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Absolute Identification by Relative Judgment

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Abstract

In unidimensional absolute identification tasks, participants identify stimuli that vary along a single dimension. Performance is surprisingly poor compared to discrimination of the same stimuli. Existing models assume that identification is achieved using long-term representations of absolute magnitudes. We propose an alternative relative judgment model (RJM) in which the elemental perceptual units are representations of the differences between current and previous stimuli. These differences are used, together with the previous feedback, to respond. Without using long-term representations of absolute magnitudes, the RJM accounts for (a) information transmission limits, (b) bowed serial position effects, and (c) sequential effects, where responses are biased towards immediately preceding stimuli but away from more distant stimuli (assimilation and contrast).

Absolute Identification by Relative Judgment

Miller (1956) drew attention to a curious phenomenon. People have great difficulty identifying stimuli from a set that varies along a single psychological continuum, even though their ability to discriminate pairs of stimuli from the set suggests that they should be very good at the identification task. This phenomenon can be seen across a wide range of stimulus attributes - the frequency and loudness of tones, the strength of tastes and smells, the magnitude of lengths and areas, the hue and brightness of colors, and the intensity and numerousness of cutaneous stimulation - suggesting some common and fundamental source of the limitation.

In an absolute identification task, participants are required to identify, with a unique label, stimuli drawn from a set of items that vary along only a single continuum. Typically, stimuli are evenly psychologically spaced. A stimulus's label is normally its ordinal position within the set. Three key phenomena, which we review in more detail below, are observed. First, there is a severe limit in the information transmitted from stimulus to response (i.e., the size of the set for which members can be identified perfectly) even when adjacent stimuli are perfectly discriminable. Second, a bow effect is observed when identification accuracy is plotted against stimulus, with an advantage for the smallest and largest stimuli. Third, there are strong sequential effects, whereby the stimuli on previous trials exert a strong bias on the response to the current stimulus.

Many theoretical accounts have been offered of one or more of these phenomena. Nearly all of these models have in common the assumption that in an absolute identification task a representation of the absolute magnitude of the current stimulus is compared to some long-term representations of the absolute magnitudes either of other stimuli from the set, particular anchor values, or particular criterial values. However, in a review for the centenary issue of *Psychological Review*, Shiffrin and Nosofsky (1994, p. 359) conclude that since Miller's (1956) classic article "...a fully unified account of the numerous range, edge, and sequential effects has not been achieved."

Here, in contrast to existing models (excepting Laming, 1984), we offer a relative judgment model (RJM) of absolute identification. The RJM does not utilize long-term representations of absolute magnitudes. Instead, the difference between the current stimulus and the previous stimulus is used, in conjunction with the feedback from the previous trial, to generate a response. Thus, the magnitude of the current stimulus is judged relative to the magnitude of only the immediately preceding stimulus (hence the name RJM). In this article, we review existing models of absolute identification and show that none offers a complete account of the phenomena described above. We then show that the RJM offers a unified account of these phenomena, and present new experimental evidence that supports the model. We begin with a review of the key empirical results.

Empirical Results in Absolute Identification

Information Transmission Limit

Using multivariate information transmission as a dependent variable (McGill, 1954), it is possible to measure the information transmitted[1] in an absolute identification task. If performance in an absolute identification task were perfect, the information transmitted would grow as the number of stimuli is increased. For example, perfect identification of two equally probable stimuli carries 1 bit of information, identification of four stimuli carries 2 bits, of eight stimuli carries 3 bits, and so on. However, the information transmitted from stimulus to response (sometimes, channel capacity) in an absolute identification task seems to be limited to very few bits (see Table 1) corresponding to perfect identification of very few stimuli across a very wide range of stimulus attributes (see Garner, 1962; Laming, 1984; Miller, 1956 for reviews). Figure 1 shows information transmitted as a function of the number of stimuli in the set (with range of the stimuli held constant) for data from Garner (1953) and Pollack (1952). (In all figures that present data, data are collapsed across participants.) With a small number of stimuli, obviously less information must be transmitted, but as the number of stimuli increases, the information transmitted from stimulus to response does not continue to increase. Although an increase in the range of stimuli (number held constant), and hence the separation of the stimuli, will produce an initial increase in information transmitted, the increase is a negatively accelerated function of range, and quickly reaches an asymptote once adjacent stimuli are discriminable (Alluisi & Sidorsky, 1958; Braida & Durlach, 1972; Eriksen & Hake, 1955a; Pollack, 1952).

Bow or Edge Effects in the Serial Position Curve

When accuracy is plotted as a function of the rank of the stimulus within the stimulus set, a characteristic bow is observed in the resulting serial position curve (e.g., Kent & Lamberts, in press; Lacouture & Marley, 2004; Murdock, 1960; W. Siegel, 1972). Performance on stimuli at the ends of the range is better than performance on mid-range stimuli even though, when presented in isolation, any two adjacent stimuli may be perfectly discriminable. As for information transmission, once stimuli are pairwise perfectly discriminable, increased spacing of items leads, at best, to only slight improvements in accuracy (Braida & Durlach, 1972; Brown, Neath, & Chater, 2002; Gravetter & Lockhead, 1973; Hartman, 1954; Lacouture, 1997; Luce, Green, & Weber, 1976; Pollack, 1952). Figure 2 shows the very similar stimulus-response confusion matrices obtained by Brown et al. (2002) for absolute identification of tones varying in their frequency. Tones were geometrically spaced, with each tone a constant ratio higher in frequency than the immediately lower tone. (Following Weber's Law, geometric spacing is typically used to produce stimuli that are presumed to be equally psychologically spaced.) Each confusion matrix is for a different stimulus spacing (from 420 - 563 Hz in the narrow spacing condition to 363 - 652 Hz in the wide spacing condition). As Figure 2 shows, increasing the stimulus spacing had almost no effect on performance.

Increasing the number of stimuli in an absolute identification task increases the size of the bow effect (Alluisi & Sidorsky, 1958; Durlach & Braida, 1969; Lacouture & Marley, 1995; Pollack, 1953; W. Siegel, 1972; Weber, Green, & Luce, 1977). Figure 3 shows the serial position curves obtained by Lacouture and Marley (1995; see also Kent & Lamberts, in press; Lacouture, Li, & Marley, 1998) for different stimulus set sizes, with a larger bow effect for larger set sizes. Note that although Stimuli 5 and 6 can be nearly perfectly discriminated when they constitute the entire stimulus set, performance on these same stimuli drops considerably when they are identified within a larger stimulus set. Simply shifting all the stimuli along the dimension, so that each stimulus increased in value by a constant multiplicative factor, has no effect on the accuracy against stimulus magnitude curve (Lacouture, 1997). The bow effect remains even after extensive practice, although small improvements in accuracy are observed (Alluisi & Sidorsky, 1958; Hartman, 1954; Weber et al., 1977; but see Rouder, Morey, Cowan, & Pfaltz, 2004, for a larger practice effect). The bow effect is greatly reduced by correcting for the asymmetry of errors on extreme verses interior stimuli (Weber et al., 1977), suggesting that the restricted opportunity to make errors at the ends of the range is a major factor underlying the bow effect (see also Eriksen & Hake, 1957). The bow effect is not due to response bias (at least, not response bias alone). In data where end responses are not used more frequently than central responses, the effect is still observed (W. Siegel, 1972). In our data from Experiment 1, the bow is observed although there is a bias against responding with extreme categories.

