Evaluating the Independent Race Model for the Stop Signal Paradigm: Context Independence is Violated at Short Stop Signal Delays

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CHAPTER I

INTRODUCTION

Cognition and action are useful because they can be controlled and directed towards the achievement of goals. Cognitive control is often understood as a set of acts of control (Friedman & Miyake, 2004; Logan, 1985; Logan, Van Zandt, Verbruggen, & Wagenmakers, 2014). These acts include formulating and coordinating strategies to complete a task (Logan, 1985), resolving the interference from previously relevant information (Friedman & Miyake, 2004), resisting the intrusion from distracting (Eriksen & Eriksen, 1974; Friedman & Miyake, 2004) and conflicting (Botvinick, Braver, Barch, Carter, & Cohen, 2001) representations, and inhibiting inappropriate responses (Logan & Cowan, 1984). Many acts of control involve modulating the current course of behavior. Modulation can be sufficient if the current course of action is largely appropriate for the current goals. However, goals can immediately and completely change, like when a green light turns red while driving. In this case, the current course of action (accelerating) must be stopped. In these instances, a behavioral kill switch, response inhibition, is a necessary act of control. Response inhibition, or stopping, is also a necessary part of changing action as goals change. Before changing to a new course of action, stopping the current course of action is necessary (Bissett & Logan, 2013; Camalier et al., 2007). Therefore, stopping is a fundamental act of control that affords behavioral flexibility whenever actions need to change with changing goals.

A primary paradigm used to understand how people are able to stop their responses as their goals change is the stop signal paradigm (Logan & Cowan, 1984), which usually involves making a choice response to a go task and stopping that response when an infrequent stop signal occurs. The main theoretical vehicle for understanding the stop signal paradigm is the Independent Race Model (Logan & Cowan, 1984; Logan et al., 2014), which assumes that a go process begins at go stimulus onset and races independently against a stop process that begins at stop signal onset. Whichever process finishes first determines behavior. Stop finishing first results in stop success; go finishing first results in stop-failure. Most research supports the independence assumption of the race model, but some recent work has suggested that this assumption does not hold when SSD is short.

In this dissertation, I aimed to validate and delineate this phenomenon. First, I explored and proposed methods for evaluating the violation of context independence. Second, I addressed the hypotheses that the violation results from specific SSD or RT values. Third, I explored the role of stop stimulus modality to provide data that sets the stage for distinguishing hypotheses for the violation. Fourth, I explored the role of stop stimulus selectivity, addressing the competing hypotheses that violations in stimulus selective stopping result from short SSDs or having to discriminate the stop stimulus. Last, I addressed the implications of these violations for stop signal data and models.

Independence and the Independent Race Model

The Independent Race Model. The Independent Race Model (Logan & Cowan, 1984) assumes that a go process begins when a go stimulus occurs, and it races in parallel against a stop process that begins when a stop signal occurs. If the go process finishes first, subjects make a response. If the stop process finishes first, the go process is inhibited and no response occurs. The finishing times of the stop and the go process are assumed to be independent random variables, so the outcome of the race is probabilistic.

This model provides a theoretical framework to understand stop signal performance. First, data show that as the delay between going and stopping increases, the p(respond|stop signal) increases (Logan & Cowan, 1984). This is explained in the race model by suggesting that SSD handicaps the race in favor of the go process or the stop process. With a long SSD, the race is handicapped in favor of the go process, and the go process often finishes first. With a short SSD, the race is handicapped in favor of the stop process, and the stop process often finishes first.

The race model predicts the p(respond|stop signal), P_r, at delay t_d in the following equation:

$$P_r(t_d) = \int_0^{\infty} f_{go}(t)(1 - F_{stop}(t - t_d))\,dt, \tag{1}$$

where $f_{go}(t)$ is the distribution of finishing times for the go process, and $F_{stop}(t - t_d)$ is the cumulative distribution function of finishing times for the stop process.

A simpler version of the race model, which I used in this dissertation, assumes SSRT is a constant. With this assumption, Equation 1 becomes:

$$P_r(t_d) = \int_0^{t_d + \mathrm{SSRT}} f_{go}(t)\,dt. \tag{2}$$

As t_d increases the integral includes a larger portion of the distribution of finishing times for the go process, increasing $P_r(t_d)$.

The second main aspect of performance captured by the race model is stop-failure RT (overt responses that escape inhibition on stop trials) tends to be faster than no-stop signal RT, and stop-failure RT tends to decrease with decreasing SSD (Lappin & Eriksen, 1966; Logan, 1981; Logan & Cowan, 1984; Logan, Cowan, & Davis, 1984). These two points are made explicit in the equation for the distribution of stop-failure RTs at a given SSD,

$$f_{sf}(t|t_d) = f_{go}(t)(1 - F_{stop}(t - t_d))/P_r(t_d), \quad (3)$$

where $f_{sf}(t|t_d)$ is the distribution of stop-failure RTs at delay t_d.

If SSRT is a constant, then the distribution of stop-failure RTs becomes:

$$f_{sf}(t|t_d) = f_{go}(t)/P_r(t_d), \quad (4)$$

for all t's from t = 0 to t = t_d + SSRT, and 0 after t_d + SSRT. The mean of this distribution is:

$$Mean_{sf} = \int_0^{t_d + \mathrm{SSRT}} t * f_{go}(t)dt/P_r(t_d), \quad (5)$$

and the median is:

$$½\, P_r(t_d) = \int_0^{Median_{sf}} f_{go}(t)dt. \quad (6)$$

I used these estimates of means and medians throughout the dissertation. Equation 4 shows how the stop-failure distribution is truncated at SSD + SSRT, so as SSD decreases more and more of the go distribution is truncated, resulting in shorter stop-failure RTs.

Independence Assumptions in the Independent Race Model. The Independent Race Model (Logan & Cowan, 1984) assumes two types of independence: context independence and stochastic independence. Formally, context independence means:

$$P(T_{go} < t|\text{no-stop-signal}) = P(T_{go} < t|t_d), \quad (7)$$

for all t and t_d. T_{go} is a random variable representing the go finishing time. This means that the finishing time distribution of the go process is unaffected by a stop signal occurring. This is commonly evaluated by comparing mean stop-failure RT to mean no-stop-signal RT. Stop-failure RTs should only include those go processes fast enough to beat the stop process, so should be faster than no-stop-signal RTs. Hence, if stop-failure RT is faster than no-stop-signal RT, then the context independence assumption is assumed to hold, and the race model is applied to the data. Below, I discuss why this is a problematic test of context independence.

Equation 7 says that the go process is unaffected by the presence of a stop signal. This is the form of context independence that is often evaluated to test the race model. The race model also assumes stop context independence, which means that the stop process is unaffected by the presence of a go signal. Therefore, the stop process should be the same across SSDs. This dissertation focuses on go context independence, and when I say "context independence" this is shorthand for go context independence.

The Independent Race Model also assumes stochastic independence. This means that the finishing times of the go and stop processes are independent on any given trial. Formally, stochastic independence means:

$$P(T_{go} < t_{go} \cap T_{stop} < t_{stop}) = P(T_{go} < t_{go}) * P(T_{stop} < t_{stop}) \quad (8)$$

for all t_{go} and t_{stop}. T_{go} and T_{stop} represent random variables for the go process and stop process, respectively.

Finally, the Independent Race Model does not assume functional independence of the stop and go processes. Functional independence means that the factors that influence the finishing times of the go process do not affect the finishing times of the stop process and vice versa. Though the question of whether the go and stop process are functionally independent has been an interesting topic of research (e.g., Logan et al., 2014), functional independence is not an assumption of the Independent Race Model and violating functional independence need not invalidate the race model. This dissertation focuses on evaluating the assumptions of the race model, focusing particularly on context independence in the go task.

Estimating Stop Signal Reaction Time. The stop signal reaction time, SSRT, cannot be measured directly, but it can be estimated with the Independent Race Model. As discussed above, the race model assumes context independence, or that the go process is the same on stop and no-stop-signal trials. This assumption justifies using the no-stop-signal RT distribution to estimate the full distribution of go processes on stop trials.

SSRT can be calculated with several methods, each with specific strengths and limitations. Logan and Cowan (1984) showed that the mean SSRT is equal to the difference between the mean no-stop-signal RT and the mean of the inhibition function. The inhibition function plots the p(respond|stop signal) against SSD. The popular subtraction method for computing SSRT is based upon this logic. In the subtraction method, a “1 up 1 down” tracking algorithm is implemented, which aims to find the SSD for which the p(respond|stop signal) = .5 (Levitt, 1971; Osman, Kornblum, & Meyer, 1986). If both go and stop win the race half of the time, the race is tied, so mean go RT = mean SSD + mean SSRT. Mean SSRT is then calculated by subtracting mean SSD from mean go RT. This method in inaccurate when there is strategic slowing of go RT or skew in the go RT distribution (Verbruggen, Chambers, & Logan, 2013).

The second method, the integration method, is a more general method that allows computation of SSRT across different SSDs and across different methods for determining SSD. However, it assumes that SSRT is a constant, so that any go RTs that finish before the stop process (SSD + SSRT) will be executed, and any go RTs that finish after the stop process (SSD + SSRT) will be inhibited, see Equation 2. When the integral equals the p(respond|stop signal), t =

t_d + SSRT, so SSRT can be estimated by subtracting t_d from t. In practice, computing the integration SSRT involves rank ordering all N no-stop-signal RTs from fastest to slowest, then finding the Mth go RT where $M = N$ x p(respond|stop signal), and subtracting SSD from the Mth go RT. Previous work using mathematical analyses (Logan & Cowan, 1984) and simulations (Band, van der Molen, & Logan, 2003; De Jong, Coles, Logan, & Gratton, 1990) has shown that assuming that SSRT is a constant does not generally bias estimates of SSRT.

The third method, the distribution method, involves calculating the full distribution of SSRTs based upon the observed no-stop-signal and observed stop-failure RT distributions (Colonius, 1990; De Jong et al., 1990). Equation 3 can be rearranged to solve for the full distribution of SSRTs at a given SSD:

$$F_{stop}(t - t_d) = 1 - f_{sf}(t|t_d)\ P_r(t_d)/f_{go}(t). \tag{9}$$

This is a non-parametric approach to estimating the SSRT distribution, so the SSRT and go RT distributions can take any form. However, this estimate is strongly affected by the tails of the observed no-stop-signal and stop-failure RT distributions. Stop-failure RTs occur infrequently (a minority of trials are stop trials, and only a subset of stop trials are stop-failure trials), so typical experiments do not gather enough trials to get reliable estimates of the SSRT. Matzke, Dolan, Logan, Brown, & Wagenmakers (2013) simulated stop signal data and showed that over 250,000 stop trials per SSD were necessary to obtain accurate estimates of SSRT distributions with this method.

A fourth class of methods, the parametric class of methods, involves making the assumption that the go and stop finishing time distributions take a specific parametric form (Logan et al., 2014; Matzke et al., 2013). By assuming a specific parametric form (like ex-Gauss in Matzke et al., 2014 or Wald in Logan et al., 2014), estimates of the full SSRT distribution can be computed with much less data than is necessary with the distribution method (Matzke et al., 2013 computed accurate SSRT estimates with only 125 stop signal trials per participant). However, these parametric methods are not as general as the previous three non-parametric methods. The parametric methods assume specific processes that give rise to distributions of specific forms. These assumptions could be wrong. The previous three methods that were derived from the original Independent Race Model (Logan & Cowan, 1984) make no parametric assumptions, so they apply to the ex-Gauss, Wald, and every other distribution that the go and stop finishing time distributions could take.

CHAPTER II

METHODS FOR EVALUATING CONTEXT INDEPENDENCE

The Influence of Violations of Independence on Stop Task Measures

Violations of context independence influence each of the three main dependent measures in the stop signal paradigm: stop-failure RT, SSRT, and inhibition functions. Violations of go context independence, by definition, slow the go process on stop trials compared to the go process on no-stop-signal trials. Therefore, violations of context independence influence stop-failure RT. If the go process differs between stop and no-stop-signal trials, then the full no-stop-signal RT distribution can no longer approximate the underlying go RT distribution on stop signal trials. An estimate of the underlying go process on stop trials is necessary in order to calculate SSRT. Therefore, violations of context independence influence SSRT. Prolonging the go process on stop trials also influences the inhibition function. The inhibition function plots the p(respond|stop signal) against SSD, so it is a measure of the relative finishing times of the go and the stop processes. When context independence is violated, the go or the stop process is slowed, adjusting the relative finishing time of going and stopping. Therefore, violations of context independence influence the inhibition function.

The preceding paragraph shows that violating context independence contaminates all three main dependent variables in the stop signal paradigm. Therefore, evaluating and understanding violations of context independence are important for every stop signal dataset.

Methods for Diagnosing Independence

Independence is often evaluated by looking at some combination of observed stop-failure RTs, predicted stop-failure RTs, and observed no-stop-signal RTs. The usual test involves comparing mean no-stop-signal RT to mean stop-failure RT, and if mean stop-failure RT is significantly faster than mean no-stop-signal RT then independence is assumed. This is a test of independence because the race model predicts that stop-failure RT will be less than no-stop-signal RT for all SSDs in which p(respond|stop signal) < 1. This can be seen in Equation 3, because the $(1 - F_{stop}(t - t_d))$ term filters out the upper tail of the go RT distribution $f_{go}(t)$. This is the case except when t_d is infinite. In that specific case the equation simplifies to $f_{sf}(t|t_d) = f_{go}(t)$.

However, there is a significant problem with testing independence by comparing stop-failure RT to no-stop-signal RT. Though this method correctly identifies violations of context independence when stop-failure RT is as long or longer than no-stop-signal RT, violations of independence can occur even if stop-failure RT is significantly less than no-stop-signal RT. This is because the race model predicts how much faster the stop-failure RTs should be than the no-stop-signal RTs at each SSD. As shown in Equation 3, the longer the stop signal delay, the less the go RT distribution is filtered at a given value of *t.* Therefore, if SSDs are long (and p(respond|stop signal) is high) the race model predicts a small difference between stop-failure RT and no-stop-signal RT. In contrast, if SSDs are short (and p(respond|stop signal) is low) the race model predicts a large difference between stop-failure RT and no-stop-signal RT. Therefore, this technique is biased towards concluding that the race model is violated when

SSDs are long and p(respond|stop signal) is high, and biased toward concluding independence when SSDs are short and p(respond|stop signal) is low.

There is an alternative method for evaluating the independence between going and stopping that involves comparing observed stop-failure RTs to the stop-failure RTs that are predicted by the race model (Logan & Cowan, 1984). Equations 3 and 4 provide expressions for the distribution of stop-failure RTs, Equation 5 provides its mean, and Equation 6 provides its median. Predicted stop-failure RT estimates can be compared to observed stop-failure RTs at a given SSD, and if these values differ, then there is significant evidence for a violation of independence.

This prediction method for diagnosing violations of context independence overcomes the problems with comparing observed stop-failure and observed no-stop-signal RT. The race model not only predicts that stop-failure RTs will be faster than no-stop-signal RTs, but it also predicts how much faster. The prediction method tests this more specific, diagnostic hypothesis that the observed data equals what is predicted by the race model. Additionally, there is no bias towards finding violations of the race model at long SSDs but not short SSDs. This is because the predictions are based upon the p(respond|stop signal) at specific SSDs, which will tend to produce long predicted stop-failure RTs at the long SSDs that produce many stop-failures and short predicted stop-failure RTs at the short SSDs that produce few stop-failures.

Violations of independence influence stop-failure RT, SSRT, and the inhibition function, but SSRT and the inhibition function are not as useful as stop-failure RT for evaluating independence. If there is evidence for violations of go context independence, then SSRT estimates are invalid. To compute SSRT, it is necessary to have an estimate of the distribution of go processes that race against the stop process. If there is context independence, then the no-stop-signal RT distribution can be assumed to be the same as the distribution of go processes on stop trials. However, if the assumption of context independence is violated then there is no available estimate of the underlying go distribution, so there is no valid way to compute SSRT.

Even if there is no evidence for violations of go context independence, the race model can accommodate longer SSRTs at short SSDs if variability in SSRT is assumed (Logan & Burkell, 1986; Logan & Cowan, 1984). At shorter SSDs, the race is biased in favor of the stop process, so most stop processes (both fast and slow) tend to win the race. As SSD gets longer, the race becomes more biased in favor of the go process, and only the faster stop processes can win the race. This is the stop-process analogue of the reduction in stop-failure RTs as SSD gets shorter. In the case of the stop process, progressively fewer and faster stop processes will win as SSD gets longer, and in the case of the go process, progressively fewer and faster go process will win as SSD gets shorter. Therefore, the race model can explain SSRT estimates that decrease as SSD increases without assuming violations of context independence. Hence, longer SSRT estimates at short SSDs could result from violations of the context independence assumption of the race model or from variability in SSRT, limiting SSRT as a diagnostic tool for evaluating violations of independence.

The inhibition function is also not always diagnostic of violations of independence. Violations of go context independence involve slowing of the go process and violations of stop context independence involve slowing of the stop process. At a given SSD in the inhibition function, if the go process is slowed more than the stop process, p(respond|stop signal) will be lower, because fewer go processes will win the race. Alternatively, if both the go process and the stop process are slowed equally, p(respond|stop signal) will not change, because the relative finishing times of the go and stop processes do not change. Finally, if the stop process is slowed more than the go process, p(respond|stop signal) will be higher, because fewer stop processes will win the race. These three possibilities show that a violation of context independence can shift the inhibition function up or down or leave the inhibition function unchanged. Additionally, ignoring the stop signal on a subset of trials, which have been called "trigger failures" (Band et al., 2003; Logan & Cowan, 1984), could mimic the effect of slowing the stop process at a given SSD, resulting in higher p(respond|stop signal). Therefore, violations can have variable effects on the inhibition function and can be mimicked by other processes (e.g., trigger failures).

Therefore, I evaluate violations of context independence by comparing predicted stop-failure RTs to observed stop-failure RT. A difference between these measures suggests a violation. SSRT and the inhibition function are not diagnostic of violations of context independence.

Assumptions about SSRT and Their Influence on Predicted Stop-Failure RTs

In order to test whether the context independence assumption of the Independent Race Model (Logan & Cowan, 1984) is violated at short SSDs, I argued that the best method involves comparing predicted stop-failure RT to observed stop-failure RT. I have presented two ways to predict stop-failure RTs, one involves assuming that SSRT is a constant (Equation 4) and the other does not (Equation 3). The method shown in Equation 3 requires estimating the full distribution of stop finishing times, F_{stop}, in order to predict which observed no-stop-signal RT would be filtered by the stop process. In principle, the distribution method for SSRT computation (Colonius, 1990; De Jong et al., 1990) allows estimation of F_{stop}, however this has been shown to be inaccurate unless extremely large amounts of data are acquired (250,000 trials at each SSD, see Matzke et al., 2013). Therefore, the distribution method is not practical to evaluate the violations of independence in the datasets presented below (or possibly any stopping dataset ever published).

Predicted stop-failure RTs can be computed by assuming that SSRT is a constant, yielding the predicted distribution (Equation 4), the predicted mean (Equation 5), and the predicted median (Equation 6). The main advantages of this first method are it is general and simple, but it has the disadvantage of assuming that SSRT is a constant, which must not be true. It is general because it does not parameterize the go or stop finishing time distribution, and can account for data produced by any distribution. It is simple because once the quantile of the go RT distribution that corresponds to the p(respond|stop signal) is computed, all go RTs faster than the RT at that quantile are the predicted stop-failure RTs. However, the assumption that SSRT is a constant may contaminate predicted stop-failure RT estimates. If SSRT has variability (and like any other reaction time it must), then there is not a single point with which all faster go processes win the race and all slower go processes lose. When SSRT happens to be slow then there will be observed stop-failure RTs longer than what are predicted by the integration method. Therefore, the predicted stop-failure RT distribution is truncated at SSD + SSRT, but the observed stop-failure RT distribution is not. This results in underprediction of stop-failure RT if SSRT is assumed to be a constant.

