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Widowhood: A Situational Life Stress or a Stressful Life Event?

It should occasion no surprise that epidemiologists have studied marital state as a factor in death and sickness for well over a century. Marriage is a hard demographic fact with obvious social consequences. In his analysis of mortality and marital state in 1858, William Farr could refer to the previous work of Deparcieux who, in the middle of the 18th century, had investigated the relative mortality of monks and nuns in France and compared them with "Tontine annuitants, consisting partly of married and partly unmarried persons."¹ The comparisons of mortality in these groups allowed for some speculation about the effects of marriage and the family on mortality.

On the basis of his own analysis, Farr wrote: "Marriage is a healthy estate. The single individual is more likely to be wrecked on his voyage than the lives joined together in matrimony." Farr's paper first appeared in the *Transactions of the National Association for the Promotion of Social Sciences* in 1858.² National data for France had come to hand and so it was possible "to determine for the first time the effect of conjugal condition on the life of a wide population."

"Young widowers," Farr observed, "under the age of 30, and even under the age of 40, experience a very heavy rate of mortality; and after 60, the widowers die more rapidly, not only than husbands, but more rapidly than older bachelors." The question Farr opened was clearly a cogent one for furthering understanding of social and environmental forces in mortality.

Karl Pearson found a more particular reason, quite contrary to Farr's broad generalities, for studying marital mortality, and in a 1903 paper he chose a very different and seemingly ingenious way to do it.³ In pursuit of the transmission of traits and the hereditary configuration of populations, Pearson and his colleagues aimed to determine the degree of assortative mating, or homogamy, from correlations between the ages at death of spouses. Pearson believed he had shown duration of life to be an inherited character. By collecting dates of death from tombstones in the churchyards of the Yorkshire dales, rural Oxfordshire, and London cemeteries, and from the records of the Society of Friends, the investigators found the correlations between ages at death they needed to support their hypothesis of homogamy. Pearson was a great statistician, and a socialist, but still more a social Darwinist.⁴ His interpretations of these data, as establishing genetic homogamy, to a modern eye seem both complex and yet simplistic, and the design of the study too is flawed (as we shall see below).

Several decades after the Pearson group, Antonio Ciocco launched a study on husbands and wives to counter Pearson's hereditarian bias.⁵ The clustering of times of death among spouses, he argued, could be taken to imply environmental rather than hereditary factors. The death of a spouse could itself be considered a factor in the survival of the widowed spouse. In a study of the paired death certificates of spouses over a period of 40 years in Washington County, Maryland—the community laboratory that has served as a nursery for generations of epidemiologists—Ciocco,

too, found a strong correlation for the age of death of spouses. In addition, a number of causes of death—tuberculosis, influenza and pneumonia, heart disease, cancer—correlated significantly among spouses and suggested various environmental interpretations. Thus, the socioeconomic environment shared by the spouses might be the common cause underlying their deaths; and infectious diseases might be transmitted from one to another. As later writers put it, alternative hypotheses might include mutual selection of poor risk mates, an unfavorable environment intimately shared by both spouses, and the deleterious effects of widowhood itself, including grief, poor diet, poverty, and the change in social life.⁶

Unhappily, the correlations of age at death among spouses in Ciocco's study, and likewise in Pearson's, could have been artifactual. To consider only the paired deaths of spouses in a defined area, and especially over a limited period, without relation to the population that gives rise to the deaths, is to ignore those spouses whose deaths did not occur within the defined period, which is to say those most likely not to have occurred close in time. Myers used a simulated population to show that this limitation could account for a large part—perhaps the whole—of the correlation of age at death among spouses found by Ciocco.⁷

Meanwhile the line of exploration begun by Farr was continued. A number of large-scale studies related registered mortality data to census populations in cross-sectional fashion.⁸⁻¹¹ One of the more interesting of these analyses again drew attention to the grave risk of mortality to the widowed, and the young widowed in particular, as noted by Farr.⁶ The withering scorn of two strong statistical critics, however, probably sufficed to turn researchers away from this approach. In reviewing such studies, the late Mindel Sheps observed that the 1950 United States census estimates of widowers included no less than 1,670 14-year-old boys! She took this to be symptomatic of misclassification problems between registration and census data.¹² So, too, did Joseph Berkson. Not a credulous man, he could not bring himself to accept as real either the systematic excess of divorced, single, and widowed for virtually every cause of death, nor the idea that grief might contribute to mortality.¹³ This non-specificity of effect provided him with more ammunition, by analogy, for his campaign against the hypothesis that smoking was a cause of lung cancer.

To the classification problem must be added two special statistical problems connected with an attribute that changes through the life cycle. Very close control of age is needed because of the dynamic shifts in marital state—in young adults from single to married, so that within every younger age group the married are bound to be older; and in older people from married to widowed, so that within older age groups, the married are always younger.¹⁴ The firm link of mortality with age will thus readily confound the results. Equally difficult is the likelihood of biased selection for remarriage. Among the widowed, the healthy are more likely than the sick to remarry; males are more likely than females to remarry because of the sexual imbalance in mortality and survivorship. Consequently, the predominance of widows is increased and the residue of widowers is quite likely to

consist of sick men subject to high mortality.