Sequential Effects

We know of no absolute identification experiment where strong sequence effects (where the response to the current stimulus is shown to depend on previous stimuli and responses) are not found. Of course, when performance in an absolute identification task is perfect, then there are no sequential dependencies. Thus the existence of sequential dependencies is likely to provide a useful insight into processing in an absolute identification task.

The most salient sequential effect is that the response given to the current stimulus is shown to be assimilated to the immediately preceding stimulus (Garner, 1953; Holland & Lockhead, 1968; Hu, 1997; Lacouture, 1997; Lockhead, 1984; Long, 1937; Luce, Nosofsky, Green, & Smith, 1982; Petrov & Anderson, in press; Purks, Callahan, Braida, & Durlach, 1980; Rouder et al., 2004; Staddon, King, & Lockhead, 1980; Stewart, 2001; Ward & Lockhead, 1970, 1971). In other words, participants are systematically biased to respond as if the current stimulus is nearer to the previous stimulus than it actually is. Figure 4 shows data from the feedback condition of Ward and Lockhead's (1970) absolute identification experiment. Stimuli were tones varying in loudness. The average error in responding on the current trial is plotted for each stimulus as a function of the stimulus on the previous trial. When the current stimulus is greater than the previous stimulus, the error is negative (i.e., the stimulus is underestimated); when the current stimulus is less than the previous stimulus, the error is positive. The five lines are approximately parallel, with positive slopes, demonstrating that assimilation takes place for all combinations of current and previous stimuli (Lockhead, 1984). Assimilation to preceding items is also observed in magnitude estimation tasks (e.g., Jesteadt, Luce, & Green, 1977), in matching tasks (Stevens, 1975, p. 275), and in relative intensity judgment tasks (Lockhead & King, 1983).

The effect of stimuli further back in the sequence on the current response is the opposite, that is, there is a contrast effect (Holland & Lockhead, 1968; Lacouture, 1997; Ward & Lockhead, 1970, 1971). Assimilation to the previous trial and contrast to trials further back has been demonstrated within the same experiments, for the same participants. Figure 5 shows the average error on the current trial (averaged across all possible stimuli on the current trial) as a function of the stimulus *k* trials ago for data from Holland and Lockhead (1968), Lacouture (1997), and Ward and Lockhead (1970). As described above, assimilation is shown to the stimulus on the immediately preceding trial ($k = 1$). Stimuli on less recent trials ($k > 1$) exhibit contrast, as shown by the reversal in the sign of the error. The contrast effect is smaller than the assimilation, and the error dependency reduces to zero with increased numbers of intervening trials.

In the experiments on sequence effects discussed so far stimulus, response and feedback are all highly correlated. Which of these is the basis for assimilation (and contrast)? We focus on this question for the remainder of this section.

The sequence effects observed are dependent on the quality of stimulus presentation. Ward and Lockhead (1971) examined performance in a standard absolute identification experiment using line length. When task difficulty was increased, by reducing the luminance and duration of line length presentations, more assimilation was observed. In the difficult condition, accuracy was low and therefore the correlation between stimuli and responses was reduced. Assimilation was demonstrated only to the previous stimulus and not the previous response. This suggests that assimilation to the previous response is only normally observed because the response is correlated with the previous stimulus (see also Garner, 1953; McGill, 1957; Mori, 1998).

Ward and Lockhead (1971) also observed assimilation to the previous trial's feedback but not to the previous response in a guessing task (although there was slight evidence of a small contrast effect to responses further back in the sequence). The guessing task was identical to an absolute identification experiment, except that the stimuli were omitted, and therefore the stimuli could not have been the cause of the assimilation observed. As the task was guessing, there was no correlation between the feedback and the responses. Thus the observation of sequential effects only for the previous feedback but not the previous response in a task where the two are not correlated, also suggests that previous responses are not the locus of sequential effects.

Manipulating the Sequence in an Absolute Identification Task

Manipulating the relative frequencies of the size of the differences between consecutive trials affects identification accuracy. In an absolute identification of loudness task, Luce et al. (1982) used four differently constrained sequences. In one condition, the sequence of trials was constrained so that the current stimulus was either identical to, one step softer than, or one step louder than the previous stimulus. This condition was called the small step (3) condition, because the current stimulus was chosen from one of three stimuli centered on the previous stimulus. In the small step (5) condition, the current stimulus was selected from five adjacent intensities centered on the previous stimulus. In the random condition, the sequence was random. In the large step condition, the current stimulus was at least four stimuli different from the previous stimulus. For all four sequence types, each intensity was equally frequent over the course of the whole experiment. From the identification confusion matrix a measure of the confusability, $d'_{i,i+1}$, of each loudness i with the adjacent loudness $i + 1$, was obtained. This method of analysis allows comparison of identification performance free from contamination by constraints imposed by the control of the sequences in each condition.[2] (The procedure for calculating $d'_{i,i+1}$ is given in Appendix A.) When $d'_{i,i+1}$ is plotted against stimulus magnitude, each condition shows a characteristic bow, with poorer performance for the middle of the range of signals (see the bottom panel in Figure 6). The key result is that the curves lie one above the other, such that tones are more confusable in the conditions where the step size is larger: In order of decreasing identification performance, the curves are small step (3), small step (5), random step, and large step. Smaller transitions seem to lead to higher accuracy (see also Hu, 1997, and Petzold and Haubensak, 2001, for similar findings). (The top panel of Figure 6 shows the corresponding bows in the accuracy serial position curves. Here the ordering of the large step and random conditions is reversed, with better performance in the large step condition because of the restricted possibility for making mistakes imposed by the restricted set of possible responses on each trial.) Further work (Nosofsky, 1983a), testing alternative hypotheses, is consistent with the idea that smaller transitions lead to higher accuracy.

Existing Models of Absolute Identification

There are many existing accounts of some of the phenomena seen in absolute identification data. The extant models can be divided into four main classes: (a) models where memories of recent stimuli are assimilated (Holland & Lockhead, 1968; Lockhead & King, 1983), (b) modified Thurstonian models (Braida et al., 1984; Durlach & Braida, 1969; Purks et al., 1980; Luce et al., 1976; Treisman, 1985), (c) limited response or processing capacity models (Lacouture & Marley, 1991, 1995, 2004; Laming, 1984, 1987; Marley & Cook, 1984, 1986), and (d) exemplar models (Brown et al., 2002; Kent & Lamberts, in press; Nosofsky, 1997; Petrov & Anderson, in press). Below, we briefly review each of these models and consider which of the phenomena outlined (limit in information transmitted, bow effects, and assimilation and contrast) are and are not accounted for by each model. Table 2 gives an overview of the scope of these models. Two themes emerge from this review. First, there are two different types of explanation as to why increasing the range of stimuli does not increase information transmitted. Some models assume a perceptual locus and others assume the limit lies in the response process. The second theme is that current models which assume that long-term representations of absolute magnitudes are the basis for absolute identification do not provide a full account of sequential effects.

