Heavy physical work and occurrence of sciatic pain: need for Poisson log linear models or for better data?

Eroton.—In a recent issue of *Occupational and Environmental Medicine*, Nurminen presented a re-examination of data from a sample of 419 workers previously analysed by Rihimaki. The objective given by Nurminen was to clarify the role that heavy physical work had in the production of back pain. The data on occurrence of sciatic pain according to age, earlier back accident, and occupation (concrete reinforcement workers versus maintenance house painters) presented in a 48 cell multiway contingency table were reanalysed with a Poisson log linear model. The conclusion was that earlier back accident, aging, and also heavy physical work (more precisely, belonging to the group of concrete reinforcement workers) were related to sciatic pain, whereas the first analysis had failed to show the role of heavy physical work.

The paper is interesting in that it reminds us of important methodological aspects on confounding, interaction, and the comparison between models. It also presents the Poisson log linear model. The reader may regret the lack of details in the presentation of this model, as it is less widely known than logistic or additive models. The model remains obscure, except for specialists who, presumably, are already familiar with it.

The reader may also regret the lack of discussion on several methodological points: is this model really more parsimonious than the logistic model, as it contains 24 parameters, which seems much for describing a 48 cell contingency table? The p value for occupation and sciatica is 0.048. Does it remain <0.05 if the terms in the model are slightly changed, or if age is treated differently? How far is it possible to draw statistical inference about associations between heavy work and sciatica, beyond this particular data set?

However, the most questionable point about this interesting statistical and methodological exercise deals with its relevance for a better understanding of the occupational determinants of sciatica. The analysis presented by Rihimaki was done more than 10 years ago. In 1985 it would have been difficult, maybe impossible, to use a Poisson log linear model, as the methodological references given by Nurminen had not yet been published. Clearly, the tools for analysis have improved in the
past decade. This is a progress which made possible the reanalysis of data if, as in this particular example, the array of data was given in the original paper. But it must be remembered that the epidemiology of occupational back disorders has also made progress in 10 years. This aspect is almost absent from the paper, whereas recent articles indicate other aspects, not the importance of assessing exposure rather than use of job titles.

In conclusion, the emphasis given to the need for sound methodology should be complemented by consideration of other important aspects: firstly, for prevention of back disorders at the workplace the first necessity is good data and studies tackling adequately assessment of exposure, recall bias, misclassification, and selection effect, preferably with a prospective design. Secondly, negative results are difficult to interpret, in general, and especially in less recent studies, as they may be negative for many different reasons. Whether or not occupation is a significant factor in the data reanalyzed by Nurminen is probably not a crucial point, as heavy physical work is, anyway, widely recognised as a risk factor for low back disorders.

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Author’s reply—In a commentary of my reanalysis1 of data from a study conducted by Riihimäki2 on the effects of heavy physical work and the occurrence of sciatic pain, Leclerc and Gueguen bring to the fore some important methodological aspects. I take on the editor’s kind request to respond to these issues.

I fully agree with Leclerc and Gueguen’s stance to search for high quality epidemiological data that are needed to study proficiently low back problems at work in a general desideratum which I discussed briefly in my paper. In particular, I referred to a recent study3 that, owing to its prospective design and many of the methodological pitfalls that often plague retrospective studies. Considerations of biases in study design, exposure assessment, etc., that threaten validity should override concerns about statistical shortcomings. If the study design is biased, there may be only limited, or no, statistical methods available to correct or to reduce bias in the data analysis phase. Conversely, a study base can be unbiased, but a published paper of the study may report an erroneous conclusion because of a deficient statistical analysis. In this situation, a reanalysis of the data is called for, not only because the data may be included in overviews, but because researchers always report under correction and one may not even point out the importance of a quantitative estimate of exposure instead of a job title. However, I judged the frequency data of Riihimäki to be unreliable because of the contrast in physical loading between the two occupational categories was so extreme.

It would be a regrettable state of affairs, indeed, if epidemiologists had not been aware of the availability of the Poisson regression in the pain. However, according my knowledge this is not the case. The log linear modelling technique is a general method to handle quantitative data that have many categorical variables. The method is particularly suitable for determining interactions between variables by fitting models that are linear in the logarithmic scale. The major use of the Poisson log linear models is to fit them to multiway frequency data—such as in table 4 of Riihimäki’s data. Poisson regression is also a good model that is often applied for incidence type data. In epidemiology, logistic models were introduced in the 1960s, whereas log linear models were first used in the social sciences. Scoring became generally available in the 1970s, when epidemiologists started to use them. A review of developments in statistical modelling in epidemiology in the 1980s included several applications of the Poisson regression. A survey of methods and statistical models located 200 occupational cohort studies published in 1990. Multivariable modelling was performed in 20% of the studies, with use divided about equally between log linear, Poisson, and proportional hazards regression. I myself first used log linear models in occupational epidemiology more than 15 years ago. Clearly, contrary to the opinion of Leclerc and Gueguen, Poisson models do not remain “obscure” for the practitioners of epidemiology.

