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RESEARCH ARTICLE

Visually guided, aimed movements can be unaffected by stimulus–response uncertainty

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Abstract Response times generally increase linearly with the logarithm of the number of potential stimulus–response alternatives (e.g., Hick's law). The ubiquity and theoretical importance of this generalization make exceptions particularly interesting. Recently, Kveraga et al. (Exp Brain Res 146:307, 2002) added a third to the two previously known exceptions, demonstrating that saccade latencies were unaffected by stimulus–response uncertainty. They suggest that visually guided saccades are exceptional, because these movements can be automatically selected using a privileged pathway: the topographically organized regions in superior colliculus that convert spatially coded visual activity into spatially coded motor commands. We report that visually guided, aimed hand movements also are unaffected by both stimulus–response uncertainty and stimulus–response repetition. A second experiment demonstrated that this lack of an uncertainty effect persists for equiluminant stimuli. This result suggests that posterior parietal cortex is not the privileged pathway eliminating stimulus–response uncertainty for hand movements. Because hand movements are not guided by mechanisms in the superior colliculus, our results cast doubt on the privileged-pathway hypothesis, at least for hand movements. Instead, the absence of stimulus–response uncertainty may occur only in tasks that do not require the stimulus to be associated with a response effector and that have high stimulus–response compatibility.

Keywords Cognitive science · Perception · Motor skills · Uncertainty · Parietal lobe

Introduction

Merkel (1885) is usually cited as providing the first demonstration that, when in a choice-reaction procedure, the appearance of one of a set of N (typically ranging from 2 up to 8 or 16) stimuli indicates to a human participant which of N possible responses should be produced, response latency increases with N, the number of stimulus–response pairs. This latency increase is often labeled an uncertainty effect. Typically, the latency increase is a linear function of the logarithm of N. This relationship is often referred to as the Hick–Hyman law after the authors of two early papers that proposed a logarithmic formulation to quantify the uncertainty effect (Hick 1952; Hyman 1953). Although disagreements continue concerning how best to quantify this relationship (e.g., Pellizer and Hedges 2003) and there are disputes as to its source, the existence of this increase has been an important constraint for the development of theories of choice reaction time (e.g., Usher and McClelland 2001) and so examples of instances in which this uncertainty effect does not hold may be of particular theoretical interest.

Because much of the research on this topic is more than thirty years old, the meta-analysis of Teichner and Krebs (1974) summarizes most of what is known about this relationship. Their analysis identifies two factors that modulate the uncertainty effect—practice and the combination of stimuli and responses studied—and considers other factors that they find to be of less importance—e.g., stimulus probability and temporal

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uncertainty concerning the stimulus. Because substantial data were available, Teichner and Krebs focus particular attention on studies involving a combination of low to moderate amounts of practice and four stimulus–response pairings constructed from two types of stimuli—one of an array of lights or one visually displayed digit from a limited set—and two types of responses—a vocal response or a keypress response; in all cases the particular mapping between the stimuli and responses was that expected to be ‘‘natural’’ or ‘‘normal’’ for participants. The key result from this meta-analysis (contained in Fig. 1 of Teichner and Krebs 1974) is that for three of these pairings—all but the condition in which participants made vocal responses to visually presented digits—there was an increase of approximately 140 ms for each $\log_2$ unit increase (i.e., doubling) of N. The intercepts for these three conditions also differed by over 200 ms, with the fastest condition being the one in which a stimulus light was responded to by a keypress and the slowest condition being the one in which vocal responses were made to one of an array of lights. Following the suggestion of Welford (1960, 1968), Teichner and Krebs conclude that the additive difference across conditions represents a translation stage that operates between stimulus categorization and response selection and thus is different from the uncertainty effect that is of primary interest.

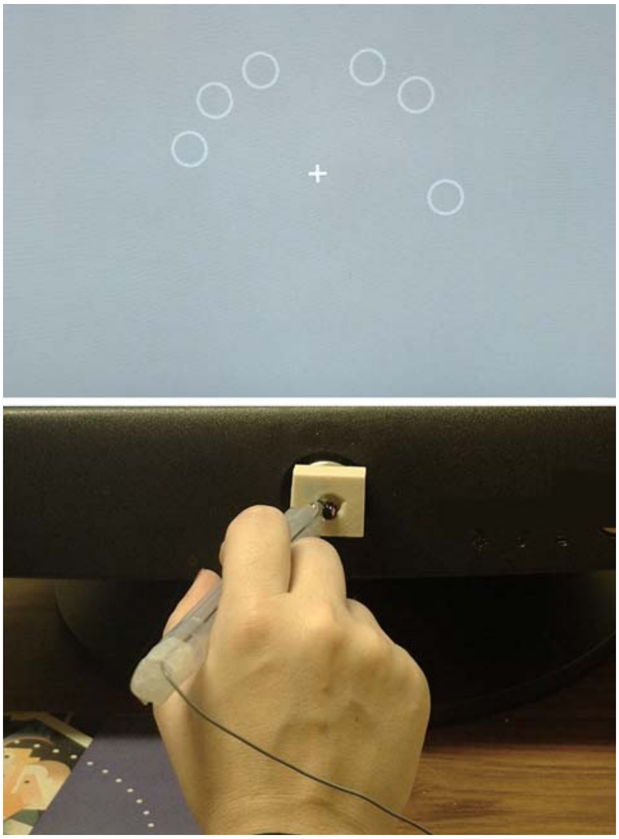

Fig. 1 Photograph showing the experimental setup. The display screen shows the fixation point and circles marking potential targets for a trial with $N = 6$—two of the eight locations are empty in this condition. The stylus is being held in the indentation of the starting point. At the tip of the stylus is an infrared light emitting diode whose position can be tracked by the OptoTrak cameras (not shown)

Against this background, the data that Teichner and Krebs (1974) summarized for the condition in which participants responded by vocally naming a visually presented digit provides an important contrast, given the subject of this paper. In their summary of four previously published experiments using this condition, the increase in response latency as N increased from 2 to 9 was effectively zero. Teichner and Krebs explain this discrepant result by noting that digit naming is a highly developed skill in the adult population, suggesting, as Mowbray and Rhoades (1959) had originally, that it is extensive, prior experience of participants with this stimulus–response combination that is responsible for the different pattern of results observed for it. This then is an invocation of what we will call the *practice* hypothesis. This hypothesis receives additional support from a second meta-analysis reported by Teichner and Krebs (1974) in which they extrapolate from data of six experiments involving extensive practice (~10,000 trials) of the task using digits as stimuli and keypress responses, one of the combinations described previously as producing a large uncertainty effect. This analysis suggests that, in this task also, the uncertainty effect might disappear—i.e., latency might become effectively independent of N—after roughly 100,000 trials.

Kveraga et al. (2002a, b) have recently described another situation, involving saccades as the response to lights as the stimulus, in which the latency to initiate a response was also observed to be independent of N. An obvious question is whether these two exceptions arise from a single mechanism. It seems possible that extensive practice is the factor underlying both of these demonstrations. Certainly, making a saccade to foveate a visual stimulus, appearing in the periphery, is an often repeated response. This suggests that for other situations involving highly compatible aiming movements to visual targets—e.g., pointing movements made by the hand/arm—latency ought also to be independent of uncertainty.

Kveraga et al. (2002a, b) suggest, however, that a different, more specific, mechanism explains the absence of an uncertainty effect for eye movements, and that this mechanism does not imply that there would be a similar absence of an uncertainty effect for other movements. Thus, they note that, only for eye movements, is there a 1-1 relation between the retinal position of a visual stimulus and the movement necessary to acquire that location as a target. So, for example, to point the hand at a target given its retinal position, the motor system must also take into account, at least, the orientation of the eyes within the head and the orientation of the head relative to the torso.[1]

Given the unique relation between retinal stimulation and eye movements, the importance of eye movements made to foveate objects in the visual periphery, and the existence of mechanisms in superior colliculus known to mediate eye movements based on retinal stimulation (Sparks and Hartwich-Young 1989), Kveraga et al. (2002) postulated that saccades in a choice reaction time task do not exhibit effects of uncertainty because of mechanisms in the superior colliculus that, for monkeys, have been shown to map retinal positions of stimuli into eye-movement control signals (Schiller and Stryker 1972; Albano 1996). Kveraga et al. (2002) suggest that these mechanisms allow the more general, movement-control mechanisms, which are normally invoked as part of response selection and that produce the uncertainty effect, to be bypassed when the required response is a saccade to the target. This, they go on to suggest, may be a privileged mechanism for this stimulus–response combination: ‘‘Even though humans are readily capable of precise visual control over manual movements, it is not clear that specific sensory inputs should be associated with specific finger movements’’ (p. 307). From this perspective, the mechanisms used to support reading, which presumably underlie the lack of uncertainty effects when spoken digits are the responses for visually displayed numerals, might also be seen as the result of a privileged mechanism, the mechanism that directly maps a visual digit onto its name.

In their summary, Teichner and Krebs (1974) identified two aspects of the stimulus–response combination used in an experiment that had an effect on movement latency: (a) the particular combination of the stimulus and response dimensions, what Kornblum et al. (1990) have more recently labeled *set-level compatibility* and (b), given the sets of stimuli and responses, the way that particular stimuli are mapped to particular responses, what Kornblum et al. (1990) labeled, *element-level compatibility* . It is set-level compatibility that, presumably, underlies the effects discussed thus far. An elegant demonstration of the effects of element-level compatibility in this context is provided by Dassonville et al. (1999). They collected data from four conditions in which participants used a joystick to control movements of a cursor to one of 2, 4, or 8 potential, circular annuli arrayed in a circle around the starting point on a CRT monitor. The four conditions were constructed by pairing two methods of cuing the target—in the *Spatial* condition, the annulus indicating the target was replaced by a filled circle; in the *Symbolic* condition, one or two letters indicating a direction (i.e., N, NE, E, SE, etc.) was displayed at the starting point—and two methods of mapping the target indicator onto the actual target—in the *Toward* condition the movement was to the indicated annulus; in the *CCW* condition the movement target was in the direction 90° counter-clockwise from the indicated annulus. The primary effect of both manipulations was to influence the slope relating response time and N, the number of response alternatives. This slope was smallest, but still reliably positive in the Spatial-Toward condition, which was expected to have the highest stimulus–response compatibility. It was largest, approximately 350 ms per $\log_2$ unit (based on values read off of the figures in this paper), in the Symbolic-CCW condition, which was expected to have the lowest stimulus–response compatibility.

