

**Gerhard Lenski, Some False Oppositions, and *The Religious Factor***

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Isaiah Berlin famously declared that there are two kinds of intellectuals: hedgehogs and foxes. The former have one big idea, stick to it throughout their careers, and find it significant for all issues.<sup>1</sup> The latter flit from idea to idea, theme to theme, glorying more in diversity and range than in depth or perseverance. The opposition is illustrative but Gerry Lenski proves it too simple.<sup>2</sup>

Lenski wrote three great books on three different important subjects: religion, stratification, and social evolution. There is indeed a unifying theme, since all are concerned in significant part with the social organization and implications of inequality. But only one has that as its primary subject. *The Religious Factor* may say more about

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<sup>1</sup> 'The fox knows many things, but the hedgehog knows one big thing'. The line comes from the Greek poet Archilochus; see Berlin (1953). Berlin originally entitled his essay "Leo Tolstoy's Historical Skepticism," but George Weidenfeld suggested the new and vastly more evocative title. "For there exists a great chasm between those, on one side, who relate everything to a single central vision, one system less or more coherent or articulate, in terms of which they understand, think and feel—a single, universal, organizing principle in terms of which alone all that they are and say has significance—and, on the other side, those who pursue many ends, often unrelated and even contradictory, connected, if at all, only in some *de facto* way, for some psychological or physiological cause, related by no moral or aesthetic principle; these last lead lives, perform acts, and entertain ideas that are centrifugal rather than centripetal, their thought is scattered or diffused, moving on many levels, seizing upon the essence of a vast variety of experiences and objects for what they are in themselves, without consciously or unconsciously, seeking to fit them into, or exclude them from, any one unchanging, all-embracing, sometimes self-contradictory and incomplete, at times fanatical, unitary inner vision. The first kind of intellectual and artistic personality belongs to the hedgehogs, the second to the foxes; and without insisting on a rigid classification, we may, without too much fear of contradiction, say that, in this sense, Dante belongs to the first category, Shakespeare to the second; Plato, Lucretius, Pascal, Hegel, Dostoevsky, Nietzsche, Ibsen, Proust are, in varying degrees, hedgehogs; Herodotus, Aristotle, Montaigne, Erasmus, Molière, Goethe, Pushkin, Balzac, Joyce are foxes."

<sup>2</sup> "Of course, like all over-simple classifications of this type, the dichotomy becomes, if pressed, artificial, scholastic, and ultimately absurd. But if it is not an aid to serious criticism, neither should it be rejected as being merely superficial or frivolous; like all distinctions which embody any degree of truth, it offers a point of view from which to look and compare, a starting-point for genuine investigation." Berlin used the phrase, in fact, precisely to point out how hard it is to classify Tolstoy, partly because Tolstoy himself recognized the limits of each approach and sought to present himself as embodying both.

stratification than most books on religion, but it is also about family life, politics, the relation of religion to science and the implications of different religious teachings and communities for social integration. Likewise, *Power and Privilege* mentions evolution and suggests the importance of technology but it is not about either. It is about who gets what and why, and explores this through analysis of different kinds of social system. It touches on religion, partly as exemplary of status group inequality. *Human Societies* has roots in both the empirical, data-driven approach of *The Religious Factor* and the concern for an overall explanation of inequality of *Power and Privilege* but it breaks substantial new ground. The picture would be even more diverse if we considered the range of Lenski's articles, but its elements would still be more connected than scattered. In short, Lenski is a good compromise between hedgehog and fox, and a living demonstration that there need not always be a forced choice between breadth and depth, creativity and scholarly persistence.

A second false dichotomy, even more familiar, opposes teaching to research (and has a corollary opposing textbooks to serious scholarship). This dichotomy is not only false but dangerous for the future of our discipline and indeed the university. It has its grains of truth, of course, notably in the fact that with only so many hours in the day professors do have to make choices between attending to students and attending to their own scholarly pursuits, and also in the gap between the reading level undergraduates generally tolerate and that which serious scholarship requires.<sup>3</sup> But here again, Lenski's work shows why we should not simply accept the opposition as our fate.

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<sup>3</sup> A fuller examination of this point would consider whether it is indeed the fact that students will tolerate much more difficult reading (and other course requirements) in other classes—say, economics, not to reach too far afield. How much this is due—if it is so—to students' intuitions of what is important, or has payoff, or to the way in which sociologists themselves have positioned their courses might also be studied.

