INTERRUPTING MENTAL ROTATION: WHAT WE KNOW WHEN

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ABSTRACT

In a handedness decision task participants are presented with pairs of stimuli in varying orientations and their task is to decide if the stimuli are the same or mirror images. It is commonly assumed that participants must imagine one stimulus rotate to the same orientation as the other stimulus to make a handedness decision (i.e., “mental rotation”). To examine whether “mental rotation” is necessary to determine the handedness of misoriented objects, I developed a dualtask procedure in which a handedness decision task is randomly interrupted with a side decision task. This interruption occurs when one of the two stimuli is shaded in, whereupon the participant is to abandon the handedness decision task and respond with the hand that is on the same side as the shaded in stimulus. If participants know the handedness of the stimulus before they are interrupted by the side decision task, reaction times will be faster on trials in which the responses to the two tasks are congruent (i.e., with the same hand) than the trials in which the responses to two tasks are incongruent. If participants do not know the handedness of the stimulus before they are interrupted by the side decision task, reaction times on all trials will be equal. The data show that participants knew the handedness of the misoriented stimulus before using “mental rotation” when they had experience with that particular stimulus. Furthermore, this knowledge in unrelated to angle.

ACKNOWLEDGMENTS

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INTRODUCTION

In a *handedness decision task*, participants are presented with a pair of stimuli in different orientations that are either identical to each other or mirror images of each other. If the two stimuli are mirror images of each other, they differ in *handedness*. The participants are asked to respond "same" (usually with a key press) if one stimulus can be rotated to match the other stimulus. If, however, the two stimuli cannot be rotated into congruence (i.e., they are mirror images), the participants are asked to respond "different."

Perhaps the most famous handedness decision task was conducted by Shepard and Metzler (1971). Shepard and Metzler presented participants with pairs of two-dimensional drawings that represented three-dimensional objects (See Figure 1).

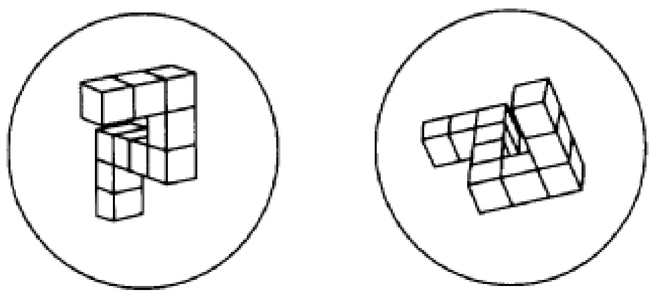


Figure 1. Examples of stimuli used by Shepard and Metzler (1971).

Half of the pairs were identical to each other except for orientation, and the other half were mirror images. Shepard and Metzler (1971) referred to the “misorientation” between the two stimuli as *angular disparity*. For example, two stimuli with an angular disparity of 180° are upside down in relation to one another, and two stimuli with an angular disparity of 90° are perpendicular in relation to one another. Shepard and Metzler varied angular disparities from 0° to 180° in 20° intervals and recorded participants’ reaction times (RT). The RT required to make a handedness decision increased linearly as the angular disparity between the two stimuli increased (See Figure 2).

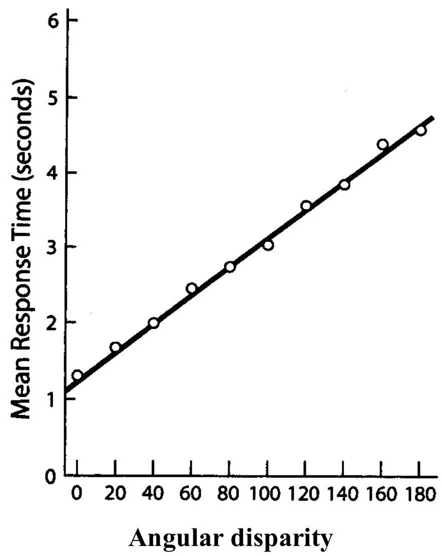


Figure 2. RT is a monotonically increasing function of angular disparity (Shepard & Metzler, 1971).

This relation between RT and angular disparity has proven to be a very robust finding (Band & Miller, 1997; Bethell-Fox & Shepard, 1988; Corballis, 1986, 1988; Heil, Rauch, & Henninghausen, 1998; Ilan & Miller, 1994; Jolicoeur; 1990; Kail, 1986; Rossi & Collyer, 1986; Ruthruff & Miller, 1995; Shepard & Cooper, 1982; Shepard & Metzler, 1971; Tarr, 1995; Tarr & Pinker, 1989).

The Shepard Model

Shepard and Metzler (1971) argue that the increase in RT as a function of angular disparity is evidence for a process of *mental rotation*, during which participants imagine one stimulus rotate to the same orientation as the other stimulus. They argue that the imagined rotation is an analog process. Shepard (2001) states that people have evolved to internalize kinematic geometry, and any mental transformation of an object must follow lawfully within an internalized space. This statement makes the important assumption that the internal process of "mental rotation" is similar to the process that takes place when observing an object physically rotate. Therefore, the intermediate stages of the mental process should have a one-to-one correspondence to the intermediate stages of an actual rotation in the physical world (Shepard & Cooper, 1973). In other words, the mental image must pass through all intermediate stages that correspond to the stages of an object physically rotating. This process is evidenced by a linear function relating RT and angular disparity. Shepard claims that, although the speed of transformation may vary, the linearity will always be present. This is what he calls the *chronometric law.* Shepard and Cooper (1973) do not claim that this one-to-one correspondence implies any "concrete structural resemblance" between the mental image and the corresponding view of the external object, or that there is anything actually rotating within the brain. Shepard and Cooper do claim "that there is a one-to-one relation between the internal representation and the corresponding external object in the specific sense that the subject is especially disposed to respond to that particular object in that particular orientation at that particular moment-if it were actually to be presented (Shepard & Cooper, 1973, p. 102)." Interestingly, participants' introspective reports of how they complete the task coincide with Shepard and Metzler's claims.

To infer an analog process of "mental rotation" from RT data, two criteria must be met (Cohen & Kubovy, 1993). The first criterion is a *positive slope criterion*, which states RT must be a monotonically increasing function of angular disparity. As discussed earlier, it should take longer to respond to stimuli with greater angular disparities because there are more intermediate orientations through which the imagined rotation must pass. Therefore, RTs should get longer as angular disparity increases. The second criterion is a *limiting rate criterion* (LRC). By definition, an analog process cannot be infinitely fast. Therefore, for "mental rotation" to qualify as an analog process there must be a limit to the speed with which participants can "rotate." The LRC is currently believed to be about 1 ms/degree. Participants are assumed to be using some other strategy for completing the task if their slopes are shallower than 1 ms/degree (Cohen & Kubovy, 1993).

Shepard and his colleagues have proposed a detailed stage model of the "mental rotation" process (Shepard & Metzler, 1971; Shepard & Cooper, 1973, 1982). I will refer to this model as the *Shepard model* (see Figure 3).

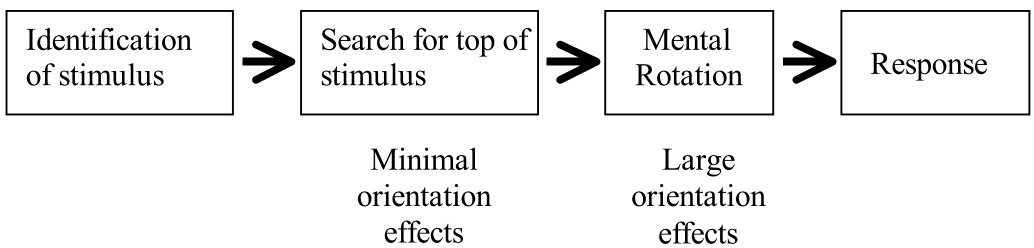


Figure 3: A schematic representation of the Shepard Model.

During the first stage of this model, the participant identifies the stimulus (i.e., acknowledges that a stimulus is present and decides what it is). Shepard and Cooper (1982) claim that the time taken to complete this stage is invariant across orientation. They base this assumption on the findings that show that one can recognize familiar stimuli from novel orientations with little effect of orientation (Shepard & Cooper, 1973). During the second stage of this model, the participant determines the orientation of the stimulus, which includes locating the top of the stimulus. Shepard and Cooper (1982) argue that this stage should show minimal orientation effects because the participant has to simply scan the display in search of the top of the stimulus. Assuming this scanning process starts at the top of the display, it should take slightly longer to find the top of a stimulus with a greater angular disparity because the top of the stimulus would be located farther from the top of the display. The third stage of the process is the "mental rotation" stage. During this stage, the participant imagines one stimulus rotating to the same orientation as the other stimulus. Shepard and Cooper assume this is a relatively slow process, and therefore accounts for the majority of the orientation effects. During the final stage of this process, the participant must compare the image of the mentally rotated stimulus to the other stimulus and make a decision about its handedness. Because the angular disparity was nullified in the previous stage, the time needed to complete this stage is unrelated to the original angular disparity between the two stimuli.

Shepard and Cooper (1973) provided support for the Shepard model by giving participants advanced information about rotated stimuli. They either gave participants advanced information about the identity, the orientation, or both identity and orientation of upcoming stimuli. The Shepard model predicts that advanced identity information should enable participants to skip the first stage of the process: identification of the stimulus. Similarly advanced orientation information should enable participants to skip the second stage of the process: determination of orientation. In both cases, the elimination of these stages should result in a constant reduction in RT across all orientations. That is, the intercept, but not the slope of the function relating RT and angular disparity, should be affected by the elimination of these stages. However, when both identity *and* orientation information is provided, participants should be able to imagine the upcoming stimulus in the specified orientation before it is presented. Consequently, performance in this condition should be invariant across orientation because the orientation invariant decision stage is the only stage that must be completed.

The data supported all predictions. Specifically, Shepard and Cooper (1973) found that simply providing advanced information about the orientation *or* identity of the upcoming stimulus did not eliminate orientation effects. It did, as predicted, additively decrease RTs. However, when advanced information about both the identity *and* the orientation of the upcoming stimulus was provided, performance was invariant across orientation.

In another experiment that lends support to the Shepard model, Shepard and Cooper (1973) measured RTs to test stimuli that departed both from the upright and from some expected orientation. The Shepard model posits that the angular disparity between the imagined stimulus and the test stimulus is the determining factor for the orientation effects. Therefore, if participants are induced to imagine a stimulus rotate from its presented orientation before the test stimulus is presented, then the data should demonstrate a linear relation between RT and the angular disparity between the test stimulus and the current position of the imagined stimulus. If, however, Shepard model is incorrect and the position of the imagined stimulus is irrelevant, then the data should show a linear relation between RT and the angular disparity between the test stimulus and it's upright position.

To test the above prediction, Shepard and Cooper (1973) presented participants with a circular display that had six small tick marks around its perimeter in 60° increments. The participants were first told the identity of the upcoming stimulus (either an "R" or a "2"). The participants were then given information about the orientation of the upcoming stimulus. The experimenter played a tape that read "up," "tip," "tip," "down," "tip," "tip" at a controlled rate of one command per half second. The participants were asked to imagine the stimulus at the upright orientation when the tape read "up," and to imagine the stimulus rotate clockwise at the same rate as the commands were read in correspondence to the 60° tick marks surrounding the perimeter of the display. For example, when the tape read "up," "tip," the participants were to imagine the stimulus at the 60° orientation, and when the tape read "up," "tip," "tip," the participants were to imagine the stimulus at the 120° orientation, and so on. The command "down" referred to the stimulus being upside down, or having an orientation of 180°. The tape was stopped at various intervals, at which point the participants were presumably imagining the stimulus in the orientation specified by the tape. In this manner, Shepard and Cooper had the participants imagine the stimulus in some orientation other than the upright before the presentation of the test stimulus.

When the tape was stopped, a test stimulus was presented. This stimulus was always the expected character (either an "R" or a "2"). However, on half of the trials the test stimulus was presented in the expected orientation and on the other half of the trials the test stimulus was presented in some unexpected orientation. The unexpected orientations differed from the expected orientations from 60° to 300° in 60° increments. The participants had to decide if the stimulus was normal or a mirror image. The Shepard model predicts that RTs to the test stimulus presented in the expected orientations should be relatively orientation invariant because when the test stimulus is presented, the participant should be imagining the stimulus in that orientation. Consequently, the only stage that would need to be completed is the orientation invariant decision stage. However, when the test stimulus is presented in an unexpected orientation, the Shepard model predicts that RTs should show orientation effects in relation to the expected orientations because the participant must imagine the stimulus rotate from the expected orientation to the presented orientation. Both of these predictions were supported in this study (Shepard & Cooper, 1973).

To review, the Shepard model is an information-processing model of how participants complete handedness decision tasks. The model assumes that "mental rotation" is an analog process that has four stages: identification of the stimulus, determination of its orientation, "mental rotation," and finally a comparison of the image to the other stimulus. Shepard and his colleagues provided substantial support for this model.

Problems with the Shepard Model

Although the Shepard model explains the RT data for handedness decision tasks in that RT is a monotonically increasing function of angular disparity, there are findings that are inconsistent with the model. One such inconsistency was observed by Kail

(1986). Kail showed that with extended practice with the same stimuli, participants completed handedness decision tasks with RTs demonstrating rotation rates faster than the proposed LRC (1 ms/degree). Participants were tested four days a week for four weeks (a total of 16 sessions and 3840 trials). Kail used the same eight alphanumeric characters as stimuli, and by the end of the experiment participants' slopes were around or below the LRC. These results indicate that participants were not using "mental rotation" to complete the handedness decision tasks. This finding casts doubt on the Shepard model, which assumes "mental rotation" is necessary to complete handedness decision tasks.

Another finding inconsistent with the Shepard model is the difference in slopes observed when stimuli are presented either simultaneously or successively in a handedness decision task. When two stimuli are presented simultaneously they are shown to the participant at the same time and they are both available for comparison (See Figure 4).

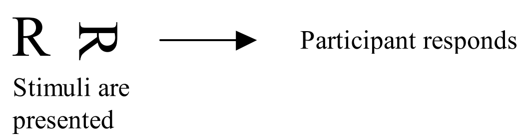


Figure 4. Simultaneous paradigm.

The participant simply compares one stimulus to the other stimulus. However, when two stimuli are presented successively, one stimulus is presented and the participant has to encode that stimulus in memory. Once this is accomplished, the participant presses a button; at which time the first stimulus disappears and the second stimulus is presented. The participant then has to compare the second stimulus to the stimulus encoded in memory (See Figure 5).

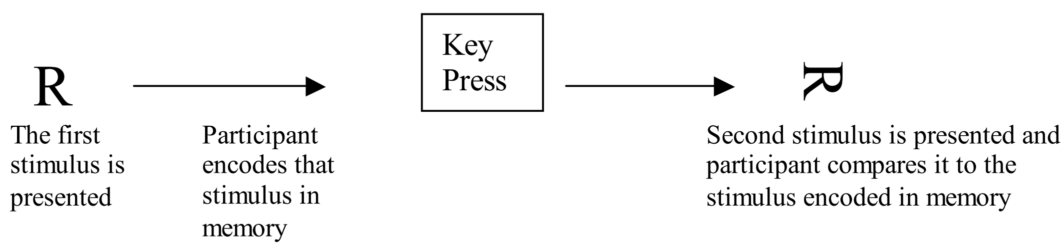


Figure 5. Successive paradigm.

The Shepard model predicts that simultaneous and successive paradigms should yield equal rotation rates. According to the Shepard model, when stimuli are presented simultaneously, all four stages of the process (identifying of the stimulus, finding the top of the stimulus, “mental rotation,” and initiating a response) are completed while both stimuli are present. Therefore, RT encompasses all four stages of the model. However, when stimuli are presented successively, the identification stage is completed in the first phase of the trial, in which the first stimulus is presented. RT is measured during the second phase of the trial, in which the second stimulus is presented. According to the Shepard model, the participant finds the top of the stimulus, performs “mental rotation,” and initiates a response during this phase of the trial. Therefore, RT encompasses all the stages of the model except the identification stage.

Because the Shepard model posits that the identification stage is orientation invariant, the simultaneous paradigm (in which RT includes the identification stage) should produce additively longer RTs than the successive paradigm. That is, there should be an effect of intercept but not slope. However, the data show that the simultaneous paradigm produces steeper slopes (i.e., slower rotation rates) than the successive paradigm. In fact, Cohen and Kubovy (1993) observed slopes of the simultaneous paradigm nearly twice as steep as the slopes of the successive paradigm (3.5 ms/degree vs. 1.8 ms/degree) (See Figure 6).

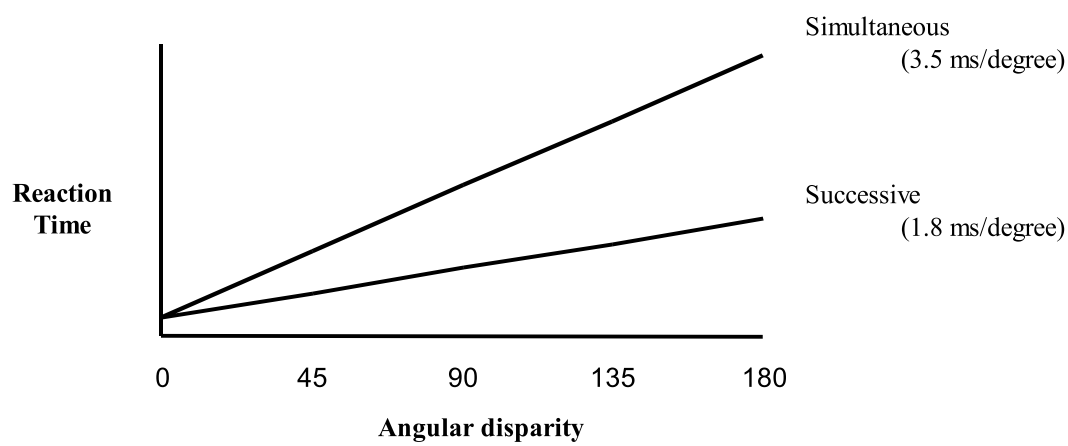


Figure 6. Slope differences between successive and simultaneous paradigms (Cohen & Kubovy, 1993).

Finally, Cohen and Kubovy (1993) found that when participants are encouraged to respond quickly (termed *RT pressure*) in a handedness decision task, and only two sets of stimuli are presented to each participant throughout the experiment, participants can complete the task without using "mental rotation." Cohen and Kubovy implemented RT pressure by sounding a tone if the participant responded too slowly on a given trial. To calculate the deadline for the implementation of the RT pressure, Cohen and Kubovy used a moving window of the previous forty, correct trials. If, on a given trial, a participant exceeded the .85 quantile of the RTs of those previous forty trials, the computer beeped after the trial was over. This beep indicated that the response was too slow on that trial, and encouraged participants to respond more quickly on subsequent trials. The authors used a moving window to calculate the RT pressure deadline to adjust for participants' improvement on the task. Thus, as participants improved, the deadline shortened. So participants were constantly pressured to respond as quickly as possible.

The RT pressure was manipulated between-groups. The group that did not receive the RT pressure showed the normal pattern of data: RT was a monotonically increasing function of angular disparity, suggesting that participants used "mental rotation" to complete the task. However, the group that received the RT pressure produced slopes that were basically flat and faster than the previously described LRC, indicating that they performed the task without using "mental rotation" (See Figure 7).

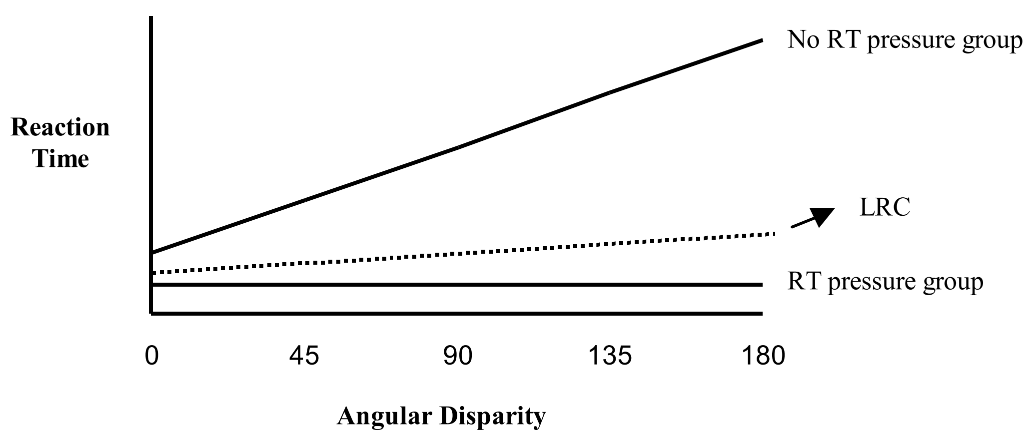


Figure 7. The RT pressure group did not use “mental rotation” and the No RT pressure group did use “mental rotation” (Cohen & Kubovy, 1993).

Because both groups performed the same task, these findings suggest that handedness information may have been available to the group that did not receive the RT pressure, but they did not use it. These findings suggest that "mental rotation" is not always necessary in handedness decision tasks.

Cohen & Blair (1998) extended these findings in a *small n* ABA design. Participants were tested during multiple sessions over a three-month period. First, participants completed a baseline stage, during which they performed standard handedness decision tasks until their slopes stabilized. During this baseline stage, all of the slopes were steeper than the LRC. Once stability was reached, RT pressure was inserted using the same procedure as Cohen and Kubovy (1993). During this experimental stage, all three of the participants' slopes dropped to or below the LRC. Two of the three participants in this study reached stability in the RT pressure condition. Once this occurred the RT pressure was removed. During this replication of the baseline stage, participants' rotation rates stayed at or below the LRC even without the RT pressure. These data suggest that participants were using a different, more efficient strategy than "mental rotation" in determining handedness. Either the RT pressure forced the participants to use handedness information already available to them or it forced them to learn a new strategy for encoding the handedness information without "mental rotation."

To review, the Shepard model is widely accepted because it accounts for most of the RT data from handedness decision tasks, in that RT is a monotonically increasing function of angular disparity. However, there are several inconsistencies with the Shepard model. First, Kail (1986) showed that with extended practice, participants could complete handedness decision tasks in times that reflect rotation rates faster than the LRC. Second, although the Shepard model predicts that the simultaneous and successive paradigms would produce equivalent slopes, the slopes of the simultaneous paradigm are steeper. Finally, Cohen and Kubovy (1993) and Cohen and Blair (1998) showed that when RT pressure is inserted in handedness decision tasks, participants are able to complete the tasks without using "mental rotation." Although the Shepard model cannot account for the above data, several theories of object recognition, many based on the Shepard model, may explain some of these findings.

Object Recognition

People are able to recognize many different objects from many different orientations. For example, one can easily recognize his/her mother whether she is sleeping, running, crying, whether she is wearing a blue dress or a red sweater, or whether she is seen from the front, side, back, etc. This remarkable ability is referred to as *object recognition* and is a central feature of perception. It is unclear whether or not "mental rotation" or some other form of normalization is required for the recognition of familiar objects presented in novel orientations.

Object recognition tasks require participants to *recognize* familiar objects while handedness decision tasks require participants to distinguish an object from its mirror image. Object recognition has been extensively studied using many different paradigms, including recognition tasks that are analogous to handedness decision tasks. In these types of tasks, participants learn a set of objects during a practice phase. In an experimental phase, test stimuli are presented, and the participants are to respond "yes" if the stimulus was one of the learned objects and "no" if it was not. Half of the test stimuli are objects from the previously learned set presented at novel viewpoints, and the other half are similar distracters. Some of the studies that use these types of recognition tasks have yielded results that show orientation effects (i.e., RT is a function of the angular disparity between the learned and presented stimuli). These studies suggest that a process of normalization, possibly "mental rotation," is needed to complete the task (Jolicoeur, 1985; Jolicoeur & Millikin, 1989; Murray, 1995; 1997; Tarr, 1995; Tarr & Pinker, 1989). However, some of these studies have shown no orientation effects indicating that no normalization is needed (Biederman, 1987; Biederman & Cooper, 1991, 1992).

It should be noted that recognizing familiar stimuli presented at novel viewpoints is different from determining the handedness of misoriented stimuli. Farah & Hammond (1988) studied a patient who had a large right middle cerebral artery territory stroke. This patient did poorly on handedness decision tasks, but well on tasks in which he had to recognize misoriented numbers, letters, and drawings. This evidence suggests that recognizing familiar objects in new orientations and determining the handedness of misoriented objects are different processes that rely on separate cognitive functions. As stated before, it is unclear whether or not a normalization process is needed for the recognition of misoriented objects.

Theories of Object Recognition

Conflicting findings have led to three types of theories regarding object recognition: Viewpoint independent, viewpoint dependent, and double-checking theories. Viewpoint independent theories assume objects are encoded independent of viewpoint, and "mental rotation" is not necessary for the recognition of familiar objects presented in novel orientations. Biederman's (1987) *recognition by components theory* (RBC) is a viewpoint independent theory. Viewpoint dependent theories assume objects are encoded in a viewer-centered fashion, and the recognition of objects presented at novel viewpoints will be impaired. Jolicoeur's (1990) *dual systems theory*, Rock's (Rock, 1974; Rock & DiVita, 1987) theory of *viewer-centered object perception*, and Tarr and Pinker's (1989) *multiple views theory* are examples of viewpoint dependent theories. Double Checking theories assume objects are encoded in a viewpoint independent fashion and normalization is done as a double-checking mechanism to ensure that no mistakes in recognition are made. DeCarro & Reeves (2000) *rotate to orient hypothesis*, and Corballis's (1988) *double check hypothesis* are examples of double-checking theories.

Viewpoint Independent Theories

Biederman's (1987) recognition by components theory (RBC) is probably the most famous orientation independent theory of object recognition. The basic assumption of RBC is that all objects can be decomposed into a relatively small set of geometrical components, such as blocks, cylinders, wedges, and cones. Biederman calls these components *geons*. According to RBC, the first step in recognizing an object is creating a three-dimensional representation of that object that is composed of these geons. The viewer recognizes the object based on the number, type, and arrangement of the geons. Because the viewer's internal representation of the object is three-dimensional, it contains information about how the object "looks" from all viewpoints. Therefore, it is an orientation independent representation and recognition of misoriented objects must be invariant across orientation (i.e., does not require "mental rotation").

It is generally accepted that "mental rotation" is used in handedness decision tasks. If one assumes that handedness information *can* be included in the RBC representation, and participants do not always use it, the RBC theory may explain the lack of "mental rotation" in the findings of Kail (1986), Cohen and Kubovy (1993), and Cohen and Blair (1998). The only difference between an object and its mirror image is that the global relations between the geons are mirror reversed. If a viewer does not encode the generally unnecessary handedness of the arrangement of geons, then he/she needs a strategy to distinguish an object from its mirror image (i.e., "mental rotation"). It is possible that the extended practice in the Kail study and the RT pressure in the Cohen and Kubovy and Cohen and Blair studies allowed participants to use handedness information that may have already been available. In these cases, "mental rotation" would not have been necessary. RBC is unable to explain the differences in slopes between the simultaneous and successive paradigms.

Tarr and Bultroff (1995) point out some shortcomings of Biederman's RBC theory. They argue that Biederman's proposal lacks generality, and the viewpoint invariant data that supports his theory may be due to "experimental artifacts." They believe that the stimuli used by Biederman were too dissimilar, and the target stimuli could be easily distinguished from the distracters based on a few diagnostic features. In this manner, participants would be using a categorization strategy to make their decisions. Tarr and Bultroff argue that a strong test of viewpoint invariance should contain stimuli that minimize the likelihood of a small set of features being diagnostic. Tarr (1995, p. 57) proposes "human object recognition may be thought of as a continuum in which the most extreme exemplar-specific discriminations recruit exclusively viewpoint-dependent mechanisms, while the most extreme categorizations recruit exclusively viewpoint-invariant mechanisms."

Viewpoint Dependent Theories

Viewpoint dependent theories assume objects are encoded in a viewer-centered fashion, and the recognition of objects presented at novel viewpoints will be impaired. Several studies have shown the recognition of misoriented objects to be orientation dependent (Jolicoeur, 1985; Jolicoeur & Millikin, 1989; Murray, 1995, 1997; Tarr, 1995; Tarr & Pinker, 1989). Tarr and Pinker (1989, p. 276) posit; "…representations used in recognition are specific to a shape in a particular orientation and a particular handedness. The representations thus are concrete or pictorial, in the sense that they are specific to the local arrangement of the object's parts in the visual field at a particular viewing orientation." Tarr and Pinker go on to argue that recognizing misoriented objects is achieved by "aligning an image of the observed object with a representation of the object at a stored orientation." Decarro and Reeves (2000) referred to this theory as the *rotate-to-recognize* theory, which states that a process of "mental rotation" from the presented viewpoint to a familiar viewpoint is necessary for the recognition of misoriented objects.

Tarr & Pinker's (1989; Tarr, 1995) multiple views theory is an extension of the rotate-to-recognize theory. Tarr and Pinker propose that people can store multiple views of the same object from different orientations. The object can be immediately recognized from each of these viewpoints. However, when the same object is seen from a novel viewpoint, the viewer must imagine the object rotate into the closest stored viewpoint.

Tarr and Pinker (1989) provided evidence for the multiple views theory in a two-phase experiment. During a learning phase, participants were presented with objects in several orientations. The participants practiced recognizing these objects in those specific orientations until their performance was equally fast across orientation. In a testing phase, the participants were presented with the same stimuli in new orientations, and were asked to identify them as quickly as possible. The data showed that RT was a function of the angular disparity between the presented stimuli and the closest previously learned orientation. Tarr and Pinker argued that participants stored the objects in the learned orientations during the learning phase of the experiment and performed "mental rotation" in the experimental phase to align an image of the presented stimulus to the closest learned orientation. From this data, Tarr and Pinker argue that represented views may be distributed so as to minimize normalization. Although performance seems orientation independent, the participant is still performing "mental rotation." Tarr and Pinker (1989, p. 277) argue that when participants have to recognize familiar objects presented in unfamiliar orientations "it triggers a transformation process that aligns the observed object with one of several learned representations via the shortest path."

Tarr and Pinker's multiple views theory accounts for Kail's (1986) data. Recall that Kail found that when participants are given extended practice with the same stimuli in a handedness decision task, participants' performance becomes orientation invariant. It is possible that the participants were able to learn the stimuli in several distributed orientations and "rotate" the test stimuli to the nearest learned orientation. This process would make performance seem orientation invariant. However, Tarr and Pinker's multiple views theory cannot explain Cohen and Kubovy's (1993) findings. Recall that Cohen and Kubovy found that when RT pressure is inserted in a handedness decision task, participants completed the task without the use of "mental rotation." Cohen and Kubovy presented stimuli in unique orientations on every trial (i.e., 0° to 359° in 1° increments) so that participants were presented with each orientation only once. In this manner, it was impossible for participants to form multiple views of the stimuli. In addition, the data contained no indication of rotation to any orientation. Multiple views theory is also unable to explain the differences in slopes between the simultaneous and successive paradigms.

Jolicoeur's (1990) dual systems theory is also a viewpoint dependent theory. Jolicoeur proposes that object recognition is accomplished by two sub-systems. The first system uses a rotate-to-recognize procedure, in which "mental rotation" is used to recognize misoriented stimuli. Performance that relies on this system is orientation dependent, in that RT is a monotonically increasing function of angular disparity. The second system that Jolicoeur proposes is orientation independent. After becoming familiar with the stimuli, this system may allow participants to extract the distinguishing features of the objects. Therefore, recognition would be orientation independent, because the distinguishing features would be readily recognized in any orientation. Jolicoeur proposes that the orientation dependent system will be used until the participant becomes very familiar with the stimuli being used, at which time the orientation independent system takes over.

If Jolicoeur's (1990) dual systems theory could be expanded to handedness decisions, it would also account for the lack of "mental rotation" found by Kail (1986). It is possible that the extended practice with the same stimuli allowed participants to use the viewpoint independent system. The dual systems theory would also account for Cohen and Kubovy's (1993) and Cohen and Blair's (1998) findings. Cohen and Kubovy and Cohen and Blair found that RT pressure can eliminate "mental rotation" only when two stimuli are used throughout the experiment. Orientation effects were still observed when different stimuli were used on each trial. This suggests that orientation invariant handedness information can be encoded only when the participants become familiar with the stimuli. This finding supports Jolicoeur's dual systems theory. However, the dual systems theory is unable to account for differences in slopes between the simultaneous and successive paradigms.

Rock takes a different view about the recognition of familiar objects presented at novel viewpoints. Rock proposed a theory of viewer centered object perception, according to which object recognition is viewpoint dependent, but "mental rotation" is not always used (Rock, 1974; Rock & DiVita, 1987). Rock makes the point that an object can be changed in certain ways and still be easily recognized. For example, if you change the color or the size of a square it will still be perceived as a square. However, if you rotate a square 45° it will no longer be perceived as a square. In fact, it will be perceived as a diamond. A square that has been rotated 45° still retains all the characteristics of its internal geometry (i.e., angles, line size, etc.) and yet it is perceived differently. Rock (1974, p. 78) concludes that, "The partial rotation of even a simple figure can prevent its recognition."

Rock and DiVita (1987) tested the theory of viewer centered object recognition by presenting participants with 3D wire objects from a single viewpoint. Recognition was later tested on these objects either from the same viewpoint, a different viewpoint that produced the same image on the retina, or a different viewpoint that produced a different image on the retina. Participants were unable to recognize the rotated objects that produced a different image on the retina. Rock and DiVita argue that these data provide support for a viewer centered (egocentric) model of object perception, according to which recognition of misoriented objects is impaired.

Rock (1974) makes the important contribution that "mental rotation" is not *automatically* initiated when one must recognize misoriented objects. For example, faces and words are very difficult to recognize when presented upside down. Because the orientation of the faces and words is readily recognized (i.e., people immediately recognize that the words and faces are upside-down), an automatic "mental rotation" process should readily re-orient them, making recognition simple. However, because recognition of faces and words is drastically impaired when they are presented upside-down, it is unlikely that "mental rotation" can be implemented in these situations. Along this same reasoning, Pani and colleagues showed that participants are unable to determine the outcome of certain rotations (Pani, 1993, 1997; Pani & Dupree, 1994; Pani, Jeffres, Shippey, & Schwartz, 1996; Pani, William, & Shippey, 1995). They have demonstrated that if the axis of rotation is tilted in relation to the environment and the object that is rotating is tilted in relation to the axis of rotation, participants are unable to determine the outcome of imagined rotations. Once again, these findings show that "mental rotation" is not a process that is automatically used in these situations.

Corballis (1986) provided further evidence that "mental rotation" is not automatically used when recognizing misoriented objects. Corballis had participants complete handedness decision tasks while retaining information in short-term memory. The memory load slowed overall RTs for the handedness decision task but had no effect on the slopes (i.e., the "rotation rates"). Corballis argued that it takes attention to set up the "mental rotation" process, but actually carrying out the process seems to be left to smaller, subordinate systems that do not compete for attention. Corballis argued that because starting “mental rotation” requires attention, it is not an automatic process.

Although Rock’s theory of viewer centered object perception cannot account for the inconsistencies with the Shepard model, he does make the important contribution that “mental rotation” is not an automatic process used for object recognition. It is, instead, a “strategy” that is perhaps used in artificial situations such as during an experimental procedure. This may even explain *why* “mental rotation” is ever used.

Double Check Theories

Double Check theories assume objects are encoded in a viewpoint independent fashion and “mental rotation” is used as a double-checking mechanism to ensure that no mistakes in recognition are made. Corballis (1988) proposed a double check model that is similar to the previously described Shepard model (See Figure 8).

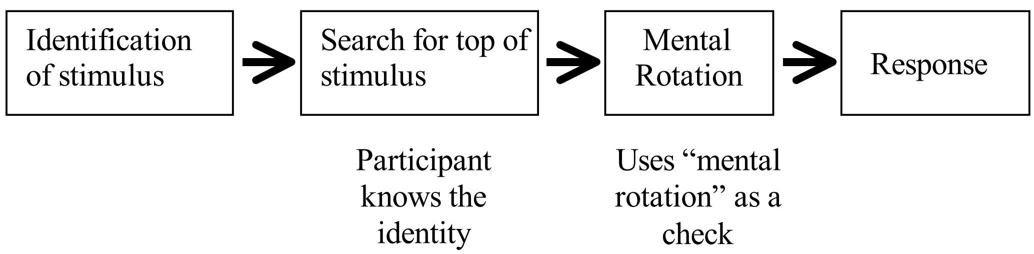


Figure 8. A schematic representation of the double check model.

As with the Shepard model, the first stage is the identification of the stimulus. Once again, this stage is assumed to be invariant of orientation. During the second stage, the participant determines the orientation of the stimulus, which presumably includes locating the top of the stimulus. Corballis proposes that the participant knows the identity of the stimuli at this point, but uses “mental rotation” to double check and make sure that the identification was correct. Finally, the participant makes a decision about the identity of the stimulus.

Corballis’s (1988) double check model does not explain *why* people double-check. DeCarro and Reeves’s (2000) *rotate to orient theory* is a double check model that provides a possible explanation. According to the *rotate to orient theory*, the first step in the recognition process is viewpoint independent. This means that participants are able to recognize an object from novel viewpoints without “mental rotation.” However, “mental rotation” is performed to establish the object’s orientation. In other words, “mental rotation” is used to ensure that the stimulus has been perceived from the correct orientation. DeCarro and Reeves argue that accidents in viewpoint are common in the “real world,” and it is possible that participants need to check an object’s orientation to confirm their decision about its identity. DeCarro and Reeves found that participants are able to identify an object before they are able to identify that object’s orientation. They argue that these data support the *rotate to orient theory* in which identity is encoded first and, “mental rotation” is used to verify the object’s orientation.

It is not clear whether the double check model can be extended to handedness decisions. However, this extension would provide plausible explanations for the inconsistencies earlier noted with the Shepard model. The double check model can explain the differences in slopes found between the simultaneous and successive paradigms by assuming participants double check their judgments more often in the simultaneous paradigm. This may be a reasonable hypothesis because in the simultaneous paradigm both stimuli are present and available for comparison at the same time. Double-checking may be relatively easy in this case because participants are able to simply look back and forth at the two stimuli. However, in the successive paradigm, the participants are unable to look back and forth at the two stimuli because one stimulus is encoded in memory. Thus, participants may be reluctant to double check. This explanation can account for the difference in slopes between the simultaneous and successive paradigms because the slope relating RT and angular disparity increases with every extra check.

The double check models may also explain the findings of Kail (1986). Recall that Kail used the same stimuli and found that extended practice drastically reduces orientation effects in handed decision tasks. Perhaps, extended practice enables participants to become more comfortable with their ability to make handedness decisions with a given set of stimuli allowing them to forgo the unnecessary double-checking stage of "mental rotation."

Finally, the double check models may also explain the findings of Cohen and Kubovy (1993) and Cohen and Blair (1998). It is possible that the RT pressure inserted in the handedness decision tasks may have encouraged or even forced the participants to abandon the unnecessary double-checking stage of "mental rotation." This would have allowed the participants to use the handedness information that was already available to them.

Several theories of object recognition have been discussed in an effort to account for the inconsistencies noted with the Shepard model. Rock's theory of viewer centered object perception (Rock & DiVita, 1987) is unable to account for any of these inconsistencies. Biederman's (1987) RBC and Jolicoeur's (1990) dual systems theory can explain the lack of "mental rotation" found by Kail (1986), Cohen and Kubovy (1993), and Cohen and Blair (1998). However, these models cannot account for the slope differences between simultaneous and successive paradigms. Tarr and Pinker's (1989) multiple views theory can account for lack of "mental rotation" due to extended practice, but not for the lack of "mental rotation" under RT pressure. The multiple views theory is also unable to account for the slope differences between simultaneous and successive paradigms. The double check model is the only model that can account for all of the inconsistencies noted with the Shepard model.

Goal of the Present Study

The goal of the present study is to investigate whether participants know the handedness of misoriented stimuli before using "mental rotation" in a handedness decision task. If participants know the handedness before using "mental rotation," this would provide support for the double check model (and possibly the RBC model). These results would suggest that the RT pressure in Cohen and Kubovy (1993) and Cohen and Blair (1998) allowed participants to use handedness information that was already available. However, if participants do not know the handedness before using "mental rotation," it would provide support for theories that propose an initial viewpoint dependent strategy of recognition, followed by viewpoint invariant recognition, such as Biederman's RBC (1987) and Joliceur's dual systems theory (1990). These results would suggest that the RT pressure in Cohen and Kubovy and Cohen and Blair forced participants to learn a new strategy for encoding the handedness information of misoriented objects. Finally, neither Tarr and Pinker's (1989) multiple views theory nor Rock's (1987) theory of viewer centered object perception can account for the lack of "mental rotation" found by Cohen and Kubovy and Cohen and Blair.

General Method

To test whether participants know the handedness of misoriented stimuli before using "mental rotation," I designed a dualtask procedure in which a handedness decision task is randomly interrupted with a side decision task. The first 100 trials of this dualtask procedure make up a practice phase. During the practice phase, participants complete standard handedness decision trials, in which two stimuli (fourteen-sided irregular polygons randomly created by the computer) are presented simultaneously and they respond "same" if the stimuli are identical except for orientation and "different" if they are mirror images (See Figure 9).

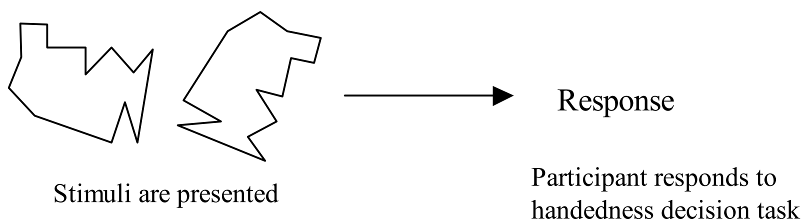


Figure 9. A standard handedness decision trial.

After completing the practice phase, the participants start the experimental phase. During this phase the participants are to treat all trials as standard handedness decision trials. However, half of these trials are randomly interrupted with a side decision task. This interruption consists of shading in either the left or the right stimulus. At this point, the participants must abandon the handedness decision task and complete the side decision task. The side decision task consists of pressing the response key that is on the same side as the shaded in stimulus. Thus, if the right stimulus is shaded in, the participants must respond with the right key, and if the left stimulus is shaded in, the participants must respond with the left key. So, there are two types of trials in the experimental phase: *interrupt* and *non-interrupt*. On the non-interrupt trials the participants complete standard handedness decision tasks, and on the interrupt trials the participants complete side decision tasks. These types of trials occur equally as often and are randomly intermixed with each other. Therefore, the participants are unable to predict when they will be interrupted, so they must begin each trial as if it were a non-interrupt trial.

The interrupts are timed so as to occur at varying stages of the "mental rotation" process proposed by the Shepard model. To calculate the appropriate interrupt time for each interrupt trial, a moving window of the previous thirty, correct, non-interrupt trials is used. The average RT for these trials is calculated and participants are interrupted at .2, .4, or .6 of that average. These values (.2, .4, and .6) will be referred to as *interrupt proportions*. The moving window allows me to interrupt the participants sooner as they get faster at the handedness decision tasks. See Figure 10 for a schematic representation of an interrupt trial.

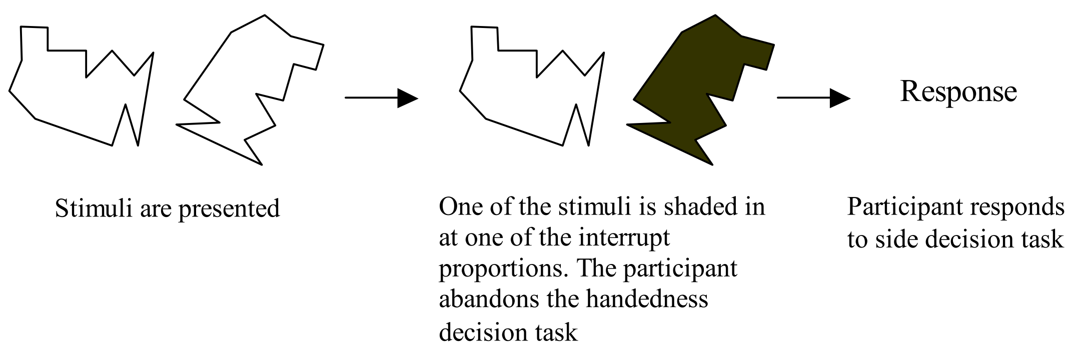


Figure 10. An interrupt trial.

There are two types of interrupt trials in this series of experiments: *facilitation* trials and *inhibition* trials. Facilitation trials are trials in which the responses to the handedness decision task and the side decision task are congruent (i.e., with the same key). Inhibition trials are trials in which the responses to the handedness decision task and the side decision task are incongruent (i.e., with different keys). The logic of this series of experiments is as follows: if participants know the handedness of the stimulus before they are interrupted with the side decision task, then RTs on facilitation trials will be shorter than RTs on inhibition trials. For example, if participants know that the two stimuli are the same before they are interrupted, they should have the "same" response hand "primed" and ready to initiate a response. If the corresponding stimulus is shaded in during the side decision task, facilitation should occur and RTs on facilitation trials should be relatively short. In contrast, during inhibition trials the opposite stimulus is shaded in. Thus, the participants will have to stop or inhibit the "same" response hand and initiate a response with the other hand. Consequently, RTs on the inhibition trials should be relatively long.

Interrupt Trial Predictions

For the interrupt trials in the dualtask paradigm, there will be a proportion of trials in which participants are interrupted with the side decision task while knowing the handedness of the misoriented stimulus. Figure 11 is another schematic diagram of an interrupt trial. The box plot represents a distribution of times in which participants know the handedness of the misoriented stimulus. The dotted line represents a certain interrupt time (i.e., a time in which one of the stimuli is shaded in). The part of the distribution above the dotted line represents the proportion of trials in which participants do not know the handedness of the misoriented stimulus before being interrupted with the side decision task, and the part of the distribution below the dotted line represents the proportion of trials in which participants do know the handedness of the misoriented stimulus before being interrupted with the side decision task

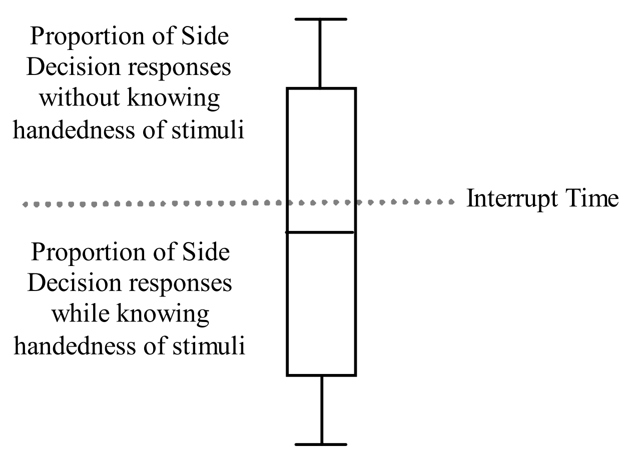


Figure 11. A distribution of times in which participants know the handedness of the stimuli.

Recall that the facilitation and inhibition effects depend on participants knowing the handedness of the stimuli before being interrupted with the side decision task. In other words, participants must know the handedness of the stimuli to have the correct hand primed on the facilitation trials and the incorrect hand primed on the inhibition trials. The following equation models the predicted results on the interrupt trials:

$$\delta * p(completedRotation) \tag{1}$$

where δ (delta) represents the facilitation and inhibition effects (i.e., the average RTs for facilitation trials should be shorter than the average RTs for inhibition trials), and *p(completedRotation)* is the proportion of side decision responses that occur while participants know the handedness of the stimuli

.

The following equation models the proportion of side decision responses that occur while participants know the handedness of the misoriented stimuli.

$$p(completedRotation) = \Theta\left[\frac{T_i - (\alpha * angle + C_1)}{\theta_{MR}}\right] \tag{2}$$

T_i is the interrupt time, α is the slope (i.e., "rotation speed"), *angle* is the angular disparity between the two stimuli, C_1 is a constant that represents the first two stages of the Shepard model, and θ_{MR} is the standard deviation of the "mental rotation" completion times. Θ (theta) represents the cumulative normal distribution and is used to model the proportion of trials.