Leonard (1959) provides a third, well-known claim to describe a situation in which there was no uncertainty effect. In that experiment the fingertips were stimulated by a 50 Hz vibration and the required response was to depress the vibrating armature. Leonard did not find a reliable increase in reaction time with N, the number of tactual choices. More recently, however, ten Hoopen et al. (1982), studied this same task with a more systematic design and better equipment. They found that the results for this task depend on stimulus characteristics that determine which of the two tactile sensory systems is stimulated; because the amplitude threshold for these two systems varies across frequency in quite different ways, it is possible to stimulate either system alone or both systems simultaneously. Tactile stimulation that was substantially supra-threshold but, because it was lower in frequency, only activated the Meissner receptors, resulted in uncertainty effects of about 60 ms per $\log_2$ unit increase in N. However, vibrations that were higher in frequency, amplitude, or both, and thus that stimulated the Pacini receptors, alone or in combination with the Meissner receptors, produced much smaller effects of uncertainty: judging from the data figures, slightly more than 10 ms per $\log_2$ increase in N. Although they do not provide a clear-cut case with no uncertainty effect, these data provide an interesting challenge for the three explanatory

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[1] For both eye movements and hand movements, this analysis presupposes that the movement can be planned without reference to the initial position of the hand or the eye.

hypotheses we have considered. It seems difficult to argue that the compatibility of the stimulus–response mapping has changed across these stimulus conditions or that the contrast here is the result of differences in practice or prior experience. One could argue that the Pacini receptors participate in a privileged pathway that does not include the Meissner receptors. In an observation that might support this position, the authors note that when only the Meissner system was activated, participants report that the location of vibration (i.e. the finger) is hard to establish even though the stimulation is well above threshold. The authors' preferred explanation invokes the concept of ideomotor compatibility (Greenwald 1970): this form of compatibility occurs when an action has to be selected and the response code of that action is activated by stimuli that resemble the sensory feed-back from the action itself. In this case, ten Hoopen et al. suggest that the sensory result of making a response is more similar to the stimulation that activates Pacini receptors and this ideomotor compatibility facilitates the response of the finger stimulated.

Returning now to the finding of Kveraga et al. (2002), that there is no uncertainty effect on choice reaction-time for eye movements made to visual stimuli with a compatible mapping rule, it is difficult to see how ideomotor compatibility could play a role; however arguments for each of the other three mechanisms that we have considered—practice, high stimulus–response compatibility[2], and privileged pathway—can certainly be made. As discussed above, Kveraga et al. lean toward the privileged-pathway explanation for their result and suggest, based on this interpretation, that the same lack of an effect of uncertainty would not be found for hand/arm movements using a similarly compatible stimulus–response mapping. One goal of this paper is to study arm/hand pointing movements under conditions that would maximize the probability of eliminating the uncertainty effect to evaluate this suggestion.

Several previous studies using joystick-mediated responses have found effects of uncertainty that were greatly reduced, but still reliably present. The Spatial-Toward condition in Dassonville et al. (1999) is one example. Most recently, Berryhill et al. (2005) found a significant 10 ms effect going from 2 to 8 choices. In a study with several intriguing methodological differences, Pellizer and Hedges (2003) also observed a small, but reliable uncertainty effect.

These near misses for joystick-mediated movements, in which the uncertainty effect is greatly reduced but not eliminated, raise almost as many questions as they answer. Like eye movements, the mapping of individual stimuli and responses in these tasks appears highly compatible. Also, given the ubiquity of mice and similar computer-related pointing devices, we would, at least in this day and age, expect most participants to have extensive practice with movements controlled in this way already. Given the other results reviewed above, it seems reasonable to expect that, if both of these pre-conditions did not hold, then the slopes relating latency to N in these experiments would not be nearly as low as they are. Indeed, when Dassonville et al. (1999) had participants use the less compatible and, presumably, less practiced CCW mapping instead of the "Toward," direct mapping, the slope of the uncertainty effect increased by over 100 ms per $\log_2$ item. However, if these movements are practiced and highly compatible, why does a small uncertainty effect remain? For saccades, Kveraga et al. (2002) found a non-significant slope for the uncertainty effect that was actually slightly negative (–1.5 ms per $\log_2$ item).[3]

One possible explanation for this difference is the privileged-path hypothesis favored by Kveraga et al. (2002). An alternative is that, although these joystick-mediated movements have a high level of spatial

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[2] When they studied anti-saccades, eye movements made in the direction opposite where the target appeared, Kveraga et al. (2002) found a large uncertainty effect.

[3] In this and several other papers (Berryhill et al. 2004, 2005), these authors emphasize the importance of the comparison between the no-uncertainty condition, $N = 1$, and the conditions normally studied in these experiments, with $N > 1$. Such a comparison is difficult to make convincingly because these conditions potentially differ in much more than the quantitative level of S-R uncertainty: the step from one to two also involves the transition from simple to choice reaction time. In the simple reaction time case, the stimulus can function simply as a "Go" signal: there is no necessity for the participant to identify the stimulus or for it to be processed at all. Also, in experiments such as this, with stimuli arrayed about the fixation, participants may be tempted to ignore the indicated fixation point and foveate the single possible stimulus location, when the response is not an eye movement; of course, when the response is an eye movement and so eye position before the stimulus onset is being monitored, participants cannot use this strategy to reduce the latency when $N = 1$. An additional issue is that, unless there is a variable fore period or catch trials, participants may anticipate the onset of the stimulus. Any of these three differences between simple and choice reaction time would lead to faster responses for $N = 1$ than might otherwise be expected, as has been found in virtually every experiment reporting data of this type. The fact that there is no difference in these conditions with saccades or smooth pursuit eye movements probably reflects more about differences in the eye-movement control system or participants' strategies than it does about the response–selection processes that are typically the focus in studies examining uncertainty in choice-reaction time tasks.

compatibility, they are not, in fact, as compatible as eye-movements in which the effector being moved is, itself, moved directly to the target. Thus, the first experiment will study a task, modeled after that of Kveraga et al. (2002), in which the participants literally point to the stimulus-target on the display screen.

Experiment 1

Experiment 1 had as its goal to determine whether we could produce conditions under which there would be no uncertainty effect for aimed hand movements. To this end, we used a design that minimized the conditions studied. Because we were looking for no effect in this research, we took steps to balance stimuli across levels of *N* that are more extensive than those found in many studies. Those studies appear often to have been modeled on older designs from experiments in which large uncertainty effects were found.[4] Although, as experimenters, we usually work hard to eliminate confounding factors, small, second-order effects may be more easily tolerated in experiments observing effects of 100–200 ms per $\log_2$ item, than in an experiment such as this one that hopes to exclude the possibility of effects smaller than 10 ms per $\log_2$ item.

Methods

Participants

Ten naive participants took part in a single session lasting approximately 90 min; five were females and five were males. They were recruited through ads posted on the UC Irvine campus; participants were required to have vision corrected to 20/20 and be right-handed. They were paid $8 per hour plus bonuses. The UCI Institutional Review Board approved the experimental protocol followed in this report.

Apparatus

A standard PC running a custom application written in C was used to control the experiment, present stimuli, and record responses. The participant used a hand-held, lightweight stylus to touch the target stimuli displayed on a Dell Model M991 CRT display operating with a resolution of 1024 × 768 pixels and a 60 Hz refresh rate. An Optotrak Model 3020 tracked an infrared emitter on the tip of the stylus recording *x*/*y*/*z* position at 100 Hz. Other emitters mounted on the display case established the frame of reference used to record the position of the emitter on the stylus. There was a small indented plastic disk, mounted on the monitor case below the center of the display, which served as the home position for the stylus at the start of each trial.

Design

Five within-participants, independent variables were manipulated in this experiment and three dependent variables were extracted from the complete movement trajectories that were collected. The primary independent variable was the number of possible targets that were presented to the participant. For reasons that will become clear later, two levels of this independent variable were studied: $N = 2$ and 6. Of the other four independent variables, two—the target position and whether the target was a repetition—will be emphasized in the data analysis. The two remaining independent variables—stimulus arrangement and the cue onset delay—were included primarily to help balance stimulus factors and ensure that participants did not begin movements before processing the cue indicating the movement target; these two factors will not be emphasized in the description of the results.

The three reported dependent variables are the latency to initiate the response, the duration of the response, and the distance from the center of the target to the tip of the stylus at the end of the movement. These three primary dependent measures were extracted from an analysis of the full movement trajectory of the stylus that was saved for each trial.

Trials were also categorized according to whether the target was in the same location as the target on the previous trial to generate the Repetition factor. This variation is important because, in a purely random sequence, repetitions are three times more likely to occur when there are only two stimuli than when there are six stimuli, and previous research has highlighted the importance of stimulus repetitions in reaction time experiments (for this paradigm, Kornblum 1969, is particularly relevant).

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[4] One interesting exception to this generalization about previous designs is the experiment reported by Pellizer and Hedges (2003). Although we feel that much can be learned from the radically different design used in this paper, we decided not to adopt it because it involves randomizing the sets of possible target locations from trial to trial rather than blocking these sets as is traditionally done. Until we have had an opportunity to convince ourselves that there are absolutely no important implications of this choice, we felt that adopting such a design would unnecessarily complicate the comparisons that we wish to make with previous research.

Stimuli were distributed across eight possible locations arranged in a semicircle 40 mm from the fixation mark at angles of ±15°, ±45°, ±75°, and ±105° from vertical. The fixation mark was 130 mm above the home-position location mounted on the display screen. Thus, the distance from the stylus-home position to the targets varied from 127 to 170 mm. Figure 1 shows an example of this arrangement with the stylus in the home position and six (of the eight potentials) targets being displayed.

A session was broken into 36 blocks of trials, which alternated between blocks with $N = 2$ and 6. The first four blocks were designated as practice; the data from these blocks are not included in the reported analyses, although in all other ways they were identical to the 32 test blocks. Throughout each block, the set of possible targets did not change. The two or six possible targets in a block were always split across the eight screen locations so that an equal number of possible targets were to the right and left of vertical. This constraint was imposed to make it more likely that participants would keep their eyes on the central fixation mark until the actual target for a trial was indicated. Given this constraint, there are exactly 16 ways of arranging either two or six possible targets across the eight locations. The design choices of eight locations and two or six possible targets were made to allow for variation in stimulus arrangements while keeping the number of possible arrangements manageably small so that they could be exhaustively sampled. Each of these possible stimulus arrangements was used once in the 32 test blocks. The order of the stimulus arrangements was randomly permuted for each participant, subject to the constraint of alternating $N = 2$ and 6 between blocks. Stimulus arrangements were randomly selected for the four practice blocks, with the only constraint being that they must be different from one another.