Each of Lenski's three major works is a landmark in its field, and each was published for a largely non-academic readership as either a tradebook or a textbook. *The Religious Factor* was published by Doubleday in 1961 and sold widely; *Power and Privilege* (originally 1966) and *Human Societies* (first edition 1970) were both published as textbooks by McGraw-Hill—and not merely published as textbooks but used in a range of different institutions.<sup>4</sup> *Human Societies* is, indeed, perhaps the most intellectually serious of all American introductory sociology texts—and one of the most commercially successful of all major works of sociological theory. The moral of the story is that sociology need not pander. Complex ideas can be presented clearly, and that students—and indeed, the general public—can be engaged in serious research and theory. This is not only a matter of clear writing, sentence by sentence, but of the clarity gained by a carefully worked out and well-integrated overall approach (by contrast to the usual grab-bag miscellany of introductory sociology texts).<sup>5</sup> The same good qualities, I might add, extended to Lenski's own successful teaching at both undergraduate and graduate levels.

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<sup>4</sup> These books were not only aimed at but found wide readerships. One effect was that each appeared in multiple editions, challenging the academic convention of references by date. Herein, I cite the original editions of *The Religious Factor* and *Power and Privilege*, and, somewhat arbitrarily, the fifth edition of *Human Societies*. At least eight editions of the latter have been published, though, and as he has taken over the main responsibilities for revision Patrick Nolan has become the lead author. The book was originally published with only Gerhard Lenski as author, then for several editions with Jean Lenski as co-author in recognition of her work on revisions. I mention these publishing arcana partly because the history of sociology is often written as though the relevant context for grasping a work is that of its publication, neither the long history of its gestation nor the subsequent years of its influence, and because individuals are often represented as simply taking positions identifiable by their names—the Lenski view—rather than formulating and reformulating their arguments in a pattern of (perhaps evolutionary) change. Using Lenski's 1987 edition is appropriate because it reports on the version of his theory presented during the period of the arguments discussed in the text.

<sup>5</sup> I know whereof I speak because I am also the author of a McGraw-Hill introductory sociology textbook, perhaps of the lesser sort that tries to present a range of perspectives more than develop its own, and indeed I am that because Gerry Lenski recommended me.

Yet another false dichotomy separates theory and research. It suggests that there are theorists who master the arcana of a past tradition, critical reason, and the ability to synthesize. And then there are researchers who collect data, worry over methods, and stick close to the empirical ground. And once again, Gerry Lenski confounds the opposition. *The Religious Factor* is based on survey research and is indeed among the most important products of the distinguished Detroit Area Study tradition. Yet it is also a book rooted in Weber, Durkheim, and Tönnies, and a book by the same author as the intensely synthetic and much more theoretical *Power and Privilege* and *Human Societies*, and the coiner of such classic concepts as “status crystallization” (Lenski 1954, 1956; see also Smith 1996). In fact, Lenski’s approximate generation included a number of sociologists who brilliantly combined theoretical and empirical agendas: Peter Blau, James Coleman, Seymour Martin Lipset, Robert Bellah, Arthur Stinchcombe and Neil Smelser and only several of the best known. It is worth asking whether we would say this so readily of later generations, and if not, whether this may indicate a pernicious rather than beneficial form of specialization.

Not all dichotomies are false, and sociology revels in pairs of opposed concepts. From pattern variables to such grand oppositions as traditional and modern to a penchant for two-by-two tables that is nearly definitive of the discipline we were binary before digitalization made it fashionable. The binary pairs are often useful ways of focusing attention, and certainly have been useful pedagogical tools. But if presenting them is a disciplinary habit, so are deconstructing them, declaring them false, and struggling to overcome them. Let us move beyond structure and action, beyond self and society,

beyond subjective and objective, we recurrently declare (though we often find it hard to follow through).

