Discussant Comments
Concurrent Session 1B: Late-Life Mortality Curves

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Presented at the Living to 100 Symposium
Orlando, Fla.
January 4–6, 2017

JEAN-MARIE ROBINE: Good afternoon. Now I think you can understand why I did the maximum not to be with you this afternoon, because I will lose one of my friends during this discussion, either Natalia or Roland. They both wrote a paper on the same topic: Natalia, aiming to demonstrate that the mortality trajectory is still increasing after the age of 110 years, and Roland, supporting the hypothesis that mortality is flat after the age of 110. I read the three papers presented in this session carefully, more than three times each. They are interesting, quite stimulating, but as you will see, I do not always agree with the authors.

What is amazing to me is that, I would say that after 10 years—because I saw one of my papers quoted by Roland, and it was published in 2005—we are still asking ourselves about the mortality trend above the age of 110. I remember a few years ago, here in the United States, there was a big debate between some of my colleagues whether the trends in old-age disability were decreasing or not. The NIA [National Institute on Aging] funded a research project in which they worked together to find some common solution and identify the right trends. At the end, coming to some consensus on the disability trends in the U.S., they published two great papers. To come to this conclusion, they used exactly the same data sets, and they exchanged their programs. They were not working together in the same room, but they had exactly the same data at hand and the programs used by each team.

I think from a practical point of view, from the perspective of actuarial sciences, still struggling with these issues of old-age mortality trajectory, after 10 years of work and research, is not really serious. I can even say it's ridiculous somewhere, and it is not in favor of the demographers, because as a scientific community, we have not been able to come to some solid results. I will go very quickly through my notes, because the three papers have been already nicely presented by their authors.

All teams referred to the work published by Jutta Gampe in 2010 about the existence of a mortality plateau among supercentenarians. In her paper "Mortality Trajectories at Exceptionally High Ages: A Study of Supercentenarians," Natalia Gavrilova and her coauthors consider that they used a much more robust method to check for the existence of such a plateau than Jutta Gampe did. They have not been able to reproduce the calculations done by Jutta, because her paper does

not go in enough details for them to reproduce her analysis. This is a problem. I am one of the cofounders of the International Database on Longevity (IDL). In the room, there are some people also participating in the permanent data collection. We have today much more data than the data used by Jutta Gampe in 2010 and by Natalia today. IDL is supposed to be like a small sister to the Human Mortality Database (HMD), providing the most recent data. Why is the data provided every year by Canada, England, and France, for example, not publicly available? It is something we have to fix. Is it a technical issue? Is it a funding issue? We know that it is quite expensive to make data freely available to the world community.

This being said, I had the same difficulties when I tried to repeat Natalia's study, using the first version of their paper. I was not able to repeat their own study and, in particular, to understand how they closed their life table. My point is that we need to know exactly how Natalia and Leonid computed their life table, in the same way they need to know exactly how Jutta Gampe did, to be able to compare their study to her study. This is the first issue. I think it is a very important issue, and it is back to what I said about the old-age disability trends in the United States. For years and years, our colleagues, epidemiologists, and demographers worked in parallel and did not work together. They really had to work together and to share their data and programs before understanding what was going on.

In her revised paper published here, Natalia and her coauthors found that mortality continues to grow with age above the age of 110 years, at least for the cohorts born after 1883. For the oldest cohorts born before 1884, it is possible that mortality at 110 years and above was almost flat. Even if Natalia did not use exactly the same groups of cohorts as Jutta Gampe (cohorts born before 1885 versus cohorts born after 1884 for Gampe; cohorts born before 1884 versus cohorts born after 1883 for Gavrilova), these results pose a serious challenge to the study published by Gampe in 2010. We can only call the two research groups to collaborate to eliminate these disturbing discrepancies.