A block consisted of 18 error-free trials. Unbeknownst to the participant, the first four trials in a block were designated as practice; these were intended to give the participant experience working with the target configuration for that block. Of the remaining 14 trials, two were randomly selected as catch trials; on catch trials no actual target was indicated and any movement by the participant was an error. Catch trials were included, along with variation in the cue onset time, to discourage participants from beginning the movement before the target stimulus had been processed. For blocks with $N = 2$, each of the two possible targets was randomly selected as the actual target six times; for $N = 6$, each possible target was randomly selected as the actual target twice. Thus, target position was balanced across the target positions within blocks, and, across blocks, target position was balanced across each of the eight locations. If, at the end of the movement, the stylus was not within the target or some other error occurred on the trial, an extra trial was added to the block and the trial type that produced the error was randomly reinserted into the sequence of trials remaining in the block.

The variation and balancing of target position and stimulus arrangement reflect our concern that factors such as the distance of the target from the starting point, the spatial relation of the target and the nearby distracters, and how the distracters are bunched could influence any, or all of (a) the time necessary to become aware of the target, (b) the time to initiate the movement, (c) the time to make the movement, or (d) the accuracy of the movement. Without substantially more knowledge about the workings of these factors than we currently have, it is impossible to completely eliminate their influence; however, our design goes a long way toward ensuring that their influence will not confound the comparison of primary interest: i.e., the comparison between the conditions with low and high uncertainty.

Because the stimulus timing was constrained by the 60 Hz refresh rate of the display, cue onset delay was randomly selected on each trial from a discrete approximation of a truncated exponential distribution. Based on this distribution, the cue was presented up to 100 ms before the expected time and as much as 350 after that time. The randomization of the cue onset delay was done independently of the randomization of the other four factors.

Procedure

Each session began with a calibration process that estimated a spatial, linear transformation that mapped the x/y coordinates of pixels on the computer monitor into the three-dimensional coordinate system used by the OptoTrak to register stylus position. This mapping was found to depend not only on the position of the display relative to the OptoTrak, but also, because of the thickness of the glass covering the phosphors on the display screen, on the height and positioning of the participant. The data required to compute this transformation were obtained by having the participant touch the stylus to nine small squares displayed at known pixel coordinates on the screen. The OptoTrak coordinates of the stylus placed at the home position mounted on the display case were also collected.

After the calibration, the participants began the experiment itself. Before starting the trials in each block, the participants were presented with a screen that required them to confirm the accuracy of the stylus mapping by moving the stylus to each of the possible targets (either 2 or 6) for that block. A green cross representing the registered cursor position was displayed on the computer monitor. This procedure not only allowed the experimenter to verify the continued accuracy of the calibration, but it gave the participant practice moving to the target locations to be used during the block. If a problem with the calibration was discovered during this procedure, the full calibration procedure was repeated; this was necessary only once during the experiment.

Each trial began with the screen blank except for a message to move the cursor to the starting location. After the stylus had remained within 3 mm of the starting location for 250 ms, this message disappeared, and a set of two or six white circles, 7 mm in radius, appeared at each of the possible target positions, determined by the stimulus arrangement in use for the block. At this point, the fixation display sequence also began. The fixation mark consisted of 1 cm intersecting horizontal and vertical lines. For the first 500 ms the fixation was not displayed, then it was displayed for 500 ms, turned off again for 500 ms, displayed a second time for 500 ms, and then finally turned off. This pattern established a rhythmic sequence. The cue-delay distribution was centered to coincide with the time when the fixation would next have turned on given this rhythmic sequence. The flashing fixation served both to establish an expected time for the cue onset and to draw visual attention to the fixation point.

During the fixation sequence, the position of the stylus continued to be monitored. If, at any time up until 150 ms before the expected cue time, the stylus left the starting area, a message explaining the problem was displayed and the trial was restarted. If the stylus moved out of the starting area after this time and before 100 ms after the cue was displayed, or, for a catch trial, any time up to 500 ms after the expected cue time, the movement was labeled an anticipation error, an error message was displayed and a new trial was randomly selected.

To cue the response, the dark inside of one of the potential target circles was filled in to match the white outline of the circle. The participant was instructed to move the stylus quickly to a point within that circle. The instructions, like those of a discrete Fitts task, emphasized minimizing the total time to complete the movement, made no mention of the distinction between movement latency and duration, and treated all movements ending within the target circle as acceptable, while all movements ending outside the target circle were labeled errors. The movement was determined to have begun when the stylus moved more than 3 mm in any direction from the starting location. The movement was determined to have ended when the stylus came within 2 mm of the surface of the display. (The calibration procedure estimated the curvature of the display surface and this curvature was taken into account when determining the end of the movement.) Movement trajectory data were retained starting 500 ms before the target cue onset and ending 500 ms after the movement was determined to have ended.

After each movement, a message was displayed giving the total movement time in hundredths of a second. In addition, a small marker was displayed at the location on the screen determined to be the movement endpoint. If the endpoint was outside of the target region, the message "MISSED TARGET" was also displayed. This feedback stayed on the screen for 2 s. At the end of this period the display was cleared and a new trial began.

After the last trial in each block, the display was cleared and a message was presented summarizing for the participant his/her performance and providing a score for the block. In addition to the score, this summary included the average total movement time, in hundredths of a second, the count of the number of errors—i.e., trials on which the movement missed the target—and the count of the number of anticipation errors. The score was calculated as the sum of the average total movement (in hundredths of a second; typically about 60), three points for each missed target and five points for each anticipation error. The participant received a bonus of $.10 for each block for which the score was less than or equal to a target score for that block. The bonuses were designed to reward good performance. Separate target scores were maintained for blocks with $N = 2$ and 6, and these scores were adjusted from block to block. Let T_i be the target score for block i and S_i the score for that block. T_1 was always set to 100, a value larger than the expected score for the first block of either condition. Subsequent target scores were computed according to a recursive formula.

$$T_{i+1} = \begin{cases} T_i - 0.67(T_i - S) & \text{if } T_i \geq S_i \\ T_i - 0.25(T_i - S) & \text{if } T_i < S_i \end{cases}$$

Data analysis

In most studies of choice reaction time, the initiation of a response—e.g., a button press—and its completion are not measured separately; instead a single event time is collected and this measure, labeled response time, is used as the primary dependent variable. When the responses are aimed movements, a period of several hundred ms or more separates two time points typically, if not unequivocally, identified as the start and end of the movement. The time from the stimulus onset to the first of these events is usually labeled the movement latency and the time between these two events is labeled the movement duration. Because the focus in choice reaction time studies is typically on processes up to and including response selection, but excluding movement control processes, movement latency is the most appropriate analog, in this study, of the reaction times that typically are reported. In addition to movement latency, however, we also present summaries of movement duration, endpoint error, the distance from the endpoint of the movement to the center of the target disk, and movement trajectories.

Although we also will report a variety of ancillary analyses for each measure, an ANOVA is the primary analysis reported for each of these three measures. This 2×2 ANOVA had two within-participants factors: N, the number of possible targets, and whether the stimulus–target combination was a repetition of that of the previous trial. This analysis collapses across three other variables: the configuration of the possible targets among the eight locations, the actual target location, and the cue-onset delay. This approach is appropriate for two reasons. First, these three variables were included in the design, not because of their intrinsic interest, but to help control potential confounding effects. Second, although there were reliable main effects of these variables, more complete analyses show that collapsing over these three variables does not change the outcome for the two variables of interest.

Based on the examination of reaction time distributions obtained for pilot data, we determined that it would be appropriate to eliminate trials from the analyses with latencies greater than 1,100 ms.

Results

Errors

There were few anticipation errors, that is movements made on a catch trial or movements that began within 100 ms of the stimulus onset on a normal trial: for $N = 2$, 1.3% for $N = 6$, 0.7%; after an arcsine transformation, this difference is not statistically significant [$t(9) = 1.69$, $P = 0.122$]. This suggests that the inclusion of catch trials and the cue-delay manipulation induced the participants to wait for the cue stimulus before initiating a response.

Movements ending outside of the target occurred more frequently for $N = 2$, 7.6%, than for $N = 6$, 5.4%; after an arcsine transformation, this difference is statistically significant [$t(9) = 2.71$, $P = 0.022$]. That there would be an average of 6.5% errors is, perhaps, not surprising given the relative difficulty of these movements (the ratio of target distance to target width ranged from 9 to 12). That there were slightly more errors for $N = 2$, may reflect a tendency of participants to make these movements somewhat more quickly, a speed–accuracy tradeoff that would increase the likelihood of finding a movement latency increase going from $N = 2$ to $N = 6$.

No trials had to be excluded because of latencies longer than 1,100 ms.

Effects of the variable cue-onset delay

Although not central to the questions being asked in this paper, we examined the effects of cue-delay for two reasons: to ensure that these effects were consistent with previous observations and to determine whether this variable needed to be included in the primary analyses, given that cue-delay was not systematically balanced across the levels of the other factors. The systematic effects of the varying cue delay were largest in the latency data, small but still present in the duration data, and virtually non-existent in the endpoint error data. Although there were substantial individual differences in the magnitude and the precise form of the cue-delay effects in the latency data, they were generally well fit in the data of each participant using a quadratic function. In the average data, the function describing the estimated relative effect of cue delay has an asymmetric U-shape. Relative to the time point established by the rhythmic fixation sequence, cues occurring 100 ms early resulted in latencies at the average; cues at the established time point resulted in latencies 6 ms faster than average; cues 50 ms later produced the fastest latencies, 7 ms faster than the average; cues occurring 350 ms after the established point produced the slowest latencies, 20 ms slower than average.

The original analysis plan for these data contemplated the inclusion of covariates to account for the variation in cue-delay. Given the design, such covariates could be expected primarily to reduce residual error. However, because these covariates apply at the level of individual trials, but the rest of the data analysis is conducted at the level of within-block means, including these covariates adds substantially to the complexity of the data analysis. Thus when it appeared that inclusion of these covariates had, at most, minor impact on the results, we elected not to include these terms in the analyses that we now report.

Movement latency

The results of the 2 × 2 ANOVA on the latency data were not significant for either main effect: for the main effect of *N*, $F(1,9) = 1.85$, $P = 0.207$; for the main effect of Repetitions, $F(1,9) = 0.002$, $P = 0.963$. However, there was a significant two-way interaction of *N* × Repetitions: $F(1,9) = 5.096$, $P = 0.050$.