One of the many features to appreciate in the work of Gerhard Lenski is that he did not work by binary oppositions. To be sure, Lenski's greatest book (Lenski 1966) identified two dimensions of stratification: power and privilege. But far from opposing these, he studied their interrelationship. Much of the book is organized by the reference to two "laws of distribution". One relates need to power, focusing on the material basis to social organization. The second relates power to privilege, focusing on the ways in which power shapes distribution—and the more limited extent to which it sometimes does not, giving way to altruism. Lenski's analysis is unfolded through a typology of societies much richer than traditional vs. modern and with an acknowledgement that systems of classification are not satisfying analyses in themselves but ways of presenting patterns which then may stimulate theoretical development (Lenski and Lenski 1987: 78).<sup>6</sup> Likewise, throughout his work on social evolution, Lenski avoided simple dichotomies of ideal and material, addressing for example the interplay of technology and ideology as parts of the "informational base" of industrial society. Lenski's theory was avowedly "materialist" but it was a theory of sociocultural evolution, not of the mere derivation of culture or social relations from material factors.

Lenski's work reveals the limits to some common oppositions by which we try to position scholars. It also offers occasion to reflect on the more 'real' but I think inadequately conceptualized and poorly worked through opposition of two generations of

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<sup>6</sup> The roots of "progressive" stage models of historical change go back at least to Lord Kames four-stage model (1778), and have commonly been much more complex than simple tradition vs modernity. The latter contrast centered on an orientation to continuity vs. an orientation to change, but has often been simplistically deployed as the current West vs the rest as well as the West's own history.

sociologists—roughly, Lenski’s and my own, that for which World War II was the formative experience of youth and that for which the 1960s was pivotal. I will not analyze this in depth, but only evoke some of the issues, and also the ways in which such generational oppositions can shape the content of sociology itself suggesting a dialectical pattern rather than any simple accumulation of knowledge (even if the dialectic has no Hegelian guarantee of progress). I shall pay special attention to comparative historical macrosociology. Finally, I will look in a related light at Lenski’s first book, *The Religious Factor*, noting both its distance from and nearness to contemporary sociological concerns.

### **A Generational Opposition.**

For some fifteen years, Gerry Lenski occupied the office next door to my own at the University of North Carolina. Though we saw each other almost daily, we were also separated, not by space or even sociological interests, but in an important sense, by generation. The late 1960s stood between us, the rise of Marxism, quarrels over approaches to research. This did not give rise to any antagonism between us but made for a difference of style that I note because versions of the same distance have been influential throughout sociology, often to the discipline’s loss.<sup>7</sup>

This generational divide skewed perceptions, shaped chains of intellectual influence, and imposed divisions among approaches. For example, the reinvention of comparative historical sociology in the 1970s and 80s can only be grasped against this backdrop. To be sure, some of the leaders in this—notably Charles Tilly—are closer to

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<sup>7</sup> Despite the distance and differences of style, Gerry Lenski was among the senior colleagues I most admired. I probably would not have gone to Chapel Hill were he not there, and I am indebted to him for numerous kindnesses. On this occasion, both the generational distance and the personal connection add to the pleasure of recognizing Gerry’s extraordinary range of accomplishments and contributions.

Lenski's age than to my own. But Tilly is an exception (and note his own chagrin at being placed among the elders in the conference that led to Theda Skocpol's *Vision and Method in Historical Sociology*; see Skocpol 1984: xi). For the most part, the renewal of comparative historical sociology, and especially its positioning as a distinct branch of the discipline, was the product of a generation marked by radical engagements with social order and change in the 1960s and by equally fierce (if not always equally radical) challenges to the dominant elites of sociology itself.<sup>8</sup> Central to the self-definition of the field was its hostility to "modernization" as an intellectual paradigm. Since Lenski was no simple modernization theorist, this might not have produced much distance from his work, but in fact the idea of social evolution became suspect partly because of its association with modernization (and with such vices—in other hands—as writing history in ways that seemed to justify contemporary power relations).

Historical sociology (the core of the new "comparative historical" subfield) was organized significantly in opposition to evolutionary theory. In effect, these were two alternative approaches to social change. Many of the new generation of historical sociologists rejected both the idea that there could be any single theory of the entire pattern of historical change and the idea that the patterns that existed could be described in terms of evolution. The rejection was not only ideological. It turned for some on resistance to the reduction of complexity—and historical specificity—many felt the evolutionary models produced. It turned for many on rejection of what seemed to be the

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<sup>8</sup> Two anthologies assessing historical sociology will appear shortly. Both have much to say about the field, but it is instructive that one edited (and mainly written) by Americans focuses significantly on the generational character of the field—and the idea of its rebirth after the 1960s—while the other (more British in provenance) assimilates historical sociology more directly to the classical tradition of sociological theory, with less attention to problems of generational succession, partly because these did not figure so much in the British context. See Adams, Clemens and Orloff (forthcoming) and Delanty and Isin (forthcoming). See also Calhoun (1997),

unilinear character of most evolutionary models (an issue Lenski wrestled with explicitly in *Human Societies*, recognizing important variations among industrial societies, for example, while ultimately arguing that convergence was the dominant trend). And it turned also on the question of just what the unit of “evolutionary” change (or selection) might be.

