Chapter 38
Gerontology with a "J": Personal Reflections on Theory-Building in the Sociology of Aging

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Geh-Ron-Tology or Jeh-Ron-Tology?

My introduction to the field of gerontology was not auspicious. I was sitting in the office of Bernice Neugarten at the University of Chicago, scared silly. I was a senior in college applying to graduate schools and Dr. Neugarten (I still can't bring myself to call her "Bernice") was interrogating me. Why should I, a philosophy major from a very small and undistinguished college, feel myself qualified to join the Human Development and Aging program at the great University of Chicago? "Well," I began, "I think I would be interested in *Geh*-ren-tology, pronouncing the 'G' as in 'guest.'" "*NO!*," she shouted. "It is pronounced *Jeh*-ren-tology!" This was the first time I had heard anyone *say* the word. The study of aging was very new in 1963.

Successful Aging: Activity or Disengagement or Both?

Theories of aging have a very short history compared to theories in other areas of science. I arrived at Chicago after the start of the first big theoretical controversy in what later came to be called the sociology of aging. In the previous decade, Robert Havighurst and his students had collected survey data that linked "successful aging," indicated by high life satisfaction scores, to the maintenance of high levels of activity and social engagement (Havighurst and Albrecht 1953). This came to be known as the "activity theory" of aging. It became immediately popular with gerontological practitioners working with the aged, because it justified what they had been doing to keep elderly patients active.

Then to Chicago came two young and ambitious researchers, Elaine Cumming and Bill Henry, who proposed exactly the opposite in their "disengagement theory of aging" (Cumming and Henry 1961). This theory boldly proposed that withdrawal was the normal pattern of aging, and that both the individual and the society benefited when aging persons disengaged from roles and activities in which they had previously been engaged. Such disengagement is universal as well as functional: satisfying for the aging individual, because it enhances life satisfaction by freeing him/her from increasingly unrealistic demands of mid-life and useful for the social system, because it helps the group maintain equilibrium while preparing younger members to fit into the retiree's position. However, disengagement theory very quickly became unpopular (Achenbaum and Bengtson 1994). The advocates of "activity theory" felt disengagement theory was overgeneralized and deterministic

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(Rose 1964). Furthermore gerontological practitioners, therapists, and nursing home administrators were alarmed because it undercut activity-based therapies. And then there was the sense that advocating disengagement from productive activities was, in a vague sort of way, un-American.

Into this theoretical fray I sailed with my dissertation research, an attempt to test (and, I thought, to refute) the postulate of "universality" in the theory of disengagement. The data, from middle-aged and retired steel workers and teachers in several nations, did not provide any support for the majority of disengagement theory's postulates. Nor did the data support activity theory. In fact, high life satisfaction varied wildly with social engagement across the two occupational groups and the eight national settings involved in the study, in no consistent "pattern of aging" (Bengtson 1969). While the retired Americans showed a high correlation between activity and life satisfaction in ways supporting activity theory, the Dutch and the Italians showed low activity and high satisfaction – supporting disengagement theory. Later, in my first research project following graduate school I developed a more systematic test of activity theory (Lemon et al. 1972). The data provided little support for the axiom that high activity produces greater well-being in aging. There was just too much variation between individuals, income groups, or men and women to support such a sweeping, one-size-fits-all theory.

I learned some important lessons about theory from my first attempts to grapple with explanations of aging, lessons I have tried to pass on to my students. The first was that a good theory must account for variability as well as for modal patterns. In aging there are not only individual differences but social and structural differences that must be taken into account by the theory. Second, a theory is in danger if it attempts to explain too much, if it over-generalizes. A one-size-fits-all theory of aging will not last very long. A third lesson (I thought) was that a theory not supported by data will wither and die. A theory must be testable and when data show the theory to be disconfirmed we must modify the theory or develop a new one.