The second issue is about the use of the Gompertz law in a cohort approach. This is related to both papers presented by the Gavrilovs. Both are supporting the hypothesis that mortality keeps increasing with age, according to the Gompertz law. Natalia presents results for the 1898 U.S. birth

cohort. On Figure 1, I displayed some data for France, for females, from the year 1950 to the year 2014 and from the cohort born in 1840 to the cohort born in 1904. The mortality trajectory with age is displayed through the annual probability of dying from age 0 to age 110. All the data are coming from the Human Mortality Database.
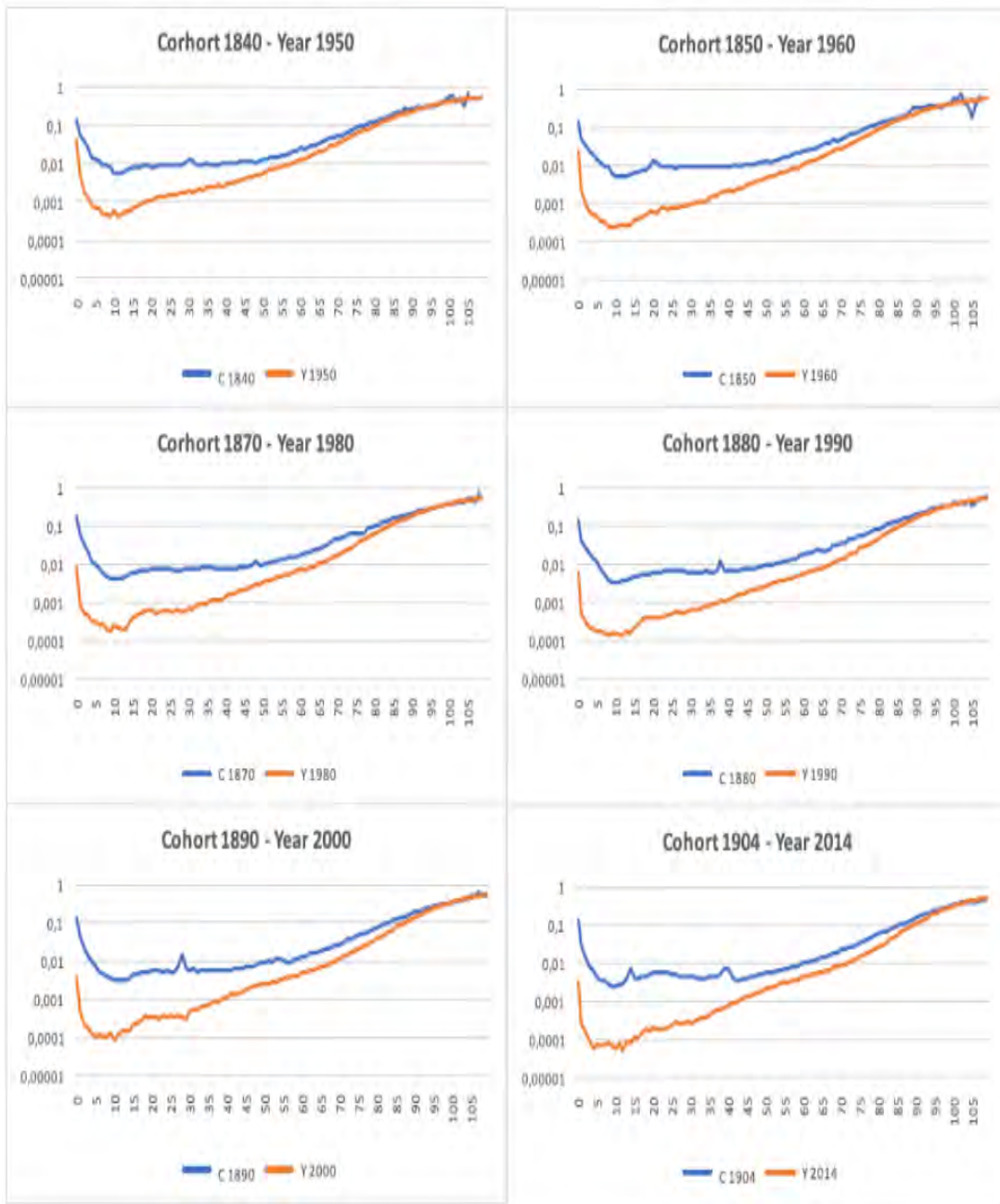

Fig. 1. Annual Probability of Dying by Age, $q(x)$, Observed in France: Period vs. Cohort Approach, Selected Periods and Selected Cohorts, Female

