of globalization'', Brenner writes, "but one of its major politico-institutional catalysts'' (p. 60).

The starting-point for this story of spatial/political change is the period from roughly the 1950s to the 1970s, when, throughout Western Europe, national state actors took on as a political problem the phenomenon known as "uneven development''. "Uneven development'' means, roughly, differential growth that yields the unequal distribution across space, of social, political, and economic resources and capabilities. Uneven development produces both core and peripheral zones. The former, typically major urban areas, are characterized by relatively dense concentrations of highly valued assets and socio-economic networks. The latter, typically rural regions or sometimes declining urban areas, are characterized by socio-economic marginality.

Brenner documents the ways European states during the Fordist-Keynesian era invested strategically in under-developed areas, as well as the measures they took (tax incentives, for instance) to stimulate and to re-direct toward the periphery important forms of private investment. He identifies, what is more, nontrivial effects of such strategies: a significant reduction in inequalities in intra-national per capital disposal income, for instance. And he explains their rationale. Mitigating the problem of uneven development within the national territory was viewed as crucial to promoting economic growth, because stimulating production and consumption in under-developed regions was understood as necessary for the rational distribution of both productive capacities and jobs. Investing strategically in peripheral zones, in other words, was viewed as good for the nation as a whole. To do so was to make efficient use of national resources, including labor power and land. It was to bring capital and jobs to the periphery, while relieving over-crowding, as well as labor and housing market pressures in urban centers. At the same time, it was a crucial means to promoting social integration and reducing conflict and unrest.

This logic no longer held, however, beginning with the crisis of the Fordist accumulation system and the Keynesian welfare state starting in the late 1970s. This turning-point in the story will be familiar to most readers. Rapid technological change drove the globalization of production. Global economic markets grew increasingly integrated, enhancing capital mobility. Meanwhile, heightened international economic competition, along with the decline of the relative political power of nation-states, undermined the Keynesian project of promoting full employment and sustained growth at the level of the national economy. Across Western Europe, the New Right mounted an ideological offensive against redistributive politics, effectively painting these as unnecessary, even counter-productive intrusions into capitalist markets. Now the idea that uneven development undermines the well-being of the nation as a whole did not hold sway. Instead, intra-national place-based socio-economic inequalities came to be widely viewed as unavoidable externalities of growth in the context of global, market-based competition.

What emerged was a new, competition-oriented political-economic regime, profoundly different from the Fordist-Keynesian system along at least two dimensions. The first is that of the organization of the state itself. Across Western Europe starting in the 1980s, Brenner demonstrates, states shifted from centralized, nationalized models of governance, to models based on the decentralization of political authority and the differentiation of governance practices, which increasingly were tailored to the circumstances – and in particular to the position in supranational place-based hierarchies – of particular cities and particular regions.

The second dimension is that of state spatial strategies. Even as states "re-scaled" their own institutions, they shifted their aims or their ends from redistribution within the national territory to competition beyond it. Brenner documents the growing emphasis, throughout Western Europe in this period, on enhancing the global competitiveness of major cities and urban regions, by encouraging inter-local competition and by concentrating resources in those localities judged to have the most competitive potential. Competition, rather than redistribution, came to be understood as the best path to national economic growth and vitality. The result was a deepening of intra-national space-based inequalities: a change, Brenner underscores, that cannot be attributed simply to the passive loss of power by nation states. Instead, his claim is, positive actions that states took (choices state actors made about how to structure political authority, for example, about how to invest collective resources and how to distribute the benefits and the burdens of growth) drove both the rescaling of political power and the deepening of uneven development.

In elaborating this historical account, Brenner does a particularly good job explaining, at what he calls the meso level, the broader political-economic shifts on which he focuses. He pays careful attention to particular decisions, patterns, and shifts in specific national and sub-national contexts, offering detailed and generally persuasive evidence of the trends that he documents. Yet at the same time, he situates these more contextual analyses within a systemic framework, attending to the ways in which broader political-economic structures constrain and enable local practices, policies, and initiatives.

The account is somewhat less successful, however, in establishing the unabated political significance of the nation-state, and in pointing to alternative, more just paths for spatial politics under conditions of global capitalism. The two difficulties are not unrelated. As far as the first is concerned, Brenner's explicit claim – that nation-states remain relatively politically powerful, even in the face of the globalizing trends that many commentators cite as weakening them – is not fully supported by the evidence he marshals. To be sure, he effectively demonstrates that it is *not* the case that states do *nothing*, or that what states do *does not matter* politically. State actions, he

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

shows, are among the proximate causes of the institutional decentralization and the shift away from redistributive politics on which his narrative focuses.