Many among the newer historical sociologists argued that the idea of relatively discrete societies was itself problematic, and that building a theory of social change around it meant underestimating the complex relationships that crossed the frontiers of such societies (Tilly 1984 summed up a decade of arguments on this point). Analyses of unequal development contributed importantly to this line of thought, emphasizing the extent to which attempts to explain social forms by intrasocietal factors neglected the impact of position in a larger structure of relations, or, as Immanuel Wallerstein (only a little younger than Lenski) put it, a world-system (see Wallerstein 1974). It also tended to read the idea of the nation-state back into earlier social formations, embedding it in the very notion of “a” society (Calhoun 1999). Such considerations not only raised questions about the unit of analysis in social evolution, but about the idea of “macrosociology”.

On the one hand, Lenski tended to retain the notion of “a” whole society; his evolutionary model addressed intersocietal selection (as well as intrasocietal development, change, and sometimes decline). Lenski relied more on ideas of group selection than do most modern evolutionary biologists (see Haines 1997). On the other hand, over the successive iterations of *Human Societies* he increasingly recognized the evolution of a world system that connected societies. He treated this as a world system of societies, and it is worth comparing this to Wallerstein’s treatment of a world system of



states (increasingly, nation-states rather than empires). The difference is significant, but so too the suggestion of the extent to which state structures define what counts as a society in Lenski's as in many other models. The most "macro" picture in Lenski's analysis, thus, is of a population of societies in which those "that have grown in size, and developed in complexity, wealth, and power, have been much more likely to survive and transmit their cultures and institutional patterns than societies that have preserved traditional social and cultural patterns and minimized innovation" (Lenski and Lenski 1987: 73). The emergence of an increasingly integrated global structure is thus a result of an evolutionary process in which the number of societies has been reduced and the features of more powerful ones extended to the rest in varying degree. Critics of modernization theory, focused especially on its Parsonsian variants, had tended to assume that evolutionary models presented an account of "progress" rather than of power relations. Lenski's theory, however, stressed power precisely as part of the evolutionary process, distributed very unevenly, but so distributed for reasons that could be explained by historical change rather than only decried as unjust.

In short, false oppositions were drawn between evolutionary models and accounts of unequal power, and between theories of "modernization" and historical sociology. Lenski did not shake off the unilinear assumptions of modernization accounts—he continued to treat many features of unequal development as matters of a prolonged "transitional phase" affecting some societies' industrialization (Lenski and Lenski 1987: 361). I do not think his theory did enough to distinguish that which is necessary in human history from that which is produced by choice (and for which there is thus a different kind of political and moral responsibility, and for which a more critical theory may be

required). Despite this, it should be clear that in the broad sense in which comparative historical sociology eventually came to be institutionalized in the discipline, Lenski was clearly a leading practitioner. And it should be clear that debate between the polarized extremes of Parsonsian functionalism and the new historical approaches tended to obscure the possibility of better explanatory connections in the middle ground. Not only Lenski, but a variety of others, fell out of the historical self-understanding of the new comparative historical sociologists because of the generational opposition. This resulted in an impoverishment of the field. Not least, it reduced the range of possible intellectual resources from which to build approaches that sought to do justice both to explanation and to interpretation, to broad pattern and to particularity. In the next generation, especially outside of sociology (not least in history itself), the old *methodenstreit* oppositions of particularistic and generalizing investigations would return in force as some pushed the attack on universalistic theory further and further into a denial of all explanatory projects, which others sought to reconstruct universalistic analyses on the basis of rational choice theory and strategic individualism (see the discussion from various vantage points in Gould forthcoming).