Assimilation models

Holland and Lockhead (1968). In Holland and Lockhead's (1968) model, participants are assumed to generate a response by adding the judged distance between the current stimulus and the previous stimulus to the feedback from the previous trial. Assimilation and contrast are accounted for in terms of the contamination of the representations of the absolute magnitudes of stimuli. Specifically, the memory of the previous stimulus is assumed to be contaminated by the memories of earlier stimuli.

Of the phenomena outlined above, Holland and Lockhead's (1968) model accounts for assimilation and contrast, but only on average. For example, consider a low magnitude stimulus on the previous trial. The stimuli on preceding trials are likely to have been larger in magnitude and thus, when the previous stimulus is confused with them, the representation of its magnitude will be an overestimate. This will cause the difference between the current and previous stimuli to be underestimated on average (as the current stimulus is also likely to be larger than the previous stimulus), and lead to the current response being biased towards the previous stimulus (i.e., assimilation). Contrast also follows: on average the current response is biased away from stimuli two or more trials ago because these stimuli are, on average, greater in magnitude than the (low magnitude) stimulus on the immediately preceding trial. However, a detailed examination reveals this account to be unsatisfactory. Typically, assimilation is observed for all combinations of current and previous stimuli (e.g., Ward & Lockhead, 1970; our Experiment 1). Holland and Lockhead's model predicts contrast in some cases. For example, consider the case in absolute identification of 10 stimuli with Stimulus 3 on the preceding trial followed by Stimulus 2 on the current trial. The confused representation of Stimulus 3 will be an overestimate (as earlier stimuli are likely to have been larger), and thus the difference between the current stimulus and the previous stimulus will be overestimated. This produces a contrast effect where assimilation is observed. In addition to this difficulty for the model, Holland and Lockhead give no account of the other phenomena listed in Table 2.

Lockhead and King (1983). In Lockhead and King's (1983, see also Lockhead, 1984) model, two assumptions are made: (a) that successive stimuli assimilate in memory; (b) that people compare each new stimulus to a collection of stimulus memories to determine a response. No psychological mechanism is chosen to motivate these assumptions, "the focus here is on a simple equation to fit the data" (Lockhead, 1984, p. 44). The equation is

$$R_n = S_n + a_1(S_n - S_{n-1}) + a_2(S_n - S_{n-2}) + a_3(S_n - S_{n-3}) + \ldots + \epsilon$$

where R_n is the response on trial n, S_n is the stimulus on trial n, $a_1 < 0$ for assimilation to S_{n-1}, $a_2 > a_3 > \ldots > 0$ for decreasing contrast to less recent stimuli, and ε is a noise term. Although such a model can inevitably describe assimilation and contrast, no consideration is given to other absolute identification phenomena. In our application of Lockhead and King's model, we have found that such a model does not offer an account of the limit in information transmitted or the bow effect.

Modified Thurstonian Models

In a Thurstonian account, presentation of a stimulus results in perception of an absolute magnitude, represented as a noisy value on an internal sensory scale. Criteria, or bounds, divide this scale into response categories. The criteria provide a long-term frame of reference for absolute magnitudes. There are important multidimensional extensions of this idea (e.g., Ashby & Townsend, 1986). The source of variability in responding in the standard Thurstonian model is the noise in the representation of the stimulus on the internal sensory scale.

A simple Thurstonian model can offer some account of the limit in information transmitted as the number of stimuli is increased with the range held constant (the "Range constant" columns in Table 2): As the number of stimuli is increased, the bounds will become closer together, and the fixed magnitude noise on the sensory scale means that a stimulus is more likely to be classified incorrectly. The bow effect can also be explained because there is a limited ability to make mistakes for stimuli at the edges of the range. For example, if the smallest magnitude stimulus is greatly underestimated, then it will still be correctly classified into the first category. The invariance of these phenomena as range is increased (the "N constant" columns in Table 2) and an account of sequential effects require further modification of the model. We evaluate three modifications below.

Durlach and Braida (1969). Durlach and Braida (1969) modified the simple Thurstonian decision model outlined above to include an internal-noise model. Durlach and Braida propose that memory can operate in one of two modes. Here we discuss only the context-coding mode that applies to absolute identification. In the context-coding mode, the presented stimulus is compared to the general context of recent stimulus presentations. The context-coding mode adds an additional source of variability in responding (over and above the perceptual noise in the simple Thurstonian model) which results from "the inability of the subject to determine the context precisely and his inability to determine or represent the relation of the sensation to this context precisely" (p. 374). The standard deviation of the context-coding noise is assumed to be proportional to the range of the stimuli, though no psychological motivation is given for this assumption. The inclusion of a source of variability that grows with the stimulus range allows Durlach and Braida's (1969) preliminary theory of intensity resolution to account for the invariance of the absolute identification phenomena as range is increased (either by increasing the number of stimuli with the spacing held constant or increasing the spacing).

To account for the bow effects, Braida et al. (1984) suggested that the general context is set by two anchors at either end of the range. A stimulus is compared to the general context by counting steps (which are some proportion of the distance between the anchors) using a noisy measurement unit. Thus, there is less variability for stimuli near one of the anchors, as a small number of steps is small, and thus the cumulative error is small.[3]

No mechanism is offered to account for sequential effects. However, Purks et al. (1980) suggest that the distributions that represent signals are unaffected by the location of the previous signal, but that the category boundaries are. By partitioning their data by the previous signal, and fitting a Thurstonian decision bound model to each partition, Purks et al. demonstrate that the separation between signal distributions was unaffected, but the locations of decision boundaries were, being shifted away from the previous signal. The next modification of the simple Thurstonian model extends this idea.

Treisman (1985). Treisman (1985) used criterion-setting theory (Treisman & Williams, 1984) to maintain response criteria in a simple Thurstonian model. Two opposing short-term mechanisms act on the criteria on a trial-by-trial basis. A tracking mechanism, motivated by the assumption that objects in the real world tend to persist, moves criteria away from the currently perceived sensory effect, increasing the probability of a repetition of the previous response. A stabilizing mechanism acts to locate criteria nearer to the prevailing flux of sensory inputs, motivated by the assumption that criteria will be adjusted to maximize information transmitted. Tracking shifts are larger in magnitude than stabilizing shifts, but decay more quickly. Thus, Treisman's model predicts assimilation to the immediately preceding stimulus, when tracking shifts will dominate, but contrast to less recent stimuli, when stabilizing shifts will dominate. However, this account does not fully explain the sequential biases. Treisman (1985, p. 192) states that the magnitude of criteria movement decreases with the distance of the criteria from the stimulus. Therefore, the model would be expected to predict greater assimilation where previous and current stimuli are similar. However, assimilation is greater where stimuli differ more, rather than less (see, e.g., Figure 4). For the same reason, Treisman's model predicts that the error in responding should be greater if the previous and current stimuli are similar: Luce et al. (1982), Nosfosky (1983), Hu (1997), and Rouder et al. (2004) found the opposite result.