Recall that the Shepard model assumes that "mental rotation" is necessary to make a handedness decision about misoriented stimuli. Therefore, the Shepard model predicts that the time in which participants know the handedness of the misoriented stimulus equals the time to complete "mental rotation." Thus, participants will know the handedness of stimuli with smaller angular disparities before they know the handedness of stimuli with larger angular disparities. The Shepard model also assumes that participants initiate a response shortly after they complete "mental rotation." If this model is correct, participants will have completed "mental rotation" and thus know the handedness of the misoriented stimulus before being interrupted with the side decision task on a greater proportion of trials with smaller angular disparities. This proportion should decrease as angular disparity increases. Therefore, the facilitation and inhibition effects should be relatively large for stimuli with small angular disparities (i.e., the average RTs for inhibition trials should be longer than the average RTs for facilitation trials). Furthermore, the disparity between the facilitation and inhibition trials should decrease as angular disparity increases.

The Shepard model also predicts that trials with longer interrupt times will have greater facilitation and inhibition effects. Longer interrupt times provide participants with more time to determine the handedness of the misoriented stimuli. Therefore, they will know the handedness of the stimuli before being interrupted with the side decision task on a greater proportion of trials with longer interrupt times. Thus, as interrupt time increases, the facilitation and inhibition effects should increase as well. In summary, the Shepard model predicts an effect of both angular disparity and interrupt proportion. Both of these effects can be observed in Figure 12 (a graphical prediction of the Shepard model for the interrupt trials).

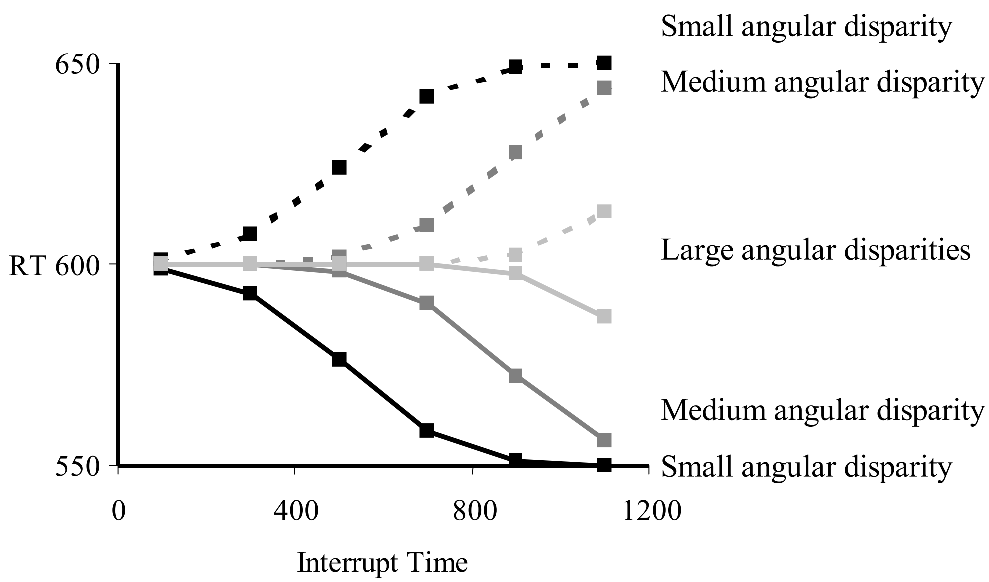


Figure 12. Predictions of the Shepard model. RT plotted as a function of interrupt time and separated by angular disparity.

Figure 12 is a graph of the average RTs for facilitation and inhibition trials plotted as a function of interrupt time and separated by angular disparity. The dotted lines represent inhibition trials and the solid lines represent facilitation trials. The darker lines represent trials with small angular disparities and as the lines get lighter angular disparity increases. Notice that the average RTs for inhibition trials is longer than the average RTs for facilitation trials and the discrepancy between the two trial types increases as interrupt time increases. This characterizes the effect of interrupt proportion. Also notice that the facilitation and inhibition effects are larger for smaller angular disparities and these effects decrease as angular disparity increases. This characterizes the effect of angular disparity.

Cohen's *d* effect size is an important concept that provides an estimate of how powerful a particular manipulation is. Although a null hypothesis test of two means may be statistically significant, Cohen's *d* effect size provides a more specific estimate of the magnitude of the difference. It is calculated by dividing the difference between two sample means by the square root of the summed variances associated with those means. This provides a measure of the distance between two sample means in terms of standard deviations. Cohen's *d* effect size can be applied to this series of experiments to investigate the magnitude of the facilitation and inhibition effects.

For this series of experiments, the average facilitation and inhibition RTs for each interrupt proportion and angular disparity will be used as the two sample means. Therefore, Cohen's *d* effect sizes are calculated by subtracted the average facilitation RT from the average inhibition RT for each interrupt proportion and each angular disparity. This value is then divided by the square root of the sum of the variances associated with each of these averages. This value provides a powerful measure of the facilitation and inhibition effects for each interrupt proportion at each angular disparity. Figure 13 is the graphical prediction of the Shepard model.

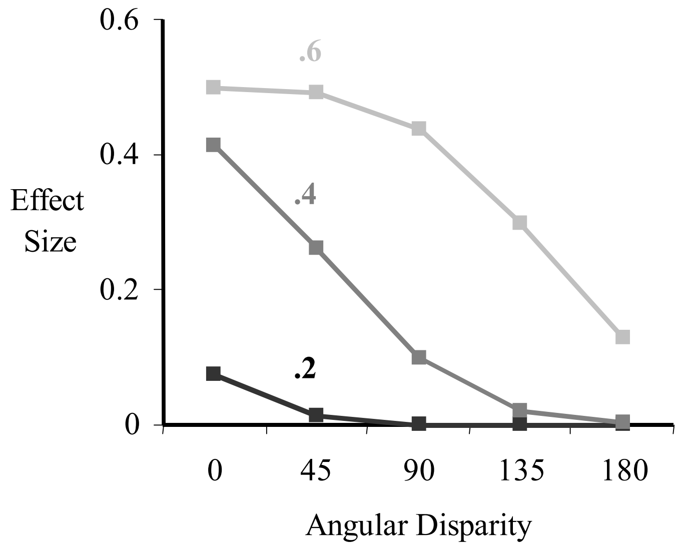


Figure 13. Shepard model predictions. Effect sizes plotted as a function of angular disparity and separated by interrupt proportion.

Figure 13 is a graph of effect sizes plotted as a function of angular disparity and separated by interrupt proportion. The dark line represents a small interrupt proportion (i.e., short interrupt times), and as the lines get lighter the interrupt proportion increases. Notice that as angular disparity increases, the effect sizes decrease. Once again, this characterizes the predicted effect of angular disparity. Notice also that the effect sizes are very small for small interrupt proportions, and increase as interrupt proportion increases. This characterizes the predicted effect of interrupt proportion. If the patterns of results from Figures 12 and 13 were obtained, I would conclude that participants must use "mental rotation" to know the handedness of misoriented stimuli. This would provide support for the Shepard model.

Recall that the double check model assumes that "mental rotation" is used as a double-checking strategy to ensure that one's answer on a trial is indeed the correct answer. Therefore, the double check model assumes that participants know the handedness of the misoriented stimulus before using "mental rotation." The double check model also assumes that participants do not initiate a response to the handedness decision task until after they complete "mental rotation." Consequently, there should be a proportion of trials in which participants are interrupted with the side decision while knowing the handedness of the misoriented stimulus, but before they have initiated a response to the handedness decision task. Once again, longer interrupt times provide participants with more time to determine the handedness of the misoriented stimulus before being interrupted with the side decision task. Therefore, the double check model predicts an effect of interrupt proportion. Specifically, facilitation and inhibition effects should increase as interrupt time increases.

Once again, the double check model predicts that participants know the handedness of the misoriented stimulus before using “mental rotation.” Therefore, participants will know the handedness of stimuli with large angular disparities at approximately the same time as stimuli with small angular disparities. Thus, the double check model predicts no effect of angular disparity. Specifically, the facilitation and inhibition effects should be invariant across angular disparity. In summary, the double check model predicts an effect of interrupt proportion but no effect of angular disparity. This pattern of results can be observed in Figure 14 (a graphical prediction of the double check model for the interrupt trials).

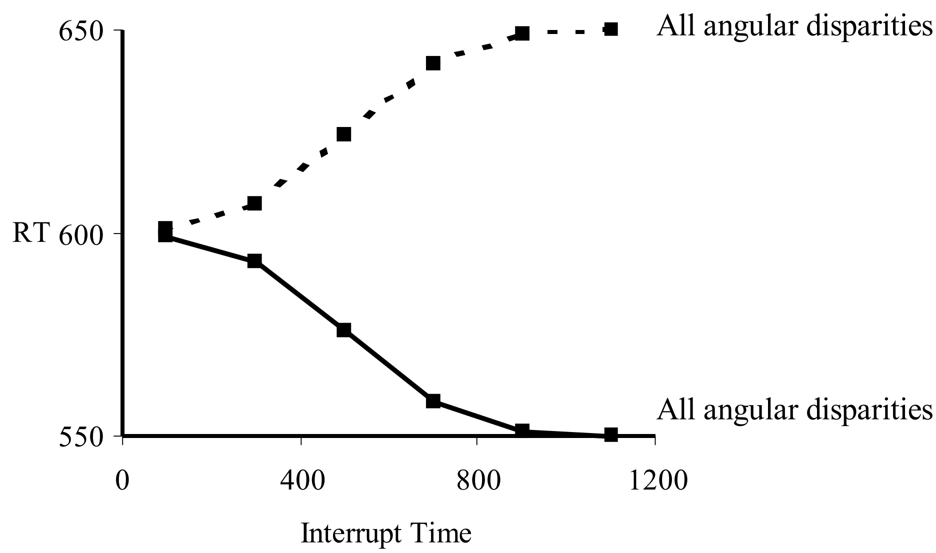


Figure 14. Predictions of the double check model. RT plotted as a function of interrupt time.

Figure 14 is a graph of the average RTs for facilitation and inhibition trials plotted as a function of interrupt time. Once again, the dotted line represents inhibition trials and the solid line represents facilitation trials. The average RTs for the inhibition trials are longer than the average RTs for the facilitation trials and the discrepancy increases as angular disparity increases. This characterizes the effect of interrupt proportion. The double check model predicts no effect of angular disparity (i.e., RTs will be invariant across angular disparities). Thus, there are no separate lines for different angular disparities in Figure 14. Figure 15 is a graphical prediction of the double check model for effect sizes.

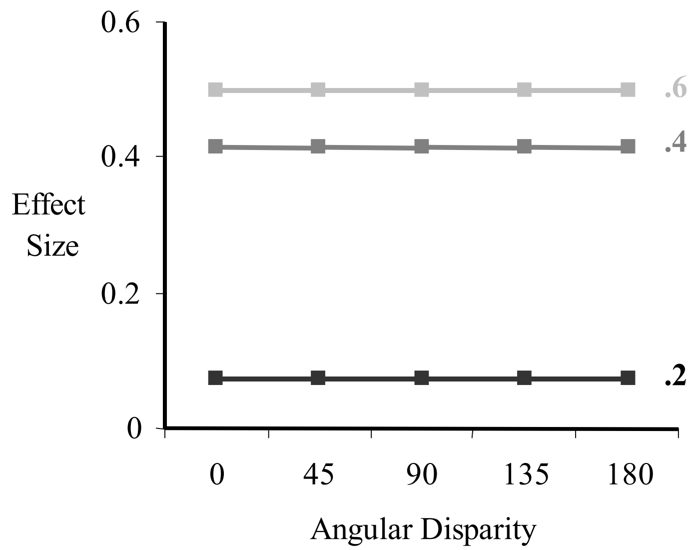


Figure 15. Double check model predictions. Effect sizes plotted as a function of angular disparity and separated by interrupt proportion.

Figure 15 is a graph of effect sizes plotted as a function of angular disparity and separated by interrupt proportion. Once again, the dark line represents a small interrupt proportion (i.e., short interrupt times), and as the lines get lighter the interrupt proportion increases. Notice that the effect sizes are very small for small interrupt proportions, and increase as interrupt proportion increases. This characterizes the predicted effect of interrupt proportion. Notice also that effect sizes are constant across angular disparities. This characterizes the absence of an effect of angular disparity. If the patterns of results from Figures 14 and 15 were obtained, I would conclude that participants knew the handedness of the misoriented stimulus before using "mental rotation." This would provide support for the double check model.

Dualtask Issues

Studies in which participants are presented with two temporally overlapping tasks have been used numerous times in the past. These types of studies have been referred to as "dual-task" experiments (Pashler, 1984). One of the major purposes of this type of experiment is to examine how a processing system breaks down as it is overloaded with information. This procedure makes it possible to examine the components of a processing system and how they work together (Pashler, 1994).

Sternberg (1969, 1998a, 1998b) argues that many of the experiments that use the dualtask method make the assumption of *pure insertion*. Sternberg (1998a, p. 734) proposes, "this assumption states that changing from task 1 to task 2 merely inserts a new processing stage." However, changing from one task to another *may* fundamentally change the way in which participants complete the task. Thus, this change may not simply insert a new processing stage.

So, a critical issue in the present series of experiments is to ensure that the insertion of the side decision task does not fundamentally change the way in which participants complete the handedness decision task. The dualtask procedure I have developed is a variant of the *titrated reaction time* (TRT) procedure developed by Meyer, Irwin, Osman, and Kounios (1988). Meyer et al. developed the TRT procedure in an effort to test various models of information processing and accumulated information in lexical decision tasks. Specifically, the authors wanted to know whether information is passed from one level of processing to the next in a discrete manner, as proposed by the *discrete stage model* (Shepard, 1969), or whether information is accumulated gradually throughout completion of the task as proposed by the cascade model (McClelland, 1979) and the *stochastic diffusion model* (Ratcliff, 1978).

The TRT procedure used by Meyer et al. (1988) consisted of two types of trials (*normal* trials and *signal* trials) that were randomly intermixed with each other. On the normal trials, the participant completed some form of a lexical decision task (i.e., decide if a string of letters is a real English word or not). On the signal trials the participants had to complete the same task, however at some point before completion of the task, an auditory signal was given to the participants. At this point, the participant was to immediately initiate a response to the lexical decision task using any information they had accumulated up to that point. As with my dualtask procedure, the participants were unable to predict which type of trial was upcoming. Therefore, they were to treat each trial as a normal trial.

Using the results from the normal trials and the accuracy and RTs on the signal trials, Meyer et al. (1988) were able to get an estimate of how much useful information the participants had accumulated at various times during the task. They did, indeed, find that under certain conditions useful information was available to the participants before they would have responded on a normal trial. Perhaps more relevant to the current dualtask procedure is the fact that intermixing normal trials and signal trials did not fundamentally change the way in which participants completed the normal trials. One of the necessary assumptions of the TRT procedure was termed the *temporal-independence assumption*. Essentially, this assumption states that inserting the signal trials did not change the way participants completed the normal lexical decision tasks. Meyer et al. (1988, p. 196) state that the temporal-independence assumption “implies that the initiation and execution of the guessing processes does not terminate the normal process prematurely or otherwise interfere with their progress toward successful completion.”

The temporal independence assumption was supported throughout the series of experiments (Meyer et al., 1988). Specifically, the results from the normal trials coincided with previous research using lexical decision tasks suggesting that participants were using normal processing to complete the trials. Further evidence for this assumption was provided in Experiment 4, in which Meyer et al. used two types trial blocks. In one block of trials, participants completed normal trials randomly intermixed with signal trials. In the other block of trials, participants were presented with only normal trials. Participants were notified of the type of trial block before each block was started. Meyer et al. compared the results of the normal trials from the two types of blocks to see if participants were using the same strategy to complete the normal trials in both conditions. Indeed, the pattern of results from the normal trials in the two conditions was very similar. The only difference was that participants were a little faster and little less accurate during the block in which the normal trials were intermixed with the signal trials. So it seems that inserting signal trials into the paradigm caused participants to sacrifice accuracy for speed. However, inserting signal trials did not fundamentally change the way in which participants completed the normal lexical decision tasks.

Meyer et al (1988) showed that this type of dualtask research is valid. Also, it seems that the violation of pure insertion is relatively nonessential in the present series of experiments. Any changes in the way in which participants complete normal handedness decision tasks will be revealed by the non-interrupt trials from the experimental phase. Specifically, I am able to tell if the participants are using "mental rotation" on these trials by calculating the slope relating angular disparity to RT. Moreover, the present series of experiments was designed to investigate whether or not "mental rotation" is necessary for the completion of handedness decision tasks. Therefore, the effects of the task switch on the completion of the side decision task are most relevant. Furthermore, it is highly unlikely and counterintuitive to suggest that adding a task switch will suddenly allow the participants to know the handedness of the misoriented stimulus if they would not have known it in a standard handedness decision trial.

In an effort to better understand the "mental rotation" process, I conducted four experiments. Experiment 1 was a validation of my dualtask procedure and analysis. The conditions of Experiment 1 required that participants use "mental rotation" to know the handedness of misoriented stimuli. Therefore, the dualtask procedure and analysis was validated because it yielded an effect of angular disparity. Experiment 2 tested whether or not participants knew the handedness of misoriented stimuli before using "mental rotation" when only two stimuli were presented to each participant throughout the experiment. Experiments 3 and 4 extended the findings of Experiments 1 and 2 with more participants and more interrupt proportions. A brief discussion of the results of each experiment will be will provided in the *Discussion* section of each experiment, however, a comprehensive review of the findings and a thorough discussion of the results will be saved for the *General Discussion* section.

EXPERIMENT 1

Experiment 1 was designed to validate the dualtask procedure and analysis. To validate the dualtask procedure I must (1) show that participants use "mental rotation" to complete the handedness decision task, (2) show that the insertion of the side decision task does not change the way participants complete the handedness decision task, and (3) show that the dualtask procedure is sensitive to the effects of both interrupt proportion and angular disparity for the interrupt trials. I used a handedness decision task that apparently *requires* the use of "mental rotation." Recall that Cohen & Kubovy (1993) showed that orientation effects could not be eliminated when different stimuli are presented on every trial. Presumably, participants are unable to encode handedness information about unfamiliar stimuli presented in novel orientations. Therefore, in Experiment 1, I presented each participant with a different pair of stimuli on each trial. I predicted an effect of angular disparity, which would show that participants must use "mental rotation" to know the handedness of the misoriented stimuli.

Method

Participants

Sixty-one undergraduate university students volunteered to participate in this experiment. Data from 21 of these participants were not used in the present analyses, because their error rates on the non-interrupt trials was greater than 25 percent. This indicated that these participants did not understand the handedness decision task. I assumed that this high error rate was due to insufficient instructions. This problem was corrected in the subsequent experiments.

Stimuli and Apparatus

The experimental sessions were conducted on one of two Gateway ™ 2000 computers with Pentium II processors running Microsoft Windows operating system, with 17'' monitors. Programs written specifically for this series of experiments were used. Throughout the experimental sessions the participants sat in front of one of the computers in a small dark room except when instructions were given. The participants were seated approximately (20 in.) 51 cm. from the screen.

A different pair of fourteen-sided irregular polygons randomly created by the computer was presented on every trial for each participant. Each polygon subtended 4.2° visual angle. A computer program written specifically for the production of these stimuli was used. First, the computer randomly selected 14 points within an imaginary 300 X 300 pixel matrix. The lines between these points were then connected clockwise around the center. The two points that were farthest from each other were scaled so that the stimulus took up 4.2° visual angle. If any side of the polygon was smaller than 0.5° visual angle, that polygon was discarded and another polygon was generated to replace it.

On each trial, two polygons were presented simultaneously, separated by 4.2° visual angle. I will refer to the polygon on the left as the *standard*. For each standard a *probe* was created and presented to the right of the standard. For half of the standards, the probe was a rotated version of the standard. For the remaining standards, the probe was a rotated mirror-reversed version of the standard. I presented standards and probes with angular disparities ranging from 0° to 315° in 45° increments.

Design and Procedure

Experiment 1 was an 8 X 3 X 2 X 2 X 2 X 2 within subject design, with eight levels of orientation (0° to 315° in 45° increments), three levels of interrupt proportion (.2, .4, and .6), two types of probe (same and different), two levels of trial type (interrupt and non-interrupt), two levels of interrupt side (left and right), and two levels of task correspondence (facilitation and inhibition). The experiment was completely factorial with all levels of each independent variable crossed with all levels of every other independent variable. Trials were randomly presented for each of the conditions. RT and percent error were the dependent variables. For the interrupt trials, I assessed facilitation and inhibition effects as described earlier in the *General Methods* section.

Practice phase. The first 100 trials were a practice phase in which the participants only completed handedness decision tasks. The participants' task was to decide if the two stimuli were the same or mirror images. The stimuli in the practice phase were presented with angular disparities ranging from 30° to 330° in 30° increments. These orientations were different from the orientations used in the experimental phase. This hindered participants from forming multiple views of the stimuli in Experiments 2 and 4 (when participants were presented with only two pairs of stimuli throughout the experiment). Half of the participants were instructed to respond by pressing the "D" key (left hand) when the stimuli were the same, and the "K" key (right hand) when the stimuli were different. The assignment of the response keys was reversed for the other half of the participants (i.e., the "K" key was the same response key and the "D" key was the different response key.) This eliminated any advantage one hand may have over the other. After each trial, accuracy feedback (i.e., "CORRECT" or "INCORRECT") was presented for 800 ms. After the accuracy feedback, there was a 500 ms blank screen before the onset of the next trial.

The practice phase allowed participants to get used to the task and using the keyboard to respond. Also, the last 30 correct trials from this block started the moving window that was used to calculate the interrupt proportions (discussed in the *General Methods* section). The participants were read the following directions before this block of trials:

> For the practice trials in this experiment, you will be presented with two polygons side by side that will vary in shape and orientation. The two polygons are considered the same if one of the polygons can be rotated in the two-dimensional plane so that it matches the other polygon. The polygons are considered to be different if they are mirror images of each other. If you decide that the polygons are the same, press the "d" key as quickly as possible. If you decide that the polygons are different, press the "k" key as quickly as possible. Speed is important, but accuracy is essential. There will be 100 practice trials in order to get used to the display and responding. Do you have any questions?

Experimental phase. There was a break between the practice phase and the experimental phase, during which the experimental directions were read to the participants (below).

> For the experimental trials, your task is the same as it was in the practice trials. That is, to decide if the polygons are the same or the mirror images of each other. So when you make your decision, press the appropriate key as quickly as possible. However, on some of the trials, either the left or the right polygon will be filled in with a white color. On these trials, **please ignore the polygons and the same-different task**, and press the response key on the same side as the filled in polygon as quickly as possible. So, if the right polygon is filled in press the "k" key, because the "k" key is on the right side of the keyboard. If the left polygon is filled in press the "d" key, because the "d" key is on the left side of the keyboard. So to review, your task is to decide if the polygons are the same or mirror images of each other. Please do this as quickly as possible. However, if one of the polygons is filled in, press the key on the side that corresponds to that polygon as quickly as possible. Do you have any questions?

The experimental phase consisted of 384 trials (not including errors). Trials in which errors were made were randomly re-presented throughout the session. There were also self-timed breaks every 100 trials.

Half of the experimental trials were non-interrupt trials (i.e., handedness decision trials), and the remaining trials were interrupted with a side decision task. The non-interrupt trials were standard handedness decision trials (i.e., like the trials in the practice phase). For an interrupt trial the stimuli were presented, and after a predetermined amount of time had elapsed either the left or the right stimulus was shaded in. At this point, the participant was to abandon the handedness decision task and respond with the hand that corresponded to the side of the shaded in stimulus. For example, the participant was instructed to respond by pressing the "K" key if the right stimulus was shaded in, and the "D" key if the left stimulus was shaded in. There were two types of interrupt trials: facilitation and inhibition. Facilitation trials were when the response key for the handedness decision task was the same as the response key for the side decision task. Inhibition trials were when the response key for the handedness decision task and the response key for the side decision task were different.

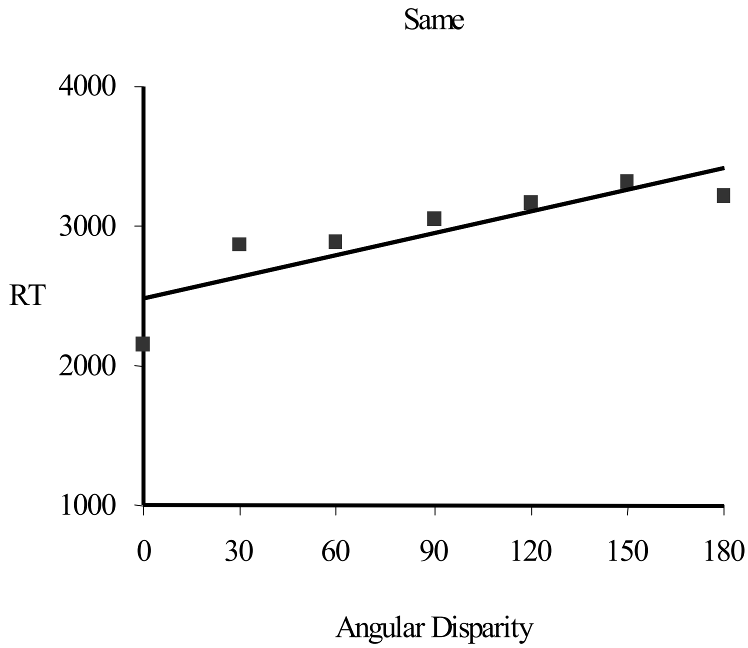
Results

There were three stages to the data analysis process: (1) Verify that the interrupt task does not affect how participants perform the handedness decision task by demonstrating that orientation effects on practice and non-interrupt trials are equivalent, (2) Identify interrupt trials in which the participants initiated a response to the handedness decision task before being interrupted with the side decision task, and (3) Assess the responses to the side decision task for facilitation and inhibition effects. All inferential analyses were conducted with an alpha level of .05. Furthermore, for all RT analyses, I only used trials in which the participant responded correctly. For the practice analysis, I excluded all trials that exceeded 10000 ms (3.27 SD from the mean, 1.25% of the trials). Most of the practice trials that were excluded were the participants' first and second trials, during which they were trying to understand the task. For the non-interrupt analysis, I also excluded all trials that exceeded 10000 ms (5.4 SD from the mean, 0.23% of the trials). For the side decision task analyses, I excluded trials with RTs exceeding 5000 ms (15.3 SD from the mean, 0.04 % of the trials).

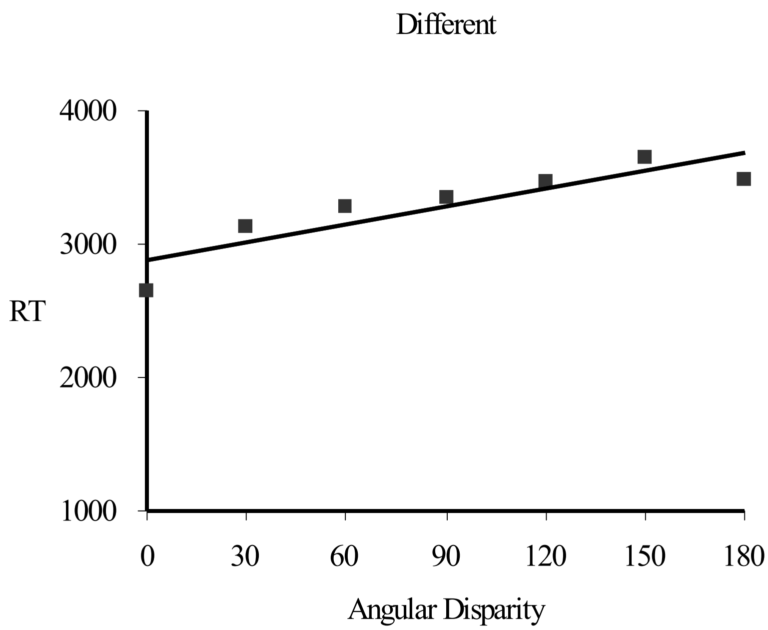
Comparison of Practice and Non-Interrupt Trials

As mentioned in the *Dualtask Issues* section of the General Methods, changing from one task to another may change the way in which participants complete either the first or the second task. Therefore, it was critical to verify that the interruption (i.e., the side decision task) did not affect how participants performed the handedness decision task. There were two types of trials in which participants were not interrupted with the side decision task; practice trials and non-interrupt trials. Participants were not interrupted with the side decision task at any time during the practice phase of the experiment. However, the non-interrupt trials were randomly intermixed with interrupt trials during the experimental phase. Therefore, if participants perform equivalently on the practice trials and the non-interrupt trials one may assume that the interruption (i.e., the side decision task) did not affect how participants performed the handedness decision task.

The average RT for each orientation was calculated for both the practice and non-interrupt trials. It is commonly found that data on either side of the 180° are symmetric (Shepard & Cooper, 1982). To accommodate this symmetry, I collapsed the data on either side of 180°. For example, for the practice trials I combined data from 150° with data from 210°, 120° was combined with 240°, 90° was combined with 270°, 60° was combined with 300°, and 30° was combined with 330°. Data from the same trials and data from the different trials were analyzed separately. Figure 16a shows the average RT of the same data from the practice trials plotted as a function of angular disparity, and Figure 16b shows the average RT of the different data from the practice trials plotted as a function of angular disparity.



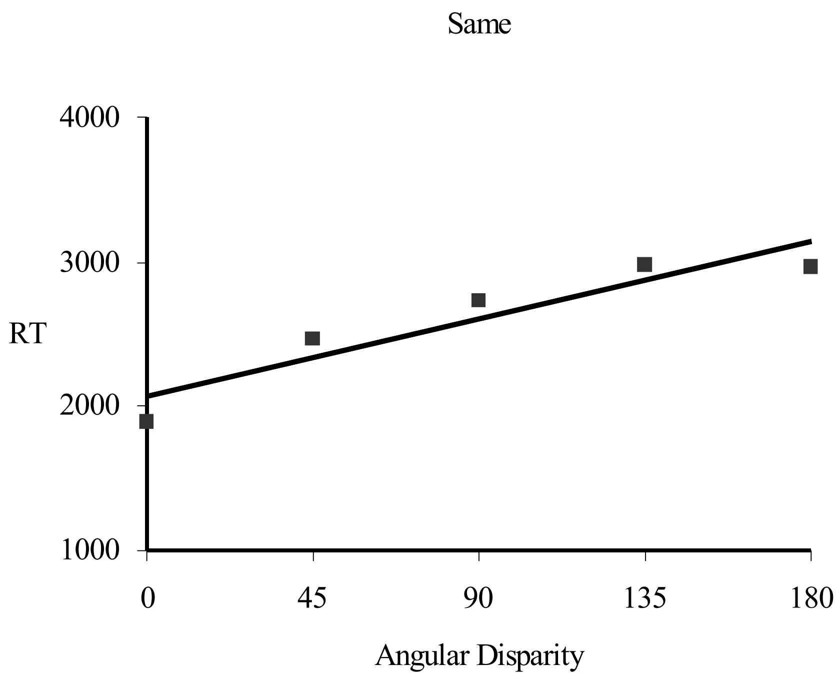
a.



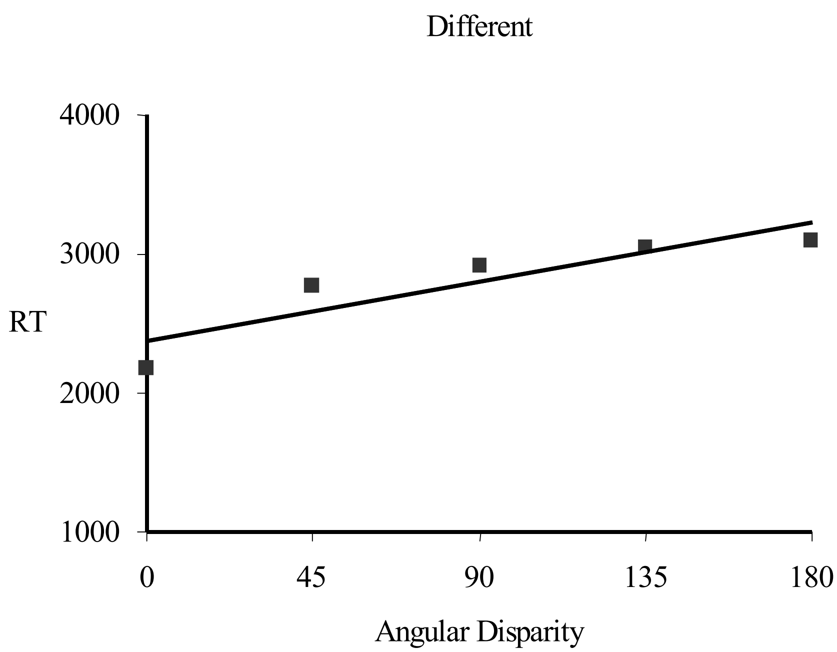
b.

Figure 16. a. Average RT for same practice trials plotted as a function of angular disparity.
b. Average RT for different practice trials plotted as a function of angular disparity.

Figure 17a shows the average RT of the same data from the non-interrupt trials plotted as a function of angular disparity, and Figure 17b shows the average RT of the different data from the non-interrupt trials plotted as a function of angular disparity.



a.



b.

Figure 17. a. Average RT for same non-interrupt trials plotted as a function of angular disparity.
b. Average RT for different non-interrupt trials plotted as a function of angular disparity.

For the same practice trials, there was a significant relation between angular disparity and RT, RT = 2475.95 + 5.25*(angular disparity), $\underline{F}$ (1, 5) = 15.09, $\underline{p}$ = .011, r^2 = .75. Both the intercept ($\underline{t}$ = 16.92) and the slope ($\underline{t}$ = 3.88) were significant. For the same non-interrupt trials there was also a significant relation between angular disparity and RT, RT = 2070.51 + 5.93*(angular disparity), $\underline{F}$ (1, 3) = 18.99, $\underline{p}$ = .022, r^2 = .86. Once again, both the intercept ($\underline{t}$ = 13.81) and the slope ($\underline{t}$ = 4.38) were significant. The slopes obtained in the practice and non-interrupt trials were greater than the previously discussed LRC indicating that participants did use "mental rotation" to complete the task. Importantly, there were no significant differences between the intercepts of the practice and the non-interrupt trials, $\underline{t}$ (8) = 1.93, $\underline{p}$ > .05, or the slopes of the practice and the non-interrupt trials, $\underline{t}$ (8) = .35, $\underline{p}$ > .05. These findings indicate that inserting the interrupt task did not affect how participants performed the handedness decision task.

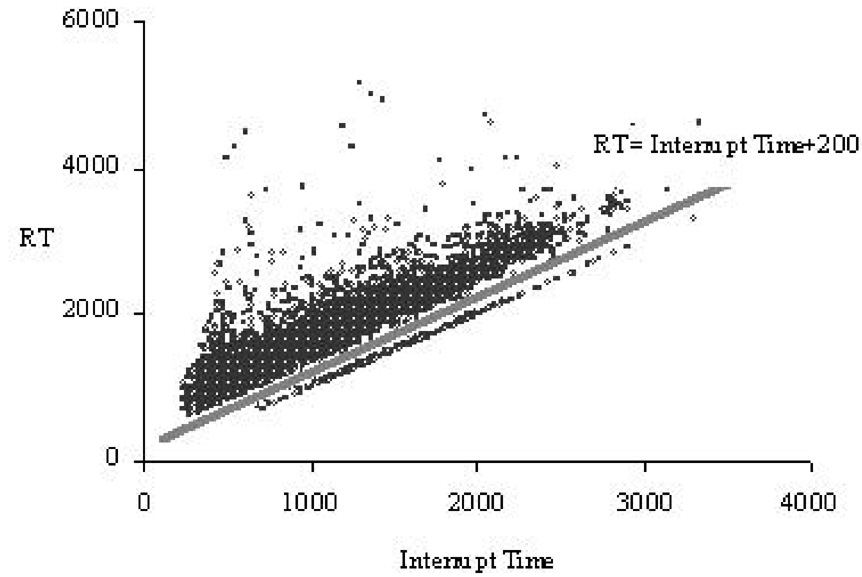
For the different practice trials, there was a significant relation between angular disparity and RT, RT = 2889.63 + 4.41*(angular disparity), $\underline{F}$ (1,5) = 17.49, $\underline{p}$ = .009, r^2 = .78. Both the intercept ($\underline{t}$ = 25.32) and the slope ($\underline{t}$ = 4.18) were significant. For the different non-interrupt trials there was also a significant relation between angular disparity and RT, RT = 2380.6 + 4.69*(angular disparity), $\underline{F}$ (1,3) = 13.03, $\underline{p}$ = .037, r^2 = .81. Both the intercept ($\underline{t}$ = 16.59) and the slope ($\underline{t}$ = 3.61) were significant. Once again, the slopes obtained in the practice and non-interrupt trials were greater than the previously discussed LRC indicating that participants did use "mental rotation" to complete the task. The intercept from practice trials was significantly greater than the intercept of the non-interrupt trials, $\underline{t}$ (8) = 2.77, $\underline{p}$ > .05. However, there was no significant difference between the slopes of the practice and the non-interrupt trials,

$\underline{t}$ (8) = .35, $\underline{p} < .05$. Recall that Shepard and his colleagues (Shepard & Cooper, 1973; Shepard & Cooper, 1982; Shepard & Metzler, 1971) posit that the "mental rotation" process accounts for the majority of the orientation effects. Therefore, the slope of the function relating RT and angular disparity specifies the "mental rotation" process. The intercept relates information about the other three stages. So, the lack of a difference between the slopes of the practice and non-interrupt trials indicates that inserting the side decision task did not change the "mental rotation" process. The difference between the intercepts is easily explained by the fact that participants were more practiced at the handedness decision task when they completed the non-interrupt trials.

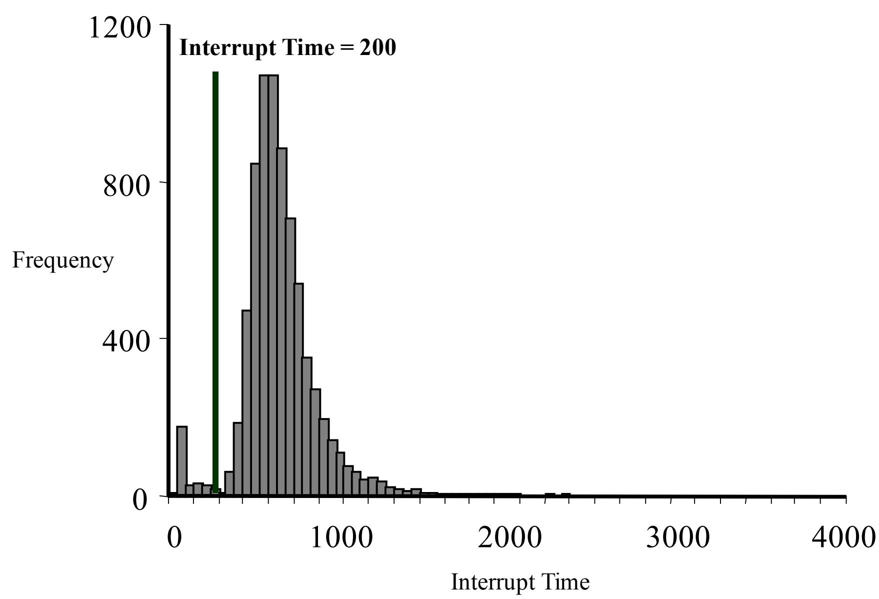
Determining the Responses to Side Decision Task

In the dualtask paradigm, participants were instructed to respond to the handedness decision task as quickly as possible, unless they were interrupted with the side decision task. Inevitably, there were some trials in which participants initiated a response to the handedness decision task and were *then* interrupted with the side decision task. On these trials participants completed their responses after the interruption, however, these were actually responses to the handedness decision task. Recall that there were two types of interrupt trials: facilitation and inhibition. If a participant initiated a response to the handedness decision task and was then interrupted with a facilitation trial, their response on that trial would be exceedingly fast and correct. This pattern of responding would be observed in a bimodal distribution of correct responses that occur after the interruption. There would be a group of very fast responses (i.e., responses to the handedness decision task that were initiated before the interruption), and a positively skewed distribution of responses to the side decision task. It is reasonable to predict that the break between these two distributions would occur approximately 200 ms after the interruption. This predication stems from simple reaction time experiments in which it takes participants approximately 200 ms to initiate and complete a response (Luce, 1986). If a participant initiated a response to the handedness decision task and was then interrupted with an inhibition trial, the participant would press the incorrect key for the side decision task. Thus, the computer would record that trial as an incorrect inhibition trial. This pattern of responding would result in a large number of errors on inhibition trials occurring within 200 ms of the interruption.

Consistent with the above predictions, the correct interrupt data is bimodal (i.e., composed of two separate distributions). When RT for the correct interrupt trials is plotted as a function of interrupt time there is a well-defined gap in the data that falls along the line in which RT equals the interrupt time plus 200 ms (see Figure 18a). In other words, there were many responses when the interrupt time was less than 200 ms and many responses when the interrupt time was greater than 200 ms. However, there were comparatively few responses when the interrupt time was approximately equal to 200 ms. This pattern can also be observed in the histogram of responses for the interrupt trials (see Figure 18b).



a.

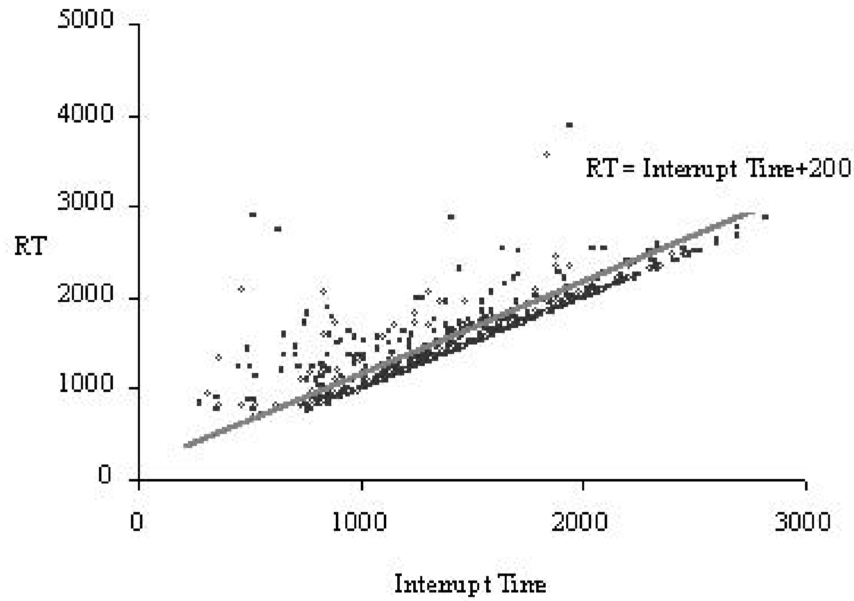


b.

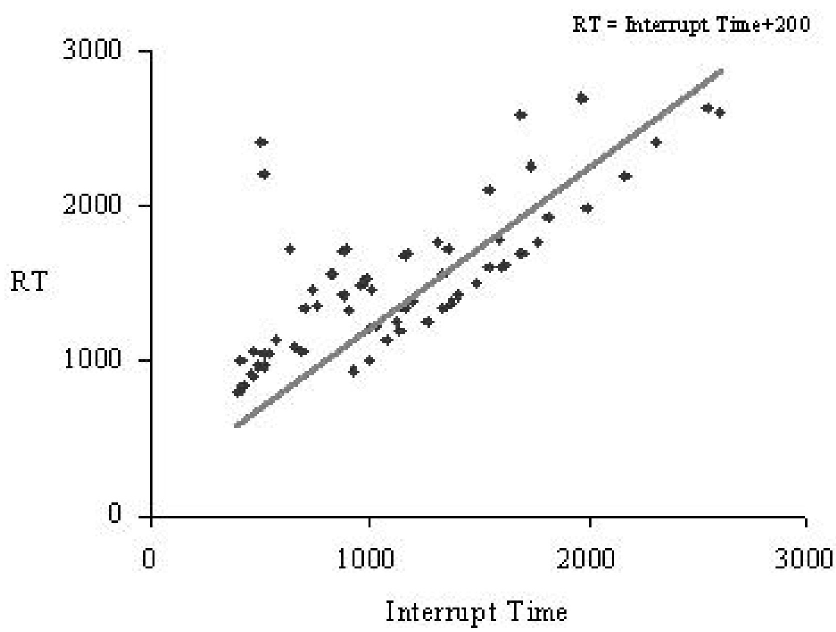
Figure 18. a. RT plotted as a function of interrupt time.
b. Histogram of responses to interrupt trials.

This gap suggests that responses occurring within 200 ms of the interrupt time are a result of a different cognitive process than the responses occurring after 200 ms of the interrupt time. The most parsimonious explanation is that responses occurring within 200 ms of the interrupt time represent a distribution of responses to the handedness decision task that were initiated before the interruption, and responses occurring after 200 ms of the interrupt time represent the distribution of responses to the side decision task. If this were the case, one would expect that a majority of the responses to inhibition trials occurring within 200 ms of the interrupt time to be incorrect, and a majority of the responses to facilitation trials occurring within 200 ms interrupt time to be correct.

When RT for incorrect inhibition trials is plotted as a function of interrupt time, most of the errors occurred below the line upon which RT is equal to the interrupt time plus 200 ms (see Figure 19a). However, when RT for incorrect facilitation trials is plotted as a function of interrupt time, there are an almost equal number of errors on either side of this line (see Figure 19b). Furthermore, of the responses occurring within 200 ms of the interruption, 94% of the inhibition trials are incorrect, while only 9% of the facilitation trials are incorrect. These data provide further support that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task and responses occurring after 200 ms of the interrupt time were responses to the side decision task.



a.



b.

Figure 19. a. Incorrect inhibition trials plotted as a function of interrupt time
b. Incorrect facilitation trials plotted as a function of interrupt time

Finally, I computed a maximum-likelihood analysis to confirm that responses occurring within 200 ms of the interruption were responses to the handedness decision task.[1] The model was based on the following assumptions: (1) When participants are interrupted with the side decision task after they have initiated a response to the handedness decision task, they continue the original response, (2) The time from initiation to completion of the response is approximately 200 ms, (3) Only the fastest responses to the handedness task are interrupted after initiation, and (4) the RT data follow a Gamma distribution. The maximum likelihood model assumes that the RT data follow a Gamma distribution because the RT data set is positively skewed. The Gamma distribution has often been used to model RT data because (1) it is positively skewed, and (2) the degree of skew is specified by a single parameter (for a review, see Luce, 1986).

The maximum-likelihood model assumes that responses occurring within 200 ms of the interruption were actually the fastest responses to the handedness decision task (i.e., responses initiated before the interruption). If this is the case, these trials represent a truncated distribution of responses to the handedness decision task. Figure 20a is a pictorial description of the entire distribution. The gray portion of the distribution from Figure 20b represents the responses occurring within 200 ms of the interruption.

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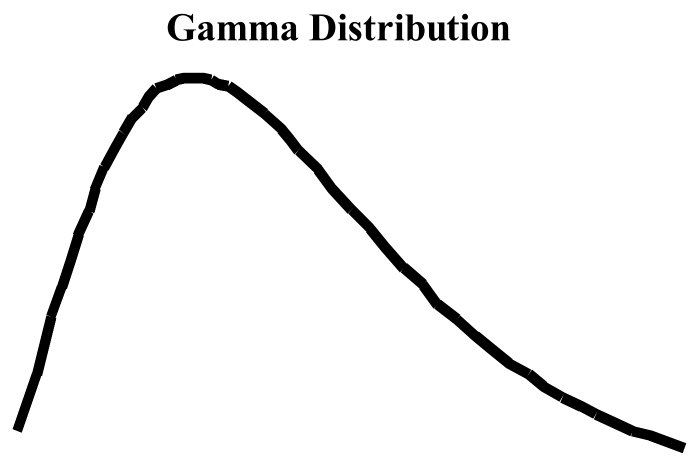
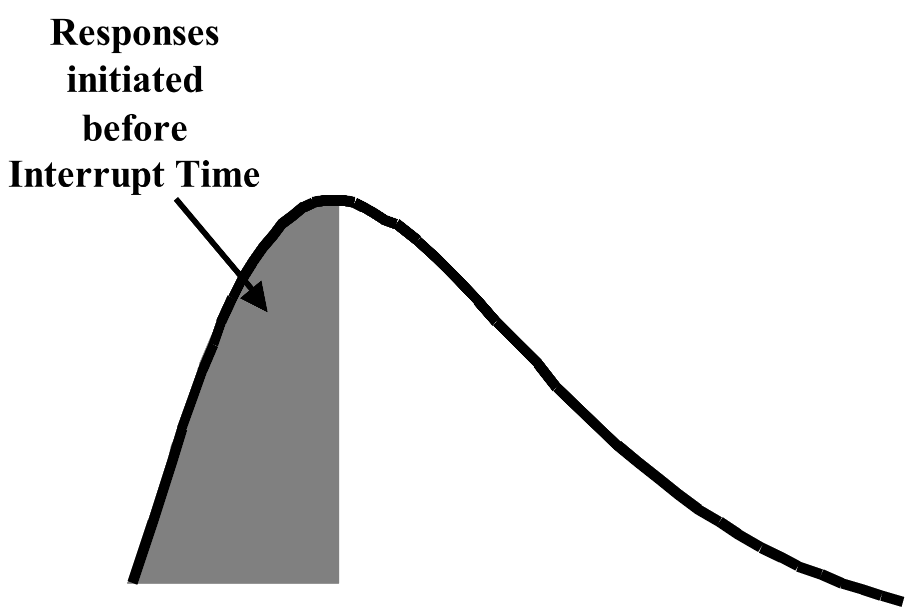
[1] This maximum-likelihood model was computed by Jon Cohen, PhD. and Dale Cohen, PhD.



a.



b.

