

LETTER TO THE EDITOR

A response to Raymond Bruyer's "Are perceptual and motor inhibition processes really dissociated?" A comment on Nassauer and Halperin (2003)"

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We were pleased by Dr. Bruyer's opinion that our paper is likely to become "influential shortly" and welcome this opportunity to respond to his thoughtful comments and cautions regarding the manuscript. We certainly agree with his view that novel manuscripts and approaches must be examined carefully and appreciate his careful examination of our manuscript. Dr. Bruyer raises three qualifications related to our analyses and interpretation of the data.

The first relates to the potential for over-interpretation of the meaning of finding the null when examining the Perceptual \times Motor conflict interaction. We agree that non-significant results must be viewed with extreme caution, and that additional research and replication is warranted. Dr. Bruyer's suggestion for a control task in which the interaction is predicted and observed is quite clever and would have been a useful addition to the study, but unfortunately we did not administer such a task. Nevertheless, in this case, we have reasonable confidence that the lack of a significant interaction is not an artifact of measurement issues such as ceiling and floor effects, or limited power as suggested by Dr. Bruyer. Unlike error scores, where ceiling and floor effects frequently occur, the use of reaction time as the primary dependent measure in this paradigm virtually precludes ceiling and floor effects. In addition, as discussed in the manuscript, other methodological concerns that might impact upon the findings such as a speed-accuracy tradeoff or overall intellectual ability do not appear to impact upon the findings. With regard to power, the interaction did not "fall just short of significance," but rather was virtually null ($F = 0.08$). To emphasize this point, we indicated in the manuscript that the p value was not just $>.10$, but rather $>.50$. Thus, it is unlikely that this non-significant finding for the interaction is spurious.

The second point raised by Dr. Bruyer was somewhat less clear to us. He indicates that traditional statistical approaches for examining main effects and interactions are not convincing in this case and that the effect of each conflict should be assessed purely, in the absence of the other

conflict. This certainly seems to make sense. His commentary indicates that we did not report the mean reaction times for the four critical subconditions. However, those means are depicted in Figure 2 in the manuscript; the precise means (SD) are indicated in the Table below.

		Perceptual Conflict	
		No	Yes
Motor Conflict	No	363 ms (48 m)	538 ms (107)
	Yes	482 ms (109)	648 ms (243)

Using these numbers, the calculated impact of perceptual conflict in the absence of motor conflict is 175 ms ($538 - 363$); the magnitude of the motor conflict effect is 119 ms ($482 - 363$), and the combined effect is 285 ms ($648 - 363$). This is clearly an additive effect ($175 + 119 = 294$ vs. 285 ms). It is not clear to us how he generated his estimates which led to an underadditive effect, but we suspect that he erroneously used marginal rather than cell means to calculate them.

Dr. Bruyer's third point relates to the use of absolute scores rather than proportional differences to determine the magnitude of the effects. Using the numbers above, perceptual conflict increases the reaction time above the no conflict condition by 48%, motor conflict by 33%, and the combined conflict by 79%, again an additive effect ($48\% + 33\% = 81\%$ vs. 79%).

Overall, while we respectfully disagree with the concerns raised by Dr. Bruyer regarding our paper, we appreciate his thoughtful consideration of the manuscript and agree that research reports should always be viewed with a "critical eye." There is no doubt that further research is warranted to more clearly define and measure the constructs of perceptual and motor conflict, and to determine the extent to which they are truly separable and independent.