Yet few would argue to the contrary, at least not in these terms. Generally, "the declining significance of the nation-state" signals, not the inability of state actors to act at all, or the political irrelevance of the actions state actors take, so much as the severe constraints upon state actors produced by the imperative to compete for increasingly mobile capital. Brenner shows that states have some range of choice in terms of which strategies to pursue in response to this imperative: which particular institutional arrangements, which particular policies to adopt with a view to competing more effectively. What he fails to show is that states have any real choice in whether to pursue this end, and in particular in whether to pursue it at the expense of efforts to alleviate problems of uneven development. Is there a genuine possibility, under contemporary political-economic conditions, for nation-state actors to pursue spatially redistributive strategies? Or is the choice to abandon redistribution in favor of competition a forced choice (on the model of "Your money or your life!")? On this crucial question, Brenner's account is equivocal.

It is not entirely surprising, therefore, that he has little to say about what a more just alternative might look like, or how to achieve it. Near the conclusion of *New States Spaces*, Brenner suggests that redressing problems of contemporary spatial politics requires recentralizing key governance functions, as well as reconstructing new forms of "'big government' committed to the pursuit of sociospatial justice at all geographical scales" (p. 301). Here the matter of government's "commitment" seems key. It is not enough, he argues, for state actors to work to ameliorate the problems the competitive system generates. Such tacks fail, because they do not address the underlying structural sources of inefficiency and inequality.

The claim, in other words, is that justice demands a radical transformation of state aims or state ends. The second difficulty with Brenner's account, however, is that it fails to explain how such transformation might occur. By his telling, the key to the progressive character of the Fordist-Keynesian system was the belief on the part of state actors – that is, on the part of those who at that time were best positioned to make the relevant political decisions – that redistribution served the good of the nation as a whole. Can some functional equivalent of this logic be re-created under contemporary political-economic conditions? Can it be re-created at the appropriate levels of governance – that is, not only at the national, but also at the sub- and trans-national levels? To advance a constructive argument about how best to promote just global political-economic relations requires addressing these questions.

Notwithstanding these shortcomings, there is much to be learned from *New State Spaces*. The book offers a rich, detailed, and theoretically sophisticated account of important historical shifts in how states exercise power in and through space. It elucidates the specifically spatial dimension of how globalizing trends shape state institutions and state practices, and thus contributes substantially to our understanding of the challenges that face progressive politics today.

CLARISSA RILE HAYWARD

THE RISE OF RATIONAL CHOICE*

The most important and influential political theory taught in the universities of the West, or at least in those of the Anglo-American world, is not Rawlsianism or communitarianism or liberalism (let alone, nowadays, Marxism). Indeed, it does not officially go under the name of political theory at all: it is the account of rational action which is delivered to political science students under the description of "rational choice theory" or (sometimes) "formal theory" or "positive political science", and which is imparted to economics students under the description simply of "economics". Though it is characteristically presented as a model of human behaviour which stands or falls simply by its predictive power, and though its exponents generally disavow any (as they would call them) "normative" implications, its central claims have inevitably had considerable influence on how modern citizens think about their actions. To describe an action as "rational" is necessarily (all other things being equal) to value it and to advocate that it be carried out. The description may also predict what the agent will do, but there is of course no real contradiction between these two aspects of the term, any more than there is between the predictive and the evaluative aspects of the description of someone as (say) "temperate", "brave", "prudent" or "just". Like those qualities, rationality is a virtue, and when we ascribe it to people, we are *both* pointing to dispositional features of them which are quite reliable in predicting their conduct, *and* praising their character. Though the founders of modern economics, and their followers in political science, might have supposed that they were engaged in a "value-free" or "scientific" investigation, in fact they were doing moral philosophy.

There are a number of extremely important areas of social life where this moral philosophy has had something striking to say. Chronologically, the first was the claim which is at the heart of modern economics as it took shape between the 1870s and the 1930s, that it is "rational" for an individual producer of some commodity (including labour) to compete against his fellow producers even where hanging together in a cartel would benefit all of them. It often comes as a surprise to modern economists to realise that the "classical" writers, from Smith onwards, did not actually believe this (nor, indeed, did the most profound of the early marginalists, Edgeworth). They believed that it was natural and on the whole to be expected that producers would, as Smith put it, "conspire" against the public, and that if the producers did not, this was because of special circumstances such as barriers in communicating among themselves. Broadly speaking, these writers shared something like Hume's view of co-operation: rational agents will co-operate where the result of co-operation is a shared benefit, but human beings are

* About S. M. Amadae, *Rationalizing Capitalist Democracy : the Cold War Origins of Rational Choice Liberalism* (Chicago and London, Chicago University Press, 2003).