Figure 20. a. Example of a gamma distribution.
b. Gray portion represents truncated distribution of responses to handedness decision task.

The maximum likelihood analysis uses this truncated part of the distribution to estimate the rest of that distribution. If the responses occurring within 200 ms of the interrupt time were responses to the handedness decision task then the estimated distribution should not be different from the distribution of responses to the non-interrupt trials.

I ran regression analyses on the means for the same and different data at each orientation estimated by the maximum-likelihood model and compared them to the non-interrupt data. For the same predicted distribution, there was a significant relation between angular disparity and RT, RT = 2103.27 + 6.12*(angular disparity), $\underline{F}$ (1, 3) = 13.2, $\underline{p}$ = .03591, r^2 = .81. Both the intercept ($\underline{t}$ = 11.32) and the slope ($\underline{t}$ = 1.68) were significant. Importantly, there was no significant difference between the intercepts of the same predicted distribution and the same non-interrupt data, $\underline{t}$ (8) = .134, $\underline{p}$ > .05, or the slopes of the same predicted distribution and the non-interrupt data, $\underline{t}$ (8) = .089, $\underline{p}$ > .05. For the different predicted distribution, there was a significant relation between angular disparity and RT, RT = 2379.7 + 5.6*(angular disparity), $\underline{F}$ (1, 3) = 11.93, $\underline{p}$ = .041, r^2 = .80. Both the intercept ($\underline{t}$ = 13.3) and the slope ($\underline{t}$ = 3.45) were significant. Once again, there was no significant difference between the intercepts of the different predicted distribution and the different non-interrupt data, $\underline{t}$ (8) = .004, $\underline{p}$ > .05, or the slopes of the different predicted distribution and the non-interrupt data, $\underline{t}$ (8) = .46, $\underline{p}$ > .05.

The estimated distribution and the non-interrupt data are plotted in Figures 21a (same data) and Figure 21b (different data).

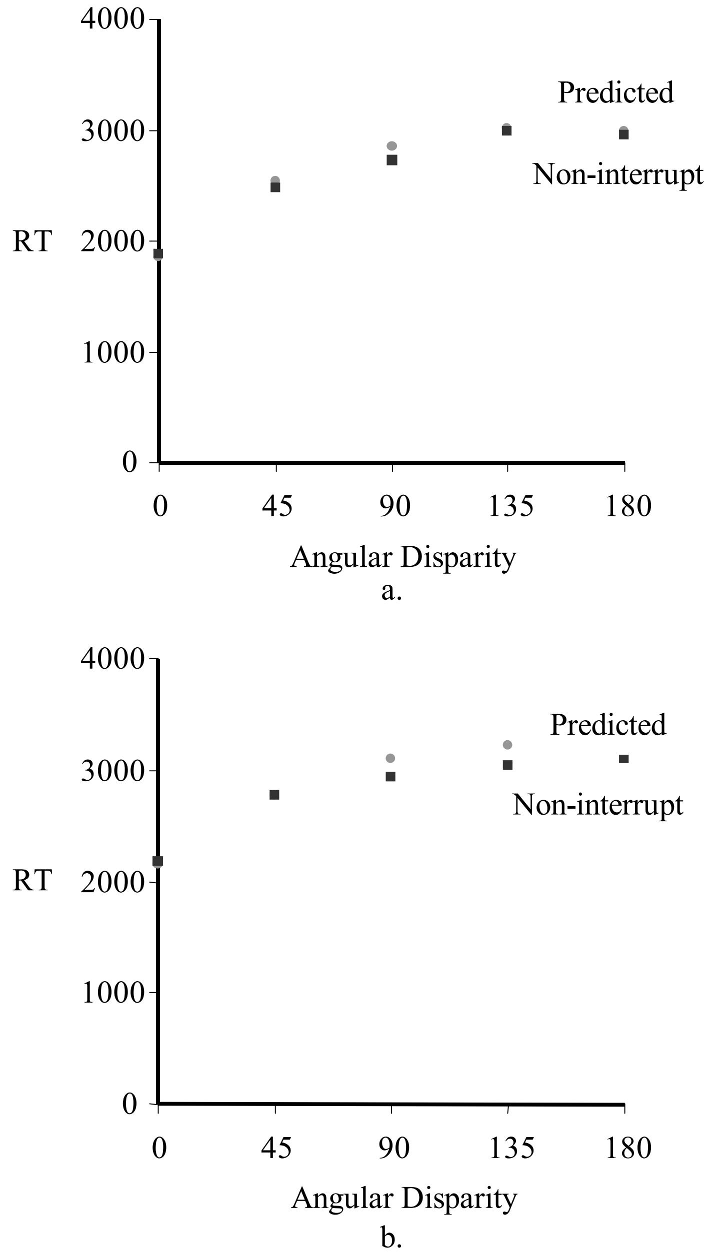


Figure 21. a. Predicted and non-interrupt data from the same trials plotted as a function of angular disparity.
b. Predicted and non-interrupt data from the different trials plotted as a function of angular disparity.

The estimated and non-interrupt data fall very closely to each other and being that there are no differences between the two distributions, I assumed responses that occurred within 200 ms of the interruption were responses to the handedness decision task. Therefore, these trials were excluded from the investigation of facilitation and inhibition effects.

Assessing Facilitation and Inhibition Effects

The final step in the data analysis procedure was to assess the responses to the side decision task for any facilitation and inhibition effects. The two dependent variables used in these analyses were RT and percent correct. Figure 22a shows a histogram of the distribution of RTs to the side decision task. As with most RT data sets, this distribution is positively skewed. To normalize this data, I calculated the reciprocal for each of these values and subtracted that score from one. This normalized the distribution and maintained the ordinal relationships between the scores. The distribution of transformed data is presented in Figure 22b. To minimize between subject variability, each participants' transformed RTs were standardized.

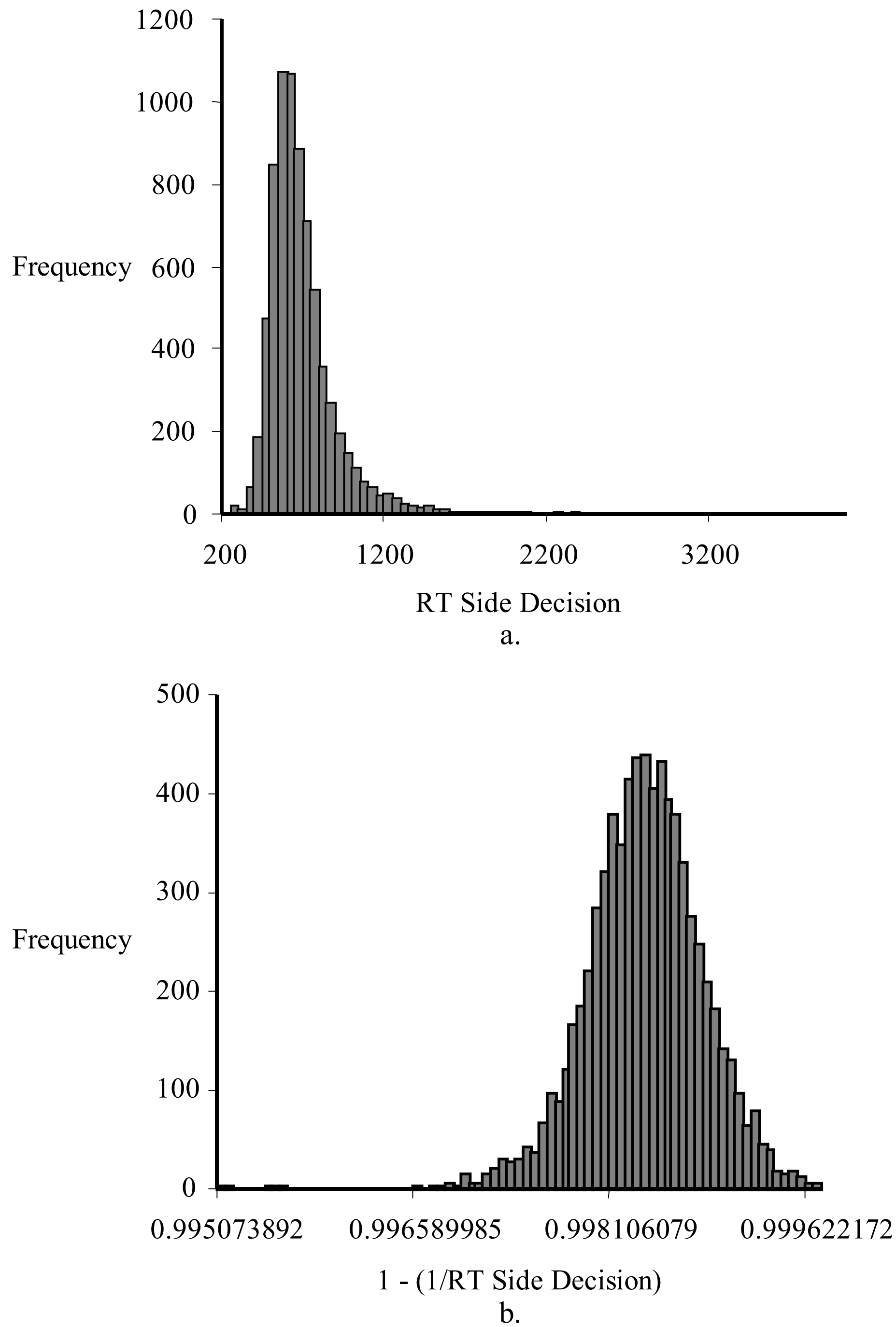


Figure 22. a. Histogram of responses to side decision task.
b. Histogram of transformed distribution of responses to side decision task.

I calculated the average standardized RT and percent error for each interrupt proportion for both facilitation and inhibition trials. These average RTs and percent errors were calculated separately for same and different data. Figure 23a shows the standardized RTs with standard errors for the same interrupt trials plotted as a function of interrupt proportion. The dotted line represents inhibition data and the solid line represents facilitation data. This pattern follows the model that was discussed in the *General Methods* section. Specifically, the average RTs on the inhibition trials are longer than the average RTs on the facilitation trials and this discrepancy increases as interrupt proportion increases. There is a clear facilitation effect in that average RTs decrease as angular disparity increases. However, there is not an inhibition effect. A similar pattern of data is observed in the percent errors for the same interrupt trials (see Figure 23b). Once again, the dotted line represents the inhibition data and the solid line represents the facilitation data. There are more errors on the inhibition trials than the facilitation trials and the discrepancy increases as interrupt proportion increases. There are no facilitation or inhibition effects present in the RT data for the different interrupt trials (see Figure 23c). However, the expected pattern of data is observed in the percent errors for the different interrupt trials (see Figure 23d).

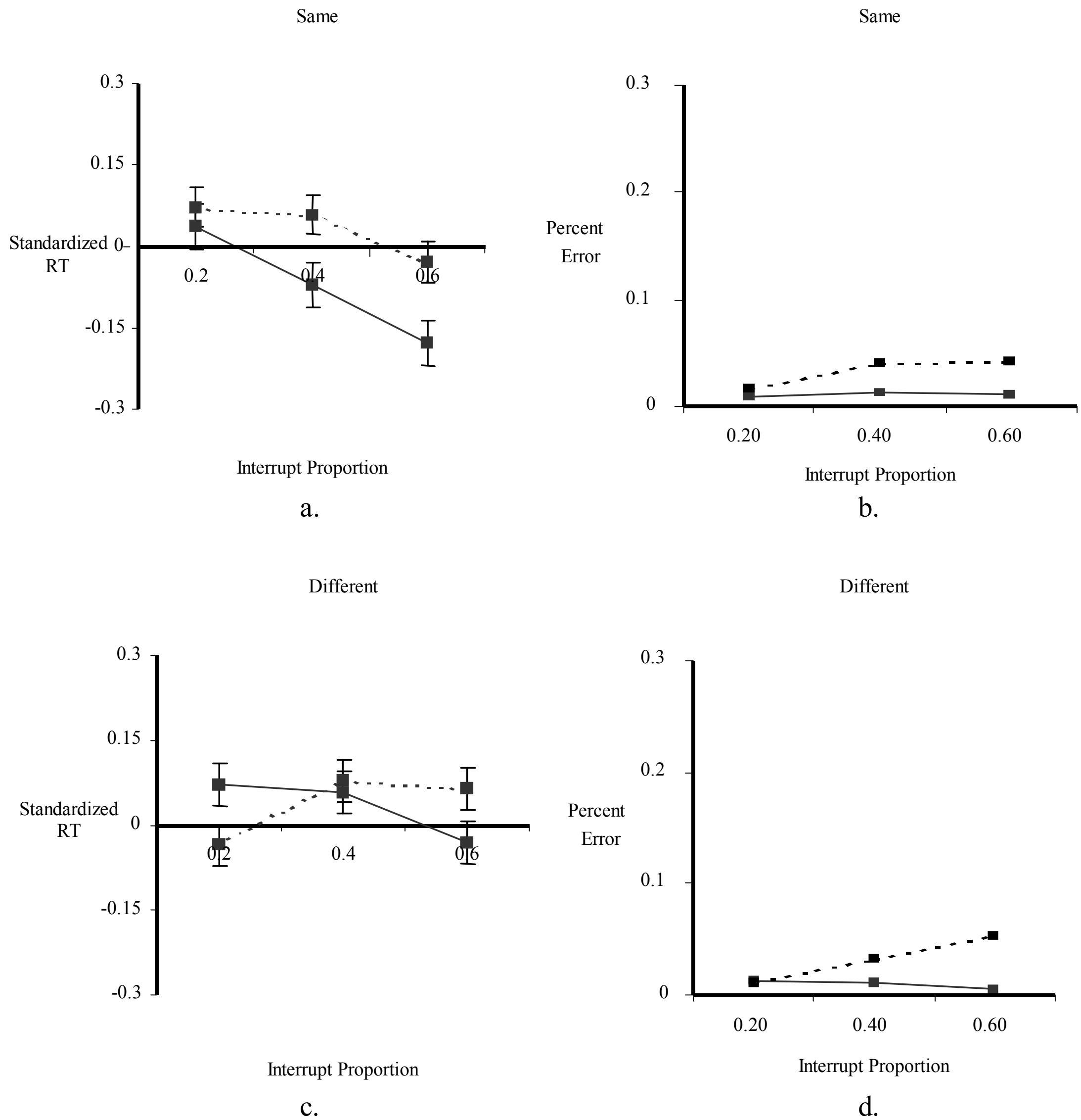


Figure 23. a. Same Standardized RTs as a function of interrupt proportion.
b. Same Percent Error as a function of interrupt proportion.
c. Different Standardized RTs as a function of interrupt proportion.
d. Different Percent Error as a function of interrupt proportion.

As noted previously, if participants must use “mental rotation” to complete a handedness decision task, they will know the handedness of stimuli with smaller angular disparities faster than that of stimuli with larger angular disparities. Thus, participants will know the handedness before being interrupted with the side decision on a greater proportion of trials with smaller angular disparities than with larger angular disparities. Therefore, it is predicted that facilitation and inhibition effects will be larger for smaller angular disparities than that of larger angular disparities.

Figures 24 and 25 show average RTs with standard errors and percent error for the same data as a function of interrupt proportion separated by angular disparity. For the RT data, there is a large facilitation effect when the angular disparity is 0°. As predicted, this effect decreases as angular disparity increases. In fact, there are virtually no facilitation effects at 135° and 180°. There are no inhibition effects at any of the angular disparities. For the percent error data, there is a large effect of inhibition at 0°, which decreases as angular disparity increases. There are no facilitation effects at any of the angular disparities.

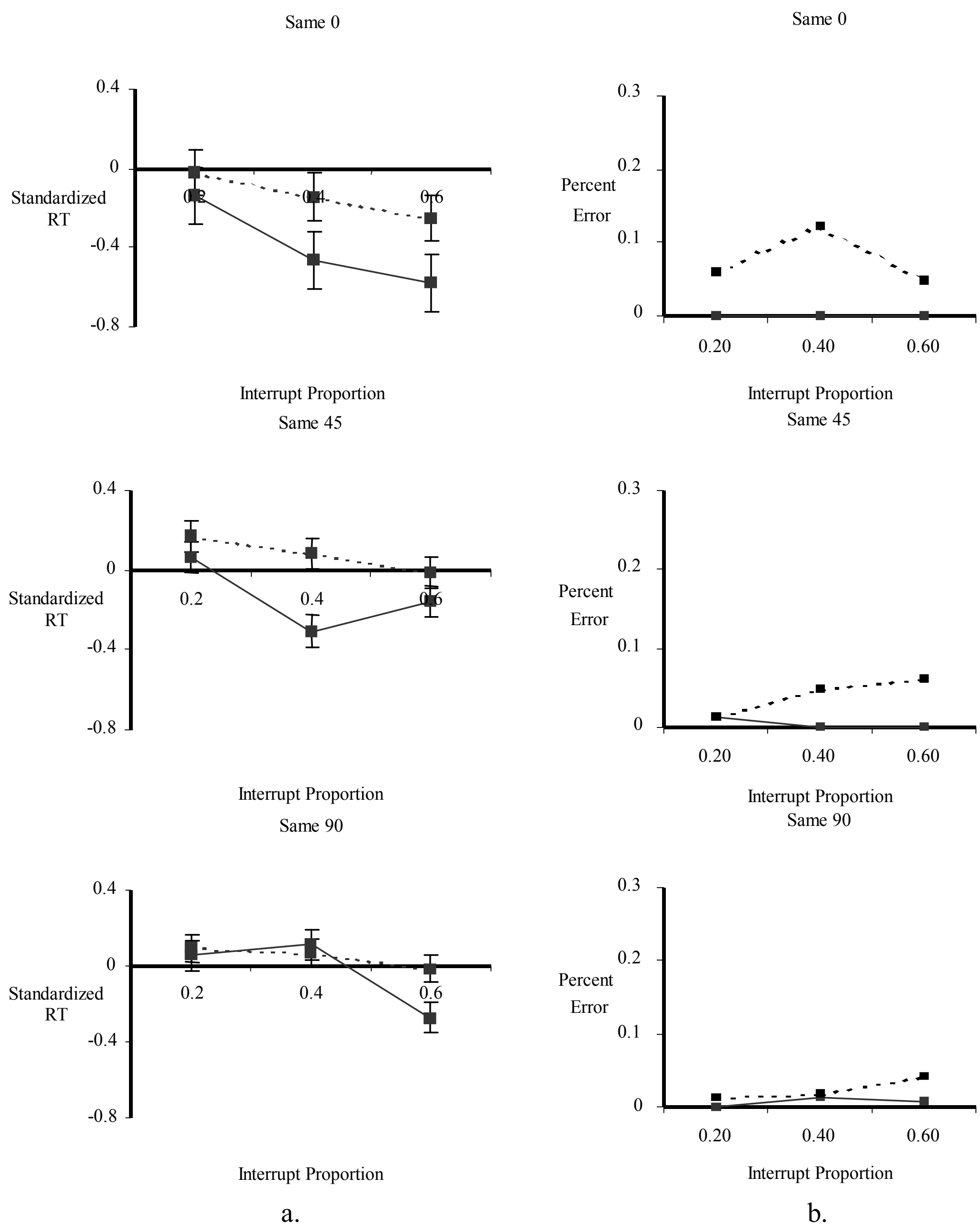


Figure 24. a. Same Standardized RTs separated by angular disparity (0° to 90°).
b. Same Percent Error separated by angular disparity (0° to 90°).

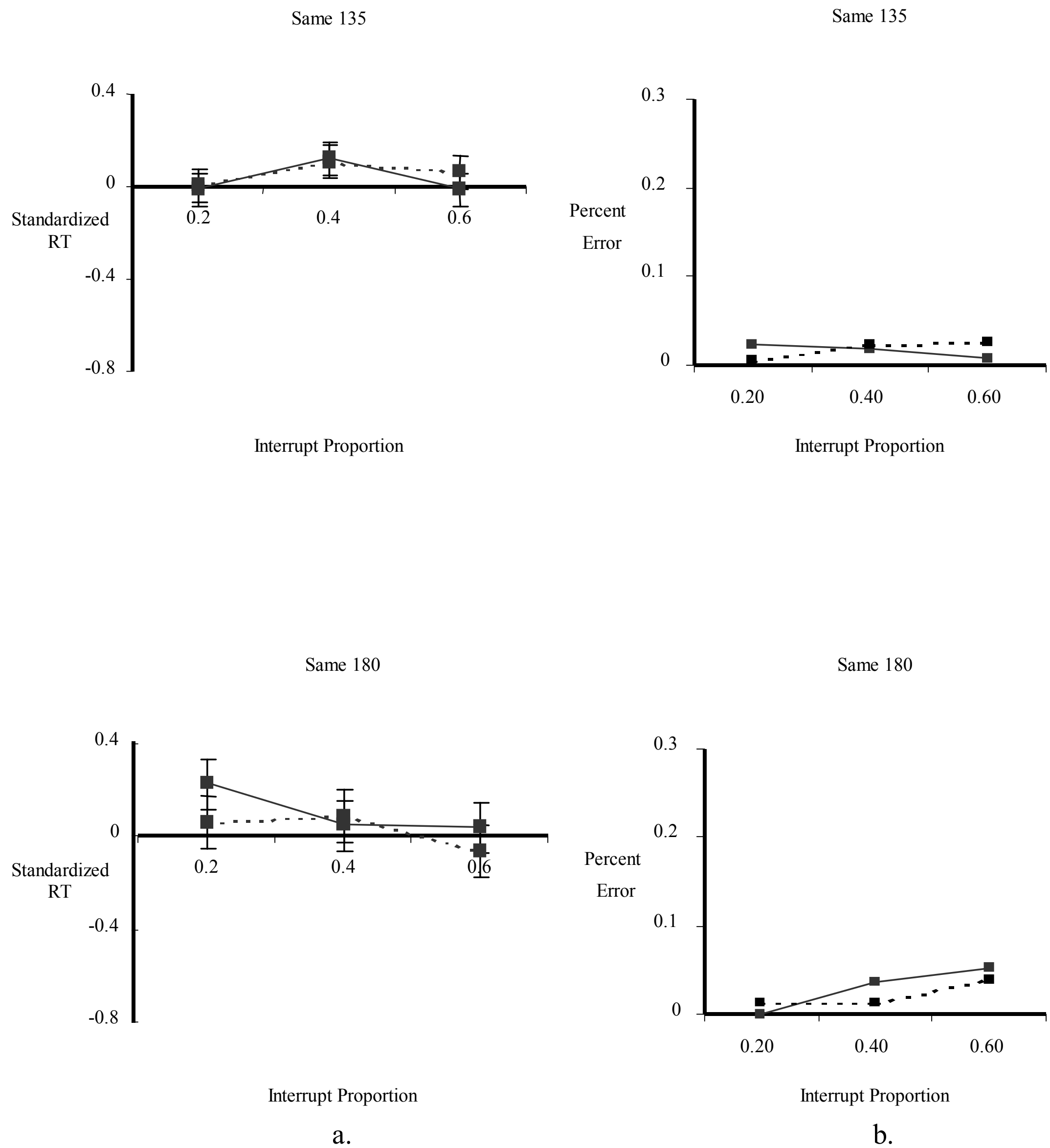


Figure 25. a. Same Standardized RTs (135° and 180°).
b. Same Percent Error (135° and 180°).

Figures 26 and 27 show average RTs with standard errors and percent error for the different data as a function of interrupt proportion separated by angular disparity. For the RT data there are very small effects of facilitation and inhibition at 0°. There are no facilitation or inhibition effects at any other of the angular disparities. For the percent error data, there is a large effect of inhibition at 0°, which decreases as angular disparity increases. There are no facilitation effects at any of the angular disparities. The average RTs and percent error from the same data suggest that participants must complete "mental rotation" before they know the handedness of misoriented stimuli. Although the different RT data does not show an effect of angular disparity, the effect is present in the error data. This suggests a possible speed accuracy tradeoff, which also indicates that participants did not know the handedness of the misoriented stimuli before using "mental rotation."

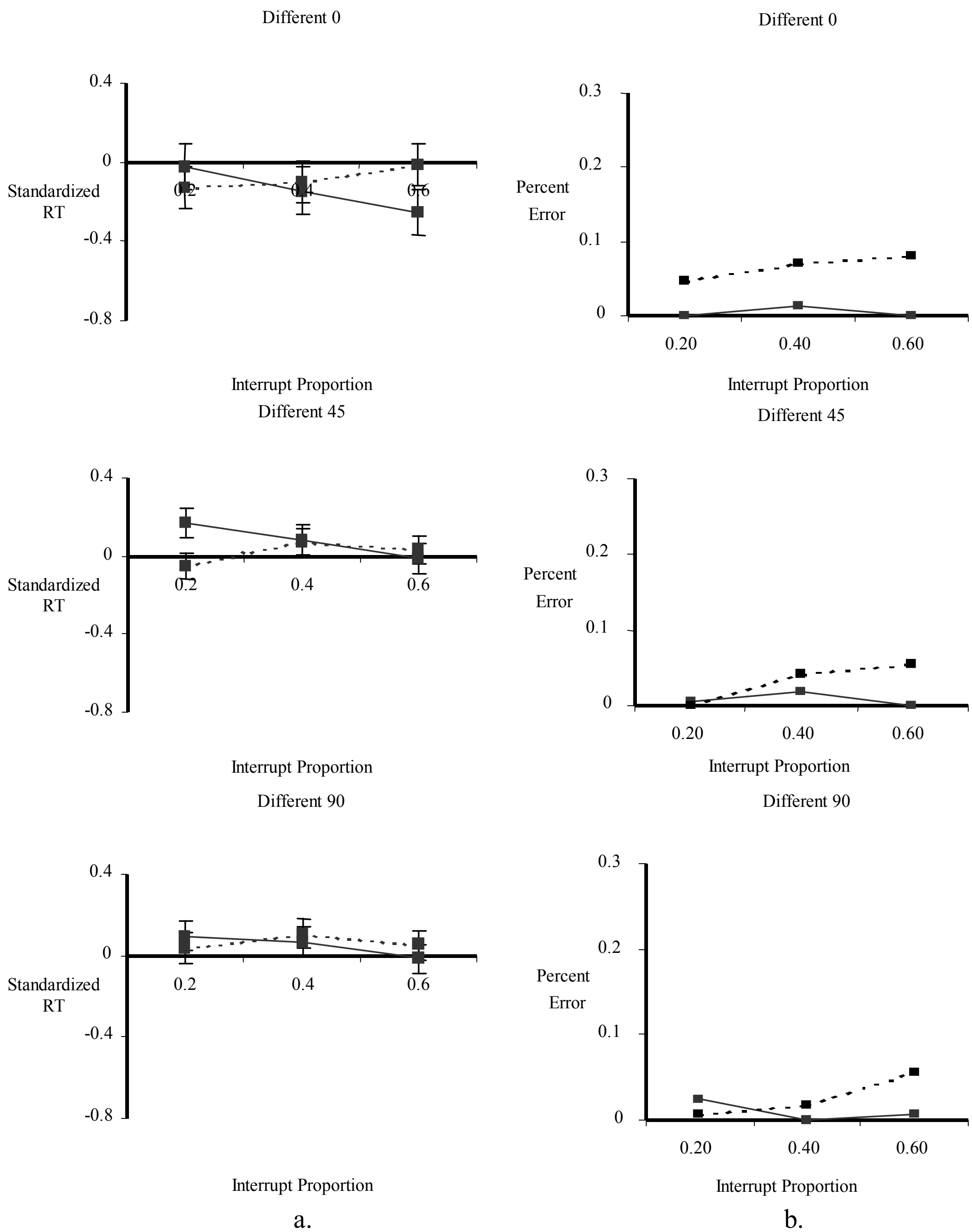
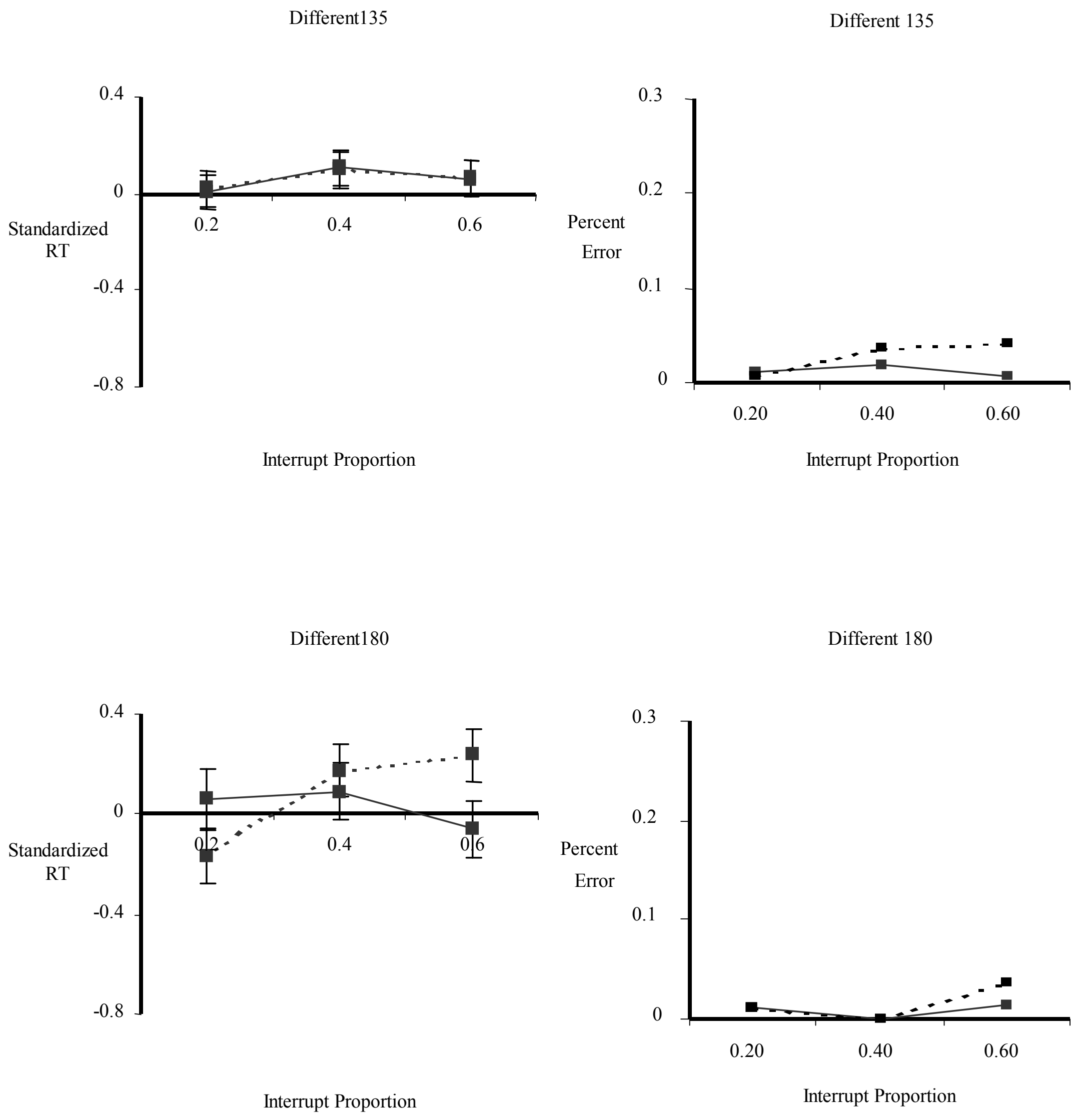


Figure 26. a. Different Standardized RTs separated by angular disparity (0° to 90°).
b. Different Percent Error separated by angular disparity (0° to 90°).



a. b.

Figure 27. a. Different Standardized RTs (135° and 180°).
b. Different Percent Error (135° and 180°).

I next calculated Cohen's *d* effect sizes for the facilitation and inhibition effects for both the RT data and the percent error data. Once again, the same and different data were analyzed separately. I subtracted the average standardized facilitation RT from the average standardized inhibition RT for each interrupt proportion and each angular disparity. I then divided this value by the square root of the sum of the variances associated with each of these averages. This value provided a measure of the facilitation and inhibition effects for RT for each interrupt proportion at each angular disparity. Larger effect sizes indicate greater disparities between facilitation and inhibition trials. Therefore, if participants must use "mental rotation" to know the handedness of misoriented stimuli, one would expect larger effect sizes at smaller angular disparities. Effect sizes were also computed for the percent error data.

Figure 28a is a plot of the effect sizes for the same RT data plotted as a function of angular disparity and separated by interrupt proportion. A separate line represents each interrupt proportion (they are labeled and go from dark to light as interrupt proportion increases). The effect sizes increase as interrupt proportion increases and decrease as angular disparity increases. Once again, this pattern of data implies that participants must use "mental rotation" to know the handedness of misoriented stimuli. I next fit log functions to each of the lines in this graph. I had to change the angular disparity of 0° to 1° to fit the log functions because the log of zero is undefined. The intercept of these functions indicates the amount of facilitation and inhibition at 0°, and the slope indicates the relationship between effect size and angular disparity. Because effect size decreases as angular disparity increases, I expected the slopes to be negative. The log functions are plotted in Figure 28b. The log function for the same data with the .2 interrupt proportion is $d_{.2}$ = -0.0236*Ln(α) + 0.1054, r^2 = .38. The log function for .4 is

$d_{.4}$ = -0.034 *Ln(α) + 0.2158, r^2 = .28. The log function for .6 is

$d_{.6}$ = -0.0314*Ln(α) + 0.2059, r^2 = .46. These functions indicate an effect of both angular disparity and interrupt proportion.

Figure 28c is a plot of the effect sizes for the same error data plotted as a function of angular disparity and separated by interrupt proportion. Once again, a separate line represents each interrupt proportion, going from dark to light as interrupt proportion increases. This pattern is very similar to that of the RT effect size data, in that effect size increases as interrupt proportion increases and decreases as angular disparity increases. I next fit log functions to each of these lines, and these functions are plotted in Figure 28d. The log function for the same data with the .2 interrupt proportion is

$d_{.2}$ = -0.4129*Ln(α) + 2.1993, r^2 = .39. The log function for .4 is

$d_{.4}$ = -0.07414*Ln(α) + 3.9774, r^2 = .68. The log function for .6 is

$d_{.6}$ = -0.2459*Ln(α) + 2.5658, r^2 = .15. Once again, these functions indicate an effect of both angular disparity and interrupt proportion.

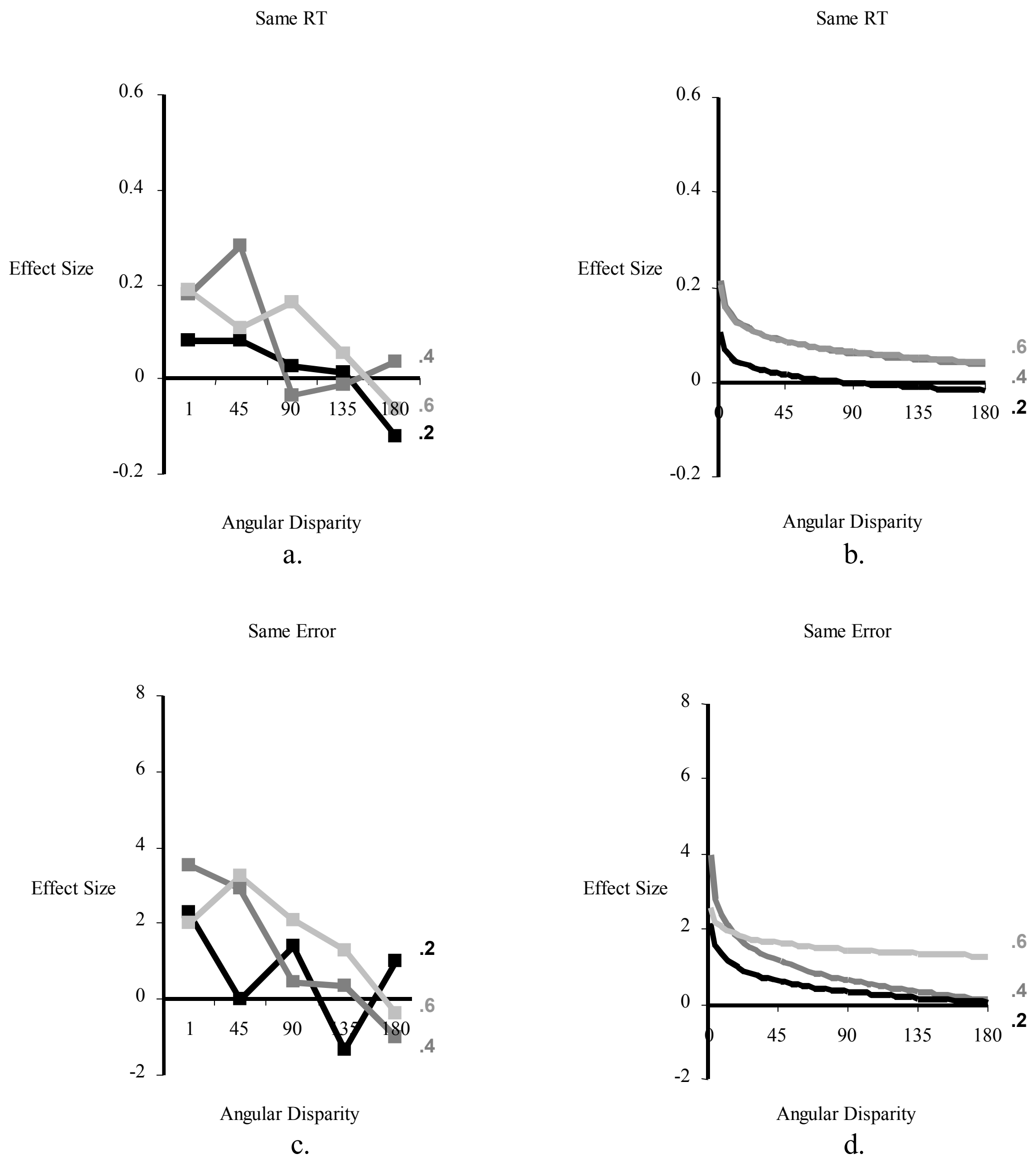


Figure 28. a. Effect Sizes for Same RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Same RTs.
c. Effect Sizes for Same Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Same Error.

Figure 29a is a plot of the effect sizes for the different RT data plotted as a function of angular disparity. The arrangement of this graph is the same as the same RT effect size graph (i.e., separated by interrupt proportion). There is an effect of interrupt proportion in that as interrupt proportion increases, so do effect sizes. However, there is no effect of angular disparity. Figure 29b is a plot of the log functions to these data. The log function for the different data with the .2 interrupt proportion is $d_{.2} = -0.0009*\text{Ln}(\alpha) -0.0825$, $r^2 = 0.0006$. The log function for .4 is $d_{.4} = -0.0005*\text{Ln}(\alpha) + 0.0206$, $r^2 = .0013$. The log function for .6 is $d_{.6} = -0.015*\text{Ln}(\alpha) + 0.145$, $r^2 = .14$. These functions indicate an effect of interrupt proportion but no effect of angular disparity.

Figure 29c is a plot of the effect sizes for the different error data plotted as a function of angular disparity and separated by interrupt proportion. There are effects of both interrupt proportion and angular disparity. Effect size increases as interrupt proportion increases and decreases as angular disparity increases. Figure 29b is a plot of the log functions to these data. The log function for the different data with the .2 interrupt proportion is $d_{.2} = -0.4129*\text{Ln}(\alpha) + 2.1993$, $r^2 = .39$. The log function for .4 is $d_{.4} = -0.7414*\text{Ln}(\alpha) + 3.9774$, $r^2 = .68$. The log function for .6 is $d_{.6} = -0.2459*\text{Ln}(\alpha) + 2.5658$, $r^2 = .15$. These functions indicate an effect of both angular disparity and interrupt proportion.

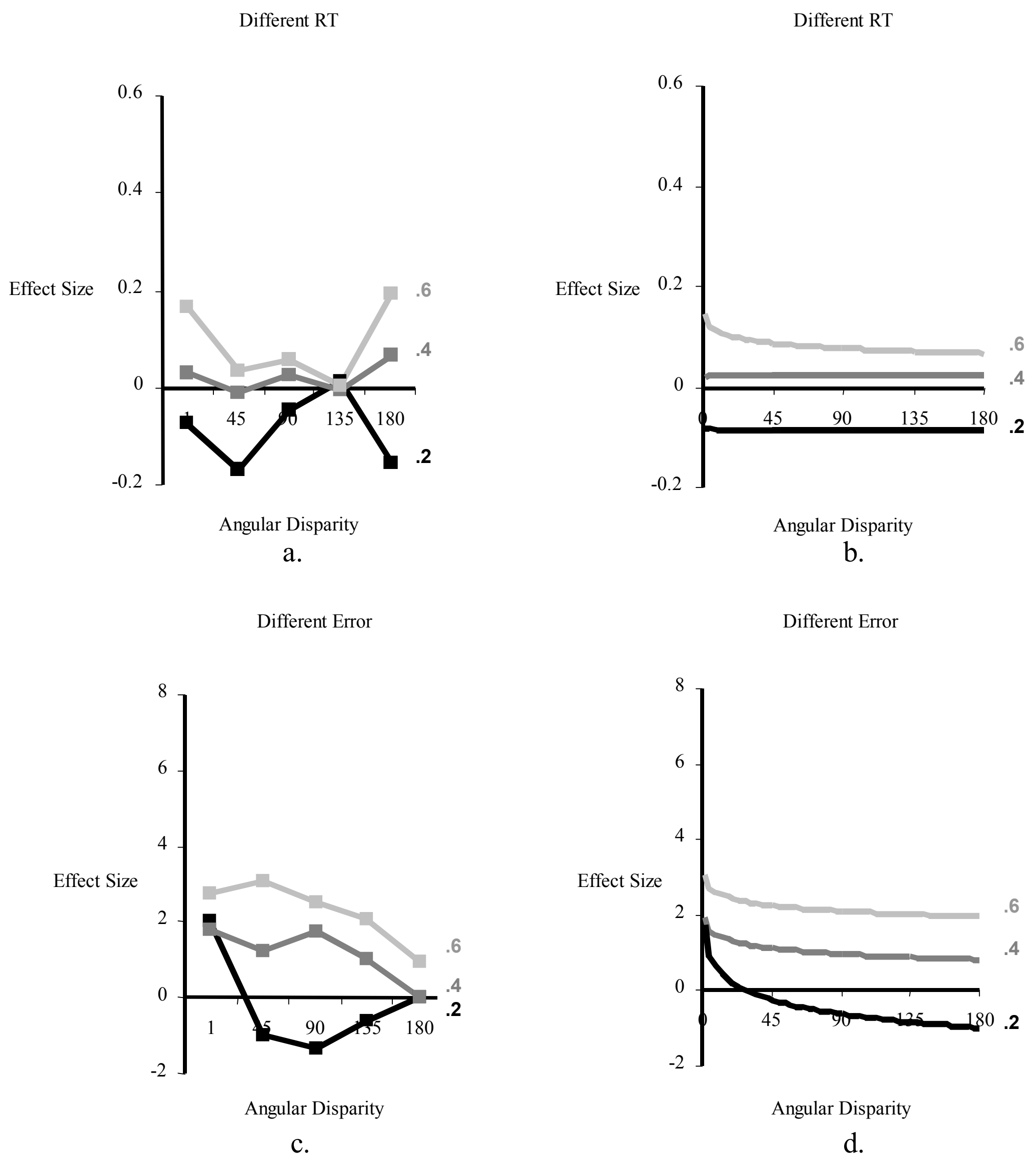
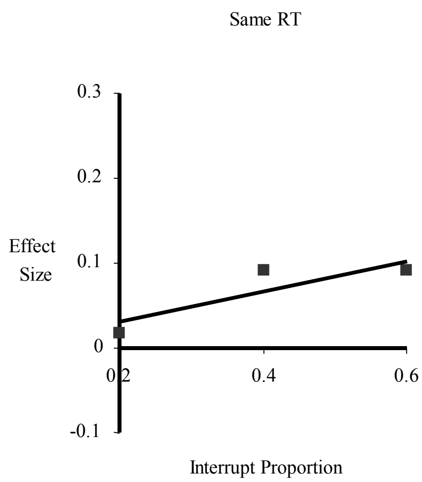
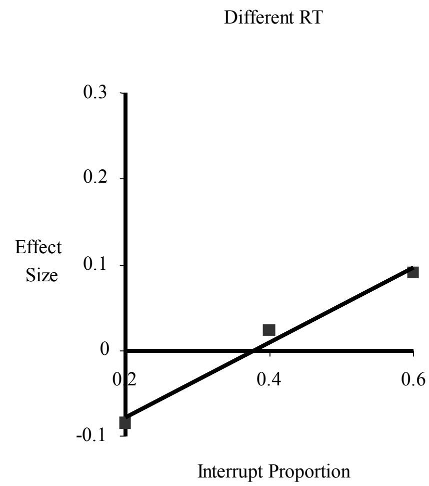


Figure 29. a. Effect Sizes for Different RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Different RTs.
c. Effect Sizes for Different Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Different Error.

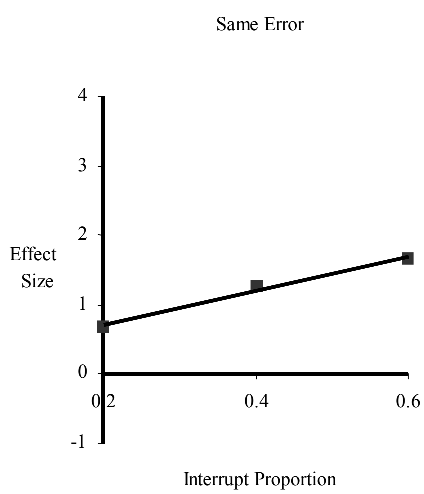
To clarify the relationship between effect sizes and interrupt proportion, I calculated effect sizes for both RT and percent error averaged across angular disparity for each interrupt proportion. These averages were calculated separately for same and different data. The effect of interrupt proportion is evidenced by the fact that average effect sizes increase as interrupt proportion increases (See Figure 30). I will not report statistics on these functions because there were only three interrupt proportions.



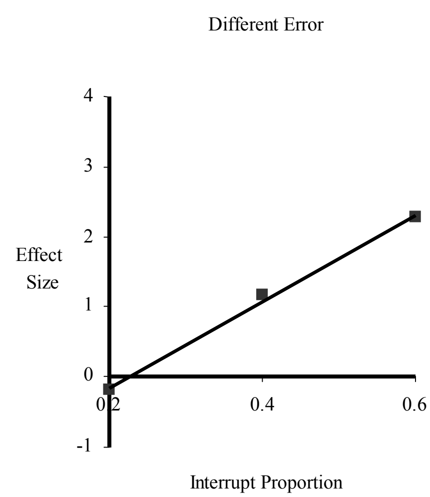
a.



b.



c.



d.

Figure 30. a. Average Effect Sizes for Same RTs plotted by interrupt proportion.
b. Average Effect Sizes for Different RTs plotted by interrupt proportion.
c. Average Effect Sizes for Same Errors plotted by interrupt proportion.
d. Average Effect Sizes for Different Errors plotted by interrupt proportion.