The magnitude of the stabilizing shift is, unlike tracking shifts, not fixed, but instead proportional to the distance between the sensory input and the nearest criterion. The magnitude of stabilization is thus proportional to the inter-criterion distance and therefore also proportional to the range. This allows Treisman's (1985) model to explain why increasing the range of stimuli does not increase information transmitted. But it also means that as the range is increased, stabilization should come to dominate, predicting a change in the pattern of assimilation and contrast which is not observed (e.g., Experiment 1 below).

Treisman's (1985) model does not predict the gradual and smooth U-shaped pattern of the bow effect (instead accuracy is approximately equal for all but the two most extreme stimuli). However, if criteria in the central region are more closely spaced, bow effects will be more U-shaped. Such a spacing would also lead to a response bias for extreme stimuli, but the opposite pattern - a central tendency in responding - is typically observed (see Experiment 1).

Luce, Green, and Weber (1976). Luce et al. (1976) proposed a modified Thurstonian model, where an attention band roves over the stimulus range. Items falling in the band result in a less variable Thurstonian representation than those that do not. As the stimulus range is increased, the probability of stimuli falling inside the fixed width band is reduced, causing a reduction in identification performance. This allows the model to predict a limit in performance as stimulus range is increased. With the additional assumption that the attention band dwells at the edges of the range of stimuli, bow or edge effects are accounted for, although no motivation for this assumption is given. The further assumption, that attention tends to dwell on the location of the last stimuli, explains the finding that there is typically reduced variation in responding when the previous stimulus is similar to the current stimulus (e.g., Luce et al., 1982). However, the attention band model does not offer an account of the systematic bias in responding to the current stimulus by preceding stimuli (i.e., assimilation and contrast).

Restricted Capacity Models

Cook, Lacouture, and Marley have presented three models of absolute identification that account for limits on information transmitted and bow effects by assuming a limited capacity process in either memory or response processes (not perceptual processes). These models can account for limits in performance as stimulus range is increased because they do not assume that the limit in information transmitted is perceptual.

Marley and Cook (1984, 1986), Karpiuk, Lacouture, and Marley (1997). In Marley and Cook's (1984, 1986) models, perception is assumed to be absolute, with the location of the stimulus represented accurately on a Thurstonian continuum. The exact location on the continuum is unavailable to the response process, and must be deduced by comparing the stimulus to the context in which it is presented. Marley and Cook assume that the context, which comprises a set of elements, must be rehearsed. Each element's activation is incremented each time a pulse arrives from a Poisson pulse process, before its activation continues to decay exponentially. There is a fixed total rehearsal capacity modeled by limiting the pulse rate across all elements. The location of the stimulus within the continuum of elements is derived by summing the activation of the elements between the stimulus and known anchors (cf. Braida et al., 1984). The anchors are assumed to be at or outside the location of the extreme stimuli. Marley and Cook (1984) show that, under these assumptions, the variability of the total activity of elements to one side of the stimulus increases with the number of elements.

Marley and Cook (1984) demonstrated that the model can account for: (a) the asymptote in information transmission as stimulus range increases, with the number of stimuli held constant, or as the number of stimuli increases, with the spacing held constant; and (b) the bow effect. Karpiuk et al. (1997) extended the model to predict reaction time distributions. Marley and Cook provide no account of the sequential effects observed. Marley and Cook also point out that, if their model were extended to provide the necessary account by assuming the range of the rehearsal is determined by the immediately preceding context, it is not clear how it could explain assimilation and contrast without a further addition to the model.

Lacouture and Marley (1991). Lacouture and Marley (1991) demonstrated that a simple network model could provide a reasonable account of the limit in information transmitted. The model was a three layer feed-forward network that learned by mean-variance back-propagation of error. Input vectors of adjacent stimuli overlapped. For example, if Stimulus 5 was presented, input unit 5 would be activated, but neighboring hidden units, 4 and 6, would also be activated to a lesser extent. The model predicts the limit in information transmitted when the number of hidden units is one, although the observed characteristic shape of the information transmitted against set size (see our Figure 1) is not well reproduced. The model does however produce a good fit to Braida and Durlach's (1972) data, where information transmitted was measured as stimulus range was varied (with set size held constant). Modeling of bow and sequence effects is not described. The model could not provide an account of sequence effects without substantial modification because, once learning has reached asymptote, the representation and processing of a stimulus is independent of immediately preceding stimuli.

Lacouture and Marley (1995, 2004). Lacouture and Marley's (1995, 2004) mapping model is a feed-forward network with one single input unit, one single hidden unit, and an output unit for each response. The activation of the input unit represents the magnitude of the stimulus. Perception is assumed to be noisy and repeated presentation of the same stimulus does not always lead to the same activation. The hidden unit normalizes this activation using a lower and an upper anchor value so that the resulting activation falls within the range 0 to 1. Fixed magnitude noise which represents a noisy mapping process is then added, resulting in a limited channel capacity. With a large number of stimuli, the resulting set of possible mean hidden unit activations will be closer together than for a smaller set and, thus, the fixed magnitude noise will have a greater effect on performance for larger sets. The mapping of the hidden unit activation onto output units acts to partition the unit interval into response categories. For each output unit, activation is accumulated over the course of the trial, with the corresponding response being emitted once the accumulator reaches a given threshold (Lacouture & Marley, 1995). The assumption of repeated intra-trial sampling of the output units allows the model to predict response times as well as accuracy, providing an extension over previous models. Lacouture and Marley (2004) replace the accumulator and threshold with a leaky, competing accumulator (Usher & McClelland, 2001) to capture full correct response time distributions.

The mapping model provides an account of the limit on information transmitted and of bow effect for different set sizes. By incorporating the (unmotivated) assumption that, after a response is made, there will be less variation in the output of that unit and those immediately adjacent to it on the next trial, the data from the sequence manipulation experiments (Luce et al., 1982) are also accounted for. However, this model suffers the same difficulty as the attention band model described above in accounting for sequence effects. Lacouture and Marley (2004) suggest three modifications to the model that might allow a future version of the model to account for sequential effects: (a) Instead of normalizing hidden unit activation by using two anchor values, previous stimulus values could be used. (b) Hidden unit activations may be contaminated with hidden unit activations from previous stimulus presentations. (c) The leaky competing accumulators may begin each trial with some residual activation carried over from previous trials.

Laming's (1984, 1997) Relative Judgment Model

Laming (1984) describes a model that accounts for the limit in information transmission and the effects of constraining possible jump sizes between successive stimuli (i.e., Luce et al., 1982). The crucial assumption in Laming's model is that all judgments are relative to the immediate preceding context (i.e., that it is the differences between successive stimuli that are used, not the absolute magnitudes of the stimuli). Further, Laming proposes that such relative judgments are limited. Specifically, Laming suggests that the current stimulus can be judged as 'much less than', 'less than', 'equal to', 'more than', or 'much more than' the previous stimulus. This judgment limit provides a limit in the information transmitted. Numerical estimates of the stimulus magnitudes are assigned such that they follow the same pattern. If the difference between stimuli is judged as 'less than', for example, then the number assigned to the estimate of the magnitude of the current stimulus is less than the estimate of the magnitude of the previous stimulus. The ordering of Luce et al.'s (1982) conditions is also explained by Laming's model. Laming shows that the variability in responding depends mainly on the mean squared jump sizes in the sequence and, as jump size predicts perfectly the ordering of performance in Luce et al.'s conditions, so does Laming's model.