At the heart of these problems is the question of time-order in cross-sectional studies. Undaunted by the scorn of critics, Michael Young and his colleagues confronted the question directly in a new study. They were impressed by the harrowing effects of grief observed in their studies of poor communities in the East End of London. Their resolve was strengthened by previous work on suicide. They cited Durkheim: "The suicides occurring at the crisis of widowhood . . . are really due to domestic anomy resulting from the death of husband or wife. A family catastrophe occurs which affects the survivor. He is not adapted to the new situation in which he finds himself and accordingly offers less resistance to suicide." Young, *et al*, carried out a prospective follow-up of a cohort of 4,486 widowers age 55 and over through the records of the General Registrar's Office, and thereby overcame the problem of temporal sequence. The distinctive finding was a higher mortality for widowers in the first six months after bereavement.¹⁵ In a later follow-up, ischemic and arteriosclerotic heart disease proved to be the disease category in significant excess; it seemed that broken hearts might be more than a metaphor. In the later follow-up, potential confounding by social class was controlled without altering the finding.¹⁶

This result did not hold in a study of women, who are perhaps harder of heart. Spurred by the study of Young and his colleagues, two members of the United Kingdom Government Actuary Department unearthed an old investigation. Sixty thousand widows awarded pensions in 1927 had been followed for five years. Among them a retrospective cohort analysis gave no sign of accelerated mortality in the first year of bereavement.¹⁷ Two other studies that seem to be without glaring methodological flaws, however, supported the inference of marked psychosocial disturbance clustered in the months immediately following bereavement. The widowed were found to be prone both to suicide¹⁸ and to entry to psychiatric care¹⁹ in that period.

After the best part of two decades, a new and sizable historical or retrospective cohort study* has been carried out with a follow-up from one to 12 years. As reported in this issue of the Journal, Helsing, Szklo and Comstock collected sufficient data on a sufficient number of paired deaths among the widowed and married controls to test for several potentially confounding variables.²⁰ To the single social class control used in the previous British cohort study they have added an array of factors, including education, church attendance, and number of persons resident in a dwelling; smoking; and age at first marriage and marital history. Thus they have been able to carry out the most comprehensive and fully controlled analysis yet made.

The result indicates a raised relative risk for widowed men but not for widowed women. Their diagrams of person/year survival by interval after entry to exposure, however, do not suggest any clustering in the period soon after bereavement (the diagrammatic presentation is confirmed by an analysis of mortality in the same terms to be published

*The authors describe their study as a "non-current prospective study," a questionable addition to the language.

elsewhere).²¹ The most important and vexing confounding factor, uncontrolled in cross-sectional studies, is remarriage. The study shows that the remarried clearly constituted a group less liable to death than the widowed who remained in that state, and even than the married. Nonetheless, three years after widowhood, those who had not remarried continued to experience higher mortality than either the remarried or the married. Although the mode of analysis does not entirely remove selective bias for remarriage, the finding supports the inference of an effect on mortality of the widowed state itself.

The high mortality among those who moved into nursing homes comes as no surprise. The finding of higher mortality among those living alone is not quite so self evident. Thus, in a survey of a total population of persons over age 80 in the Lancashire town of Stockport in the 1950s, people living alone were in better shape not only than those who lived with relatives or had moved into residential care, but even than those living with spouses.²² Only those who lived alone, it seemed, could maintain their independence. The epidemiologist, who must cope with human vagaries and human diversity, must always be prepared for local circumstances to overturn preconceptions acquired under different conditions.

The slow unfolding of this story is not yet at an end. Exits have been found from blind alleys; errors have been eliminated step by step; some pieces of knowledge have acquired stability and others put in doubt and discarded. But not everything fits. In the face of post-bereavement clustering of mortality for males found in the British cohort study, together with the similar clustering for suicide, for entering psychiatric care, and for depression, the absence of clustering in the new cohort is puzzling. The British cohort study is flawed in the comparisons from which the excess relative mortality of widowers derives: widowers' rates are precisely based on the follow-up of a known cohort, while the married rates appear to be based on the regular statistics for deaths taking deaths of married men as numerator, and census data as denominator.** The new study of Helsing, *et al*, in turn, loses somewhat by its perhaps unavoidable use of broad age groups—18 to 44 years and then ten-year intervals—in a situation in which exquisite precision is of the greatest importance for good control.

Nonetheless, the new study supports the existence of an effect of widowhood on mortality in men, and discounts such an effect in women. The conflict between its results and those of previous studies leaves us confused on the question of whether the effect is the consequence of the specific events accompanying the loss of a wife—bereavement as agent—or of the continuing distress that follows on the specific event—the bereaved situation as environment.²³ We move forward, in concept, in technical sophistication, and in the refinement of analyses. Yet rather more than a century of study has not provided certitudes in our understanding of the consequences of those few marital roles tenable in Western societies. Since widowhood can be seen both as a specific

life event and as a continuing stressful situation, its translation into the terms of acute loss, bereavement, mourning, social and physical distress, or even of the lifting of a burden invites further epidemiological study.

MERVYN SUSSER, MB, BCh, DPH

Address reprint requests to Mervyn Susser, MB, BCh, DPH, Gertrude H. Sergievsky Professor of Epidemiology, and Director, Gertrude H. Sergievsky Center, Faculty of Medicine, Columbia University, 630 West 168th Street, New York, NY 10032.

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**That the married rates are so derived is a supposition; nothing specific is said in the paper.