The loss was not only at the level of broad theoretical (or atheoretical) approaches. One of the most important empirical points Lenski made in *Power and Privilege* (and developed further in *Human Societies*) was not fully absorbed: the centrality of agrarian societies to human history and thus to any adequate theory of social life and historical social change. Comparative historical sociology—proud of bringing history back in and escaping ethnocentrism—has focused overwhelmingly on the modern

era, on industrializing or industrial societies, and indeed, on the West.<sup>9</sup> Ironically, whatever its distortions, modernization theory engaged the rest of the world and the range of societal forms more substantially than most of the new comparative historical sociology (the major exception to this being work in the world systems and dependency perspectives).

Along with evolution, functionalism became a term of opprobrium for a generation. Lenski was no Parsonsian, but he explicitly and continuously embraced functional analysis. Among other reasons, he saw in this an important continuity between biology and sociology which ought not to be jettisoned. It was also basic to his idea of what constituted social life, the complex interdependency of social groups that enables individuals as well as the groups themselves to survive. Lenski's commitment to the idea of a "macrosociology," may have reflected his strong engagement with the notion of 'system', including an interest in cybernetics and in seeing commonalities in different forms of encoding information as instructions to achieve pattern in various processes, from the cellular to the social. Here he shared a good deal with Niklas Luhmann (e.g., 1984), though neither much engaged or influenced the other. Lenski's style was, in fact, much more historically particular, and his evolutionary theory attended much more to differences in internal organization and in the sources and patterns of change in different kinds of societies than did Luhmann's theory which was (for the most part) offered at a

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<sup>9</sup> This said, it should be noted that at least many of the newer historical sociologists tried to be theoretically specific about distinctions like that between capitalism and industrialism, suggesting that modernization theory made the wrong category fundamental. And if the new wave of comparative historical work grew heavily out of Western European foci, it has certainly extended to a range of other settings and done so with more reflexive attention to the implications of Western dominance for the constitution of other social forms than was characteristic of modernization theory. But, it remains striking that historical sociology is as focused on Europe and the US as it is, and has been as little influenced by perspectives rooted in other contexts—like subaltern studies—as it has been.

higher level of abstraction and generality. Lenski's sociology was more directly historical and comparative.

Certainly, at a personal level and in his teaching (and textbook) Lenski sought to push sociologists to pay more attention to the rich variety of social life. He insistently reminded readers (and listeners) of the limits of generalizations based on only a single type of society (let alone a single society), taken out of its comparative (and evolutionary) context.

### **The Religious Factor**

As I suggested, Gerry Lenski reveals the faults of many false oppositions. Perhaps more importantly, in the best traditions of empirical sociology, he preferred to make them into variables. One of the most important examples of this is his use of Tönnies' typology of community and association. In *The Religious Factor*, Lenski sought to operationalize each as a dimension of religion and to see how they interacted and differed in his data. It is still the case, forty years later, that the distinction is inadequately appreciated. The difference between gauging the extent of commitment to a formal association, its purposes or its doctrines, and gauging the embeddedness of individuals in communities of co-religionists is basic. It speaks, for example, to two different dimensions of the rise of Evangelical Christianity in recent decades and what it means to its adherents and to American society. One of the important points made by Lenski in 1961 is that the two dimensions don't have to covary (they did in his study, but weakly), and that a high measure of one doesn't imply any specific content or extent of the other. There is no reason that megachurches that offer some version of all-encompassing community to their members need to be Evangelical, for example, or to hold the same doctrines.

To read *The Religious Factor* today does contain its methodological shocks. It is a major analysis carried out almost entirely in cross-sectional terms, with percentages, correlation coefficients, and tests of significance its statistical armament. It is, in other words, a reminder that it was only in the 1960s that sociologists began routinely to use multivariate analytical methods. Yet one is also impressed with the care Lenski and his colleagues took to try to use controls and ask questions about the robustness of their data or interpretations. It is a shock of a different kind to realize that one of the books innovations was to distinguish Black from White Protestants—which not only yielded important information about racial difference but revealed sharper contrasts among white Christians than had previous studies which failed to differentiate by race—including even a major one by the redoubtable methodologist Samuel Stouffer. As it happens, some of Lenski’s findings on race and religion prefigure much more recent “discoveries” of the importance of conservative religious doctrine among Black Americans, including those who seemed “liberal” when asked economic questions. And one of the contributions *The Religious Factor* could have made, had more learned from it, would have been to complicate the typical omnibus usage of terms like “liberal” which Lenski showed to be poor packaging of a diverse range of attitudes (whether with regard to religion or to politics and other secular matters).