Laming (1984) does not offer an account of the bow in the serial position curve, or of assimilation and contrast. Indeed, Laming states that an additional principle will be required to provide an account of the sequential effects observed in magnitude estimation and absolute identification. He suggests taking into account the prior expectations of the distribution of stimulus magnitudes as a candidate principle.

Exemplar Models

Exemplar models (Medin & Schaffer, 1978; Nosofsky, 1986) assume a long-term memory for each stimulus's magnitude, together with the label associated with that stimulus. On presentation of a stimulus, the probability of a given response is given by similarity of the presented stimulus to the memory of the stimulus associated with that response divided by the summed similarity of the presented stimulus to each stimulus memory.

Brown, Neath, and Chater (2002). Brown et al.'s (2002) model of scale-invariant memory, perception and learning (SIMPLE) has been applied to absolute identification data (as well as free, serial, and probed recall memory tasks). The model is an exemplar model of absolute identification, and is equivalent to the generalized context model (Nosofsky, 1986) in its application to absolute identification.

Exemplar models of absolute identification provide a reasonable account of bow effects. Bow effects are accounted for because items at the end of the range have fewer similar neighbors to be confused with. However, exemplar models do not predict the gradual bowing that is typically observed: Instead, all items tend to show almost identical levels of performance, except for superior performance on the very edge items. It is possible to provide a better fit by biasing the responses associated with more extreme stimuli. However, this bias for extreme responses is at odds with the central tendency in responding that is typically observed. Further, as described above, the bow effect is still observed in data where each response is used equally often (W. Siegel, 1972) or where middle responses are used more often (see Experiment 1).

Exemplar models face a further problem. Recall that increasing the spacing of stimuli does not remove the bow effect, and leads only to a slight improvement in accuracy. Exemplar models, however, predict a large improvement in accuracy, as items become more discriminable. Brown et al. (2002) introduce the assumption that discriminability is inversely related to stimulus range and show that, with this additional assumption, the bow effects are invariant under stimulus range. Exemplar models do not predict any curves in *d'* without further assumptions.

In their simplest form, exemplar models offer no account of sequence effects. When adapted to predict sequence effects, typically by assuming more recent exemplars are more available in memory and/or weighted more heavily in the subsequent decision process (e.g., Nosofsky & Palmeri, 1997; see also Elliott & Anderson, 1995) the models do not correctly predict sequence effects observed in classification (Stewart and Brown, 2004; Stewart, Brown, & Chater, 2002). Increased weighting of more recent items will make a prediction similar to assimilation, as repetition of the previous response will be more likely. However, the criticism applied to the Thurstonian models above applies: An increased probability of repetition is not equivalent to assimilation. Further, this modification will provide no account of contrast.

Nosofsky (1997). Nosofsky (1997) applied Nosofsky and Palmeri's (1997) exemplar-based random walk model, which is an extension of the generalized context model (Nosofsky, 1986), to predict responses and reaction times in absolute identification. Stimuli are represented by normal distributions on a psychological continuum, with stimuli at the edges of the range assumed to be less variable. In this way, an account of bow or edge effects is built into the model. On presentation of a test item, the model assumes memories race to be retrieved. The probability of a memory being retrieved at a given time is a function of the exemplar's similarity to the test item, and the exemplar's strength in memory. Once an item is retrieved, a counter for the associated category label is incremented and all others decremented. The remaining items then race again. When any counter falls too low, the response associated with the counter leaves the race. When a counter reaches a given threshold, the response associated with that counter is emitted, with the reaction time being a function of the sum of the times for each retrieval. No mechanism is outlined for prediction of sequence effects, and no explanation is offered on the invariance of the bow in serial position when stimulus range is altered.

Petrov and Anderson (in press). Petrov and Anderson (in press) present a scaling model (ANCHOR) based on the ACT-R architecture (Anderson & Lebière, 1998) that they apply to absolute identification and category rating. The perception of the absolute magnitude of a stimulus is compared to anchors or exemplars stored in memory. Perception is assumed to be stochastic. The selection of exemplars is also stochastic, and depends upon the similarity between the exemplars and the target stimulus and also upon the frequency and recency with which each exemplar was previously used. Exemplars compete for selection. One exemplar is selected and the associated response is retrieved. If there is a discrepancy between the exemplar magnitude and the percept magnitude, then an adjustment is applied to the response to correct it either up or down. The system is adaptive and, after feedback, the location of the associated exemplar is assimilated towards the percept.

Petrov and Anderson fitted the model to their own data from an absolute identification of nine stimuli. The model was able to fit simultaneously the information transmitted, central tendency in responding, assimilation (on average), and a small practice effect. The model did not predict bows in *d*', but was able to predict an accuracy advantage for end stimuli because of the limited opportunity for errors at the ends of the range.

Petrov and Anderson did not model the effect of increasing the number of stimuli (with the range held constant) or the range of the stimuli (with the number held constant) and, thus, it is uncertain whether the model could account for the effects of these variables. However, the model does include noisy components that are independent of the spacing of the stimuli, and so it may well be able to account for the effects without modification.

Petrov and Anderson did not examine assimilation in detail. In Figure 7, we show the predictions of ANCHOR for the effect on the current response (a) of the previous stimulus and current stimulus (top panel) and (b) for the effect of the stimulus at different lags (bottom panel). We used the parameters that Petrov and Anderon (in press, pp. 23-24 and p. 35) report as best fitting their data and ran 200 simulations of 450 trials. Although ANCHOR does predict assimilation on average, it does not predict the detailed pattern that is normally observed (e.g., Figure 4 and Experiment 1) in which assimilation increases as the difference between the previous and current stimuli increases. In ANCHOR, sequential effects are caused by exemplars being weighted by their recency of use. Thus, ANCHOR fails to predict the more detailed pattern of assimilation for the same reason as Treisman (1985) and Luce et al.'s (1976) models: Predicting that the response associated with the previous stimulus is more likely to be repeated is not the same as predicting that the current response will be biased towards the previous stimulus. Further, the model does not predict contrast to stimuli at lags of 2 or greater, and instead predicts assimilation to stimuli at these lags.

Kent and Lamberts (in press). Kent and Lamberts (in press) present an application of Lambert's (2000) extended generalized context model (EGCM) to absolute identification. The EGCM differs from the GCM in assuming that the amount of information about a stimulus magnitude increases over the time course of the stimulus presentation as stimulus elements are sampled. The probability that sampling is halted and a response is given increases as more elements are sampled. The EGCM was able to predict bow in accuracy and mean correct reaction times, as well as the complete reaction time distributions for individual stimuli. By allowing more generalization for larger set sizes and less sampling for larger set sizes, the EGCM could also predict changes in accuracy and reaction time as set size varied. An alternative, more parsimonious modification, in which the information contributed by each additional sample was a decreasing function of the number of samples, also allowed the model to account for set-size effects. Kent and Lamberts did not model the effect of changing the stimulus spacing. However, without altering the discriminability (cf. Brown et al., 2002), the EGCM predicts that performance will increase greatly with increased spacing (limits in information transmitted have also not been modeled). The model does not predict any sequential effects.