Consistent with the lack of a significant main effect of *N*, the difference of the average data for $N = 6$ minus that for $N = 2$ is 2.9 ms with a 95% confidence interval that extends from –1.9 to 7.9 ms. Considered as a slope, this mean is 1.8 ms per $\log_2$ item and even the largest estimate consistent with the confidence interval corresponds to a slope of 5.0 ms per $\log_2$ item.

The significant interaction of *N* × Repetitions complicates a straightforward interpretation of this main effect. As Fig. 2 shows, there is no effect of *N* when the target on a trial is an immediate repeat of that on the previous trial [$\Delta = 0.8$ ms, $t(9) = 0.242$]. However, when the target on a trial is not a repetition of that on the previous trial, there is a small, but significant increase in latency from $N = 2$ to 6 [$\Delta = 6.7$ ms (2.6 ms per $\log_2$ item), $t(9) = 3.230$, $P = 0.021$; note that this planned comparison is statistically significant even using $\alpha = 0.025$ dictated by a Bonferroni adjustment for multiple comparisons (Maxwell and Delaney 2004)]. It generally is important to evaluate the effects of *N* broken out by levels of Repetitions, because, as noted by Kornblum (1969), in a balanced design such as this one, repetitions are roughly three times more likely for $N = 2$ trials than for $N = 6$ trials.

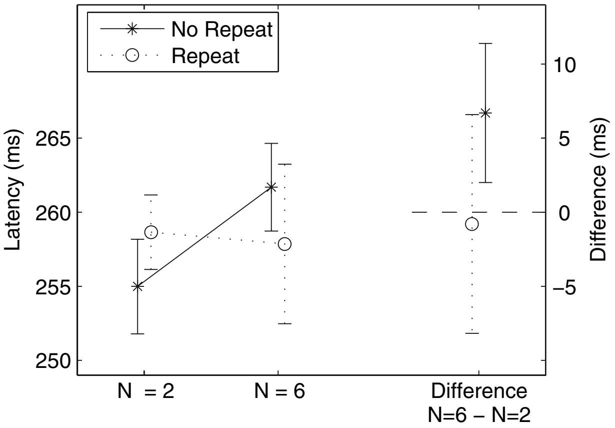

Fig. 2 The four points on the left show the data for the *N* × Repetitions interaction. The error bars on each point are 95% confidence intervals computed separately for the data in each of the four conditions after the estimated main effect of participants was removed. The two data points on the right show the mean difference for $N = 6$ minus $N = 2$ separately for those trials in which the stimulus–response combination is a repetition of the previous trial (*circles*) and those trials with no repetition (*asterisks*). The error bars are 95% confidence intervals for the difference computed from a correlated samples *t*-test

Movement duration and endpoint error

Within the 2 × 2 ANOVA of the movement duration data there was not a statistically significant result either for the main effect of Repetitions or for the interaction of *N* × Repetitions: for the main effect of Repetitions, $F(1,9) = 0.452$, $P = 0.518$; for the two-way interaction of *N* × Repetitions, $F(1,9) = 0.104$, $P = 0.754$. However, there was a significant main effect of *N*, $F(1,9) = 15.719$, $P = 0.003$.

Looking at the main effect of *N*, for $N = 2$, the average movement duration was 313 ms, and, for $N = 6$, the average movement duration was 322 ms. The 9 ms difference has a 95% confidence interval from 4 to 14 ms, so, although this difference is statistically reliable it is also quite small.

A similar analysis looking at the endpoint error—i.e., the average distance between a target center and the endpoint of movement directed to that target—revealed no statistically significant effects: for the main effect of *N*, $F(1,9) = 0.002$, $P = 0.964$; for the main effect of Repetitions, $F(1,9) = 0.269$, $P = 0.617$; for two-way interaction of *N* × Repetitions: $F(1, 9) = 0.001$, $P = 0.978$. For both $N = 2$ and 6, the average endpoint error was 3.29 mm; the targets were 7 mm in radius. The endpoint difference between $N = 2$ and 6 was 0.00086 mm with a 95% confidence interval from ±0.22 mm.

Movement trajectories

The left-hand panel of Fig. 3 shows, for a single, typical participant, the average trajectory to each of the eight possible target locations. The trajectories, which vary considerably between participants, are indistinguishable across the $N = 2$ and 6 conditions. Each average trajectory was created by first estimating, for each of the 48 movements to a target, the *x*- and *y*-coordinates of the stylus at each of 21 time points spaced equally between the moment when the movement was judged to have begun and when it was judged to have ended, and then averaging. Thus, the 21 points on each average trajectory represents an average across 48 movements. What is of interest here is whether the movement trajectories diverge quickly or whether participants utilized a quick-movement strategy to minimize movement latency. According to this strategy, the participant initiates a generic movement, in the general direction of the targets, at the onset of the

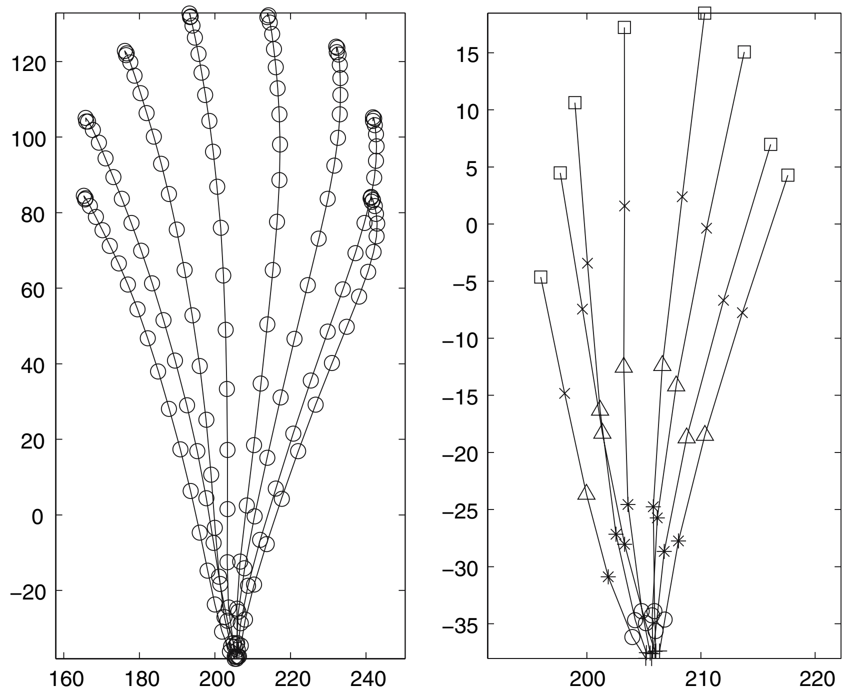

Fig. 3 *Left panel*: Averaged movement trajectories to each of the eight locations separated from the data of a typical participant. Each averaged trajectory represents data from 48 movements. Before averaging, individual movement trajectories were time normalized and resampled into 21 points so that the interval between two successive points represents about 5% of a complete movement. *Right panel*: The first six data points of the averaged trajectories shown on the left. Plus signs mark the points identified as the start of each movement. Open circles mark points at which only one-twentieth of the movement was complete. Open squares mark points at which one-quarter of the movement is complete

stimulus and then refines the movement to reflect the actual target. To focus on this issue, the right-hand panel of Fig. 3 shows just the first six data points for each of the average trajectories: i.e., from the start of the movement to the point, temporally, one-quarter of the way through the movement. Although it is clear from this figure that the mean trajectories have diverged within the first quarter of the movement in a way that makes sense given the layout of the targets, this figure does not provide the information necessary to assess these differences against the variability between movements or across participants.

To address this issue more systematically, a multivariate, classification analysis (Krzanowski 1988) was applied, separately, to the data from each of the 21 time points, plotted in Fig. 3, for each participant.[5] This analysis shows that even for the first time point after the start of the movement, a point at which only one-twentieth of the movement was complete (the points marked by open circles in the right-hand panel of Fig. 3), an average of 22% of movements can be correctly classified (across the 10 participants, this ranged from a minimum of 18% to a maximum of 30%); after one-quarter of the movement (the points marked by open squares in the right-hand panel of Fig. 3), 53% of movements can be correctly classified (range across participants: 43–67%); after one-half of the movement, 81% of movements can be correctly classified (range across participants: 70–90%). If the data being classified were completely random, we would expect only 12.5% of the movements to be correctly classified. The large initial value of these correct classifications and the speed with which this percentage increases, argue against participants having relied heavily on the quick-movement strategy.

Perfect classification, especially early in a movement, is a strict criterion; although 53% of the movements were correctly classified after one-quarter of the movement was completed, 47% of the movements were incorrectly classified. Thus, it may also be instructive to examine what errors are made when a movement is incorrectly classified. Recall, as shown in Fig. 1, that the stimuli were arranged in the shape of a horseshoe, making it natural to characterize errors in terms of steps going around the perimeter of the horseshoe. This suggests looking at movements classified as going either to the target location or one of the two locations on either side of the target. After one-twentieth of the movement, 51% of the movements meet this criterion (range across participants: 43–67%); after one-quarter of the movement trajectory this increases to 92% (range across participants: 85–99%). These findings provide further evidence against the hypothesis that participants relied heavily on the quick-movement strategy.

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[5] At each point, data triples, consisting of x-position, y-position, and direction of movement in the xy-plane, were extracted from the trajectories recorded for a participant's 384 movements using cubic-spline interpolation. The 48 triples for each target were then fit using a multivariate normal distribution. Based on the eight fits, estimates were computed for the probability of a movement to each target being correctly classified or incorrectly associated with one of the seven other locations.

Discussion

This experiment demonstrates that latency of aimed hand movements can be essentially independent of uncertainty. In this independence, aimed pointing movements appear to be similar to saccades (Kveraga et al. 2002). A slight complication to this result is that there was a statistically reliable latency increase with N for trials on which the target was not a repetition of that on the previous trial. However, the slope describing this increase (1.8 ms per $\log_2$ item) is less than one-fortieth of that typically reported in experiments studying Hicks' law.

This result extends those of other recent studies that reported somewhat larger, but still by the standards of this literature quite small, effects of uncertainty on movement latency for pointing movements made with a joystick in a displaced reference frame (Dassonville et al. 1999; Pellizer and Hedges 2003; Berryhill et al. 2005). One of these studies, Dassonville et al. (1999), has effectively made the point that stimulus–response compatibility is an important determinant of the uncertainty effect. Against the background of this result, it is perhaps not surprising that the direct pointing movements used in this experiment led to a smaller uncertainty effect than those observed using more indirect, joystick-mediated, pointing movements.