To clarify the relationship between effect sizes and angular disparity, I calculated effect sizes for both RT and percent error averaged across interrupt proportion for each angular disparity. Again, these averages were calculated separately for same and different data. For the same RT data, there was a significant relationship between angular disparity and effect size, effect size = 0.1728 - 0.00118*(angular disparity), F (1, 3) = 37.464, p = .008, r^2 = .926. Both the intercept (t = 8.1229) and the slope (t = 6.1208) were significant. For the different RT data, there was no relationship between angular disparity and effect size, effect size = 0.00164 $^+$ 0.00008*(angular disparity), F (1, 3) = 0.08, p = .794, r^2 = .026. Neither the intercept (t = 0.0527) nor the slope (t = 0.2842) was significant. For the same error data, there was a significant relationship between angular disparity and effect size, effect size = 2.69 - 0.0166*(angular disparity), F (1, 3) = 99.205, p = .002, r^2 = .97. Both the intercept (t = 14.6427) and the slope (t = 9.96) were significant. For the different error data, there was a significant relationship between angular disparity and effect size, effect size = 1.9 - 0.00902*(angular disparity), F (1, 3) = 17.414, p = .025, r^2 = .853. Both the intercept (t = 7.9745) and the slope (t = 4.173) were significant.

The effects of angular disparity are evidenced by negative relationships between average effect size and angular disparity (See Figure 31). As mentioned previously there was an effect of angular disparity in all these data except for the different RT data. However, the effect of angular disparity is present in the different error data.

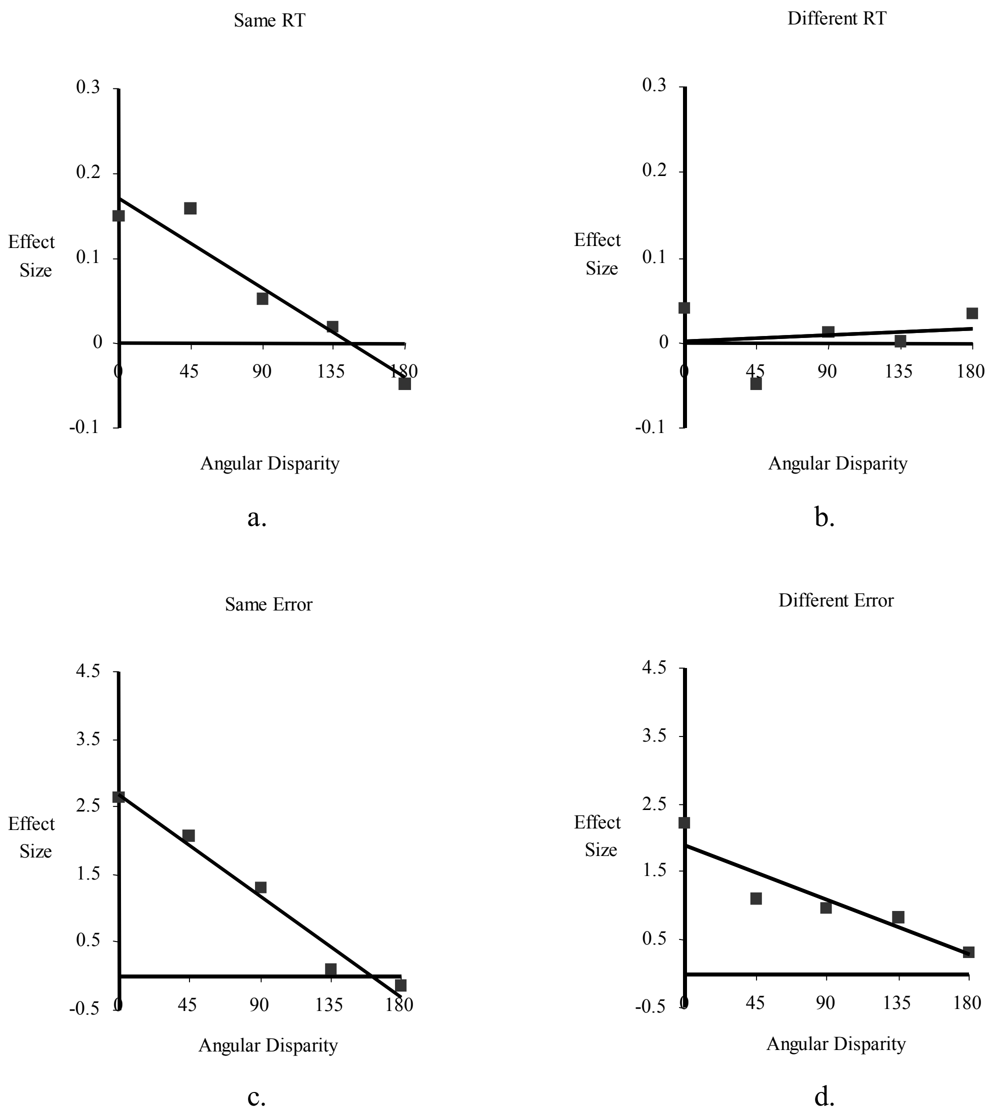


Figure 31. a. Average Effect Sizes for Same RTs plotted by angular disparity.
b. Average Effect Sizes for Different RTs plotted by angular disparity.
c. Average Effect Sizes for Same Errors plotted by angular disparity.
d. Average Effect Sizes for Different Errors plotted by angular disparity.

In summary, there were effects of both interrupt proportion and angular disparity for the same data. Both effects were present in the RT and the percent error analyses. For the different data, there was an effect of interrupt proportion and angular disparity in the error analysis. However, there was no effect of angular disparity in the RT analysis. As mentioned earlier, this is likely the result of a speed accuracy tradeoff. These results indicate that participants must use "mental rotation" to know the handedness of misoriented stimuli.

Discussion

Experiment 1 was designed to validate the dualtask procedure and analysis. Therefore, I used a task that required the use of "mental rotation." I presented participants with a different pair of stimuli on each trial because Cohen and Kubovy (1993) showed that orientation effects could not be eliminated when different stimuli are presented on every trial. Thus, I predicted both effects of interrupt proportion and angular disparity, which would indicate that participants did not know the handedness of the misoriented stimuli before using "mental rotation."

I first compared the data from the practice trials and the non-interrupt trials and found that the insertion of the side decision task did not change the way participants completed the handedness decision task. I next turned my attention to the interrupt trials. I concluded that responses occurring within 200 ms of the interrupt time were actually responses to the handedness decision task that had been initiated before the interrupt time. Therefore, I excluded these trials from the investigation of facilitation and inhibition effects. I concluded that there were effects of both interrupt proportion and angular disparity. Facilitation and inhibition effects increased as interrupt proportion increased, as predicted by the model discussed in the *General Methods* section. Also, the effects decreased as angular disparity increased, indicating that participants did not know the handedness of the misoriented stimuli before using “mental rotation.”

Experiment 1 showed that the dualtask procedure and analysis can detect effects of both interrupt proportion and angular disparity. Again, I predicted an effect of angular disparity because I presented participants with a different pair of stimuli on each trial. In Experiment 2, I tested whether or not participants must use “mental rotation” to know the handedness of misoriented stimuli when they are only presented with two pairs of stimuli throughout the experiment.

EXPERIMENT 2

Experiment 1 was a validation of the dualtask procedure. We presented participants with a different pair of stimuli on each trial and there was both an effect of interrupt proportion and angular disparity. As interrupt proportion increased, the facilitation and inhibition effects increased. As angular disparity increased, the facilitation and inhibition effects decreased. These effects were present in both the RT data and the error data. These findings indicate that participants did not know the handedness of the misoriented stimuli before using “mental rotation.” These results were expected because participants were presented with a different pair of stimuli on each trial.

Recall that Cohen and Kubovy (1993) found that when participants are presented with a different pair of stimuli on each trial, orientation effects cannot be eliminated. However, when participants are presented with only two sets of stimuli throughout the experiment and RT pressure is added to the handedness decision task, orientation effects are eliminated. Experiment 2 was designed to investigate if participants knew the handedness of the misoriented stimuli before using “mental rotation,” or if the RT pressure in Cohen and Kubovy and Cohen and Blair (1998) forced participants to use a different strategy to complete the handedness decision task. Thus, Experiment 2 is exactly like Experiment 1, except I presented participants with only two sets of stimuli throughout the entire experiment.

Method

Participants

Forty-seven undergraduate university students volunteered to participate in this experiment. Data from five of these participants were not used in the present analyses, because their error rates on the non-interrupt trials was greater than 25 percent, indicating that they did not understand the handedness decision task.

Stimuli and Apparatus

The stimuli and apparatus were the same as in Experiment 1 with the exception that only two standards were created for each participant. Therefore, participants were only presented with two sets of stimuli throughout the experiment. The presentation of these stimuli alternated from trial to trial, ensuring that participants did not “rotate” the probe to the same orientation as the probe from the previous trial. As in Experiment 1, the stimuli were fourteen-sided irregular polygons randomly created by the computer.

Design and Procedure

The experimental design and procedures were identical to those of Experiment 1.

Results

The data analysis procedures were identical to those of Experiment 1. Once again, there were three stages to the analysis procedure: (1) verify that inserting the side decision task did not affect how participants performed the handedness decision task, (2) determine which responses were to the side decision task, and (3) investigate facilitation and inhibition effects. Once again, all inferential analyses were conducted with an alpha level of .05. For all RT analyses, I only used trials in which the participant responded correctly. For the practice analysis, I excluded all trials that exceeded 10000 ms (3.31 SD from the mean, 1.49% of the trials). For the non-interrupt analysis, I also excluded all trials that exceeded 10000 ms (7.94 SD from the mean, 0.07% of the trials). For the side decision task analyses, I excluded trials with RTs exceeding 5000 ms (14.44 SD from the mean, 0.04 % of the trials).

Comparison of Practice and Non-Interrupt Trials

As in Experiment 1, the average RT for each orientation was calculated for both the practice and non-interrupt trials and collapsed around 180°. Once again, the same and different data were analyzed separately. Figure 32a shows the average RT of the same data from the practice trials plotted as a function of angular disparity, and Figure 32b shows the average RT of the different data from the practice trials plotted as a function of angular disparity. Figure 33a shows the average RT of the same data from the non-interrupt trials plotted as a function of angular disparity, and Figure 33b shows the average RT of the different data from the non-interrupt trials plotted as a function of angular disparity.

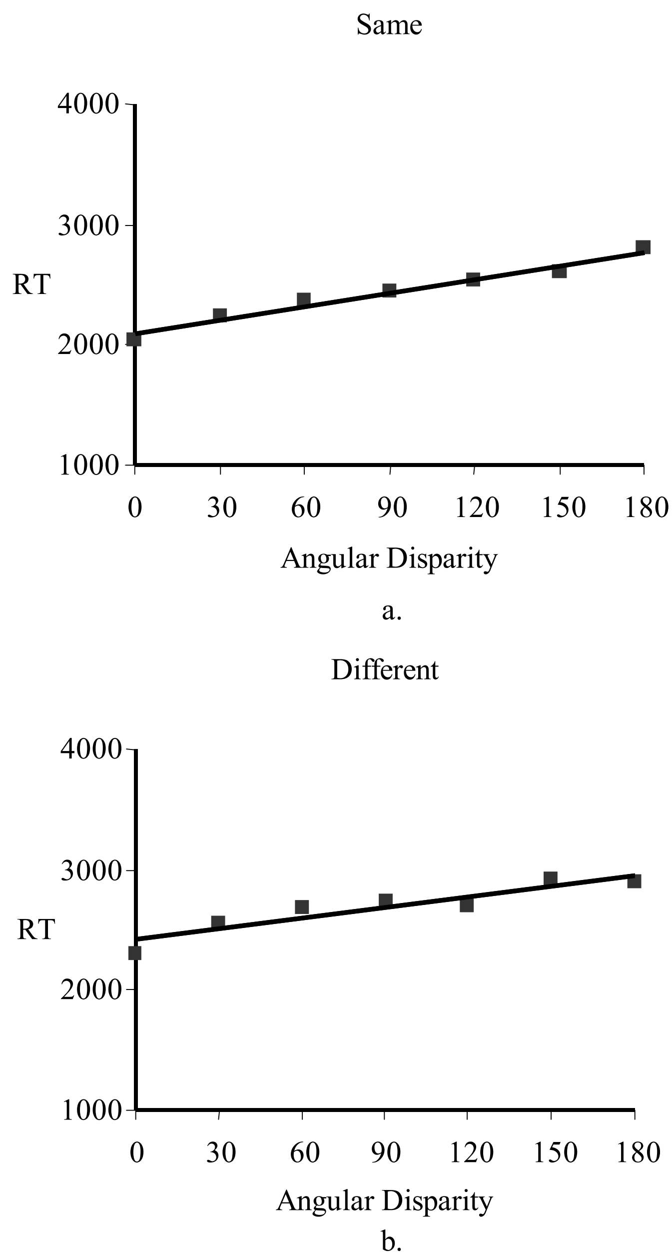
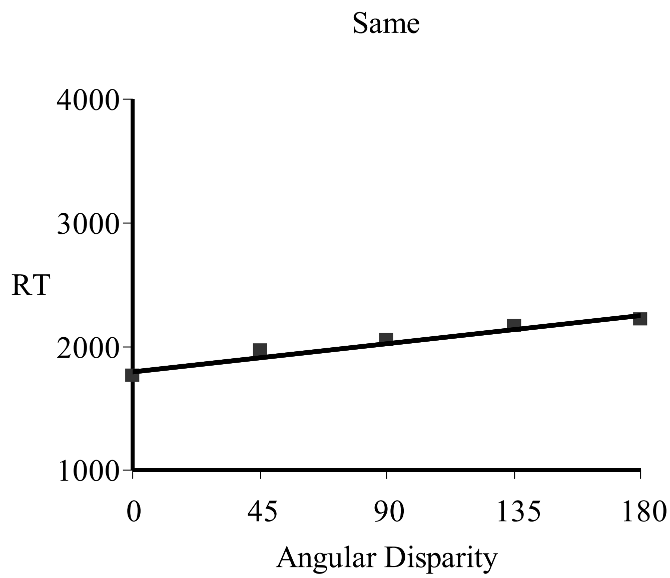
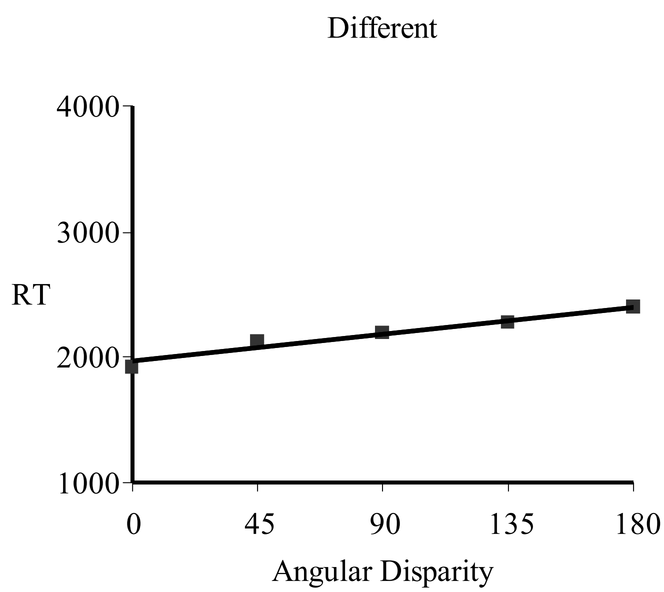


Figure 32. a. Average RT for same practice trials plotted as a function of angular disparity.
b. Average RT for different practice trials plotted as a function of angular disparity.



a.



b.

Figure 33. a. Average RT for same non-interrupt trials plotted as a function of angular disparity.
b. Average RT for different non-interrupt trials plotted as a function of angular disparity.

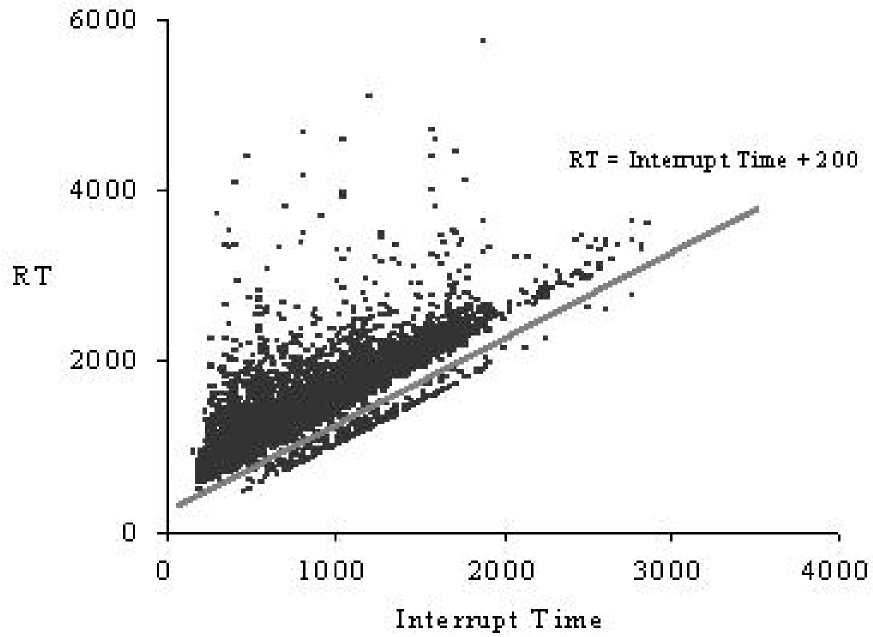
For the same practice trials, there was a significant relation between angular disparity and RT, RT = 2090.7 + 3.84*(angular disparity), F (1, 5) = 156.88, $p < .001$, $r^2 = .97$. Both the intercept (t = 63.03) and the slope (t = 12.53) were significant. For the same non-interrupt trials there was also a significant relation between angular disparity and RT, RT = 1801.34 + 2.55*(angular disparity), F (1, 3) = 64.34, $p = .004$, $r^2 = .96$. Once again, both the intercept (t = 51.32) and the slope (t = 8.02) were significant. The slopes obtained in the practice and non-interrupt trials were greater than the previously discussed LRC indicating that participants did use "mental rotation" to complete the task. The intercept for the practice trials was significantly larger than the intercept for the non-interrupt trials, t (8) = 6.01, $p < .05$. The slope for the practice trials was also slightly larger than the slope for the non-interrupt trials, t (8) = 2.6, $p < .05$. Although the slopes and intercepts of the practice trials were larger than that of the non-interrupt trials, I do not believe that inserting the side decision task changed how participants performed the handedness decision task. Recall that in this experiment, participants were only presented with two pairs of stimuli. Therefore, it is likely that a practice effect was responsible for the differences, not the insertion of the side decision task.

For the different practice trials, there was a significant relation between angular disparity and RT, RT = 2418.42 + 2.9*(angular disparity), F (1, 5) = 27.63, $p = .003$, $r^2 = .85$. Both the intercept (t = 39.62) and the slope (t = 5.25) were significant. For the different non-interrupt trials there was also a significant relation between angular disparity and RT, RT = 1963.19 + 2.44*(angular disparity), F (1, 3) = 72.36, $p = .003$, $r^2 = .96$. Both the intercept (t = 62.02) and the slope (t = 8.5) were significant. Once again, the slopes obtained in the practice and non-interrupt trials were greater than the

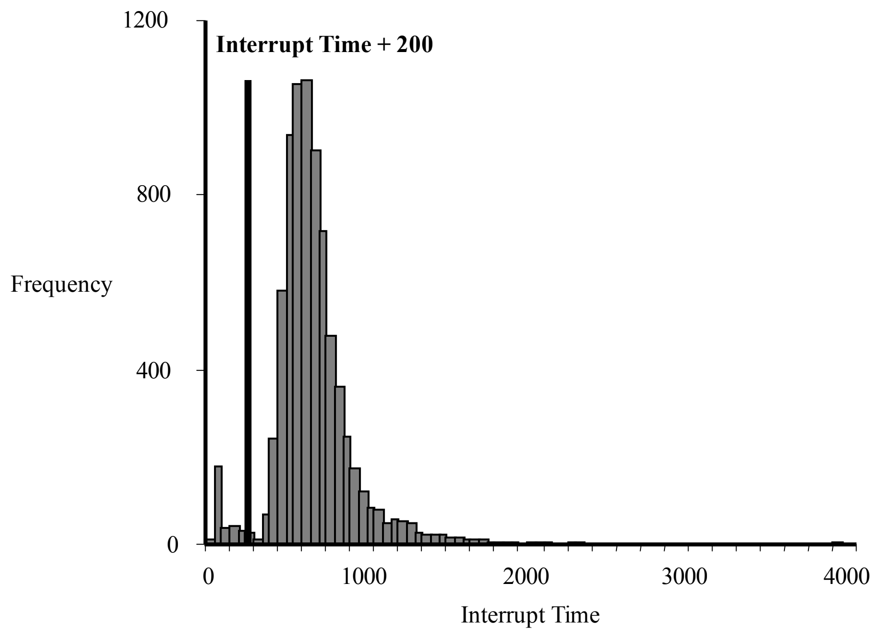
LRC indicating that participants did use “mental rotation” to complete the task. The intercept for the practice trials was significantly larger than the intercept for the non-interrupt trials, t (8) = 6.62, $p < .05$, however, there was no significant difference between the slopes, t (8) = 0.7, $p > .05$. Recall that the slope indicates the “mental rotation” stage. Therefore, I concluded that inserting the side decision task did not change the way participants completed the handedness decision task.

Determining the Responses to Side Decision Task

As in Experiment 1, the correct interrupt data are bimodal. When RT for the correct interrupt trials is plotted as a function of interrupt time there is a well-defined gap in the data. Once again, this gap falls along the line in which RT equals the interrupt time plus 200 ms (see Figure 34a). This pattern can also be observed in the histogram of responses for the interrupt trials (see Figure 34b). This gap suggests that responses occurring within 200 ms of the interrupt time represent a distribution of responses to the handedness decision task that were initiated before the interruption, and responses occurring after 200 ms of the interrupt time represent the distribution of responses to the side decision task.



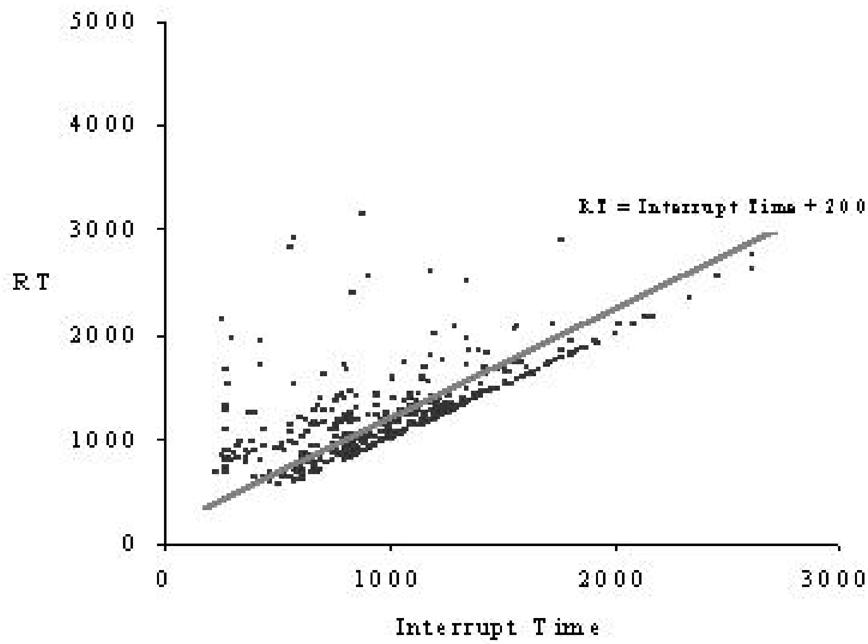
a.



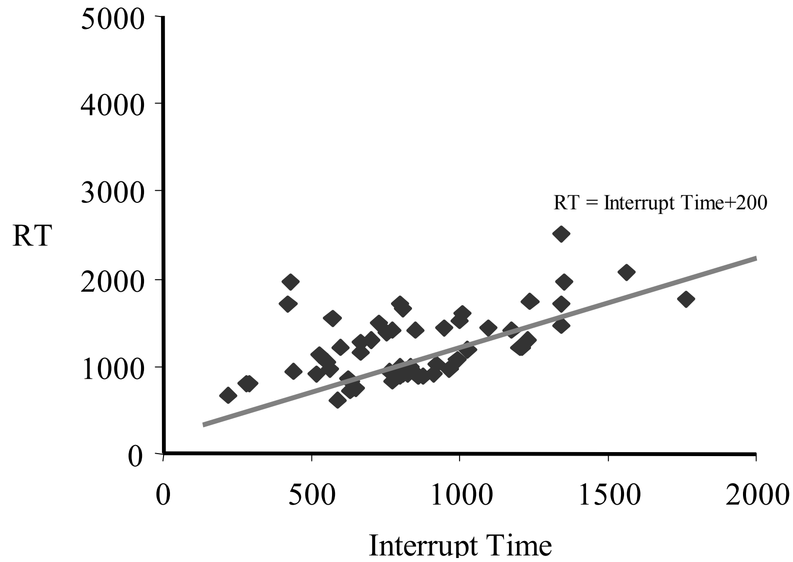
b.

Figure 34. a. RT plotted as a function of interrupt time.
b. Histogram of responses to interrupt trials.

As in Experiment 1, when RT for incorrect inhibition trials is plotted as a function of interrupt time, most of the errors occurred below the line upon which RT is equal to the interrupt time plus 200 ms (see Figure 35a). However, when RT for incorrect facilitation trials is plotted as a function of interrupt time, there are an almost equal number of errors on either side of this line (see Figure 35b). Furthermore, of the responses occurring within 200 ms of the interruption, 95% of the inhibition trials are incorrect, while only 7% of the facilitation trials are incorrect. These data provide further support that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task and responses occurring after 200 ms of the interrupt time were responses to the side decision task.



a.



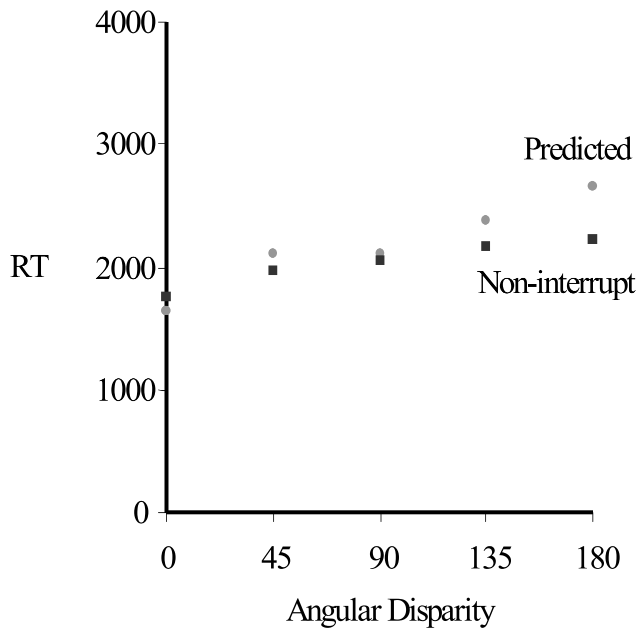
b.

Figure 35. a. Incorrect inhibition trials plotted as a function of interrupt time
b. Incorrect facilitation trials plotted as a function of interrupt time

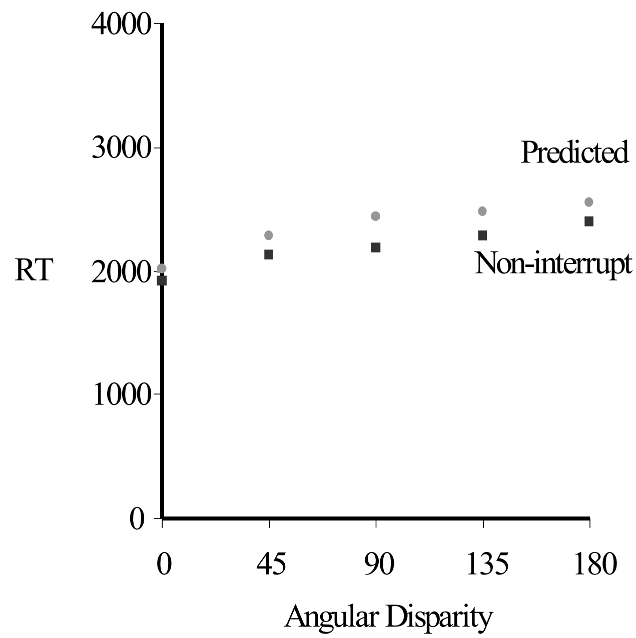
As in Experiment 1, I computed a maximum-likelihood analysis to confirm that the responses occurring within 200 ms of the interrupt time were responses to the handedness decision task. This analysis used the responses that occurred within 200 ms of the interrupt time to estimate an entire distribution of responses to the handedness decision task. I ran regression analyses on the means for the same and different data at each orientation estimated by the maximum-likelihood model and compared them to the non-interrupt data. For the same predicted distribution, there was a significant relation between angular disparity and RT, RT = 11711.24 + 5.17*(angular disparity), F (1, 3) = 47.05, p = .006, r^2 = .94. Both the intercept (t = 20.58) and the slope (t = 6.86) were significant. There was no significant difference between the intercepts of the same predicted distribution and the same non-interrupt data, t (8) = 0.99, p > .05, however the slope of the predicted distribution is significantly greater than that of the non-interrupt data, t (8) = 3.19, p < .05.

For the different predicted distribution, there was a significant relation between angular disparity and RT, RT = 2097.33 + 2.85*(angular disparity), F (1, 3) = 24.88, p = .015, r^2 = .89. Both the intercept (t = 33.3) and the slope (t = 4.99) were significant. There was no significant difference between the intercepts of the different predicted distribution and the different non-interrupt data, t (8) = 1.51, p > .05, or the slopes of the different predicted distribution and the non-interrupt data, t (8) = .63, p > .05.

The estimated distribution and the non-interrupt data are plotted in Figures 36a (same data) and Figure 36b (different data). The estimated and non-interrupt data fall very closely to each other. Although the slope from the same predicted distribution is slightly larger than that of the non-interrupt data, these distributions are still remarkably similar. Therefore, I concluded that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task, and they were excluded from the investigation of facilitation and inhibition effects.



a.

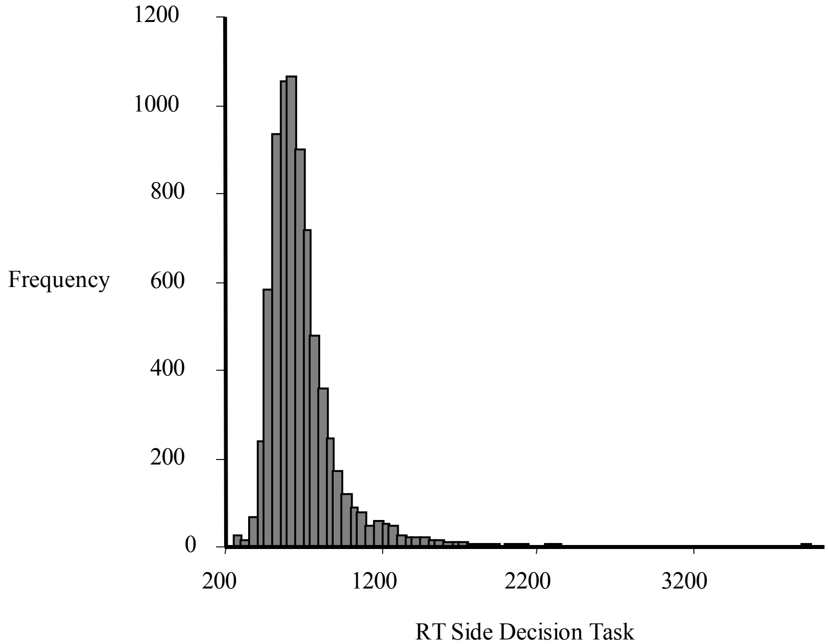


b.

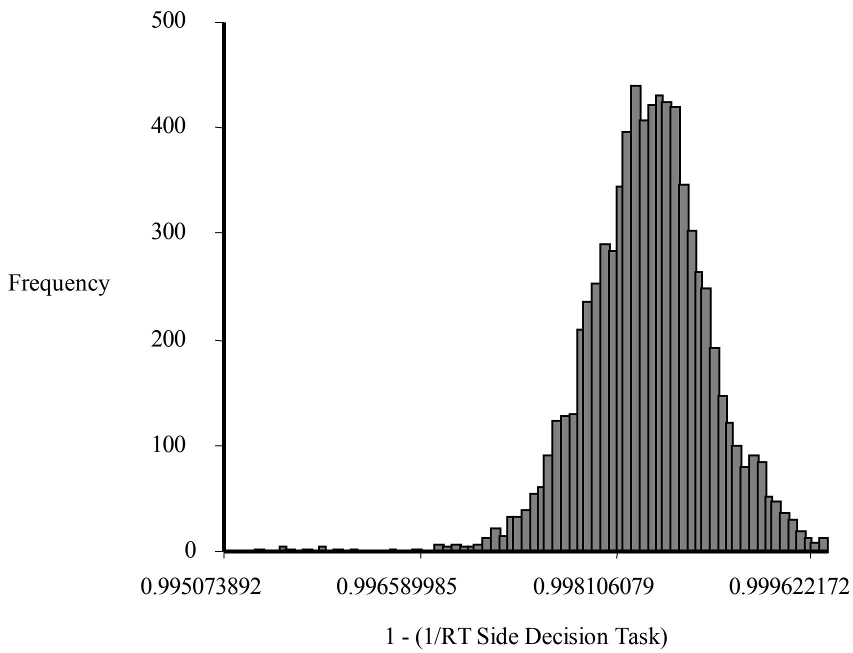
Figure 36. a. Predicted and non-interrupt data from the same trials plotted as a function of angular disparity.
b. Predicted and non-interrupt data from the different trials plotted as a function of angular disparity.

Assessing Facilitation and Inhibition Effects

The final step in the data analysis procedure was to assess the responses to the side decision task for any facilitation and inhibition effects. The two dependent variables were RT and percent correct. Figure 37a shows a histogram of the distribution of RTs to the side decision task. As in Experiment 1, this distribution is positively skewed. To normalize this data, I calculated the reciprocal for each of these values and subtracted that score from one. This normalized the distribution and maintained the ordinal relationships between the scores. The distribution of transformed data is presented in Figure 37b. To minimize between subject variability, each participants' transformed RTs were standardized.



a.



b.

Figure 37. a. Histogram of responses to side decision task.
b. Histogram of transformed distribution of responses to side decision task.

I calculated the average standardized RT and percent error for each interrupt proportion for both facilitation and inhibition trials. Once again, the same and different data were analyzed separately. Figure 38a shows the standardized RTs with standard errors for the same interrupt trials plotted as a function of interrupt proportion. The dotted line represents inhibition data and the solid line represents facilitation data. As in Experiment 1, the pattern of data follows the model that was discussed in the *General Methods* section. Specifically, the average RTs on the inhibition trials are longer than the average RTs on the facilitation trials and this discrepancy increases as interrupt proportion increases. There is a clear facilitation effect in that average RTs decrease as angular disparity increases. However, there is not an inhibition effect. The inhibition effects are present in the percent error data. That is, there are more errors on the inhibition trials than the facilitation trials and the discrepancy increases as interrupt proportion increases (see Figure 38b). There are no facilitation or inhibition effects present in the RT data for the different interrupt trials (see Figure 38c). However, the expected pattern of data is observed in the percent error data for the different interrupt trials (see Figure 38d).

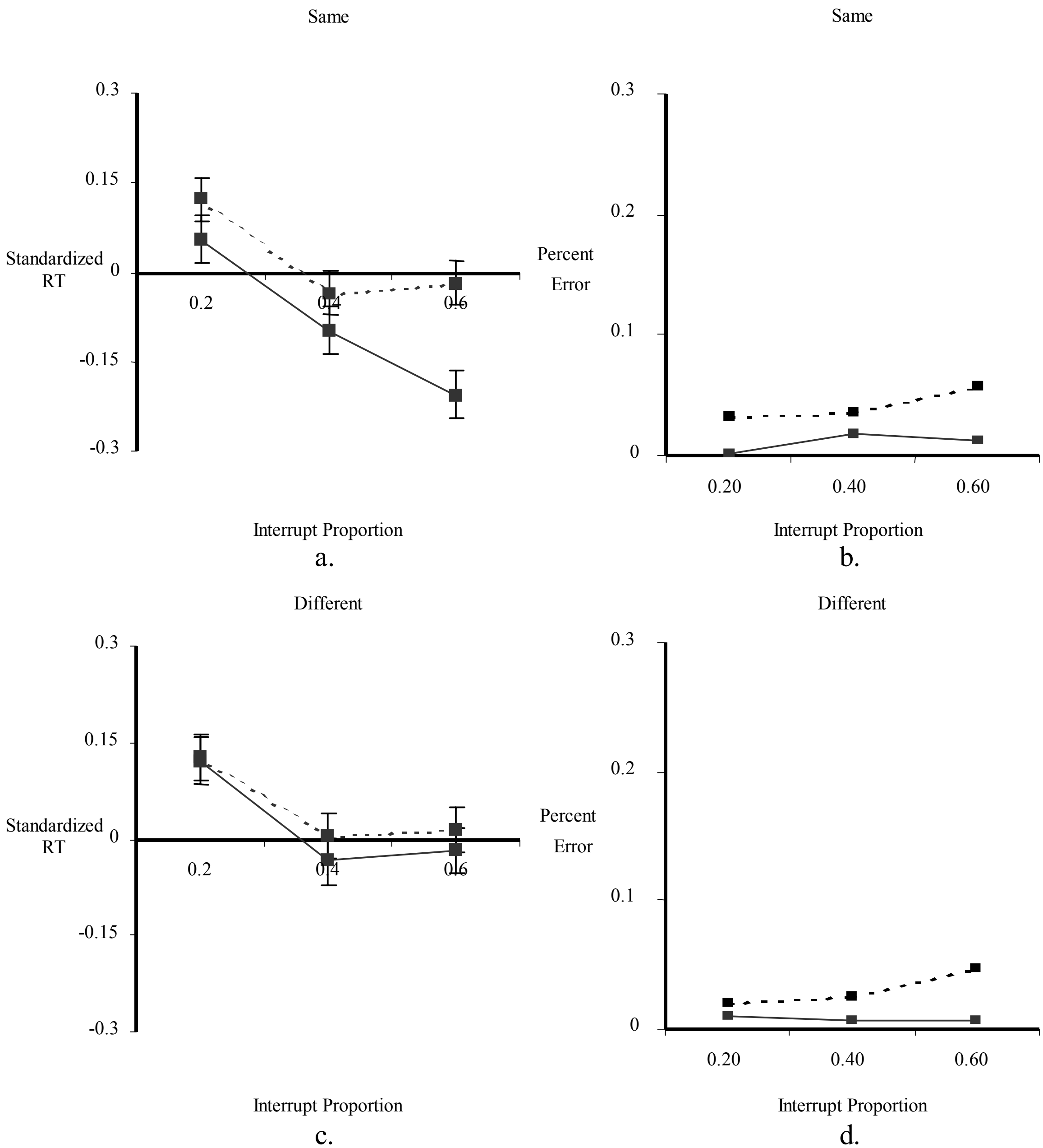


Figure 38. a. Same Standardized RTs as a function of interrupt proportion.
b. Same Percent Error as a function of interrupt proportion.
c. Different Standardized RTs as a function of interrupt proportion.
d. Different Percent Error as a function of interrupt proportion.

As noted previously, if participants must use "mental rotation" to determine the handedness of a misoriented stimulus, they will know the handedness of stimuli with smaller angular disparities faster than stimuli with larger angular disparities. Therefore, facilitation and inhibition effects will be larger for smaller angular disparities than larger angular disparities.

Figures 39 and 40 show average RTs with standard errors and percent error for the same data as a function of interrupt proportion separated by angular disparity. As shown in Figure 38, there is a general facilitation and inhibition effect. However, this effect is independent of angular disparity. In other words, the facilitation and inhibition effects are equal in magnitude across angular disparities. This pattern is also evident in the percent error data. This indicates that participants knew the handedness of stimuli with large angular disparities at approximately the same time as stimuli with small angular disparities, suggesting that they did not have to complete "mental rotation" to know the handedness of the misoriented stimuli.

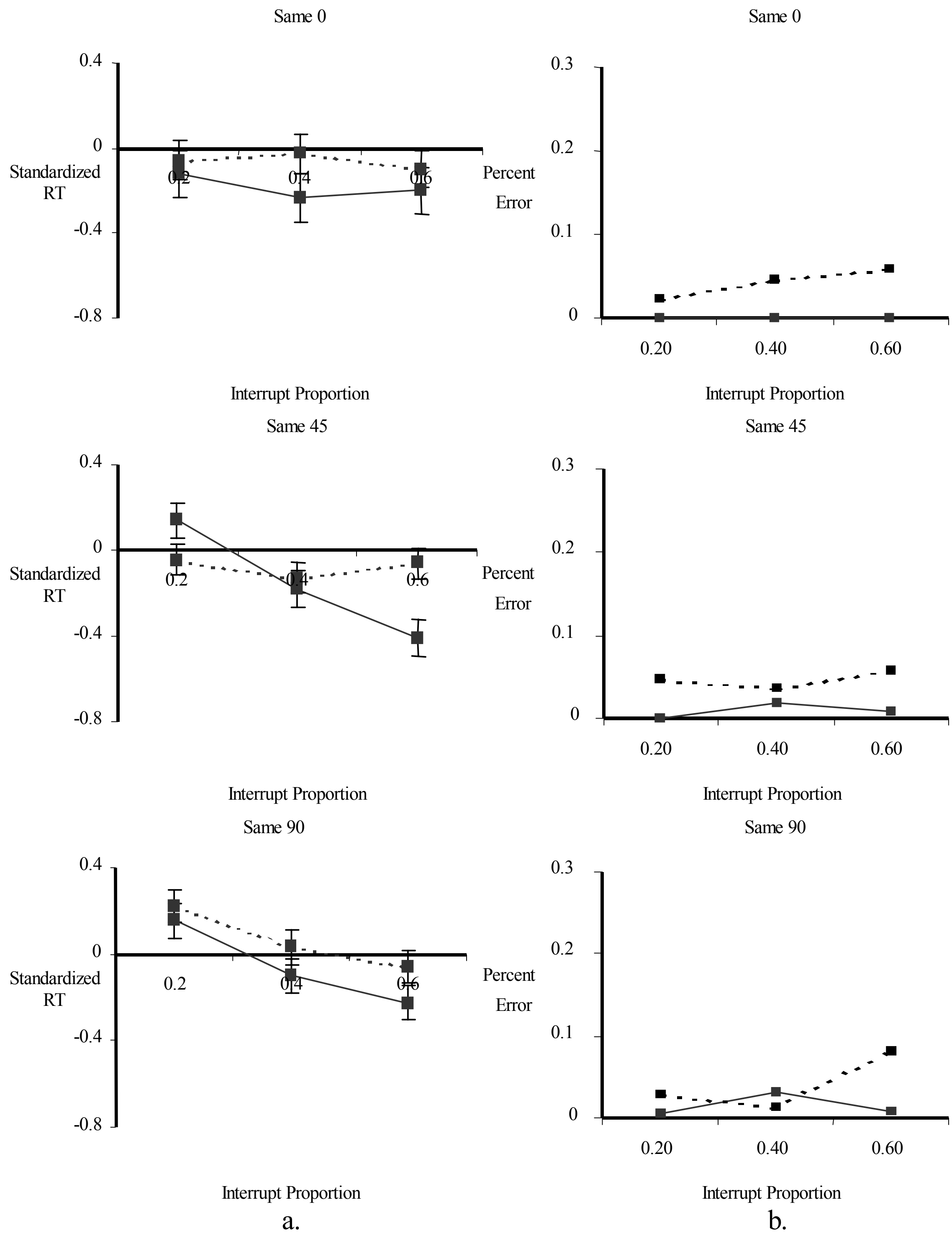


Figure 39. a. Same Standardized RTs separated by angular disparity (0° to 90°).
b. Same Percent Error separated by angular disparity (0° to 90°).

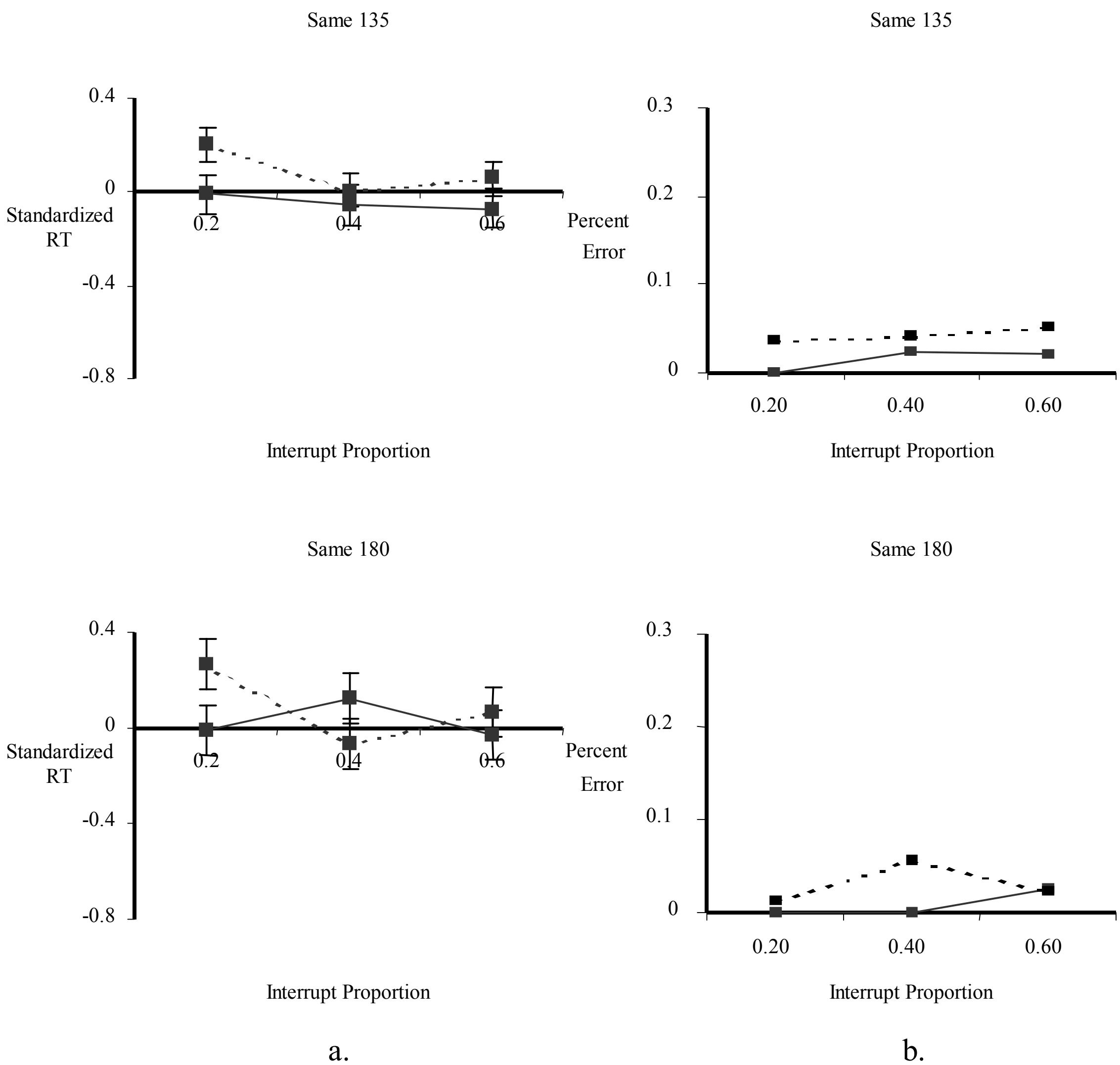


Figure 40. a. Same Standardized RTs (135° and 180°).
b. Same Percent Error (135° and 180°).

Figures 41 and 42 show average RTs with standard errors and percent error for the different data as a function of interrupt proportion separated by angular disparity. Recall that in this experiment there are no overall facilitation or inhibition effects present in the RT data for the different interrupt trials (see Figure 38). Figures 41 and 42 confirm that there are no differences across angular disparity (i.e., there are no effects at any angular disparity). Figure 38 also shows that inhibition effects were present in the percent error data. Figures 41 and 42 show that this effect is independent of angular disparity (i.e., it is equal in magnitude across angular disparity). Although the different RT data do not show facilitation or inhibition effects, the effects are present in the error data. Furthermore, these effects are independent of angular disparity. The data from this experiment are somewhat noisy so it is difficult to interpret the findings. However, the lack of a difference across angular disparity for the RT data and the percent error data directly opposes the Shepard model.

