Critique on Servos & Goodale (1994): Final year student essay (reprinted with permission)

Visual information about an object's structure and location are used to plan the movements of the limbs and hand before the grasping movement even begins (Goodale et al, 1991). In binocular feedback there are several types of cues available, such as stereomotion, diplopia and retinal disparities. Important information about the hand's trajectory could, therefore, be provided by the moving hand. Therefore it is necessary to establish whether binocular vision contributes anything to the online control of prehension or whether monocular vision will do just as well. Previous studies, notably Servos et al (1992), have found that monocular visual cues are less effective than binocular cues for the on-line control of manual prehension.

Servos and Goodale (1994) performed two experiments. In experiment one there were two types of trial: complete binocular vision throughout the trial, and initial binocular vision replaced with monocular vision once the reaching movement was initialised. In experiment two the two types of trial were: complete monocular vision and initial monocular vision replaces with binocular vision once the reaching movement was initialised, essentially a reverse of the first experimental trials.

They found that when subjects only had monocular feedback they produced prolonged deceleration phases. In the second experiment they also found that subjects had increased times to contact with the object. They also found that providing binocular feedback resulted in shorter times for object contact than was observed when only monocular feedback was available, suggesting that binocular information provides an important source of feedback information in prehension as reaching movements were disturbed when binocular feedback information was removed, although the effects were not as dramatic as when binocular information was absent.

The claim that monocular vision results in an underestimation of object distance was first put forward by Servos et al (1992), and is put forward again here. That is to say that this underestimation results in an increase in the time taken to adjust the posture of the hand and fingers, resulting in longer object contact times. The more access subjects had to binocular feedback, the less time they spent in contact with the objects.

Servos et al (1992) suggest that these findings show that binocular vision plays an important role in the initial programming of prehension movements and also makes an important contribution to the on-line control of such movements. They also suggest that the reaches made under monocular feedback conditions may reflect less efficient on-line visuomotor control. They then suggest that the setting of some of the early kinematics, such as

maximum grip aperture, have to rely on the initial view as there is no time for visual feedback to modify their production. They conclude by saying that the deceleration phase was a consequence of the need to adjust a trajectory that was programmed on the basis of initial underestimation of object distance and that the use of monocular information to perform these adjustment sis not as efficient as using binocular information.

The main problem with this paper is the fact that in the two experiments performed, different subjects were used for each. This should in fact have been a single experiment or two experiments using the same subjects. There is a very small N so individual differences will be rider, regardless of repeated measures analysis. The subjects should have been pooled and made to do both sets of trials, this way a larger N would have been achieved while also giving more accurate results.

The authors of this study decided not to statistically compare the two subject groups, their reasons being the small number of subjects and the presence of large individual differences. This would seem illogical, as the whole point of the experiment was to find a causal relationship between the two sets of data; if the same subjects had been used for each experiment, this would have been possible. In Servos et al's much cited 1992 paper the same subjects were used for each section of this study, binocular and monocular reaches were directly compared. This should have been done in what appears to be a follow-up study.

Another area of concern with the subjects was the fact that they were all right-handed. This in itself is not a problem but all subjects were tested for eye dominance and only seven subjects in each experiment had right-eye dominance. The eighth subject in experiment one had left-eye dominance, and we are left to speculate as to the eye dominance of the eighth subject in experiment two. Left-eye dominance in right-handed individuals is rare and generally implies a variable eye dominance (Rogers & Bradshaw, 1993). This may be the case with the eighth subject in experiment two, in which case the data should have been excluded from the study as their data would not have been accurate.

The dependent measures raised another area of concern. The time in contact with the object was defined as the "point during the deceleration phase of the reach that the index finger was 2.0cm above the table surface subtracted from movement duration" (Servos & Goodale, 1994, p121). It is not reasonable to assume that subjects are in contact with the object at this point. Point of contact refers to just that, the point at which contact is made with the object. There is no allowance for the fact that subjects way have positioned their hand at 2.0cm above the table in preparation for contact with the object

which, on the basis of the results from the trial involving monocular vision, would not be an unreasonable suggestion.

The authors have compared the two graphs produced in the experiments. However, if they required their conclusions to be more than hypothetical it would have been necessary to ensure the axes on the graphs were equal, this way the differences and significant findings could have been highlighted. Of course this brings us back to the issue of statistical analyses. In the discussion the authors say that maximum grip aperture varied as a function of initial viewing conditions, larger under initial binocular vision than under initial monocular vision. It is not possible to say this when no statistical analyses have been conducted on the two sets of data. This lack of analyses on the data is a problem for the drawing of conclusions as only inferences can be made where there is no statistical evidence to back up any claims. However, had they referred to Cumming et al (1991) they would have found support for their claims, as Cumming et al state "Changes in ocular convergence do alter perceived shape, suggesting substantial changes in the subject's scaling of horizontal disparities." (1991, p411).

Some more discrepancies in the discussion of the results must be highlighted. The authors say that the deceleration time can be "entirely accounted for" (Servos & Goodale, 1994, p125) by the time spent in low-velocity phase, due to this not differing within experiments. This did not differ within experiments, but it did differ between the two experiments: this should have been highlighted even though no statistical analyses had been carried out, this could possibly have been quite a significant finding. Also the authors state that the magnitude of peak deceleration does not differ across conditions. What they fail to mention is that they are drawing conclusions from data not presented in the results section. There are no figures to show magnitude anywhere in either of the tables, if they wish to discuss this feature of their results they should have included the necessary data.

The conclusions the authors have drawn are actually accurate, however, they should have used the same subjects in both experiments and thus been able to statistically compare the results.

References

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