By these three criteria, the activity theory of aging should now be encountered only in the footnotes of gerontology textbooks. But still, to this date, activity theory lives on, 60 years later, as the implicit theoretical base for a recent best-seller, *Successful Aging* (Rowe and Kahn 1998), touted as the prescription for a good old age. The activity-is-good formula is presented (sadly) without any indication that the authors were aware of the earlier incarnation of activity theory, or that they were conscious of the individual variations and social structural constraints that critics noted must be taken into account, or that they were aware of the *lack of empirical evidence to support activity theory* (Schmeeckle and Bengtson 1999). I had thought that such one-size-fits-all, grand theories of "successful aging" that ignore diversity, personality preferences, or structural inequalities had been put to rest a half-century ago. It seems to me that theoretical indifference has been one of the greatest limitations to the development of cumulative knowledge about aging.

The Generation Gap: Family Solidarity and the Generational Stake Theory

My first job after Chicago was with James Birren at his new Institute on Aging at the University of Southern California. I had also been recruited by John Clausen (with Glen Elder) at the University of California Berkeley, but USC got me by offering a magnificent $500 per year more than Berkeley. Those were frugal years. They were also the years of protest, with the Civil Rights Movement and the Student Rights Movement and the Anti-Vietnam War Movement and the Counter-Culture Movement each gaining more publicity month by month. It appeared that the "generation gap" was wide and deeply rooted in radically different political and economic values between youth and their elders. I saw it differently. In a review of the literature on generational differences I argued that generational contrasts were more apparent than real (Bengtson 1970), and that there were far greater differences *within* generations than *between* them. This was brought home to me 40 years later

during the heated 2010 elections with the "Tea Party" protesters, a political movement led by mostly 60ish activists. It struck me that these people are of the same "generation" that produced the Anti-War and Students Rights protests of the 1960s and 1970s. They are the *other* members of this generation, the group in their age cohort that *did not* protest back then.

I began a study to test the extent of generational differences with a study of 2,000 grandparents, parents, and young adults that would examine values, attitudes, and relationships across generational lines. I had not the foggiest notion that this study would go on for another 35 years as the Longitudinal Study of Generations (Glass et al. 1986; Bengtson et al. 2002) and that in 2005 the Wave 8 data would be collected under Merril Silverstein (Silverstein and Bengtson 1997), the new principal investigator.

In our first look through the data we did note distinct generational differences (Bengtson 1975). These were not, however, in political or social or moral perceptions or even (much to our surprise) in attitudes toward sex. They were rather in *perceptions of intergenerational relationships*, what we called "family solidarity" (Bengtson et al. 1976; Bengtson and Schrader 1982). We found differences in the perception of family solidarity that showed a consistent *generational bias* – parents rated their relationship as more warm and cohesive, for example, than the children rated it, and parents also reported more frequent rates of interaction than did their offspring. This was true whether the children were 18 or 48 and whether the parents were 48 or 88. What would explain this?

I developed the "generational stake" theory with Joe Kuypers, my buddy from graduate school (he got the Berkeley job I turned down for $500), to account for the consistent bias that continually colored the two generations' perceptions of their common relationship. We argued (Bengtson and Kuypers 1971) that each generation had an investment, in terms of emotion, time, and money (depending on the life stage) in the other generation; these investments were inevitably unequal. The parents' stake or investment in their adolescent and young adult children was huge, involving enormous commitments of time, money, love, and support in them. This investment prompted many parents to justify it by perceiving a warm and cohesive response from their child, a high degree of value similarity and frequent interaction – in other words, high solidarity. The child's lesser investment in the relationship, and higher investment in individuation, caused him/her to minimize the cohesion, similarity, and contact. Their differing developmental stakes caused a systematically different perception of their common relationship. It was an explanation that many family theorists found useful in explaining family dynamics. But it was also a theory that questioned the validity of survey data that was based on the report – potentially biased by the generational stake – of one family member alone; thus it was not nearly as popular with researchers using large-scale survey data. To adequately compensate for the generational stake bias would require collecting data from at least one other family member, an expense most surveys are not willing to undertake.