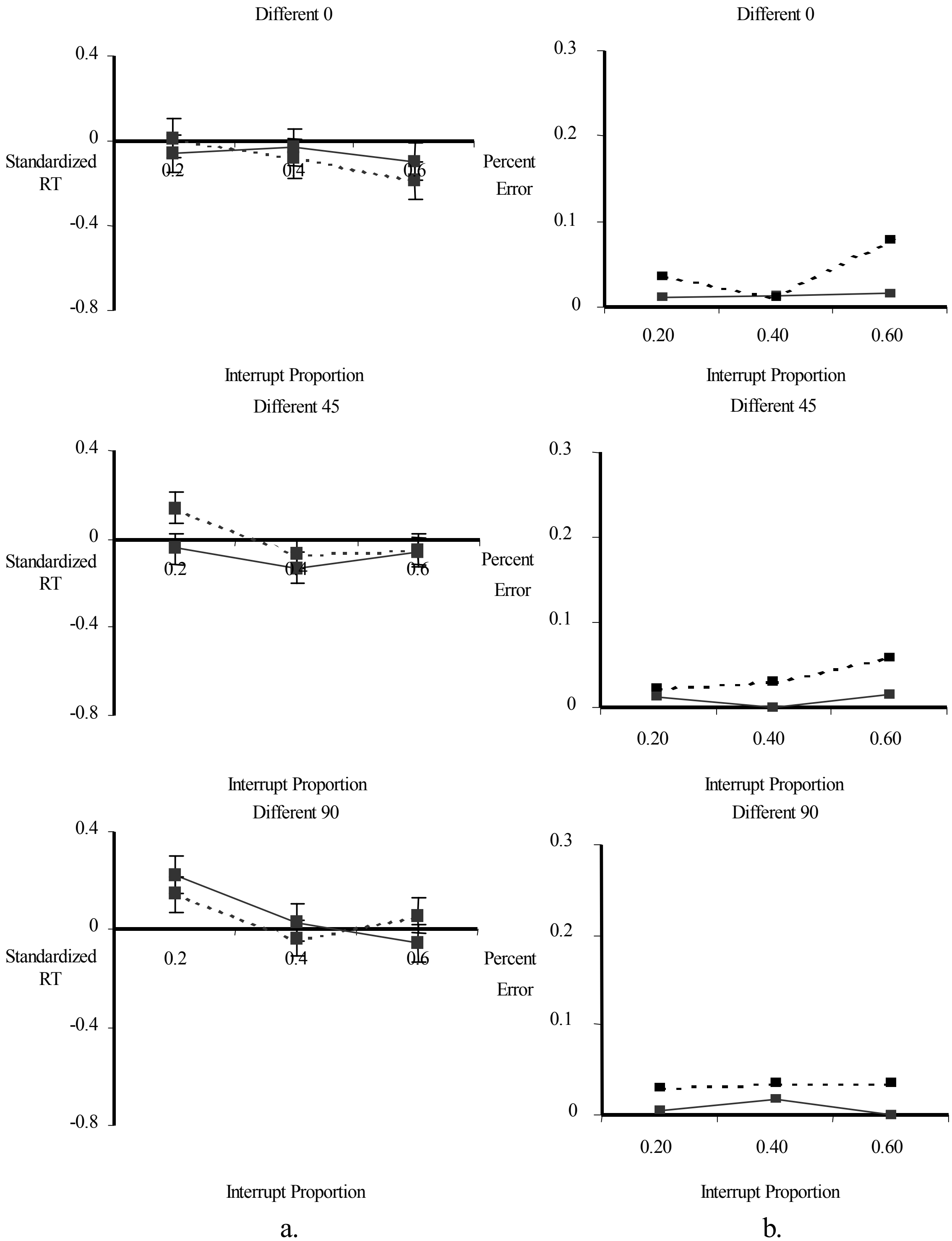


Figure 41. a. Different Standardized RTs separated by angular disparity (0° to 90°).
b. Different Percent Error separated by angular disparity (0° to 90°).

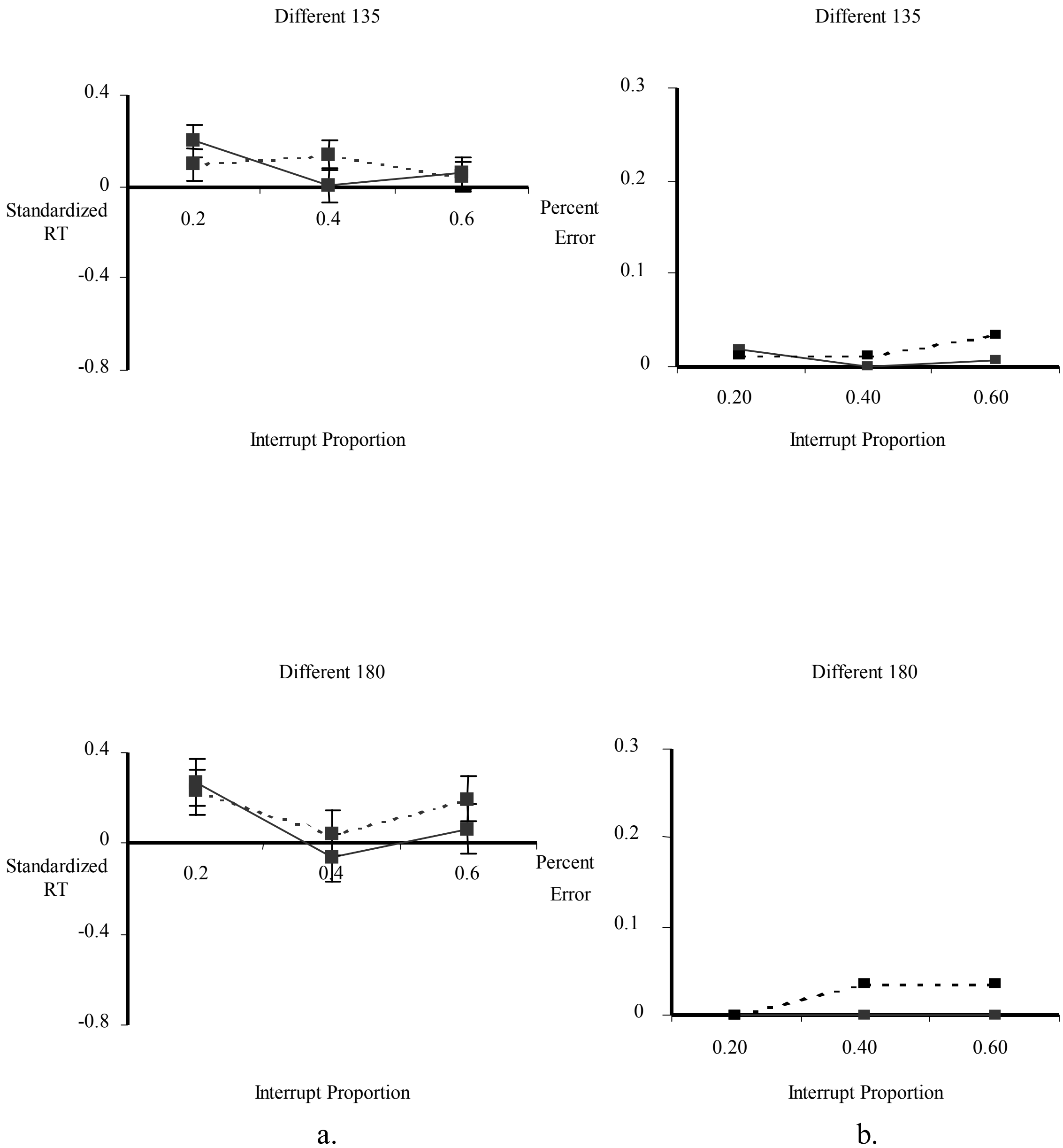


Figure 42. a. Different Standardized RTs (135° and 180°).
b. Different Percent Error (135° and 180°).

As in Experiment 1, I calculated Cohen's *d* effect sizes for the facilitation and inhibition effects for both the RT data and the percent error data. Once again, the same and different data were analyzed separately. Recall that effect sizes provide a measure of the facilitation and inhibition for each interrupt proportion at each angular disparity. Larger effect sizes indicate greater disparities between facilitation and inhibition trials. Therefore, if participants used "mental rotation" to solve the handedness decision task, one would expect larger effect sizes at smaller angular disparities.

Figure 43a is a plot of the effect sizes for the same RT data plotted as a function of angular disparity and separated by interrupt proportion. Once again, a separate line represents each interrupt proportion. I fit log functions to each of the lines in this graph (they are plotted in Figure 43b). It is difficult to interpret these results. It looks as if effect size increases as interrupt proportion increases, however the .2 interrupt proportion does not conform to this conclusion. It also appears that there is no effect of angular disparity, however, the .4 interrupt proportion does not conform to this conclusion. The log function for the same RT data with the .2 interrupt proportion is $d_{.2} = 0.017 * \mathrm{Ln}(\alpha) - 0.0008$, $r^2 = .08$. The log function for .4 is $d_{.4} = -0.0037 * \mathrm{Ln}(\alpha) + 0.1709$, $r^2 = .46$. The log function for .6 is $d_{.6} = 0.0034 * \mathrm{Ln}(\alpha) + 0.0961$, $r^2 = .01$. As mentioned earlier, it is difficult to interpret these findings.

Figure 43c is a plot of the effect sizes for the same error data plotted as a function of angular disparity and separated by interrupt proportion. I next fit log functions to each of the lines in this graph (they are plotted in Figure 43d). The error effect size data is very similar to that of the RT effect size data. It appears as if effect size increases as interrupt proportion increases, however the .2 interrupt proportion does not conform to this conclusion. It also appears that there is no effect of angular disparity. However, there is a slight decrease in effect size as angular disparity increases for the .4 interrupt proportion. The log function for the same data with the .2 interrupt proportion is $d_{.2} = 0.0561*\mathrm{Ln}(\alpha) + 1.6867$, $r^2 = .02$. The log function for .4 is $d_{.4} = -0.2093*\mathrm{Ln}(\alpha) + 1.7854$, $r^2 = .12$. The log function for .6 is $d_{.6} = -0.2059*\mathrm{Ln}(\alpha) + 2.6858$, $r^2 = .11$. Once again, it is difficult to interpret these findings.

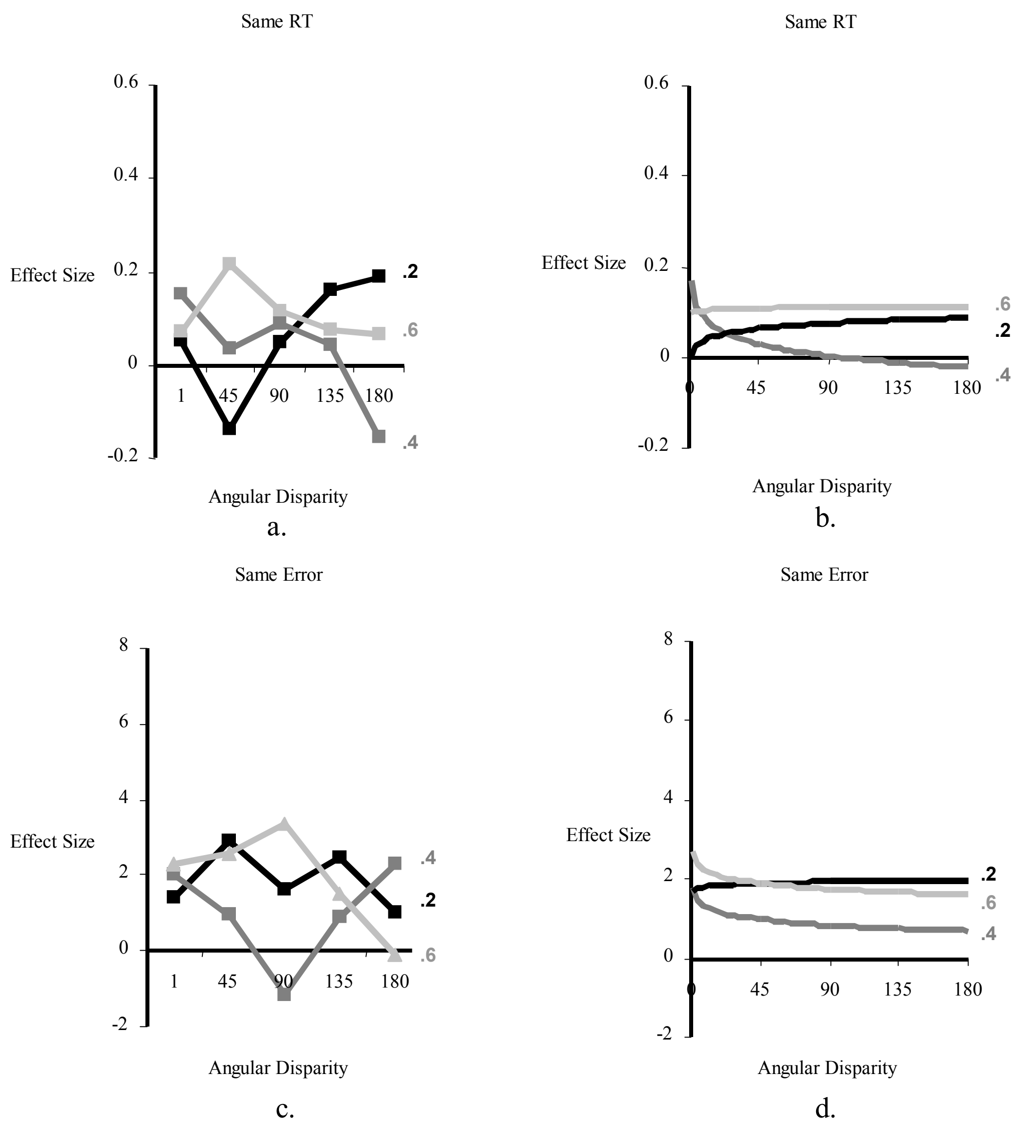


Figure 43. a. Effect Sizes for Same RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Same RTs.
c. Effect Sizes for Same Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Same Error.

Figure 44a is a plot of the effect sizes for the different RT data plotted as a function of angular disparity. The arrangement of this graph is the same as the same RT effect size graph (i.e., separated by interrupt proportion). Figure 44b is a plot of the log functions to these data. Recall that in Experiment 2 there were no facilitation or inhibition effects. This result can also be observed in Figures 44a and 44b (i.e., the effect sizes approximate 0). The log function for the different data with the .2 interrupt proportion is $d_{.2} = -0.022*\mathrm{Ln}(\alpha) + 0.09$, $r^2 = .25$. The log function for .4 is $d_{.4} = 0.0215*\mathrm{Ln}(\alpha) - 0.0489$, $r^2 = .39$. The log function for .6 is $d_{.6} = 0.025*\mathrm{Ln}(\alpha) - 0.0275$, $r^2 = .62$. These functions indicate little if any facilitation or inhibition effects.

Figure 44c is a plot of the effect sizes for the different error data plotted as a function of angular disparity and separated by interrupt proportion. The log functions for these data are plotted in Figure 44d. These data show an effect of interrupt proportion in that as interrupt proportion increases, so do effect sizes. However, these data show no effect of angular disparity. The log function for the different data with the .2 interrupt proportion is $d_{.2} = -0.1537*\mathrm{Ln}(\alpha) + 1.1649$, $r^2 = .16$. The log function for .4 is $d_{.4} = .3175*\mathrm{Ln}(\alpha) + 0.103$, $r^2 = .58$. The log function for .6 is $d_{.6} = -0.0112*\mathrm{Ln}(\alpha) + 2.0705$, $r^2 = .01$. As mentioned previously, the different error data show no effect of angular disparity, which directly opposes the Shepard model.

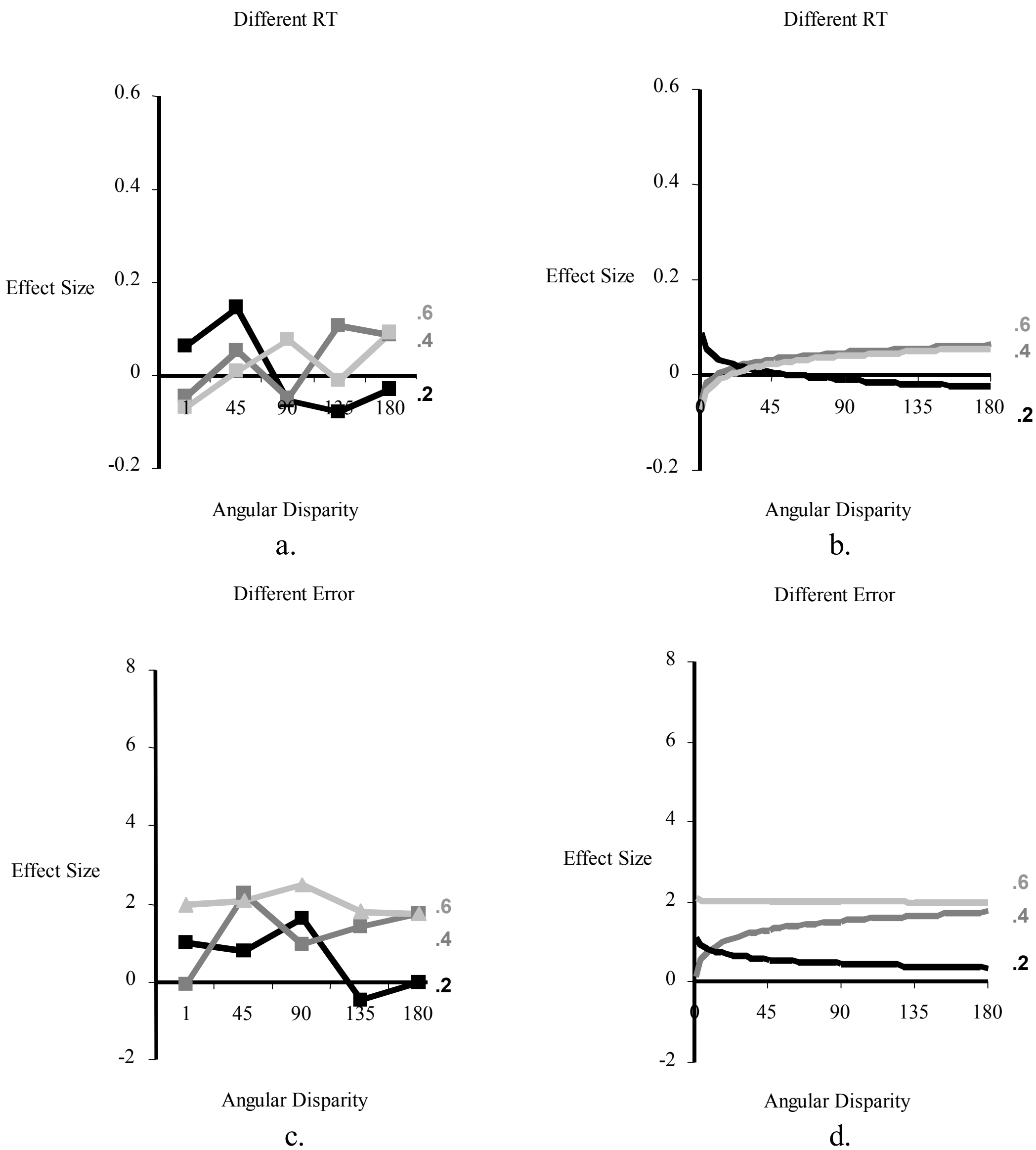


Figure 44. a. Effect Sizes for Different RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Different RTs.
c. Effect Sizes for Different Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Different Error.

To clarify the relationship between effect sizes and interrupt proportion, I calculated effect sizes for both RT and percent error averaged across angular disparity for each interrupt proportion. These averages were calculated separately for same and different data. The effect of interrupt proportion is evidenced by a nondegenerative function relating average effect size to interrupt proportion (See Figure 45). I will not report statistics on these functions because there were only three interrupt proportions.

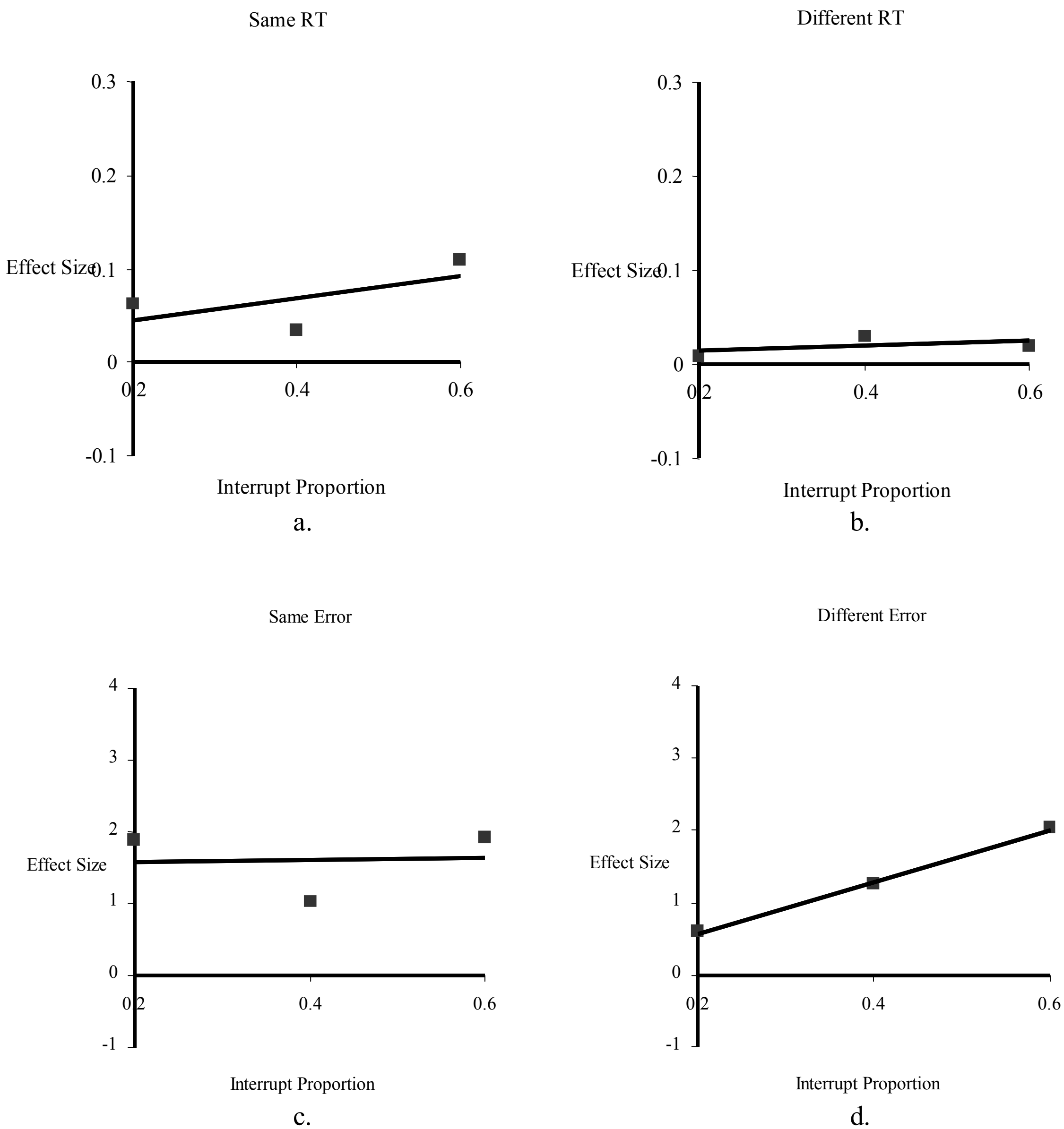


Figure 45. a. Average Effect Sizes for Same RTs plotted by interrupt proportion.
b. Average Effect Sizes for Different RTs plotted by interrupt proportion.
c. Average Effect Sizes for Same Errors plotted by interrupt proportion.
d. Average Effect Sizes for Different Errors plotted by interrupt proportion.

To clarify the relationship between effect sizes and angular disparity, I calculated effect sizes for both RT and percent error averaged across interrupt proportion for each angular disparity. Again, these averages were calculated separately for same and different data. For the same RT data, there was no relationship between angular disparity and effect size, effect size = 0.0809 - 0.00014*(angular disparity), F (1, 3) = 0.3457, p = .593, r^2 = .1057. Neither the intercept (t = 3.168) nor the slope (t = 0.5956) was significant. For the different RT data, there was no relationship between angular disparity and effect size, effect size = 0.0056 $^{+}$ 0.00016*(angular disparity), F (1, 3) = 0.2732, p = .637, r^2 = .0835. Neither the intercept (t = 0.1716) nor the slope (t = 0.5227) was significant. For the same error data, there was no relationship between angular disparity and effect size, effect size = 2.0565 - 0.00494*(angular disparity), F (1, 3) = 5.1062, p = .109, r^2 = .63. The intercept was significant (t = 7.975), however the slope was not (t = 2.26). For the different error data, there was no relationship between angular disparity and effect size, effect size = 1.379 - 0.00088*(angular disparity), F (1, 3) = 0.0821, p = .7932, r^2 = .02663. The intercept was significant (t = 4.0688), however the slope was not (t = 2.26). All of these functions indicate no relationship between effect size and angular disparity (See Figure 46).

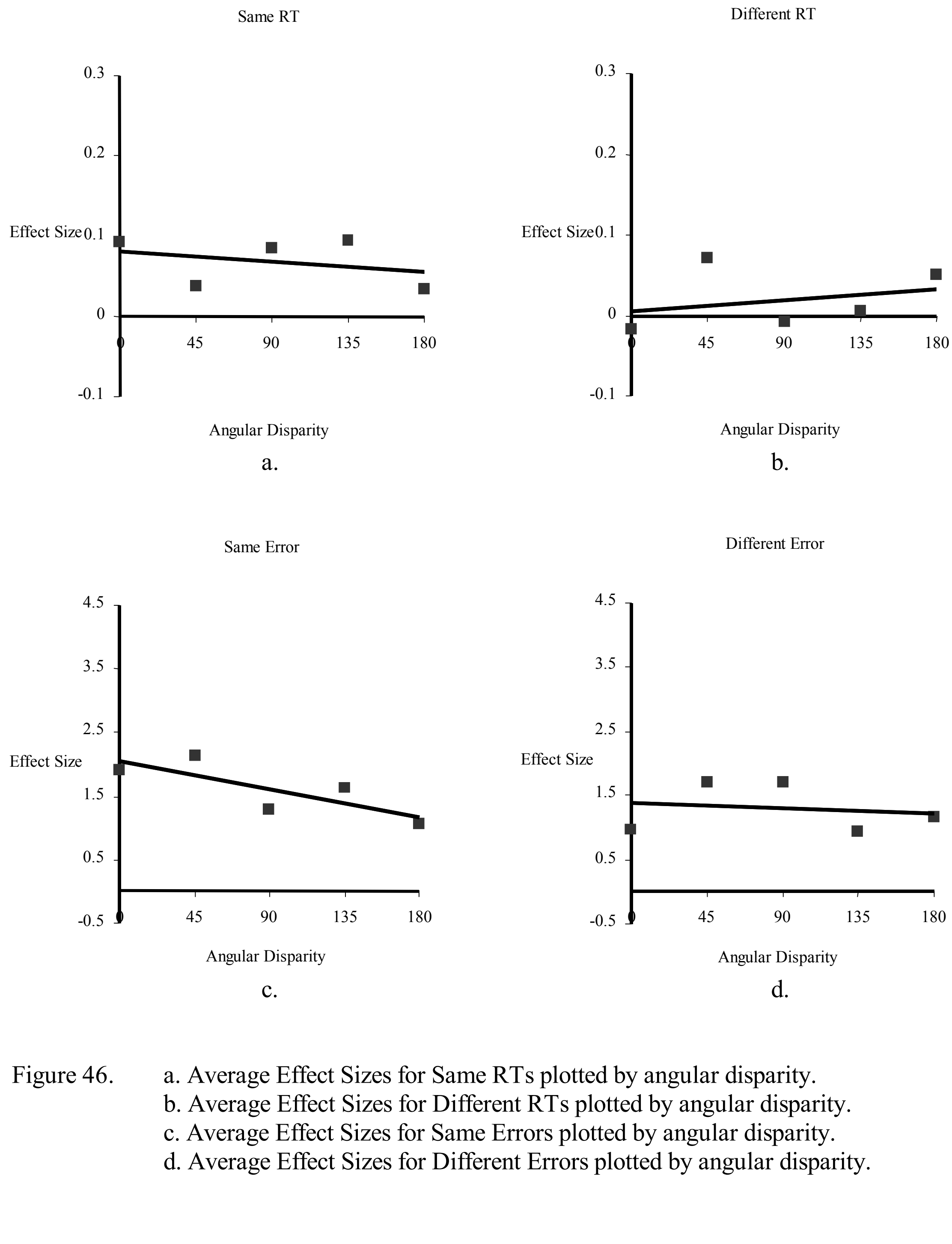


Figure 46. a. Average Effect Sizes for Same RTs plotted by angular disparity.
b. Average Effect Sizes for Different RTs plotted by angular disparity.
c. Average Effect Sizes for Same Errors plotted by angular disparity.
d. Average Effect Sizes for Different Errors plotted by angular disparity.

In summary, the data from Experiment 2 were noisy and difficult to interpret. There was an effect of interrupt proportion evidenced by nondegenerative relationships between facilitation and inhibition effects and interrupt proportion. There was no effect angular disparity, which opposes the Shepard model.

Discussion

Experiment 2 was designed to test whether participants knew the handedness of misoriented stimuli before using “mental rotation.” Therefore, only two stimuli were presented to each participant throughout the experiment. I first compared the data from the practice trials and the non-interrupt trials and found that the insertion of the side decision task did not change the way participants completed the handedness decision task. I next concluded that responses occurring within 200 ms of the interrupt time were actually responses to the handedness decision task. Therefore, I excluded these trials from the investigation of facilitation and inhibition effects.

For the interrupt trials, the Shepard model predicted an effect of both interrupt proportion and angular disparity. Conversely, the double check model predicted an effect of interrupt proportion, but no effect of angular disparity. The results of Experiment 2 were somewhat conflicting. The same RT and error data seem to support the double check model, however, the data were noisy. The different RT data show no facilitation or inhibition effects, while the different error data show only inhibition effects. Once again, the lack of an effect of angular disparity opposes the Shepard model. The evidence from Experiment 2 seem to support the double check model, however, this is far from conclusive.

Experiment 1 showed that the dualtask procedure and analysis can detect effects of both interrupt proportion and angular disparity. Experiment 2 tested whether participants must use "mental rotation" to know the handedness of misoriented stimuli when they are only presented with two pairs of stimuli throughout the experiment. The results of Experiment 2 were difficult to interpret. The data seem to support the double check model, however I felt that more thorough investigations should be conducted. Experiments 3 and 4 were replications of Experiments 1 and 2 with more participants and more interrupt proportions. The larger number of interrupt proportions allowed a more comprehensive test between the Shepard model and the double check model.

EXPERIMENT 3

The results of Experiment 1 indicated that participants did not know the handedness of the misoriented stimuli before using "mental rotation." The results of Experiment 2 were somewhat unclear. For this reason, Experiment 3 was a replication of Experiment 1 with more participants and more interrupt proportions.

Method

Participants

141 undergraduate university students volunteered to participate in this experiment. Data from 16 of these participants were not used in the present analyses, because their error rates on the non-interrupt trials was greater than 25 percent. This indicated that these participants did not understand the handedness decision task.

Stimuli and Apparatus

The stimuli and apparatus were the same as in Experiment 1.

Design and Procedure

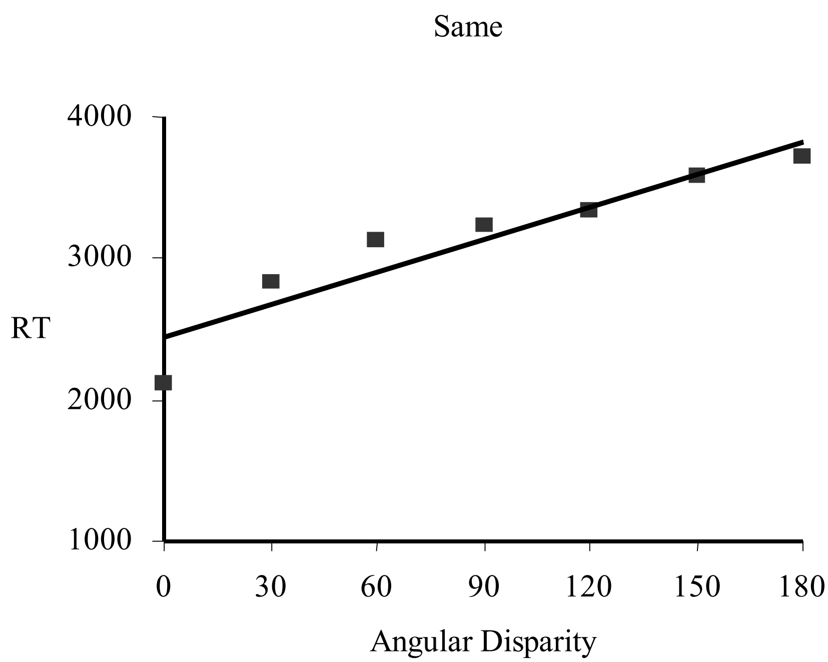
The experimental design and procedures were identical to those of Experiment 1 with one exception. We increased the number of interrupt proportions from three to six. The new interrupt proportions were .15, .30, .45, .60, .75 and .90. Therefore, Experiment 3 was a 8 X 6 X 2 X 2 X 2 X 2 within subject design, with eight levels of orientation (0° to 315° in 45° increments), six levels of interrupt proportions (.15, .30, .45, .60, .75 and .90), two types of probe (same and different), two levels of trial type (interrupt and non-interrupt), two levels of interrupt side (left and right), and two levels of task correspondence (facilitation and inhibition). The same dependent variables (RT and percent error) were used.

Results

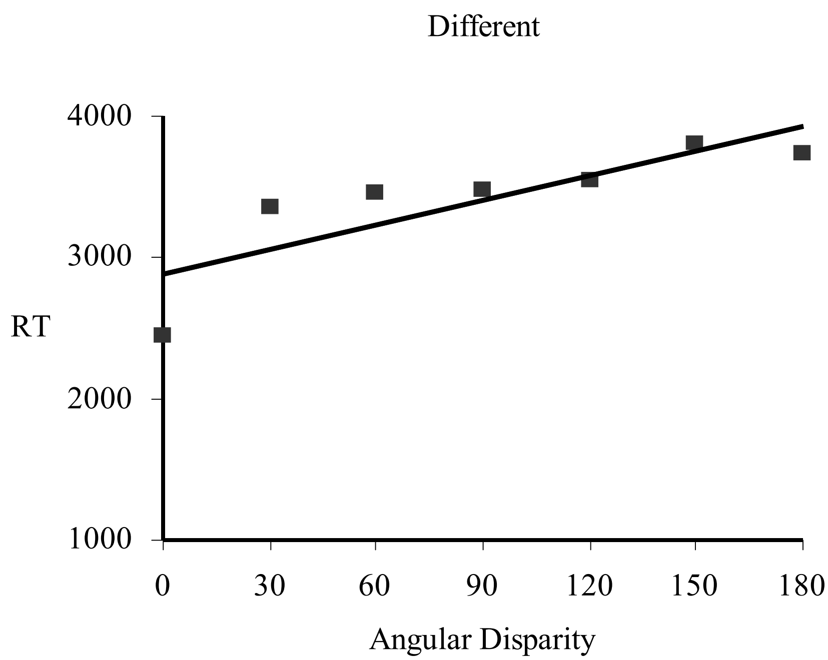
The same three stages of data analysis were used in this Experiment. I had to (1) verify that inserting the side decision task did not affect how participants performed the handedness task, (2) determine which responses were to the side decision task, and (3) investigate facilitation and inhibition effects. Once again, all inferential analyses were conducted with an alpha level of .05. For all RT analyses, I only used trials in which the participant responded correctly. For the practice analysis, I excluded all trials that exceeded 10000 ms (4.16 SD from the mean, 1.78% of the trials). For the non-interrupt analysis, I also excluded all trials that exceeded 10000 ms (6.13 SD from the mean, 0.17% of the trials). For the side decision task analyses, I excluded trials with RTs exceeding 5000 ms (16.51 SD from the mean, 0.05 % of the trials).

Comparison of Practice and Non-Interrupt Trials

As in Experiments 1 and 2, the average RT for each orientation was calculated for both the practice and non-interrupt trials and collapsed around 180°. Once again, the same and different data were analyzed separately. Figure 47a shows the average RT of the same data from the practice trials plotted as a function of angular disparity, and Figure 47b shows the average RT of the different data from the practice trials plotted as a function of angular disparity. Figure 48a shows the average RT of the same data from the non-interrupt trials plotted as a function of angular disparity, and Figure 48b shows the average RT of the different data from the non-interrupt trials plotted as a function of angular disparity.

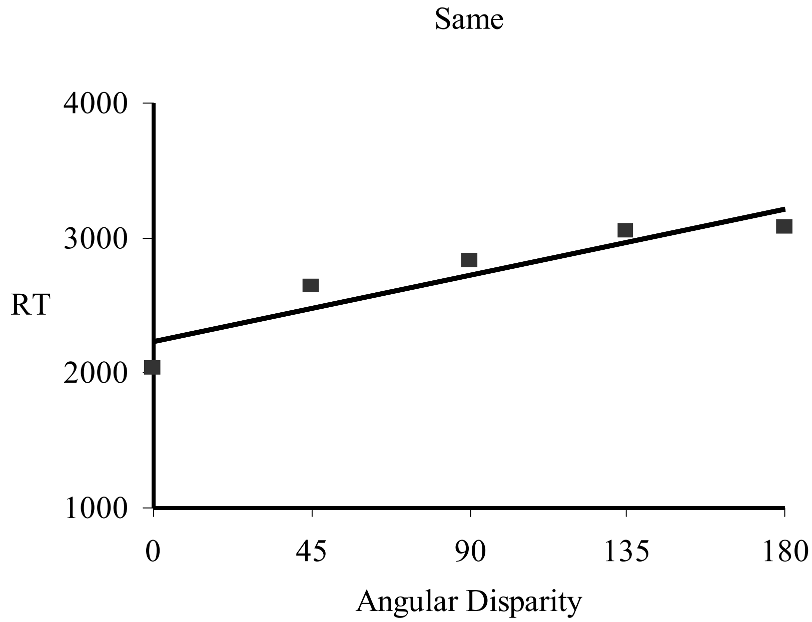


a.

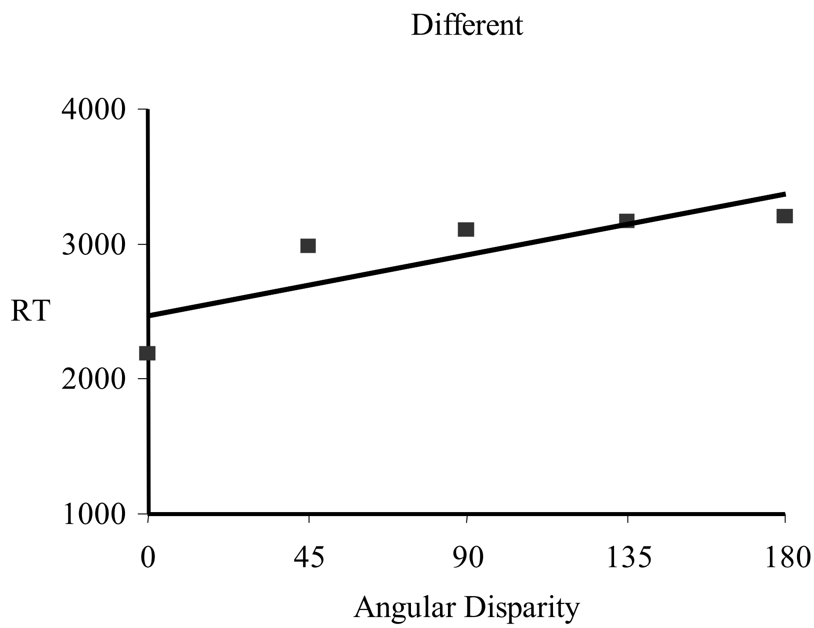


b.

Figure 47. a. Average RT for same practice trials plotted as a function of angular disparity.
b. Average RT for different practice trials plotted as a function of angular disparity.



a.



b.

Figure 48. a. Average RT for same non-interrupt trials plotted as a function of angular disparity.
b. Average RT for different non-interrupt trials plotted as a function of angular disparity.

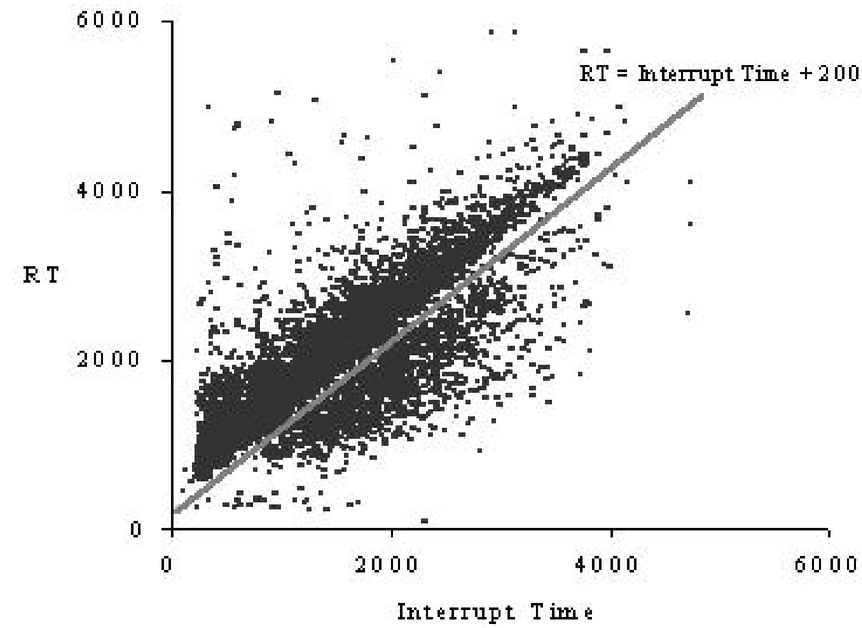
For the same practice trials, there was a significant relation between angular disparity and RT, RT = 2441.09 + 7.71*(angular disparity), $\underline{F}$ (1, 5) = 37.11, $\underline{p}$ = .002, r^2 = .88. Both the intercept ($\underline{t}$ = 17.83) and the slope ($\underline{t}$ = 6.09) were significant. For the same non-interrupt trials there was also a significant relation between angular disparity and RT, RT = 2231.54 + 5.52*(angular disparity), $\underline{F}$ (1, 3) = 17.34, $\underline{p}$ = .025, r^2 = .85. Once again, both the intercept ($\underline{t}$ = 15.28) and the slope ($\underline{t}$ = 4.16) were significant. The slopes obtained in the practice and non-interrupt trials were greater than the previously discussed LRC indicating that participants did use "mental rotation" to complete the task. Importantly, there were no significant differences between the intercepts of the practice and the non-interrupt trials, $\underline{t}$ (8) = 1.05, $\underline{p}$ > .05, or the slopes of the practice and the non-interrupt trials, $\underline{t}$ (8) = 1.19, $\underline{p}$ > .05. These findings indicate that inserting the interrupt task did not affect how participants performed the handedness decision task.

For the different practice trials, there was a significant relation between angular disparity and RT, RT = 2885.86 + 5.76*(angular disparity), $\underline{F}$ (1, 5) = 11.44, $\underline{p}$ = .02, r^2 = .67. Both the intercept ($\underline{t}$ = 15.67) and the slope ($\underline{t}$ = 3.38) were significant. For the different non-interrupt trials there was no significant relation between angular disparity and RT, RT = 2482.93 + 4.95*(angular disparity), $\underline{F}$ (1, 3) = 6.67, $\underline{p}$ = .08, r^2 = .67. Although, not statistically significant, this relationship approached significance with a $\underline{p}$ value of .08. Furthermore the slope was greater than 1 ms/degree indicating that participants were using "mental rotation" to complete the task. The intercept ($\underline{t}$ = 11.76) was significant, but the slope ($\underline{t}$ = 2.58) was not. Once again, there were no significant differences between the intercepts of the practice and the non-interrupt trials, $\underline{t}$ (8) = 1.43, $\underline{p}$ > .05, or the slopes of the practice and the non-interrupt trials, $\underline{t}$ (8) = 2.05, $\underline{p}$ > .05.

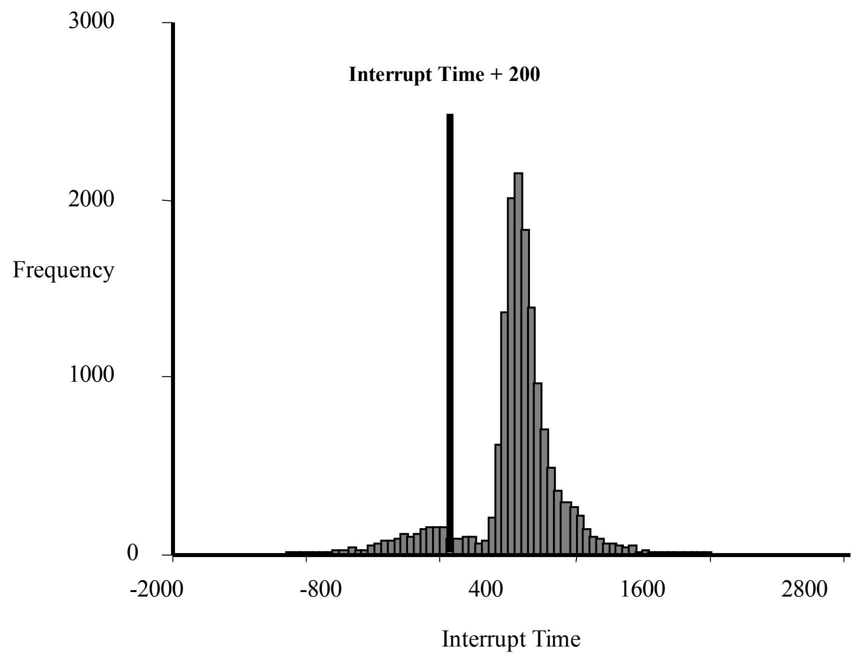
These findings indicate that inserting the interrupt task did not affect how participants performed the handedness decision task.

Determining the Responses to Side Decision Task

As in Experiments 1 and 2, the correct interrupt data are bimodal. When RT for the correct interrupt trials is plotted as a function of interrupt time there is a well-defined gap in the data. Once again, this gap falls along the line in which RT equals the interrupt time plus 200 ms (see Figure 49a). This pattern can also be observed in the histogram of responses for the interrupt trials (see Figure 49b). This gap suggests that responses occurring within 200 ms of the interrupt time represent a distribution of responses to the handedness decision task that were initiated before the interrupt time, and responses occurring after 200 ms of the interrupt time represent the distribution of responses to the side decision task.



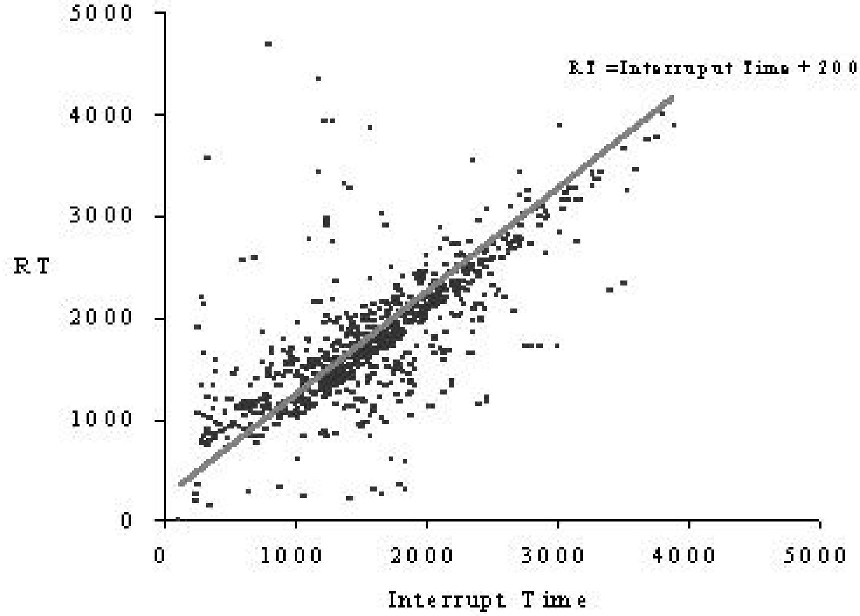
a.



b.