A second method is to use parametric methods (Logan et al., 2014; Matzke et al., 2013) to capture the go RT and SSRT distributions. There parametric methods have the advantage that they do not assume that SSRT is a constant, but they lack the generality and simplicity of the integration method. The go and stop processes have variability when predicting stop-failure RT, so the predicted stop-failure RT distribution is not truncated like in the integration method. However, as discussed above, the parametric methods cannot be generalized beyond their assumed finishing time distributions (e.g., ex-Gauss, Wald). The parametric methods also requires complicated model fitting.

Therefore, the first method is simpler and more general but it has the shortcoming of assuming that SSRT is a constant, which must not be true. To distinguish between these two methods, simulations below assume independence between going and stopping as well as variability in go RT and SSRT. The simulations test the degree to which predicting stop-failure RT with the assumption that SSRT is a constant underpredicts the "observed" stop-failure RT. If the underprediction is small, the first method will be used because of its generality and simplicity. If the underprediction is large, the first method would not be diagnostic of violations of independence, because even simulated data with independence shows violations of independence. Therefore, the more complicated parametric approach would be necessary. To foreshadow, the simulations revealed small underpredictions of observed stop-failure RT (especially when comparing predicted and observed stop-failure RT with medians, see below), suggesting that the assumption that SSRT is a constant does not strongly influence the accuracy of the prediction. Therefore, predicted stop-failure RTs are computed with the assumption that SSRT is a constant.

Evaluating Median and Mean Stop-Failure RTs as Measures of the Violation

One way to quantify observed and predicted stop-failures is with a measure of central tendency. Ideally, the measure of predicted stop-failure RT would perfectly match the observed stop-failure RT in a dataset in which it is known that there is independence between going and stopping. Therefore, any deviation from equality in an experimental dataset could be more confidently ascribed to the violation of context independence and not another source of prediction error.

Previous work by Band et al. (2003) argued that observed minus predicted stop-failure RTs “should not be used as a test of the independence assumption underlying the horse-race model” (p. 136). They simulated a large amount of data that included independence between going and stopping and tested whether predicted values differed from observed values. They showed that predicted values underestimate observed values, especially at short SSDs, when the go process is less variable, and when the stop process is more variable. Importantly, they compared the observed mean and predicted mean stop-failure RTs (see Equation 5).

Band et al. (2003) assumed that SSRT is a constant, as I did in the following simulations. As discussed above, this may lead to underpredictions of stop-failure RT, because if there is variability in the stop process then the predicted stop-failure RT distribution will be truncated at SSD + SSRT but the observed stop-failure RT distribution will have some stop-failure RTs longer than SSD + SSRT. Means will be more sensitive to long RTs that would be included in the observed stop-failure RT distribution but not in the predicted stop-failure RT distribution, so comparing median observed and predicted may reduce the prediction errors. Estimating the median is easy (see Equation 6). With the assumption that SSRT is a constant, the predicted median is the quantile at ½ * $P_r(t_d)$.

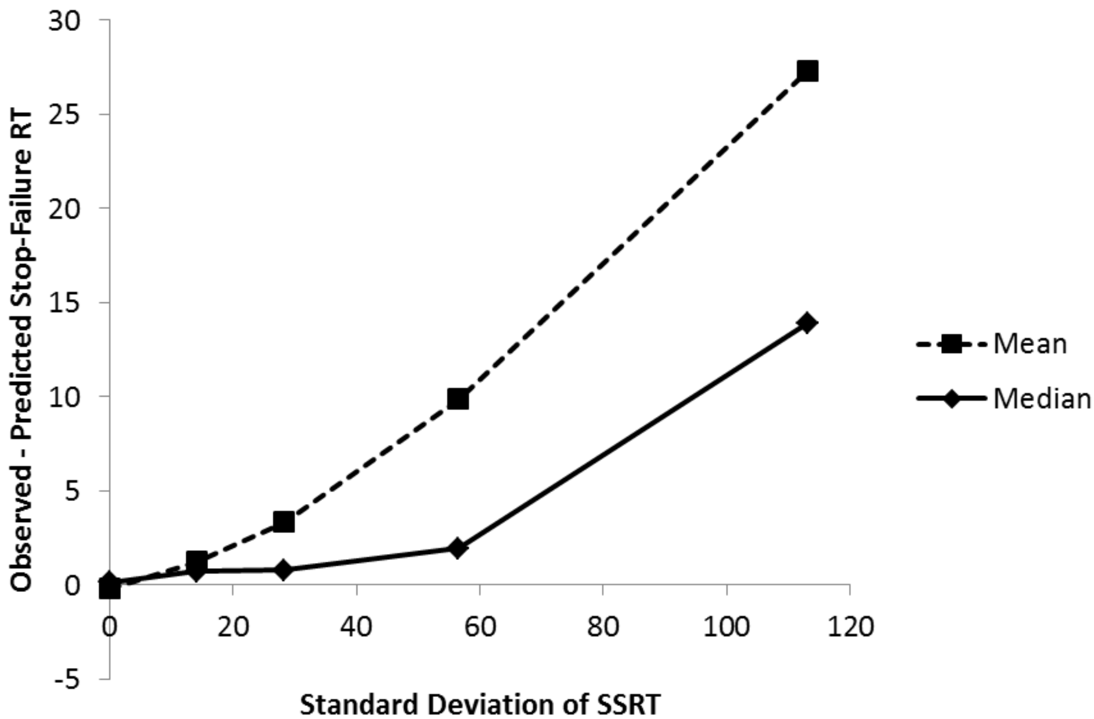
The following simulations tested the influence of assuming SSRT is a constant on predicted stop-failure RT by simulating data with go and stop variability but independence between going and stopping. If the prediction error is smaller in medians than in means, this supports using medians to test for violations of context independence to reduce measurement error. Also, if the prediction errors in medians are small, then this shows that the assumption that SSRT is a constant only has a small effect on the prediction error of medians. Relating to the previous section, this would support using the integration method (that assumes SSRT is a constant) to predict stop-failure RTs, as this method has the advantages of generality across all distributions of go and stop processes as well as the advantage of simplicity.

Simulated Data with Independence

Method

To compare prediction error in the mean and median predictions, I simulated new data (that included context independence between going and stopping) with various degrees of variability in the stop process. For the go process, one set of ex-Gaussian parameters were used that were the average of the parameters used by Band et al. (2003), Matzke et al., (2013), and Verbruggen et al. (2013) (μ = 420, σ = 80, τ = 103). For the stop process, the same mean was used across all estimates (250) but a range of variability was used. Initially, σ and τ values of 0, 10, 20, 40, and 80 were manipulated factorially. σ and τ had similar effects on predictions errors, with both tending to increase prediction errors to a similar degree, so I only report prediction errors (see Figure 1) when sigma and tau are equal (σ and τ both equal to 0, 10, 20,

Figure 1. Simulations of observed stop-failure RT – predicted stop-failure RT with context independence. Median predicted stop-failure RTs are more accurate than mean predictions, and there is little difference between observed and predicted median stop-failure RTs until very large standard deviations of SSRT.



40, and 80 which produce standard deviations of 0, 14, 28, 57, and 113, respectively). 300,000 go trials and 200,000 stop trials were simulated for each set of stop parameters, and SSD was tracked. Then the observed stop-failure RT at the most frequent SSD was compared to the predicted stop-failure RT at the most frequent SSD when using all no-stop-signal trials as the underlying go process. The most frequent SSD was used because noise due to measurement error is reduced at the most frequent SSD.

Results and Discussion

The difference between the observed and predicted stop-failure RT was smaller when comparing medians than comparing means (see Figure 1). This suggests that at low to moderate standard deviations in the stop process (<=55 ms), there is almost no evidence of underprediction of stop-failure RTs in median estimates. At very large variability, medians reduce the underprediction to half the underprediction of means. This supports the median as the better measure of central tendency for comparing observed and predicted stop-failure RTs. This also shows that when SSRT is assumed to be a constant, there is minimal prediction error when estimating central tendency with the median. Therefore, I assumed that SSRT is a constant for predicting stop-failure RTs throughout the dissertation.

Comparing Violations Across the Distribution of Observed and Predicted Stop-Failure RTs

The previous section argues that comparing the median observed and predicted stop-failure RT provides a measure of the violation in central tendency that is minimally influenced by measurement error. However, focusing on only the central tendency of a distribution of RTs can lose valuable information about the rest of the distribution (Balota & Yap, 2011; Ratcliff & Murdock, 1976; Van Zandt, 2002).

Therefore, in addition to the analysis of the median violation, I analyzed violations across the distribution of observed and predicted stop-failure RTs. If the prediction is perfect then each quantile of the predicted stop-failure RT should equal each respective quantile of the observed stop-failure RT, and deviation from equality is evidence of a violation of independence. To evaluate the goodness of fit of the predicted stop-failure RT distribution at a given SSD to the observed stop-failure RT distribution at that SSD I used quantile-quantile (Q-Q) plots (Ratcliff, Spieler, & McKoon, 2000; Thomas & Ross, 1980). A Q-Q plot is a graphical method for comparing two distributions by plotting their quantiles against each other. If the two distributions are equal to each other, the Q-Q plot will be linear with a slope of 1 and an intercept of 0. According to the race model (Logan & Cowan, 1984), the predicted stop-failure RT distribution should equal the observed stop-failure RT distribution.

To find the predicted stop-failure RTs, the part of the full distribution of no-stop-signal trials that would beat the stop process (i.e., the portion identified on the right side of Equation 2) is separated out (RT < SSD + SSRT). Then each observed stop-failure RT is plotted against the predicted stop-failure RT from the quantile of the predicted stop-failure RT distribution that corresponds to it. For example, if there are eight stop-failure trials at that SSD for that subject, then the fastest observed stop-failure RT is plotted against the 11^{th} percentile of the RT < SSD + SSRT distribution, where .11 is computed from (1/(8+1)). This is done for all eight observed stop-failure until the eighth observed stop-failure RT is plotted against the 89^{th} percentile of the RT < SSD + SSRT distribution (8/(8 +1)).

With each Q-Q plot, a linear regression slope and intercept can be computed. The slope depends on the ratio of standard deviations of the two distributions (σ_o / σ_p), so if the observed distribution has greater variability than the predicted, the slope will be greater than 1, which would be evidence of a violation in dispersion. The intercept = $\mu_o - \mu_p\sigma_o/\sigma_p$, and therefore is influenced by both the mean and the dispersion of the distributions. I focus on the slope and not the intercept, as the intercept can be derived from mean and slope, so provides little additional information (as discussed above, I use median instead of mean as it provides a less biased estimate of central tendency).

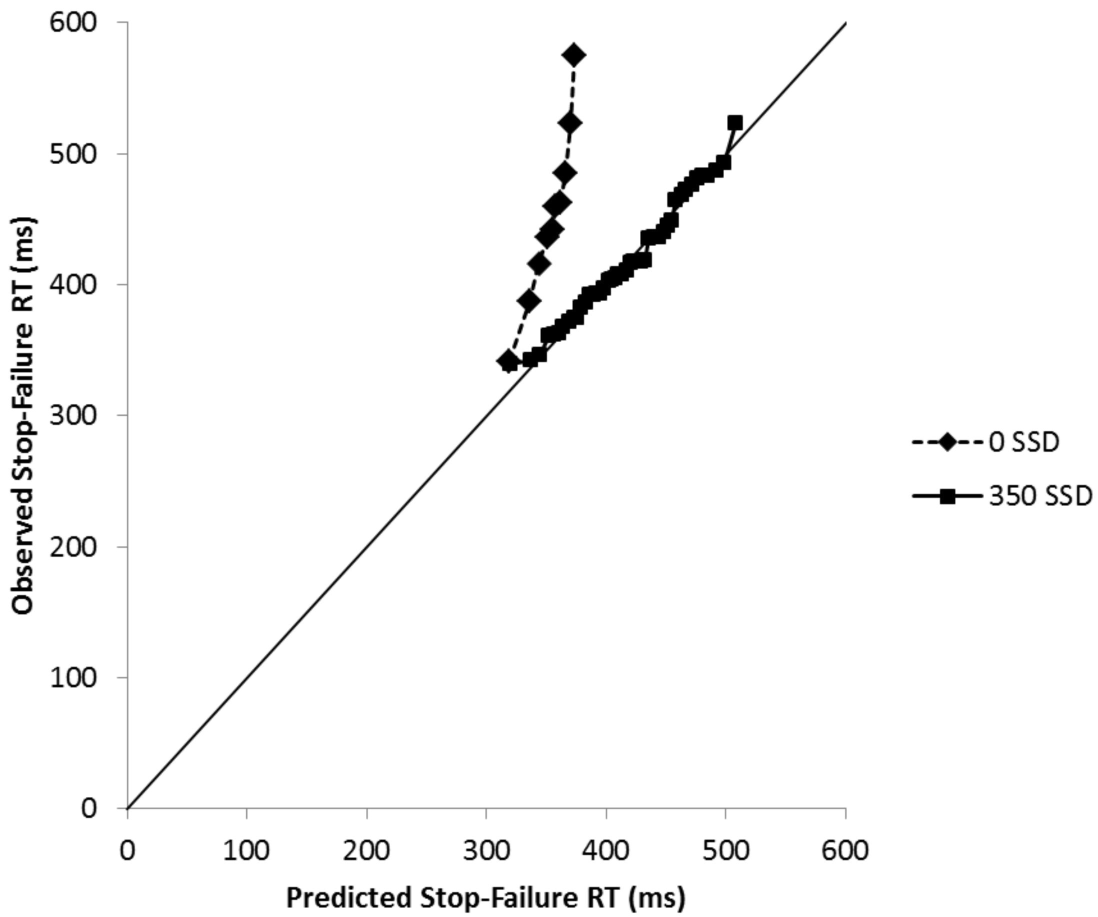
Figure 2 shows example Q-Q plots for a subject from the Fixed SSDs 2 dataset (detailed below) at a short SSD of 0 ms and a longer SSD of 350 ms. The slope was much steeper than 1 at 0 SSD (3.8) revealing much more dispersion in the distribution of observed stop-failure RTs than predicted stop-failure RT at this SSD, a sign of a violation of context independence. The slope was very similar to 1 at 350 SSD (.96) which is a sign that the race model predicts the dispersion of the observed stop-failure RTs well at this subject's 350 ms SSD but not their 0 ms SSD.

Observed stop-failure RTs having either a longer central tendency or greater dispersion than predicted stop-failure RT is evidence that the observed stop-failure distribution is different from the predicted stop-failure distribution. Hence, a longer median or a steeper slope is evidence that the context independence assumption of the Independent Race Model is violated.

Determining the Best Estimate of the Underlying Go Process at Each SSD

The prediction of stop-failure RT should be based upon the best estimate of the underlying go process at each SSD. As described above, what has been used in the past is the full no-stop-signal distribution. This does not take into account variability in go RT over time. It

Figure 2. Example Q-Q plots from Fixed SSDs 2 Subject 5 at 0 SSD and 350 SSD. Large positive linear regression slope at 0 SSD (slope = 3.8) reveals much more dispersion in the observed stop-failure RT distribution than the predicted stop-failure distribution, signifying a violation of the race model. Slope of ~1 at 350 SSD (slope = .96) reveals similar dispersion in the observed stop-failure RT as in the predicted stop-failure distribution, signifying that the race model predicts the dispersion of the observed stop-failure RT distribution well.



is well established that RTs are nonstationary and nonindependent over trials (Gilden, 2001; Wagenmakers, Farrell, & Ratcliff, 2004). Nelson, Boucher, Logan, Palmeri, & Schall (2010) showed that go RT slowly fluctuates over the course of stop signal studies, and SSD fluctuates with it when SSD is tracked. Consequently, SSD tends to be short when RT is fast and long when RT is slow. Therefore, when tracking SSD (which is done in most of the studies presented below), using the full no-stop-signal distribution to estimate stop-failure RT may overpredict stop-failure RT at short SSDs and underpredict stop-failure RT at long SSDs.

An alternative to using the full no-stop-signal distribution is to use the RTs that immediately precede stop trials at a specific SSD. This aims to capture the fluctuation in go RT, so that short SSDs are predicted based upon the fast RTs that precede them and long SSDs are predicted based upon the slow RTs that precede them. In comparison to the first selection criterion, this second selection criterion bases the predictions on fewer go RTs (only no-stop-signal RTs preceding stop trials vs. all no-stop-signal RTs).

A third selection criterion bases the prediction on the no-stop-signal RTs that immediately precede only stop-failure trials, not all stop signals as in the second selection criterion. No-stop-signal RT before stop-failure trials has been shown to be faster than no-stop-signal RT before stop-success trials (Bissett & Logan, 2011; Nelson et al., 2010). Therefore, assuming go RT on the trial before a stop signal is predictive of underlying go RT on the subsequent stop trial, then the underlying go RT on stop-failure trials should be faster than the underlying go RT on stop-success trials. This selection criterion attempts to account for this proposed difference between underlying go RT on stop-failure trials and underlying go RT on stop-success trials. One shortcoming of this third selection criterion is that it bases the predictions on an even more restrictive set of no-stop-signal trials than the second selection criterion. Therefore, the first selection criterion has the most power but the least tailoring to RT fluctuations, and the third selection criterion has the least power and the most tailoring to RT fluctuations, with the second selection criterion in between.

To compare across these three selection criteria, the median observed stop-failure RT minus the median predicted stop-failure RT across SSD is plotted in Figure 3, averaged across all the conditions from all of the studies that were analyzed in this dissertation. Figures 3a, 3b, and 3c show the prediction based upon all no-stop-signal trials, no-stop-signal trials that immediately precede stop trials at that SSD, and no-stop-signal trials that immediately precede stop-failure trials at that SSD, respectively. Figure 3a shows a large underprediction at both short (<= 200ms) and long SSDs (>=400 ms), with small underpredictions at intermediate SSDs. Figure 3b reveals a large underprediction at short SSDs which is virtually gone at SSDs >=300 ms. Figure 3c reveals a large underprediction at short SSDs but an asymptote well above 0 at intermediate and long SSDs (>=250 ms).

All three selection criteria are consistent with violations of the context independence assumption of the Independent Race Model (Logan & Cowan, 1984; Logan et al., 2014) at short SSDs. Figure 3a is the only figure that shows an increase in violation at long SSDs compared to intermediate SSDs. This is consistent with the above argument that disregarding fluctuations in RT over time results in underprediction at long SSDs. Figure 3c suggests that the race model does not apply at any SSDs. This seems unlikely, considering the literature has shown that most datasets follow the assumptions of the race model, especially if SSDs are long. Figure 3c also

Figure 3. Median observed stop-failure RT – median predicted stop-failure RT when the prediction is based upon the entire no-stop-signal RT distribution (3a), only no-stop-signal trials that immediately precede stop trials at that SSD (3b), or only no-stop-signal trials that immediately precede stop-failure trials at that SSD (3c).

Figure 3a.