Let me say a little more about *The Religious Factor*, the least famous of Lenski’s three major books. At the outset, Lenski writes: “Religion is far too important a matter to be ignored or dealt with superficially, as has been the custom among American social scientists for a generation or more” (Lenski 1961: vii). There was an increase in attention to religion in the 1960s, with the work of Robert Bellah perhaps most prominent. Though

there continued to be important work on religion published afterward, it was for the most part pushed to the margins of the discipline. In the 1980s, Robert Wuthnow did as much as anyone to revitalize the field, with major works like *The Restructuring of American Religion* (1988). And more recently there has been a renewal of work on several themes, including perhaps most prominently new work on rational choice and religious adherence. But unfortunately, Lenski's early statement remains true forty years after it was written. Sociology (and social science more generally) has had a hard time keeping religion in the center of its attention. When there has been major work on religion, moreover, in two important senses it has generally not followed up important aspects of Lenski's study.

First, many studies focus their attention not on religion as such but various aspects or dimensions of religious life. Beliefs, or social movements, or organizational structures all get attention, which is fine, but different from studying religion as a basic institutional dimension of contemporary society. Part of the issue here is that with the widespread move away from functionalism the idea of organizing sociological work in terms of big institutional domains like family, education, religion, and focusing attention on their inter-relations went into decline. This conceptual approach did not require a strictly functionalist theory—Lenski's work exemplifies this—but functionalism was the most important theory to underwrite the general approach. In any event, in a good deal of work there was a sort of decomposition and decontextualization of religion as an object of attention. Harrison White's (1970) study of career structures in the ministry was brilliantly innovative, but it was no more a sociology of religion than Peter Blau's (1955) *Dynamics of Bureaucracy* was a sociology of social welfare agencies. In anthropology,

where the study of religion had been more central to the discipline, something of the same thing happened as the study of religion was decomposed into myth, ritual, and symbol on the one hand, and shifted out of the context of institutional analysis on the other—as for example there might be studies of “new religious movements as protests”. The central organizing theme that was lost was not just religion, but religion as a central part of the analysis of social integration. And indeed, such integration is a central concern of Lenski’s *The Religious Factor*.

Second, the very name of Lenski’s book suggests another potential path for analysis that has not been followed as much as one might wish. This is the study of the influence of religion on secular institutions, behaviors, and relationships. There have been many more studies of religion as such—of churches, denominations, Jewish revival or Christian sects--than there have been worthy successors to Lenski’s concern for how religion figured in the whole of social life—the “religious factor”. The continuing, ever ebbing and flowing debates over the extent (or absence) of “secularization” have focused overwhelmingly on the extent of religious belief and practice, not on the impact of religion in the rest of social life. Indeed, in some ways the word secularization has changed its meaning. It is often used to refer to irreligion, to the “other” of religion, the not religious parts of the world which have putatively expanded their scope. But this misses a crucial dimension of the history of secularization. This is the history of growing religious involvement in secular—this worldly—life. This is what was at the heart of Weber’s interest in the Protestant ethic. It is a story that goes back to antiquity, but takes on a special form in modernity. Lenski’s *The Religious Factor* rightly points our attention to the study of how religion enters the secular world as distinct from whether it simply

gives way before it. Lenski did not predict growing religious influence in every sense, though he wrote in a period in which he and others detected a revival. Rather he saw a possible “drift towards compartmentalization” that would couple an increase in religious communalism (more at the expense of ethnicity than turned out to be the case) with a reduced sense of responsibility for those outside their group. People might not be more religious, but religion would matter more for organizing people’s integration into groups.

At one level, Lenski is clearly wrong on this. He writes, “There seems little doubt that socio-religious groups are rapidly replacing ethnic groups as the basic units in the system of status groups in American society” (363). Not yet. However, we should not be too hasty. In important ways, the growth of the so-called new religious right in recent years fits Lenski’s model very well. In many ways, it goes farther than he imagined. Political and more broadly social activism joins the actively religious not just from different Protestant denominations but also across the Protestant/Catholic divide and with increasing frequency bring Jews and Christians together in alliances if not religious communities. Denomination matters less, but membership in the broad network of Evangelicals works in many ways as Lenski suggested. It provides a primary status group identity for members. It binds them communally to each other. And, at least in some cases, it reduces their concern for the rest of society (witness the extent to which “missions” work is redefined as service to fellow communicants).

