COMMENT ON "DEALING WITH DISCOUNTING"

To the Editors:

Leon Martens and Eddy van Doorslaer (5) in their article criticized a study by Vermeer et al. (7) for not discounting gained life-years. Martens and Van Doorslaer reanalyzed the results of the Vermeer et al. study with a 5% discount rate and showed how discounting could be incorporated in the Declining Exponential Approximation of Life Expectancy (DEALE) method (1) that was used to calculate life expectancy after acute myocardial infarction. In this comment we argue that the article by Martens and Van Doorslaer is misleading. The method that they used is only one out of at least four possible methods to discount gained life-years that give fundamentally different results, and it is far from obvious that this specific method should be used in this case.

In the study by Vermeer et al. intracoronary thrombolysis was compared with conventional treatment after acute myocardial infarction. The survival after myocardial infarction was ascertained during the first year; thereafter, survival was assumed to be the same for both the treatment and the control groups. The whole gain in life expectancy from intracoronary thrombolysis therefore arises due to the decrease in risk during the first year. Because the effects of the treatment arise during the first year there is no need to discount gained life-years. The aim of discounting is to adjust for the different timings of costs and effects. Because both costs and effects occur during the first year, however, the timings of costs and effects are the same in this case.

The method proposed by Martens and Van Doorslaer discounts life expectancy per se. With this method, which also has been used by Weinstein and Stason (8), cumulative survival probabilities are discounted before they are added to form life expectancy. A reduction in risk this year will affect all future cumulative survival probabilities, and will therefore be discounted back to present value during every year of the remaining maximum lifetimes. With this method the life-years gained are discounted more heavily for younger persons than for older persons, owing to different remaining maximum lifetime. Gained life-years are also discounted more heavily for women than for men for the same reason. The normative principle behind cost-effectiveness analysis is that one life-year should be valued equally for everyone, which implies that the aim of the health care sector ought to be to maximize the number of gained life-years. It is clear that if the discounting method advocated by Martens and Van Doorslaer is used, one gained life-year will not be valued equally for everyone. One of the arguments used by Martens and Van Doorslaer in favor of their discounting method is that it avoids inconsistencies in the analysis. This is indeed one of the most compelling arguments in favor of discounting gained life-years (2;3;4). If gained life-years are not discounted it is always profitable to postpone a project (4). However, it is a common misinterpretation in the literature (8) that this argument can be used in favor of discounting gained life-years with the method used by Martens and Van Doorslaer. It is enough to discount for the time of the risk reduction to avoid inconsistencies, and there is no need to discount life expectancy per se. This can be seen clearly from using the estimates by Vermeer et al. of the difference in life expectancy and costs between the control and treatment groups. The difference in life expectancy was 0.7 life-years and the difference in costs was NLG 7,000, leading to a cost of NLG 10,000 per life-year gained with intracoronary thrombolysis.

Now assume that the intracoronary thrombolysis treatment program is postponed 1 year. Then it is correct to discount the life-years gained from the second to the first year, since the risk reduction is achieved in year 2. But of course if both costs and effects are discounted with the same discount rate, we will arrive at the same cost per life-year gained as in the initial situation and there is no inconsistency. This method of discounting is one of the four methods for the discounting of effects identified by Johannesson and Jönsson (3) that have been used in the literature. It is the only discounting method that does not discount life expectancy per se and the only method that is neutral with respect to priorities between age and sex groups. It can therefore be argued that this method was chosen by Vermeer et al. (intentionally or unintentionally) and that since the effects arise during the first year there was no need for discounting. The article by Martens and Van Doorslaer therefore shows (although unintentionally) the great difference between this discounting method and the one that they advocate. In order to see the difference between the two methods, it could also be useful to consider what would happen if lives gained was the output measure instead of life-years gained. In such a case it is obviously possible to use only a discounting method that discounts for the timing of the risk reduction and the Martens and Van Doorslaer method is thus not applicable in such a case.

A way to argue for the method used by Martens and Van Doorslaer would be to use the expected utility hypothesis the way Rosen (6) did. It is assumed that there is a utility (consumer surplus) attached to every future life-year of maximum lifetime, and it is assumed that the utility of every life-year is discounted back to present time. The cumulative probability of survival for every life-year is then viewed as the probability of obtaining the utility of that life-year. If the risk of mortality is reduced during year 1, all future cumulative probabilities of survival will change and be discounted back to present time. To use this approach it is necessary to assume that the utility of a life-year is constant throughout life.

To use the two remaining discounting methods, out of the four methods identified by Johannesson and Jönsson, some kind of assumption about decreasing marginal utility with respect to life expectancy has to be made. It is not necessary, however, to invoke assumptions about the shape of the utility curve with respect to life expectancy or to use the expected utility hypothesis in order to avoid inconsistencies. Until more is known about the value of risk reductions and the value of gained life-years in different patient groups, it would therefore seem to be more prudent to discount only for the different timings of risk reductions and not to discount life expectancy per se. If we wish to go on comparing results from different studies and therapies it would also be advantageous if the same method were used consistently or if the method used were stated explicitly, which is not always the case today. It is also important to be aware of the distributional implications of different methods with regard to priorities between different age and sex groups. Sensitivity analysis can also be used to test whether or not the conclusions are sensitive to the method used. (For a more complete description of the different methods used to discount gained life-years the reader is referred to Johannesson and Jönsson [3].)

We also point out that this comment is not meant as a defense of the Vermeer et al. study. That study has important limitations of its own. The discounting of life-years gained, however, is not one of them. Furthermore, it is important to stress that because economic evaluation is a dynamic process involving methodological develop-

ment, standardized methodology such as that proposed in Drummond et al. (2) has to be used with great care.

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ON THE DISCOUNTING OF GAINS IN LIFE EXPECTANCY

Reply to Johannesson and Levin

To the Editors:

It is rather surprising that Magnus Johannesson and Lars-Ake Levin (J&L) (4), in their criticism of our article, end with virtually the same message as ours, namely that the use of standardized, consistent, and explicit methodology would be of great value in the improvement of the comparability of the results of cost-effectiveness analysis. However, we fear that their note may not be a step forward in this respect.

In our article (6) we adopted the following reasoning: If the administration of thrombolytic therapy reduces the mortality risk of treated patients (even if this reduction occurs only in the first year), then this implies that a higher proportion of patients will survive the first year and experience additional life years (or months, or days) during