The generational stake theory was an attempt to explain differences in the perception of one age group by another in the context of the family. What about the larger society? What explains differences in the perception of age groups, and what are the consequences as an individual moves into advanced age?

Linking Macro and Micro: The Social Breakdown and Reconstruction Theories of Aging

The years from 1965 to 1980 were an amazingly innovative period in the sociology of aging. Social interactionism inspired some innovative perspectives on aging, such as Rose's (1962) subculture of aging theory, which explained (and justified?) the initial development of Leisure World retirement communities across America. Modernization theory was proposed by Cowgill and Holmes (1972)

to explain the devalued status of the elderly, also called "ageism." Riley et al. (1971) presented a theory of age stratification that linked societal change and individual change over time to processes of cohort flow and individual aging. A political economy approach that linked social structures to the experiences of the aged was outlined by Estes (1979).

Kuypers and I, with the audacity of youth, wanted to try to connect these perspectives together in a theory that linked macro- and micro-social levels. We came up with the Social Competence vs. Breakdown theory (Kuypers and Bengtson 1972), which started with symbolic interactionist concepts (what today might be called the interpretive perspective) and ended with something vaguely resembling the political economy of aging. We argued that an older individual's sense of self, his/her ability to mediate between self and society, and the society's willingness to acknowledge his/her competence are related to the kind of social labeling and valuing s/he experiences in aging.

The Social Breakdown model of aging begins (Stage 1) with the vulnerability in health or economic status that characterizes many older persons – for example, an older woman who is a new widow, experiencing failing health, and who has lost her job to a younger worker. While dealing with these personal crises at the micro-social level, she is also feeling the victimizations of ageism, a cultural or macro-social condition reflecting devaluation of elderly persons in an America that values youth over age and equates an individual's worth with his or her productivity or wealth. Then, for this woman, a vicious spiral downward is set in motion (Stage 2) with additional micro-social crises having to do with role losses (involuntary retirement, loss of social contacts through illness, loss of intimacy through widowhood), leading her to feel confused about what she should do, where to turn for support, and how to behave in responding to these losses in her life. Unless she finds new age-appropriate reference groups, she may lack social feedback concerning who she is, what roles and behaviors she can usefully perform, and what value she is to her social world. Resulting from this feedback vacuum is (Stage 3) dependence on external sources of self-labeling and identification, many of which are stereotypical images of the elderly as useless and obsolete, a characterization common in a society which places so much importance on productivity. As one woman put it, "I feel old, sick, dumb, and ugly." The individual who accepts such negative labeling is then inducted into the negative, dependent situation of the final stage of life (Stage 4), learning to act like old people are supposed to act. This reduces her social and psychological competence even more, with a corresponding atrophy of skills involving social and cognitive coping (Stage 5). This, in turn, makes her even more vulnerable to even greater debilitation, thus setting the stage for still another round of the vicious spiral of the Social Breakdown Syndrome.

How might the cycle of Social Breakdown be broken? The efforts of practitioners and policy-makers to meet the needs of an aging population represent attempts to make beneficial inputs to this end. We proposed a Social Reconstruction theory with inputs at each stage. To ameliorate problems in each stage efforts could be made to (1) liberate the individual from an age-inappropriate view of status, the functional ethic; (2) improving social services to elderly persons or lessening the debilitating environmental conditions faced by elderly persons. We also applied this model to family problems involving the elderly (Bengtson and Kuypers 1985) with what I think was a practical list of interventions for therapists and practitioners working with families and elders: (1) clarify the nature of the crisis event; (2) suggest roles of short-term involvement for each potential family caregiver; (3) discuss moralisms, unrealistic expectations, and guilt; (4) program some quick short-term successes; and (5) follow-up and develop external supports.