Richard Tuck, Frank G. Thomson Professor of Government at Harvard University [richard-tuck@harvard.edu].
Arch. europ. sociol., XLVI, 3 (2005), 587-593—0003-9756/05/0000-823$07.50 per art + $0.10 per page©2005 *A.E.S.*

often not rational, particularly where judgement of remote outcomes is necessary. So mechanisms of control or coercion will usually be needed to get people to collaborate, but these mechanisms are necessary because people are *not* rational, rather than (as modern rational choice theorists suppose) because they *are*. The essence of a competitive industry, the classical economists believed, was not that the individual producers would defect from a cartel, but that it was open to new producers to enter the industry and undercut the existing ones. Without free entry, they supposed, the human impulse to collaborate would always ensure a more or less cartelised production. This was true (for them) even where large numbers of producers were involved, and the contribution of each to the total output was correspondingly very small: the enormous weight which modern economists have put on the concept of "perfect" competition, defined as the situation where it is rational to defect from a cartel and undercut its price because one producer doing so will have no appreciable effect on the overall price of the commodity, would have seemed very remarkable to the classical authors.

The theory of perfect competition was in place from the early 1930s onwards, notably in the hugely influential work of Edward Chamberlin (*The Theory of Monopolistic Competition*, 1933). Its practical implication was that rational conduct on the part of a self-interested individual was no longer taken to consist in forming associations with other like-minded individuals and extracting benefits from their rivals (from consumers in the case of producers, from employers in the case of workers), but in breaking away from and undermining such associations. For economists of the late 19th century, associations and cartels were pre-eminently rational mechanisms for their members, though the rest of us might suffer from their activities, and might need to use state power against them. But for economists of the mid- and late 20th century, such associations were *ir*rational, and their members should accordingly not suppose that they were acting consistently when they continued to participate in them. If state power was necessary to break them up (as was often still the case), then the power was being used to compel people to do what was independently in their own interests, and it should therefore be seen as to some degree non-controversial and non-partisan. Paradoxically, though the writers of this period extolled individual freedom, they had produced a theory of coercion which was much more insidious than its precursors, since it required those subjected to it to believe that it was meeting their own wishes when it forced them to co-operate.

This idea remained the preserve of economists until the 1950s, but in that decade it began to be generalised to cover other forms of social and political life. The first writer to do so was Anthony Downs in his famous book *An Economic Theory of Democracy* (1957); the main point of his book was to analyse democratic politics as a competition for votes among the producers of political programs (i.e. parties), but towards its end Downs observed that

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

according to what had become the standard economists' model of rational behaviour, there was no good reason to vote in most elections, since one's vote will have a negligible influence on the result. This quickly became a popular puzzle in political science departments, where it continues to be studied under the name of "the problem of turnout". The effect of Down's argument has been to call into question the instrumental character of voting; instead, the most fashionable interpretation of voting is that it is "expressive", that is, it conveys a message about the political loyalties of the voter. It is hard not to think that the falling turnouts in all Western democracies, and the corresponding sense that the electoral process is primarily a kind of theatre of politics, are part of the same change in attitudes to democracy as the one taking place in the academic study of the subject.

Seven years later the idea was further extended to include virtually all social activity, in another famous work, Mancur Olson's *The Logic of Collective Action*. Olson argued that any form of collective action in large groups – including pressure groups, trades unions, elections, and the state itself – faced the problem that voluntary individual participation is irrational. Some form of coercive mechanism, or set of inducements, would be necessary even if all the participants were rational and if all would enjoy appreciable benefits as a result of their collaboration. Olson was ostensibly neutral on the question of whether such mechanisms should be put in place (and he was also remarkably insouciant about the ease of doing so – for his arguments against the rationality of collaboration also apply in most cases to the construction of collaborative coercive institutions such as police forces). But he tended in his discussions of particular issues to stress the diminution in human freedom which any large-scale collaborative enterprise represented: for example, he argued that Hayek and the other defenders of the market had been wrong in supposing that the state ownership of industries was a restriction of economic liberty, since the real restriction on liberty came from the provision of state services such as defence and not from the state's intervention as a producer in a market. The same point was made with much greater force by James Buchanan, who had independently come to the same conclusions as Olson (each acknowledged this in the works which they published in 1965). Buchanan had already collaborated with Gordon Tullock on another famous book, *The Calculus of Consent* (1962), in which they had argued that only unanimity on important social measures legitimated them; Olson was however somewhat wary of the unanimity rule, on the reasonable grounds that it handed too much bargaining power to each individual in the group. It is also the case that a unanimity rule is in effect a coercive mechanism, as it obliges people to collaborate in ways that they might not freely choose, since they have (for example) to defer to the wishes of an outlying member of the group and cannot simply ignore him and construct the kind of enterprise which they might otherwise have created.