This result is also consistent with a recently published result that studied pointing movements (Kveraga et al. 2006). Unfortunately, a primary focus of that paper is the difference between the condition with $N = 1$ and those with $N > 1$, so the data summary does include as much detail about differences with $N > 1$ as it might. In addition, the experimental design ignores the issues of stimulus layout, which are discussed more extensively below, and the reported analyses do not consider the problem of target repetitions. However, reading off of the data displays, it appears that going from $N = 2$ to 8 there was a total latency increase of less than 10 ms and thus a slope of, at most, 5 ms per $\log_2$ item.

In evaluating results obtained using pointing movements as responses, as opposed, for example, to button presses, one concern in the interpretation is whether participants may have engaged in a strategy of initiating the response before they were prepared to make a movement to the specific target on that trial (Smith and Carew 1987). So, for example, participants might try to anticipate the onset of the cue and make a generic initial movement. Alternatively, participants might wait for the cue onset to initiate the generic movement. In either case, further processing to guide the ongoing movement to the actual target would, under this "quick-movement" strategy, only take place after the movement began (and the movement latency period had ended). There are several reasons to be confident that participants did not use this strategy in this experiment.

Specific assurance that participants did not try to anticipate the onset of the target stimulus comes from two elements of the design and procedures used in this experiment: the variable cue-onset delay and the presence of catch trials, in which no target stimulus was presented. These aspects of the experimental procedure along with the observation that there were very few errors of the type in which the participant responded on a catch trial or responded early to the stimulus provide strong evidence that participants did not simply anticipate the time at which the stimulus indicating the movement target would occur.

If participants did not initiate movements prior to the stimulus, it remains conceivable that, having waited for the stimulus, the movement was initiated before movement planning was complete. For example, realizing that all of the targets required a movement upward from the starting point, participants might have tried to minimize movement latency by initiating a generic movement at the onset of the stimulus, and then refining the movement plan while carrying out the movement (i.e., during the movement duration interval). There is some support for this version of the quick-movement strategy in the 9 ms increase in movement duration going from $N = 2$ to 6. Marshalling evidence against this possibility is difficult because the alternatives to be compared are not well specified. Whether participants use the quick-movement strategy or not, movements to all targets start at a single location and must diverge before they end; the question then becomes one of how quickly the movements separated and was this too fast to be consistent with the quick-movement strategy.

The average movement trajectories in Fig. 3 and the classification analysis applied to those trajectories suggest that most, if not all, movements in this experiment reflect their targets from the start. Although these data cannot exclude the possibility that participants occasionally adopted the quick-movement strategy, it seems unlikely that use of this strategy explains the lack of an uncertainty effect in the movement latency data.

This experiment differed from most previous studies of the uncertainty effect in that both target position and the distribution of potential target positions across locations were systematically balanced. We felt that this balancing was particularly important in a study the goal of which was to demonstrate a null effect. Analyses, not reported here in detail, indicate that there were systematic and relatively large effects (10–20 ms) of target location and potential target set. This confirms, as has been noted previously by others (e.g., Semjen et al. 1978), that the precise spatial layout of the stimuli in a choice-reaction experiment can have relatively large effects. Although such effects might be easily lost in the noise for experiments observing effects of over 100 ms per $\log_2$ item, they can be critical in experiments such as this one that are attempting to examine conditions in which there is putatively no effect of uncertainty.

Experiment 2

The Introduction for Experiment 1 discussed three hypotheses that might characterize situations in which there is no uncertainty effect: (a) the practice hypothesis, (b) the compatibility hypothesis, and (c) the privileged mechanism hypothesis. Although participants did not receive extensive practice in Experiment 1, one might argue that they brought from their life outside the laboratory extensive pre-task experience in situations similar to the experimental task and thus that the finding of Experiment 1 is consistent with the practice hypothesis. One might also argue that this result is consistent with the compatibility hypothesis; in fact we suggested earlier that the higher compatibility of this task, compared with a task using joy-stick mediated responses in a displaced reference frame, could result in smaller uncertainty effects.

Experiment 1 does, however, tend to undermine the privileged-mechanism hypothesis offered by Kveraga et al. (2002). The attractiveness of this account rests largely on the presumption that saccades are unique in yielding response latencies that are independent of the number of possible targets. However, four distinct tasks have now been documented to yield no uncertainty effect: the saccade task, the digit-naming task, the vibration-keypress task and the aimed-movement task of Experiment 1. It seems implausible that any two of these tasks can be explained in terms of the same privileged mechanism. Hence, if we are to lean on the privileged mechanism hypothesis to explain all of these cases, we are committed to at least four distinct privileged mechanisms. The results of Experiment 1 weigh especially heavily against the privileged-mechanism hypothesis because of the similarities between the saccade and aimed-movement tasks: both tasks require the use of a single effector (the eyes in the saccade task, the hand in the aimed-movement task) to capture one of *N* possible, spatially distinct, target locations.

Of course, except for Occam's razor, there is not a compelling reason why there might not be several privileged mechanisms underlying this set of results. If one wished to suggest that a variant of the privileged mechanism hypothesis should also explain the data from Experiment 1, then the posterior parietal cortex would be a reasonable place to look for the neural substrate underlying this mechanism. Research in both humans and monkeys (e.g., Flanders et al. 1992; Buneo et al. 2002) suggest that areas of the posterior parietal cortex have a function in the sensorimotor transformations that underlie visually guided reaching. Specifically, it has been suggested that the posterior parietal cortex could transform visual target locations from retinal to hand-centered coordinates. Just as Kveraga et al. (2002) have suggested that similar neural mechanisms in superior colliculus might avoid the uncertainty effect for eye-movements made directly to targets, it seems plausible that the neural-mapping mechanisms for hand movements in the posterior parietal cortex might avoid the uncertainty effect for pointing movements directly to targets. Interestingly, other research in this area (for a review, see Snyder et al. 2000) suggests that other areas of the posterior parietal cortex might also play a similar role for eye movements.

With these thoughts providing the context, the second experiment had two goals. The first goal was simply to replicate and extend the demonstration in Experiment 1 that pointing movements to targets could, at least under some conditions, have movement latencies unaffected by uncertainty. Although it seems unlikely that this result is an artifact of the particular stimulus arrangement used, Experiment 2 used the same arrangement to compare both keypress and pointing responses. We expect that with keypress responses there will be latency increases with uncertainty of about 100 ms per $\log_2$ items, but that with pointing responses we will again observe nonexistent to at most, very small, effects of uncertainty on latency.

The second goal of this experiment was to test a version of the privileged-mechanism hypothesis that explains the lack of an uncertainty effect in Experiment 1 based on activity in the posterior parietal cortex. The basis for this test is a comparison of the results using equiluminant stimuli, discriminable from the background only by their color, versus the results using stimuli discriminable by their luminance, as in Experiment 1. The logic behind this manipulation depends on the fact that the posterior parietal cortex lies in the dorsal visual stream, the dorsal stream receives its inputs primarily, if not exclusively from the magnocellular (as opposed parvocellular) projections from the lateral geniculate nucleus, and magnocellular projections code only luminance, and not chromatic, information (Livingstone and Hubel 1988). Thus, following the lead of previous research (e.g., Graves 1996; Pitzalis et al. 2005), we assume that equiluminant stimuli would not directly be discriminable from the background by processes in the dorsal stream such as those in the posterior parietal cortex. If posterior parietal cortex is the neural substrate of a privileged mechanism that bypasses the uncertainty effect for aimed hand movements, we would expect those effects to re-emerge—that there would be uncertainty effects on latency for pointing movements of about 100 ms per $\log_2$ item—when the stimuli are equiluminant with their background.

Methods

Participants

Eight naive participants took part in five sessions lasting approximately 1 h each; four were females and four were males. Participants were recruited through ads posted on the UC Irvine campus. They were required to have vision corrected to 20/20 and be right-handed. They were paid $8 per hour plus bonuses. The UCI Institutional Review Board approved the experimental protocol used.

Apparatus

The apparatus used in this experiment was identical to that used in Experiment 1 with one addition. For experimental sessions involving keypress responses, as opposed to pointing responses, participants responded using the keys on the home row of the keyboard (the keys “A” “S” “D” and “F” on the left hand and “J” “K” “L” “;” on the right hand).

Design

In addition to the independent variables manipulated in Experiment 1, this experiment included two new factors, each with two levels, that were manipulated within participants and blocked across days: response mode—pointing (as in Experiment 1) versus keypress responses—and stimulus type—gray versus green. Experiment 1 used white stimuli against a medium gray background. In this experiment, the background was the same medium gray (in CIE coordinates $x = 0.284$, $y = 0.306$, and $Y = 30$ cd/m^{-2}). In the Gray stimulus condition, the stimulus circles were the same color gray, but lighter than the background ($Y = 40$ cd/m^2). In the Green stimulus condition, the stimulus circles were a moderately saturated green ($x = 0.300$, $y = 0.504$) with an intensity that was close to $Y = 30$ cd/m^{-2}, but that was selected, separately for each participant, to be subjectively equiluminant with the gray background. Each of the four combinations of these two factors was tested in a separate one-hour experimental session on one of the four test days of the experiment. The order of the four conditions was balanced across sets of four participants using two, separate 4 × 4 digram-balanced Latin squares (Wagenaar 1969). Each of the four experimental sessions duplicated the design used for each of the sessions in Experiment 1, with 36 blocks of 18 error-free trials, which alternated between blocks with $N = 2$ and 6, the first four of which were designated as practice.

The first day in this experiment had no analog in Experiment 1. After the consent process was completed, the Participant ran a procedure to determine the green light that was subjectively equiluminant with the background. Once this procedure was complete, each participant ran four 5-block sessions, one in each of the four conditions that would be tested during the subsequent test days.\

Procedure

The procedures used for this experiment are an elaboration of those used in Experiment 1. Two procedural additions were required. The first procedural addition was needed to identify a subjectively equiluminant green stimulus for each participant. On day 1 each Participant completed a brief (10 min) psychophysical task, described in more detail below. This task was designed to identify a set of RGB display values that produced a green light of moderate saturation that was equiluminant with the gray background. The second procedural addition was needed to accommodate sessions in which responses were made using keypresses rather than pointing movements. Because no pointing data were collected in these sessions, the OptoTrak calibration procedure, which began each Session in Experiment 1, was omitted. Also, the procedure to confirm accuracy of the OptoTrak calibration, which began each block in Experiment 1, was replaced by a procedure designed to reinforce the display-position onto response-key mapping to be used in that block. This procedure, along with other minor changes to the structure of a trial, is described more fully below.