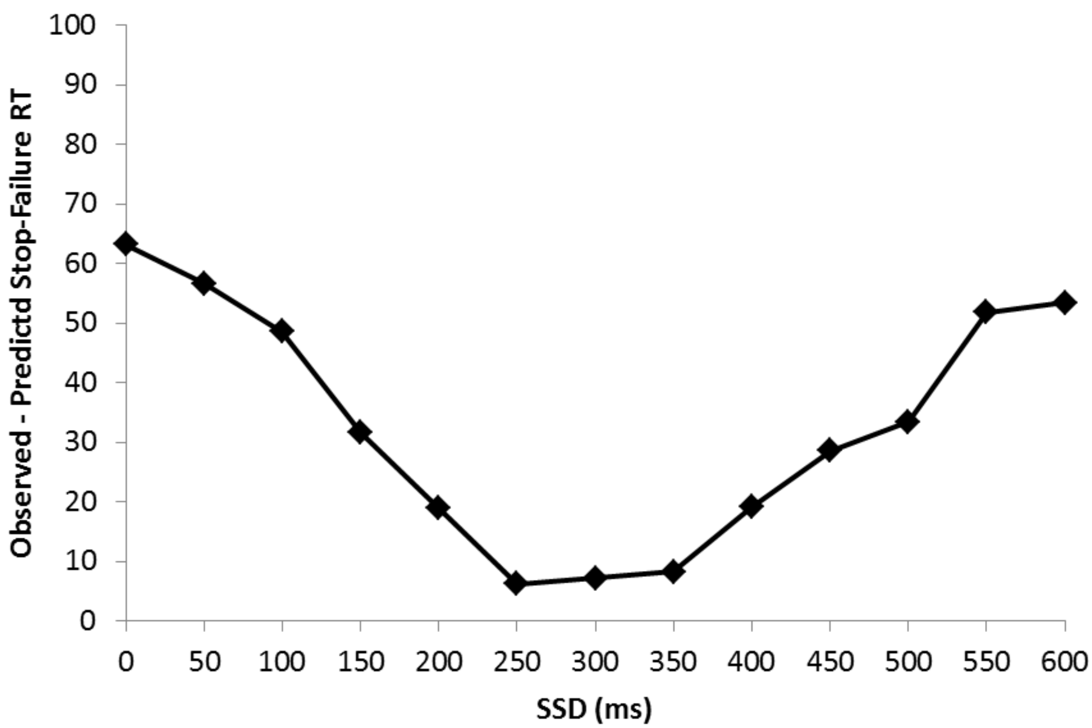
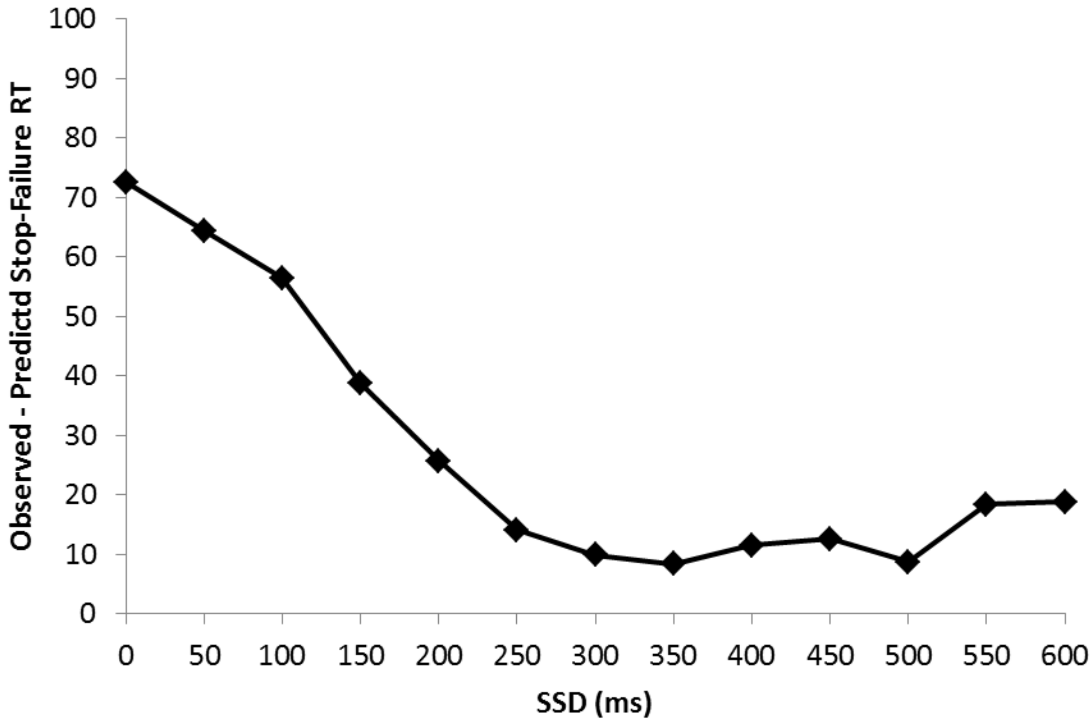
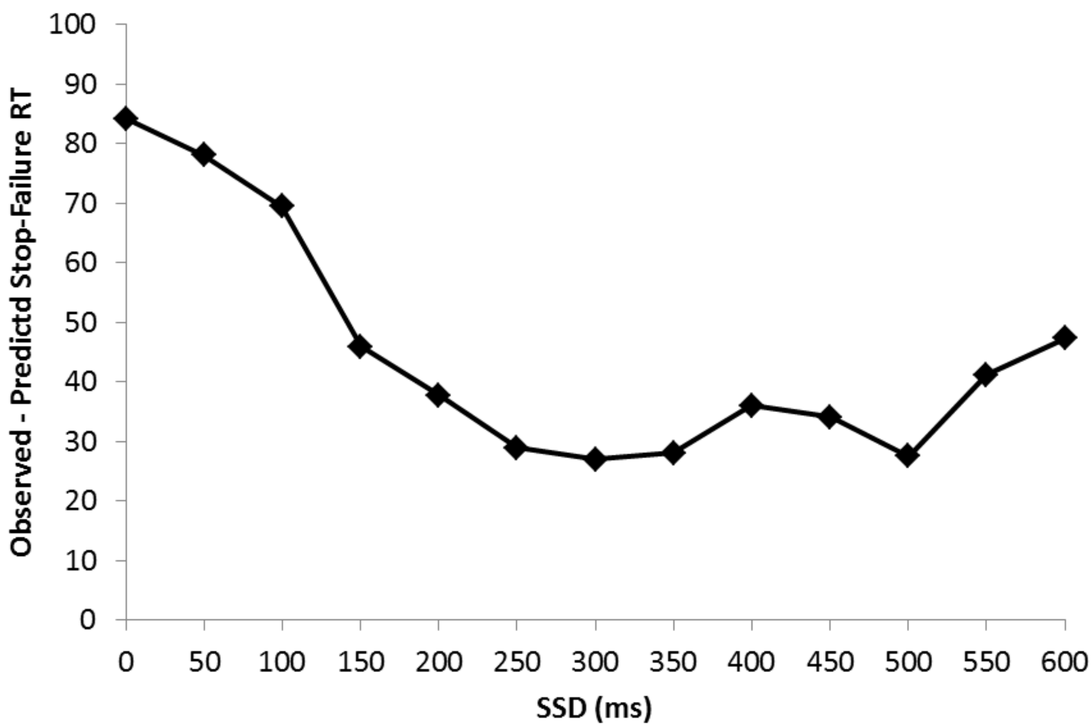


Figure 3b.



100
90
80
70
60
50
40
30
20
10
0
Observed - Predictd Stop-Failure RT
0
50
100
150
200
250
300
350
400
450
500
550
600
SSD (ms)

Figure 3c.



100
90
80
70
60
50
40
30
20
10
0
Observed - Predictd Stop-Failure RT
0
50
100
150
200
250
300
350
400
450
500
550
600
SSD (ms)

involves the smallest number of go trials used to predict stop-failures, especially at short SSDs, as it only uses trials immediately preceding stop-failures (which are rare at short SSDs). The selection criterion presented in Figure 3b accounts for fluctuations in RT over time while including more trials than the selection criterion in 3c. Therefore, the selection criterion in 3b that predicts stop-failure RTs based upon all go trials immediately preceding stop trials seems to be the best compromise between power and accuracy of prediction, so this selection criterion is used throughout the dissertation for computing medians.

The predictions for the Q-Q plots are based upon the full no-stop-signal distribution instead of only the go trials that immediately precede stop trials. This is in order to always have at least as many trials in the RT < SSD + SSRT distribution as there are observed stop-failure trials. If the RT < SSD + SSRT distribution is computed based upon only go trials that immediately proceed stop trials at a given SSD, sometimes the RT < SSD + SSRT distribution has fewer trials in it than there are observed stop-failure trials at that SSD, so a unique prediction for each observed value cannot be computed. Therefore, the full no-stop-signal distribution was used to produce the distribution of observed and predicted stop-failure RTs for the Q-Q plots.

CHAPTER III

EXPLORING VIOLATIONS OF CONTEXT INDEPENDENCE AT SHORT SSDS

In Chapter 1, I discussed the Independent Race Model for stopping. In Chapter 2, I argued for a set of methods to evaluate the context independence assumption of the race model. First, I argued for assuming SSRT is a constant to predict stop-failure RT, as this method does not require assuming a parametric form for the go or stop distributions, and new simulations presented here showed that assuming SSRT is a constant minimally influences the accuracy of predicted stop-failure RT. Second, I showed in simulations that comparing the median predicted and observed stop-failure RT (instead of the mean) reduces prediction error in data in which independence holds, supporting the median as a better estimate of central tendency when comparing observed and predicted stop-failure RT. Third, I argued that central tendency is not a sufficient measure of violations, and suggested specific methods for analyzing the full distribution of observed and predicted stop-failure RT across SSD. Fourth, I argued for predicting the underlying go process on stop-failure trials based upon the no-stop-signal RT that immediately precede stop trials at a given SSD, as this takes into account fluctuations in RT over time (Nelson et al., 2010).

In Chapter III, I show that there is evidence in the literature of long stop-failure RTs at short SSDs. I then apply the new methods from Chapter II to a large amount of data. Previous work has had a comparatively small amount of data, with few or no manipulations that test hypotheses about the violations, and have used ineffective methods (e.g., comparing observed stop-failure RT to observed no-stop-signal RT). The current work includes a large amount of data and applies the new methods described above to a variety of tasks and manipulations that address hypotheses for the violations.

Previous Work Exploring Violations at Short SSDs

Race models for stopping (Boucher, Palmeri, Logan, & Schall, 2007; Logan & Cowan, 1984; Logan et al., 2014) make predictions for stop-failure RT across SSD. Mean stop-failure RT should be fastest at short SSDs, and should approach mean no-stop-signal RT as p(respond|stop signal) approaches 1 at longer SSDs (see Equations 3 and 4). Some studies have evaluated whether these predictions are supported by data.

Hans Colonius and colleagues (Akerfelt, Colonius, & Diederich, 2006; Colonius, Ozyurt, & Arndt, 2001; Gulberti, Arndt, & Colonius, 2014; Ozyurt, Colonius, & Arndt, 2003) tested whether stop-failure RT across SSD aligned with the predictions of the Independent Race Model. Across all of their studies, they found evidence at short SSDs for stop-failure RTs that were longer than expected with the race model. In some subjects, their stop-failure RTs at short SSDs were longer than their mean no-stop-signal RTs. This suggests large violations of the context independence assumption of the Independent Race Model (Logan & Cowan, 1984; Logan et al., 2014). Across their datasets, the violations tended to occur at SSDs <= 150 ms. Gulberti et al. (2014) mentioned that long stop-failure RTs may result from stopping and going sharing common resources, but they did not specify what resources may be shared or why resources are only shared at short SSDs.

Additionally, SSRTs were found to be very long at short SSDs (Gulberti et al., 2014). However, as discussed above, if the assumption of context independence is violated then SSRT estimates from the race model are invalidated. This argues for focusing on stop-failure RTs to evaluate violations of the race model instead of SSRT.

Boucher, Stuphorn, Logan, Schall, and Palmeri (2007) also showed long stop-failure RT at short SSDs, with some longer than mean no-stop-signal RT. They did not rigorously test observed versus predicted stop-failure RT, but the violations of the race model appeared to occur at SSDs <200ms, with the strongest violations at the shortest SSDs in all effectors. They argued that on these trials, subjects successfully stopped, and then as a result of impatience in maintaining fixation, subjects responded at a later time. This explanation seems more plausible in eyes than in hands, as eyes frequently sample the environment with fast saccades, and staying fixated in one location for an extended period of time may be difficult. This explanation seems less plausible in hands, because hands are often at rest and do not frequently sample the environment by touching without the goal to do so.

Other research groups have shown mean stop-failure RTs (averaged across all SSDs) that were longer than what is predicted by the race model (De Jong, Coles, & Logan, 1995; van Boxtel, van der Molen, Jennings, & Brunia, 2001; van den Wildenberg, van der Molen, & Logan, 2002; Verbruggen, Liefooghe, & Vandierendonck, 2004), but they did not evaluate stop-failure RT at specific SSDs. Jennings, van der Molen, Brock, & Somsen (1992) showed predicted stop-failure RTs shorter than observed at the 50 ms SSD but not the 150 ms SSD. Logan & Cowan (1984) showed in three subjects that predicted stop-failure RTs were longer than observed stop-failure RTs at SSDs <= 150 ms. Some datasets showed good fits between observed and predicted stop-failure RT (De Jong et al., 1990; Osman et al., 1986), but they did not sample short SSDs (i.e., most subjects had minimum SSDs > 200 ms). Some work has shown that stop-failure RT was faster than no-stop-signal RT (e.g., Camalier et al., 2007, who used step variants of the stop signal task), but as argued above, this comparison is weak.

There is also some evidence that stop-failure RTs are very long at short SSDs in monkeys. For example, Hanes and Schall (1995) showed that at their shortest SSD (25 ms), stop-failure RT were considerably longer than no-stop-signal RT for both of the monkeys that were tested. In one of the monkeys, stop-failure RT at the second shortest SSD (50 ms) was longer than predicted but not longer than no-stop-signal RT. There were very few stop-failure trials at these short SSDs, so this is not strong evidence against the race model. Stop-failure RT for SSDs >=75ms appeared to follow the predictions of the race model. One challenge with this dataset is that the stop signal was the reappearance of the fixation point, so stop signals presented with short SSDs (and especially 0 SSDs) may have been difficult to see.

The evidence for violations of independence have been largely dismissed by the authors and not taken as strong evidence against the race model. The strongest justification for this dismissal is simulated stop data that showed that as SSRT variability increased (and go RT variability decreased), predicted stop-failure RT progressively underestimated observed stop-failure RT (Band et al., 2003). Therefore, underpredictions of stop-failure RT can be explained without assuming context dependence. However, Equation 4 shows that stop-failure RT should progressively increase with SSD (also see the explanation following Equation 4), which is sometimes not observed at short SSDs (Akerfelt et al., 2006; Colonius et al., 2001; Gulberti et al., 2014; Logan & Cowan, 1984; Ozyurt et al., 2003). Additionally, variability in SSRT or go RT cannot explain stop-failure RT that are longer than no-stop-signal RT (Akerfelt et al., 2006; Colonius et al., 2001; Gulberti et al., 2014; Ozyurt et al., 2003). For stop-failure RT to be longer than no-stop-signal RT, the underlying go process must be slowed on stop trials compared to go trials (i.e., context dependence), which invalidates the Independent Race Model (Logan & Cowan, 1984; Logan et al., 2014).

Taken together, these results suggest that at the shortest SSDs (<=50ms), stop-failure RTs appear to be prolonged in eyes and hands and in monkeys and humans. Some human subjects show violations at SSDs as long as 100-150ms, and there was little evidence of violations at SSDs > 200ms.

What Variables Influence the Failure of Context Independence? Stop signal tasks are all similar in that subjects engage a response on all trials but occasionally need to stop that response when a stop signal occurs. However, stop signal tasks can differ in many ways. The modality of the go stimulus is almost always visual, but the modality of the stop signal is often auditory (e.g., Logan & Cowan, 1984) but can also be visual (e.g., Lappin & Eriksen, 1966). As discussed above, subjects can respond with different effectors, with the most common being keypress responses with hands (Logan & Cowan, 1984) and saccadic eye movements (Hanes & Schall, 1995). Additionally, stop tasks can be simple or selective. In simple stopping, subjects stop all responses when one and only one stop signal occurs. In stimulus selective stopping (Bedard et al., 2002; Bissett & Logan, 2014), two different signals can be presented on a trial, and subjects must stop if one of them occurs (stop signal), but not if the other occurs (ignore signal). In motor selective stopping (Aron & Verbruggen, 2008; Logan, Kantowitz & Riegler, 1986), subjects stop some of their responses but not others.

In the literature, the small number of studies that focused on stop-failure RT at short SSDs makes it difficult to determine whether any of these variables influence the violation of context independence at short SSDs. If they do, this will help to tailor methodological recommendations for how to reduce or eliminate the failure of context independence (e.g., if the violation is smaller with visual stop signals than with auditory stop signals then use visual stop signals if interested in applying the race model). Also, understanding how these variables impact the violation of context independence may help to inform the mechanisms underlying it. For example, if the violation of context independence is stronger when go and stop stimuli are presented within the same modality, this would be consistent with going and stopping sharing modality-specific resources (Wickens, 1980).

Applying New Methods to Determine which SSDs Violate Context Independence

The Dataset and General Analyses Across All Experiments. There is evidence that the context independence assumption of Race Models (Boucher, Palmeri, et al., 2007; Logan & Cowan, 1984; Logan et al., 2014) is violated at short SSDs. However, some of the evidence is based upon a small number of studies often running a small number of subjects with atypical responses for humans (eyes/arm movements and not manual responses, Akerfelt et al., 2006; Boucher, Stuphorn, et al., 2007; Colonius et al., 2001; Gulberti et al., 2014; Ozyurt et al., 2003). Other experiments either compare observed stop-failure RT to no-stop-signal RT, which I argued to be a weak test of the independence assumption of the race model, or base the prediction on mean stop-failure RT, which I showed has considerably more bias than median stop-failure RT.

In order to validate and delineate the violations at short SSDs, I completed an analysis of data that I have acquired at Vanderbilt. This work was an improvement over previous work investigating violations because it includes: a larger set of subjects and data than previous work, tasks common in the literature (e.g., keypress responses), manipulations that address hypotheses about causes of the violation, and the new methods described above. This analysis was aimed at delineating the effects of SSD and other frequently manipulated factors on the violation. The dataset included 24 conditions from 14 sets of subjects for a total of 323 subjects and 471,240 trials (see Table 1, which lists the conditions in the order in which they appear in the dissertation). Some of the data have already been published (see final column of Table 1), but other data has not and are presented as new experiments.

In each condition, I computed both predicted and observed stop-failure RT for each subject at each SSD for which they have at least five stop-failure trials. Cutoff points smaller than five resulted in very noisy individual subject data, and cutoff points larger than five eliminated too many subjects. This resulted in only a subset of subjects contributing to the group average at a given SSD, because only a subset of subjects have five stop-failure trials at any given SSD.

First, I use descriptive statistics to examine the range of SSDs under which the violation occurred. This orients the more focused analyses that investigated the effect of go speed, stop stimulus modality, and stop selectivity, because these subsequent comparisons focus on the range of SSDs identified as the range under which violations often occurred.

At Which SSDs Does the Violation of Independence Occur? This analysis was foreshadowed in Figure 3b, which averaged the violation in central tendency (median observed stop-failure RT – median predicted stop-failure RT) across all of the studies and plotted it against SSD. Figure 4 shows the same average as in Figure 3b, but also plots each constituent condition that makes up the average. For each condition, an SSD is included in the figure

Table 1. Information on reanalyzed datasets. Study # links the information here to the numbers in Figures 4 and 5. N is the number subjects. Trial N is the number of trials per subject in that study or condition. Go Stim is the set of go stimuli for that study. Stop Stim is the set of stop stimuli for that study. Published? tells whether the data have been previous published, and if no, in which paper.