Olson's theory thus called into question the rationality of traditional, large-scale collective action. At more or less the same time the other great

https://doi.org/10.1017/S0003975605250208 Published online by Cambridge University Press

achievement of rational choice theory, Kenneth Arrow's so-called "Impossibility Theorem", called into question the possibility of finding any political programme (including any distribution of goods among a population) which could satisfy divergent interests and at the same time meet a set of rather weak liberal conditions, principally that the outcome should not be Pareto-inferior to an alternative, that it should preserve transitvity of preferences, that it should not be imposed dictatorially (i.e. one person's preferences should not determine the result irrespective of the other's preferences, nor should an outcome be imposed irrespective of *anyone*'s preferences), and that people's preferences between any two alternatives should not depend on the presence or absence of a third alternative. (This last condition among other things rules out interpersonal comparison of utilities and with it traditional Utilitarianism.)

To understand the force of Arrow's case, one must remember that by the 1930s both Left and Right shared a vision of social choice in which contentious judgements had been reduced to a minimum. In particular, both sides in the great struggle of that decade had taken on board the central claim of neo-classical economics that a perfectly competitive outcome in a market would be a Pareto optimum, and would therefore in some sense be agreed on by everyone, whatever their other values; "rational" planning then seemed to be social engineering designed (at least in the first instance) to bring about such an outcome. This process is seen for example in the UK in the transition from Pigou-style Welfare Economics to the New Welfare Economics of Kaldor and Hicks. The Left (both the advocates of central planning in the Western democracies such as Abba Lerner, and, in practice, the organisers of central planning in the Soviet Union) believed however that a centralised planning process could mimic the actions of a perfect market, and a real market would necessarily be corrupted by the monopolistic tendencies of modern capitalism; the Right (notably of course Hayek) argued that only an actual market could act as the appropriate calculating mechanism. It was these two views which Arrow's work undermined, since he showed that no such outcome could be compatible with what one would suppose were the natural principles of a rational democratic process. Arrow was clear that his target was as much Hayek as Kaldor or Lerner: the market was no better than any other mechanism at generating an outcome which met his conditions. Arrow's own proposed solution was in effect a kind of deliberative democracy: citizens should be encouraged to discuss their fundamental disagreements and work towards a unanimity which could be the basis for the allocative process, so that the problem of reconciling divergent interests disappeared, at least at some deep level. Though the issue with which Arrow was concerned was very different from that which Olson was to address, it could be said that in both cases the effect of their work was to suggest that traditional electoral democracy was a less rational way of solving social problems than had usually been assumed during the previous hundred years or so.

It is clear just from these two examples that the implications of this brand of political analysis for conventional political theory have been very considerable. This is so irrespective of whether one thinks that the arguments have actually been persuasive; I would myself say that Olson's arguments in the end are not, whereas Arrow's may be, though his ideas are also of more limited scope than people often suppose – Arrow's "Theorem" shows that something is impossible which not many people had seriously supposed was possible, whereas Olson (if correct) showed that one of our deepest assumptions about instrumental human action is mistaken. It is also clear that this movement badly needs a proper historical analysis: it is of great importance for our modern understanding of politics at the highest intellectual level that we know what the point of these changes in political theory were and the social circumstances in which they developed. On the whole the reminiscences of the people concerned are – no doubt intentionally – couched in the language of the natural scientist's memoir, in which their story is presented as one in which the barriers to a true understanding of the subject fell away, and this is not particularly helpful when it comes to our getting a decent historical understanding; the participants anyway may not have been fully conscious of what they were doing and how it fitted into the general political developments of their time.