Motivation of the Relative Judgment Model

Having reviewed the key empirical phenomena and the existing models of absolute identification, we next lay out the motivation for the RJM. We have made two main choices in developing the RJM. First, we assume that the locus of the limit in performance is not perceptual but judgmental. Second, we assume that judgment is relative and not absolute. We give our motivation for these assumptions below.

The Locus of the Effects is Not Perceptual

As we have reviewed above, as the range of the stimuli is increased, performance quickly reaches asymptote (Braida & Durlach, 1972; Brown et al., 2002; Eriksen & Hake, 1955a; Gravetter & Lockhead, 1973; Hartman, 1954; Luce et al., 1976; Pollack, 1952). Further, stimuli that can be identified perfectly when presented in isolation are poorly identified when presented within a larger set (Lacouture & Marley, 1995; see also Nosofsky, 1983a; Pollack, 1953). Typically, the variability in magnitude estimates is approximately two orders of magnitude greater than variability in threshold discriminations of the same stimuli (Laming, 1997; see also Miller, 1956; Shiffrin & Nosofsky, 1994). Theorists have taken two different approaches in accounting for these effects. One approach is to assume that the locus of the limit in performance is perceptual, and that when the range of stimuli is increased, there is a large increase in perceptual noise that keeps the information transmitted at the same level. For example, Luce et al. (1982) assume that perceptual noise increases with stimulus range because of a limit in the range over which attention can be focussed. Braida and Durlach (1972) and Gravetter and Lockhead (1973) assume that the noise in the location of the criteria in a Thurstonian model increases with stimulus range. (Although Braida and Durlach's and Gravetter and Lockhead's assumption concerns noise in the location of criteria rather than percepts, it is still noise on a perceptual scale with perceptual units.) Instead, in the RJM, we assume that what limits performance is noise in the processes of mapping a continuous valued estimate of the response onto response categories. Lacouture and Marley's (1995, 2004) mapping model makes the same assumption. They assume that stimulus magnitudes are scaled onto a hidden unit activation which ranges over the unit interval and that constant variance noise (completely independent of the stimulus range) is responsible for the limit in capacity. In the RJM, in assuming that mapping rather than perceptual noise is responsible for the limit in capacity, we do not require any additional assumptions to explain the lack of an improvement in performance when stimulus range is increased.

One reason for this approach is parsimony. As described above, the limit in information transmitted is approximately constant across a wide range of stimulus types (see Table 1; Miller, 1956; Garner, 1962; Laming, 1984). The differences in the exact amount of information that can be transferred are perhaps less important that the fact that the limit in channel capacity seems to be generally so low. Miller (1956, p. 86) concludes, "There seems to be some limit built into us either by learning or by the design of our nervous system, a limit that keeps our channel capacities in this general range." The fact that similarly low limits in channel capacity are found across such a wide range of stimulus attributes suggests that there is a common cause to this limitation, especially as the same bow and sequential effects are observed across the same wide range. Of course, it could be that this cause is duplicated across the different sensory apparatus used in each task. But a more parsimonious explanation is that the cause resides not in the perceptual system, but in the judgment system responsible for producing responses.

Relative Rather than Absolute Judgment

A limitation in all of the above models (excepting Lockhead and King's, 1983, descriptive model) is the difficulty in predicting the ubiquitous pattern of assimilation and contrast (see, e.g., Figure 5 and Experiment 1). For example, a variety of modifications of the Thurstonian model have not proved adequate: Allowing the location of the criteria to be updated from trial to trial (Treisman, 1985) and allowing a resolution improving attention band to shadow stimuli (Luce et al., 1976) have both failed. Similarly, an adequate account of sequential effects has also eluded exemplar models, where the weighting of recent exemplars and the updating of their locations from trial to trial has also failed (Petrov & Anderson, in press). Here, we propose that these models find accommodating sequential effects difficult because they are based on the assumption that long-term absolute magnitude information is the basis for absolute identification performance. In the Thurstonian models, the position of the criteria provide long-term absolute magnitude information. In the Lacouture and Marley's (1991, 1995, 2004) connectionist model, long-term absolute magnitude information about the most extreme stimuli is used in rescaling each stimulus magnitude. In the exemplar models, the memory for the magnitude of each exemplar provides long-term absolute magnitude information. It may be that some future modification of these models would allow them to fully predict the pattern of sequential effects, but in the RJM we show how these sequential effects follow naturally from a relative judgment account.

J. A. Siegel and W. Siegel (1972) review evidence that long-term representation of attributes such as pitch and loudness may be very poor: Memory for pitch, as measured in a same-different judgment task, decays very rapidly with the duration of tone or unfilled interval between the standard and comparison tones (Bachem, 1954; Harris, 1952; Kinchla & Smyzer, 1967; Koester, 1945, as cited in Massaro, 1970, and Wickelgren, 1966; König, 1957; Tanner, 1961; Wickelgren, 1966, 1969; Wolfe, 1886, as cited in Massaro, 1970, and Wickelgren, 1966). Massaro (1970) found that, if the intervening tone was similar to the standard, this disrupted judgment further. In their review article, J. A. Siegel and W. Siegel conclude that the limit in absolute identification performance "is *not* limited by stimulus information, but rather by subjects' inability to maintain multiple representations of sensory stimuli in memory" (p. 313). If a long-term representation of the absolute magnitude of a stimulus cannot be maintained successfully across only a single intervening stimulus, or even an unfilled interval, in a trial of a same-different judgment task (where the intervening stimulus can be ignored), then it is very unlikely that long-term representations of absolute magnitude can be maintained across the (on average) larger number of intervening trials in an absolute identification experiment.

In the RJM, we instead suggest that, in the absence of stable and accurate long-term representations of the absolute magnitudes of stimuli, participants instead rely upon a relative comparison of the current stimulus to the previous stimulus. This relative difference is then used in conjunction with the feedback from the previous trial to generate a response (cf. Holland & Lockhead, 1968). Our intuition, which we test below, was that a model where responding on the current trial depends on information from the preceding trial might offer a simple account of sequential effects. We are not the first to suggest that psychophysical judgment might be relative. In reviews of the psychophysical literature, Helson (1964) and Laming (1997) both suggest that psychophysical judgment is relative. Lockhead (1992; 2004) also reaches this conclusion, although he suggests that, because single attributes cannot be abstracted from the object in which they occur, it is entire objects, rather than their constituent attributes, that are judged relative to one another. In absolute identification, where objects (stimuli) vary on only a single attribute, these alternatives are equivalent.