Study #	Experiment	N	TrialN	Go Stim	Stop Stim	Published?
1	Fixed SSDs 1	24	1200	triangle-circle-square-diamond	500Hz	No
2	Fixed SSDs 2	24	2400	triangle-circle-square-diamond	500Hz	No
3	Deadline 1 300ms	24	480	triangle-circle-square-diamond	500Hz	No
4	Deadline 1 500ms		480	triangle-circle-square-diamond	500Hz	
5	Deadline 1 700ms		480	triangle-circle-square-diamond	500Hz	
6	Deadline 2 300ms	24	480	triangle-circle-square-diamond	500Hz	No
7	Deadline 2 500ms		480	triangle-circle-square-diamond	500Hz	
8	Deadline 2 700ms		480	triangle-circle-square-diamond	500Hz	
9	Stop Probability .2	24	1200	triangle-circle-square-diamond	500Hz	Bissett & Logan (2011) E. 1
10	Stop Probability .4		1200	triangle-circle-square-diamond	500Hz	
11	Saccadic Eye Movements	11	600	X right or left	500Hz, 750Hz, or 1000Hz (counterbalanced across subjects)	No
12	Between-Subjects Modality Auditory 1	24	1200	triangle-circle-square-diamond	500Hz	Bissett & Logan (2012b) E. 1
13	Between-Subjects Modality Auditory 2	24	1200	triangle-circle-square-diamond	500Hz & 750Hz	Bissett & Logan (2012b) E. 2
14	Between-Subjects Modality Visual 1	24	1200	triangle-circle-square-diamond	Orange & lue colored star around shape	Bissett & Logan (2012b) E. 3
15	Between-Subjects Modality Visual 2	24	1200	triangle-circle-square-diamond	Black bar above & black bar below shape	Bissett & Logan (2012b) E. 4
16	Within-Subjects Modality Auditory 1	24	1200	triangle-circle-square-diamond	500Hz	Bissett & Logan (2012b) E. 5
17	Within-Subjects Modality Visual 1			triangle-circle-square-diamond	Orange colored star	
18	Within-Subjects Modality Auditory 2	24	1200	triangle-circle-square-diamond	500Hz or 750Hz	Bissett & Logan (2012b) E. 6
19	Within-Subjects Modality Visual 2			triangle-circle-square-diamond	Bar above or bar below shape	
20	Between-Subjects Stimulus Selective Stop	24	1200	triangle-circle-square-diamond	500Hz & 750Hz (one stop one ignore)	Bissett & Logan (2014) E. 1
21	Within-Subjects Central Go Simple Stop	24	520	Bracket right or left	500Hz & 750Hz (both stop)	No
22	Within-Subjects Central Go Selective Stop		520	Bracket right or left	500Hz & 750Hz (one stop one ignore)	
23	Within-Subjects Peripheral Go Simple Stop		520	X right or left	500Hz & 750Hz (both stop)	
24	Within-Subjects Peripheral Go Selective Stop		520	X right or left	500Hz & 750Hz (one stop one ignore)	

(and the grand average black trace) if at least five subjects had at least five stop-failure trials at that SSD. Therefore, not every study or every subject contributes to the average at each SSD. This plot shows that the violation is large at 0 SSD and reduces until reaching an asymptote at 300 ms. Some studies reached the asymptote earlier, but there is very little deviation from zero prediction errors after the 300 ms SSD. Likewise, the slope of the Q-Q plots suggests that violations in dispersion decrease until 300 ms, then asymptote close to the predicted slope of 1 after 300 ms (see figure 5).

To test for a significant violation at a given SSD across studies, 95% confidence intervals were constructed around the mean of the differences between observed and predicted median stop-failure RTs for each study at each SSD. The confidence interval was the mean plus or minus 1.96 times the standard error of the mean. If the 95% confidence interval included zero then that study at that SSD was categorized as not violating independence. If zero fell outside the 95% confidence interval then that study at that SSD was categorized as violating independence. Figure 6 shows that almost all studies show violations at very short SSDs (<=150 ms), and very few violations at SSDs >=300 ms.

This analysis brings into question the results for any stop signal experiment that included SSDs < 300 ms. If researchers are interested in applying the race model to their data, this analysis suggests that SSDs < 300 ms should be avoided. Implications and suggestions are further explored in the General Discussion.

I restricted all subsequent analyses of violations of context independence to SSDs < 300ms, as this is the range under which the violation often occurs. When I compared the violation across conditions, I only included an SSD if it was < 300ms and if all conditions being

Figure 4. Median observed – median predicted stop-failure RT across all conditions. See Table 1 to link the numbers in the legends to specific studies/ conditions.

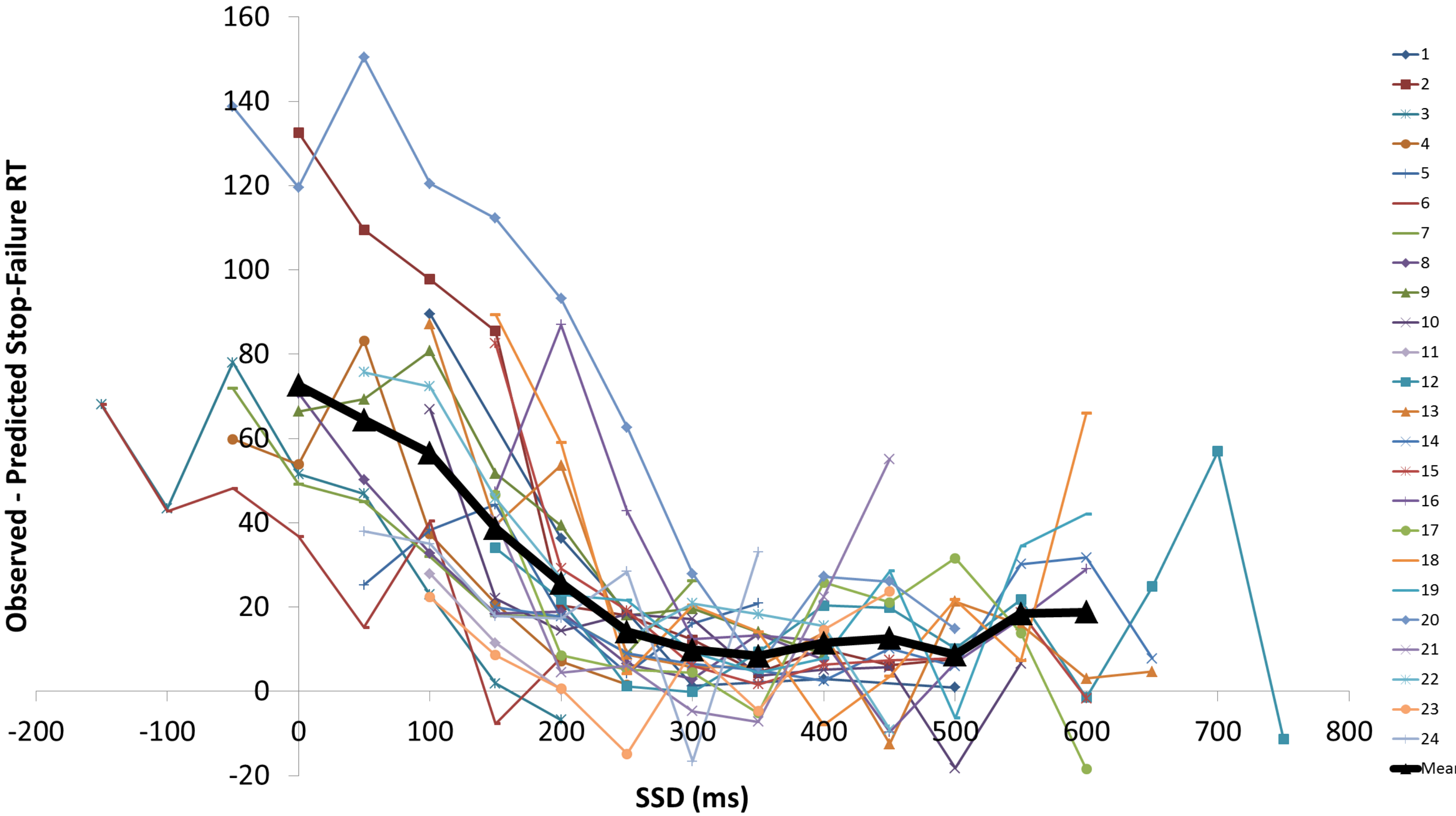


Figure 5. Slope of Q-Q plots across studies. See Table 1 to link the numbers in the legends to specific studies/conditions.

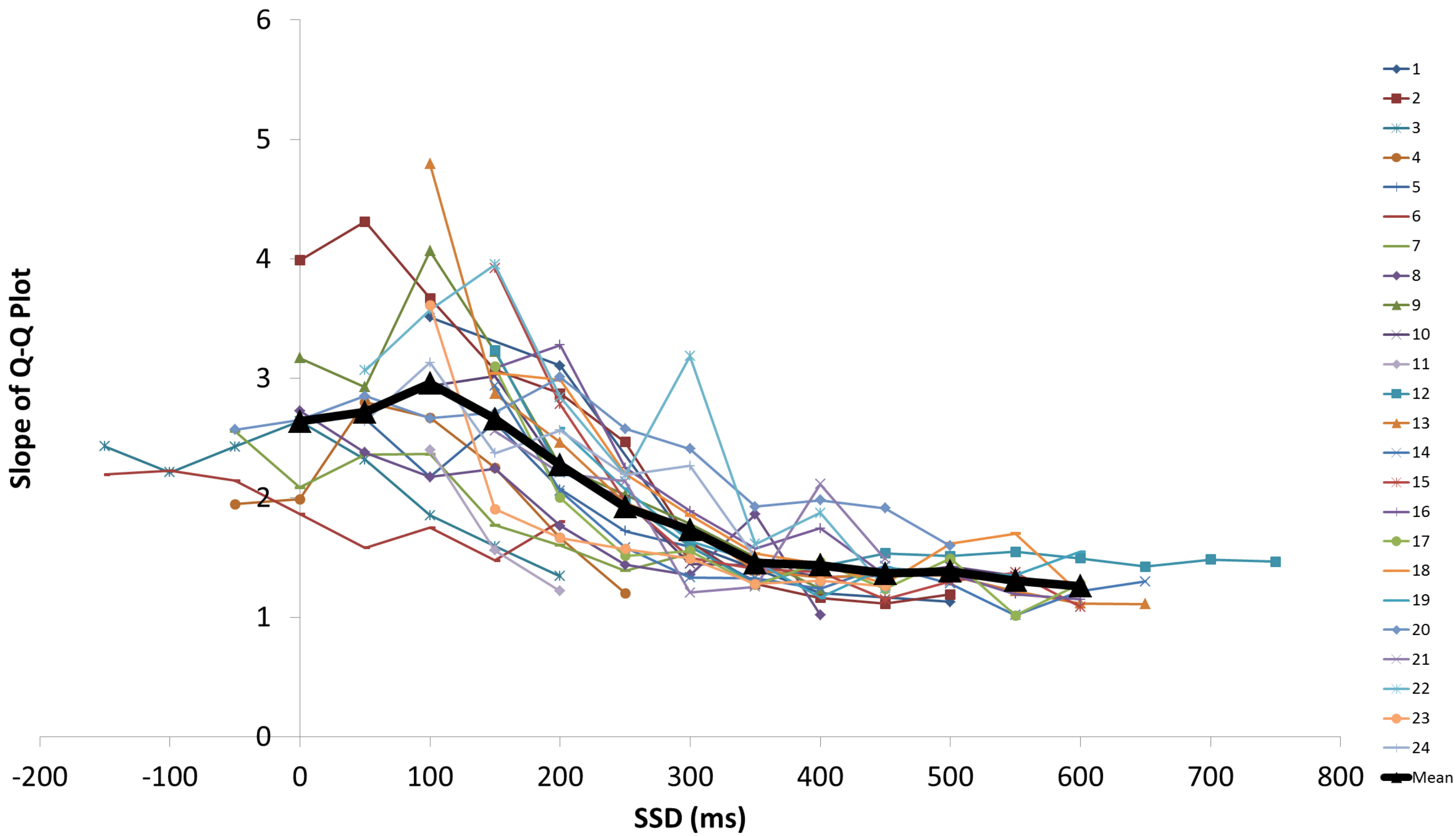
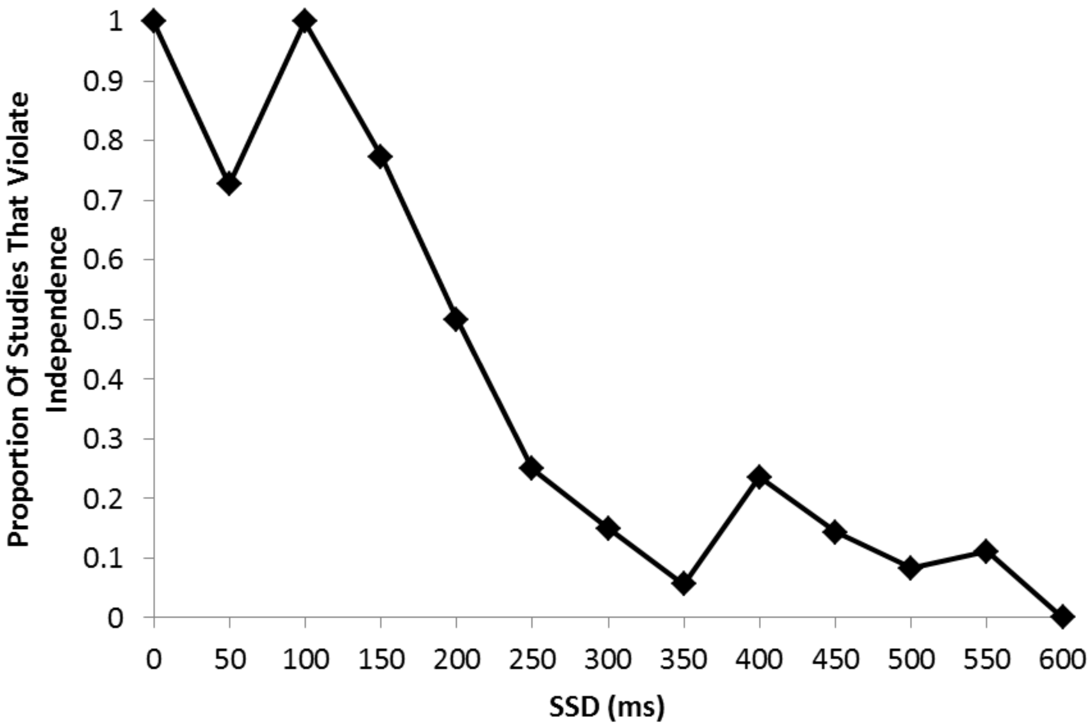


Figure 6. Proportion of all analyzed studies and conditions that violate the independence assumption (as indexed by zero being outside the 95% confidence interval of the median observed – median predicted stop-failure RT across subjects) of the race model across SSDs.



compared had at least five subjects with at least five stop-failure RTs at that SSD. Therefore, when two conditions were compared they were compared across the same range of SSDs (in order to help ensure that any differences between conditions were not driven by differences in SSDs over which the violation was evaluated), but different comparisons in this dissertation were evaluated over different ranges. Then each subject in each condition was either included or excluded in the group average for that condition based upon whether they had a sufficient number of stop-failure trials in the chosen range for that study. As mentioned to above, the criterion was the subject needed to have at least one SSD (within the chosen range for that study) with at least five stop-failure trials. If they had more than one SSD within the chosen range for that study, the dependent variables (observed stop-failure RTs, predicted stop-failure RTs, or slopes of Q-Q plots relating observed to predicted stop-failure RTs) were averaged across the SSDs that passed the criterion of at least five stop-failure trials.

CHAPTER IV

SPEED AND THE VIOLATION OF CONTEXT INDEPENDENCE

In most stop signal studies, conditions with fast RTs tend to have short SSDs. This is because the very common 1 up 1 down tracking algorithm results in short SSD when RTs are fast and long SSD when RTs are slow. RTs fluctuate over time (Nelson et al., 2010), and the short SSDs are likely to occur when RT tends to be fast. To distinguish between the hypotheses that fast RT or short SSD produce violations, fast subjects are compared to slow subjects at the same SSDs, and fast conditions are compared to slow conditions to see whether conditions affect the violations at the same SSDs.

Additionally, RT may have to be sufficiently long in comparison to SSD for the violation to occur. This is most obvious when SSD and RT are the same value. Even if SSD is short, there is no time for the stop process to influence going, so the violation cannot occur. On a given trial, the interval between SSD and RT can be viewed as the time window under which the interaction between stop and go can unfold. This time window may need to be sufficiently large for a violation to occur.

Examining Violations Across a Wide Range of Fixed SSDs in Fast and Slow Subjects.

The datasets that are analyzed below determine SSD with the tracking method. They had many subjects and allow comparison across conditions of interest (e.g., stimulus selective vs. simple stopping), but all of the studies tracked SSD, resulting in a small percentage of the trials being sampled at the short SSDs that are of interest. This results in a subset of subjects, likely the faster subjects, populating the SSDs of interest. In order to sample many short SSDs, two studies were run with a broad range of SSDs (100-500 in 100 ms increments in Fixed SSDs 1, 0-500 in 50ms increments in Fixed SSDs 2) with 48 stop signals at each SSD, which is many more than would typically be sampled at short SSDs with a 1 up 1 down tracking algorithm. This resulted in many subjects contributing to stop-failure RT estimates at all SSDs, strengthening the conclusions and showing that the results from the tracking studies cannot be explained by a small number of fast subjects who sample the short SSDs.

Also, fixed SSD experiments allow fast and slow subjects to be compared at a given SSD. When SSD is tracked, fast subjects tend to have short SSDs and slow subjects tend to have long SSDs. However, when SSD for all subjects are chosen from a fixed set, both fast and slow subjects sample both short and long SSDs. This allows a test of the individual difference of speed at the same short SSDs.

Fixed SSDs 1

Method

Subjects. Twenty-four young adults recruited from the Nashville area were given $12 for a single one-hour session. All subjects had normal or corrected-to-normal vision. One subject was replaced for mean go RT more than 3 standard deviations above the group mean RT, but all other subjects met this criterion and had overall go accuracy above 85%.

Apparatus and Stimuli. The experiment was run on a Pentium Dual-Core PC running E-Prime 1 (pstnet.com). The stimuli were presented on a 19-inch cathode ray tube monitor. The go task was to respond to a single black shape (triangle, circle, square, or diamond) on a white background presented in the center of the screen. The height and width of each shape was 4 cm at the longest point. Subjects responded on a QWERTY keyboard. The stop signal was a 500 Hz tone (70dB, 100ms) presented through closed headphones (Sennheiser eH 150).

Procedure. Each trial began with a 500 ms fixation cross, followed by the presentation of the go stimulus for 850 ms, and followed by a 1000 ms blank-screen inter-trial interval (ITI).

The go task was to respond as quickly and accurately as possible based upon the identity of the centrally presented shape. Two of the shapes were mapped on the “z” key and the other two were mapped onto the “m” key, and subjects responded with their left and right index fingers, respectively. The shape to key response mapping was counterbalanced across subjects.

A stop signal occurred on a random 20% of all trials, and subjects were instructed to try their best to stop their response when they heard it. There were five SSDs: 100ms, 200ms, 300ms, 400ms, and 500ms. The SSD was randomly selected on each stop trial with the only constraint being each was presented exactly 48 times for each subject.

Subjects were instructed to respond quickly and accurately to the shapes, and then were given 12 trials of experimenter-supervised practice on trials without stop signals. Then stop signal trials were introduced, and subjects were instructed to also do their best to stop on stop signal trials. They were given another 12 trials of practice that included 2 stop signals. After practice, subjects completed the main task of 5 blocks of 240 trials each. Between blocks, subjects were given feedback on the speed and accuracy of their no-stop-signal trials from the previous block.