This attempt at linking micro- and macro-theory was very well received by gerontological social workers but virtually ignored by sociologists in the field of aging. Why? I learned some valuable lessons from this failure. The theory, while innovative and quite unique in the field of aging, tried to account for too much. It was bewilderingly eclectic, borrowing ideas from symbolic interactionism and phenomenological psychology and Marx and political economy theory. It attempted to both explain and prescribe, linking theory with its practical application (which, according to social work textbooks, it succeeded in doing). And the lesson that unfortunately impressed me the most was the

one least relevant to substantive theory: We did not publish it in the right place. We should have published in a sociology journal or the *Journal of Gerontology* instead of in human development journals and as chapters in books. As I have emphasized to my students since then, too many sociologists will not take seriously anything that is not published in a sociological journal.

Race, Ethnicity, and Differences (or Similarities) in Aging

As an assistant professor I suddenly became, by default, principal investigator of a huge National Science Foundation study of black, Mexican-American, and Anglo Los Angeles middle-aged and elderly people. This project took several unexpected turns. One, happily, was an Oscar in 1977 in the Best Picture Documentary category for *Number Our Days*. This was based on ethnographic research by Barbara Meyerhoff (1978), a member of our NSF research team, of elderly Jews in Venice, California. These older individuals had created their own culture of aging far from memories of their Holocaust losses and far from their upwardly mobile children.

Also unexpected was the suspension of the project by NSF because of charges of racism. A group of minority community activists protested that there were no minorities represented on the research staff and there were no provisions for benefits to come back to the communities that furnished the data – the USC research project was a classic case of white elites exploiting minority groups. As a result the project was re-organized, the original principal investigator resigned (replaced by a woefully inexperienced new PI, me) and recruitment began for minority staff and graduate students. A Community Research Planning Committee (note that it was not an *advisory* committee) was established. Their mandate was to vote on actions at each step of the research process, from designing the survey instrument to submitting papers for journal publication. After months of heated meetings and mutual mistrust, the minority groups and the academic researchers finally got to work designing the community survey.

The initial protests and conflicts led eventually to a third unexpected outcome of the project. We *did* end up working together. Moreover, the research that resulted turned out to be *better* because of it. The Community Planning Committee found areas in the survey we were proposing that were poorly framed or not covered at all. My favorite example is the family relationship section and the questions we had drafted about children and grandchildren, brothers and sisters. A member of the Black Caucus asked, "But what about children you raise that aren't your children?" Reluctantly, and complaining inwardly about the cost of each additional question in a 1,200-respondent survey, we added some questions about "children you have raised that are not your own." When the survey results came in we found that *over 50%* of the black respondents reported they had raised children who were not biologically theirs but who they considered family (Burton and Bengtson 1982). Had we omitted this question we would not have learned about support networks outside the children usually asked about in surveys, nor about the investments, emotional and financial, that middle-aged and elderly minority respondents were making to children not their own.

This project was the most memorable experience of my research career, and I learned more from it than from any other. This awkward experiment in community-academic collaboration proved useful for other researchers (Bengtson et al. 1977) and today community involvement in social research is taken for granted. I should add a comment about the personal support I received from the Community Research Planning group. These were difficult times for me personally because my wife had been diagnosed with leukemia and my girls were 3 and 6 years old at the time. Several members of the black and Mexican American caucuses came to her funeral a year later. I will never forget what one member, Carmella Banks, said to me: "Vern, you and your daughters will *always* be members of *our* community."

A fourth set of unexpected consequences involved the research results of the project and their implications for theory. At that time the dominant orientation was what came to be known as the multiple jeopardy theory, in which the disadvantages of, say, an elderly black woman was explained by her cumulative minority status of being black, old, poor, and a woman. I wondered about this, thinking that by contrast there may be resources available to this woman that may buffer her disadvantages – particularly family support, in contrast to white elderly's lack of family support. I wanted, with the audacity of youth, to challenge the multiple jeopardy theory. So we began analyzing the data expecting to find marked differences between the black, Mexican-American, and Anglo individuals reflecting social aspects of aging. For example, we "knew" that minority groups placed higher emphasis on familistic values and that whites' higher mobility led their elders to feel lonely and often abandoned by their families. But the data did *not* support the hypotheses of either racial–ethnic advantage *or* disadvantage in family support for the aged. We did find that Mexican Americans had higher rates of contact with and affection toward family members than blacks and Anglos, but the differences were less than expected. We found that Anglos *expected* more assistance from their children than blacks or Mexican Americans, but there were insignificant differences in the *actual* provision or receipt of care. The most important finding was this: there were more differences *within* the three groups – black, Mexican American, and Anglo – in family relations and other aspects of aging than there were differences *between* the groups (Bengtson et al. 1990). We found little evidence in the data of this cross-ethnic comparative study to support the multiple jeopardy theory of aging; but we saw little evidence of data to disconfirm it either.

