replaced now by contingency and/or by a multitude of possible contributing factors, among whom the former studied structural ones still count as valid. The under-determination of outcomes is emphasized, next to the complex constitution of human agency. Comparisons are rendered more difficult as we are not allowed to look upon groups, societies, nations etc. as coherent entities any more. Of course, it is still possible to compare. Clemens points to the fact that comparisons can to focus more on processes than on "entities" (p. 513), yet the whole analysis will have to be embedded in an extensive historical narrative. This is fine for historians in general, but more difficult for those social scientists that aim at some sort of model-building.

Finally, it should be stressed that this manifesto regards mainly the US historical sociologists. The situation in France, Great Britain, Germany, and the Netherlands (among others) is different. For example, Norbert Elias, Fernand Braudel, and E.P. Thompson figure only in a marginal way in the debates between the third and the second waves. Nevertheless, the contributions are valid too for non-US scholars, if only because they provide such an excellent overview of the state of the art within US historical sociology. But perhaps the largest accomplishment is the provocative introduction by Adams, Clemens and Orloff. After all, the social sciences and history only thrive through debates. In stirring up the discussion in a provocative way they stimulated the "rethinking" of this wonderful discipline: historical sociology. Let us accept the challenge and join the debate!

Marjolein 't Hart

Lawson, George. Negotiated Revolutions. The Czech Republic, South Africa and Chile. Ashgate, Aldershot 2005. xi, 272 pp. £47.50; DOI: 10.1017/S0020859007022869

George Lawson cannot get over it! He can't believe that the era of large-scale social revolutions has passed. It's hard to tell if this lament for the past stems from his own ideological blinders or those of such intellectual mentors as Eric Hobsbawm or Fred Holliday. Whatever the answer to this question, Lawson has produced a book that might have been interesting and innovative but instead is badly flawed.

It is not just that Lawson rejects what he calls Francis Fukuyama's "infamous" end of history thesis. Rather, the author has reinvented the definition of revolution to encompass just about any change of regimes or transition process. In doing so he has drained away any recognizable meaning from the term revolution itself. Most of us think of revolution as encompassing the American, French, Russian, Mexican, Chinese, Cuban, Iranian, and maybe a few other cases. But Lawson has now redefined the term to include the three case studies in this book: the Czech Republic (Czechoslovakia at the time), South Africa, and Chile. Chile? Yes, Chile in 1988–1990 as it replaced the dictator Augusto Pinochet with a more open and democratic regime. But whatever we think of the other two cases, Chile in this period can best be described as undertaking a pacted transition to democracy or perhaps a democratic restoration. It was definitely not a revolution by any acceptable definition of that term.

This is a very curious book. One does not know what to do with it. Is this one of the last gasps (yet another) of European academic Marxism or "structuralism", of ideological pleading disguised as serious scholarship? Why do so many European academics find the old, tired Marxian approach and categories still attractive, whereas Americans do not? Is it that Lawson is from Mars and this reviewer from Venus?

Or perhaps it is the disciplinary approach that Lawson brings to this study. The author spends an entire, highly self-conscious introductory chapter telling us that his approach is that of "historical sociology". At the end of this exercise the reader remains entirely uncertain what historical sociology is, or is not. It is definitely not Comte, Durkheim, or Weber, but something more radical than that. Then, the author weds his historical sociology to a very strained and elementary definition of international relations. This approach, so far as one can tell, only accomplishes the goal of enabling a scholar apparently trained as a sociologist to roam into the international relations field, but without mastering the field and its literature.

Entirely lacking in Lawson's analysis, however, is any comprehension of political science and its literature. Is he unaware of this literature? As a sociologist, does he believe that politics is only a dependent variable, subordinate to overarching social forces? One concludes that all three of Lawson's introductory chapters are really an exercise in self-justification; it's a familiar feature of today's sociological writing, enabling sociologists to talk about the IR and political science fields but doing so superficially and not knowing whereof they speak. There is abundant political science literature that would have helped Lawson both theoretically and in understanding his three cases; unfortunately he seems unaware of any of it.