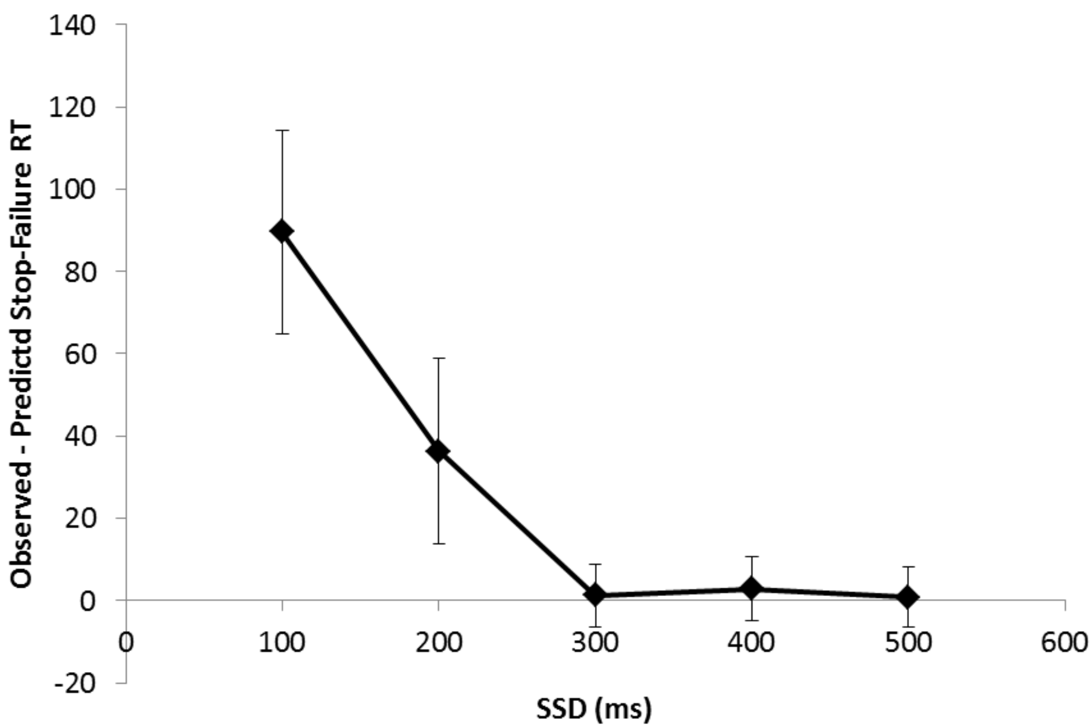
Results

Correct no-stop-signal RT was 490ms and accuracy was 94%. Stop-failure RTs (which cannot be omissions) were compared to no-stop-signal RTs excluding omissions. When averaged across all SSDs, mean stop-failure RT was 469ms, which was significantly shorter than mean no-stop-signal RT excluding omissions (*M* = 489 ms), $t(23) = 5.93$, $p < .001$. The norm in the literature is to either assess context independence with this test of means or to not test the assumption of context independence in any way. By this measure, the context independence assumption of the race model was not violated. Go accuracy on stop-failure trials did not change across SSD (repeated-measures ANOVA $p = .65$).

To evaluate the independence assumption of the race model across SSDs, predicted stop-failure RTs were compared to observed stop-failure RTs for each subject at each SSD. Figure 7 shows the size of the violation in central tendency (median observed stop-failure RT – median predicted stop-failure RT) and the error bars are 95% confidence intervals of the violation at that SSD. At the 100ms and 200ms SSDs, predictions significantly underestimated observed stop-failure RTs, revealing a violation of context independence, but at SSDs of 300ms, 400ms and 500ms there was no sign of a violation. This is evidence that the violations at SSDs < 300ms shown in Figures 4 and 5 are not the result of using an SSD tracking algorithm.

To evaluate the effect of RT on the size of the violation, the data were median split into the 12 subjects with the fastest no-stop-signal RT and the 12 subject with the slowest no-stop-signal RT. Then the observed – predicted stop-failure RT values for each subject at each SSD below 300 ms (the range of SSDs in which a violation often occurs) were computed, and then these values were averaged across SSD (in this case, across the 100 ms SSD and the 200 ms SSD). Subjects who did not have any SSDs < 300ms with at least 5 stop-failure RTs were removed (3 subjects). A 2 (Observed vs. Predicted) x 2 (Slow vs. Fast) mixed ANOVA on stop-

Figure 7. Fixed SSDs 1 median observed – median predicted stop-failure RT values across SSDs. Error bars are 95% confidence intervals.



failure RTs was run. There was a significant main effect of observed versus predicted, $F(1, 19) = 57.35$, $MSE = 687$, $p < .001$, with longer observed stop-failure RTs ($M = 438$ ms) than predicted ($M = 380$ ms). This is evidence of a violation at short SSDs. There was also a significant interaction, $F(1, 19) = 9.55$, $MSE = 687$, $p = .006$, which was driven by a larger violation of central tendency in slower subjects ($M = 87$ ms) than faster subjects ($M = 37$ ms). Slower subjects also had larger slopes ($M = 3.92$) in the linear regression fit to the Q-Q plots than faster subjects ($M = 2.72$), $t(19) = 2.2$, $p = .039$. This shows that the slower subjects had larger violations in central tendency and dispersion than the faster subjects.

Fixed SSDs 2

In order to sample more values at short SSDs, Fixed SSDs 2 was run. Instead of 5 SSDs equally spaced between 100-500ms, 11 SSDs were equally spaced between 0-500ms.

Method

Subjects. Twenty-four young adults recruited from the Nashville area were given $24 for a single 2-hour session. All subjects had normal or corrected-to-normal vision. All subjects had mean go RT within 3 standard deviations of the group mean RT, and overall go accuracy above 85%.

Apparatus and Stimuli. The apparatus and stimuli for Fixed SSDs 2 matched Fixed SSDs 1.

Procedure. The procedure for Fixed SSDs 2 was the same as Fixed SSDs 1 with the following exceptions. The probability of a stop signal was .22 instead of .2. There were 11 SSDs with 48 stop trials each: 0ms, 50ms, 100ms, 150ms, 200ms, 250ms, 300ms, 350ms, 400ms, 450ms, and 500ms. There were 10 blocks of 240 trials each. At the end of the 5th block, subjects took a 5-minute break before beginning the second half of the experiment.

Results.

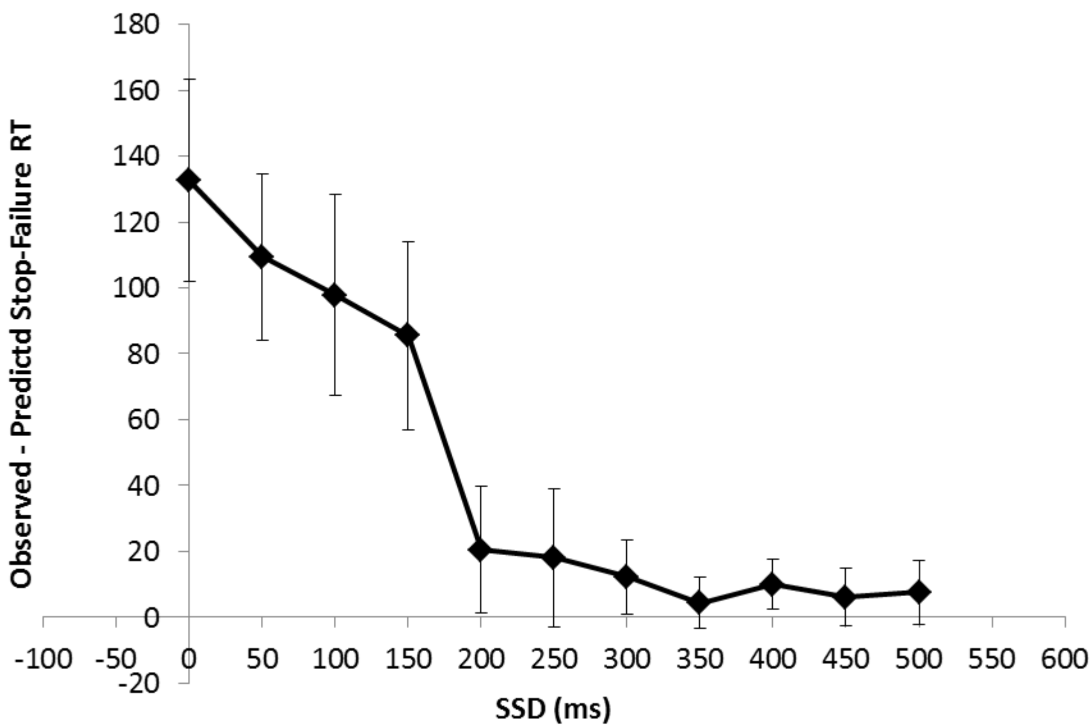
Correct no-stop-signal RT was 473ms and accuracy was 95%. Mean stop-failure RT was 456ms, which was faster than mean no-stop-signal RT excluding omissions (M = 472 ms), $t(23)$ = 3.81, $p < .001$. By this measure, the race model was not violated in this dataset. Go accuracy on stop-failure trials did not change across SSD (repeated-measures ANOVA $p = .15$).

Like Fixed SSDs 1, predicted stop-failure RTs were compared to observed stop-failure RTs for each subject at each SSD. Figure 8 shows that at the four shortest SSDs (0-150 ms), the predicted stop-failure RT were much faster than the observed stop-failure RT. The predictions were also significantly faster than the observed at 200ms, 300ms, and 400ms, but each of these difference were small compared to the earliest four SSDs. Taken together with the evidence from Figures 4, 5, 6, and 7, this suggests that violations are large and consistent at the earliest SSDs (<200 ms), sometimes occur at intermediate SSDs (200-250ms), and seldom occur at long SSDs (>300 ms).

Like Fixed SSDs 1, to evaluate the effect of RT on the size of the violation, the data were median split into the 12 subjects with the fastest no-stop-signal RT and the 12 subject with the slowest no-stop-signal RT. Then the size of the violations were compared across these two groups (no subjects were excluded). Like Fixed SSDs 1, a 2 (Observed vs. Predicted) x 2 (Slow vs. Fast) mixed ANOVA on stop-failure RTs was run. There was a significant main effect of observed versus predicted, $F(1, 22) = 75.86$, $MSE = 688$, $p < .001$, which resulted from observed stop-

Figure 8. Fixed SSDs 2 median observed – median predicted stop-failure RT values across SSDs.

Error bars are 95% confidence intervals.



failure RT (*M* = 441 ms) being longer than predicted (*M* = 376 ms). This is evidence of a violation at short SSDs. The significant interaction showed that the violation of central tendency was larger in slower subjects (*M* = 92 ms) than faster subjects (*M* = 40 ms), $F(1, 22) = 11.83$, $MSE = 688$, $p = .002$. Replicating Fixed SSDs 1, this shows that the violation was larger in slower subjects. The violation in dispersion, as measured by the slope of Q-Q plots, did not show a significant difference between the slower subjects (*M* = 3.55) and the faster subjects (*M* = 2.99), $t(22) = 1.2$, $p = .23$.

Discussion: Fixed SSD Experiments

Some studies that have compared observed and predicted stop-failure RT have used SSD tracking, yielding few stop trials with short SSDs. I aimed to replicate the long stop-failure RTs at short SSDs in an experiment that included many short SSDs. Both Fixed SSD Experiments showed violations at SSDs <=200ms. Fixed SSDs 2 also showed violations at 300ms and 400ms, which were unusual (see Figure 6).

These results converge with the analysis of the other 22 conditions (see Figures 4 and 5), which all track SSD with a 1 up 1 down tracking algorithm. This shows that violations are common in SSDs <=200ms and infrequent in in SSDs >=300ms, and this is the case when SSD is tracked or sampled from a set of fixed values.

To test the relationship between RT and the violation, the subjects were split into those with faster than the median no-stop-signal RT and those with slower than the median no-stop-signal RT. The difference between observed and predicted median stop-failure RT was larger in subjects with slower RT than subjects with faster RT. There was mixed evidence of larger violations in dispersion in the slower subjects than the faster subjects, with Fixed SSDs 1 showing this result (p = .04) but the difference did not reach significance in Fixed SSDs 2 (p = .23). The result that the violations were larger in central tendency and sometimes larger in dispersion suggests that the violation is related to RT, with greater violations when RT is longer.

The next studies test whether the positive relationship between RT and the size of the violation is a stable individual difference or can be manipulated within a subject by manipulating their RT.

Manipulating RT at the Same SSDs

Fixed SSDs 1 and 2 used a wide range of fixed SSDs for both fast and slow subjects and showed that the violation was larger in subjects who had slower no-stop-signal RT. This could be explained by two classes of explanations: the violation is a stable individual difference that tends to be related to no-stop-signal RT across subjects, or the violation can be adjusted by a subject by way of adjusting their no-stop-signal RT. If the violation is a stable individual difference, and subjects who have larger violations also tend to have longer RTs, then manipulating RT within a subject should not influence the size of the violation. Alternatively, if the violation is adjusted as subjects adjust their RT, then the violation should become smaller when experimental manipulations result in faster RT and the violation should become larger when experimental manipulations result in slower RT.

Deadline 1

To manipulate RT within subjects, three different go RT deadline conditions were introduced: 300 ms, 500 ms, and 700 ms. Go RTs are around 500 ms when no deadline is present in similar stop tasks with similar subjects. The shortest deadline should put large time pressure on subjects to complete their go response before the deadline, the intermediate deadline should put some time pressure on the subject, and the long deadline should put little time pressure as most responses are completed by 700 ms if no deadline is imposed.

Method

Subjects. 24 subjects were recruited from the Nashville community and were compensated $12 for a single one-hour session. All subjects had normal or corrected-to-normal vision. Seven subjects were replaced whose probabilities of successful stopping fell outside the 95% confidence interval of 0.5.

Apparatus and Stimuli. The apparatus and stimuli for Deadline 1 matched Fixed SSDs 1.

Procedure. The procedure was the same as Fixed SSDs 1 with the following exceptions. The deadline for the go response was manipulated by varying go-stimulus duration (300ms, 500ms or 700ms) and instructing subjects to respond before the go stimulus disappeared. There were six blocks of 240 trials each, and go-stimulus duration varied across the first three blocks in an order that was counterbalanced across subjects. The order of blocks for each subject was the same for the first three blocks and the last three blocks. Each trial began with a 500ms fixation display, followed by the go stimulus. A 1000 ms ITI followed the go stimulus on every trial.

On a random 25% of trials, a stop signal occurred that indicated that subjects should withhold their response for that trial. The delay between the onset of the go stimulus and the onset of the stop signal (stop signal delay, or SSD) was varied with a tracking algorithm to achieve a p(respond|stop signal) = .5 (Levitt, 1971; Osman et al., 1986). When subjects inhibited successfully SSD increased by 50ms; when subjects failed to inhibit SSD decreased by 50ms. There were three separate SSD tracking algorithms, one for each deadline.

Subjects were told to try to respond before the go stimulus left the screen, and to sacrifice go response accuracy in order to respond before the deadline (responses were still recorded after the deadline). After the instructions, subjects were given 24 trials of experimenter-supervised practice with the 500ms deadline. After practice, subjects completed the main task. At the end of each block, subjects were given feedback on mean RT and mean accuracy from that block, as well as the percentage of trials in which they met the deadline.

Results

Correct no-stop-signal RT were 323 ms, 397 ms, and 440 ms in the 300 ms, 500 ms, and 700 ms deadline conditions, respectively, $F(2, 46) = 122.8$, $MSE = 689$, $p < .001$. This shows that the deadline manipulation was successful in producing different RTs within subjects, even though many subjects were unable to meet the 300 ms deadline on many trials. Go accuracy was 71%, 85%, and 91% in the 300 ms, 500 ms, and 700 ms deadline conditions, revealing a speed-accuracy tradeoff, $F(2, 46) = 95.3$, $MSE = .002$, $p < .001$.

First, I tested for context independence with the usual method. A 2 (No-stop-signal vs. Stop-failure) x 3 (Deadline: 300 ms, 500 ms, and 700 ms) repeated measures ANOVA was run to test whether mean no-stop-signal RT (excluding omissions) differed from mean stop-failure RT, and if so whether this difference interacted with deadline. There was a significant main effect of no-stop-signal versus stop-failure, $F(1, 23) = 16.5$, $MSE = 531$, $p < .001$, with stop-failure RT ($M = 377$ ms) faster than no-stop-signal RT ($M = 393$ ms). There was also an interaction, $F(2, 46) = 6.5$, $MSE = 91$, $p = .003$, with the difference between no-stop-signal RT and stop-failure RT being smaller in the 300 ms deadline (8 ms) than the 500 ms deadline (18 ms) or the 700 ms deadline (21 ms). This usual measure of the race model suggests that there were not violations (as evidenced by the significant main effect), except possibly at the shortest deadline (as evidenced by the significant interaction).

To evaluate the effect of speed pressure on the violations of the race model, median observed minus median predicted stop-failure RTs were compared across deadline at SSDs ranging from 50 ms to 200 ms (these were the SSDs for which there were at least 5 subjects with at least 5 stop-failure RTs at each of the three deadlines). Four subjects were removed for having no SSDs with at least five stop-failure RTs in this range in at least one of the deadline conditions. A 2 (Observed vs. Predicted) x 3 (Deadline: 300 ms, 500 ms, and 700 ms) repeated-measures ANOVA on stop-failure RTs was run. There was a main effect of observed versus predicted, $F(1, 19) = 34.88$, $MSE = 702$, $p < .001$, with observed stop-failure RT ($M = 367$ ms) slower than predicted ($M = 339$ ms). This revealed a violation of independence at short SSDs in this dataset. However, there was not a significant interaction of observed versus predicted stop-failure RT and deadline, with similar violations at 300 ms ($M = 23$ ms), 500 ms ($M = 34$ ms) and 70 ms deadlines ($M = 29$ms), $F\ (2, 38) = 1.1$, $MSE = 269$, $p = .33$. In contrast, the slopes of the Q-Q plots did differ across deadline conditions, with the shallowest slope in the 300 ms conditions ($M = 1.79$), and steeper slopes in the 500 ms ($M = 2.37$) and 700 ms ($M = 2.36$) conditions, $F(2, 38) = 4.6$, $MSE = .61$, $p = .025$.

There were no differences in the violation in central tendency across deadline, but the there was less violation in dispersion at the 300 ms deadline, suggesting that applying a very short go RT deadline reduced the upper tail of the observed stop-failure RT distribution.

Deadline 2

Deadline 2 was a replication of Deadline 1 except trial length was equated across deadline in the Deadline 2 experiment. This manipulation was used to ensure that the results from Deadline 1 cannot be explained by different trial durations across the deadline conditions.

Method

Subjects. Twenty-four young adults recruited from the Nashville area were given $12 for a single one-hour session. All subjects had normal or corrected-to-normal vision. Three subjects were replaced whose probabilities of successful stopping fell outside the 95% confidence interval of 0.5.

Apparatus and Stimuli. The apparatus and stimuli for Deadline 2 matched Deadline 1.

Procedure. The procedure for Deadline 2 was the same as Deadline 1 with the following exception. In Deadline 2, the go stimulus duration plus ITI always equaled 1500ms. Thus, ITI was 1200ms, 1000ms, and 800ms for go durations of 300ms, 500ms, and 700ms, respectively.

Results

Correct no-stop-signal RT were 323 ms, 394 ms, and 443 ms in the 300 ms, 500 ms, and 700 ms deadline conditions, respectively, $F(2, 46) = 93.1$, $MSE = 936$, $p < .001$. This replicates Deadline 1, showing that the deadline manipulation was successful in producing different RTs within subjects, even if many subjects were unable to meet the 300 ms deadline on many trials. Go accuracy was 70%, 83%, and 88% in the 300 ms, 500 ms, and 700 ms deadline conditions, revealing a speed-accuracy tradeoff, $F(2, 46) = 107.7$, $MSE = .002$, $p < .001$.

As in Deadline 1, a 2 (No-stop-signal vs. Stop-failure) x 3 (Deadline: 300 ms, 500 ms, and 700 ms) repeated measures ANOVA was run to test whether mean no-stop-signal RT (excluding omissions) differed from mean stop-failure RT, and if so whether this difference interacted with deadline. Like Deadline 1, there was a significant main effect of no-stop-signal versus stop-failure, with faster stop-failure RT(M = 380 ms) than no-stop-signal RT (M = 397 ms), F(1, 23) = 27.3, MSE = 366, p < .001. Unlike Deadline 1, there was no interaction of no-stop-signal versus stop-failure and deadline, F(2, 46) = 2.4, MSE = 109, p = .10. Therefore, this usual measure suggests that the context independence assumption of the race model was not violated.