Figure 49. a. RT plotted as a function of interrupt time.
b. Histogram of responses to interrupt trials.

As in Experiments 1 and 2, when RT for incorrect inhibition trials is plotted as a function of interrupt time, most of the errors occurred below the line upon which RT is equal to the interrupt time plus 200 ms (see Figure 50a). However, when RT for incorrect facilitation trials is plotted as a function of interrupt time, there are an almost equal number of errors on either side of this line (see Figure 50b). Furthermore, of the responses occurring within 200 ms of the interrupt time, 85% of the inhibition trials are incorrect, while only 13% of the facilitation trials are incorrect. These data provide further support that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task and responses occurring after 200 ms of the interrupt time were responses to the side decision task.



a.

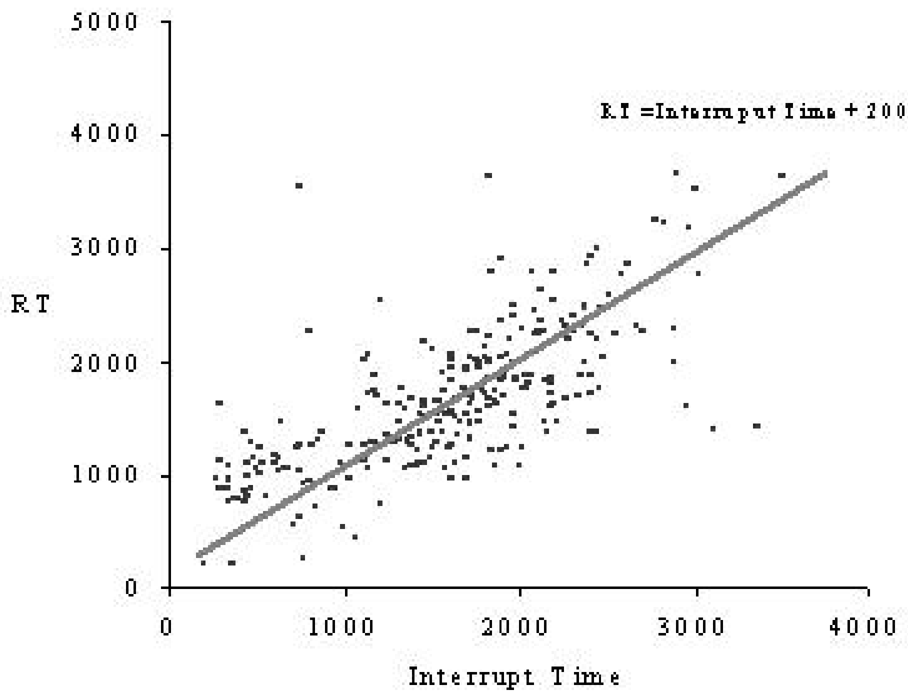


Figure 50. a. Incorrect inhibition trials plotted as a function of interrupt time
b. Incorrect facilitation trials plotted as a function of interrupt time

As in Experiments 1 and 2, I computed a maximum-likelihood analysis to confirm that the responses occurring within 200 ms of the interrupt time were responses to the handedness decision task. This analysis used the responses that occurred within 200 ms of the interrupt time to estimate an entire distribution of responses to the handedness decision task. I ran regression analyses on the means for the same data at each orientation estimated by the maximum-likelihood model and compared them to the non-interrupt data. The maximum likelihood model was unable to converge on a prediction for the different data. The failure to converge is most likely due to too much noise in the different data. For the same predicted distribution, there was a significant relation between angular disparity and RT, RT = 2594.002 + 7.81*(angular disparity), $\underline{F}$ (1, 3) = 20.15, $\underline{p}$ = .02, r^2 = .87. Both the intercept ($\underline{t}$ = 13.53) and the slope ($\underline{t}$ = 4.48) were significant. There were no significant differences between the intercepts of the same predicted distribution and the same non-interrupt data, $\underline{t}$ (8) = 1.5, $\underline{p}$ > .05, or between the slopes of the predicted distribution and the non-interrupt data, $\underline{t}$ (8) = 0.96, $\underline{p}$ > .05.

The estimated distribution and the same non-interrupt data are plotted in Figure 51. The estimated and non-interrupt distributions are relatively close to each other. Furthermore, there were no significant differences between either the intercepts or slopes of these distributions. Therefore, I concluded that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task, and they were excluded from the investigation of facilitation and inhibition effects.

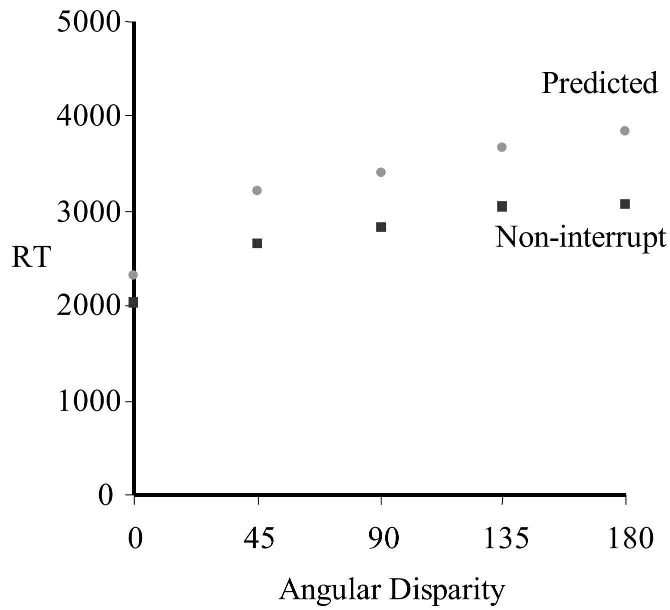


Figure 51. Predicted and non-interrupt data from the same trials plotted as a function of angular disparity.

Assessing Facilitation and Inhibition Effects

The final step in the data analysis procedure was to assess the responses to the side decision task for facilitation and inhibition effects. Figure 52a shows a histogram of the distribution of RTs to the side decision task. As in Experiments 1 and 2, this distribution is positively skewed. To normalize the data, I calculated the reciprocal for each of these values and subtracted that score from one. The distribution of transformed data is presented in Figure 52b. To minimize between subject variability, each participants' transformed RTs were standardized.

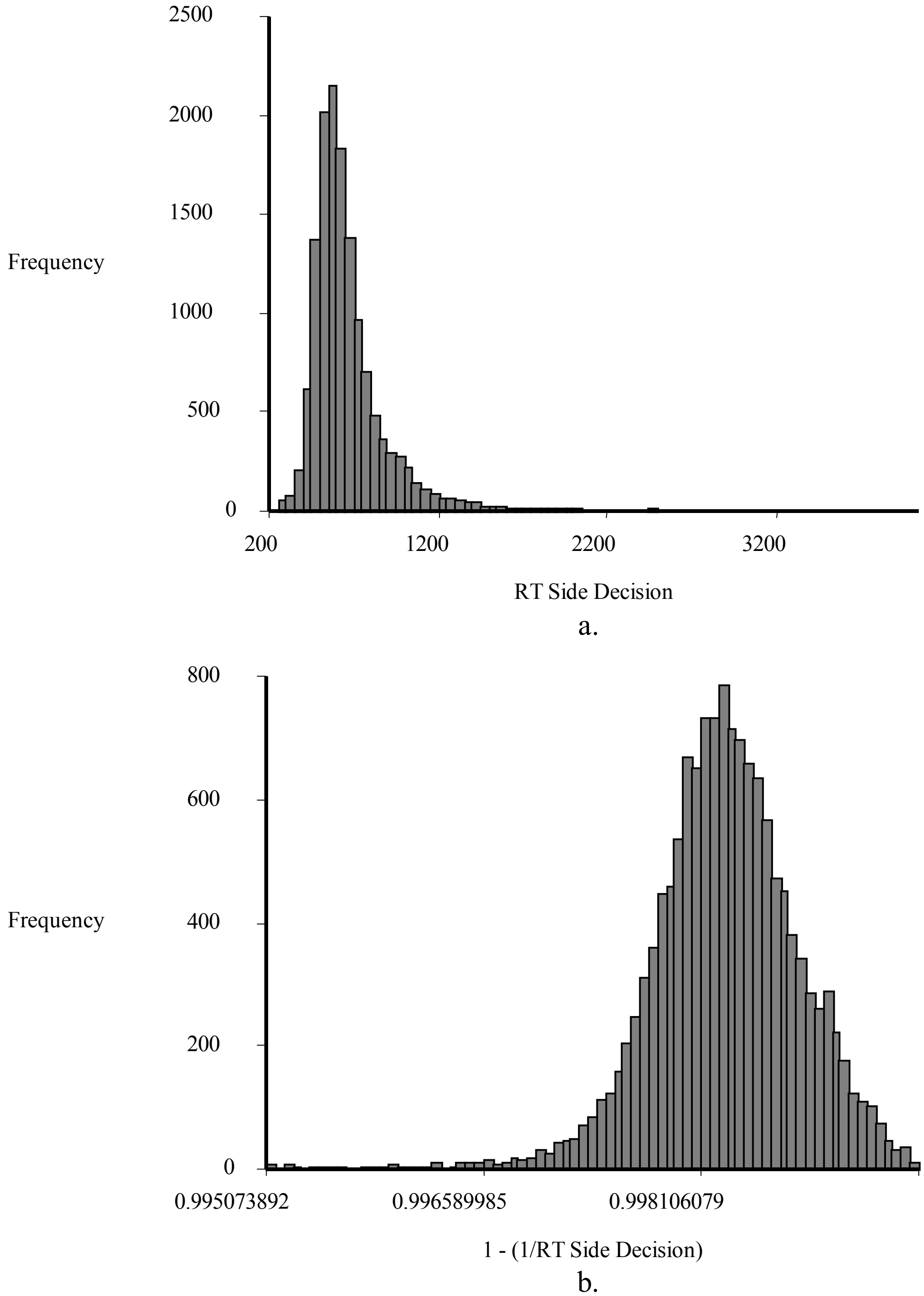


Figure 52. a. Histogram of responses to side decision task.
b. Histogram of transformed distribution of responses to side decision task.

As in Experiments 1 and 2, I calculated the average standardized RT and percent error for each interrupt proportion for both facilitation and inhibition trials separately for same and different data. Figure 53a shows the standardized RTs with standard errors for the same interrupt trials plotted as a function of interrupt proportion. The dotted line represents inhibition data and the solid line represents facilitation data. There are facilitation and inhibition effects. Specifically, the average RTs on the inhibition trials are longer than the average RTs on the facilitation trials and this discrepancy increases as interrupt proportion increases. Inhibition effects are also present in the percent error data (see Figure 53b). There are also facilitation and inhibition effects in the RT data (see Figure 53c) and percent error data (see Figure 49d) for the different interrupt trials.

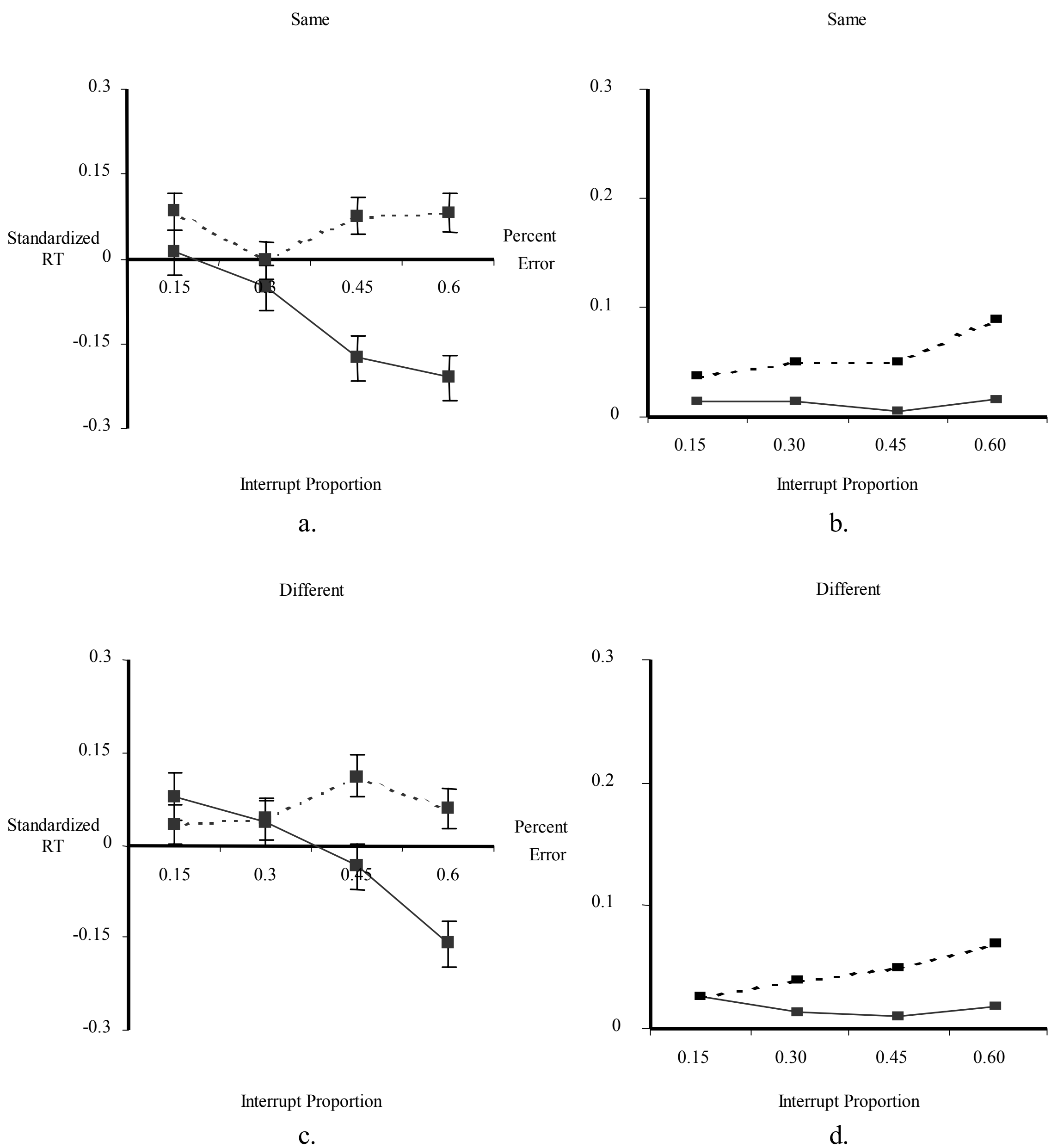


Figure 53. a. Same Standardized RTs as a function of interrupt proportion.
b. Same Percent Error as a function of interrupt proportion.
c. Different Standardized RTs as a function of interrupt proportion.
d. Different Percent Error as a function of interrupt proportion.

As noted previously, if participants must use “mental rotation” to complete a handedness decision task, they will know the handedness of stimuli with smaller angular disparities faster than that of stimuli with larger angular disparities. Thus, participants will know the handedness before being interrupted with the side decision on a greater proportion of trials with smaller angular disparities than with larger angular disparities. Therefore, if participants must use “mental rotation” to complete a handedness decision task, facilitation and inhibition effects will be larger for smaller angular disparities than larger angular disparities.

Figures 54 and 55 show average RTs with standard errors and percent error for the same data as a function of interrupt proportion separated by angular disparity. For the RT data, there is a large facilitation effect when the angular disparity is 0°. As predicted, this effect decreases as angular disparity increases. In fact, there are virtually no facilitation effects at 135° and 180°. There are no inhibition effects at any of the angular disparities. For the percent error data, there is a large effect of inhibition at 0°, which decreases as angular disparity increases. There are no facilitation effects in the percent error data at any of the angular disparities.

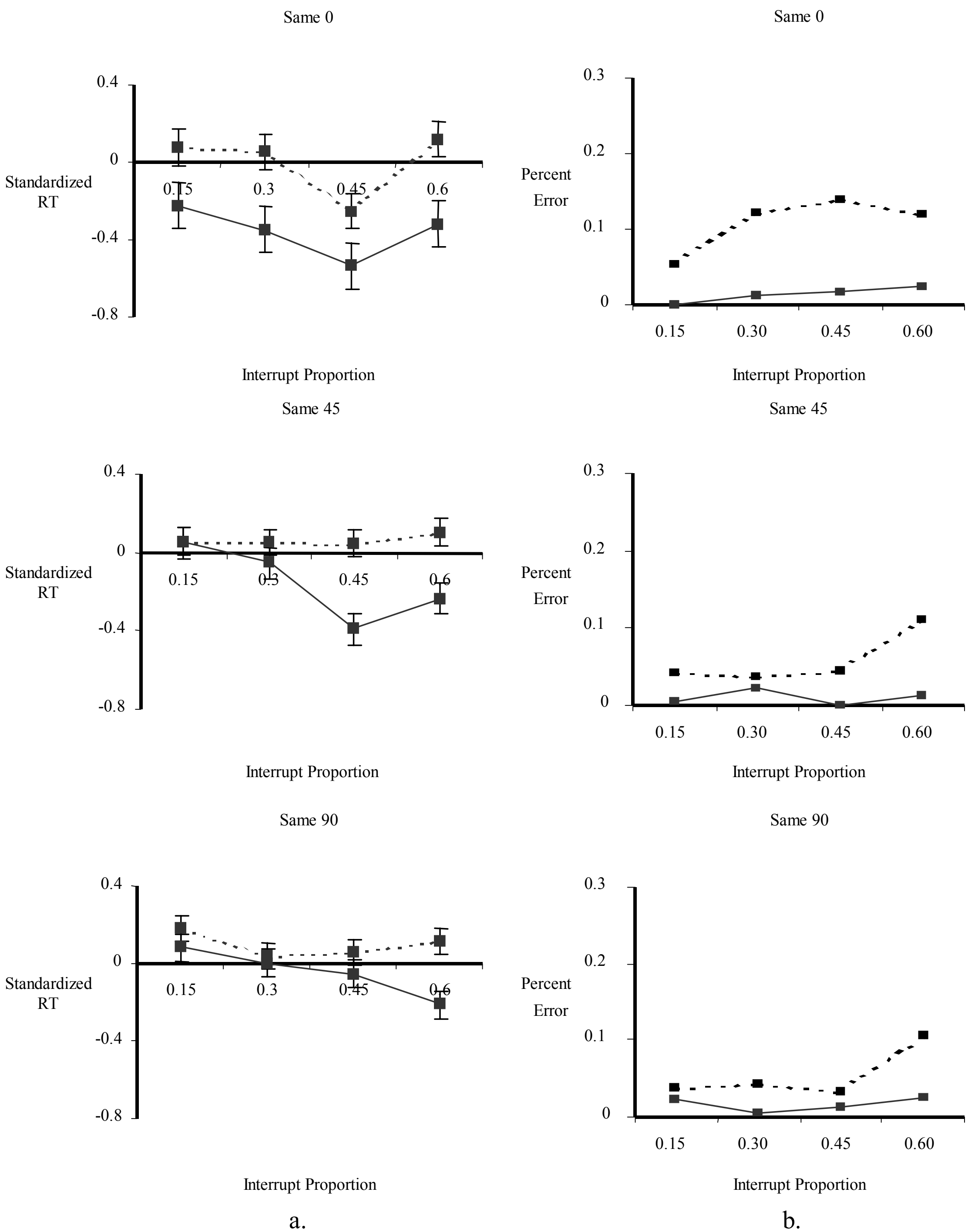


Figure 54. a. Same Standardized RTs separated by angular disparity (0° to 90°).
b. Same Percent Error separated by angular disparity (0° to 90°).

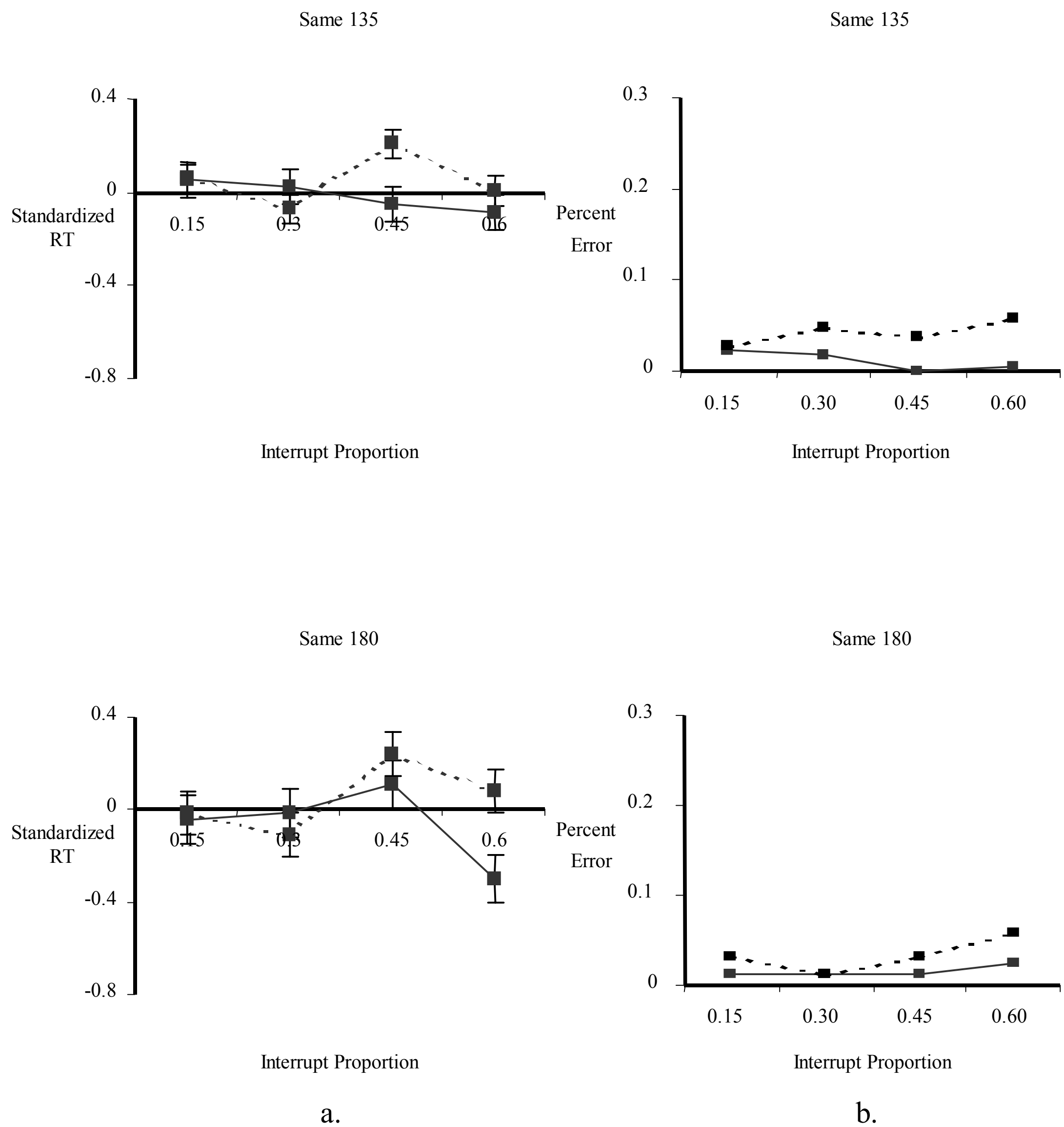


Figure 55. a. Same Standardized RTs (135° and 180°).
b. Same Percent Error (135° and 180°).

Figures 56 and 57 show average RTs with standard errors and percent error for the different data as a function of interrupt proportion separated by angular disparity. For the RT data there is a facilitation effect at 0°. There are no facilitation or inhibition effects at any other angular disparity. For the percent error data, there is large effect of inhibition at 0°, which decreases as angular disparity increases. There are no facilitation effects at any of the angular disparities. The average RTs and percent error from the same data suggest that participants must complete “mental rotation” before they know the handedness of misoriented stimuli. Although the different RT data does not show an effect of angular disparity, the effect is present in the error data. This suggests a possible speed accuracy tradeoff, which also indicates that participants did not know the handedness of the misoriented stimuli before using “mental rotation.”

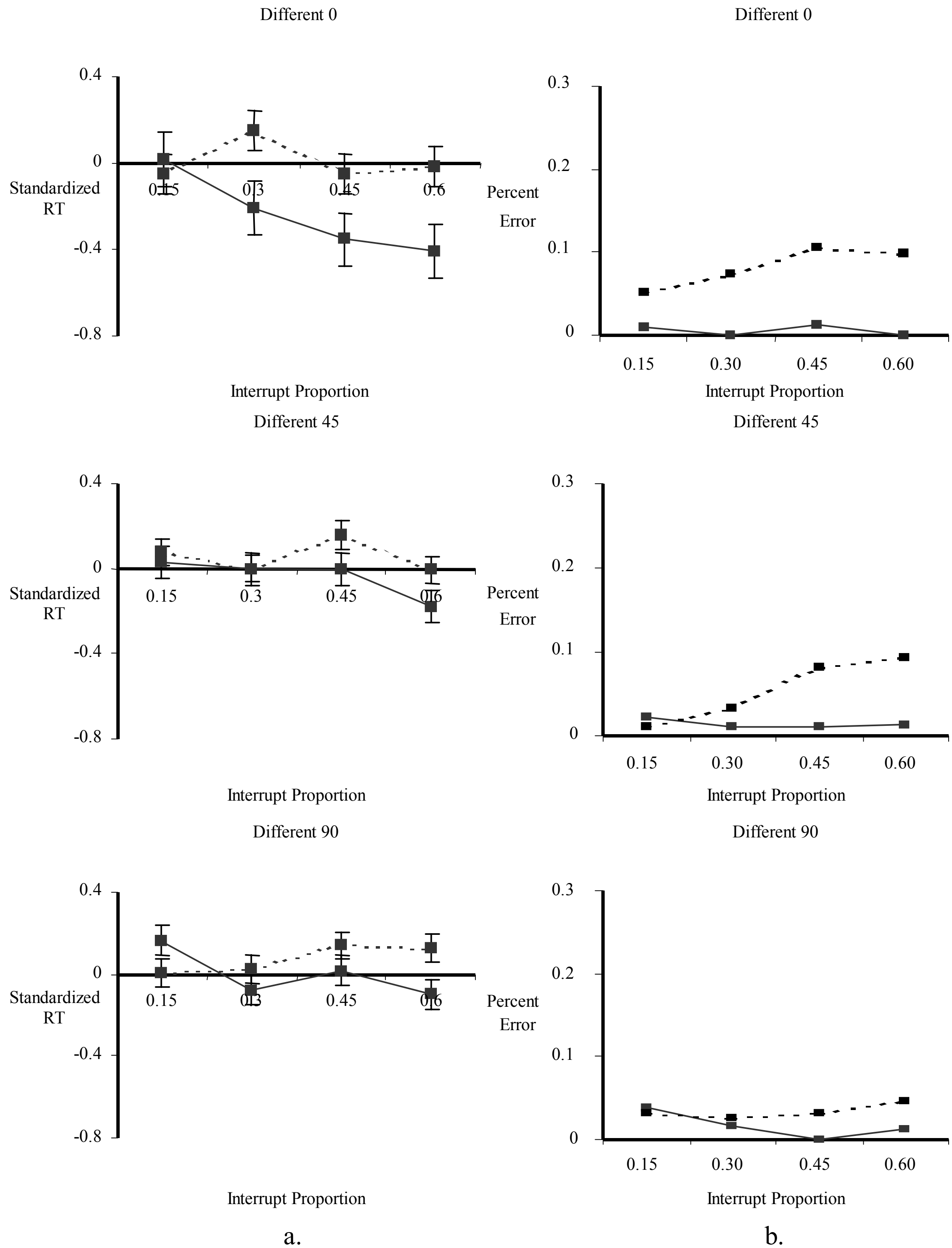
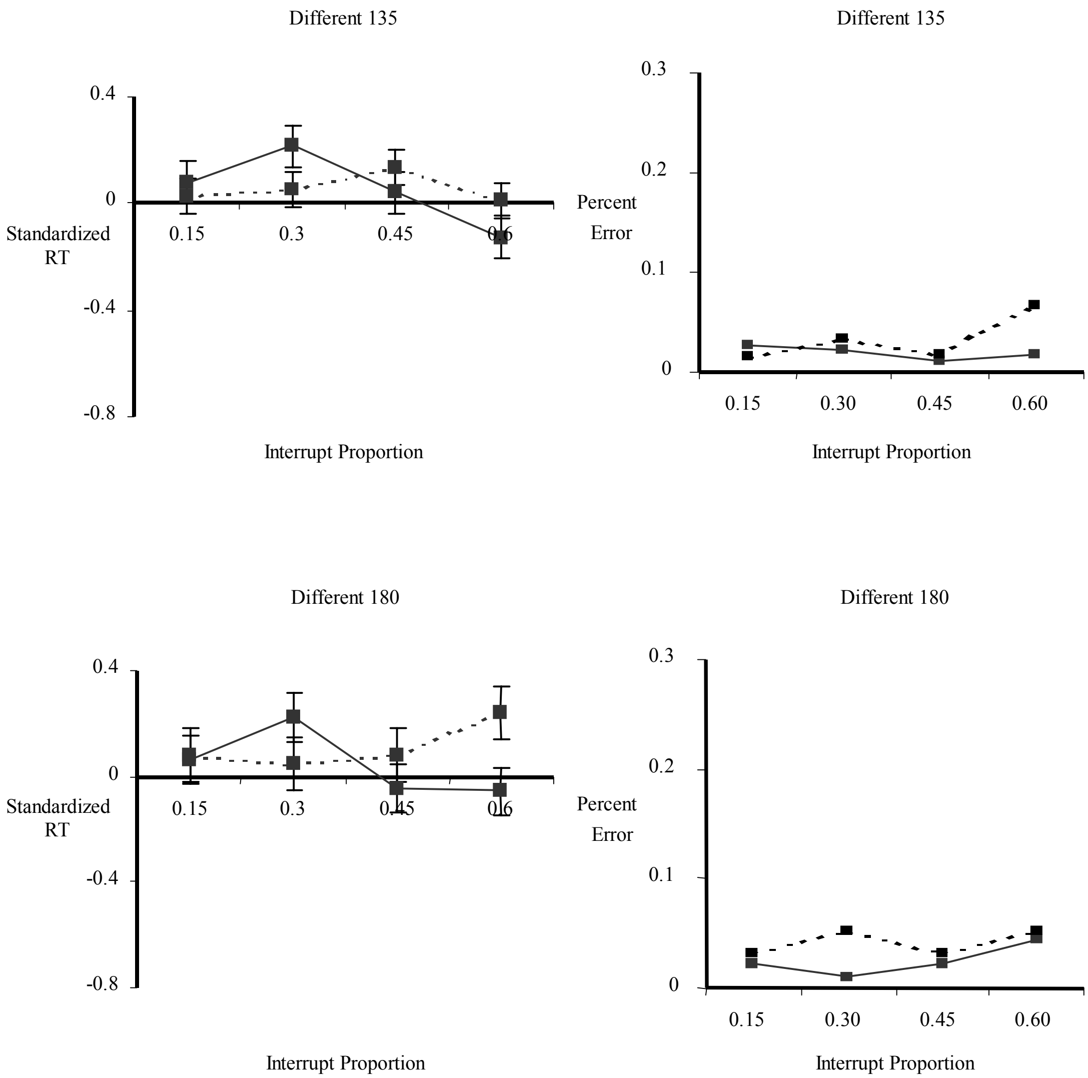


Figure 56. a. Different Standardized RTs separated by angular disparity (0° to 90°).
b. Different Percent Error separated by angular disparity (0° to 90°).



a. b.

Figure 57. a. Different Standardized RTs (135° and 180°).
b. Different Percent Error (135° and 180°).

I next calculated Cohen's *d* effect sizes for the facilitation and inhibition effects for both the RT data and the percent error data. Once again, the same and different data were analyzed separately. Recall that if participants must use "mental rotation" to know the handedness of the misoriented stimuli, one would expect larger effect sizes at smaller angular disparities.

Figure 58a is a plot of the effect sizes for the same RT data plotted as a function of angular disparity and separated by interrupt proportion. Once again, a separate line represents each interrupt proportion. Generally, the effect sizes increase as interrupt proportion increases. The .15 and .30 interrupt proportions seem to represent a floor where little facilitation or inhibition effects are observed. The .45 and .60 interrupt proportions seem to represent a ceiling where no more facilitation or inhibition effects are possible. Also the effect sizes decrease as angular disparity increases. These data imply that participants must use "mental rotation" to know the handedness of misoriented stimuli. I next fit log functions to each of the lines in this graph. The log functions are plotted in Figure 58b. The log function for the same data with the .15 interrupt proportion is $d_{.15} = -0.0451*\mathrm{Ln}(\alpha) + 0.2426$, $r^2 = .84$. The log function for .30 is $d_{.30} = -0.0656 *\mathrm{Ln}(\alpha) + 0.2875$, $r^2 = .95$. The log function for .45 is $d_{.45} = -0.0121*\mathrm{Ln}(\alpha) + 0.2119$, $r^2 = .89$. The log function for .60 is $d_{.60} = -0.0136*\mathrm{Ln}(\alpha) + 0.2556$, $r^2 = .13$. These functions indicate an effect of both angular disparity and interrupt proportion.

Figure 58c is a plot of the effect sizes for the same error data plotted as a function of angular disparity and separated by interrupt proportion. This pattern is very similar to that of the RT effect size data, in that effect size increases as interrupt proportion increases and decreases as angular disparity increases. I next fit log functions to each of these lines, and these functions are plotted in Figure 58d. The log function for the same data with the .15 interrupt proportion is $d_{.15} = -0.3084*\text{Ln}(\alpha) + 2.5031$, $r^2 = .51$. The log function for .30 is $d_{.30} = -0.4145 *\text{Ln}(\alpha) + 3.1483$, $r^2 = .51$. The log function for .45 is $d_{.45} = -0.2569*\text{Ln}(\alpha) + 3.1385$, $r^2 = .35$. The log function for .60 is $d_{.60} = 0.0622*\text{Ln}(\alpha) + 2.2776$, $r^2 = .02$. These functions indicate an effect of both angular disparity and interrupt proportion.

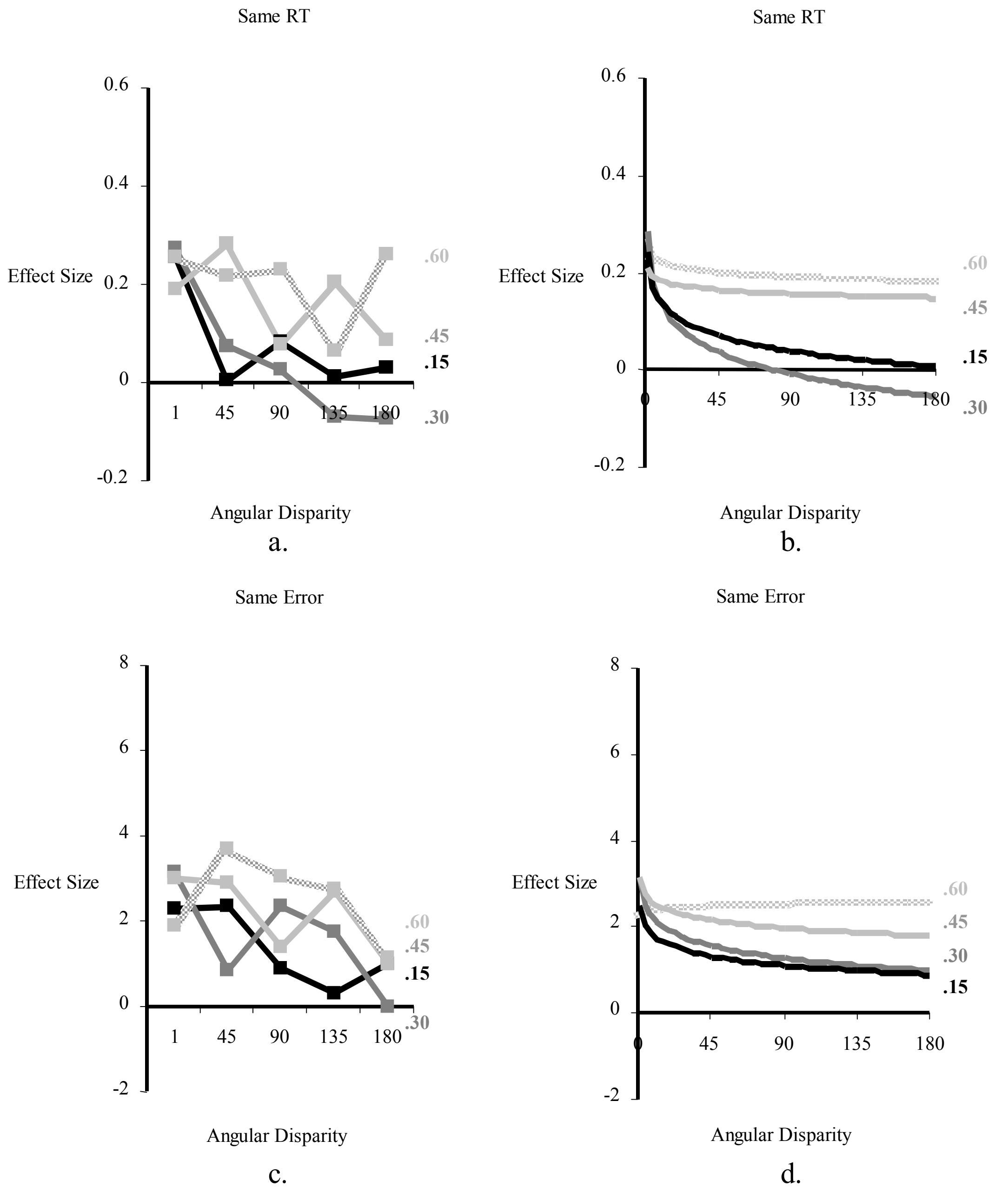


Figure 58. a. Effect Sizes for Same RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Same RTs.
c. Effect Sizes for Same Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Same Error.

Figure 59a is a plot of the effect sizes for the different RT data plotted as a function of angular disparity. There is an effect of interrupt proportion in that as interrupt proportion increases, so do effect sizes. There also seems to be an effect of angular disparity. The log functions for these data are plotted in Figure 59b. The log function for the different data with the .15 interrupt proportion is $d_{.15} = 0.0028*\text{Ln}(\alpha) - 0.0406$, $r^2 = .01$. The log function for .30 is $d_{.30} = -0.0641 *\text{Ln}(\alpha) + 0.2427$, $r^2 = .78$. The log function for .45 is $d_{.45} = -0.0239*\text{Ln}(\alpha) + 0.2064$, $r^2 = .95$. The log function for .60 is $d_{.60} = -0.024*\text{Ln}(\alpha) + 0.256$, $r^2 = .50$. These functions indicate an effect of both angular disparity and interrupt proportion.

Figure 59c is a plot of the effect sizes for the different error data plotted as a function of angular disparity and separated by interrupt proportion. There is an effect of interrupt proportion in that effect size increases as interrupt proportion increases. There is also an effect of angular disparity in that effect size decreases as angular disparity increases. Figure 59d is a plot of the log functions to these data. The log function for the different data with the .15 interrupt proportion is $d_{.15} = -0.3632*\text{Ln}(\alpha) + 1.3978$, $r^2 = .58$. The log function for .30 is $d_{.30} = -0.3405 *\text{Ln}(\alpha) + 2.6748$, $r^2 = .70$. The log function for .45 is $d_{.45} = -0.3272*\text{Ln}(\alpha) + 3.0528$, $r^2 = .27$. The log function for .60 is $d_{.60} = -0.2416*\text{Ln}(\alpha) + 2.9736$, $r^2 = .19$. These functions indicate an effect of both angular disparity and interrupt proportion.

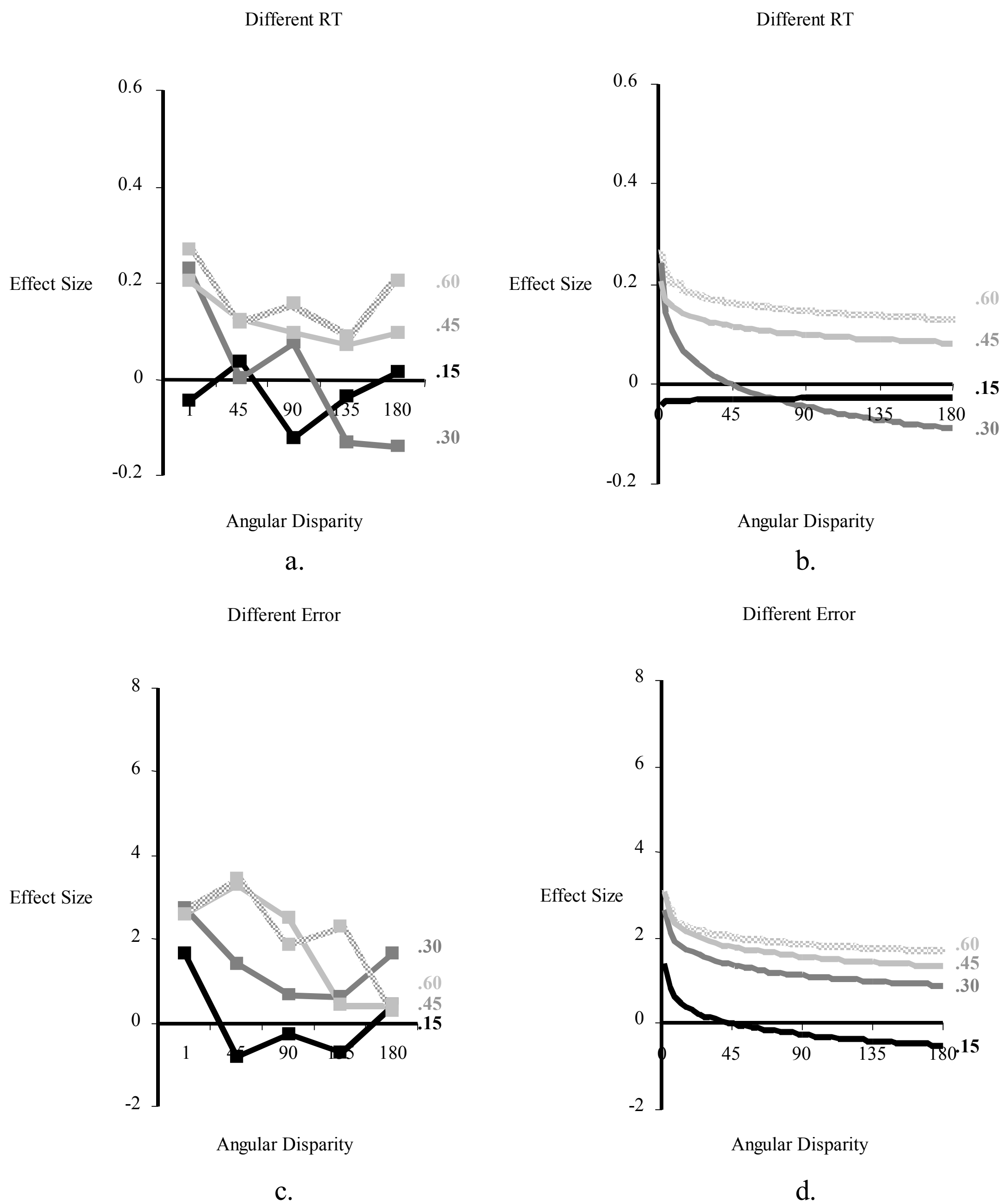


Figure 59. a. Effect Sizes for Different RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Different RTs.
c. Effect Sizes for Different Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Different Error.

To clarify the relationship between effect sizes and interrupt proportion, I calculated effect sizes for both RT and percent error averaged across angular disparity for each interrupt proportion. Again, these averages were calculated separately for same and different data. The effect of interrupt proportion is evidenced by the fact that average effect sizes increase as interrupt proportion increases (See Figure 60). I will not report statistics on these functions because there were only four interrupt proportions.

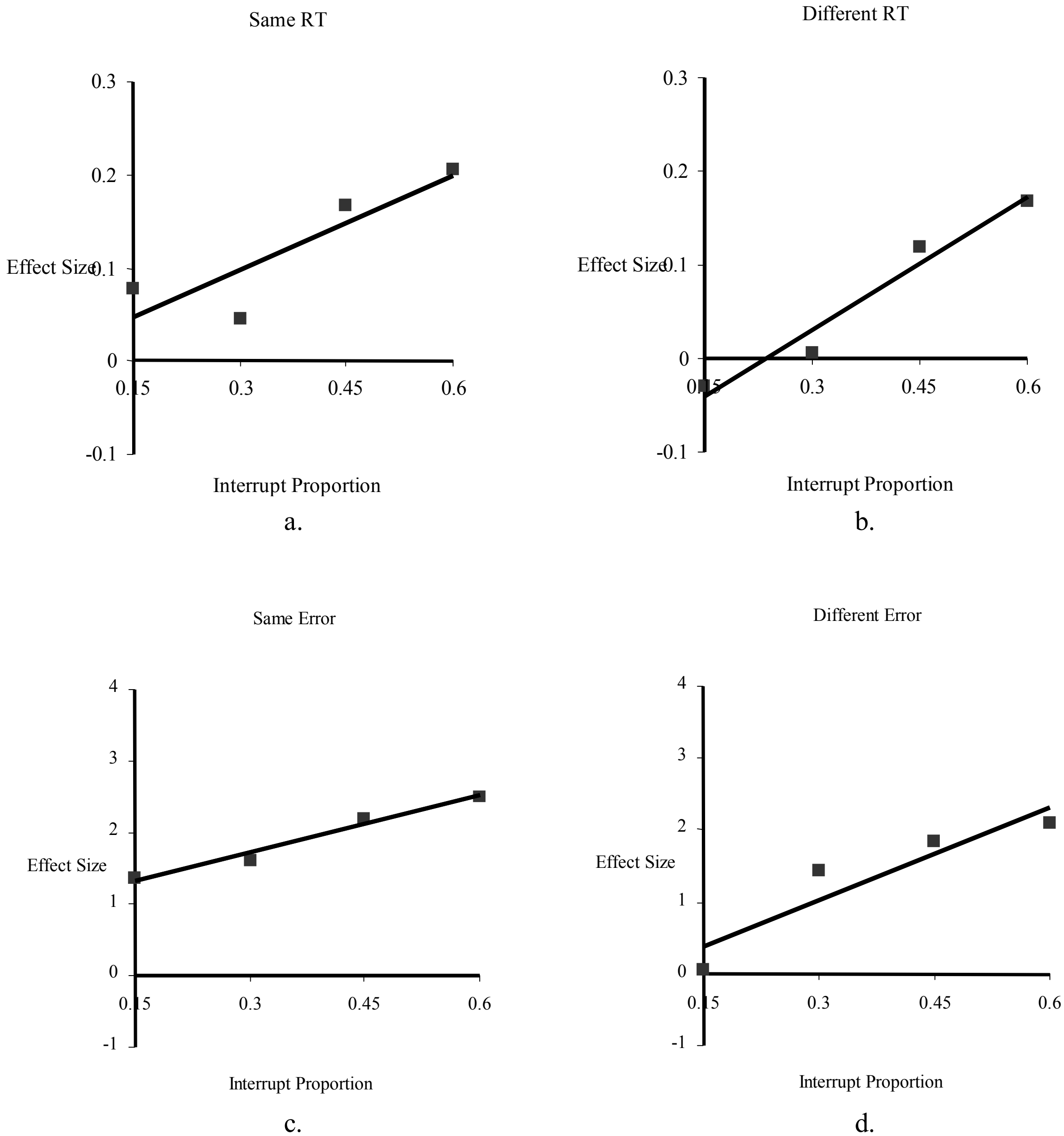
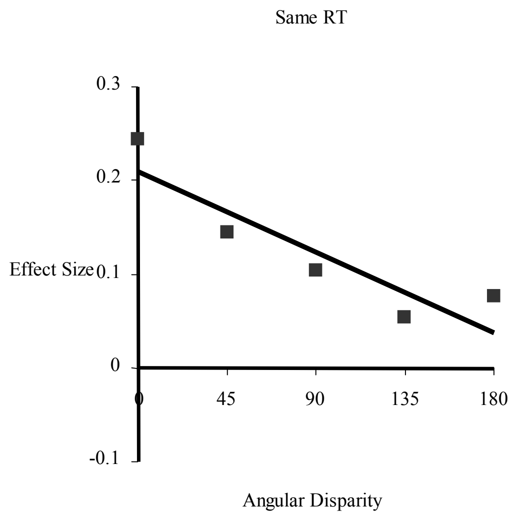


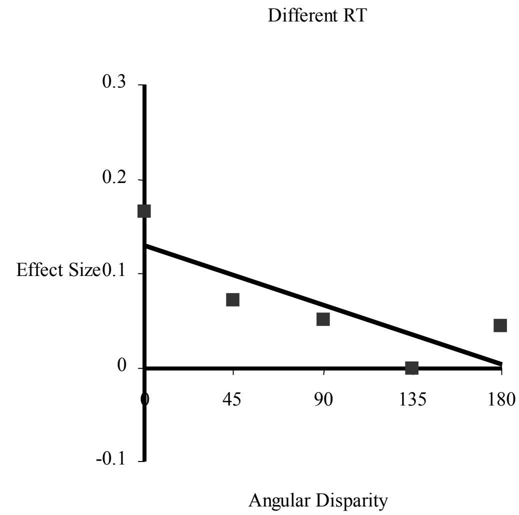
Figure 60. a. Average Effect Sizes for Same RTs plotted by interrupt proportion.
b. Average Effect Sizes for Different RTs plotted by interrupt proportion.
c. Average Effect Sizes for Same Errors plotted by interrupt proportion.
d. Average Effect Sizes for Different Errors plotted by interrupt proportion.