There is no easy single pattern to identify. That not all of Lenski’s generalizations and predictions have held up only shows him to be a genuine empirical scientist. And of course, there are a variety of specific findings in *The Religious Factor* that are interesting in themselves, sometimes as historical data, sometimes as propositions worthy of being



tested today. I can't resist mentioning one, in passing. Over half of the respondents to the 1958 Detroit Area Study—a little over 55%--indicated that “if a workable plan could be developed for a single government for the whole world” they would like to see the United States join it (158). I won't try to connect this to the current stand of the Bush administration on the International Criminal Court or its invasion of Iraq (or make the sort of political statement that sometimes drove a wedge between Lenski's generation and mine—though I doubt we disagree on this issue).

Let me close, though, with another connection. One of the themes that *The Religious Factor* takes up importantly is the intertwining of religious life with immigration—both within and to the United States. Lenski didn't pretend to offer the last word on this; indeed, he went out of his way to indicate that much more needed to be learned. But it is interesting how from the vantage point of 1961 it looked like the story of immigration was mainly a story of Americanization, of absorption into an established pattern rather than its deep transformation. Of course, 1961 was well in advance of the enthusiasm for ethnicity and difference that would distinguish the era of *The Religious Factor* from our own. A reviewer pushed Gerry on the question of ethnic differences among American Catholics. Gerry acknowledged that these were significant (and in the original book had stressed especially the overrepresentation of the Irish among the clergy). But in the preface to the revised edition, he wrote, “...at the present time there are clearly differences within the Catholic group linked with ethnicity. I have not explored these in great detail because it seems clear that with increasing intermarriage across ethnic lines, these differences will disappear and a common American Catholic subculture will emerge” (1961: x). Yes and no. In recent years a new wave of

immigration has wrought considerable transformations on American religion. American Catholicism is increasingly Hispanic in its membership if not its leadership. And the effects don't stop there but extend to rapid growth of Evangelical Protestantism among Asians, to the appearance of Buddhism as a significant American religion and, perhaps most remarked, to the rise of Islam to the status of one of the larger faiths in the United States. This phenomenon is just beginning to receive the kind of attention Lenski called for (see Alba and Raboteau forthcoming and specifically on Islam, Leonard forthcoming). Indeed, the Detroit Area Study for 2003 focuses on Muslims in Detroit, under the direction of Wayne Baker, some forty-five years after that which produced *The Religious Factor*.

*The Religious Factor* stands the test of time not simply as a bundle of empirical findings, but as a demonstration of the importance of studying the way religion works in all of social life. It may, in fact, be the least of Lenski's three major works. But that only points to the enormity of his other achievements, on which our other contributors to this symposium focus.

## **Conclusion**

Sociology, in common with most sciences, develops by fits and starts, steps forward and back, detours and sidetracks, at least as much as simple cumulative growth in knowledge. One of the ways in which improvements in our intellectual understandings advance is by the tendency of each generation to grow frustrated with the intellectual orientations adopted by that of its parents' and to try either to overthrow or at least to counterbalance these with sharply divergent arguments. This produces some of the energy behind research and theory formation. It also provides one of the mechanisms by

which sociology is continually tested against lived experience and changes in the social world—including what seems problematic and what seems obvious in the social world. But it can also obscure a good deal.

The lines of generational contest join with many other ways in which sociologists too sharply state the divergence of intellectual perspectives. We habitually overstate contrasts between theories, and sometimes even between empirical analyses. This is understandable as collective culture or as strategic career-making, but it is often a source of intellectual weakness. It means that potential complementarity is missed, that protagonists in the development of new analyses draw on less rich ranges of resources than they might, and that there are too often more rewards for simplistic clashes than subtly more complex views of the world. Gerhard Lenski was not, himself, much given to binary oppositions and even less to polemics. Nonetheless, his work has been categorized not just in terms of its own merits but in terms of various overdrawn debates. This is a pity, because it has much to offer those who may diverge from his style or disagree with some of his findings. And of course, it is also a pity because there is much to agree with and tools we could use to build a stronger sociology.

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