To replicate Deadline 1 and evaluate the effect of speed pressure on the violations of the context independence assumption of the race model, median observed – median predicted stop-failure RTs were compared across deadline at SSDs ranging from 0 ms to 200 ms (these were the SSDs for which there were at least 5 subjects with at least 5 stop-failure RTs at each of the three deadlines). Four subjects were removed for having no SSDs with at least five stop-failure RTs in this range in at least one of the deadline conditions. A 2 (Observed vs. Predicted) x 3 (Deadline: 300 ms, 500 ms, and 700 ms) repeated-measures ANOVA on stop-failure RTs was run. There was a main effect of observed versus predicted, F(1, 19) = 41.81, MSE = 510, p < .001, with observed stop-failure (M = 358 ms) slower than predicted (M = 331 ms). This is consistent with a violation of independence at short SSDs. However, there was not a significant interaction of observed versus predicted and deadline, F(2, 38) = 1.5, MSE = 385, p = .23, as there were similar violations at 300 ms (M = 18 ms), 500 ms (M = 31 ms) and 70 ms deadlines (M = 31ms). However, like Deadline 1, the slopes of the Q-Q plots did differ across deadline, F(2, 38) = 5.8, MSE = .24, p = .011, with the shallowest slope in the 300 ms condition (M = 1.68), then the 500 ms condition (M = 2.02), then the 700 ms condition (M = 2.2).

Discussion: Deadline Experiments

In the Fixed SSD experiments, slower subjects had larger violations of the race model at short SSDs. I suggested two classes of hypotheses: Degree of violation is a stable individual difference that relates to RT, or degree of violation modulates with RT. To distinguish these two alternative, two experiments were run in which RT was modulated by imposing a go task deadline. If degree of violation modulates with RT, then the violation size should scale with the length of the deadline. In both studies, there were significant violations of the race model at all deadlines. There were also large differences in go RT across deadline conditions, showing that the deadline was successful in manipulating go RT within-subjects. However, there was no significant difference in median violation across deadline, but a significant difference in the slope of the Q-Q plot, with steeper slopes at the longer deadlines. This suggests that deadline selectively influences the upper tail of the distribution of observed stop-failure RTs, with the upper tail being longer at longer deadlines.

The violation may be small at short deadlines because subjects sometimes make a fast guess as to which go response should be made (Ollman, 1966; Dutilh, Wagenmakers, Visser, & van de Maas, 2011), removing the go response selection stage. If the interaction between going and stopping results from going and stopping sharing limited resources, removing response selection may reduce competition between going and stopping for common resources. This is addressed further in the Chapter VI Discussion.

Stop Probability

In 2011, Gordon Logan and I published a paper that manipulated the probability of a stop signal between .2 and .4 across sessions within the same subjects (Bissett & Logan, 2011

Experiment 1). Our results showed that go RT increased with the probability of a stop signal, which is in line with previous work (Logan, 1981; Logan & Burkell, 1986). I reanalyzed the 2011 data to test whether the size of the violation differs in the 20% and 40% stop signals conditions. If the violation is larger in the (slower) 40% condition, this is consistent with a subject's violation modulating with RT. If the opposite pattern is shown in which the violation is smaller in the higher probability 40% condition, this is consistent with the violation decreasing as subjects prioritize the stop task. Gordon Logan and I have argued that the stop signal paradigm involves balancing the competing go and stop goals by flexibly shifting goal priority between going and stopping (Bissett & Logan, 2011, 2012a, 2012b). As the probability of a stop signal increases, priority shifts from go to stop. Therefore, a smaller violation in the 40% condition would be consistent with the violation decreasing when the stop task is prioritized.

Method

The methods are described in detail in Experiment 1 of Bissett & Logan (2011). The methods are similar to Fixed SSDs 1 except SSD was tracked with a 1 up 1 down tracking algorithm (as in Deadline 1). The go stimuli were four visual shapes (triangle, circle, square, or diamond) mapped onto two keypress responses. The stop signal was a 500 Hz auditory tone.

Results

Correct no-stop-signal RT were faster in the 20% stop signals condition (*M* = 459 ms) than the 40% stop signals condition (*M* = 514 ms), $t(23) = 3.3$, $p = .003$. Averaging across all SSDs, a 2 (No-stop-signal excluding omissions vs. Stop-failure) x 2 (20% vs. 40% stop signals) repeated-measures ANOVA was run. There was a significant main effect of no-stop-signal versus stop-failure, $F(1, 23) = 7.4$, $MSE = 5441$, $p = .012$, with mean stop-failure RT faster (*M* =

452 ms) than mean no-stop-signal RT (*M* = 486 ms). There was also a significant interaction, $F(1, 23) = 28.26$, $MSE = 149$, $p < .001$, with the difference between no-stop-signal RT and stop-failure RT larger in the 40% stop signal condition (*M* = 47 ms) than in the 20% condition (*M* = 21 ms). Therefore, by the usual measure comparing no-stop-signal and stop-failure RT, the context independence assumption of the race model was not violated in either condition, but there was evidence that stop-failure RTs were more similar to no-stop-signal RTs in the 20% stop signals conditions than the 40% stop signals condition.

To evaluate the effect of stop signal probability on violations of independence, observed and predicted median stop-failure RTs were compared across stop probability at SSDs ranging from 100 ms to 250 ms (these were the SSDs for which there were at least 5 subjects with at least 5 stop-failure RTs at each of the two probabilities). Three subjects were removed for having no SSDs with at least five stop-failure RTs in this range in at least one of the probability conditions. A 2 (Observed vs. Predicted) x 2 (20% vs. 40% stop signals) ANOVA was run on stop-failure RTs. There was a main effect of observed versus predicted, $F(1, 20) = 14.2$, $MSE = 1710$, $p = .001$, with observed stop-failure RT (*M* = 414 ms) slower than predicted stop-failure RT (*M* = 380 ms). This is evidence of a violation in central tendency. The significant interaction showed that the median violation in the 20% stop signals condition (*M* = 42 ms) was larger than the violation in the 40% stop signals condition (*M* = 27 ms), $F(1, 20) = 5.3$, $MSE = 222$, $p = .03$. The violation across the distribution of stop-failure RTs, as measured by the slope of the Q-Q plots, did not differ between the 20% stop signals condition (*M* = 2.68) and the 40% stop signals conditions (*M* = 2.56), $t(20) = .585$, $p = .57$. The larger violation in central tendency in the 20% stop probability condition is inconsistent with the violation modulating in size with go RT, as the faster condition had the larger violation. It is more consistent with the violation being smaller when the stop process is prioritized.

Discussion: Stop Probability

RT increases with stop signal probability, so to provide another test of the effect of manipulating RT within-subjects, violations were compared between a (faster) low stop probability condition and a (slower) high stop probability condition. There was a significant violation across both probabilities, but the violation was larger in the faster, low probability stop signal condition. This is inconsistent with the violation modulating up with slower RT and down with faster RTs.

The smaller violation in the high stop probability condition may be related to shifts in priority between going and stopping. Gordon Logan and I have argued that subjects shift priority from going to stopping as the probability of a stop signal increases (Bissett & Logan, 2011, 2012a, 2012b). A stop process that is given higher priority may more potently inhibit the go process, and a stop process that is given lower priority may more weakly inhibit the go process, slowing but not completely inhibiting it on some trial. The idea that variability in the potency of inhibition could explain violations of independence will be explored further in the General Discussion.

Does the Violation of Context Independence Depend on Effector?

Most human stop signal studies have subjects make manual responses on a keyboard, but some also use saccadic eye movements. Saccadic eye movements tend to be much faster than keypresses, therefore SSDs sample almost exclusively from short SSDs. However, previous research (Akerfelt et al., 2006; Boucher, Stuphorn, et al., 2007; Colonius et al., 2001; Gulberti et al., 2014; Oezyurt et al., 2003) suggests that the violation of context independence in saccadic eye movements may occur over a more restricted range than keypresses. Violations in an eye-movement study were compared to a manual study to test the effect of effector on the extent of the violation. This is also a converging test of speed, because saccadic responses tend to be much faster than manual responses.

Fast Saccadic Eye Movements Versus Slower Manual Responses at Same SSDs

Stopping manual responses to auditory stop signals that occurred on 20% of trials (Experiment 1, Bissett & Logan, 2011) were compared to stopping saccadic eye movements to auditory stop signals that occurred on 20% of trials. If the violation is independent of speed and effector, then it should not differ between hands and eyes. If the violation differs between effectors, this could result from the different effectors being more or less susceptible to the violations or because fast (or slow) responses are more susceptible to the violation.

Method

The methods for the experiment involving stopping manual responses to auditory stop signals are presented in detail in Experiment 1 of Bissett & Logan (2011). The go stimuli were four visual shapes (triangle, circle, square, or diamond) mapped onto two keypress responses. The stop signal was a 500 Hz auditory tone. The following section describes the methods for the unpublished data that involves stopping saccadic eye movements to auditory stop signals.

Subjects. Eleven young adults recruited from the Nashville area were given $60 for five one-hour sessions on five consecutive days. All subjects had normal or corrected-to-normal vision. Four subjects were replaced because their eyes could not be tracked satisfactorily and two subjects were replaced because they did not show up for some of the sessions.

Apparatus and Stimuli. The experiment was run on a PC running SR Research Experiment Builder software connected to a PC running the Eyelink 2000. The stimuli were presented on a 19-inch cathode ray tube monitor displaying a 1024 x 768 pixel resolution. The go task was to saccade to a black X presented on the right or left side of the screen. The X was 50 pixels by 50 pixels, and its center was positioned at 172 x 384 if presented on the left and 852 x 384 if presented on the right. The stop signal was either a 500 Hz tone, 750 Hz tone, or 1000 Hz tone (70dB, 100ms) for a given subject, and the tone choice was counterbalanced across subjects. The tone was presented through closed headphones (Sennheiser eH 150).

Saccades were registered by the Eyelink if above a velocity threshold of 30°/sec (and remained above the threshold for 4ms) or an acceleration threshold of 8000°/sec/sec. The minimum motion threshold was .1°. Saccades were registered as correct on a go trial if they landed within a circle around the target X with a radius of 170 pixels. Stop trials were registered as correct if no saccades were registered.

Procedure. Subjects completed 5 sessions across five consecutive days. The first session was a training session in which subjects completed only no-stop-signal trials, and they received trial-by-trial feedback as to whether their response was recorded as correct by the eye tracker. This session was intended to train subjects to fixate appropriately during the fixation period and to make saccades to the X that the eye tracker recognizes as correct when the go stimulus appeared. The final four sessions involved simple stopping to auditory stop stimuli, simple stopping to visual stop stimuli, selective stopping to auditory stop and ignore stimuli, and selective stopping to visual stop and ignore stimuli. The order of the final four sessions was counterbalanced across subjects. I focused on the results from the simple stopping to auditory stop stimuli session. Simple stopping to auditory stimuli is the norm in my data and the questions of whether modality and stimulus selectivity influence the violation are addressed later in the dissertation by focusing on larger datasets that involve manual responses.

Subjects pressed the spacebar to begin each trial, which initiated drift correction and began a 500ms fixation period before the target appeared for 1000ms, followed by the 850 ms blank-screen ITI.

SSD was tracked with a 1 up 1 down tracking algorithm (Levitt, 1971; Osman et al., 1986). Auditory stop signals were presented on 20% of all trials. Subjects were instructed to look promptly at the X when it appeared, but try to remain fixated on the center of the screen if they heard a tone. After instructions, subjects were given 20 trials of practice. The main task included 10 blocks of 60 trials per session. At the end of each block, subjects were given rest but no feedback.

Results

The saccadic eye-movement data were taken from the simple stopping to auditory stop signals condition, and the manual data were taken from the 20% stop signals condition of Bissett and Logan (2011, Experiment 1). A 2 (No-stop-signal vs. Stop-failure) x 2 (Manual responses vs. Saccades) mixed ANOVA showed a main effect of manual versus saccades, $F(1, 33) = 108.1$, $MSE = 7862$, $p < .001$, with manual responses ($M = 449$ ms) slower than saccades ($M = 211$ ms), as expected. There was also a main effect of no-stop-signal versus stop-failure, $F(1, 33) = 26.5$, $MSE = 227$, $p < .001$, with no-stop-signal RT ($M = 384$ ms) slower than stop-failure RT ($M = 364$ ms), as expected with the race model. The interaction was not significant, *F*(1, 33) = .02, *MSE* = 227, *p* = .89. By this measure, the race model applied to both manual responses and saccades.

To evaluate the violation across effectors, the violation in eyes was compared to the violation in hands at the SSDs in which both groups had at least five subjects with at least five stop-failure trials each (100 ms, 150 ms, and 200 ms). Three manual subjects and one saccadic eye movement subject were removed for having no SSD with at least five stop-failure RTs in this range. A 2 (Observed vs. Predicted) x 2 (Manual responses vs. Saccades) mixed ANOVA was run on stop-failure RTs to evaluate the violation in central tendency and the influence of effector on the violation. The main effect of observed versus predicted was significant, *F*(1, 29) = 13.1, *MSE* = 1247, *p* = .001, with observed stop-failure RT (*M* = 351 ms) longer than predicted stop-failure RT (*M* = 309 ms). This revealed a violation of independence in central tendency. The interaction revealed that the violation with manual responses (*M* = 57 ms) was larger than the violation with saccades (*M* = 12 ms), *F*(1, 29) = 5.6, *MSE* = 1247, *p* = .025. As shown above, hands have much longer RT than eyes, so to test whether the violations would be similar if compared as a proportion of RT, proportional violations were computed ((Median Observed Stop-Failure RT – Median Predicted Stop-Failure RT)/Median No-Stop-Signal RT). However, even the proportional violation in hands (*M* = 12%) was larger than the proportion in eyes (*M* = 5.7%), *t*(29) = 2.4, *p* = .02. The slopes of the Q-Q plots also were larger in hands (*M* = 2.99) than in eyes (*M* = 1.71), *t*(29) = 2.7, *p* = .012. This shows that though there was a significant violation in both hands and eyes, the violation was larger in central tendency and dispersion in hands than in eyes.

Discussion: Effector Experiment

Saccadic eye movements tend to be considerably faster than manual responses. In order to test the generality of the violation across effectors, as well as a convergent test of speed, saccadic eye movements were compared to manual keypresses across subjects with the same probability of a stop signal and similar stop stimuli. The violation of independence was significant in both hands and eyes at short SSDs, showing that violations at short SSDs occur across different effectors. However, the violations at short SSDs were larger and the distributions of observed stop-failure RTs were more variable in hands than in eyes. The range over which the violation occurred did not differ, with both violating at 100 ms and 150 ms SSDs, but not 200 ms. The violations in central tendency remained larger in hands than eyes when violations were computed as a proportion of mean RT. This shows that the slower manual responses had larger violations than the faster saccadic eye movements.

Discussion: The Effect of Speed on the Violation

Four manipulations were brought to bear on the relationship between short SSD violations and speed: Fixed SSDs, Deadline, Stop Probability, and Manual responses versus Saccades. First, Fixed SSDs showed that the median violation was larger in slower subjects than faster subjects. Second, Deadline showed that when subjects were pushed to go much faster than they normally would, the violation in central tendency was unaffected but the slope of the Q-Q plots showed that there was less violation in dispersion at the shortest deadline. Third, Stop Probability showed that the faster, lower probability conditions showed a larger violation. Lastly, Manual responses versus Saccades showed that the faster eye movements had smaller violations in central tendency and dispersion than the slower manual responses.

Taken together, the results are mixed. The larger violation in central tendency in fast than slow subjects was consistent across the two Fixed SSDs studies. In contrast, when put in a condition in which RTs were fast (300 ms deadline, 20% stop signals, saccadic eye movements), the violations were sometimes similar in central tendency but less variable than longer RT conditions (300 ms deadline) sometimes larger (20% stop signals) and sometimes smaller (saccadic eye movements). This suggests that manipulating speed may have variable effects according to the nature of the manipulation.

The mixed results of speed do not lend themselves to a simple, unifying conclusion. However, the results do provide clarity on one central point: the violation occurs at both fast and slow RTs in all comparisons. Violations occur in fast and slow subjects and in fast and slow conditions. Therefore, neither fast nor slow RTs are a necessary condition for the violation. This is in contrast to the relationship between SSD and the violation, in which violations are large and consistent at short SSDs and largely nonexistent at long SSDs. Therefore, short SSDs appear to be both sufficient and necessary to produce the violation in most subjects and most datasets, but long (or short) RTs are neither sufficient nor necessary to produce violations. Therefore, the violation appears to result from short SSDs, and RT has a modulatory influence on the violation that depends on the nature of the manipulation that affects RT.

CHAPTER V

STOP STIMULUS MODALITY AND THE VIOLATION OF CONTEXT INDEPENDENCE

All of the studies in the reanalysis have visual go stimuli, which are ubiquitous in stop signal tasks, but some of the studies in this reanalysis have auditory stop signals and others have visual stop signals. In the following section, I ask whether the violation is present for both auditory and visual stop signals. I also ask whether the violation is similar in magnitude when stop signals are auditory or visual. Significant violations with both auditory and visual stop signals would support a central, amodal interaction between going and stopping. If the violation does not differ between auditory and visual stop signals, this would suggest that no aspect of the interaction is modality specific. If the violation is only present with visual stop signals, or if the violation is larger with visual stop signals, this is consistent with going and stopping sharing modality-specific resources (Wickens, 1980). If the violation is only present with auditory stop signals, or if the violation is larger with auditory stop signals, this would be consistent with cross-modality attentional shifts slowing performance more than within-modality attentional shifts (Laberge, Van Gelder, & Yellott, 1970; Turatto, Benso, Galfano, & Umilta, 2002), or the auditory stop signals being intrinsically alerting (Posner, Nissen, & Klein, 1976), alerting attention away from go processing and slowing stop-failure RTs.

Between-Subjects Modality Manipulation

To test the effect of modality, I first compare two studies that use auditory stop signals to two studies that use visual stop signals but are otherwise identical.

Method

The methods are described in detail in Experiments 1-4 of Bissett & Logan (2012b). For the following analyses, Experiments 1 and 2 are combined to produce the auditory stop signals dataset and Experiments 3 and 4 are combined to produce the visual stop signals dataset. To summarize, the methods were similar to Fixed SSDs 1 except SSDs were tracked with a 1 up 1 down tracking algorithm. The auditory stop signals were tones (Experiment 1 and 2), and the visual stop signals were colored stars (Experiment 3) or black bars presented above or below the go stimulus (Experiment 4). Go responses were manual keypresses.

Results

A 2 (No-stop-signal vs. Stop-failure) x 2 (Auditory vs. Visual) mixed ANOVA on RTs was run. The main effect of no-stop-signal versus stop-failure was significant, $F(1, 102) = 475.1$, $MSE = 485$, $p < .001$, with no-stop-signal RT (M = 607 ms) longer than stop-failure RT (M = 540 ms). This suggested that the race model applied to these data. There was no main effect of modality and no interaction (both p's > .2), showing that RTs and this measure of the violation did not differ across modality.

To evaluate the violation across stop signal modality, the violation with auditory stop signals was compared to the violation with visual stop signals at the SSDs <300 ms in which both groups have at least five subjects with at least five stop-failure trials each (150 ms, 200 ms, and 250 ms). Seventeen auditory subjects and 12 visual subjects were removed for having no SSD with at least five stop-failure RTs in this range in at least one of the modality conditions. For the remaining 31 auditory and 44 visual subjects, a 2 (Predicted vs. Observed) x 2 (Auditory vs. Visual) mixed ANOVA was run on stop-failure RTs. There was a significant main effect of observed versus predicted, $F(1, 73) = 27.1$, $MSE = 942$, $p < .001$, with observed stop-failure RT ($M = 430$ ms) longer than predicted stop-failure RT ($M = 404$ ms). However, the interaction did not reach significance, $F(1, 73) = .2$, $MSE = 942$, $p = .69$, as there was a similar violation with auditory ($M = 29$ ms) as with visual ($M = 24$ ms). The slopes of the Q-Q plots did not differ between auditory stop signals ($M = 2.43$) and visual stop signals ($M = 2.19$), $t(73) = 1.2$, $p = .24$. This is evidence that the violation is present with both auditory and visual stop signals, but the violation did not differ in central tendency or dispersion between auditory and visual stop signals.