Stewart and Brown (2004) have found evidence in perceptual categorization that supports the idea that the response on the current trial is generated by comparing the current stimulus to the preceding stimulus. They examined sequential effects in a binary categorization of tones varying in frequency, where low frequency tones belonged to one category and high frequency tones belonged to the other. If participants could maintain even only a single long-term absolute magnitude (the category boundary), then this categorization task should be trivial, as stimuli could simply be compared to this reference point and categorized accordingly. Instead, Stewart and Brown found strong sequential effects, consistent with participants making an ordinal comparison between the current stimulus and the preceding stimulus. Accuracy was only high when comparison to an immediately preceding stimulus determined the categorization. For example, if a stimulus was lower in magnitude than the preceding stimulus, and the preceding stimulus was from the low category, then the current stimulus was correctly categorized as a member of the low category. These data are consistent with the idea that the categorization of the previous stimulus, together with a judgment of the difference between the current stimulus and the previous stimulus, inform the current categorization decision.

Other data are difficult to explain with an absolute account. If judgment were absolute, then the effect of the previous stimulus on the current response should be viewed as a biasing of absolute judgment. Attenuation of sequential effects should therefore lead to an improvement in identification performance. Stewart and Chater (2003) found that a manipulation which attenuated sequential effects instead reduced identification accuracy. Stewart and Chater had participants perform an absolute identification of eight loudnesses. However, each loudness was randomly presented as either a pure, sinusoidal tone or a white noise hiss. When consecutive stimuli were of different types (a hiss followed by tone or a tone followed by hiss) there was a significantly smaller correlation between the previous stimulus and the current response compared to when stimuli were of the same types (two consecutive hisses or two consecutive tones). Accuracy was also significantly lower when consecutive stimuli were of different types compared to when consecutive stimuli were of the same type. This result is the opposite to what would be expected if absolute judgments are being made: Reducing the biasing caused by the previous stimulus should have increased accuracy. However, this result is expected if the loudness of current stimulus is judged relative to the previous stimulus: A switch in the stimulus type will make the comparison of loudnesses more difficult, reducing the accuracy on the current trial.

The idea that long-term representations of absolute magnitudes are not available may well be too strong. There are some data that are problematic for this view. Ward and Lockhead (1970) and Ward (1987) ran several psychophysical tasks requiring either absolute or relative judgment (absolute identification, category judgment, estimation of the ratio of successive magnitudes, absolute magnitude estimation and cross modality matching). On different days, they varied the loudness of the entire stimulus set. Whether or not participants were performing relative or absolute judgment tasks, the judgments on each day were systematically biased towards the stimulus-response mapping from the previous day. This suggests that some representation of the absolute magnitudes of stimuli persists over an interval of at least one day. Thus, it may be that long-term absolute magnitude information is available in absolute identification, but that its representation is rather poor or "fuzzy" (Ward, 1987, p. 226) and not sufficient to support absolute identification. Alternatively, the information may be available, but (for some unknown reason) not used. Consistent with this possibility, long-term absolute magnitude information seems to be weighted more heavily when instructions suggest using a long-term frame of reference (DeCarlo & Cross, 1990; DeCarlo, 1994) or when inter-trial intervals were large (DeCarlo, 1992). (Stewart and Brown, 2004, give a more detailed discussion of these data.) Our core claim - that absolute identification is achieved by relative judgment - is consistent with either the possibility that long-term representations of absolute magnitudes are poor or that the long-term representations are (for some unknown reason) unused.

Summary

In summary, two shortcomings of existing models have motivated the RJM. Models which assume the locus of the limit in information transmitted is perceptual fail to predict (or require modification to predict) that channel capacity remains severely limited even for very large stimulus spacings. In the RJM, the limit in channel capacity is not perceptual. Models which use long-term representations of absolute magnitudes do not capture the sequential effects adequately. In the RJM, as the name suggests, judgment is instead relative to the immediately preceding stimulus. Next we give a detailed specification of the RJM.

Mathematical Specification of the Relative Judgment Model

In what follows, we refer to the current trial in an experiment as trial *n*, the previous trials as trial n - 1, and the kth most recent trial as trial n - k. The physical magnitude of the stimulus on trial n is denoted X_n, the rank of the stimulus within the set S_n, the response R_n, the feedback F_n, and the error in responding $E_n = R_n - S_n$.

The elemental unit admitted to the decision process is assumed to be the difference between S_n and S_{n-1}. In other words, what is admitted to the decision process on trial n is not some representation of the magnitude of S_n, but a representation of the difference between S_n and S_{n-1}. This difference, $D_{n,n-1}$, is given by the logarithm of the ratio of the physical magnitudes.

$$D_{n,n-1} = A \ln\left(\frac{X_n}{X_{n-1}}\right) \tag{1}$$

where A is a constant that depends on the sensory dimension. The use of the ratio follows from Weber's Law. A rearrangement of Equation (1) gives $D_{n,n-1} = A \ln(X_n) - A \ln(X_{n-1})$. If Fechner's logarithmic law relating physical magnitude to the subjective, psychological percept holds, then $D_{n,n-1}$ is the arithmetic difference between psychological magnitudes. If stimuli are geometrically spaced with spacing r (i.e., each stimulus is a constant ratio r larger in physical magnitude than the next highest in magnitude), as is nearly always the case in absolute identification experiments, then

$$D_{n,n-1} = A \ln(r)(S_n - S_{n-1}) \quad . \tag{2}$$

This difference $D_{n,n-1}$ is assumed to be contaminated by residual representations of earlier differences $D_{n-1,n-2}$, $D_{n-2,n-3}$, ... Equivalently, elements of the representation of $D_{n,n-1}$ are assumed to be confused with elements of the representations of $D_{n-1,n-2}$, $D_{n-2,n-3}$, ... (cf. Estes, 1950). The result of this confusion or contamination is labeled $D^C_{n,n-1}$.

$$D^C_{n,n-1} = \sum_{i=0}^{n-2} \alpha_i D_{n-i,n-i-1} \tag{3}$$

The α coefficients are constrained to be in the range $0 \le \alpha \le 1$. The coefficient for the current difference α_0 is fixed at 1. Further, the coefficients are constrained to be monotonically decreasing (i.e., $\alpha_i > \alpha_{i+1}$), so that more recent differences are more likely to be confused with the current difference. The idea that representations may be confused is quite ubiquitous in psychology. What is unique in the RJM is the assumption that it is stimulus differences that are confused, and this follows from our initial assumption that it is stimulus differences rather than absolute magnitudes that are elemental. That is, $D^{C}_{n,n-1}$ can be considered the result of a confusion of stimulus differences in exactly the same way as any other representations might be confused.

To produce a response, the difference $D^{C}_{n,n-1}$ is converted to a difference on the response scale by dividing by a constant λ. (λ represents the subjective size of the difference that corresponds to a single unit on the response scale; see Luce & Green, 1974, and Marley, 1976, for a similar approach in magnitude estimation.) The result is then added to the feedback from the previous trial (cf. Holland & Lockhead, 1968). It is at this point that we assume that there is a limit in channel capacity. Next we outline the form that this limit takes in the RJM.

