Session 2A Discussant Comments

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Modeling Mortality at High Ages

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TOM EDWALDS:

We just heard three papers, which I will cover in reverse order because they actually make more sense that way. Bob Howard called his paper the "apple pie" of this session, and I will start with the apple pie even though it is considered improper to start with dessert.

I think the progression of ideas here is that first, when you're studying mortality, you have to watch out for data contamination. You've got to look through your data first, then you look at the drivers of mortality differences before you construct your model. So, I believe it makes a little more sense to cover the three papers of this session in that order.

Mr. Howard is talking about data contamination in "Liars, Cheaters and Procrastinators: How They Upset Mortality Studies." The types of contamination he talks about are well known. It has been discussed at this and prior symposiums, including the presentation we just heard from Dr. Zhu, based on the paper he wrote with Zhi Li, "Logistic Regression for Insured Mortality Experience Studies," that high age mortality data is often contaminated by incorrect ages at death, incorrect birth dates, late reported deaths or unreported deaths. You know that will affect your analysis, but Mr. Howard actually considered the question "How exactly will it affect the analysis?" What will the mortality curve from contaminated data look like if we knew for sure that actual mortality really followed a true Gompertz curve? What will our raw data tabulations look like if we introduce some known errors into hypothetical data following a true Gompertz curve? He does this for each type of contamination, and then compares each contaminated tabulation to the underlying hypothetical true data.

Mr. Howard's paper has the catchiest title of the three in this session, referring to "liars, cheaters and procrastinators." The liars are the overstated ages in your data. In the paper, Mr. Howard goes through a lot of combinations of prevalence and magnitude of overstated ages contaminating the data, and pretty much all of them show some kind of mortality deceleration away from the Gompertz curve. He includes one very plausible example where the deceleration actually leads to decreasing mortality rates as age increases above a high threshold.

The cheaters are the unreported deaths and the procrastinators are the late reported deaths in your data. For both of these types of contamination, Mr. Howard shows that analyzing the data using a death records or extinct generations type of approach is more or less going to solve the problem. The contaminated curves do not differ materially from the true underlying curve. However, it's not always possible to use this approach, so most actuarial studies are done from administrative data. He refers to pension administration, but the same issue arises with insurance data. You calculate your exposures based on your portfolio of insured lives or annuitants, and you try to match your reported death claims to those exposures. So if some deaths are not reported, you're adding to your exposure where exposure doesn't exist. Mr. Howard shows that it really doesn't take a lot of unreported deaths to cause a deceleration in the mortality curve. He also shows that late reported deaths cause a deceleration in the mortality curve, although the effect is delayed to very high ages, and it depends somewhat on the method used for handling late reported claims in the analysis.

One of my observations is that it was very easy to construct mortality deceleration using assumed contamination. Any one of the types of data contamination Mr. Howard investigated created spurious mortality deceleration. Furthermore, in other studies presented at Living to 100 symposiums, we have seen evidence that tabulations from cleansed data tend to look more like Gompertz curves. For example, in 2002, Bert Kestenbaum and Renee Ferguson presented a paper in which they took Medicare data and thoroughly scrubbed it. They found no mortality deceleration up to age 109 with very reliable data. Our friends Leonid Gavrilov and Natalia Gavrilova have also published more than one paper showing that, when you thoroughly cleanse the data and use the appropriate technique to measure mortality, there is no apparent mortality deceleration. So, perhaps mortality deceleration is more of an artifact of data contamination than a fact of mortality at advanced ages.

Mr. Howard has shown the non-reporting of even a very small percentage of deaths can create big downturns in the tabulated mortality rates at advanced ages. This leads me to conclude that we need to be very careful when dealing with advanced age mortality analysis. A death records or extinct generations method is going to be more stable with respect to incomplete reporting of deaths, so if you can work with only the reported deaths and avoid including any spurious exposures, your mortality estimates will be a little more accurate.

Regardless of the method you use to analyze your mortality data, you should always look for unreported deaths when you're doing a study. For example, check the Social Security Death Master File against your exposure base to see if you can find any deaths there that your data is showing as active exposures.

Also, as Mr. Howard mentioned, you should adjust your experience for incurred but not reported (IBNR) claims. In our internal studies, we make the IBNR adjustment to the expected claims. We estimate the percentage of unreported claims by lag and a few other key variables, then we multiply the raw expected claims by the complement of that percentage. For example, if we estimate for a given cell that 10 percent of the claims are unreported, then we expect that 90 percent of them are reported, so we take .9 times the raw expected claims for that cell. Thus, when we look at our actual-to-expected ratio, we've adjusted it for the fact that some claims have not yet been reported.