Within-Subjects Modality Manipulation

To bolster the between-subjects analysis that was just presented, two datasets were analyzed that interleaved auditory and visual stop signals within the same session (Bissett & Logan, 2012b, Experiments 5 and 6). The violation was compared between stop signals that were auditory and stop signals that were visual.

Method

The methods are described in detail in Experiments 5-6 of Bissett & Logan (2012b). For the following analyses, Experiments 5 and 6 were combined, but for each subject auditory and visual stop signals were analyzed separately. The procedure was similar to Experiments 1-4, except within each session in Experiments 5 and 6 auditory and visual stop signals were randomly interspersed, instead of subjects experiencing only auditory or visual stop signals. In both experiments, the go stimuli were four visual shapes (triangle, circle, square, or diamond) mapped onto two keypress responses. Experiment 5 used stop signals that were either 500 Hz tones or orange colored stars presented around the go stimuli. Experiment 6 used stop signals that were either auditory tones (500 Hz and 750 Hz) or black bars presented above or below the go stimuli.

Results

Overall mean stop-failure RT significantly differed from mean no-stop-signal RT (excluding omissions) with auditory stop signals (*M'*s 537 ms vs. 594 ms, $t(47) = 11.3$, $p < .001$), and with visual stop signals (*M's* 532 ms vs. 594 ms, $t(47) = 16.2$, $p < .001$). This suggests that the race model applied to these data.

To evaluate the violation across stop signal modality, the violation with auditory stop signals was compared to the violation with visual stop signals at the SSDs <300 ms in which both modality stop signals had at least five subjects with at least five stop-failure trials each (150 ms, 200 ms, and 250 ms). Eighteen subjects were removed for having no SSD with at least five stop-failure RTs in this range in at least one of the modality conditions, leaving 30 subjects in each modality. A 2 (Predicted vs. Observed) x 2 (Auditory vs. Visual) repeated-measure ANOVA was run on stop-failure RTs. There was a significant main effect of predicted versus observed, $F(1, 29) = 11.8$, $MSE = 993$, $p = .002$. Observed stop-failure RT ($M = 449$ ms) was longer than predicted stop-failure RT ($M = 413$ ms). There was also a significant interaction with a larger violation when the stop signal was auditory ($M = 55$ ms) than when it was visual ($M = 17$ ms), $F(1, 29) = 10.4$, $MSE = 1040$, $p = .003$. The slopes of the Q-Q plots were also steeper with auditory stop signals ($M = 2.73$) than with visual stop signals ($M = 2.05$), $t(29) = 3.2$, $p = .004$. This is evidence that there are significant violations of independence at short SSDs in both modalities, but the violations were particularly large with auditory stop signals.

Discussion: The Effect of Stop Signal Modality on the Violation

The results from 6 published studies were brought to bear on the question of what is the effect of stop signal modality on the violation of context independence. In both the between-subject and within-subject analyses, both auditory and visual stop signals revealed significant violations of independence at short SSDs. This shows that neither auditory nor visual stop signals are necessary to produce the violation.

When the violations with visual and auditory stop signals were compared directly, the results were mixed. The between-subjects experiments showed no difference between the violation for auditory and visual stop signals. The within-subjects experiments showed that the violation was larger in central tendency and dispersion with auditory stop signals than with visual stop signals.

These data are evidence against the hypothesis that the violation results from overlapping within-modality resources (Wickens, 1980), as there was no evidence that the violation was larger when both the go and stop stimuli were presented in the same visual modality. These data provide some support for the violation being larger in auditory than in visual stop signals. Greater violation with auditory than visual stop signals is consistent with cross-modality shifts in attention slowing performance more than within-modality shifts (Laberge et al., 1970; Turatto et al., 2002), or the auditory stop signals intrinsically orienting (Posner et al., 1976) attention away from go processing, slowing stop-failure RTs. The lack of consensus between the between-subject and within-subject analyses suggests that additional research will be necessary to conclude that the violation is larger with auditory stop signals.

CHAPTER VI

STIMULUS SELECTIVE STOPPING AND THE VIOLATION OF CONTEXT INDEPENDENCE

Bissett and Logan (2014) recently published a paper suggesting that subjects use a variety of strategies in a specific type of selective stopping paradigm called stimulus selective stopping. In stimulus selective stopping, subjects stop to the one stop signal but do not stop to a similar, ignore signal. We argued that selective stopping can be done by using more than one strategy. First, subjects may prolong the stop process by adding a discrimination stage (*Independent Discriminate then* Stop). Second, subjects may stop non-selectively and then restart the response if the signal was an ignore signal (*Stop then Discriminate*). Third, we discovered a variant of the first strategy in which the requirement to discriminate the stop or ignore stimulus interacted with the go process, slowing go RT and violating the context independence assumption of the race model (*Dependent Discriminate then Stop*). We found that many subjects used the third strategy, some used the second strategy, and virtually no subjects used the first strategy. We argued that in stimulus selective stopping subjects either used a non-selective stopping mechanism (Stop then Discriminate), or violated the race model when acting selectively on the stop and ignore stimuli (Dependent Discriminate then Stop).

The reanalysis in this dissertation brings the strategic heterogeneity suggested by Bissett and Logan (2014) into question. To categorize subjects into strategies, Bissett and Logan (2014) compared mean go RTs on the three main trial types: no-stop-signal, stop, and ignore. If stop-failure RT < no-stop-signal RT and no-stop-signal RT = ignore RT, subjects were categorized as

Independent Discriminate then Stop. If stop-failure RT < no-stop-signal RT and no-stop-signal RT < ignore RT, subjects were categorized as Stop then Discriminate. If stop-failure RT >= no-stop-signal RT and no-stop-signal RT < ignore RT, subjects were categorized as Dependent Discriminate then Stop. Therefore, the distinction between the frequently observed strategies (Stop then Discriminate and Dependent Discriminate then Stop) was made by testing whether mean stop-failure RT was less than no-stop-signal RT. However, the Dependent Discriminate then Stop subjects tended to be faster than the Stop then Discriminate subjects. SSD was tracked with a 1 up 1 down tracking algorithm, so the fast subjects had more short SSDs, and this dissertation has shown that whenever SSDs are short the context independence assumption of the race model breaks down. Therefore, selective stopping may not involve any sources of violation that are not present in simple stopping. In both, context independence may be violated when SSD is short.

If simple stopping and selective stopping violations do not differ, then this is evidence that selective stopping does not involve any additional sources of violation that are not present in simple stopping. This would suggest an adjustment to the Bissett and Logan (2014) framework on stimulus selective stopping, especially the suggestion that the violation between going and stopping results from subjects choosing to discriminate the stop stimulus before stopping. If there is larger violation in selective stopping than simple stopping, this supports the Bissett and Logan (2014) idea that the additional processing requirements in stimulus selective stopping contribute to violations of independence.

Between-Subjects Selective Versus Simple Stopping

Two published experiments, Bissett and Logan (2011) Experiment 1 20% stop signals condition and Bissett and Logan (2014) Experiment 1, were compared. In Bissett and Logan (2011), there was only one signal and it was always a signal to stop. In Bissett and Logan (2014) there were two signals, one was the stop signal and the other was an ignore signal, which informed subjects that they should respond quickly and accurately just as if no signal had occurred. The two experiments had the same percentage of stop signals (20%) and used similar auditory tones. Their SSD distributions also overlapped considerably, such that the violation at SSDs <300 ms can be compared over the range of 0-250 ms.

Method

The methods are described in detail in Experiment 1 of Bissett & Logan (2011) and Experiment 1 of Bissett & Logan (2014). The former is the simple stopping group and the latter is the selective stopping group. In both groups, subjects responded to go stimuli that were black shapes on a white background, and the stop (and ignore in the selective stopping experiment) stimuli were auditory tones. Go responses were manual keypresses.

Results

To evaluate the race model in these data, a 2 (No-stop-signal vs. Stop-failure) x 2 (Simple vs. Selective) mixed ANOVA on RT was run. Both main effects were significant, with no-stop-signal RT (M = 504 ms) being slower than stop-failure RT (M = 491 ms), $F(1, 46) = 8.3$, $MSE = 502$, $p = .006$, and RT in the selective stopping task (M = 546 ms) being slower than RT in the simple stopping task (M = 449 ms), $F(1, 46) = 20.4$, $MSE = 11188$, $p < .001$. However, the interaction was not significant, $F(1, 46) = 2.5$, $MSE = 502$, $p = .12$. This analysis suggests that the race model applied to both data sets.

To evaluate the effect of stop stimulus selectivity on violations of independence, observed and predicted stop-failure RTs were compared across SSDs ranging from 0 ms to 250 ms (these were the SSDs for which there were at least 5 subjects with at least 5 stop-failure RTs in each of the two conditions). Two subjects were removed from each group for having no SSDs with at least five stop-failure RTs in this range. A 2 (Predicted vs. Observed) x 2 (Simple vs. Selective) mixed ANOVA was run on median observed and predicted stop-failure RTs to evaluate the effect of selectivity on the violation. There was a main effect of predicted versus observed, $F(1, 42) = 85.5$, $MSE = 1296$, $p < .001$, which resulted from observed stop-failure RT ($M = 472$ ms) being longer than predicted stop-failure RT ($M = 401$ ms). There was also an interaction, with the median violation in selective stopping ($M = 96$ ms) being larger than in simple stopping ($M = 46$ ms), $F(1, 42) = 10.3$, $MSE = 1296$, $p = .003$. The violation in dispersion, as measured by the slope of the Q-Q plots, did not differ between the simple condition ($M = 2.76$) and the selective condition ($M = 2.81$), $t(42) = .17$, $p = .86$. The larger violation in central tendency in selective than simple stopping is consistent with the hypothesis that additional processes specifically involved in stimulus selective stopping, possibly discrimination, contribute to the larger violation of independence in stimulus selective stopping.

Within-Subjects Selective Versus Simple Stopping

The between-subject comparison of selective and simple stopping showed larger violation in central tendency in selective than simple stopping. However, the selective subjects were also slower than the simple stopping subjects, so the larger violation may have resulted from longer RT or the requirement to selectively stop. In the next study, Within-Subjects Selective Versus Simple Stopping, the same subjects did selective and simple stopping and their no-stop-signal RTs were similar across conditions. Therefore, any increase in the extent of violation in selective versus simple stopping can be attributed to the requirement to stop selectively to specific stimuli.

Method

Subjects. Twenty-four young adults recruited from the Nashville area were given $36 for two 90-minute sessions on consecutive days. Two subjects were replaced, one for not showing for the second session and the other for having a probability of stopping outside the 95% confidence interval of the expected probability of stopping.

Apparatus and Stimuli. The apparatus was the same as all previous keypress experiments, though the stimuli differed in the following ways. The go task in both sessions began with three “+” signs, one in the center of the screen flanked horizontally by one two inches to the left and one two inches to the right. The go task differed across sessions and the order of sessions was counterbalanced across subjects. In one session, the central + changed to an “<” or “>” which informed subjects to respond “z” or “m” on the keyboard, respectively. In the other session, either the left or the right + changed to an X which informed subjects to respond z or m, respectively. All stimuli were presented in 24 point font. Both 500 Hz tones and 750 Hz tones were presented through closed headphones (Sennheiser eH 150). There were three conditions in each session, simple stopping with 20% stop signals, simple stopping with 40% stop signals, and selective stopping with 20% stop signals and 20% ignore signals. In the simple stopping conditions, both tones were stop signals. In the selective stopping session, one tone was a stop signal and the other tone was an ignore signal.

Procedure. The procedure was the same as Fixed SSDs 1 with the following exceptions. SSD was tracked with a "1 up 1 down" tracking algorithm (Levitt, 1971; Osman et al., 1986). Each session included three conditions, the 20% simple stopping condition, the 40% simple stopping condition, and the selective stopping conditions (20% stop signals and 20% ignore signals). The following analysis only considered the 20% simple stopping condition and the selective stopping condition. The order of conditions was counterbalanced across subjects, but the order was the same for both the central and peripheral session for each subject.

In simple stopping blocks, subjects were instructed to stop if either the high (750 Hz) or the low (500 Hz) tone was presented. In the selective stopping block, subjects were instructed to stop to one of the two tones and ignore the other (which tone was the stop signal was counterbalanced across subjects). Subjects stopped to the same tone in both sessions.

Subjects were given 10 trials of experimenter-supervised practice on trials without stop signals. They were given another 8 trials of practice that included stop signals for their first condition of the day, and 6 trials of practice before starting each of the subsequent two sessions of each day. After the initial practice, subjects completed two blocks of 260 trials each for the first condition, then practiced the second condition and completed two blocks of 260 trials each for the second condition, then practiced the third condition and completed two blocks of 260 trials each for the third condition. This procedure was repeated in the second session. Between blocks, subjects were given feedback on the speed and accuracy of their no-stop-signal trials from the previous block.

Results

A 2 (Simple vs. Selective) x 2 (Central vs. Peripheral) x 2 (No-stop-signal RT vs. Stop-failure RT) repeated-measures ANOVA was run. There were no significant main effects of simple versus selective or central versus peripheral, and those two variables did not interact, showing that RT was similar across simple and selective stopping and central and peripheral go stimuli. There was a significant main effect of no-stop-signal RT versus stop-failure RT, $F(1, 23) = 29.2$, $MSE = 921$, $p < .001$, as no-stop-signal RT ($M = 477$ ms) was longer than stop-failure RT ($M = 453$ ms). There was also a significant interaction between selective versus simple and no-stop-signal RT versus stop-failure RT, $F(1, 23) = 19.7$, $MSE = 304$, $p < .001$, with a larger difference between no-stop-signal RT and stop-failure RT in the simple conditions ($M = 35$ ms) than in the selective conditions ($M = 13$ ms). To examine whether the difference between no-stop-signal RT and stop-failure RT was significantly different from zero in the selective condition, a planned contrast was run while collapsing across central versus peripheral. This planned contrast showed that the difference between no-stop-signal and stop-failure RT was significantly different from zero, $F(1, 23) = 12.37$, $MSE = 304$, $p = .002$. Therefore, the usual measure of the context independence assumption of the race model suggests that the race model applied to these data.

To evaluate the effect of stimulus selective stopping on violations of independence, observed and predicted stop-failure RTs were compared across SSDs ranging from 150 ms to 250 ms (these were the SSDs for which there were at least 5 subjects with at least 5 stop-failure RTs in each of the four conditions). Twelve subjects were removed for having no SSDs with at least five stop-failure RTs in this range. The violation in central tendency was evaluated in a 2

(Central vs. Peripheral) x 2 (Simple vs. Selective) x 2 (Observed vs. Predicted) repeated-measures ANOVA on stop-failure RT. There was no main effect or interaction involving central versus peripheral (all *p*'s > .05), and that is not a primary variable of interest, so a simpler 2 (Simple vs. Selective) x 2 (Observed vs. Predicted) ANOVA was run on stop-failure RTs. There was a significant main effect of observed versus predicted, $F(1, 11) = 8.4$, $MSE = 308$, $p = .014$, with observed stop-failure RT ($M = 379$ ms) longer than predicted stop-failure RT ($M = 364$ ms). This revealed a violation in central tendency at short SSDs. There was also an interaction, with a larger violation in selective stopping ($M = 23$ ms) than in simple stopping ($M = 6$ ms), $F(1, 11) = 6.2$, $MSE = 143$, $p = .03$. To examine whether the difference between observed and predicted stop-failure RT was significantly different from zero in the simple stopping condition, a planned contrast was run. This planned contrast showed that the difference was not significant, $F(1, 11) = 1.57$, $MSE = 143$, $p = .24$. The slopes of the Q-Q plots revealed a larger violation in dispersion in selective ($M = 2.54$) than simple stopping ($M = 1.8$), $t(11) = 2.88$, $p = .015$. The larger violation in central tendency and dispersion in selective than simple stopping is consistent with the hypothesis that additional processing specifically involved in stimulus selective stopping contributes to the violation. This converges with the between-subject results showing a larger violation in selective than simple subjects.

Discussion: Stimulus Selective Versus Simple Stopping

Bissett and Logan (2014) argued that the stop stimulus discrimination process that is necessary in stimulus selective stopping but not simple stopping results in a violation of context independence, at least in some of their subjects. However, the subjects who violated context independence (Dependent Discriminate then Stop) in those data also tended to have fast go

RTs and short SSDs. Therefore, the subjects who violated context independence (Dependent Discriminate then Stop) may not have chosen a different strategy than those who did not violate context independence (Stop then Discriminate), but instead they may have more frequently sampled the short SSDs that result in long stop-failure RTs.

In the preceding analysis, the violation at short SSDs was compared between simple and selective stopping. If the violation was the same at short SSDs between simple and selective stopping, then the additional demands in selective stopping do not influence the violation in any way. Both between-subjects and within-subjects, the analyses showed that the violations at the same short SSDs were larger in central tendency in stimulus selective stopping than in simple stopping. The within-subject study also suggested that the violation in dispersion may be larger in selective than simple stopping. This result is consistent with Bissett and Logan's (2014) claim that the additional demands to discriminate the stop and ignore stimulus in stimulus selective stopping result in an increased rate of violations of the race model.

One explanation for the larger violation in selective than simple stopping is that the size of the violation at short SSDs depends on the complexity of go and stop processing. Figures 4, 5, and 6 suggest that SSD being short is a necessary condition to produce the violation, as there is little evidence of violations at SSDs >= 300ms. Given a short SSD, the size of the violation may be dependent upon the complexity of processing. The discrimination necessary in selective stopping is more time consuming, complex process than the detection that is necessary is simple stopping (Donders, 1869/1968). As a converging piece of evidence from a previous experiment, the smaller violation in dispersion for the short deadline condition may have resulted from guessing, and guessing may involve less go processing than discriminating the stimulus and responding accurately. Therefore, the size of the violation at short SSDs may modulate with the complexity of go and stop processing.

However, this dissertation calls into question the strategy categorization in Bissett and Logan (2014). The violations in selective stopping are qualitatively similar to simple stopping in that there are large violations at short SSDs and little or no evidence of violations at long SSDs. The above analyses show that there is a quantitative difference with larger violations at short SSDs in selective than simple stopping, but this result does not require assuming different strategies across subjects.

If there are not different strategies in selective stopping, then how are subjects doing stimulus selective stopping? They may all Stop then Discriminate, or they may all Discriminate then Stop. If all subjects are stopping then discriminating, then in both selective and simple stopping a non-selective stopping mechanism is engaged on stop trials, which races against a go process. Only after this non-selective stop process completes does the subject discriminate, so discrimination should not influence the race. An open question is if the same non-selective stopping mechanism is engaged in simple and selective stopping when a stop signal occurs, then why is there a larger violation in selective than simple stopping at short SSDs. One possibility is that even though subjects do not discriminate before stopping is complete, they still have to prepare to discriminate. This preparation may complicate the task, and interact with the go process on stop trials, prolonging stop-failure RTs at short SSDs.

An alternative is that all subjects Discriminate then Stop. As suggested above, the complexity of processing on stop trials may influence the size of the violation at short SSDs. Discrimination in selective stopping is more complex than detection in simple stopping

(Donders, 1868/1969), which may produce a stronger interaction between going and stopping at short SSDs. The subjects who appeared to Stop then Discriminate in Bissett and Logan (2014) may not have sampled the short SSDs necessary to produce the violation.

