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LETTER TO THE EDITOR

Are perceptual and motor inhibition processes really dissociated? A comment on Nassauer and Halperin (2003)

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The *Journal of the International Neuropsychological Society* published recently a paper that should become influential shortly. However, given its potential impact, this study has to be examined carefully.

According to Nassauer and Halperin (2003), their new computerized tasks would be able to show that perceptual inhibition and motor inhibition are dissociated, that is, independent mechanisms. The authors designed a set of six subtests by crossing two dichotomic variables, namely, absence *vs*. presence of a perceptual conflict, and absence *vs*. presence of a motor conflict, which led to four experimental conditions. The logical rationale underlying this study was the *additive-factor method* offered by Sternberg (1969) in the field of mental chronometry. According to this framework, if the two kinds of inhibition recruit separate mental resources, their simultaneous involvement in a given task will lead to an increase of response latency corresponding to the addition of both kinds of inhibition; conversely, if the two kinds of inhibition are not dissociated but share common resources, the resulting increase of latency should be higher than an addition, the supplementary cost resulting from the need for resources sharing. Concretely, in the former case, one should observe a significant main effect of each kind of inhibition but no interaction while, in the latter case, the interaction should be significant. Nassauer and Halperin (2003) claimed that their results support the dissociation thesis.

Three qualifications apply. First, the dissociation thesis relies on the *prediction of a null effect* (of the interaction). Now, the meaning of an absence of effect is generally overestimated, as it can result from a lot of factors not theoretically relevant, such as the power of the tests, the perceptiveness of the tools (floor and ceiling effects), and so on. To really argue such absence of effect, Nassauer and Halperin (2003) should report a control task where a significant interaction would be predicted and verified.

Second, the reported *measures* of main effects and interaction are not really convincing. Indeed, given the rationale of the ANOVA, the main effect of perceptual inhibition is assessed for the two conditions of motor conflict (conflict + no conflict) pooled, and the main effect of motor inhibition is assessed for the two conditions of perceptual conflicts (conflict + no conflict) pooled. And, accordingly, there was no significant interaction, with a perceptual inhibition of 170 ms (calculated from the means mentioned in the paper), a motor inhibition of 114 ms (calculated from the means mentioned in the paper), and a combined effect of about 280 ms (deduced from the inspection of Figure 2), that is, an additive effect. Now, it seems to me that the effect of each conflict should be assessed "purely", that is, in the absence of the other conflict. By means of such a procedure and by estimating the not reported means of the four subconditions, I note a perceptual inhibition of about 215 ms, a motor inhibition of about 185 ms, and a cumulated inhibition of about 270 ms. This value is clearly underadditive, and would lead to an interaction effect.

Third, measures limited to the *absolute differences* are generally biased, as the baseline can differ according to the task considered. Therefore, an inhibition effect of, say, 100 ms, has not the same meaning in both tasks. And indeed Nassauer and Halperin (2003) reported a mean latency of 423 ms in the perceptual no-conflict, against 451 ms in the motor no-conflict. Clearly, some correction for overall accuracy (or speed) is needed. In neuropsychology, this case is well known in studies of laterality (comparisons of groups of unilaterally brain injured subjects, divided visual field studies or dichotic tasks in healthy participants), and several indices of asymmetry have been proposed in the literature. For example, the size of the conflict effect can be weighted by the baseline condition and expressed as a percentage: (conflict − no conflict)/no conflict. In the present study, this leads to values of 40% for the perceptual con-

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flict, 26% in the motor conflict, and 64% for the combined conflicts (additivity), by taking the values used by Nassauer and Halperin (2003), but this leads to 57, 49, and 72%, respectively, by taking my method (underadditivity). Another method would be to compute the ratio (conflict − no conflict)/(conflict + no conflict), which leads to values between −1 and +1. In the present study, this would lead to values of 0.17 for the perceptual conflict, 0.11 for the motor conflict and 0.28 for the combined conflict (additivity) by taking the values used by Nassauer and Halperin (2003), but to values of 0.22, 0.20 and 0.26 by taking my method (underadditivity).

The tool offered by Nassauer and Halperin (2003) is clearly promising, but needs to be clarified. One is reminded of the valuable attempt made by Dunn and Kirsner (1988) who pointed to some weakness of the double dissociation method (see Shallice, 1988), and suggested consideration of the reversed association procedure. Several interesting empirical studies have resulted.

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