LIVING TO 100 SYMPOSIUM*

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Session 7
Mortality "Laws" and Models—Part 1

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MR. ERIC STALLARD: I did a presentation earlier, and I wanted to see if I could draw a few connections between the last two papers on the extreme value theory. Unfortunately, I didn't have an opportunity to go through the papers in detail, but my understanding is that the initial assumption is that the ages at death are independent and identically distributed (iid). I think it's worthwhile to point out that the presentation that I did shows that in fact that assumption is not correct. The ages at death are independent, but they're not identically distributed. Slide 25 in my presentation shows that at high ages you can have a tremendous degree of variation in mortality, from numbers that are below 10 percent to numbers that are almost above 50 percent. I would give an explanation of how at very high ages, above 110 and even above 115, for example, you can have someone (a single individual) survive from 115 to 122, for example. If there was a very low mortality risk at each age, then that could happen. That was one comment.

I did a paper with my colleague Ken Manton in the *Journal of Gerontology*—I believe it was in 1997—where we looked at the issue of the limits of life expectancy. We took the approach that a fixed limit was probably not a realistic concept, because that would imply that on one day your mortality rate was effectively zero, and then it jumped to infinity. A stochastic limit would appear to be a more sensible way of conceptualizing that. If you thought of mortality, say, being at 50 percent at the highest ages, then only one out of every 1,024 people would survive 10 years, and so if you had 1,000 supercentenarians, you might get one in 1,000 surviving to age 120. Then out of that, it would be one out of a million who would survive to 130, and then one out of a billion. The practical limit, I would say, would be if you said how far one out of a billion or one out of 10 billion would survive. That way you don't need a fixed limit. It's like asking what the limit is for running a mile, and somebody says, "It's 3:30." If somebody runs 3:29.9, that prediction is wrong. It may happen.

Then the final comment is about using U.S. data. I've looked at a lot of U.S. data, and I'm always kind of appalled at the quality of it. It's not that good, and so you have to be careful when you're fitting models. Always be very skeptical of the data.

Certainly those comments would for the last two presenters. But I thought of the iid assumption because the mathematics is great. You go through this stuff and you come up with these beautiful formulas, but I think it would be important to ask, how would those results change mathematically if you put in heterogeneity and frailty, the obvious examples, or gamma-distributed frailty and inverse-Gaussian frailty? Then there's a whole range of other distributions that you presumably could consider. Anybody in academia could write 10 or 20 papers on that and create a career of extending extreme value theory beyond the "iid" to just "id" and then saying what the variability is.

MR. ZHONGXIAN (JERRY) HAN: I think that brings up a very good question. Because of technical reasons, it's really hard to think about models that do not satisfy those extreme value theorem conditions, but one intuition I have is that for the iid, independence is normally correct. For the identical, that would be in your data. When you collect your data, is your data collected from the same area, or is your data collected from the same country? Definitely, country will matter in our estimation, so that's why we are going to limit our study in a particular country or area. Especially for an insurance company, if its business is local or regional, I believe collecting local data would make more sense for this type of study to compare with the life tables in advanced ages. Still, that raises a very good question.

Also, regarding the comment on the stochastic limiting age, I agree with that. In my paper I presented here, I use a linear model, but actually when I looked at the end of the last century, it looks like to me that the linear trend stopped and it starts to fluctuate on the limiting age. We're trying to see if we can fit in some other models, especially for exponentially decaying to a certain level, but still a stochastic model.

MS. MARIE REDINA L. MUMPAR-VICTORIA: I would like to comment about mathematical possibilities of developing models. Actually, two years ago I was starting to model the dynamics of our population as well as the dynamics of those who are purchasing family insurances, but at the moment it's not working because we do not have the data. I was trying to describe this movement through a series of differential equations. Indeed that's true—20 or more papers would be possible.

MS. ANNA M. RAPPAPORT: I'd like to bring us back to Dr. Perls and ask you all, as modelers, a question. I heard him talking about different paths to getting to 100 and have been thinking about the issues confronting my Committee on Post-Retirement Needs and Risks. Regarding the whole question of practical solutions, I think paths are very important, and I know we've been focusing on the extreme high ages, but I was wondering if you all have comments about the potential for modeling different paths or about different paths.

MS. KATHRYN A. ROBERTSON: I have sort of a simple answer to that. When we've used the covariates to look at things like the year of birth or the birth cohort, I think if you had similar data to bring people into different categories, you could probably use the covariate in the same way, to look at different variables of the distribution, depending on things. Maybe the only issue would be whether or not you're looking at a variable that's a continuous variable or if you're looking at something that's a categorical variable. I don't know if there would be difficulties there.

