Informal Discussant Transcript

Session 3A
Mortality Age Patterns: Trends and Projections

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KATHLEEN WANG: We have been trying to understand more about the very old ages, especially age 100 and above, where we do not really have any information on the insured population. We have also been looking at the human mortality database and other publicly available data sources. As actuaries, we try to find one model that fits all the ages from 18 to 110. What we saw was indeed a slight flattening of the mortality curve starting from age 100. So the next step we took was to run a logistic regression to capture the flattening of the curve. We thought a logistic curve would probably be a better fit compared to an exponential model. When we were experimenting with our models, we tried splitting between the older ages and the younger ages. When we took age 70 to age 110, and fit the data using both logistic regression and exponential regression, to our surprise, we found that, as you presented, an exponential regression fits much better than a logistic regression, even though we do see a flattening at the very end. So we don’t know if this is an indication that the older age and younger age follow two different exponential curves and should not be forced into one single logistic model, or maybe there’s another more theoretical explanation to that observation. I would like to hear your comments and thoughts on that.

NATALIA GAVRILOVA: We found that mortality trajectory at older ages depends on what indicator of hazard rate is used. Sometimes people use one-year probability of death as an estimate of the hazard rate and this results in a logistic curve even if hazard rate follows the Gompertz law. Nadine showed very nicely that it is very important to use correct hazard rate estimates, and mortality rate is a good estimate up to age 110. As for the question about two exponential regressions, we studied mortality trajectories in wider age intervals—from 40 to 105 years—and found that regression slope in semi-log scale does not change. Thus, we do not observe two exponential regressions with different slopes.

NADINE OUELLETTE: It's very important to distinguish between the three concepts of what are death rates for discrete time intervals, the instantaneous death rates, which are well approximated by death rates for really small age intervals in discrete time, and probabilities of death. I'm not sure I have an answer to the question that was asked, but I think it gives me the opportunity to come back to one of the discussant points, which was, in our case we used data from age 100 and above, while Natalia used data starting from age perhaps 80 or 90. I think that this is something that would be worth examining. I think we should perhaps invest some time in validating data below age 100 at least for the French Canadian centenarians to be able to fit a model based on a longer age trend. However, we are struggling to make this decision because it's very demanding in terms of efforts, money, [and] time to validate younger and younger people because there are more and more of them. I think it's an important point and it may explain some of the discrepancy. Another point is, I was wondering whether Natalia and Leonid tried to compute pure demographic death rates using the Social Security data and the other datasets that they have access to. Did you try to do the same exercise as I did in the first picture that I presented, that is computing death rates, so death counts in the age intervals divided by the exposure to risk for the given age intervals, but for different lengths? In other words, instead of using the Nelson-Aalen estimator or the actuarial estimate, just try to approach the instantaneous death rate by simply computing pure death rates, but for very short age intervals, so one month perhaps.

NATALIA GAVRILOVA: Yes, we tried this, but mostly for mice and rats data because there's no difference simply because the mortality rate is a good estimator of the hazard rate. There might be differences if you use very wide age intervals. One year, for example, for humans is not very wide. It starts producing bias data after age 110, but before this age, which is practical, it does not.

JAY OLSHANSKY: I have three comments. The first one has to do with the first presenter on German mortality. First of all, congratulations on using APC models. It's appropriate. It's nice to see that this is being done. The only thing that puzzled me was at the beginning of the presentation when you were extolling the virtues of extrapolating life expectancy at birth, and I find this particularly problematic for a number of reasons. The most obvious of which is whether you should be considering life expectancy. The metric operates on the same scale as the Richter scale. So when you look at the Richter scale and how it's operating, the difference between 7.0 and 7.1 is not the same as the difference between 9.0 and 9.1. When you're projecting life expectancy at birth using a ruler, as was being done here, the underlying assumption is that death rates will decline at an accelerating pace, not at a linear pace, and there's no population, including Germany, experiencing dramatically accelerating reductions in death rates. So I actually find the linear extrapolation of life expectancy at birth is not a very good idea. The second point has to do with this continuous mention of Gompertz and Gompertzian mortality at older ages. You know, he only wrote four papers. Gompertz himself said that his model does not apply to populations over the age of 80, so if anyone observes decelerating mortality, it’s not overturning a Gompertzian paradigm; there is no Gompertzian paradigm past the age of 80, so please read Gompertz’s papers. If you don’t want to read his papers by the way, we wrote a summary in 1997 entitled “Ever Since Gompertz” in *Demography*, so it’s all explained there. My third comment has to do with Nadine’s presentation, which, by the way, I thought your research was absolutely brilliant and your presentation was crystal clear. You started to do what I was hoping you guys would do, and that is talk to each other and exchange because I don’t know who to believe. But I think that it’s real important what you’re doing, and so I’d like to see a little bit more exchange. Who’s right? What is it that was done wrong by one of you? I need to see more exchange because I can’t make a judgment myself on who’s right and who’s wrong. It’s important, I think, on the analysis that’s been done.

MATTHIAS BORGER: Thank you very much for your comment. I think the question whether we should extrapolate linear life expectancy trends is quite controversial. Yesterday we heard that it’s the reasonable thing to do, others believe it is not. It’s just some assumption we make for the example in our paper. In different settings, different assumptions might be more reasonable. Relating to the life expectancy at birth, we also applied the model to life expectancy at age 65, so we derived the period parameters from a life expectancy extrapolation starting at age 65. We did that for the U.S., but I didn’t show it because it’s not part of the paper. It was part of a different project, but extrapolating remaining life expectancies at age 65 worked quite well from our point of view. So the projection methodology is quite flexible. Depending on the specific setting, life expectancies at different ages can be extrapolated. Moreover, one could also apply different structures for the life expectancy extrapolations. If one does not believe in the linear trend, the extrapolation could exhibit some other shape.