So we must welcome Sonja Amadae's *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism*. For the first time, someone has tried to tell the story of how these changes occurred, with a particular focus on Arrow, though Amadae also considers in some detail *The Calculus of Consent* and the writings of William Riker, who was one of the first professional political scientists (i.e. not an economist) to utilise the new ideas. Amadae noticed that a surprising proportion of the principal players in her story at one point in their lives were connected with the famous (or, to those of us alert to the world in the 1960s, the infamous) RAND Corporation. The main exception is Riker, but even he had copious contacts with RAND and its personnel. The first part of Amadae's book is accordingly a gripping account of the history of RAND, and its growth from a military research establishment (Research AND Development), funded by the USAAF and the Douglas Aircraft Company, to a general centre for "rational" policy making of a quasi-mathematical kind. She herself summarises this part of her argument with the section heading "Virtually All Roads to Rational Choice Lead from RAND" (p. 75).

The association of these theorists with RAND leads Amadae to the subtitle of her book – that is, to the claim that rational choice theory's appearance was intimately bound up with the Cold War and the need to justify the kind of liberal capitalism which American policy makers saw as their bulwark against Soviet Communism. Clearly, at some level this must be correct; as I have said, Arrow's work in particular can most plausibly be seen as a response to the pre-war central planning debate. But it was intended to close down the debate as futile, and not to show that only the central planners were in error. One of the difficulties of writing the history of the Cold War was that the kind of capitalism which many Cold Warriors wanted to defend was not much like the intensely market-driven version which we associate with the post-Reagan years of supposed victory in the War. Amadae is well aware that Arrow was as hostile to Hayek, and the idea of market democracy, as he was to the central planners; but to make her case she has to stress instead (pp. 128-129) the fact that he was widely misunderstood, and that his basic assumptions were the kind of individualist axioms which also underpinned the market theorists. However, there is nothing historically odd about someone employed by RAND in the 1950s thinking of themselves as being in a broadly Left, social democratic tradition, and that seems to have been Arrow's own politics, as it patently is the politics of Arrow's principal successor in his field, Amartya Sen. Democratic socialism was as we all know recruited into the Cold War (e.g. *Encounter*), but it doesn't follow that the ideas of the democratic socialists were the *product* of the War.

Amadae's determination to link the emergence of rational choice theory to the Cold War also leads her to the most theoretically contentious claim in the book, that 1950s rational choice theory was not simply (as one might have supposed) the gradual expansion of the ideas of pre-War economists into a wider domain of social enquiry, but was something distinctively new. Her argument here turns on a distinction between the account of rational conduct in marginalism and that in rational choice theory. According to Amadae, marginalist economics supposed that rational conduct was instrumental, in the sense of selecting the most efficient means to a given end; rational choice theory, on the other hand, was simply interested in *consistent preferences*. But this distinction is far from clear, as what counts as the most "efficient" means to a given end is precisely that the overall allocation of the agent's resources (time, money, etc.) is such that there is no alternative allocation which he prefers. To choose an "inefficient" means over an "efficient" one is therefore to have inconsistent preferences. It is true that the first marginalists did not talk explicitly about consistency, but the terminology was introduced into economics in the 1930s, and moved straightforwardly from there into rational choice theory. (The first example of the new language which I know of in an English context is Felix Kaufmann's "On the Subject-Matter and Method of Economic Science" in *Economica* for 1933, though there may well be earlier cases; Kaufmann's approach influenced Robbins, but Kaufmann himself subsequently moved away from economics and into the general philosophy of science.) Moreover, it is obvious that Olson's version of rational choice theory was quite explicitly intended to be the transfer to politics of the fundamental assumptions of contemporary economics, as was Downs's theory. The point is that economics is itself a political theory, of an especially important kind, and it is a mistake to suppose that it is not until its terminology and theoretical apparatus finds its way into what we call political science that we can say a change in political theory has occurred. Amadae is forced by virtue of her commitment to the difference between marginalism and rational choice theory to play down the importance of Olson and Downs, but (judging at least by what my rational choice colleagues say) they have had more influence on the development of the theory in political science than even Arrow has. Moreover, just as in the 1930s modern economics had to some extent been neutral between Left and Right, so it was in the 1950s: the change in how we think about political life, and the scepticism about democracy, it could be said, dwarfed even the difference between socialism and capitalism.

However, even though Amadae has (I think) overstressed the importance of the Cold War in the origins of rational choice theory, and has not given due weight to the fact that many of its practitioners would describe themselves as on the Left, she has at least tried to produce an explanation of it which accords it proper recognition as a major shift in political theory. Everyone who wants to think about these issues is going to be in Amadae's debt: she has bravely opened up the whole subject, and has shown us all the questions which we will need to consider.

RICHARD TUCK