MR. EDWARD HUI: This is sort of a long question. I wanted to bring it up prior to our table discussions just so people can mull it over. In the past few days, we've seen a lot regarding the modeling of population mortality, some with annuitant mortality and long-term care. I am interested in knowing your thoughts on the modeling of life insurance mortality, in particular the convergence of mortality at the old ages by class, whether it's a preferred or a rated class. I can personally imagine convergence well before 100, but I was curious to get your opinions as to when, how and to what degree. Also, to set the stage and sort of highlight a concern of mine, prior to three years ago in the United States, the industry as a whole had not gotten very much volume of, say, 70+ issue-age business. I think a lot of pricing was done using a flat percentage of an industry experience table. In today's conditions, we're seeing a lot of products geared more toward the old ages. I don't have exact figures, but I've noticed that the growth in these products and the volume have far exceeded the natural growth of, say, the Baby Boomers. From an underwriting perspective, Dr. Perls mentioned that cognitive function was a very big predictor, but that's not currently done in life insurance. In looking at, say, costs of insurance (COIs) for certain products, I've noticed that in some of them, the preferred classes never get above 100 per 1,000, even at attained age 100. This isn't just for a couple of companies; it's for several. I wanted to bring more attention to this point, but I also want to open it to the floor and to the panel, especially M.D.s or pricing actuaries working with the old-age products. At what ultimate age would you feel that the life insurance mortality begins to converge to population, and how does it converge?

Finally, I have a separate thought. When Al Klein presented his preliminary report on old-age mortality, he mentioned that female mortality at the old ages as a percentage of the Valuation Basic Table (VBT) was higher than males. I think that might be attributed to the fact that when looking at the VBT, female mortality never approaches population, whereas male mortality does. So again, those are my thoughts.

MR. THOMAS P. EDWALDS: I think I'd better address that one because I think I'm the only one here who is actually working in industry. I certainly agree with you that the use of a flat percentage of a standard mortality table as a way to price, especially high-issue-age business, is not the right approach. That percentage should approach 1 over time. I'm not sure that it actually needs to reach it. I think you can use a model like one of the ones that we had here, which would generate a curve that would approach it asymptotically. You wouldn't have to pick an age and say that it's going to be equal at age 100 or whatever. One observation I would make is that insured mortality is better than population mortality because of the selection that we do in underwriting. By the time you get out to age 100, I'm not sure that any of that is still valid. I think that anybody who lived to 100 is somebody who could have bought an insurance policy if the person had applied for one, so it maybe should be equal at some high age like that.

MS. RAPPAPORT: Tom, it would seem to me that if you had a uniform socioeconomic or uniform pool of population, it ought to converge. But if the population could be divided into radically different subpopulations, then it ought to converge to the subpopulation from which you're drawing your pool of applicants. That's not from any knowledge of what's happening in the insurance industry now, but just thinking about it, when you have these diverse subpopulations, it would be the population mortality for that right subpopulation.

MR. EDWALDS: I agree. My point is that if we look at the really extreme high ages, the population mortality at the high ages is essentially the population mortality for the best subpopulation.

MR. HUI: I completely agree with your point. At attained age 100, for certain products there's really not a concern. Say for a 45-year-old issue age, there's very little impact at age 100, but, again, we're seeing, say, term-to-100-type products where they issue up to age 90. In those cases, how significantly does underwriting wear off, and when does it approach population?

MR. EDWALDS: Certainly at age 90, one would conjecture that the underwriting would wear off rather rapidly, but there would still be an underwriting effect initially, if you are in fact willing to write insurance on somebody who is 90. If you have done any kind of underwriting, then I would presume that that is not part of the population that has the highest mortality at age 90. I agree that taking whatever percentage you get in that first year of selection and applying that indefinitely for the rest of the table is a bad approach. As to exactly how many years to grade it over, I think that's purely a matter of opinion. I don't have any data that would say that it's three years or it's five or whatever. As a matter of actuarial judgment, I would say that you ought to bring it up toward the population at some point and not be too aggressive with either your underwriting or your pricing at that age.

MS. NATALIA GAVRILOVA: This is more a comment or a word of caution to the last presenter about using old life tables. For example, in 1901, there were no reliable data for old ages. An example is the census of 1930. There were about 100 centenarians, according to this census, and about 800 white centenarians, according to the census, and over 2,000 non-white centenarians, and so you may get an impression about the reliability of this data. So those who constructed life tables before 1950 or so used data extrapolation for old ages. The real data were very unreliable at this time.