Identification of equiluminant stimuli

At the beginning of each participant’s first session, a motion-minimization method was used to identify the intensity of a medium saturation green ($x = 0.300$,

$y = 0.504$) that was subjectively equiluminant with the background gray ($x = 0.284$, $y = 0.306$, and $Y = 30$ cd/m^2). Stimuli for this procedure were generated and presented using Matlab 6.5 with the Psychophysics Toolbox extensions (Brainard 1997). As shown in Panel a of Fig. 4, the stimuli were foveally presented annuli (1.2° radius) divided into 16 distinct patches presented on a light background. As Panels b and c of Fig. 4 illustrate schematically for one set of the four quadrants of the annulus (with the curvature removed to simplify the illustration), each patch could be filled with one of four colors, and the position of the patches was shifted half the width of a patch once every 66.7 ms. If the colored, test patches are subjectively equiluminant with the neutral gray patches, then, despite these shifts, there is no perception of rotational motion in this display. However, if the test patches are not subjectively equiluminant with the neutral gray patch then the display will appear to rotate, with the direction (clockwise versus counterclockwise) of perceived motion depending on the relative, subjective luminance of these two segments. The Participant's task was to identify the predominant direction of the apparent motion by pushing the left arrow key if the annulus appeared to rotate counter-clockwise and the right arrow key if it appeared to rotate clockwise.

Two staircases were interleaved; one which started with an intensity for the green, test patch well below 30 cd/m^2 and the other that began with an intensity well above this level. Once both staircases had generated eight reversals, the procedure terminated. The two staircases occasionally resulted in estimates that differed slightly; in those cases, the average of the two RGB settings was used.

Procedures for sessions using keypress responses

The procedure for the keypress responses was designed to be as similar as possible to that used for the pointing responses. So, for example, the presentation and timing of the stimuli on each trial were identical. A similar system of providing trial-to-trial feedback about errors and block-to-block feedback on overall performance was also used, with separate target scores maintained for each response condition. However, there were differences between details of the pointing and keypress response conditions. For example, it was not necessary on days with keypress responses to do OpToTrak calibration at the start of the session or before each block.

The mapping of response finger/keys onto the eight possible stimulus locations was fixed within and across participants. Because the combination of possible target locations used as stimuli varied from block to block, the finger/key combinations used to make responses also changed from block to block. In the $N = 2$ condition, in which only one finger on each hand

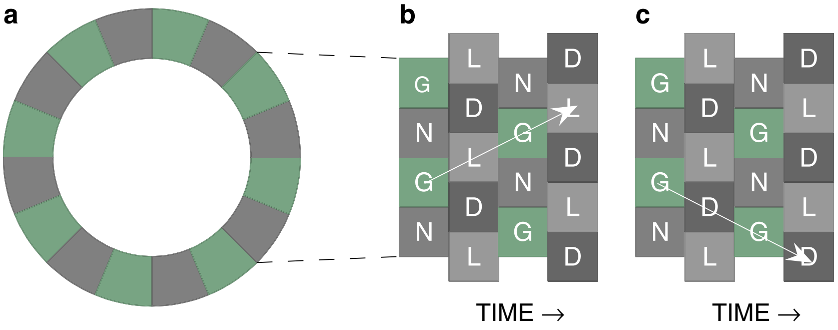

Fig. 4 Panel a contains an example, exaggerated for clarity, of the stimuli used to assess subjective equiluminance. At any time, the stimulus was an annulus consisting of eight alternating pairs of filled patches. Panels b and c show how the patches in a quadrant of the annulus changed over time. Each column in these panels represents a frame displayed for 66.7 ms. As shown, the patches in a frame alternated between either a neutral gray (*patches labeled N*), or the green to be matched to it in luminance (*patches labeled G*) and either a light gray (*patches labeled L*) or dark gray (*patches labeled D*). In addition, on successive frames, the position of the patches moved by half of their width ($\pi/8$ radians of rotation) so that when frames with the same patch colors appeared, every second frame, the position of their patch colors would be interchanged. The resulting apparent motion depended on whether the luminance of the green test patches (G) was perceived as lighter as or darker than that of the neutral gray patches (N). As shown in Panel b, when the green (G) patches are perceived to be lighter than the neutral gray (N) patches, the motion perception system associates them with the light gray (L) patches and the neutral gray (N) patches are associated with the dark gray (D) patches. In this case, as the white arrow indicates, there is a percept of counter-clockwise rotation of the annulus. Panel c illustrates the case when the green (G) patches are perceived to be darker than the neutral gray (N) patches and thus are seen to cohere with the dark gray (D) patches. In this case, as the white arrow indicates, there is a percept of clockwise rotation of the annulus. When the green (G) and neutral gray (N) patches are sufficiently close to being subjectively equiluminant, there is no percept of rotation

was used to make responses, there was little tendency to be confused about the finger/key required to respond to the stimulus on any given trial. However, in the $N = 6$ condition, associating the finger/key with which to respond to each stimulus in the set sometimes posed a challenge. To help participants overcome this challenge, at the start of each block, the set of target locations for that block was displayed. Then, in turn, each target location was highlighted and the letter on the keyboard, which was associated with the key that was the response for that target, was displayed until the appropriate key was pressed. The instructions emphasized that all eight of the participant's fingers should be on the appropriate response keys at the start of each trial, even in the $N = 2$ blocks. The experimenter in the room with the participant checked visually to ensure that these instructions were followed.

Data analysis

As for Experiment 1, the dependent variable of primary interest is movement latency; however, we also report movement duration, and endpoint error. The primary focus of the analysis for each variable will be a $2 \times 2 \times 2 \times 2$ fully within-participants ANOVA. This ANOVA includes the two within-participants factors that were the focus in the analysis for Experiment 1: N, the number of possible targets, and whether the stimulus-target combination was a repetition of that of the previous trial. In addition, this analysis includes two additional within-participants factors: the Response mode—pointing or keypress—and the Color of the stimulus—gray or equiluminant green. As in Experiment 1 and for the same reasons, this analysis collapses across three other variables: the configuration of the possible targets among the eight locations, the actual target location, and the cue-onset delay.

Results

Errors

Across participants, 28 (0.37%) trials had to be excluded and replaced because of latencies longer than 1,200 ms. Most of the trials treated this way were in the Keyboard response condition (24) rather than the Pointing response condition (4).

There were again few anticipation errors, that is, movements made on a catch trial or movements that began within 100 ms of the stimulus onset on a normal trial: this happened on 3 (0.08%) trials in the Keyboard response condition and on 24 (0.64%) trials in the Pointing response condition. Again this suggests that the inclusion of catch trials and the cue-delay manipulation successfully induced the participants to wait for the cue stimulus before initiating a response.

In the Pointing response condition, a trial was classified as an error when the movement ended outside of the target circle; trials in the Keypress response condition were classified as errors when an incorrect key was pressed. Based on an analysis of error proportions after an arcsine transformation, errors occurred more frequently for Keypress than for Pointing responses [$F(1,7) = 57.73$, $P = 0.000$]. When making Pointing responses, there was not a reliable effect of the number of stimuli: there were 4.3% errors for $N = 2$, and 7.6% for $N = 6$. In contrast, for the Keypress responses, there was a consistent effect of number of stimuli: 6.5%, for $N = 2$, and 11.7%, for $N = 6$ [for this difference, $F(1,7) = 25.59$, $P = 0.001$]. The two-way interaction of these factors was statistically reliable [$F(1,7) = 39.73$, $P = 0.000$]. There was also an overall effect of Color: for Gray stimuli there were 7.6% errors, but only 5.3% errors for Green stimuli [$F(1,7) = 7.76$, $P = 0.027$]. There were, however, no systematic interactions between Color and the other factors or any effects of Repetition.

When errors occurred, an additional trial of the same type was added to that block of trials. These make-up trials maintained the balancing of the design despite the occurrence of errors. Data from trials with errors were not included in the analyses of latency, movement duration, or movement endpoint error.

Latency

Figure 5 summarizes the latency data from Experiment 2. Each of the figure's four panels displays the data from one of the four combinations of Response mode and stimulus Color in the same way that Fig. 2 summarized the results of Experiment 1.

A striking pattern in the $2 \times 2 \times 2 \times 2$ ANOVA for the latency data was that stimulus Color had no statistically reliable effect, either alone or in combination with any of the other factors. Because the levels of the gray and equiluminant green used in this experiment had been chosen based on pilot work to be roughly equisalient, the absence of a main effect did not come as a surprise [$F(1,7) = 0.046$]. However, if, as had seemed possible, the effect of uncertainty were to return for pointing movements made to equiluminant green stimuli, we would expect to see a significant three-way interaction of $N \times \text{Color} \times \text{Response}$. Instead, this three-way interaction, the two other

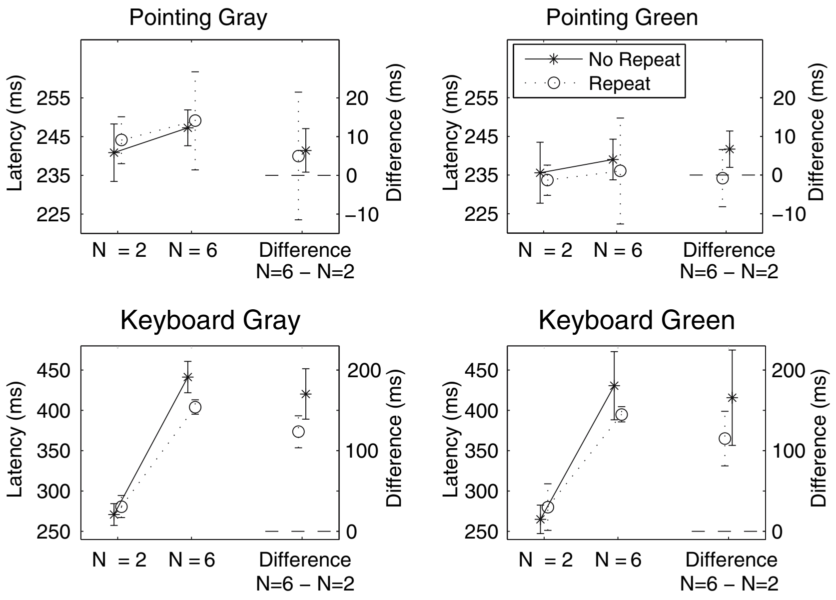

Fig. 5 Response latencies, in Experiment 2, as a function of *N*, Repetition, Response, and Color. Each panel shows data from one of the four combinations of Response and Color. In each panel, the four points on the left show the data for the *N* × Repetitions interaction. The error bars on each point are 95% confidence intervals computed separately for the data in each of the four conditions after the estimated main effect of participants was removed. The two data points on the right show the mean difference for *N* = 6 minus *N* = 2 separately for those trials in which the stimulus–response combination is a repetition of the previous trial (*circles*) and those trials with no repetition (*asterisks*). The error bars are 95% confidence intervals for the difference computed from a correlated samples *t*-test

three-way interactions involving Color, all three two-way interactions involving color, and the four-way interaction were all non-significant [for all of these contrasts, the *F*(1,7) < 1; the largest *F*-value was 0.519]. The lack of any effect of Color can be seen in Fig. 5 by comparing the left and right panels in each row; not only is the pattern of the data qualitatively similar across each pair of panels in a row, but the analogous data points in the panels for gray and green stimuli are usually within milliseconds of each other. This similarity suggests that the lack of any interaction with color is not simply to due to a lack of power to detect an effect of reasonable size.