The bottom line from Mr. Howard's paper is that you must know your data. When you're going to do a study, before you start cranking through a model, know what you have. Understand the potential inaccuracies in the data and address them proactively in your analysis.

Drs. Gavrilov and Gavrilova take this advice very seriously. They are very careful to cleanse their data, and their paper "Predictors of Exceptional Longevity: Effects of Early-Life Childhood Conditions, Midlife Environment and Parental Characteristics," explains thoroughly how they go about cleansing their data. They actually did two studies here. In his presentation at this session, Dr. Gavrilov talked a little bit more about the first one, in which they identified genealogies of centenarians from the 1890-91 birth cohort, found a demographically matched set of controls born in the same year who all died exactly at age 65, and linked the genealogies to the census records from 1900 and 1930. In the second study, they linked genealogies of centenarians born between 1880 and 1895 to the genealogies of their parents, siblings, spouses and in-laws. In each study, they analyzed the data to look for predictors of differences in mortality.

A consequence of the Gavrilovs very careful scrubbing of their data is that they end up with somewhat small datasets. In the in-law study, they started by finding genealogies of 40,000 alleged centenarians, but when they finished removing all of the data that did not meet their criteria, they had only 1,711 centenarians left. From an actuarial point of view, that’s not a lot of deaths, but it is enough for a medical style study, which is the approach they used. They treated being a centenarian as a rare condition and then applied the statistical methods developed for rare disease studies to these centenarians.

In the study of the 1890-91 birth cohort, they linked the online genealogies to both the 1900 and 1930 censuses. In his talk, Dr. Gavrilov pointed out how difficult that process was. The advantage of this linkage is that it enables simultaneous analysis of childhood and midlife factors as predictors of exceptional longevity. The conclusion of their analysis is that genetics is the dominant predictor of longevity. Specifically, your chances of living to 100 are enhanced the most if either or preferably both of your parents lived to be 80 or more.

In the presentation he just gave, it appeared to me that

Dr. Gavrilov may have updated the results presented in the paper. One of the predictors of exceptional longevity for females identified in the paper was late age at first marriage, but I did not see that on the chart he just showed. The more surprising of the two female-specific predictors of exceptional longevity identified in the study was having a radio in the house in 1930. It is surprising this would be a predictive factor at all, and even more surprising it only applied to females. My initial thought was that it could be a proxy for socioeconomic status. Not everybody had radios in 1930 because that was the beginning of the Great Depression. However, having a radio in the house in 1930 was not at all predictive of male longevity; it was only predictive of female longevity. That would imply actually listening made a difference.

The Gavrilovs have published several papers in which they linked datasets to study the effect of earlier life characteristics on the achievement of age 100. Some of these earlier findings were modified by this study, when they simultaneously analyzed the effects of childhood characteristics and midlife characteristics. On one of Dr. Gavrilov's slides, he listed variables in the study that were not confirmed as being predictors of exceptional longevity. For example, in a previous study he found that being raised on a farm was a strong predictor of being a centenarian, but once he linked both childhood and midlife characteristics, he found the actual predictor was being a farmer. Certainly, if you were raised on a farm, you're a lot more likely to be a farmer, but once it is determined what your career will be, having been raised on a farm is no longer a factor. Being a farmer is what is actually predictive of becoming a centenarian. This was a strong predictor for males, but somewhat weak for females. Dr. Gavrilov suggested hard work may have been the key predictive factor, but I would like to point out that wives of farmers also work very hard. I've never lived on a farm, but I'm a Midwesterner and I do know that being a farmer is not just hard work for the husband, it's hard work for the wife, too. So being a farmer is not quite as strong a predictor for females surviving to age 100 as it is for males, but I don't think the difference is due to hard work.

Another difference between this and prior studies concerns the question of region of birth as a predictor of exceptional longevity. In this study, the Gavrilovs found that being born in the northeast region was a strong predictor of male longevity. In a previous study, he had found that being raised as a child in the west was a strong predictor of longevity. I didn't quite understand why those findings didn't match. Perhaps Dr. Gavrilov could comment on that.