To clarify the relationship between effect sizes and angular disparity, I calculated effect sizes for both RT and percent error averaged across interrupt proportion for each angular disparity. Again, these averages were calculated separately for same and different data. For the same RT data, there was a significant relationship between angular disparity and effect size, effect size = 0.2093 - 0.00095*(angular disparity), F (1, 3) = 12.946, p = .0368, r^2 = .8119. Both the intercept (t = 7.2016) and the slope (t = 3.598) were significant. For the different RT data, there was no relationship between angular disparity and effect size, effect size = 0.1285 - 0.0007*(angular disparity), F (1, 3) = 5.685, p = .0972, r^2 = .6546. However, this relationship approached significance. The intercept was significant (t = 3.9833), however the slope was not (t = 2.3843). For the same error data, there was a significant relationship between angular disparity and effect size, effect size = 2.7575 - 0.00928*(angular disparity), F (1, 3) = 19.428, p = .0217, r^2 = .8662. Both the intercept (t = 11.8851) and the slope (t = 4.4077) were significant. For the different error data, there was a significant relationship between angular disparity and effect size, effect size = 2.2683 – 0.01016*(angular disparity), F (1, 3) = 39.3321, p = .0082, r^2 = .9291. Both the intercept (t = 12.7) and the slope (t = 6.2715) were significant.

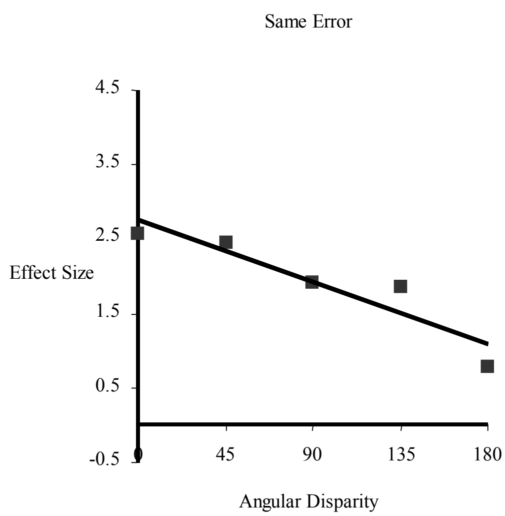
The effects of angular disparity are evidenced by negative relationships between average effect size and angular disparity (See Figure 61). As mentioned previously there was a significant, decreasing relationship between effect size and angular disparity in all these data except the different RT data (which approached significance).



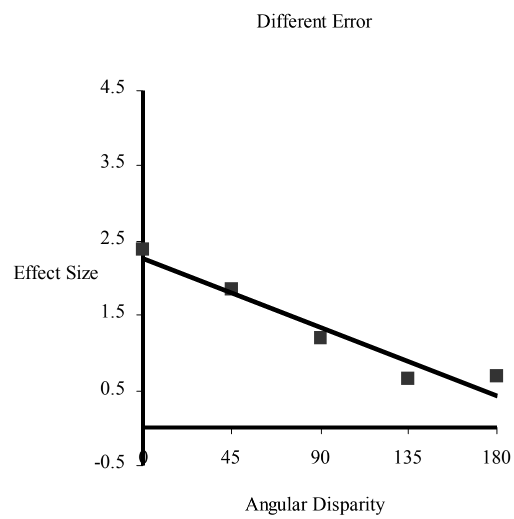
a.



b.



c.



d.

Figure 61. a. Average Effect Sizes for Same RTs plotted by angular disparity.
b. Average Effect Sizes for Different RTs plotted by angular disparity.
c. Average Effect Sizes for Same Errors plotted by angular disparity.
d. Average Effect Sizes for Different Errors plotted by angular disparity.

In summary, there were effects of both interrupt proportion and angular disparity for the same data. Both effects were present in the RT and the percent error analyses. For the different data, there was an effect of interrupt proportion and angular disparity in the error analysis. Although, the effect of angular disparity in the different RT data was not significant, there was a decreasing trend between effect size and angular disparity. These results indicate that participants must use “mental rotation” to know the handedness of misoriented stimuli.

Discussion

Experiment 3 was a replication of Experiment 1. I first determined that the insertion of the side decision task did not change the way participants completed the handedness decision task. Next, I concluded that responses occurring within 200 ms of the interrupt time were actually responses to the handedness decision task. Finally, I concluded that there were effects of both interrupt proportion and angular disparity. Facilitation and inhibition effects increased as interrupt proportion increased and decreased as angular disparity increased. These findings indicate that participants did not know the handedness of misoriented stimuli before using “mental rotation.”

Experiments 1 and 3 showed that the dualtask procedure and analysis can detect effects of both interrupt proportion and angular disparity. I predicted an effect of angular disparity because I presented participants with a different pair of stimuli on each trial. Experiment 4 was a replication of Experiment 2 with more participants and more interrupt proportions. Experiment 4 was designed to test whether participants must use “mental rotation” to know the handedness of misoriented stimuli when they are only presented with two pairs of stimuli throughout the experiment.

EXPERIMENT 4

Experiment 3 replicated the findings of Experiment 1. Specifically, there was both an effect of interrupt proportion and angular disparity. As interrupt proportion increased, facilitation and inhibition effects increased. As angular disparity increased, facilitation and inhibition effects decreased. These effects were present in both the RT data and the error data. These results were expected because a different pair of stimuli was presented on each trial. Experiment 4 was a replication of Experiment 2 with more participants and more interrupt proportions.

Method

Participants

137 undergraduate university students volunteered to participate in this experiment. Data from eleven of these participants was not used in the present analyses, because their error rates on the non-interrupt trials was greater than 25 percent. This indicated that these participants did not understand the handedness decision task.

Stimuli and Apparatus

The stimuli and apparatus were the same as in Experiment 3 with the exception that only two standards were created for each participant. Therefore, participants were only presented with two sets of stimuli throughout the experiment. As in Experiment 2, the stimuli were presented in alternating fashion from trial to trial. This was done to ensure that participants did not “rotate” the probe to the same orientation as the probe from the previous trial.

Design and Procedure

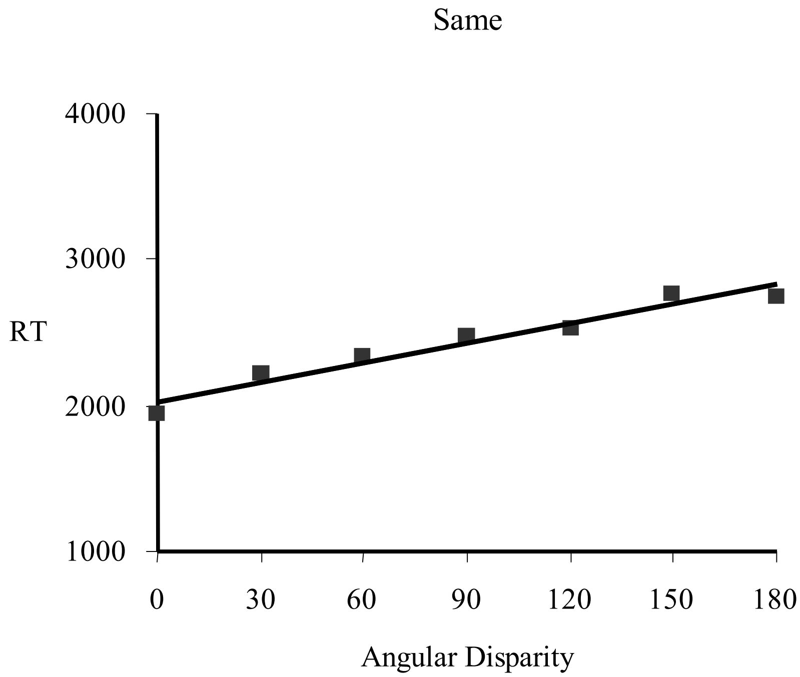
The experimental design and procedures were identical to those of Experiment 3.

Results

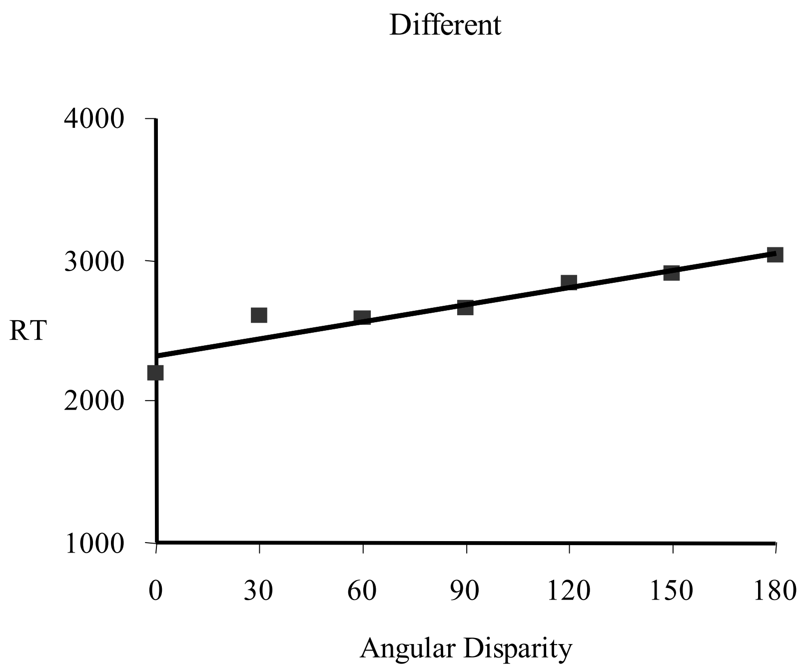
The same three stages of data analysis were used in this experiment. I had to (1) verify that inserting the side decision task did not affect how participants performed the handedness task, (2) determine which responses were to the side decision task, and (3) investigate facilitation and inhibition effects. Once again, all inferential analyses were conducted with an alpha level of .05. For all RT analyses, I only used trials in which the participant responded correctly. For the practice analysis, I excluded all trials that exceeded 10000 ms (4.79 SD from the mean, 2% of the trials). For the non-interrupt analysis, I also excluded all trials that exceeded 10000 ms (7.83 SD from the mean, 0.2% of the trials). For the side decision task analyses, I excluded trials with RTs exceeding 5000 ms (40.32 SD from the mean, 0.04 % of the trials).

Comparison of Practice and Non-Interrupt Trials

As in the previous experiments, the average RT for each orientation was calculated for both the practice and non-interrupt trials and collapsed around 180°. Once again, the same and different data were analyzed separately. Figure 62a shows the average RT of the same data from the practice trials plotted as a function of angular disparity, and Figure 62b shows the average RT of the different data from the practice trials plotted as a function of angular disparity. Figure 63a shows the average RT of the same data from the non-interrupt trials plotted as a function of angular disparity, and Figure 63b shows the average RT of the different data from the non-interrupt trials plotted as a function of angular disparity.

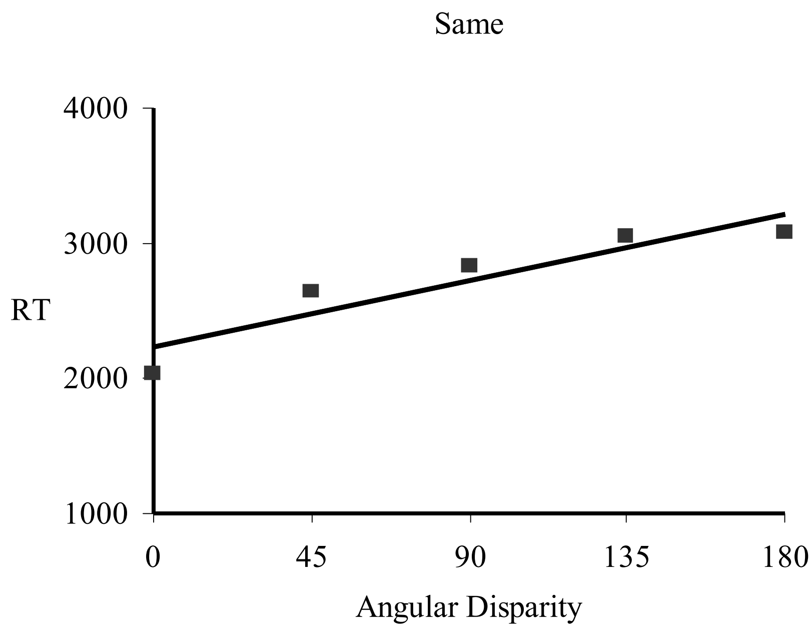


a.

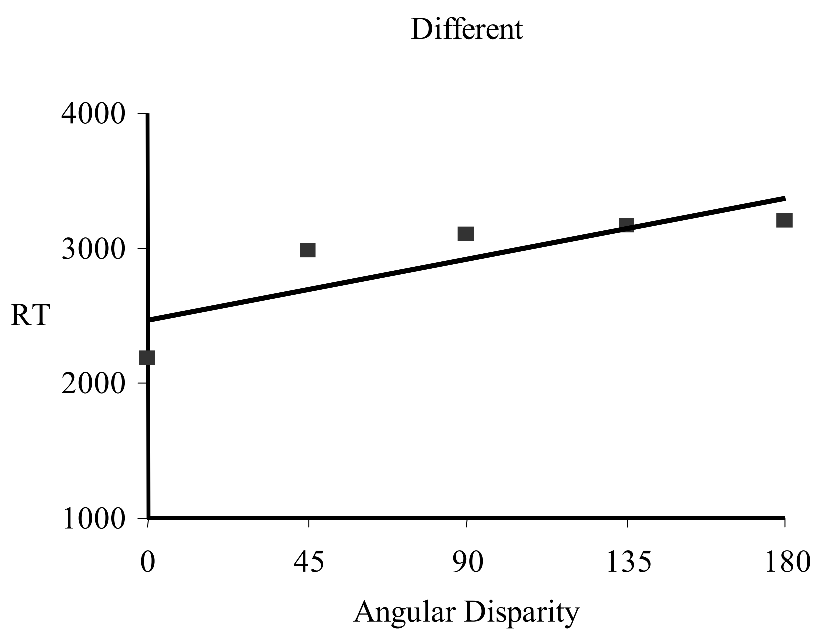


b.

Figure 62. a. Average RT for same practice trials plotted as a function of angular disparity.
b. Average RT for different practice trials plotted as a function of angular disparity.



a.



b.

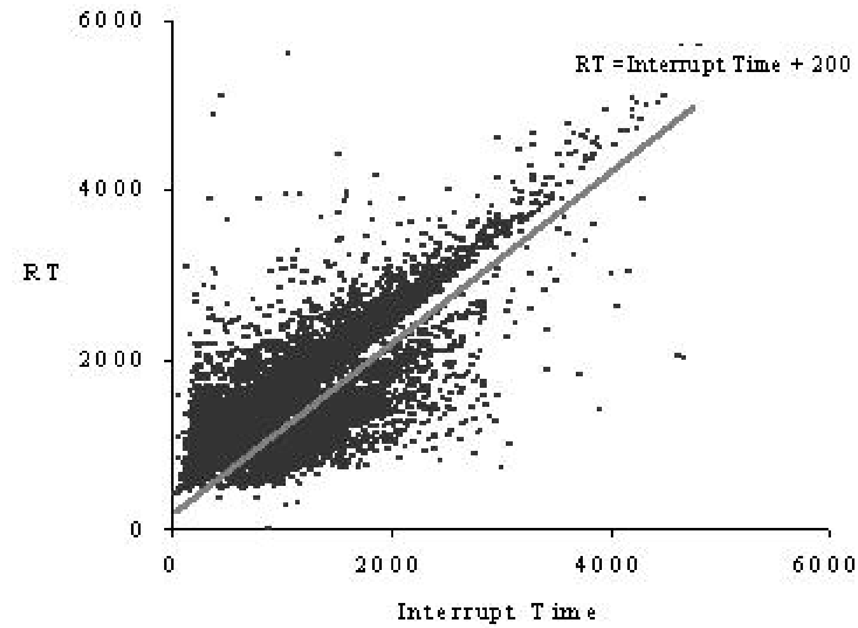
Figure 63. a. Average RT for same non-interrupt trials plotted as a function of angular disparity.
b. Average RT for different non-interrupt trials plotted as a function of angular disparity.

For the same practice trials, there was a significant relation between angular disparity and RT, RT = 2034.702 + 4.4*(angular disparity), F (1, 5) = 95.81, $p < .001$, $r^2 = .95$. Both the intercept (t = 41.844) and the slope (t = 9.79) were significant. For the same non-interrupt trials there was also a significant relation between angular disparity and RT, RT = 1573.894 + 2.79*(angular disparity), F (1, 3) = 141.046, $p = .001$, $r^2 = .98$. Once again, both the intercept (t = 66.746) and the slope (t = 11.88) were significant. The intercept and the slope of the practice trials were significantly greater than those of the non-interrupt trials, t (8) = 8.36, $p < .05$ for the intercepts, and t (8) = 3.15, $p < .05$ for the slopes. Although these differences were significant, both of the slopes are greater than the LRC indicating that participants were using "mental rotation" to complete the task. The difference between the practice trials and the non-interrupt trials is likely due to the fact that participants had more practice with the same stimuli when they completed the non-interrupt trials. Therefore, I concluded that inserting the interrupt task did not affect how participants performed the handedness decision task.

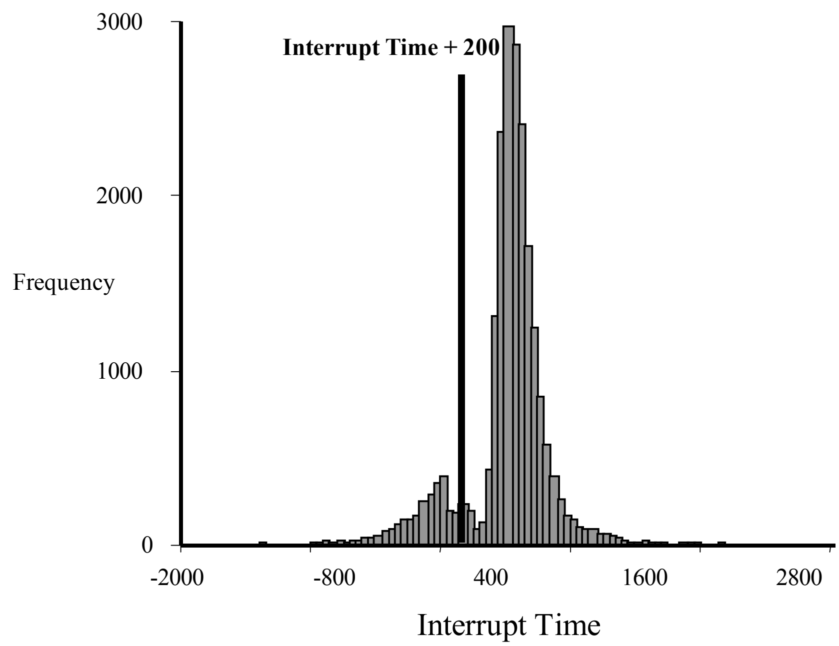
For the different practice trials, there was a significant relation between angular disparity and RT, RT = 2328.853 + 3.992*(angular disparity), F (1,5) = 48.15, $p < .001$, $r^2 = .91$. Both the intercept (t = 62.228) and the slope (t = 6.939) were significant. For the different non-interrupt trials there was also a significant relation between angular disparity and RT, RT = 1769.423 + 2.536*(angular disparity), F (1,3) = 39.045, $p = .008$, $r^2 = .93$. Again, both the intercept (t = 39.55) and the slope (t = 6.249) were significant. The intercept of the practice trials was significantly greater than the intercept of the non-interrupt trials, t (8) = 7.3, $p < .05$, however there was no significant difference between the slopes of the practice and non-interrupt trials, t (8) = 1.93, $p > .05$. These findings indicate that inserting the interrupt task did not affect how participants performed the handedness decision task.

Determining the Responses to Side Decision Task

As in the previous experiments, the correct interrupt data are bimodal. When RT for the correct interrupt trials is plotted as a function of interrupt time there is a well-defined gap in the data. Once again, this gap falls along the line in which RT equals the interrupt time plus 200 ms (see Figure 64a). This pattern can also be observed in the histogram of responses for the interrupt trials (see Figure 64b). This gap suggests that responses occurring within 200 ms of the interrupt time represent a distribution of responses to the handedness decision task that were initiated before the interruption, and responses occurring after 200 ms of the interrupt time represent the distribution of responses to the side decision task.



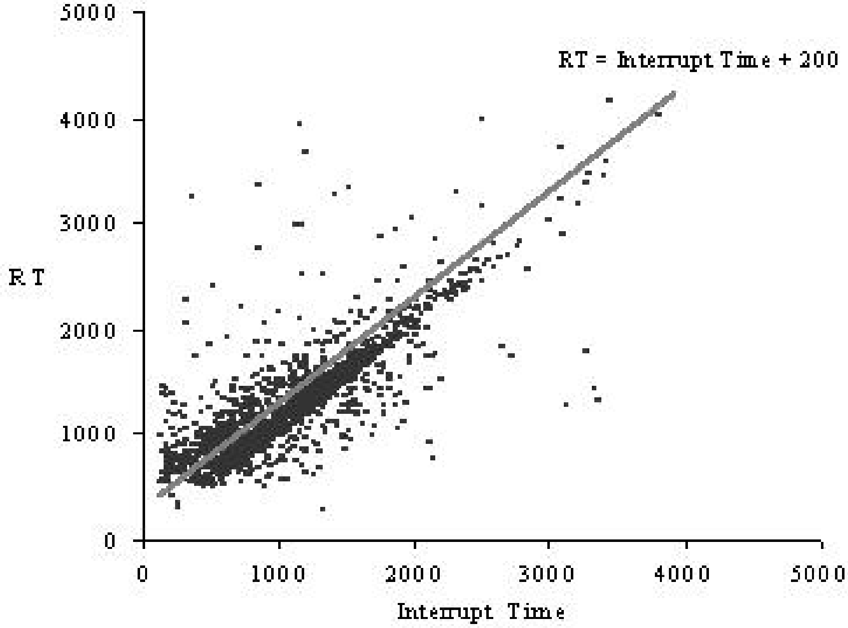
a.



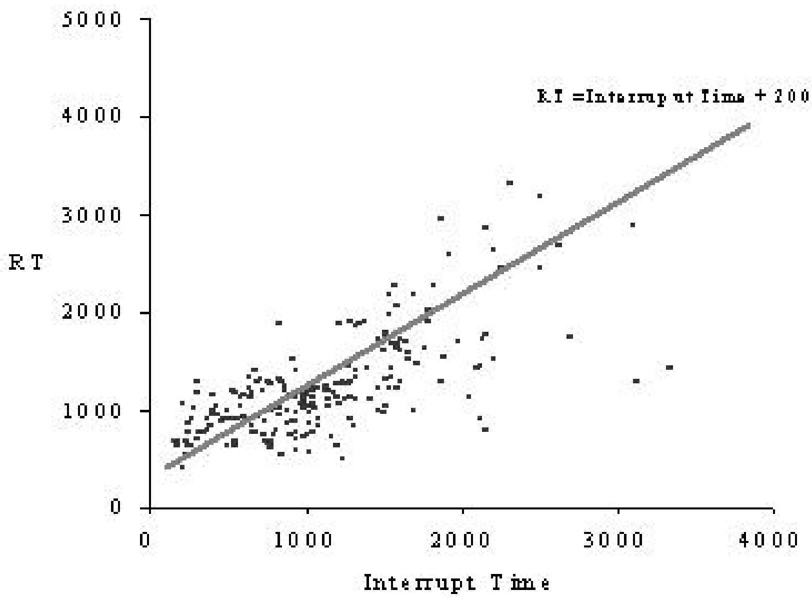
b.

Figure 64. a. RT plotted as a function of interrupt time.
b. Histogram of responses to interrupt trials.

As in the previous experiments, when RT for incorrect inhibition trials is plotted as a function of interrupt time, most of the errors occurred below the line upon which RT is equal to the interrupt time plus 200 ms (see Figure 65a). However, when RT for incorrect facilitation trials is plotted as a function of interrupt time, there are an almost equal number of errors on either side of this line (see Figure 65b). Furthermore, of the responses occurring within 200 ms of the interruption, 94% of the inhibition trials are incorrect, while only 7% of the facilitation trials are incorrect. These data provide further support that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task and responses occurring after 200 ms of the interrupt time were responses to the side decision task.



a.



b.

Figure 65. a. Incorrect inhibition trials plotted as a function of interrupt time
b. Incorrect facilitation trials plotted as a function of interrupt time

I next ran the maximum-likelihood analysis to confirm that the responses occurring within 200 ms of the interrupt time were responses to the handedness decision task. I ran regression analyses on the means for the same data at each orientation estimated by the maximum-likelihood model and compared them to the non-interrupt data. The maximum likelihood model was unable to converge on a prediction for the different data. As in Experiment 3, this failure is likely due to too much noise in the different data. For the same predicted distribution, there was a significant relation between angular disparity and RT, RT = 1795.919 + 3.91*(angular disparity), F (1, 3) = 69.082, p = .003, r^2 = .96. Both the intercept (t = 34.64) and the slope (t = 8.31) were significant. The intercept of the predicted distribution is significantly larger than that of the non-interrupt data, t (8) = 3.83, $p < .05$. However, there is no significant difference between the slopes of the predicted and non-interrupt distributions, t (8) = 2.1, $p > .05$.

The estimated distribution and the same non-interrupt data are plotted in Figure 66. The estimated and non-interrupt distributions are relatively close to each other. Furthermore, there were no significant differences between the slopes of these distributions. Therefore, I concluded that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task, and they were excluded from the investigation of facilitation and inhibition effects.

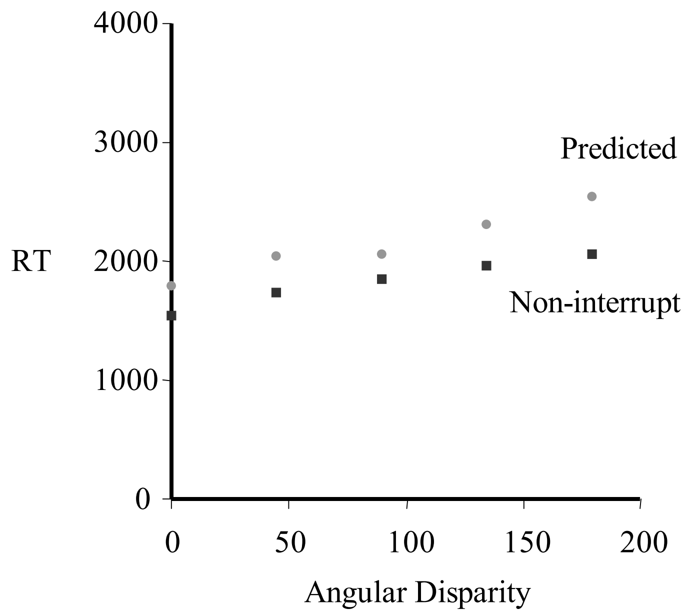
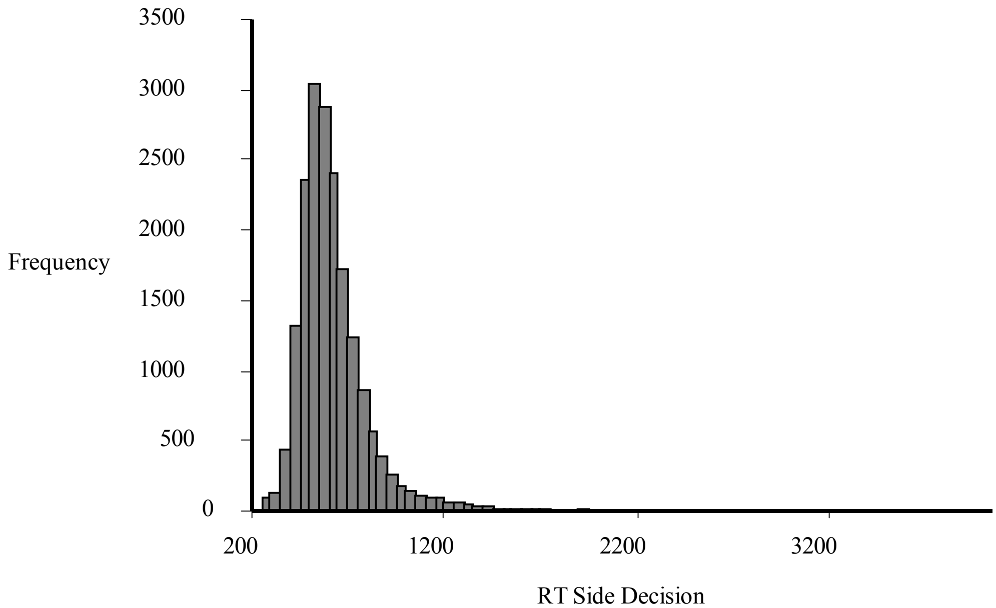


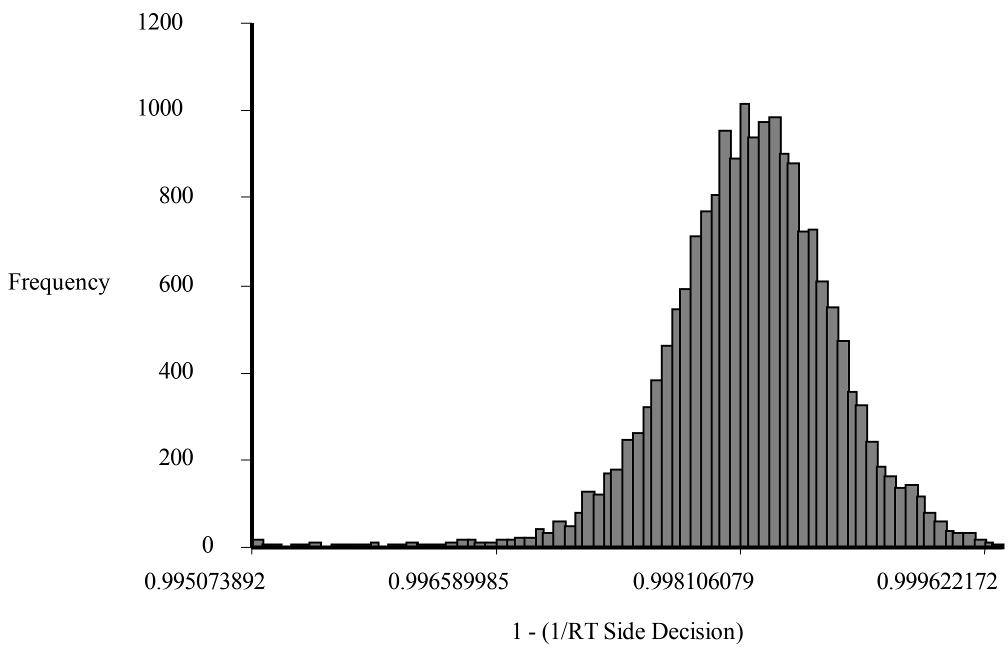
Figure 66. Predicted and non-interrupt data from the same trials plotted as a function of angular disparity.

Assessing Facilitation and Inhibition Effects

The final step in the data analysis procedure was to assess the responses to the side decision task for facilitation and inhibition effects. Figure 67a shows a histogram of the distribution of RTs to the side decision task. Again, this distribution is positively skewed. To normalize the data, I calculated the reciprocal for each of these values and subtracted that score from one. The distribution of transformed data is presented in Figure 67b. To minimize between subject variability, each participants' transformed RTs were standardized.



a.



b.

Figure 67. a. Histogram of responses to side decision task.
b. Histogram of transformed distribution of responses to side decision task.

I next calculated the average standardized RT and percent error for each interrupt proportion for both facilitation and inhibition trials separately for the same and different data. Figure 68a shows the standardized RTs with standard errors for the same interrupt trials plotted as a function of interrupt proportion. Again, the dotted line represents inhibition data and the solid line represents facilitation data. Once again, the average RTs on the inhibition trials are longer than the average RTs on the facilitation trials. Facilitation effects are evidenced by a decrease in average RTs on the facilitation trials as interrupt proportion increases. However, there are no inhibition effects in the same RT data. Inhibition effects are present in the percent error data (see Figure 68b). A similar pattern is observed in the different trials. Specifically, there are facilitation effects in the RT data (see Figure 68c) and inhibition effects in the percent error data (see Figure 68d).

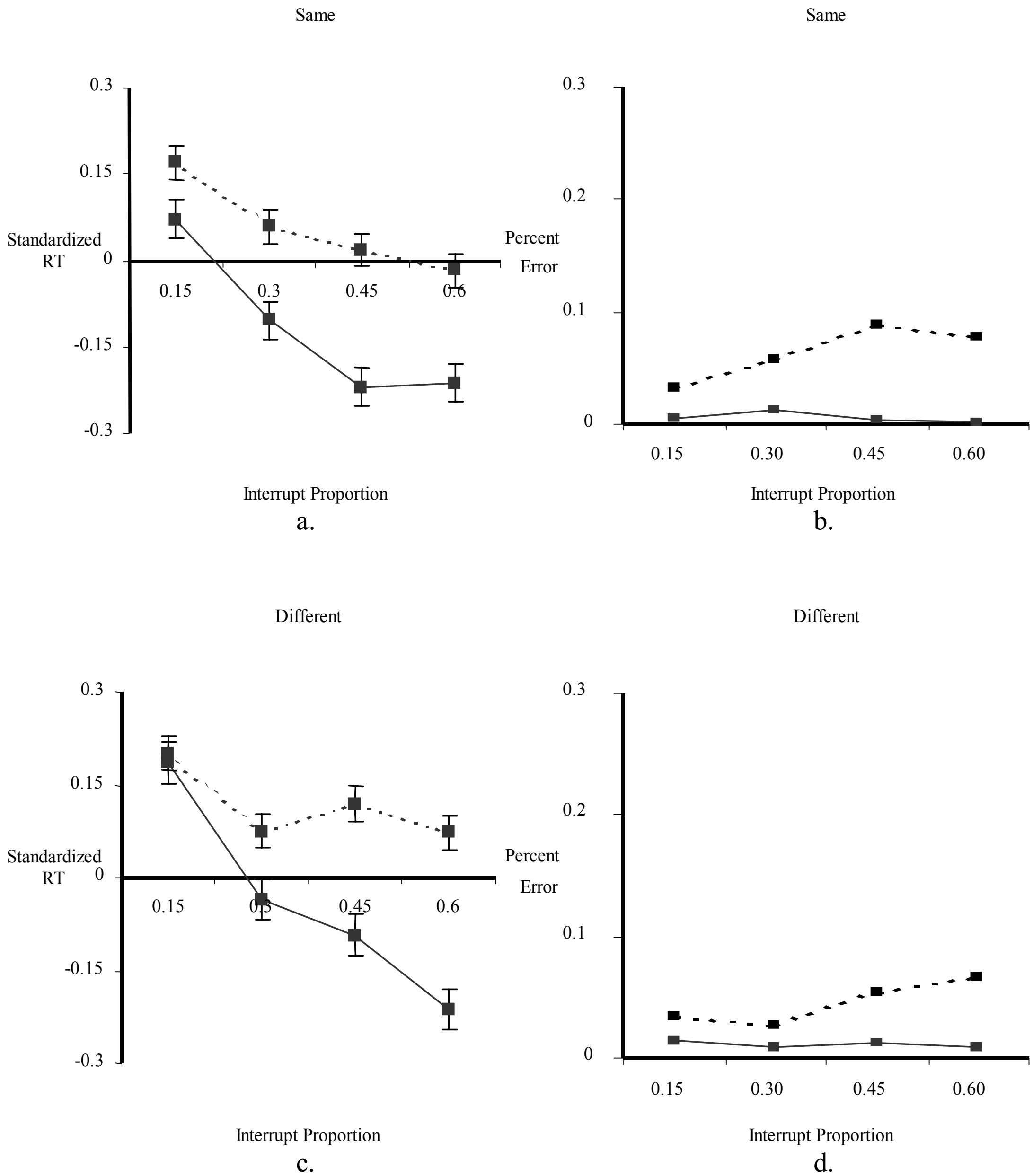


Figure 68. a. Same Standardized RTs as a function of interrupt proportion.
b. Same Percent Error as a function of interrupt proportion.
c. Different Standardized RTs as a function of interrupt proportion.
d. Different Percent Error as a function of interrupt proportion.

As noted previously, if participants must use “mental rotation” to determine the handedness of misoriented stimuli, facilitation and inhibition effects will be larger for smaller angular disparities than larger angular disparities. However, if participants do not have to complete “mental rotation” to determine the handedness of misoriented stimuli, the facilitation and inhibition effects should not be a function of angular disparity. Figures 69 and 70 show average RTs with standard errors and percent error for the same data as a function of interrupt proportion separated by angular disparity. For the RT data, there are facilitation effects for each angular disparity except 180°. There are no inhibition effects at any of the angular disparities. However, there are large inhibition effects at each angular disparity for the percent error data. Again, because the facilitation and inhibition effects are not dependent on angular disparity, it seems that participants know the handedness of the misoriented stimuli before using “mental rotation.”

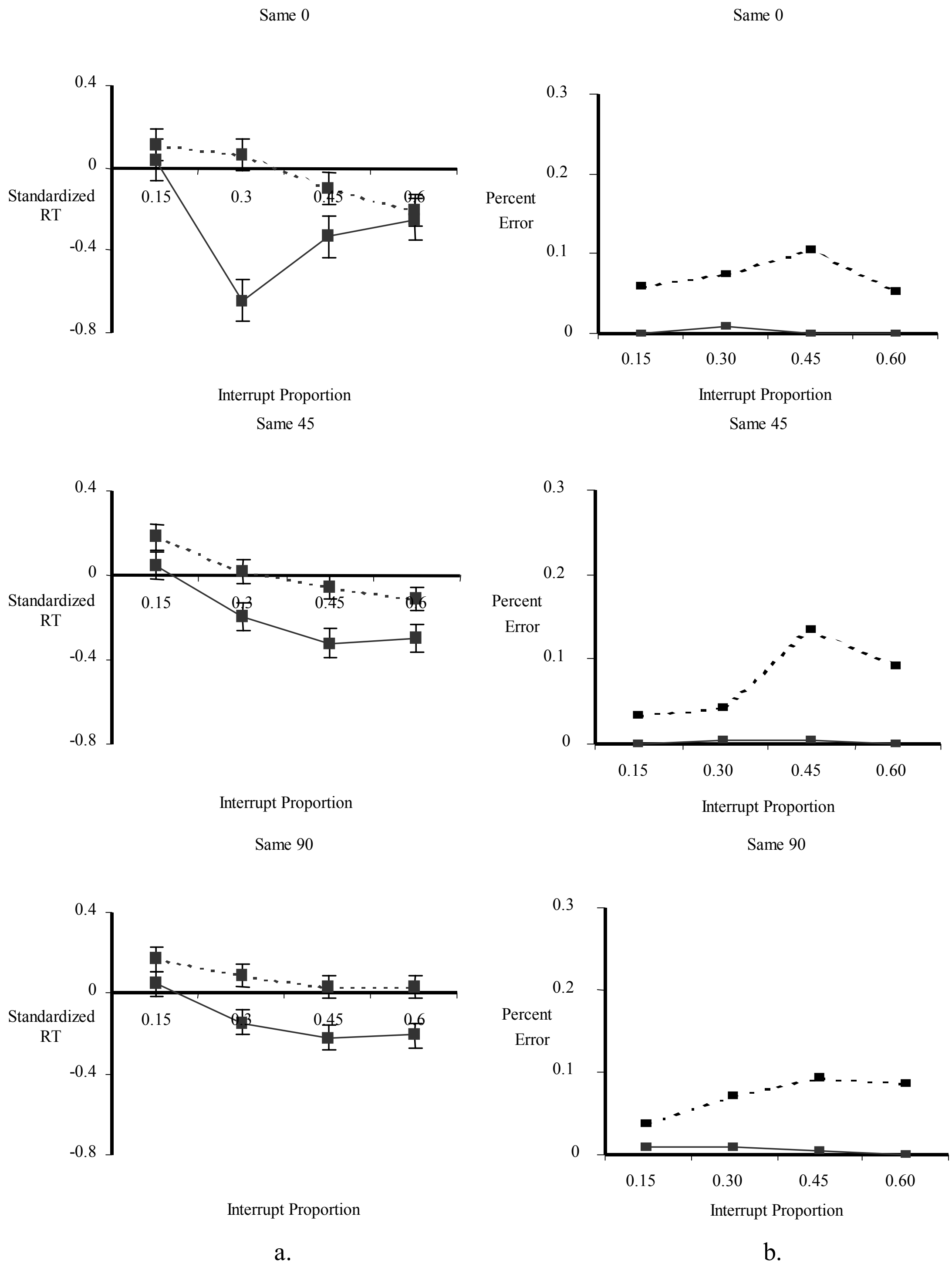


Figure 69. a. Same Standardized RTs separated by angular disparity (0° to 90°).
b. Same Percent Error separated by angular disparity (0° to 90°).

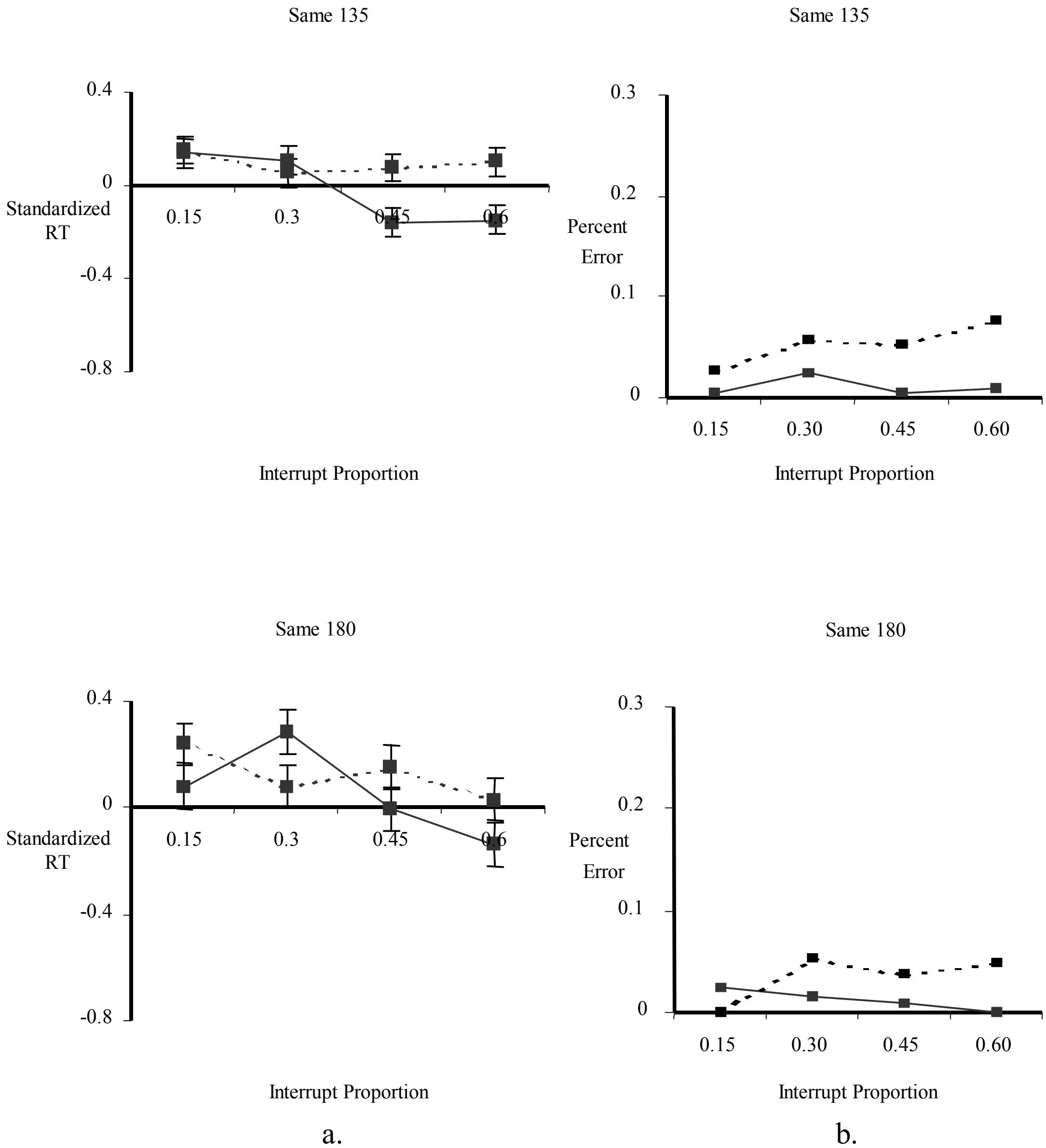


Figure 70. a. Same Standardized RTs (135° and 180°).
b. Same Percent Error (135° and 180°).

Figures 71 and 72 show average RTs with standard errors and percent error for the different data as a function of interrupt proportion separated by angular disparity. For the RT data, there are facilitation effects at each of the angular disparities. However, there are no inhibition effects. For the percent error data, there are inhibition effects at each of the angular disparities. However, there are no facilitation effects. The average RTs and percent error from the same and different data suggest that participants do not have to complete “mental rotation” before they know the handedness of misoriented stimuli.

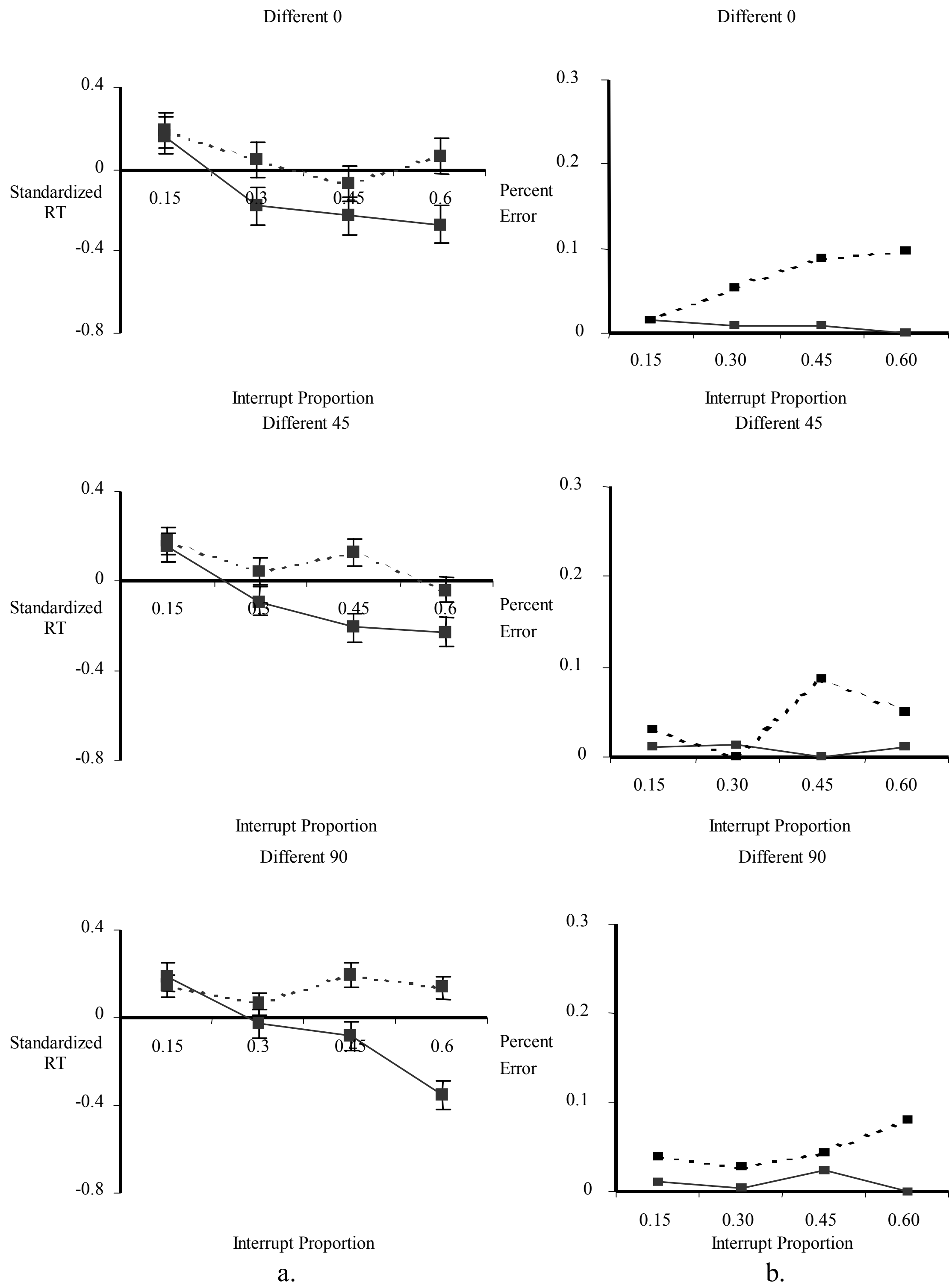


Figure 71. a. Different Standardized RTs separated by angular disparity (0° to 90°).
b. Different Percent Error separated by angular disparity (0° to 90°).