Figure 5 does show the expected, large interaction between Response mode and the number of items [*F*(1,7) = 121.14, *P* = 0.000]. This can be seen in Fig. 5, by comparing the two panels in the top row with those in the bottom row, for either column: generally, for pointing responses the effects of N are, as in Experiment 1, small, but for Keyboard responses the effects of *N* are orders of magnitude larger. However, the analysis of variance shows that this two-way interaction is embedded in the three-way interaction, *N* × Response × Repetition [*F*(1,7) = 11.90, *P* = 0.011], which necessarily complicates its interpretation. To decompose this three-way interaction, we focus on the difference between *N* = 2 and 6 for the four combinations of the Repetition and Response mode factors summarized in Table 1.

As in Experiment 1, for Pointing responses there was a small increase in latency going from *N* = 2 to 6. This effect, however, was only marginally reliable when there was not a target repetition [if we use a Bonferroni correction to protect the alpha level for multiple comparisons (Maxwell and Delaney 2004), then, for these tests, α = 0.025 and this comparison

Table 1 In Experiment 2, the average latency difference for *N* = 6 minus *N* = 2 broken down by repetition and response mode but averaged across stimulus color

Response mode	Repetition	Mean difference *N* = 6 minus *N* = 2 (ms)	Lower bound	Upper bound	Difference as a slope (ms per $\log_2$ item)	*F*(1,7)	*P*
Pointing	No	4.9	0.3	9.5	3.1	6.44	0.039
	Yes	3.7	–6.2	13.6	2.3	0.77	0.409
Keyboard	No	168	124.5	211.4	106	84	0.000
	Yes	119	93.2	145.1	75	117	0.000

would not be considered statistically reliable] and, when the target was repeated, the data are consistent with there being no effect. Further clouding the interpretation, the two-way interaction of *N* and Repetition is not statistically reliable for these Pointing data [$F(1,7) = 0.09$]; thus, although the difference may be statistically reliable in one case and not in the other, there is no evidence that the size of the effect of *N* differs for repetitions and non-repetitions. In both cases, it is small.

As in previous research looking at uncertainty using keypress responses, Table 1 shows that there were large effects of *N* with this Response mode. Also, consistent with previous research, there was a large, reliable interaction with Repetition: the effect of *N* was reliably larger when the target was not a repetition of that on previous trial than when it was [$F(1,7) = 17.78$, $P = 0.004$]. However, even for repetitions the effects of *N* were substantial: 75 ms per $\log_2$ item.

Movement duration and EndPoint error

The duration data were analyzed using a $2 \times 2 \times 2$ ANOVA with *N*, Repetition, and stimulus Color as the three within-participants factors. (Recall that there is no analog for movement duration when responses were made by pressing a key.) In this analysis there were no significant effects [for *N*, $F(1,7) = 1.49$, $P = 0.261$; for $N\times$ Repetition, $F(1,7) = 2.272$, $P = 0.175$; for the three-way interaction of $N \times$ Repetition $\times$ Color, $F(1,7) = 1.47$, $P = 0.265$; for both remaining main effects and the two interactions, $F(1,7) \leq 0.61$]. The absence of significant effects in this analysis reflects the small differences between the mean durations rather than imprecision in the data. For example the average duration was 371.4 ms for $N = 2$ and 373.9 ms for $N = 6$. The 2.5 ms difference has a 95% confidence interval from –2.4 to 7.4 ms.

A similar analysis examined the influences on end point error, the distance between the center of the target and the movement endpoint. As in the duration analysis and replicating the results of Experiment 1, there was not a significant effect of any of the three factors or their interactions; in fact, only two of the seven *F*-ratios had values greater than 1 [for *N*, $F(1,7) = 2.74$, $P = 0.14$; for Repetitions, $F(1,7) = 2.00$, $P = 0.20$]. As for the durations, the absence of significant effects in this analysis reflects the small differences between the mean durations rather than imprecision in the data. For example the average endpoint error was 3.07 mm for $N = 2$ and 2.96 mm for $N = 6$ (the targets had a 7 mm radius). The –0.11 mm difference has a 95% confidence interval from –0.25 to 0.04 mm.

Discussion

This experiment was intended to address two questions. The first was to strengthen the results of Experiment 1 by replicating them and, by looking for uncertainty effects when the same stimuli were used with keypress responses, to show that the Experiment 1 results were not an artifact of some aspect of the stimuli or the procedure used. The second was to test a specific version of the privileged-pathway hypothesis, which explains the lack of an uncertainty effect in Experiment 1 based on activity in the posterior parietal cortex, by comparing performance when the stimuli differed from the background either in luminance or in the green saturation of equiluminant stimuli.

The results of Experiment 2 provide unequivocal answers to both questions. Using the same stimuli and procedures, there is a large uncertainty effect for keypress responses and, once again an effect that is, at most, negligible for pointing responses. Although slightly larger than the uncertainty effect observed in Experiment 1, the increase in latency of 2.7 ms per $\log_2$ item is over 30 times smaller than the increase of 90.5 ms per $\log_2$ items (obtained by averaging over the interaction with the Repetition effect) obtained for keypress responses. These results replicate the lack of an uncertainty effect for pointing movements observed in Experiment 1 and demonstrate that this finding is not an artifact of the experimental procedures or stimuli.

Experiment 2 also found that the difference between luminance-defined versus equiluminant, green stimuli was negligible. Specifically, as Fig. 5 shows, there is no evidence that this change in the stimuli influenced the effects of uncertainty. As described in the introduction to this experiment, the hypothesis that a privileged mechanism in the posterior parietal cortex accounts for the lack of uncertainty effects for pointing movements suggests that uncertainty effects should return for responses made to equiluminant, green stimuli. The absence of even a hint of the three-way interaction of stimulus color, response mode, and number of items is, therefore, evidence against this version of the privileged mechanism hypothesis.

One possibly discordant note for this interpretation of the lack of an effect associated with the color manipulation is that, as with latency, there also was no difference in movement duration or endpoint error associated with switching from the gray to green stimuli. To explain why this might be a problem, we need to provide some additional background about theories of motor control. Since the work of Woodworth (1899), motor theorists have often found it useful to consider aimed movements to have two phases: an initial-impulse phase, which is pre-programmed and ballistic, and a current-control phase, which comes towards the end of the movement and uses feedback to correct any deviations in the intended path of the movement. Typically, the latency of a movement and the initial portion of the movement trajectory are thought to reflect the operation of the initial-impulse phase of control, the later part of the movement trajectory and the endpoint error are thought to reflect the current-control phase, and overall movement duration would reflect the influences of both phases (a more complete discussion of this idea and its history is given by Meyer et al. 1990).

The ''where''/''how'' interpretation of the ventral/dorsal streams of cortical, visual processing given by Milner and Goodale (1995) suggests that the control of pointing movements involves dorsal stream processes in the posterior parietal cortex. Glover and Dixon (2001, 2002) have further refined this claim, arguing that different neural substrates in the parietal cortex underlie movement planning, which determines the parameters of the initial impulse phase, and the current-control phase. According to this analysis, only the current-control processes are located in the dorsal stream of visual processing, and thus only these processes would fail to receive, at least directly, the information carried in the parvocelluar projections. Glover and Dixon posit that the neural substrate for initial-impulse planning processes is in the inferior parietal lobule. This area, although in the parietal cortex, is physically close to the temporal lobe and, according to Glover and Dixon, behaves as though it were part of the ventral stream of visual processing. Thus, an argument similar to that outlined for testing equiluminant stimuli might also lead us to expect some disruption of the current-control phase of pointing movements made to equiluminant stimuli. This disruption should be evidenced by increases in movement duration and/or endpoint error. This expectation is clearly violated by the observed lack of an effect of the color manipulation on either measure.

A possible for the observed lack of an effect of the shift from gray to green stimuli on the current control of the pointing movements depends on movement difficulty. If the spatial accuracy requirements of movements were sufficiently undemanding, then their preplanned, initial impulses might be expected to produce movements ending within their targets on most trials. In this case corrections, reflecting the operation of the current-control processes, would rarely be necessary and so there would be little opportunity to see behaviorally the effects of the degradation in the operation of these processes, following the switch from gray to equiluminant green stimuli.

Although this explanation is convenient, consideration of the characteristics of the pointing movements used in this experiment suggest that it is wrong. Movements, such as those studied in this experiment, which are characterized by the goal of minimizing movement time to a fixed-size target, exhibit a form of speed-accuracy tradeoff known as Fitts's law (Fitts, 1954; see Meyer et al. 1988 for a discussion of why the pointing movements used here should be categorized in this way). According to Fitts's law, the difficulty of these movements is characterized completely by the ratio of the movement distance and the target size. For the pointing movements in this experiment, this ratio varies from 9.1 to 12.1, across the different target locations.[6] Research by Meyer and his colleagues (summarized in Meyer et al., 1988, 1990), supporting the stochastic optimized submovement model as an explanation for Fitts's law, shows that unless this ratio is much smaller—i.e., less than 2 or 3—the optimal strategy for minimizing movement time requires corrective submovements to successfully complete a substantial proportion of movements within the target while minimizing movement time. According to this model, it is the initiation and guidance of these corrective submovements that constitutes the Woordworth's current-control phase and thus these corrective submovements are what we would expect to be disrupted by the switch from gray to green stimuli.