A final alternative is that the Stop then Discriminate and Discriminate then Stop strategies are used by different subjects, as suggested by Bissett and Logan (2014). If the larger violation in stimulus selective stopping than simple stopping is the result of the additional requirement to discriminate the stop and ignore stimuli, then it should only be present in those subjects who Discriminate then Stop, because subjects who Stop then Discriminate do not discriminate the stop and ignore stimuli before stopping on stop trials. Therefore, the subset of subjects who Stop then Discriminate should have violations similar to the violations in simple stopping, and only the subjects who Discriminate then Stop should have larger violations than simple stopping. Unfortunately, subjects who Stop then Discriminate tend to have slow RTs and long SSDs, making it difficult to evaluate their violations at short SSDs when SSD is determined by tracking. Therefore, to test whether the larger violations in stimulus selective stopping are specific to subjects who Discriminate then Stop, a stimulus selective stopping experiment that uses a wide range of fixed SSDS (like in Fixed SSDs 2) will be necessary in future research. This experiment could test whether the large violations in stimulus selective stopping are specific to subject who Discriminate then Stop, which would support the conclusion that the larger violation in stimulus selective stopping results from the requirement to discriminate the stop signals interacting with the requirement to select a go response. It would also be consistent with the heterogeneity of strategies proposed by Bissett and Logan (2014).

To conclude, the larger violations at short SSDs in selective than simple stopping support the Bissett and Logan (2014) claim that the additional processing demands in stimulus selective stopping contribute to violations of independence. The requirement to discriminate the stop and ignore stimuli may make the processing on stop trials more complex, and complexity may modulate the size of the violation. However, these results bring into question the heterogeneity of strategies across subjects in stimulus selective stopping proposed by Bissett and Logan (2014). Fast and slow selective stopping subjects may be using the same strategy, but only the fast subjects are sampling the short SSDs that are necessary to yield violations of independence.

CHAPTER VII

GENERAL DISCUSSION

Summary

Previous work suggests that the context independence assumption of the Independent Race Model (Logan & Cowan, 1984), which is the main theoretical vehicle used to understand stop signal data, is violated at short SSDs. However, previous work often had few subjects doing a particular task, with few or no manipulations that test hypotheses about the violation. Previous work also often evaluated independence by comparing no-stop-signal RT to stop-failure RT, which I argued in the introduction is not a sensitive measure of independence.

In this dissertation, I proposed new methods for evaluating context independence. In simulations I showed that median estimates of the violation of context independence were less biased than mean estimates. I used Q-Q plots to examine the dispersion of the full distribution of observed and predicted stop-failure RTs, because the race model should not only predict the central tendency of the observed distribution, but all quantiles of the observed distribution. Based upon previous evidence of slow, random fluctuation of RT over time (Nelson et al., 2010), I argued for basing predictions only on no-stop-signal RTs that precede stop trials.

These new measures were applied to 323 subjects across 24 conditions and 471,240 total trials. This work used a larger number of subjects and trials than previous work, with a variety of manipulations that address hypotheses for the violation, and it uses the new methods described in the previous paragraph.

First, aggregating across all conditions showed that most conditions violate the independence assumption at SSDs < 200ms, the proportion reduces considerably at 200 ms and 250 ms until very few subjects violate at SSDs >= 300 ms (see figures 4, 5, and 6). This shows the ubiquity of the violation in a large sample of subjects and conditions.

Second, the relationship between speed and the violation was examined. Short SSDs and fast RTs tend to co-occur, so fast RTs may produce the violations and not short SSDs. I found no consistent evidence for this alternative hypothesis. Using fixed SSDs, I showed that the violation was larger with slower subjects than with faster subjects. Several experiments manipulated RT and conditions with faster RT produced violations that were sometimes similar in size (300 ms deadline), sometimes larger (20% stop signals), and sometimes smaller (saccadic eye movements) than in conditions with slower RT. Therefore, individual differences in RT relate to individual differences in the size of the violation, but manipulations of RT appear to have variable effects according to the nature of the manipulation. In spite of the mixed results across manipulation of RT, one clear conclusion is apparent: neither long nor short RT is sufficient or necessary to produce the violation. The violation occurs when SSD is short, and that conclusion applies to both short and long RTs.

Third, the relationship between stop signal modality and the violation was addressed. Visual go stimuli are the norm in stopping research, but both visual and auditory stop signals are well represented in the literature. The data showed that violations occurred with both auditory and visual stop signals, showing that the effect is not modality-specific. There was no evidence for greater violations with visual stop signals, but there was some evidence suggesting no difference between modality and other evidence suggesting a larger violation with auditory stop signals. The inconsistency across conditions suggests that additional research will be necessary to conclude whether the violation is larger with auditory stop signals.

Last, violations in simple stopping were compared to violations in stimulus selective stopping. The violation was larger in stimulus selective stopping than simple stopping, supporting the hypothesis that additional processing requirements in stimulus selective stopping contribute to the violation of the context independence. This work brings into question the heterogeneity of strategies proposed by Bissett and Logan (2014), and suggests that one strategy may be sufficient to explain the violations at short SSDs in stimulus selective stopping.

Implications and Suggestions

These results have important implications for stop signal research. Independence between going and stopping (Logan & Cowan, 1984; Logan et al., 2014) or independence until a very late interactive stage (Boucher, Palmeri, et al., 2007) are essential assumptions of the race models that are used to understand virtually every stop signal dataset. This work is evidence that current race models do not capture stopping behavior at short SSDs. Therefore, data that include short SSDs may have invalid estimates of SSRT, inhibition functions, and stop-failure RTs.

There may be practical solutions that help to acquire new data that do not violate the race model and extract valid data from existing datasets in which the race model may be violated. These recommendations come in four main classes: restricting the range of SSDs, using conditions that reduce the violation, focusing the SSRT estimate on specific SSDs, or excluding subjects.

The first class of potential solution is to restrict the range of SSDs. One way to do this is to use a range of fixed SSDs that do not include the early SSDs that have a high likelihood of violating the race model (<300 ms). This dissertation suggests that intermediate and long SSDs should not show evidence of a violation of independence, so race models should apply to these data. However, such long SSDs would result in very high probabilities of responding given a stop signal unless go RT is also long. There is evidence that subjects adjust their RT to the range of SSDs, with longer RTs for longer SSDs (Lappin & Eriksen, 1966; Logan, 1981; Ollman, 1973). Therefore, subjects may slow down or wait for the longer SSDs in order to improve their ability to stop. However slowing of RT, especially progressive slowing, may contaminate measures like SSRT (Verbruggen et al., 2013).

It may be better to experimentally prolong RT. For example, if SSDs ranged from 300-600 ms with 50 ms increments, choice RTs similar to what were shown in this dissertation (400-600 ms) would often have no chance to be inhibited as the response would occur before the stop signal. One way to experimentally prolong RT would be to use more go choice response alternatives. Logan et al. (2014) showed that when subjects had six choice alternatives, mean go RT was around 700 ms. The combination of these longer RTs and longer SSDs could result in sampling the intermediate part of the inhibition function, which had been shown to yield the most accurate estimates of SSRT (Band et al., 2003).

The second class of potential solutions for addressing the violation is to use the results from this dissertation to choose experimental conditions that reduce the violation. The previous suggestion of experimentally prolonging go RTs and SSDs may dodge the violation entirely, so that experimental manipulation may prove sufficient. The results in chapters IV, V, and VI suggest that some other manipulations may reduce the violation. Stop probability experiments showed a larger violation with 20% stop signals than 40% stop signals, so using 40% stop signals may result in a smaller violation. The analyses of auditory versus visual stop signals showed some evidence that the violation was larger with auditory stop signals, so using visual stop signals may result in a smaller violation. The stimulus selective stopping versus simple stopping experiments showed a larger violation with stimulus selective stopping than simple stopping, so doing simple stopping should produce smaller violations than stimulus selective stopping. This solution only works in cases where the conditions to avoid, for example stimulus selective stopping, are not the conditions of interest.

The third class of potential solutions is to eliminate some SSDs from the SSRT computation. The first two solutions require experimental adjustments, so this third solution may be the best option for data that have already been acquired. This third solution also does not require implementing unusual stop signal manipulations like difficult go tasks and long SSDs. When SSD is tracked with a 1 up 1 down tracking algorithm, a distribution of SSDs are sampled, and the mean of that distribution is usually 200-250 ms shorter than mean RT (as evidenced by the mean method for calculating SSRT yielding values in the 200-250ms range). If part of the SSD distribution is < 300ms, then this dissertation suggests that the race model may be violated at these SSDs. However, unless go RTs are very fast, much of the SSD distribution should be >= 300 ms, where violations are rare. Therefore, SSRT could be computed with the integration method for each SSD >= 300 ms, and then averaged across SSDs. A more tailored approach would involve testing whether there is a violation at a given SSD across subjects or for each subject, and to only include SSDs that do not violate the race model in the SSRT computation. However, these approaches may include SSDs at the tails of the inhibition function, which are noisy.

An alternative approach involves finding the SSD that occurs most frequently and testing for a violation of independence. If there is a violation, test the second most frequent SSD for a violation, and continue testing until you find the SSD that occurs most frequently but does not violate context independence. When this SSD is found, compute integration SSRT at that single SSD. This focuses the SSRT estimate on the one SSD that is nearest to the central, most informative part of the inhibition function but also does not violate the race model.

The fourth class of solutions involves eliminating subjects. This may be necessary if SSD was tracked and one or more fast subjects exclusively sampled SSDs < 300 ms that produced a violation. The previous three classes of solutions are preferred if possible, as this solution may selectively exclude subjects with certain behavior, like the faster subjects if SSD was tracked. Therefore, a random sample of subjects may be less like their intended population of interest once the faster subjects in the sample are selectively excluded. This may be especially problematic when comparing subject populations or clinical groups, as some groups (e.g., young adults) may have fast RT and shorter SSDs than other groups (e.g., older adults), resulting in a difference in the propensity to violate and therefore different exclusion rates. Considering this problem with this method, this method is not recommended. If a significant number of subjects exclusively or almost exclusively sample SSDs in which violations occurs, the researchers should consider rerunning their study after implementing solutions 1 or 2.

Perhaps the best “solution” to the violations of independence is to embrace the violation. This dissertation documents the violation across a large sample of subjects in different conditions. In virtually all of them, the race model is violated at short SSDs. This suggests that stopping works differently at short SSDs than at long SSDs, and stopping works differently at short SSDs than what is proposed by existing models. The presence of the stop signal interacts and slows go processing. To better understand the relationship between going and stopping, new models that explain this interaction at short SSDs are necessary. In the following section I discuss and speculate about how race models might account for the violation, but this is only a small first step towards understanding the cause of these violations. Future work will hopefully embrace the violation, searching for a fundamental understanding of the interaction between going and stopping at short SSDs.

Toward a Race Model That Can Accommodate Violations of Independence.

The contribution of this dissertation is documentation of the violations of the independence assumption of the race model at short SSDs. It is shown to be widespread across a large sample of subjects in various experimental conditions. A fundamental question remains unknown: What is causing this violation? I speculate in the following sections by entertaining different mechanisms and how they might be accommodated in modern race models.

How Could Race Models Accommodate Violation of Independence? Throughout this dissertation I have focused on the original Independent Race Model (Logan & Cowan, 1984) which is a non-parametric model that is general enough to capture a variety of data. This choice was made because the dissertation is focused on diagnosis, and I wanted to eliminate the possibility of diagnosing violations because of specific parametric assumptions of a process model.

Recent alternatives to the Independent Race Model (Boucher, Palmeri, et al., 2007; Logan et al., 2014; Lo, Boucher, Pare, Schall, & Wang, 2009; Salinas & Stanford, 2013) have characterized the go task as a stochastic accumulation process towards a threshold, and if the go process reaches the threshold a response is made. These models afford more leverage to investigate the underlying processes involved in going and stopping. The final sections of the dissertation explore how the parameters of modern race models (Boucher, Palmeri, et al., 2007; Lo et al., 2009; Logan et al., 2014; Salinas & Stanford, 2013) could be adjusted to accommodate violations of independence.

A commonality of many modern race models is that on stop trials, the stop process involves two stages. First, there is an afferent stage in which the stop signal is apprehended. Then there is an interactive stage in which the stop process inhibits (Boucher, Palmeri, et al., 2007) or decelerates (Salinas & Stanford, 2013) the go process so that it does not reach threshold. In order to fit behavioral data that appears independent (i.e., stop-failure RT < no-stop-signal RT), the stop process must inhibit go accumulation very late (i.e., close to SSD + SSRT) and very potently (Boucher, Palmeri, et al., 2007).

Capacity Sharing or Fusion Prolonging the Afferent Stage. At short SSDs, the behavioral data did not appear independent, because stop-failure RT were longer than expected in the race model. One way to account for this behavioral result in modern race model frameworks is to suggest that at short SSDs, the rate of go accumulation and afferent stage stop accumulation is limited. This could result from going and stopping sharing capacity at short SSDs. Capacity has been characterized as the sum of rates in a stochastic accumulator model framework (Logan et al., 2014; Townsend & Ashby, 1983). If go and stop share capacity, and capacity is limited, then apprehending the stop signal may reduce the rate of go accumulation on stop trials (compared to no-stop-signal trials), slowing stop-failure RT and producing behavior that violates the context independence assumption of race mode.

Logan et al. (2014) asked whether go and stop processes share capacity in the stop signal paradigm. They manipulated the number of go choice alternatives and showed that go RT increased with the number of choice alternatives but SSRT did not. Model fits suggested that capacity is limited between different go alternatives, but the stop process did not share capacity with the go process.

However, most of the SSDs from the Logan et al. (2014) data were >= 300ms, and stop-failure RT tends to be well predicted by race models at these intermediate and late SSDs. At shorter SSDs, the stop stimulus could conflict with going. Duncan said "…if simultaneous stimuli are to be independently identified, with a separate response for each, then some effect of divided attention is almost always to be expected" (page 275, 1980).

Other work has shown that stimuli presented at short stimulus-onset-asynchronies can appear to be simultaneous and can interact. White (1963) showed that successive flashes of light were often judged as only a single flash if presented with a stimulus onset asynchrony <100ms. Interactions between stimuli do not only occur within-modality, and the presentation of a sound can influence visual judgment if the two stimuli occur with a SOA <=100ms (Maeda, Kanai & Shimojo, 2004; Spence, 2011). Therefore, the slowing of go RT at short SSDs may reflect the cost of sharing limited attentional resources between the go and stop stimuli, and this cost may only be apparent when two stimuli interact as a result of being presented in close succession.

This explanation relates to the concept of fusion from the cross-modal literature (Ernst, 2007; Spence, 2011). The idea is that as stimuli co-occur (either in time or one is predictive of the other) the probability that two stimuli fuse (or become integrated into a single percept) increases. One possibility is that when the go and stop stimuli occur close in time, they are sometimes fused together into a single percept, or partially fused into a single percept. If the go stimulus and the stop stimulus are partially fused, they may be perceived as more similar to each other.

In a stochastic accumulator model framework, the drift rate for the go process may be based upon the similarity of the percept to the target stimulus divided by the similarity of the percept to all possible stimuli. Likewise, the drift rate of the stop process may be based upon the similarity of the percept to the stop signal divided by the similarity of the percept to all possible go and stop stimuli. If the go and the stop stimuli are fused or partially fused, then the similarity between the go stimulus percept and the stop stimulus will increase, because the go percept will have some of the attributes of the stop signal. Likewise, the similarity between the stop signal percept and the go stimulus will increase, because the stop percept will have some of the attributes of the go signal. Therefore, if fusion increases the similarity between the go percept and the stop stimulus and the stop percept and the go stimulus, then the drift rate of the go process and the stop process will decrease. Decreased go drift rate at short SSDs will produce prolonged stop-failure RT, and decreased stop drift rate at short SSDs will produce prolonged SSRT.

Therefore, the afferent, perceptual stage of stop processing may share limited capacity and rate of accumulation with the go accumulation process. This may only occur at short SSDs because simultaneous or near-simultaneous stimuli can fuse (Ernst, 2007; Spence, 2011). If go and stop stimuli are fused, this could increase their perceptual similarity, reducing their discriminability, and reducing the rate of accumulation of go and stop activation. Reduced rate of accumulation of go would slow stop-failure RTs, and reduced accumulation of stop would slow SSRTs.

Weakening the Stop Process or Blocking Go Input Could Slow the Deceleration of Go Activation. An alternative to afferent processing reducing the rate of go accumulation is that the final stage of stop processing in which the stop process inhibits (Boucher, Palmeri, et al., 2007) or decelerates (Salinas & Stanford, 2013) go processing is less potent at short SSDs. If inhibition were less potent, the deceleration of the go process could unfold over time. Therefore, sometimes when stop began to inhibit go the go process would not reach threshold, but others times when the stop process began to inhibit the go process it would only slow the accumulation of the go process but not stop it from reaching threshold, producing long stop-failure RT. This deceleration of the go process could result in stop-failure RT that were longer than expected in the race model, especially at the long tail of the stop-failure RT distribution.

Recent modeling work has suggested that inhibition may not be necessary to explain the interaction between stopping and going (Salinas & Stanford, 2013). All that is necessary is that the go process decelerates such that it never reaches threshold. Therefore, this weakening of the stop process hypothesis may not involve inhibition, and it may involve another process like blocked input (Logan & Cowan, 1984). Blocked input is an alternative to interactive inhibition, and it suggests that after the stop signal is perceived, the drive to accumulate go activation is removed, slowing or stopping the go accumulation. Therefore, if at short SSDs this blocking process is weakened, this could result in a slowing of go accumulation (resulting in long stop-failure RT) instead of an immediately turning off of go activation (which cannot result in long stop-failure RT).

Any mechanism that results in weak deceleration of go activation at short SSDs is viable, and it may not be possible to distinguish weaker inhibition from weaker blocked input empirically. An open question is what is producing this deceleration. One possibility is that the potency of inhibition from stop onto go (or the potency of the blocking mechanism that blocks the input to the go process) is variable across trials. Sometimes the stop process strongly inhibits the go process after being apprehended, as suggested in Boucher, Palmeri, et al. (2007), but other times the stop process only weakly inhibits the go process after being apprehended. If SSD is long, the go process has had a long time to accumulate so is likely highly active. Therefore, the weak inhibition from stop onto go has little influence on the highly active go process, and has little time to influence the highly active go process before it reaches threshold. Hence, there is little evidence of the weak inhibition from stop onto go at long SSDs. Alternatively, at short SSDs, the go process has had little time to activate so is only weakly active. Therefore, when the stop process only weakly inhibits the go process, it has a long time to compete with the go process before either reaches threshold. In those instances in which the go process reaches threshold (producing a stop-failure trial), the prolongation of stop-failure RT is the byproduct of the drawn out competition between the go process (that had little activation when it began to interact with the stop process because only a small amount of time had elapsed) and weak inhibition. Hence, weak inhibition from stop onto go may only be apparent when SSD is short.

Conclusions

The context independence assumption of the Independent Race Model (Logan & Cowan, 1984; Logan et al., 2014) assumes that the presence of the stop signal does not affect go processing. Some previous work has brought this assumption into question at short SSDs. In this dissertation, new methods for evaluating context independence were proposed that overcome issues with previous analysis methods. These new method were applied to a large dataset with several different conditions. At short SSDs, the violation occurred in fast and slow subjects, fast and slow conditions, auditory and visual stop signals, and simple and selective stopping. The only necessary and sufficient condition for producing the violation was short SSDs, with violations occurring often at SSDs < 200ms, sometimes at 200ms and 250 ms SSD, and seldom at SSDs >= 300 ms. The violation was larger in slow subjects than fast subjects, suggesting that the violation is a stable individual difference that relates to go RT. The violation was as large or larger with auditory than visual stop signals, consistent with the violation being largely amodal. The violation was larger in stimulus selective than simple stopping, supporting Bissett and Logan's (2014) assertion that the requirement to discriminate stop and ignore stimuli contributes to the violation. However, the violation only occurred at short SSDs in stimulus selective stopping, bringing into question the strategy categorization of Bissett and Logan (2014). The main contribution of this work is documenting the large and consistent violation of independence at short SSDs. This sets the stage for theorizing about the cause of the violation, a process that I began by speculating about how race model could accommodate violations of independence.

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