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Letter to the Editors and Reply on: "Air Bag Effectiveness as a Function of Impact Speed" *Traffic Injury Prevention*, 4:128–135, 2003

6 June 2003

Dear Mr. Nusholtz:

I read with great attention your recent publication in *Traffic Injury Prevention* (2003; 4:128–135) "Air Bag Effectiveness as a Function of Impact Speed" as it is a subject most close to my own research interests.

The focus of your research is only slightly different from some experience of mine published 3 years ago in the *American Journal of Public Health* (2000; 90:1575–1581). In your recent article, you are investigating the "fading" effects of air bag effectiveness with severity of crash for head injuries MAIS 3+ among drivers of vehicles model year 1988 onward in frontal crashes, whereas I was looking for net effectiveness (gains and losses) across crash severity for all injuries, grouping MAIS into three categories (0, 1–3, and 4–6), and among drivers of vehicles model year 1986 onward. In addition to other findings, I also reported the "fading" effects that you present. Thus, your statement about "there [being] no clear indications of the effectiveness trend with respect to velocity of the crash" (p. 129) is not completely true since there is, at least, this additional reference.

More importantly, I am particularly curious regarding your finding on statistically significant interaction between air bag deployment and safety belt use, as I explored myself that relationship a few years ago and could not find it. I notice you used NASS/CDS 1993–99 whereas I used 1993–96 and I wonder if the additional years of data compensate for the loss of vehicle model years (you did not include model years 1986 and 1987). I could not find any reference to the total sample size that you included in your final logistic regression to help me confirm whether my hypothesis of small sample size (once I had included other covariates) was responsible for my lack of statistical finding. In this regard, and preceding the current Table II, I found myself missing another table where, in addition to the sample size inquired about above, the reader would see the distribution of your sample with regard to the different covariates.

Back to your results in Table II, I guess that you run the logistic regression with MAIS <3 as your outcome of interest since all the parameter estimates presented in this table make sense only if interpreted in that fashion (e.g., the higher the delta-V the less likely to have MAIS <3, air bag deployment making more likely to have MAIS <3). I would appreciate confirmation of this observation. I also thought it was interesting that Table II presents parameter estimates, as this is not the customary practice among biostatisticians; instead of presenting the parameters, most publications will report the odds ratios (ORs) derived from exponentiating such parameters. Regardless of the indicator, I am even more surprised (and intrigued) by the magnitude of your findings. The transformations of the information in Table II into ORs would result in protective effects of safety belts and no air bags of 2.1, no safety belts and air bags of 21, and safety belts and air bags of 65. These ORs are well above the published literature and outstanding for any public health intervention. If true, they certainly deserve great attention.

I am very happy to see that you included pretty much the same covariates I did in the logistic regression model: age of the driver (I explored gender and height also), mass ratio (I also did curb weight and wheelbase) in addition to air bag deployment, safety belt use, severity of crash (delta-V), and several interaction terms. I wonder if you could confirm whether you used longitudinal or total delta-V for your analysis.

Your publication is of great interest even today, although it applies to pre–1998 model years. I hope similar analyses will be repeated for more modern vehicles with updated air bag systems. I know the scientific community will be interested in your contribution.

I am looking forward to reading more of your work and hopefully seeing you at another conference (we first met in SAE a few years ago where I presented some of the work I am discussing here).

María Seguí-Gómez, M.D., Sc.D.
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13 June 2003

Dear Dr. María Seguí-Gómez:

Thank you for the insightful response. We are aware of your publication in *American Journal of Public Health* (2000; 90: 1575–1581) and we read it with great interest. However, we were very confused by your analysis. For example, we have seen few (probably due to coding errors), if any, deployments

below 5 km/h. Thus, it is not clear how you estimate a 10% change in effectiveness for crashes below 5 km/h.

We also have not seen much data on crashes above 60 km/h and we could not understand how you have estimated effectiveness at higher speeds. There were enough questions that we could not compare the results of the two studies. We then tried to correspond with you by e-mail but we were apparently unsuccessful. Nonetheless, even though we could not compare the two studies we should have referenced your paper and we apologize for that.

Fundamentally, there are eight questions raised in your letter:

1. ". . . effects of air bag effectiveness with severity of crash for head injuries . . ." (paragraph 2)
2. "I also reported the 'fading' effects that you present." (paragraph 2)
3. ". . . your statement about . . . is not completely true . . ." (paragraph 2)
4. "I could not find . . . the total sample size that you included in your final logistic regression to help me confirm whether my hypothesis of small sample size . . . was responsible for my lack of statistical finding." (paragraph 3)
5. "I guess that you run the logistic with MAIS <3 as your outcome . . ." (paragraph 4)
6. ". . . most publications will report the odds ratios . . ." (paragraph 4)
7. "I also thought it was interesting that Table II presents parameter estimates, as this is not the customary practice among biostatisticians; instead of presenting the parameters, most publications will report the odds ratios derived from exponentiating such parameters. . . . I am even more surprised by the magnitude of your findings. The transformations of the information in Table II into ORs would result in protective effects of safety belts and no air bags of 2.1, no safety belts and air bags of 21, and safety belts and air bags of 65." (paragraph 4)
8. ". . . if you could confirm whether you use longitudinal or total delta-V . . ." (paragraph 5)

In the following we will address the points listed above:

1. In our article we presented three different approaches to analyze the air bag effectiveness (*gain and losses*) as function of crash severity: a theoretical analysis, a nonparametric estimation, and a parametric estimation (logistic analysis).