The red curves represent the period trajectories, and the blue curves display the cohort trajectories. If you look at the red curves, the period trajectories, you can see above the age of 15 or the age of 20 years a linear increasing trend, i.e., the Gompertz trend. But if you look at the blue curves, the cohort trajectories, you can see that the shape is totally different. You can look at the different birth cohorts selected on the figure, 1840, 1850, '70, '80, '90 or 1904, which is the last cohort reaching 110 today. For a very long part of their life, the females of these cohorts did not experience an increase in mortality with age. This is because, from the age of about 15 years to the age of 40, 45 or 50 years, the yearly improvement in mortality has offset the expected increase in mortality due to increasing age. In actual birth cohorts, cohort members aged for 25 or 30 calendar years without seeing their level of mortality increasing, just because the next age has been lived in the next year. On average, one year's progress in mortality offsets one year of age in these historical cohorts.

We cannot say that in a cohort approach, the mortality trajectory follows the Gompertz law. Actually, the mortality is almost flat from age 15 to age 50 in real cohorts, then goes increasing. When Gompertz developed his model in 1825, the distinction between cohorts and periods did not exist. Nobody at that time, including Gompertz, imagined that mortality could change over time and, therefore, could change by birth cohort. At that time, mortality was thought to only change by attained age. Therefore, the modern framework in which we can test the Gompertz law is the period approach. Indeed, when demographers observe a slowdown in mortality trajectory with age, for the highest ages, it is always in a period framework, showing a strong increase in mortality with age among the younger adult people. Period data generally show a linear increase in mortality from young adult age to the age of 85 or 90 years old, before showing a slowdown.

Gompertz provided a meaningful explanation about the capacity to face stress or to face death. With age, people lose the ability to face death, and this is why mortality is increasing with attained age. Gompertz noted that he repeated his observation in many tables. Clearly, he was not interested in showing any kind of change over time. On the contrary, he wished to demonstrate that the increase in mortality with age follows a law, i.e., something constant. This is very

important. The Gompertz model is only working, conceptually speaking, in a period approach, everything else being equal. It works under the same mortality condition. It cannot work if mortality conditions are changing over time, especially if mortality is declining over time, like in the cohort approach.

The third issue is about the adult longevity revolution. I think that I presented this revolution in this seminar a few years ago. Therefore, I will go quickly. I just updated the figures to cover the year 2014.