MR. HAN: I completely agree with you. I find that the life tables of early ages kind of smooth the data instead of real observation, and maybe some estimates over there and, therefore, is a kind of estimator of the parameter estimate, which probably could underestimate those increasing over time. We need to set up a really good system to keep records of these advanced ages, because all those mathematical models heavily depend on actuaries and dependable data.

MR. WARD KINGKADE: I want to ask a question that is directed mainly to the paper from the Philippines. How do you interpret a Makeham-type model in which the constant is negative? It perversely tends to be, in a lot of these fits (I've fit this model to U.S. data myself).

MS. MUMPAR-VICTORIA: Actually, theoretically it's possible because the assumption of Makeham is that the A must be greater than negative B, and the B must be positive. So for the practical applications, it means that accidents don't contribute much to the set of data, so it's actually the deterioration that causes death.

MR. STALLARD: The question was raised about how much variation there could be in mortality between people who were very healthy at the point of selection and people who weren't so healthy. The model that I presented was designed to answer that type of question. These results, which I actually didn't write into the paper, are still pretty preliminary, but at least I think they'll give a range of variability that may inspire people to pay a lot of attention to how much variability is there. I will just give these results for females. The average life expectancy for females at age 65 is between 19 and 20 years. The calculations that I did for the healthiest of the four pure types that I had came in at 25.9 years life expectancy for that group. For the least healthy, it was 6.2 years. For someone who was a combination of the first two types, it was 19 years, so that person in my view was kind of an average person. At age 80, the healthiest type had about 11.5 years life expectancy, and the least healthy type had about 4.5. That's a fairly sizable difference. If you're selling an annuity and you think you're selling to an average person with a 6- or 7-year life expectancy, but you're really selling to people with an 11.5-year life expectancy, that might impact the profitability of your product. You can comment on that.

MS. MUMPAR-VICTORIA: In our case, for variability of the select, we did not consider it yet. As I have mentioned, the data is raw data gathered from 90 percent of the insurance companies. But the concern of my paper is to provide interpolated values on those that do not have values still, because up to now all these data are sparse and there are missing points. We very well know that interpolation is very helpful, especially within the range of the data set, since interpolation variance is small, especially when the fit is good, and I found a good fit.

FROM THE FLOOR: I'd like to make a philosophical point. First, I think we should probably stop trying to model the maximum human life span, because it's a quantity that's intuitively unappealing. Most of the evidence I've seen at this conference, and, indeed, flipping through various studies, suggests that the ultimate q does indeed tend to roughly 0.50. I think the energy of a group of people such as this would probably go better toward trying to build a model with more explanatory power as to the causes of death. I ask myself, what is a model? The definition I recall from a distant textbook is that a model is a cut-down, simplified version of reality to aid understanding and expand the area of knowledge. Now, to my mind's eye, having a look at a lot of the evidence we've seen and having clear guidelines that the ultimate q, a critical and observed q, tends to 0.50 in a diverse range of populations, I think that would, for practical purposes, be very good to parameterize any sort of descriptive statistical model that we come up with. To my mind's eye, one descriptive statistical model is as good as the next, as long as it fits the data. If it fits the data, one descriptive model is indistinguishable from the next. However, if we invest slightly more energy in parameterizing and refining the model, such as the Gavrilovs' reliability theory model, whereby we could monitor the different systems that contribute to mortality more effectively, we'd get a clearer insight. It would be an area where additional energy could be expended, I believe more fruitfully, toward addressing the sorts of things that might happen in the future. When you have a descriptive model not based on any underlying rationale, the future parameters are just guesses, really, because the process described hasn't got any meaning. If a model is a simplified, cut-down version of reality, I think it's important to realize that reality evolves over time, and a descriptive model that we develop now can be projected in the future with any sort of arbitrary assumptions. But you need one radical change in the underlying process, one development—people stop smoking, people start smoking, cholesterol is cured—and it changes the picture completely. Energy should be more heavily invested in understanding all the factors that contribute to the table—a sort of multiple-decrement approach but looking toward the future and trying to collect data that could facilitate a more thorough and scientific model that describes the process.

MR. EDWALDS: I agree that trying to find a model that both has a theoretical basis and that fits the data is the goal. Certainly that was what Gompertz did initially. I do like the reliability theory model that the Gavrilovs proposed. I think that anybody who is willing to pursue parameterization with that could certainly get some cooperation from me. I think it would be very worthwhile to look at how to exactly turn that into a model that fits the data we have. It can help us project forward.