Leclerc and Gueguen misinform by asserting that “In 1985 it would have been difficult, maybe impossible, to use a Poisson log linear model, as no methodology references for that of Nurminen had not yet been published.” The methodological references that I gave in my paper were chosen to be either quite recent or widely available publications. But an old book by Haberman (reference 15, Nurminen)5 that presents the Poisson log linear model was published in 1978. The program GLIM, used by Riihimäki in her analysis, is fully implemented for fitting a Poisson log linear model. Actually, I first analysed the data with GLIM. I later carried out the analysis with the S-PLUS system because of its better capabilities for graphical display.

Leclerc and Gueguen are concerned about the parsimony of the log linear model that consumed 24 parameters to put a structure on grouped data with 48 frequencies; this left 48-24=24 degrees of freedom (df). The logistic model accepted by Riihimäki contained only seven parameters to structure the 24 proportions; there remained 24-7=17 df. However, my reanalysis indicated that Riihimäki’s logistic model lacked the necessary terms to capture the non-linear associations present in the data. Thus, the number of parameters in these models are not comparable. The parsimony of the log linear model that I referred to dealt with the formulation of age as a second order polynomial variable instead of a categorical variable. As age was involved in multiple interaction terms, the use of age as a categorical variable would have included too many parameters in the model. Thus, it could be argued that the more complex log linear model was more parsimonious than the simpler logistic model because the former had more df—that is, a greater number of free parameters.

Leclerc and Gueguen were also concerned whether the significant p value (0.048) for the interaction term between occupation and sciatica would stay below the arbitrary 0.05 level with slight changes in the model specification. The answer is no, it probably would not be that robust; nor be a critical question. I have expressed elsewhere, and this is a widely accepted stance of modern epidemiologists, that significance testing should not be performed in the spirit of decision making according to a predetermined criterion (p<0.05). Better here that quantitative estimates of the effects of risk factors on the occurrence of sciatica should also be provided.

To what extent can the conclusions drawn from the reanalysis be generalised? Leclerc and Gueguen inquire specifically about the possibility of drawing statistical inferences about the associations between heavy work and sciatica beyond the data set. The results from a statistical analysis are, of course, bound to the particular timing and setting of the study. Hence it cannot be inferred statistically that the findings are generalisable to a new population or some population at large. Causal inference is not statistical by nature; rather it strives to determine scientific explanations that would explain the results of the statistical analysis in a logically compelling way. The statistical method is thus empirically showing about effect modification with important intrinsic features of the people under study.

Leclerc and Gueguen regard my reanalysis merely as a “statistical and methodological exercise”, as they question its relevance for the better understanding of the occupational determinants of sciatica. Yet, the main conclusion of my reanalysis regarding the role of heavy physical work in sciatica that is seen in the original analysis. Further, Leclerc and Gueguen maintain that the finding that heavy physical work in jobs in concrete reinforcement is a significant risk factor of sciatica in the data is not “crucial”, as this relation is today widely recognised to hold for heavy physical work and low back disorders in general. Nevertheless, in view of future meta-analyses, it is important that the information contained in all available data sets bearing on the scientific issue are efficiently extracted and irreproachably interpreted. The generic theme of the November 1997 issue in which my article appeared was aging. As emphasised by Consiglio et al. in the same issue, confounding by age is often mixed up with effect modification by age. I pointed out that this basic methodological distinction was not realised in the original analysis; this oversight prevented the finding that age modified the effect of work in different occupations.

Finally, I am of the same opinion as Leclerc and Gueguen that, by and large, negative studies are difficult to interpret. But, Riihimäki’s study was noteworthy as negative, because the study hypothesis—heavy physical work is related to sciatic pain—was actually supported by the data. Riihimäki simply failed to show the relation statistically. This is the main point that I tried to bring out in my paper.
In conclusion, it remains enigmatic to me why Leclerc and Guguen, in the title of their commentary, juxtapose the use of Poisson modelling with the quality of epidemiological data.

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