Oscar winners live almost 4 yr longer than less successful film stars. Although criticized, this sort of finding still features frequently on the Internet and public media and also in medical literature.

In 2001, an article was published based on 235 Oscar winners, 527 nominees (non-winners), and 887 performers who were never nominated (controls).\(^1\) Controls were selected from performers who were in the same film as the nominees and were also the same sex and approximately the same age as the nominees. In the primary analysis, survival was measured from performers’ date of birth. Each performer was classified as a winner or non-winner from the outset. One reported analysis used winner as a time-dependent covariate to reflect the fact that all started out as non-winners but that some changed status over time. A basic analysis showed that the total life span of winners was 3.9 yr longer than controls and this was statistically significant. A slightly more sophisticated analysis that accounted for the dynamics of moving from non-winner to winner status found less dramatic differences.

The original analysis classified those who ultimately won an Oscar as winners from the outset and took no account of the time it had taken for them to become good enough actors to win one. This gave them an inbuilt survival advantage by crediting years before winning towards survival after winning. These ‘immortal’ years were a requirement for membership in the winners’ group and winners had to survive long enough to win. Performers who did not win had no minimum survival requirement; indeed, some had died before some winners had won. To estimate the longevity benefits of winning an Oscar, the comparison should begin at the time each performer first wins, and the longevity contest should only include those alive at the same age as the winner was when they won—matched controls.

This is in fact a well-known source of bias and in 1843, Farr\(^3\) described the statistical artifact created by classifying persons by their status at the end of follow-up and analysing them as if they had been in these categories from the outset. He used as examples the greater longevity of persons who had reached higher ranks—bishops vs curates, judges vs barristers, and generals vs lieutenants. Analyses overlooking this bias are still common today. In some longevity comparisons,\(^4\) \(^5\) the consequences of an incorrect conclusion are minor. However, in the evaluation of life-extension benefits of therapy, the consequences are more serious. The well-cited examples include the benefits of cardiac transplantation where waiting time for transplant was not taken into account;\(^6\) asbestos in lung cancer;\(^7\) corticosteroids in chronic obstructive pulmonary disease;\(^8\) and the results of treatment in Acquired Immunodeficiency Syndrome.\(^9\) There are methods to avoid this bias and there are also methods
to control for this effect when analysing data from observational studies.\textsuperscript{10}

To further illustrate the problem, I will expand on two of these areas—inhaled corticosteroids and cardiac transplantation. Around 2001, two studies were published looking at the effect of inhaled corticosteroids in patients discharged from a first hospitalization for chronic obstructive airways disease.\textsuperscript{11, 12} Essentially, patients were followed-up on discharge from hospital and were then given inhaled corticosteroids as clinically indicated at varying times—and were subsequently split into two groups—those who received corticosteroids before 90 days post-discharge and those who did not. The timing baseline was hospital discharge. This is the source of bias since those given steroids at say 80 days will necessarily be alive at 80 days—giving them an immortal bias of 80 days—a major advantage over their unexposed ‘controls’ who had died before receiving their steroids because they had been guaranteed to be alive when the drug was prescribed.\textsuperscript{8}

Time-dependent bias is still common in clinical trials.\textsuperscript{13} Using a Medline database search to identify all observational studies that used a survival analysis published in nine leading medical journals between 1998 and 2002, studies were identified which were susceptible to time-dependent bias if a time-dependent covariate analysis was not used. In other words, studies in which the entry criteria to be enrolled in the study could potentially be affected by time taken to meet the criteria. Of the 682 such studies identified, 127 contained a ‘baseline immeasurable’ time-dependent factor and 52 of all survival analyses of studies with a time-dependent factor were susceptible to time-dependent bias. In 35 studies, the bias was not noted and correction of the bias could have qualitatively changed the study’s conclusion in many.

Time-dependent observational and interventional outcomes are common in intensive care research. Examples include time to develop organ dysfunction, time on ventilator, and survival analysis. The effect of varying analysis methodology on the same data set in an intensive care population was studied and compared with the original results using time-varying Cox’s regression.\textsuperscript{14} In this example, data were derived from a prospective cohort study of ventilated intensive care unit (ICU) patients where the time-variable effect of interest was the occurrence of delirium. The effect of delirium on length of stay and mortality on the ICU and at 6 months was analysed using both time-fixed and time-varying methods. The Kaplan–Meier analysis using a time-fixed definition of exposure showed that delirium was associated with a delayed discharge from ICU (by roughly 5 days) \((P<0.0001)\). However, when the same analysis was performed taking into account the time-varying nature of the onset, then the curves for those with and without delirium were identical. Although both ways of analysing the data revealed that delirium was associated with an increased mortality at 6 months, and this effect was underestimated by using the more usual time-fixed method of analysis.

How should this source of bias be eliminated? In papers comparing two groups, such as winners vs nominees, one should carefully examine when and how persons enter a group. Does being in, or moving into a group, have a time-related requirement? Another way of asking the same question is, is the classification based on the status at time zero or later? If later, is this accounted for in the analysis? Careful allocation to each of the groups with due respect to the precise timing is the best way to avoid this bias. The term ‘group’ may also be a problem in clinical trials as it suggests that group membership is fixed from the outset and it may be preferable to re-classify groups at different times. Also there are several statistical methods of avoiding this source of bias and these include Cox’s proportional hazards regression with time-varying co-variates,\textsuperscript{15, 16} a modified form of the Kaplan–Meier analysis,\textsuperscript{10} and the Poisson regression.\textsuperscript{17} There are also many definitive textbooks on the subject covering the theory and the precise methodology of this statistical approach.\textsuperscript{18}

Failure to use appropriate time-varying data collection or analysis techniques can have serious consequences and may result in biased and perhaps incorrect conclusions. Interestingly, life expectancy of Oscar winning screenwriters is shorter than for nominees—by 3.6 yr (\(P=0.004\)).\textsuperscript{19} However, the average estimated lifespan of all screenwriters (both winners and nominees) exceeds the equivalent estimate for all actors. Is it that Oscar-winning screenwriters engage in behaviour more deleterious to health than do other screenwriters?

Conflict of interest
None declared.

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