Figure 2 displays the distribution of the individual life spans in the period life tables for French females. From 1816, beginning of the data set, to 1934—i.e., for more than one century— the modal age at death—i.e., the typical age to die—remains between 72 and 76 years without clear trend. As the only way to increase the modal age at death is to decrease the mortality above it, we have an empirical proof that there is no change in mortality above the age of 70 years until the 1940s. However, on the same figure, we can observe, from 1816 to 1934, a large decrease in mortality before the modal age at death. In other words, until 1934, we are observing a fall in infant mortality as well as in premature mortality before the age of 50 years but no change at all in old-age mortality. After World War II, after 1945, we observe a shift in the distribution of individual life spans, with a strong increase in the modal age at death: the adult longevity revolution.
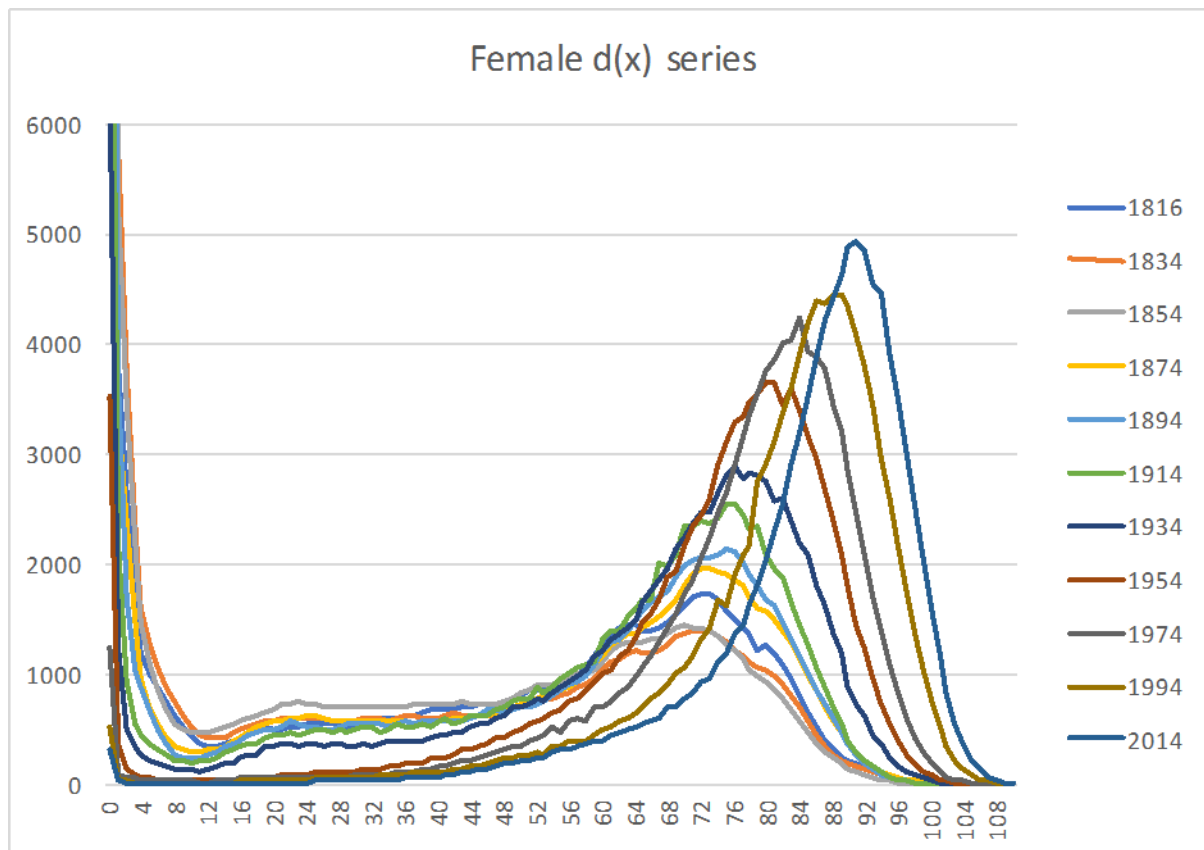
Fig. 2: Distribution of the Individual Life Spans, *d*(*x*), Observed in France: Selected Periods From 1816 to 2014, Female

As Leonid said, it was totally unexpected. Nobody was expecting to experience this revolution in adult longevity. But what is important, contrary to what Roland said about the observation of the Gompertz law between the age of 20 and 85, in all countries and in all periods, as a common characteristic, this adult longevity revolution is not something which is occurring in the same way everywhere. On this issue, we are diverging, and this is something which is new. The most important place to look at this divergence in the adult longevity revolution is to look at the age of 90 years, because that age of 90 today is close the current female modal age at death. It is the age where most females are dying today. In the 5-COOP project [Five Country Oldest Old Project], we are monitoring carefully five countries because they have very good data: Sweden, France, Denmark, Switzerland and Japan. From 1950 to about 1985, the probability to survive to

the age of 90 years is about the same in the five countries, slowly increasing from 5 percent to 15 percent. Today only 30 percent of the females will reach the age of 90 in Denmark, whereas there are 50 percent in Japan. We may be very similar between the ages of 15 and 85 years, but after that age, our countries are experiencing different mortality conditions.

It's less spectacular because we are on the other side of the density distribution of the individual life spans, but at the age of 100, we are still observing large differences between Japan, France and the three other countries. In terms of mortality level, it is the same. From 1950 to 1985, the probability of dying at the age of 100 was about the same among the five countries monitored, and declining at the same pace, but then since 1995, we observe similar divergence among them.