The nonparametric estimation focus is on the air bag effectiveness in preventing MAIS 3+ head (only) injuries. The logistic regression analysis was used to evaluate the air bag effectiveness to reduce serious-to-fatal (MAIS 3+) for all body injuries. All three of the approaches supported the same conclusions: the air bag effectiveness is, in general, a decreasing function of crash severity.

2. This statement needs some clarifications. Both of the two articles report that the air bag effectiveness is a function of the crash speed. But the effectiveness trend suggested in your article for the velocity ranges that we used is opposite to the one supported by our study (all the three of the analyses described in point 1).

For example, your analysis suggests that the air bag effectiveness, in preventing serious injuries, is an *increasing function* of crash severity with a consistent difference between female and male drivers for a crash severity between 13 km/h (8 mph) and 53 km/h (33 mph): increasing from injurious to no effect for female drivers, increasing from no effect to positive effect for male drivers. In our analysis there is no significant difference (in terms of air bag effectiveness) between female and male drivers.

Moreover, our study shows that the air bag effectiveness is a *decreasing function* of crash severity: It ranges from a positive effect at lower velocities to a no effect/injurious effect at the higher velocities. In Figure 1 we plotted together our estimated air bag effectiveness curves and our approximation of the curves proposed in your article (for serious injuries) transformed into air bag effectiveness.

The air bag effectiveness is plotted as *positive* in the range in which the air bag has a *protective effect*.

The dotted lines, in the figure, are only an approximation of the air bag effectiveness curves (as we have transformed them) proposed in your article. We did not plot the actual curves (proposed in your article) because we were not able to understand how they were generated.

3. We did not cite "explicitly" your analysis because of all the questions we had. However, we could have cited it without significant comments, but this is a question of style. We apologize if this decision has created a misunderstanding and in hindsight we should have cited it.

4. For the logistic regression analysis the weighted sample size was 1,698,337 (raw data size was 4,369) of which there was a weighted sample size of 76,270 (raw data size was 754)

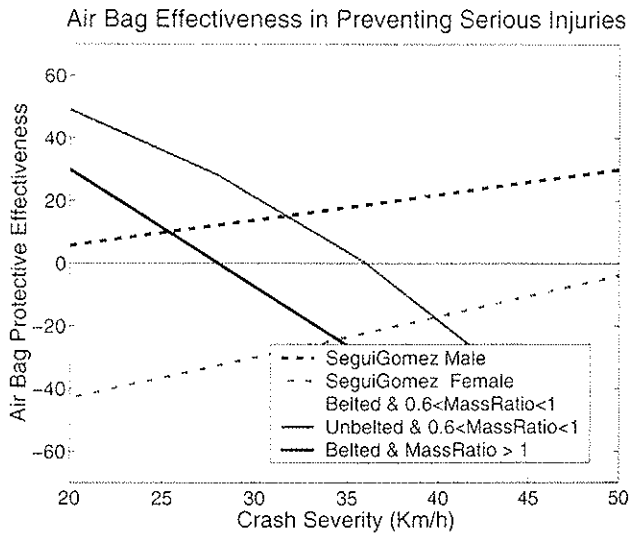

Figure 1

serious-to-fatal injury cases. We do not understand why you did not get significance.

5. Yes, the outcome in the logistic analysis was MAIS <3.

6. We consider it more informative to report *all* the parameter estimates and to summarize the conclusions in terms of air bag effectiveness. From the parameter estimates presented in Table II in our article, the reader can evaluate all the odds ratios of interest. Moreover, we did not completely understand this comment because in your article the results of the multivariate models are (partially) presented in your Table 2 in terms of parameter estimates and not odds ratios.

7. We apologize for not knowing the behavioral and cultural nature of biostatisticians. However, the odds ratios that would be calculated from our results are not a single valued scalar but rather a function of many variables representing a response surface. Therefore, although the estimated odds ratio for the seat belt only is 2.1, as you were suggesting, the other two odds ratio evaluations are not correct: It appears that you have forgotten to include some of the interaction terms. For example, the following two tables contain the estimated odds ratios, for the mass-crash severity scenarios considered in our analysis.

Estimated odds ratios for the air bag when the driver is (already) belted:

	10–15 mph	16–20 mph	21–25 mph	>25 mph
Mass ratio ≤ 0.6	6.2	4.4	3.1	2.1
0.6 < Mass ratio ≤ 1	3.0	2.1	1.5	1.0
Mass ratio > 1	1.4	1.0	<1	<1

Estimated odds ratios for the air bag when the driver is unbelted:

	10–15 mph	16–20 mph	21–25 mph	>25 mph
Mass ratio ≤ 0.6	4.2	2.9	2.1	1.4
0.6 < Mass ratio ≤ 1	2.0	1.4	1.0	<1
Mass ratio > 1	1.0	<1	<1	<1

For the odds ratio evaluation we have used the middle point for the velocity bins while for the mass ratio bins we used the standard ordered code:

Mass ratio < 0.6	0.6 < Mass ratio < 1	Mass ratio > 1
Coded as 1	Coded as 2	Coded as 3

In conclusion, the average air bag odds ratio for low velocity (for crash severity below 15 mph) is around 2.0 to 3.0 and for crashes above 25 mph, 1 to below 1.

8. We have used the total delta-V, because we were considering it an estimate of crash severity. However, since we focused on frontal crashes (direction of force between 11 and 1 o'clock), there should be only slight difference between total delta-V and longitudinal delta-V.

Once again thank you for your letter (e-mail). It helps in thinking through all the complex issues. I remember our meeting at SAE. I greatly enjoyed talking with you at that conference and I look forward to meeting you again. Keep in touch.

Guy Nusholtz
Daimler-Chrysler Corporation