Another predictor of longevity the Gavrilovs have studied in prior papers is month of birth. This study is showing that, at least for males, being born in the second half of the year is a predictor of longevity. That is somewhat consistent with earlier findings.

The second study, the siblings vs. in-laws study, was a good way to analyze the nature vs. nurture issue because the sample includes people who live together but don't have the same genetic pool, people who have the same genetic pool but don't live together and people who have neither in common. For married couples, their life environment from middle age on is essentially identical, but they're from different families, so they don't have the same genes. Siblings have the same genetic pool, but they typically don't live together. Siblings-in-law don't live together and don't have the same gene pool. However, the socioeconomic status of all of them will be similar.

In this study, the Gavrilovs found that genetic factors are very strong predictors of longevity, at least for males. So if you're male, you have a better chance of surviving longer if your brother was a centenarian than if your sister was a centenarian, and if either one of those was a centenarian, that's better than if your wife becomes a centenarian, which is still better than if one of your in-laws becomes a centenarian. So having a strong genetic tie to a centenarian made a significant difference in longevity, at least for males. However, the study also shows that environment is a factor in longevity because the centenarian in-laws consistently lived longer than average for the population, even though they didn't have the same genetics as their centenarian relatives. Another finding supporting the role of environment in longevity is that wives of centenarians lived longer than sisters-in-law of centenarians, so actually living with somebody who became a centenarian results in better longevity than just being related through marriage to them.

The Gavrilovs' paper has lots of interesting findings. I encourage you to read the full paper.

I saved the Zhu and Li paper "Logistic Regression for Insured Mortality Experience Studies" for last because this is the whole package. They construct a model of mortality, which is ultimately what is needed to make practical use of any study of mortality data. Furthermore, they create a model to estimate insured q_x, not just the population q_x. They identify the effective predictors of mortality and create a model that can be extrapolated both over thin data and into the future, which is all very exciting. This is a very good approach to mortality model construction. My first reaction when I read the paper was, "Gosh, I should have read this sooner." The bottom line is that the results of the model seem very reasonable, so it appears to be a good method.

There are a couple of limitations to the modeling approach. One is that the model does not produce ultimate rates. Dr. Zhu alluded to ultimate rates in his presentation, but the model will produce different slopes for duration and issue age, without a built-in mechanism to change one or both when the ultimate duration is reached. Therefore, different issue age and duration combinations that produce the same attained age will produce different mortality rates, no matter what the durations are. This is not necessarily a big issue, but it does mean the model works better for select data.

Another limitation concerns mortality deceleration. Dr. Zhu claims that the adjustment of the model to add a second decrement for lapses is a way of implementing mortality deceleration. This is not necessarily true. The high age q_x only approaches a limiting value other than 1 or 0 if the mortality decrement and the lapse decrement in the model have the same beta or slope for X1, which I think represented issue age. It seems far more likely to me that the slope for the mortality decrement would be steeper than the slope for the lapse decrement, which still results in q_x approaching 1 as age increases. But this is not a major defect in the model because mortality deceleration is not necessarily a fact, as Mr. Howard demonstrated. So a model design that may or may not yield a limiting value of q_x other than 1 is flexible without being overly constrained.

I did have some gripes about the modeling approach used by Drs. Zhu and Li. The first is that too much of the data was excluded before the model was fit. The dataset used by the authors contained 1.6 million deaths, but after they applied their filter, the resulting dataset had only 170,000. I have a strong preference for using all the data when I'm doing an actuarial study, particularly if I'm using a sophisticated statistical model. Let the statistical model identify the predictors of mortality and see what the results are. I realize the intent was to use a more homogeneous dataset, but I believe the way the filter was applied was flawed. This is particularly problematic because it eliminated 89 percent of the data. The filter eliminated all policies with face amounts under $50,000. This was applied across the board, without any adjustment for inflation. When the data was grouped into face amount bands, the authors adjusted the face amounts for inflation, but that was after the filter was applied. $50,000 of insurance in 1950 was a heck of a lot of money. In the Individual Life Experience Committee data, the median face amount in 1976 was $10,000. That's as far back as I can trace it. In prior years, the median face amount was a lot lower. It didn't reach $50,000 until 1985. So cutting the data this way is getting rid of lots of policies issued before 1985.

My biggest gripe with the paper is that the font used in the formulas is microscopic. How can you expect anybody to read that? But I do think this is an exciting new approach and one I hope to actually use myself.

I want to congratulate and thank all the authors. I think these are three very good papers.