The research on aging in minority communities caused me to rethink several sociological generalizations I had accepted about social inequality. Could it be that we sociologists tended to maximize *between*-group differences – at the level of race, gender, or wealth – while minimizing, or perhaps even missing, differences *within* these social categories? Then there was another thing I became aware of, something even more relevant today than it was then. In contemporary sociology we are preoccupied with social inequality, with power, conflict, empowerment, and hegemony the social forces to be examined or critiqued. But there are other social forces as well: cohesion, cooperation, supportiveness, solidarity. My hope is that in the next decade we will get back to studying what binds people together as well as what drives them apart, and that sociologists will look at aging individuals for the resilience their lives represent as much as the social inequalities that make them vulnerable.

The Future of Theory in the Sociology of Aging

Since I can claim almost 50 years of experience doing research in the sociology of aging, I think I am allowed to offer some observations about the future of our field. First, in the sociology of aging today we appear to be drifting away from science and careful observation as practiced in related fields such as the psychology and biology of aging and toward the interpretive methods of history and the study of literature. Some among us have felt that this is a welcome trend and have vigorously advocated that we move away from the reductionism of positivism and move toward the freedom and questioning of critical analysis. But such polarization is unfortunate, I think; we need both. For one thing, I think we can profit greatly by following the model of cumulative knowledge building through observation, classification, prediction, and intervention or control, a model that has characterized science and has led to so many developments for the betterment of humankind's health and well-being. At the same time, however, I believe that we who do research in aging have responsibility to work for social justice, to advocate for dependent elders, and above all to add knowledge that can be useful for interventions. To accomplish this I believe that the process of developing theory – e*xplanations* – is the best way to develop more effective *interventions* to

improve the lives of older people, interventions such as more effective health practices and more equitable social policies regarding aging.

Second, we need to develop more cross-disciplinary dialogs. Successful collaboration across fields to forge multidisciplinary models of aging and intervention is all too rare for those of us doing research in the sociology of aging. In part that may be because of an increasingly insular or antiscientific culture in sociology at large, which our colleagues in medicine, psychology, biology, and economics simply cannot understand. We need to recognize that aging is a multifaceted dynamic and requires multiple research perspectives to understand it. We need to believe in ourselves and that we have something useful to offer our research colleagues in medicine, biology, and psychology.

My third concern is that in the sociology of aging today we seem to be forgetting our history and our past attempts, whether successful and unsuccessful, to explain and to understand. Some of these old theories have been useful, others not; but each one of the attempts to build explanations, particularly those that have failed to be supported by subsequent data, can teach us a good deal about age and society or social processes of aging. Knowing their history can protect us against making their mistakes.

Finally, I believe we have an intellectually exciting future before us because of the creative cohorts of younger researchers that I see entering the sociology of aging. This is evident from the papers they present at the Gerontological Society of America and the American Sociological Association meetings. We have a responsibility to help them as they develop their professional competence and humanitarian concern. We need to encourage them to propel their research toward theory, beyond mere description and classification. We need to remind them that such explanations can be used to change things for the better, to produce more effective social policy about aging and more effective programs and health interventions for older people with problems. We need to remind them that they will do a better job of theorizing about age and aging than those of us who were in the first generation of the sociology of aging were able to do. This is because they will be able to learn from our mistakes and our successes. And because they will already know how to say "Gerontology" with a "J."

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