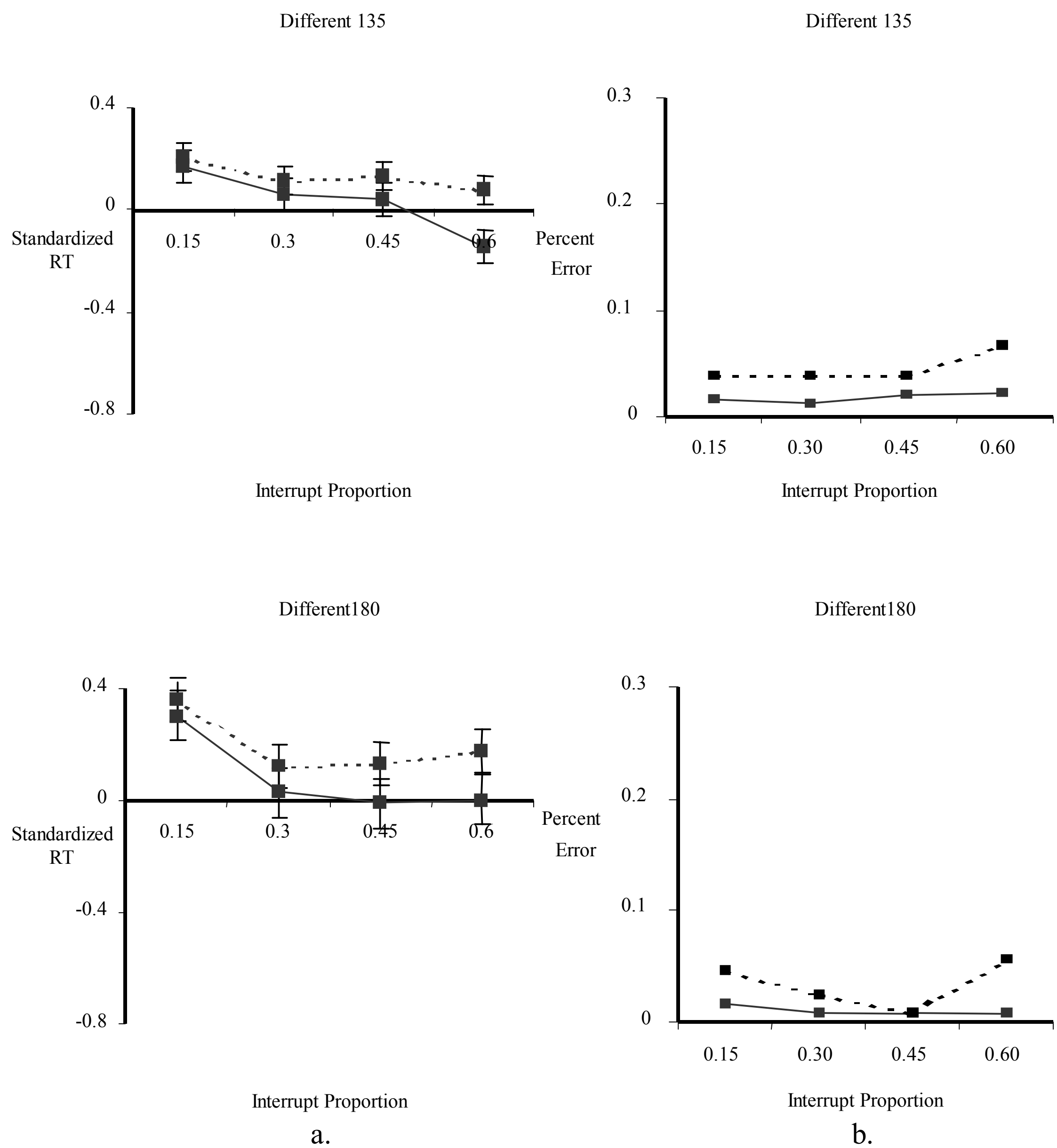


Figure 72. a. Different Standardized RTs (135° and 180°).
b. Different Percent Error (135° and 180°).

I next calculated Cohen's *d* effect sizes for the facilitation and inhibition effects for both the RT data and the percent error data. Once again, the same and different data were analyzed separately. Recall that if participants must use "mental rotation" to determine the handedness of the misoriented stimuli, one would expect larger effect sizes at smaller angular disparities. On the other hand, if participants do not have to complete "mental rotation" to know the handedness of misoriented stimuli, effect sizes should not be a function of angular disparity.

Figure 73a is a plot of the effect sizes for the same RT data plotted as a function of angular disparity and separated by interrupt proportion. Once again, a separate line represents each interrupt proportion. Generally, the effect sizes increase as interrupt proportion increases. However, the effect sizes are not a function of angular disparity. These data suggest that participants do not have to use "mental rotation" to determine the handedness of misoriented stimuli. I next fit log functions to each of the lines in this graph. The log functions are plotted in Figure 73b. The log function for the same data with the .15 interrupt proportion is $d_{.15} = 0.0048 * \mathrm{Ln}(\alpha) + 0.0653$, $r^2 = .04$. The log function for .30 is $d_{.30} = -0.1078 * \mathrm{Ln}(\alpha) + 0.5218$, $r^2 = .86$. The log function for .45 is $d_{.45} = 0.0015 * \mathrm{Ln}(\alpha) + 0.1497$, $r^2 = .02$. The log function for .60 is $d_{.60} = 0.0245 * \mathrm{Ln}(\alpha) + 0.0347$, $r^2 = .76$. These functions indicate an effect of interrupt proportion but no effect of angular disparity.

Figure 73c is a plot of the effect sizes for the same error data plotted as a function of angular disparity and separated by interrupt proportion. This pattern is similar to that of the RT effect size data, in that effect size increases as interrupt proportion increases but is not a function of angular disparity. The log functions for each of these lines are plotted in Figure 73d. The log function for the same data with the .15 interrupt proportion is $d_{.15}$ = -0.4717*Ln(α) + 3.4694, r^2 = .25. The log function for .30 is $d_{.30}$ = -0.0941 *Ln(α) + 2.9587, r^2 = .06. The log function for .45 is $d_{.45}$ = -0.1461*Ln(α) + 4.5153, r^2 = .03. The log function for .60 is $d_{.60}$ = 0.2858*Ln(α) + 2.4826, r^2 = .25. These functions indicate an effect of interrupt proportion but no effect of angular disparity.

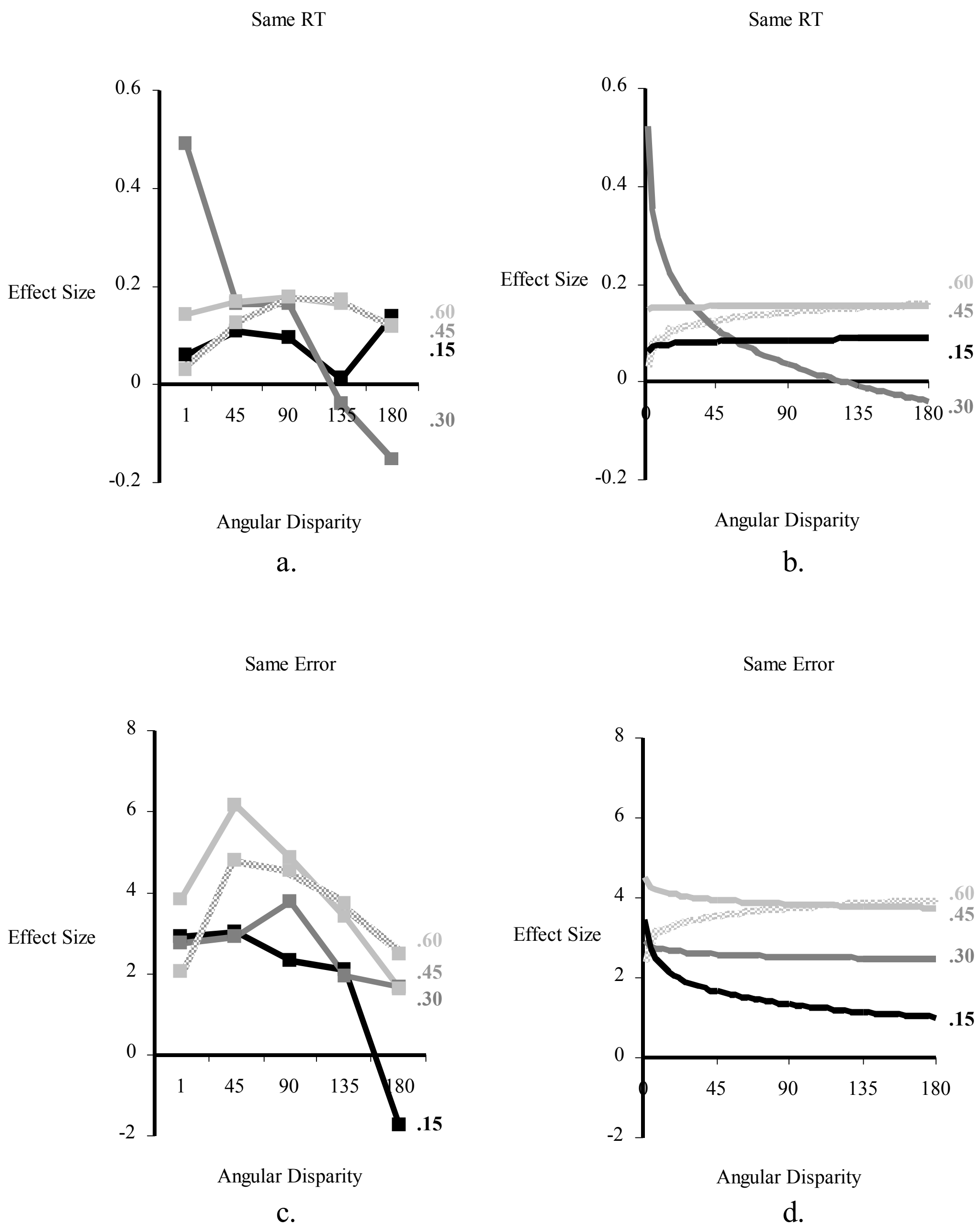


Figure 73. a. Effect Sizes for Same RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Same RTs.
c. Effect Sizes for Same Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Same Error.

Figure 74a is a plot of the effect sizes for the different RT data plotted as a function of angular disparity. There is an effect of interrupt proportion in that as interrupt proportion increases, so do effect sizes. However, there is no effect of angular disparity. The log functions for these data are plotted in Figure 74b. The log function for the different RT data with the .15 interrupt proportion is $d_{.15} = 0.0001*\text{Ln}(\alpha) + 0.0016$, $r^2 < .001$. The log function for .30 is $d_{.30} = -0.0162 *\text{Ln}(\alpha) + 0.1477$, $r^2 = .83$. The log function for .45 is $d_{.45} = 0.0006*\text{Ln}(\alpha) + 0.1425$, $r^2 = .0003$. The log function for .60 is $d_{.60} = -0.0058*\text{Ln}(\alpha) + 0.2061$, $r^2 = .02$. These functions indicate an effect of interrupt proportion but no effect of angular disparity.

Figure 74c is a plot of the effect sizes for the different error data plotted as a function of angular disparity and separated by interrupt proportion. Again, there is an effect of interrupt proportion, but no effect of angular disparity. Figure 74d is a plot of the log functions to these data. The log function for the different data with the .15 interrupt proportion is $d_{.15} = 0.3249*\text{Ln}(\alpha) + 0.0921$, $r^2 = .85$. The log function for .30 is $d_{.30} = -0.1394 *\text{Ln}(\alpha) + 1.606$, $r^2 = .03$. The log function for .45 is $d_{.45} = -0.4587*\text{Ln}(\alpha) + 3.7841$, $r^2 = .24$. The log function for .60 is $d_{.60} = -0.1405*\text{Ln}(\alpha) + 3.5525$, $r^2 = .08$. Again, these functions indicate an effect of interrupt proportion but no effect of angular disparity.

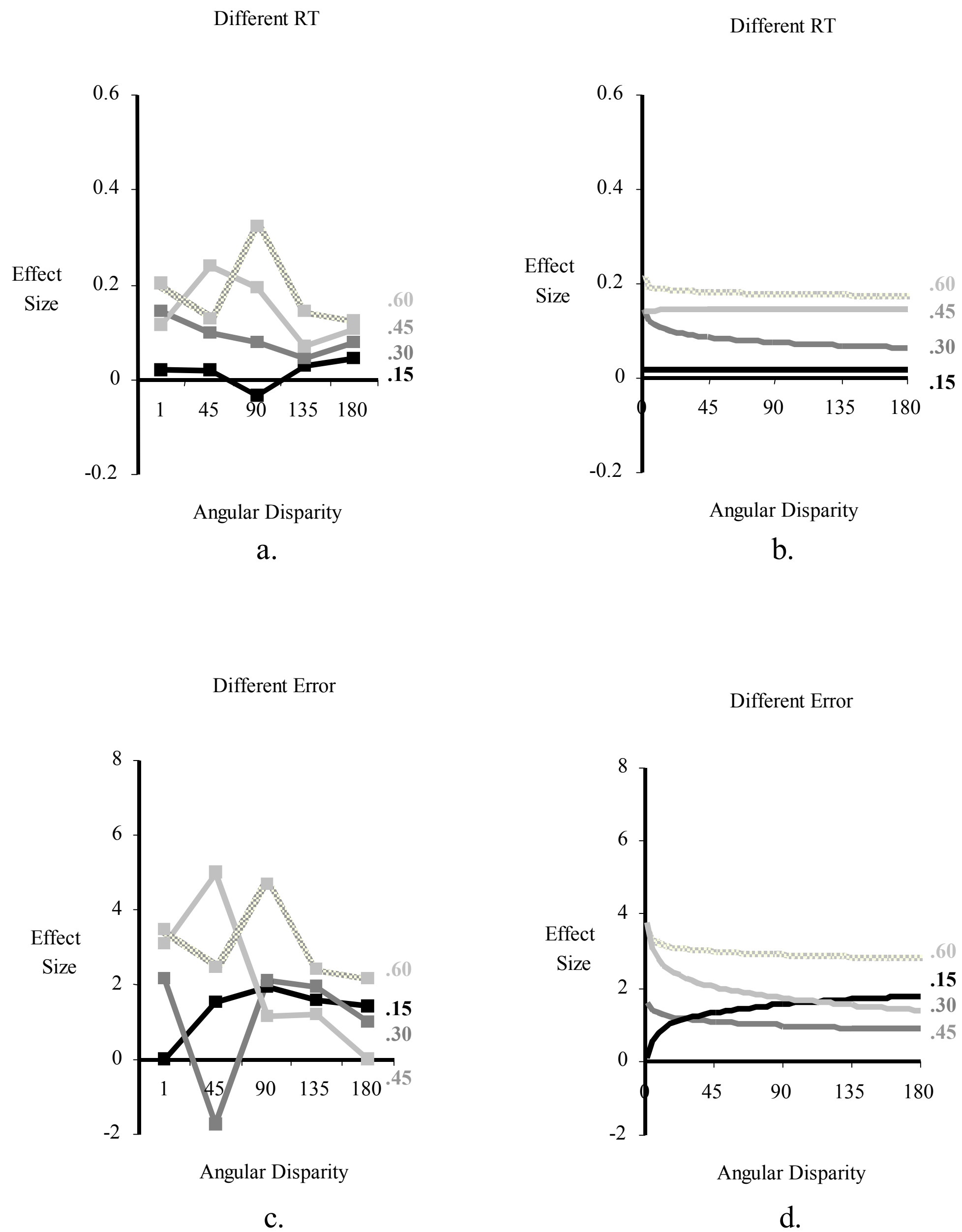


Figure 74. a. Effect Sizes for Different RTs plotted as a function of angular disparity.
b. Log Functions of Effect Sizes for Different RTs.
c. Effect Sizes for Different Error plotted as a function of angular disparity.
d. Log Functions of Effect Sizes for Different Error.

To clarify the relationship between effect sizes and interrupt proportion, I calculated effect sizes for both RT and percent error averaged across angular disparity for each interrupt proportion. Again, these averages were calculated separately for same and different data. The effect of interrupt proportion is evidenced by the fact that average effect sizes increase as interrupt proportion increases (See Figure 75). I will not report statistics on these functions because there were only four interrupt proportions.

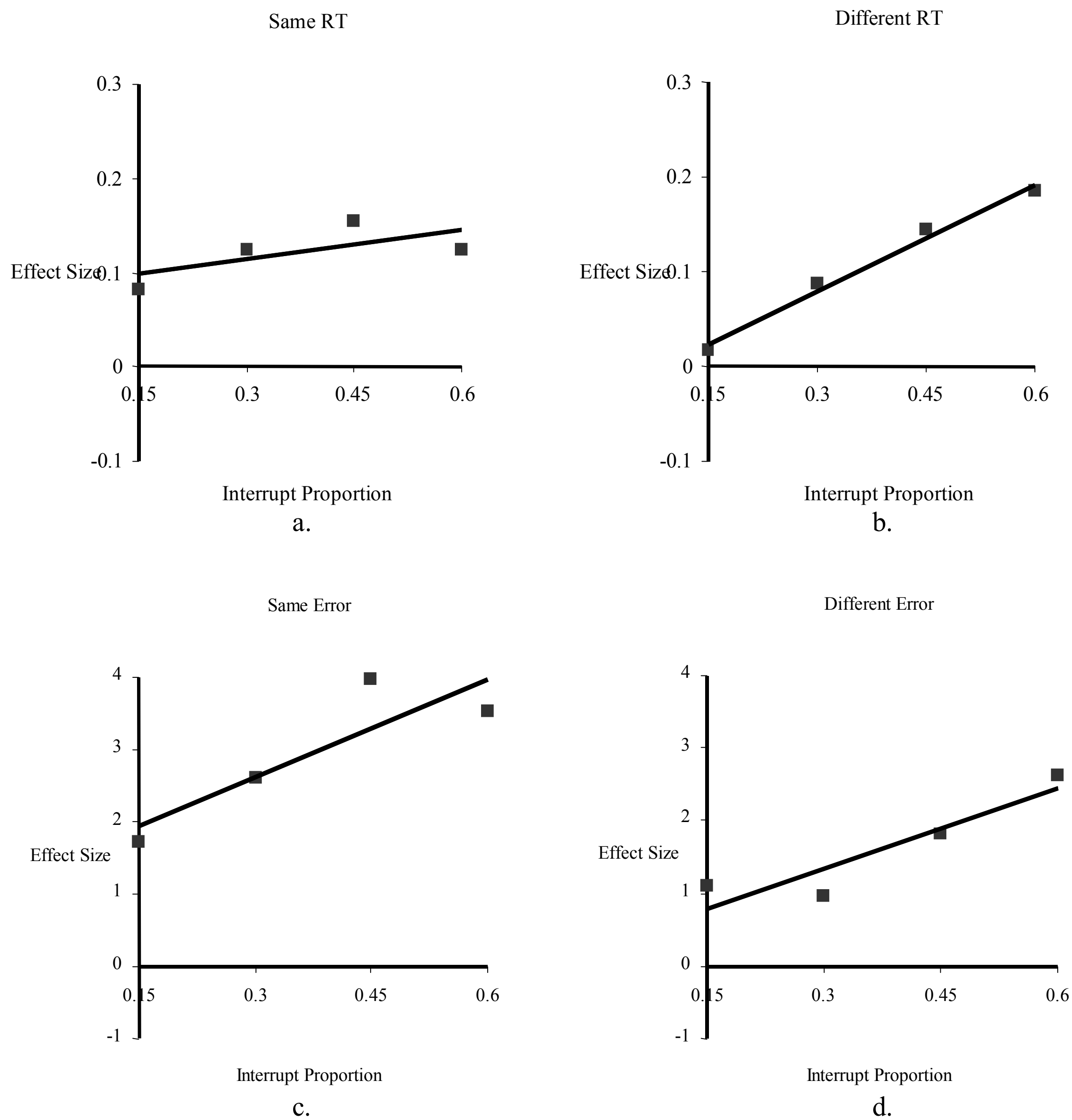


Figure 75. a. Average Effect Sizes for Same RTs plotted by interrupt proportion.
b. Average Effect Sizes for Different RTs plotted by interrupt proportion.
c. Average Effect Sizes for Same Errors plotted by interrupt proportion.
d. Average Effect Sizes for Different Errors plotted by interrupt proportion.

To clarify the relationship between effect sizes and angular disparity, I calculated effect sizes for both RT and percent error averaged across interrupt proportion for each angular disparity. Again, these averages were calculated separately for same and different data. For the same RT data, there was a significant relationship between angular disparity and effect size, effect size = 0.1855 - 0.00071*(angular disparity), F (1, 3) = 22.0666, p = .0182, r^2 = .88. Both the intercept (t = 11.202) and the slope (t = 698) were significant. For the different RT data, there was no relationship between angular disparity and effect size, effect size = 0.1316 - 0.00026*(angular disparity), F (1, 3) = 2.2774, p = .2284, r^2 = .4315. The intercept was significant (t = 6.986), however the slope was not (t = 1.509). For the same error data, there was no relationship between angular disparity and effect size, effect size = 3.9976 - 0.01148*(angular disparity), F (1, 3) = 2.1997, p = .2347, r^2 = .423. The intercept was significant (t = 4.6856), however the slope was not (t = 1.4831). For the different error data, there was no relationship between angular disparity and effect size, effect size = 2.3 - 0.0047*(angular disparity), F (1, 3) = 2.3765, p = .2208, r^2 = .442. The intercept was significant (t = 3.4182), however the slope was not (t = 0.2266). Figure 76 is a graph of each of these functions.

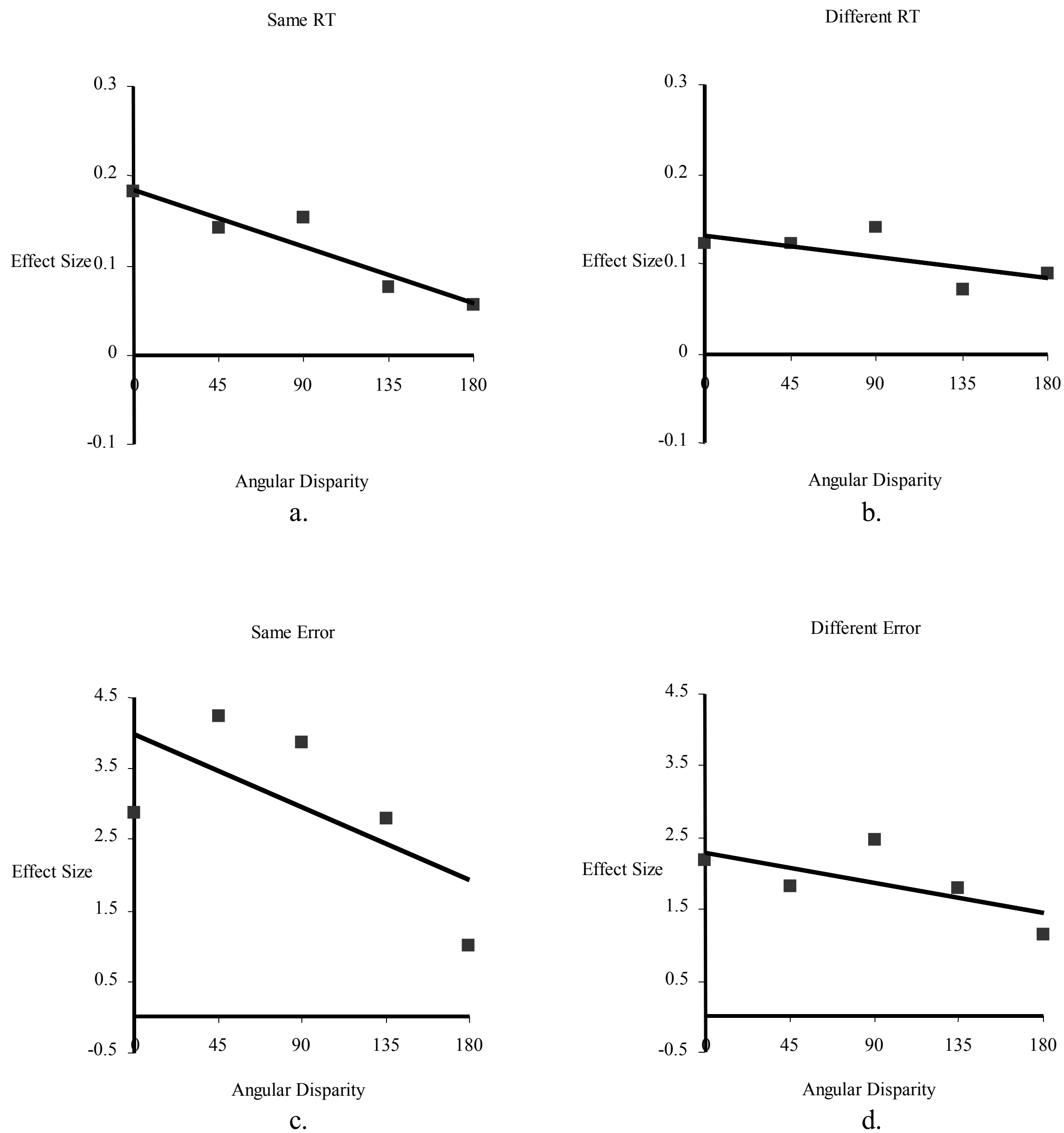


Figure 76. a. Average Effect Sizes for Same RTs plotted by angular disparity.
b. Average Effect Sizes for Different RTs plotted by angular disparity.
c. Average Effect Sizes for Same Errors plotted by angular disparity.
d. Average Effect Sizes for Different Errors plotted by angular disparity.

The relationship between effect size and angular disparity for the same RT data is confusing given the other RT and error data, as well as the data from Experiment 2. I therefore carefully examined the data and discovered that there were two aberrant data points. Furthermore, these data points (0° and 180° at the .30 interrupt proportion) were based on a maximum of two trials for each participant. After removing these two points, there was no relationship between angular disparity and effect size, effect size = 0.10932 $^{+}$ 0.000066*(angular disparity), $\underline{F}$ (1, 3) = 0.0513, $\underline{p}$ = .8353, r^2 = .0168. The intercept was significant ($\underline{t}$ = 3.4182), however the slope was not ($\underline{t}$ = 0.2266). Figure 77 is a graph of the same RT effect sizes averaged across interrupt proportion and plotted by angular disparity with the two aberrant data points removed.

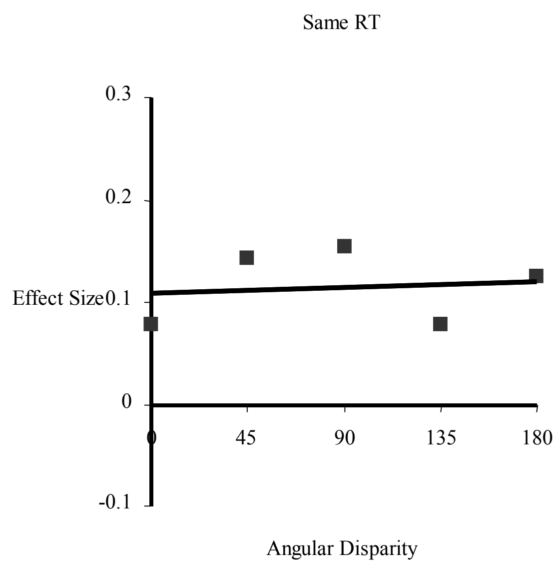


Figure 77. Average Effect Sizes for Same RTs plotted by angular disparity with aberrant data points removed.

In summary, there was an effect of interrupt proportion (i.e., facilitation and inhibition increased as interrupt proportion increased). However, there was no effect of angular disparity.

Discussion

Experiment 4 was a replication of Experiment 2. I first determined that the insertion of the side decision task did not change the way participants completed the handedness decision task. Next, I concluded that responses occurring within 200 ms of the interrupt time were actually responses to the handedness decision task. Finally, I concluded that there was an effect of interrupt proportion but no effect of angular disparity. Facilitation and inhibition effects increased as interrupt proportion increased but were not a function of angular disparity. Again, these results oppose the Shepard model and indicate that participants knew the handedness of misoriented stimuli before using “mental rotation.”

GENERAL DISCUSSION

The dualtask paradigm used in the current series of experiments was designed to investigate whether participants know the handedness of misoriented stimuli before using “mental rotation” in a handedness decision task. For all four experiments, there were effects of angular disparity on both the practice and non-interrupt trials indicating that participants were using “mental rotation” to complete the handedness decision tasks. In Experiments 1 and 3, participants were presented with a different pair of stimuli on each trial. There were effects of both interrupt proportion and angular disparity on the interrupt trials indicating that participants did not know the handedness of the stimuli before using “mental rotation.” In Experiments 2 and 4, participants were presented with only two pairs of stimuli. For these experiments, there were effects of interrupt proportion, but no effects of angular disparity on the interrupt trials. These results indicate that participants did know the handedness of the stimuli before using “mental rotation.” Therefore, it seems that participants’ ability to determine the handedness of a misoriented stimulus depends on the amount of experience they have with that stimulus. To make these conclusions, several assumptions of the dualtask paradigm had to be validated.

Validity of the Dualtask Procedure

The dualtask procedure was developed to assess whether, in a handedness decision task, participants have handedness information before they complete “mental rotation.” To accomplish this, the dualtask procedure, (1) requires participants to complete handedness decision tasks, (2) on a random selection of trials, interrupts participants with a side decision task before they respond to the handedness decision task, and (3) measures the degree of facilitation or inhibition of participants’ responses to the side decision task. The dualtask procedure and analysis rest on three assumptions. The first assumption is that the insertion of the side decision task does not change the way participants complete the handedness decision task. The second assumption is that interrupt trials are sensitive to fluctuations in participants’ knowledge of the handedness of the stimuli. The third assumption involves the analysis of the interrupt trials. Specifically, I assume that all responses completed within 200 ms of the interrupt time are actually responses that were initiated to the handedness decision task. Because the dualtask paradigm is new, its assumptions must first be validated.

The possibility of being interrupted with the side decision task may influence participants’ performance on the handedness decision task. That is, if participants expect an interruption, they may use a distinctly different strategy to determine the handedness of a stimulus. If this were the case, the different strategy should be evident in their data. Specifically, there should be a difference in the pattern of data between trials when participants never expected an interruption, and trials when participants may expect an interruption. If, however, the participants follow the directions and treat every trial as a handedness decision trial, their data on those trials in which they never expected an interruption should be similar to those trials in which they may expect an interruption. Fortunately, in the practice phase of the experiment, participants were never interrupted with the side decision task. Therefore, the practice trials provide a measure of participants' performance when no interruption is expected. To verify the assumption that inserting the side decision task did not change the way participants completed the handedness decision task, I compared the data from the practice trials to the data from the non-interrupt trials. If there were no differences between participants' performance on the practice and non-interrupt trials, one could conclude that inserting the side decision task did not change the way participants completed the handedness decision task.

For Experiments 1 and 3, there were no differences between participants' performance on the practice and non-interrupt trials. However, there were some differences between participants' performance on the practice and non-interrupt trials for Experiments 2 and 4. In particular, participants were somewhat faster on the non-interrupt trials than the practice trials. These differences were minimal and most likely due to practice effects. Recall that participants in Experiments 2 and 4 were given a small set of stimuli throughout the experiment. Because participants had more experience with the stimuli during the non-interrupt trials relative to the practice trials, one would expect participants to be faster on the non-interrupt trials. In contrast, participants in Experiments 1 and 3 never saw the same stimulus twice. Because participants in Experiments 1 and 3 had no more experience with the stimuli during the non-interrupt trials relative to the practice trials, one would expect no practice effects. Our data mimicked these predictions. Importantly, in all experiments, the slopes relating RT to angular disparity were greater than the LRC indicating that participants were using "mental rotation." I concluded from these findings that inserting the side decision task did not change the way that participants completed the handedness decision task.

A second assumption is that the dualtask procedure, using only the interrupt trials, is sensitive to fluctuations in participants' knowledge of the handedness of the stimuli. If one assumes that participants acquire information about the stimulus over time, then longer presentation times should allow the participants more time to acquire knowledge about the handedness of the stimulus. Therefore, participants will know the handedness of the stimulus before being interrupted with the side decision task on a greater proportion of trials with larger interrupt proportions than trials with smaller interrupt proportions. Hence, larger interrupt proportions should have greater facilitation and inhibition effects. Importantly, the predicted relation between interrupt proportion and the degree of facilitation and inhibition effects is rather complicated. However, in all cases, the predicted relation is nondegenerative, but the shape of the function is not specified. For all four experiments, there were effects of interrupt proportion. These effects were characterized by facilitation and inhibition effects increasing as interrupt proportion increased. Thus, the dualtask procedure is sensitive to fluctuations in participants' knowledge of the handedness of the stimuli.

The third assumption is that all responses on the interrupt trials less than 200 ms were actually responses that were initiated to the handedness decision task. In the dualtask paradigm, participants were instructed to respond to the handedness decision task as quickly as possible, unless they were interrupted with the side decision task. Inevitably, there were some trials in which participants initiated a response to the handedness decision task and were *then* interrupted with the side decision task. On these trials participants completed their response after the interruption, however, these were actually responses to the handedness decision task. If these responses were included in the interrupt analysis, it would result in a large proportion of errors on inhibition trials, and a large proportion of exceedingly fast, correct responses on the facilitation trials.

To determine which responses were to the side decision task, I first looked at the distributions of responses to the interrupt trials and concluded that they were bimodal with a gap located around the interrupt time plus 200 ms. It was reasonable to assume that this gap represented a break between two distributions (i.e., one distribution of fast responses to the handedness decision task and another distribution of responses to the side decision task). To test this prediction, I constructed a maximum likelihood model that used the fast responses (those occurring within 200 ms of the interrupt time) to estimate an entire distribution of hypothesized responses to the handedness decision task. If the responses occurring within 200 ms of the interrupt time were responses to the handedness decision task, the predicted distribution should closely match participants' performance on non-interrupt trials. To test this, I compared the predicted distributions to those of the non-interrupt trials. In most cases, there were no differences between the two distributions. In the cases that there were statistical differences, the distributions fell relatively closely to each other. These results indicated that responses occurring within 200 ms of the interrupt time were responses to the handedness decision task that had been initiated before the interruption. Therefore, these responses were excluded from the examination of the facilitation and inhibition effects in the interrupt trials.

Before conclusions could be made from the present series of experiments, the assumptions of the dualtask procedure had to be validated. I first compared the data from the practice trials and the non-interrupt trials and found that the insertion of the side decision task did not change the way participants completed the handedness decision task. I next examined the effect of interrupt proportion on the interrupt trials and demonstrated that that the dualtask procedure is sensitive to fluctuations in participants' knowledge of the handedness of the stimuli. Finally, I concluded that responses on the interrupt trials occurring within 200 ms of the interrupt time were actually responses to the handedness decision task. These assumptions being validated, I investigated the effects of angular disparity on facilitation and inhibition.

Assessing the Effects of Angular Disparity

The effect of angular disparity on facilitation and inhibition in the side decision task provides a measure of participants' knowledge of handedness information before they respond to the handedness decision task. In other words, the effect of angular disparity on facilitation and inhibition reveals whether or not participants must complete "mental rotation" to determine the handedness of misoriented stimuli.

Recall that "mental rotation" is assumed to be an analog process. If participants must use "mental rotation" to determine the handedness of misoriented stimuli, they will know the handedness of stimuli with smaller angular disparities faster than stimuli with larger angular disparities. Consequently, participants will have the correct hand primed on the facilitation trials and the incorrect hand primed on the inhibition trials before being interrupted with the side decision task on a greater proportion of trials with smaller angular disparities than trials with larger angular disparities. Thus, if participants must use "mental rotation" to determine the handedness of misoriented stimuli, then facilitation and inhibition effects would decrease as angular disparity increases. The fact that the results of Experiments 1 and 3 conformed to these predictions indicates that "mental rotation" was necessary to determine the handedness of misoriented stimuli. Furthermore, these findings demonstrate that the dualtask procedure is sensitive to the effects of angular disparity.

If participants do not have to complete "mental rotation" to determine the handedness of misoriented stimuli, they will know the handedness of stimuli with larger angular disparities at the same time as stimuli with smaller angular disparities. Therefore, participants would have the correct hand primed for facilitation trials and the incorrect hand primed for inhibition trials for larger angular disparities at approximately the same time as stimuli with smaller angular disparities. Therefore, facilitation and inhibition effects would not be a function angular disparity. The fact that Experiments 2 and 4 conformed to these predictions indicates that "mental rotation" was not necessary to determine the handedness of misoriented stimuli. Furthermore, the fact that Experiments 1 and 3 did reveal effects of angular disparity indicates that if there were effects of angular disparity in Experiments 2 and 4, the dualtask procedure and analysis would have detected these effects.

The differing results of Experiments 1 and 3 and Experiments 2 and 4 must be due to participants' experience with the stimuli. In Experiments 1 and 3 participants were never given more than one trial of experience with a given stimulus. In these conditions, participants were unable to determine the handedness of misoriented stimuli without using "mental rotation." However, in Experiments 2 and 4 participants acquired extensive experience with a small set of stimuli. In these conditions, participants were able to determine the handedness of misoriented stimuli without using "mental rotation." Because the experiments were identical in every other aspect, these data show that participants' ability to determine handedness information about a misoriented stimulus without "mental rotation" is directly related to the amount of experience they have with that particular stimulus. These conclusions coincide with the findings of Cohen and Kubovy (1993) and Cohen and Blair (1998).

Recall that Cohen and Kubovy (1993) and Cohen and Blair (1998) found that when RT pressure is inserted in a handedness decision task, participants could complete the task without using "mental rotation." The lack of "mental rotation" in these experiments was only observed when participants were presented with a limited number of stimuli. When participants were presented with a different pair of stimuli on each trial, they were unable to complete the task without using "mental rotation." These results, together with the results of Experiments 1 through 4, suggest that the visual system is able to extract orientation free information only when the viewer has extensive experience with a given set of stimuli

An alternative hypothesis is that participants in Experiments 2 and 4 formed multiple representations of the stimulus, each in a different orientation allowing them to determine handedness information about the stimuli without using "mental rotation." Tarr and Pinker (1989) demonstrated that participants would form multiple orientation specific representations if given the opportunity. The multiple representations hypothesis, however, is unlikely to explain our results. Specifically, to persuade their participants to form multiple orientation specific representations, Tarr and Pinker presented participants with the same stimulus in each orientation over 200 times. Even when provided with this extensive exposure, the slopes relating RT to angular disparity were approximately 1 ms/degree. So it seems that for participants to form multiple orientation specific representations of a stimulus, they need scores of presentations of that stimulus in a specific orientation. Participants in Experiments 2 and 4 saw each stimulus in each orientation only 12 times. Given this limited exposure, it is doubtful that participants were able to form multiple orientation specific representations of the stimuli. Additionally, Cohen and Kubovy (1993) provide evidence that participants *can* form an orientation free representation of a stimulus in a single experimental session. The authors presented participants with stimuli in unique orientations on every trial (i.e., 0° to 359° in 1° increments). Thus, the authors denied participants the opportunity to form multiple orientation specific representations by never showing them the same stimulus in the same orientation. Nevertheless, participants were still able to complete the task without using "mental rotation." Thus, the multiple orientation specific representations hypothesis is unlikely to explain our results.

In summary, it seems that given the opportunity, the visual system is capable of creating orientation free representations of stimuli. To create an orientation free representation of a stimulus the visual system needs some experience with that particular stimulus. It remains uncertain how much experience with a stimulus the visual system requires to create an orientation free representation.

Models of Object Recognition

There are several classes of object recognition theories: viewpoint dependent, viewpoint independent, and double-checking theories. These theories make different assertions about what information is available in people's mental representation of a stimulus.

Viewpoint dependent theories assume objects are encoded in a viewer-centered fashion, and the recognition of objects presented at novel viewpoints will be impaired. Jolicoeur's (1990) *dual systems theory*, Rock's (Rock, 1974; Rock and DiVita, 1987) theory of *viewer-centered object perception*, and Tarr and Pinker's (1989) *multiple views theory* are examples of viewpoint dependent theories. In addition, the previously discussed and widely accepted Shepard model is a viewpoint dependent theory. These theories posit that information relevant to handedness recognition is orientation specific. This means that participants will not be able to determine the handedness of a misoriented stimulus without using a process of "mental rotation." The Shepard model can account for the findings of Experiments 1 and 3, in which participants were unable to determine the handedness of misoriented stimuli without using "mental rotation." However, the Shepard model cannot account for the findings of Experiments 2 and 4, in which participants were able to determine handedness information without using "mental rotation."

Viewpoint independent theories assume objects are encoded independent of viewpoint, and the recognition of familiar objects presented in novel orientations will not be impaired. Biederman's (1987) *recognition by components theory* (RBC) is a viewpoint independent theory. Viewpoint independent theories assert that the visual system recovers the three-dimensional shape of a stimulus regardless of orientation. Therefore, these theories predict that "mental rotation" is not necessary to determine the handedness of stimuli. Viewpoint independent theories can explain the facilitation and inhibition effects of Experiments 2 and 4. However, they cannot explain the results of the practice and non-interrupt trials in these experiments, in which participants used "mental rotation" to complete the handedness decision tasks. Viewpoint independent theories are also unable to account for the results of Experiments 1 and 3 in which participants were unable to determine handedness information about stimuli without using "mental rotation."

Double checking theories assume objects are encoded in a viewpoint independent fashion and normalization (e.g., "mental rotation") is done as a double-checking mechanism to ensure that no mistakes in recognition are made. DeCarro & Reeves (2000) *rotate to orient hypothesis*, and Corballis's (1988) *double check hypothesis* are examples of double-checking theories. The double check model asserts that, although participants do not initiate a response to a handedness decision task until they have completed "mental rotation," handedness information is available to them prior to using "mental rotation." The double check model is consistent with the findings of Experiments 2 and 4. Recall that in Experiments 2 and 4, participants were able to determine handedness prior to "mental rotation," however participants did not initiate a response to the handedness decision task until after they completed "mental rotation." Nevertheless, the double check model cannot account for the findings of Experiments 1 and 3 in which no handedness information was available to participants prior to using "mental rotation."

A New Theory of Object Recognition

The findings of the present series of experiments indicate that handedness information is sometimes available to participants before using "mental rotation." In particular, participants' ability to determine the handedness of a misoriented stimulus without using "mental rotation" is a function of the amount of experience they have with that stimulus. Moreover, even when handedness information is available to participants they still use "mental rotation" to complete handedness decision tasks. These findings beg two questions: (1) is recognition of everyday objects orientation specific or orientation free, and (2) why do people use "mental rotation" if they already know the handedness of a stimulus?

The ability to recognize misoriented objects is a central feature of perception. There are two ways the visual system can represent objects, orientation dependent or orientation free. There are costs and advantages associated with both types of representations. In particular, orientation dependent representations likely are less costly to form, but there is an added cost every time one must recognize a misoriented object. In contrast, orientation free representations likely take longer to form, but once formed, there is no additional cost to recognizing a misoriented object.

Orientation dependent representations are relatively easy to construct. In fact, when people are shown a stimulus only once, they use an orientation dependent representation to recognize the stimulus. This is an efficient strategy if one views a stimulus very infrequently. However, each time an orientation dependent representation is used to recognize a misoriented stimulus, a process of “mental rotation” must be employed. Thus, if one views a stimulus frequently, orientation dependent representations are inefficient because “mental rotation” is a slow process. In fact, the original studies by Shepard and Metzler (1971) revealed slopes relating RT to angular disparity of approximately 17 ms/degree. Given this rotation speed, it would take more than three seconds to imagine a stimulus rotate 180°. Given the rotation speeds observed in the current series of experiments (approximately 4 ms/degree), it would take just under one second to complete the same task. It should be noted that one second is a long time relative to most visual processes. In fact, it has been shown that participants can accurately identify a stimulus in 25 ms (e.g., Jolicoeur & Landau, 1984). Thus, the initial cost of forming an orientation dependent representation is low, however, there are extra costs every time that representation is used to recognize a misoriented stimulus.

For the visual system to be useful, vision must be accurate and virtually instantaneous. Orientation free representations can be used to quickly recognize misoriented stimuli because “mental rotation” is not necessary. However, there is a cost of constructing orientation free representations. In particular, to recover the three-dimensional shape of an object, one must view the object from several different viewpoints. This requirement exists because the retina is two-dimensional and an infinite number of three-dimensional shapes could produce the same retinal projection. Once an orientation free representation is formed, it is much more efficient than an orientation dependent representation, because the visual system need not use a normalization process to recognize misoriented stimuli. Hence, when one is expected to recognize a stimulus repeatedly, an orientation free representation is much more efficient than an orientation dependent representation.

In the natural environment, (1) people must recognize objects repeatedly, and (2) the visual system has ample opportunity to construct orientation free representations of most objects. Therefore in the natural environment, orientation free representations are more efficient over the long term than orientation dependent representations. In contrast, under experimental situations in which participants view an object only one time in one orientation, it is *efficient* to use orientation dependent representations. Because these experimental situations are not representative of people's natural environment, the results of experimental situations may not be generalizable to the natural environment. It is more likely that object recognition in the natural environment depends on orientation free representations.

Another major implication of the current series of experiments is that handedness decision tasks are inadequate tests of the information available to the visual system when people perceive an object. Most studies that use handedness decision tasks assume that participants use "mental rotation" to complete the task because their internal representations of the stimuli are orientation dependent. However, Experiments 2 and 4 demonstrate that when the visual system constructs orientation free representations, participants still use "mental rotation" to complete handedness decision tasks. Therefore, handedness decision tasks, as currently used, do not distinguish between orientation free representations and orientation dependent representations.

It is likely that the "mental rotation" process is a double checking mechanism participants use to ensure accuracy. It is uncertain why participants would employ an unnecessary double checking process. DeCarro & Reeves (2000) suggest that because accidents in viewpoint are common in the "real world," participants engage in double checking to make sure that they perceived the stimulus in the correct orientation. Corballis (1988) suggests that participants rotate to ensure that the identification of the stimulus was correct. The data from the present series of experiments are unable to differentiate between these theories. However, it is clear that participants unnecessarily use "mental rotation" even though handedness information is available.

SUMMARY

The dualtask paradigm used in the current series of experiments was designed to investigate whether participants know the handedness of misoriented stimuli before using "mental rotation" in a handedness decision task. In some cases handedness information is available to participants before using "mental rotation." The ability to determine handedness information about a stimulus without using "mental rotation" is a function of how much experience participants have with that stimulus. Thus, the visual system is capable of constructing an orientation free representation of an object only when it has experience with that object. It remains uncertain how much experience with a stimulus the visual system requires before it can create an orientation free representation.

The present experiments also reveal that participants will use "mental rotation" to complete the handedness decision tasks even when they already have handedness information available. These findings support the double check model, which assumes "mental rotation" is an unnecessary double checking strategy. Therefore, handedness decision tasks are inadequate tests of the information available to people when they perceive an object.

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