A possibly more promising explanation for the lack of an effect of the color manipulation on movement duration and endpoint error follows from the fact that only the color of the movement target was varied. Thus, although information about the target might not have been directly available to parietal processes when the stimuli were equiluminant green, this manipulation did not affect the information about the stylus available during the movement. Consider two possible ways that concurrent visual information might be used during a movement. If the visual information used to guide movements during the current-control phase is the relation of the simultaneously-perceived position of the stylus and the target, then the color manipulation would presumably disrupt the current control process. However, if only the online information about the stylus is needed by the current control process, perhaps because the position of the target is known and does not change, then the color manipulation might plausi-

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[6] In Fitts' law, the index of difficulty is defined as $\log2(2 \times D/W)$. Using this index, the difficulty of the movements used in this experiment ranges from 4.2 to 4.6.

bly have had no effect on movement duration and endpoint error, as was observed. Evidence supporting this explanation comes from studies that systematically varied the availability of concurrent visual information about the hand making the movement and the target (Carlton 1981; Heath 2005). These studies found that movements in which vision of the limb was eliminated once the movement began, were slower, less accurate, and exhibited fewer corrective submovements. However, when vision of the target was made unavailable after the movement onset, there was no measurable effect of this manipulation. It is this second condition that is analogous to the condition in the current experiment using equiluminant green targets.

To counter our suggestion that the results of Experiment 2 disqualify the privileged mechanism hypothesis as an explanation for the lack of stimulus uncertainty effects for pointing, one might ask whether the superior colliculus could be the locus of the privileged pathway [just as Kveraga et al. (2002) suggest that it does for eye movements] that was unaffected by the stimulus-color manipulation. This is unlikely, however. Certainly, the activity of neuron populations in superior colliculus has been shown to be correlated with ongoing arm movements in primates (Werner et al. 1997a, b). However, as Stuphorn et al. (2002) conclude, it is unlikely that the role of these neurons is to code the target location of a pointing movement or the intention to make such a movement, as is clearly the case in the parietal cortex. Instead it appears that these neurons have a role in the coordination of eye and hand movements, perhaps coding the mismatch between the target of an intended or ongoing movement and the point of visual fixation.

General discussion

We presented three hypotheses that might characterize situations in which there is no uncertainty effect: (a) the practice hypothesis, (b) the compatibility hypothesis, and (c) the privileged mechanism hypothesis. For pointing, the lack of an effect of the color manipulation in Experiment 2 provides evidence against a version of the privileged mechanism hypothesis depending on control processes within the posterior parietal cortex and receiving primary visual input through the dorsal visual stream. Of course, this does not eliminate the possibility that there exist other privileged-mechanism loci outside of the dorsal visual stream. However, given the current literature characterizing the role of superior colliculus in hand movements (Stuphorn et al. 2002), it seems unlikely that superior colliculus plays this role for hand movements as has been hypothesized for eye movements (Kveraga et al. 2002). The practice and compatibility hypotheses remain possible explanations that could be applicable to the hand movements studied here. It is, however, not clear that either of these hypotheses should be seen as a providing an explanation for the lack of an uncertainty effect for pointing movements that is itself both necessary and sufficient.

Our experiences preparing and conducting this experiment lead us to propose a fourth consideration, the effector selection hypothesis. We believe that the absence of the necessity for effector selection may help explain why we did not observe an uncertainty effect for pointing movements, as well as the exceptions to the general observation of an uncertainty effect previously described for eye movements, keypresses to vibratory stimuli, and digit naming. The basic idea underlying this hypothesis is that there will be an uncertainty effect when the participant must choose the proper effector to use for the required response based on the identity of the stimulus; when the response effector is known ahead of time, there is the *possibility* that no effect of uncertainty will be observed. This idea strikes us as similar to the suggestion of Rosenbaum (1980), that pre-planning for the direction and extent of a pointing movement cannot take place if the arm to be moved is unknown. Unlike the three hypotheses we have previously described, we do not see this hypothesis as an attempt to state a single necessary and sufficient condition for the observation of no effect of uncertainty. Instead, we suspect that this outcome depends on several conditions; it is only when all of these conditions are met that no uncertainty effect will be observed. Although we may not know all of the necessary conditions, we suspect that to obtain no uncertainty effect it is necessary that that the task not require effector selection based on the stimulus and that it involve a highly compatible stimulus–response mapping. Amount of practice may also be important; however, because of the problems distinguishing pre-experimental from task-specific practice the evidence for this is, we feel, less compelling. Omitted from this list is the requirement of a privileged mechanism that bypasses the response-selection process for the task in question.

We believe that this proposal provides an explanation for the results in the literature. For example, in the experiments reported here as well as the eye-movement experiments of Kveraga et al. (2002), the tasks clearly do not involve effector selection, because only a single effector is used, the direct stimulus–response mappings appear to be maximally compatible, and participants are highly experienced in variants of the task studied. When the stimulus–response mapping is compatible, but not direct, as in tasks involving joystick movements to a displaced target (e.g., the Toward condition of Dassonville et al. 1999; Berryhill et al. 2005), the observed stimulus-uncertainty effects are substantially smaller than those traditionally associated with Hick's law. In spatially incompatible versions of the joystick (the CCW condition of Dassonville et al. 1999) and eye-movement tasks (the anti-saccade condition of Kveraga et al., 2002), full-size stimulus-uncertainty effects emerge. A similar analysis is conceivable for the digit-naming results summarized by Teichner and Krebs (1974). Although not usually thought of as an object of effector selection, the vocal system is the only effector with which responses are made in this task and the stimulus–response mapping has, at least for native readers of English, become highly compatible through extensive experience at this task.

The three other stimulus–response combinations considered by Teichner and Krebs (1974), provide a useful contrast to the digit naming task. Like the keypress condition in Experiment 2, in the two tasks summarized by Teichner and Krebs that used keypress responses—i.e., in which the stimuli were a matched row of lights or a visually displayed digit—it was necessary to choose the response effector—i.e., the hand/finger combination—based on the identity of the stimulus. The necessity of making this selection may have created a situation in which stimulus uncertainty effects emerged, according to our explanation. The same result occurs, for a different reason, in the task requiring naming of a digit in response to a stimulus that was one of a row of lights. In this situation, although effector selection was again unnecessary, the stimulus–response mapping was arbitrary and not well learned, again creating a situation in which stimulus uncertainty effects emerged.

The exception to the generality of the uncertainty effect that may be more difficult to explain in this framework is that involving keypress responses to finger vibrations (Leonard 1959; ten Hoopen et al. 1982). The perplexing question here is why effector selection based on the stimulus is not also required to complete this task. Our explanation is that the vibration of a specific finger triggers a reflex-like response for that finger. What we have in mind here are not spinally mediated stretch reflexes, but something more like what are often called long-loop reflexes (Marsden et al. 1978): in humans there is a second pathway by which changes in the position of the arm, wrist and fingers can result in one of several possible, cortically mediated triggered responses—e.g., to resist or give way to the force. The latency of these responses is longer than that of a stretch reflex, but faster than a typical voluntary response. These responses also differ from standard reflexes in that they are programmable by the brain: e.g., voluntarily we can change the response to a stretch. Of particular importance to our explanation, however, is that these responses are specific to the part of the body that was stimulated—i.e., effector selection appears to happen automatically for these responses.

An important test of the effector selection hypothesis would be experiments that change the necessity of making an effector selection while maintaining a high-level of stimulus–response compatibility. Although there have been studies comparing at uni- versus bi-manual movements (Proteau and Girouard 1984; Proctor and Wang 1997), we know of no studies that have both looked at stimulus uncertainty effects and varied the number of effectors. Proctor and Wang (1997) have a bearing on our larger hypothesis, however. Their Experiment 3 found that set-level, stimulus–response compatibility is higher for pointing movements when bimanual responses are paired with spatial stimuli and when unimanual stimuli are paired with verbal stimuli (the words "left" and "right"). This result seems contrary to our interpretation of the results in this paper, because they imply that unimanual pointing movements are not maximally compatible when the targets are specified spatially, but we found at most minimal effects of stimulus uncertainty with unimanual pointing to spatially defined targets.[7]

Broader implications

Understanding the locus of perceptual-motor effects is important for those trying to understand and model functional brain architecture. This type of modeling has is a primary goal of the EPIC architecture (Meyer and Kieras 1977a, b) and instantiations of ACT-R (Anderson et al. 2004) have also focused on this goal. The effector selection hypothesis, if correct, and, more generally, the identification of tasks in which response latency is uninfluenced by stimulus uncertainty have important implications for our understanding of the locus of these effects. For example, several models of stimulus identification have drawn support from the fact that they predict a logarithmic increase in response latency as the number of stimulus–response alternatives increases based on the operation of perceptual-

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[7] However, a post-hoc comparison of the results in Proctor and Wang (1997) Experiment 1B and Experiment 2 leads to the opposite result for a comparison of uni- versus bi-manual keypress movements. This appears to be an area that could use further exploration.

identification processes (Christie and Luce 1956; Lacouture and Marley 1991; Laming 1966; Usher and McClelland 2001). Using a variety of simulations, Usher and McClelland (2001) extensively explored this relation and found that it depends on the assumption that accuracy is held constant as the number of alternatives increases. With this assumption, they conclude that the logarithmic stimulus uncertainty effect can be understood as compensation required for maintaining a constant stimulus-identification error rate given the increased opportunity for errors with more stimulus alternatives.

The existence of tasks without stimulus uncertainty effects, including the data reported here, challenge this explanation. If the same stimuli can be associated with both the presence and absence of large stimulus uncertainty effects depending only on which of two response modes, both with compatible mappings, is used, then the argument that the stimulus uncertainty effects are due to stimulus processing per se, becomes less plausible. This position can be saved by arguing, as Kveraga et al. (2002) have done for eye movements, that the lack of a stimulus uncertainty effect reflects a privileged processing mechanism that somehow bypasses the ‘‘normal’’ stimulus identification procedures that produce these effects. It is our concern with these larger implications that have led us to look carefully at the privileged mechanism hypothesis for pointing movements studied here. Although our argument can always be challenged by someone positing a privileged mechanism that we have not considered, we believe that the obvious candidates have been shown to be unlikely for pointing tasks.

A key methodological aspect of the experiments in this area is that the stimuli are highly discriminable and thus errors are more likely to represent failures of stimulus–response mapping or response execution than failures of stimulus identification. This is different from tasks that are usually the focus of stimulus identification models such as those studied by Usher and McClelland (2001): e.g., they report data from a task in which participants identify length differences in the sides of a rectangle where the differences ranged from 0.1 to 0.5%. Given this difference in stimuli, it seems likely that stimulus identification time varies with the number of stimuli in our experiment, but that this effect is small—perhaps this is the source of the small effects of *N* that remain in our experiments.

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