Nor is it clear how Lawson carried out his research. It appears the book's analysis is based entirely on secondary sources; it provides decent, journalism-level accounts of his three cases but is quite unoriginal. In the preface, the author mentions visiting cybercafés in Chile, and in his chapter on Czechoslovakia he appears to have had one interview with a former Communist Party official. But does he speak the language of these countries? How long was he in each one? Did he visit and do research in any libraries or archives while there, or did he just sit in his cybercafé and watch the world go by? Whom did he interview, if anyone besides the one Czech? What methodology did he use? Did he practice participant observation? How did he go about doing his research? None of these questions are answered in this book. Nor has the publisher offered us a clue: nowhere in this book is there as much as a sentence about the author or his background.

Let us try to be fair to Mr. Lawson. The title of his book is "Negotiated Revolutions" – to my mind a contradiction in terms. An event is a revolution or it is a negotiated agreement or pact, but it is hard to be both at the same time; a negotiated settlement is definitely not a revolution. Lawson does set forth a schema of what he means by this fractured term; he talks about revolutionary situations, revolutionary events, and revolutionary outcomes. His definition of revolution is the "rapid, mass, forceful, sympathetic transformation of a society's principal institutions and organizations".

This is a problematic formulation which Lawson never resolves. Are his revolutions limited to the political sphere or, as in the classic formulations, are they accompanied, or perhaps preceded, by massive economic and social transformations as well? What is the driving force behind revolution – is it class change as in Marx or something else? What about the role of ideology? Are revolutions inevitably accompanied by violence or are they peaceful? Are they abrupt or can they be evolutionary, over a long period of time? These are all fundamental questions absolutely essential to any new theory of revolutions; yet in Lawson's formulation, they are either not answered or they are answered so broadly and ambiguously that almost any regime change would fall under his definition.

This could have been a good and interesting book. Lawson seems initially to have been on to a good idea. For there are a variety of transformations occurring in the world, including his own three case studies, that seem to fall short of revolution but are more than mere run-of-the-mill rotations among party elites. The author's "negotiated revolutions" might be a useful designation for one such of these. But there are many more varieties and possibilities, and Lawson's attempt to stuff them all into the straightjacket of a single category is too confining, unhelpful, and downright inaccurate.

Think of India's recent transformation to a more market-friendly economy, China's evolution away from Marxism-Leninism, the people's movements that brought a kind of democracy to Indonesia and the Philippines, Mexico's transition from one-party to multi-party democracy and from corporatism to free associability. Or, whatever we think of George W. Bush, how about "regime change" in Iraq and Afghanistan, Bulgaria's and Romania's entry into the EU, the orange revolution in Ukraine, or the rose one in Georgia? Do all of these fit the negotiated revolution category? The list could go on but the point is obvious: this single category is too broad and ambiguous to fit the wide variety of fundamental transformations under way in today's world.

What Mr Lawson should have done, I believe, is to have fashioned a spectrum, or perhaps several categories, to encompass the kinds of changes he has in mind by his single "negotiated revolutions" category. Because there are fundamental transformations under way in many countries that are not quite full-scale revolutions like the French or Russian ones, but are more than mere everyday electoral shifts. He could then, as it were, "string out" his cases along this spectrum, or, alternatively, utilize several categories to encompass the variety of cases available (even Lawson recognizes his Chile case does not fit his "negotiated revolution" category very well) and thus arrive at a new and interesting typology. A well-established political science literature has already done that with, for example, both the variety of authoritarian regimes in this world and the more recent variety of transitions to democracy. There is no reason Lawson could not have done something similar and very useful by disaggregating his fuzzy, unclear, too-broad "negotiated revolutions" category.

Why did Lawson go astray? I can surmise four possible explanations. First, his instructors may have led him afield by force-feeding him a diet of old-fashioned, inappropriate, often romanticized, literature on revolutions that no longer seems very useful. Second, he appears to be a prisoner of his discipline, sociology, which cannot accept the autonomy of political variables and, when it does cross over into the political field, seems superficial and about at a high school civics ("good government") level. A third conjecture: Europeans and Americans are so far apart on this as well as so many other issues that there is no longer any meeting of the minds. And fourth, the author seems unaware of vast bodies of international relations, foreign policy, comparative politics, and general political science literature that would have been very helpful in advancing his research.

This could have been an interesting and path-breaking book; unfortunately it falls considerably short of that level.

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