NATALIA GAVRILOVA: I just would like to answer that I don’t see any disagreement with our results and Nadine’s results because, for example, we emphasize that the short interval is important when you use probability of death, not the mortality rates, simply because if you use smaller intervals, the probability of death may produce more accurate estimate of hazard rates and for mortality rates it does not matter as much. If you study death rates, what you're doing by decreasing the age interval is you're simply increasing noise, statistical noise and nothing else, and that does not affect the trajectory. In our simulation study, we showed that after age 110, there is a tendency for spurious mortality deceleration when you use one-year mortality rate. And in Nadine's data, a slight mortality deceleration is observed just around age 110 years. So her observation of mortality deceleration may be due to limitations of death rate as a hazard rate estimator at very old ages.

TOM GETZEN: I'm actually not an actuary. I'm an economist. I just take your data at face, so I always get concerned when there's disagreement. I liked all three papers, but I want to pick a bone, and it strikes me that there are two really different questions. To me, actuarial science is partly business, and another side of it is that it's science. There's an incredibly important scientific question about life expectancy at old ages, which affects lots of things that we know about what might happen as science progresses over the next 100 years. That's why there's a tremendous interest in it, which may or may not translate into a practical interest in rating or the financing of the longevity risk piece. That’s where I’m going to pick a little bone with you. Tail risk in a lot of hedged things is where there’s an order of magnitude so that an entire asset sometimes may go up or down by a 100-fold in value. Whereas actually even a very rough rule of thumb like mortality equals 1 past 110 may get us close enough. So for developing a rule of thumb, and like Matthias’ paper, as you point out, it has lots of value for developing, if you will, the financial aspects of longevity risks, but even if it introduces some error in some other way. I think it might be useful for us to separate this very important biological and demographic scientific question into what are acceptable rules of thumb for pricing products, which in some cases we can use things which we know violate reasonable assumptions.

MATTHIAS BORGER: That’s a very, very good comment, and there’s one thing I would like to add here. Recently there’s been lots of research in stochastic mortality modeling. We just went one step back and looked at what’s mostly relevant in practice. When we look at insurance companies in Germany, but also other countries, they’re first of all looking for a best estimate projection. The uncertainty around this projection is another thing they need to look at, too, but, the most important thing is to find a proper best estimate first. That’s why we took a step back from the stochastic models Johnny Li introduced and applied some basic and rather simple modeling approaches. You’re totally right that it’s very important to get from the interesting scientific stuff the practical relevant ideas.

NATALIA GAVRILOVA: I would like to add that it’s a really important for financial and actuarial science to know more about mortality trajectories at older ages.

NADINE OUELLETTE: I think that perhaps today, even us in the room who are very sensitive to mortality decline and the fact that people are living longer and longer, we somehow underestimate the importance of the age trajectory of death rates at older ages. This will have greater and greater impacts on our calculations in the future, and especially for you actuaries. Natalia said that she doesn’t see any disagreement. I don’t say it’s a full disagreement, but I am not so sure that if you have, of course, discrete data that it’s a good thing to use the probability of death to estimate the hazard rate. I see this being done in many studies, and this is wrong. I mean if you look at trends in death rates or instantaneous death rates or force of mortality, this plateau’s at around .7. Probabilities of death plateau much earlier. They plateau at around .5. We’re not completely sure about that, but some studies have suggested that this is where the plateaus lie. So I mean if you compare probability of death on the same graph as mortality rates, then you will be tempted to think that in one case there’s more deceleration than the other. It’s just in terms of what kind of measure you’re comparing; they correspond to two different concepts. That’s the point that I wanted to make.

NATALIA GAVRILOVA: Nadine talked about one-year probability of death, not one month. The shape of graphs for one-month probability of death would not differ from the shape of graphs for one-year mortality rate.

NADINE OUELLETTE: So if you compute death rates for one-year age intervals or for half-a-year age intervals, the series lie on top of each other. One is more variable than the other but the trends do not differ. Whereas for probability of deaths, of course if your age intervals is shorter, your probability of dying in that age interval is lower than in a wider age interval. So these are not comparable per se. You have to go through the probability of surviving and multiplying these in order to go back to one-year probability of death. So I think these are important points.

TOM JONES: Natalia, we've read your paper over and over again. One of the issues that we have when we price transactions, we're doing a lot of longevity reinsurance deals now, is the old age mortality and the projection assumptions are the two things that always are cropping up, particularly with second lives that come through the liability. In the first presentation, Matthias, when you were talking about projection and improvement, it looked like from the graphs, the implied improvement rate, if I was looking at this from a CMI type of modeling, it looked like 2.5 or 3 percent pretty much at every age, and I think the two things are nestled together a little bit because what your base table is and then what your improvement assumption is at those older ages drive whether you win or lose deals or are pricing them correctly. I guess the question is, is it really true that you would be proposing, if it was a CMI infrastructure, something like a 2.5 percent long-term rate, or have you not tested that kind of analysis with your modeling?

MATTHIAS BORGER: If one believes in the linear trend in life expectancies, then one will obtain the 2.5 percent annual improvement, which is in the same ball park as the numbers mentioned yesterday. I understand though that actuaries have difficulties [assuming] mortality improvements of that magnitude to last until infinity. However, we've seen yesterday that life expectancy increases have constantly been under estimated. Therefore, maybe it's not a bad idea to use a projection which is stronger than we think it might have to be, in order to account for the bias in previous insufficient projections.