Therefore, back to the question raised about future forecast by Leonid Gavrilov and Natalia in their paper "Historical Evolution of Old-Age Mortality and New Approaches to Mortality Forecasting." Do we have to use the longest time series available? Obviously no, and it was beautifully demonstrated by the Gavrilovs. We don't need to look at the data before the '60s if we want to forecast life expectancy. It is obviously good advice to suggest looking for "the most optimal base period for mortality forecasting." But do we need to have homogeneous one-factor country-specific time series? This is not clear, because if we are producing long-term forecasts, can we use short-term series to do that? There is a contradiction between this and whether our period of reference should be country-specific. Can I take 10 years into the U.S., 20 years in France and 35 years in Germany, because each trend is homogeneous? What can be the value of such forecasts? Back again to what Roland showed, between the age of 15 and 85, we have the same phenomenon in all our countries, so here we have something very strong we can use.

The last element that we have to consider is the observation in different countries of a kind of stop-and-go pattern. For a few years, a country can stop making progress, and then it comes back a few years later. It would be a mistake to produce some long-term forecasts based on such stops.

In their paper, "Where Is the Level of the Mortality Plateau?" Roland Rau and colleagues found that the initial frailty model (the gamma-Gompertz model), "*the only demographically meaningful multiplicative model that holds at the plateau,*" fit better the data than a Gompertz model. But their results show a very large variation in mortality plateaus for the 16 countries

studied. This variation cannot be explained easily. In the gamma-Gompertz model, the level of the mortality plateau is the ratio of the slope parameter b and the variance of the gamma distribution g. If all actuaries and all demographers have a good idea of what is the b parameter and what can be its value, I cannot say the same for the g parameter. Can we have a meaningful graphical representation of how the initial frailty is distributed? Again, we have all a good idea of what is the mortality trajectory by age and what are the possible values for the parameter b. To go ahead with the gamma-Gompertz approach, we need to have a better understanding on how the initial frailty is distributed among populations.

I would like to add a few words on the frailty model. What is attractive with the Gompertz model is the bio-mathematic explanation provided: With the passage of time, with increased age, we are losing our ability to combat death. This is really meaningful. It is why the Gompertz law is so important in our disciplines. With the frailty model, we are supposed to get something similar, and it is exactly what Jim Vaupel underlined in a recent paper.

This model is fitting well a new approach in geriatrics, which has almost the same name: the frailty phenotype proposed by Linda Fried and her team in 2001. The frailty phenotype approach is the most powerful concept in modern geriatrics.

Actually, the frailty phenotype does not have the same definition as the initial frailty in the model proposed by James Vaupel. In the model proposed by Linda Fried and colleagues, frailty is accumulating during the life course with the passage of time. In the initial frailty model, "frailty" is not so meaningful. It seems to be too simple because the initial level of frailty, or of robustness at birth, seems to be a constant characteristic that people should keep all their life. Because of this simplicity, all people will be on parallel mortality trajectories. Is it realistic? In the definition of frailty given by Linda Fried and colleagues, shared by almost all geriatricians in the world today, frailty is a syndrome of a decreased reserve and resistance to stressors, big or small, resulting from cumulative declines across multiple physiological systems and causing vulnerability to adverse outcomes, such as mortality.

At birth, people may be more frail or more robust, but from this starting point, people can be on steeper or flatter frailty trajectories. Some people can lose their robustness faster than others. Actually, it's just making the initial frailty model more complicated, because people are no longer on parallel trajectories. Even if some people start their life being very robust, they can lose this

robustness faster than others. Conversely people can start their life being more frail, but they can retain their reserve better than others. It's making the trajectories more complicated, but at least it is much [more] realistic, and it is like that that most geriatricians are seeing the aging process today.

This is something I would like to discuss with all of you and also something which was disturbing me when reading the three papers. I got, with your papers, a lot of figures showing the shape of the mortality trajectory and/or the distribution of the length of life. Therefore, I have a good idea of the different distributions. But for the distribution of initial frailty, I have a very poor idea of its shape, except it is a gamma distribution. If you look in Wikipedia, what a gamma distribution is, you get that gamma distributions may have many different shapes. In summary, I have a clear idea of the shape of the mortality trajectory, I have some good ideas of the shape of the distribution of the individual life spans, but I have no idea at all of the shape of the distribution of the initial frailty. Roland, it will be great if you can bring some clarification of that.