The limit in the channel capacity in the RJM is assumed to arise from noise in mapping the stimulus difference onto the response scale. Lacouture and Marley (1995, 2004), Marley and Cook (1984, 1986), and Petrov and Anderson (in press) also assume that the limit in channel capacity is (at least partly) a result of noisy mapping. These authors give detailed mechanistic accounts of the mapping process: in terms of a limited capacity rehearsal of the context in a Thurstonian model (Marley & Cook, 1984, 1986); in terms of noisy activation of a single connectionist unit (Lacouture & Marley, 1995, 2004); or in terms of exemplars competing for selection (Petrov & Anderson, in press). Here we do not choose between these accounts or offer our own mechanistic account. Instead we borrow a general principle from all of these accounts. In each account, as the number of response categories is increased, fixed magnitude noise in the mapping process leads to greater confusion between response categories. For example, in Lacouture and Marley's mapping model, stimulus magnitudes are represented by the activation of a hidden unit in a connectionist network. The activation of the hidden unit varies between 0 and 1. Fixed magnitude noise is added to the activation of the hidden unit. In experimental blocks where the set size is larger and, thus, the spacing of stimuli on the hidden unit's unit interval is closer, the fixed magnitude noise causes greater confusion between response categories. In the RJM, we make the additional assumption that, on a given trial, some responses can be ruled out because of knowledge of F_{n-1} and the sign of $D^C_{n,n-1}$. The limited capacity is then used to represent only the remaining responses. For example, if S_n is perceived as being less than S_{n-1}, then only those responses less than F_{n-1} are represented. If S_n is greater than S_{n-1}, then only those responses greater than F_{n-1} are represented. This assumption follows directly from the initial assumption that judgment is relative.

We assume that the noise in the mapping process is normally distributed with variance σ^2, and that this variance will be constant from trial to trial (and also from experiment to experiment). However, as we outlined above, the effect of this noise on responding will not be constant from trial to trial because the number of candidate responses will vary from trial to trial. Consider the example illustrated in Figure 8A for the absolute identification of 10 stimuli. If $S_{n-1} = 4$ and $S_n = 8$, then because $D^C_{n,n-1}/\lambda \approx 8 - 4 = +4$ is positive, R_n must be higher than $F_{n-1} = 4$. The response scale must now be partitioned into six responses (i.e., 5, 6, 7, 8, 9, and 10). Compare this case with the case illustrated in Figure 8B. Now, $S_{n-1} = 6$ and, as before, R_n must be higher than $F_{n-1} = 6$. Now, however, the same limited capacity can be used to represent only four candidate responses (i.e., 7, 8, 9, and 10). In this latter condition, the same variance noise in mapping translates into less confusion among responses, because the limited capacity is partitioned into fewer response categories.

Two pieces of empirical evidence support this assumption. First, there is almost never a problem deciding whether S_n is higher or lower than S_{n-1}. In only 1.2% of the responses in Experiment 1 (below) was the sign of the differences between consecutive stimuli mistaken (i.e., responding with a higher number than F_{n-1} when S_n was lower in frequency than S_{n-1}, and vice versa). We describe the second piece of evidence in Experiment 2, which provides a direct test of the assumption that F_{n-1} is used together with $D^C_{n,n-1}$ in generating R_n.

Equation 4 implements the conversion of $D^C_{n,n-1}$ into a difference on the response scale, and then the subsequent addition to F_{n-1} within a fixed limited capacity that is used to represent the range of possible responses.

$$\boldsymbol{R_n} = F_{n-1} + \frac{D^C_{n,n-1}}{\lambda} + \rho \boldsymbol{Z} \tag{4}$$

where $\boldsymbol{Z}$ is a normally distributed random variable that represents the noise in the mapping process with a mean of 0 and a standard deviation of σ, and ρ represents the range of possible responses (given the sign of $D^C_{n,n-1}$ and F_{n-1}) and scales the fixed noise within the limited capacity onto the response scale. Thus $\boldsymbol{R}_n$ is a normally distributed random variable. ρ is specified exactly in Equation 5.

$$\rho = \left\{ \begin{matrix} N - F_{n-1} & if\, D^C_{n,n-1} > +\chi \\ 1 & if - \chi \leqslant D^C_{n,n-1} \leqslant +\chi \\ F_{n-1} - 1 & if\, D^C_{n,n-1} < -\chi \end{matrix} \right\} \tag{5}$$

where N is the number of stimuli and χ is a criterion whose magnitude $D^C_{n,n-1}$ must exceed for S_n and S_{n-1} to be assumed to be different. χ is assumed to be fixed at half of the stimulus spacing throughout this paper.

It is important to acknowledge here that Equations 4 and 5 do not represent a detailed mechanistic account of how the difference between the current stimulus and the previous stimulus is combined with the feedback from the previous trial to produce a response. We simply intend Equations 4 and 5 to represent the assumption that a fixed magnitude noise in the mapping process has a greater effect on trials where there are more candidate responses. As we described above, the prediction that noise in the mapping process will increase as the set size increases emerges from many accounts of absolute identification in which the mapping process is specified in detail (e.g., Lacouture & Marley, 1995, in press; Marley & Cook, 1984, 1986; Petrov & Anderson, in press). In the RJM, we assume that, the noise varies not only from block to block in an experiment as the set size is manipulated, but also from trial to trial as the number of available responses varies (constrained by F_{n-1} and $D^{C}_{n,n-1}$). In Equations 4 and 5, we assume that the noise in responding will grow linearly with the number of available responses. We return to this issue later in this article, and suggest that this simple assumption may need to be modified.

We assume that the location of the N - 1 criteria, labeled $x_1, x_2, ..., x_{N-1}$, that partition the response scale is such that accuracy is maximized. The probability of a given response r is given by the total density of $\boldsymbol{R_n}$ within the range

$$x_{r-1} < \boldsymbol{R_n} < x_r \qquad (6)$$

with the lower and upper bounds replaced by -∞ and +∞ for the lowest and highest responses respectively. Figure 9 shows the placement of criteria that maximizes the proportion of correct responses for the parameters that best fit the data from Experiment 1. The dashed lines are drawn at 1.5, 2.5, ..., 9.5. Notice that the optimal placement for criteria is more extreme than criteria half way between integer values on the scale. This displacement is approximately a linear function of the distance of the criteria x_r from the center of the scale. (When predicting the displacement of x_r from $r + 1 / 2$ as a linear function of r, 99.96% of the variance is accounted for.)

This outward displacement of optimal criteria happens because of a mathematical fact proved by James and Stein (1961). James and Stein demonstrated that when predicting three or more population means from three or more observations (one from each population), the best estimate of each mean is not given by each observation. Instead, estimates derived by shrinking each observation towards the grand mean of all the observations are better.[4] Efron and Morris (1977) provide an accessible introduction to James-Stein estimators, and give the example of baseball players: The best estimate of a player's batting average in the next season is not given by their score in the previous season. A better estimate is derived if their score is shrunk towards the grand mean of all baseball players' scores. Here, we are trying to estimate the correct response from a single noisy estimate (i.e., a single sample from $\boldsymbol{R}_n$). James and Stein's result tells us that the single sample from $\boldsymbol{R}_n$ will be too extreme compared to the mean response. Thus, it is optimal to place the criteria at more extreme locations. Equation 7 gives the location of each criterion.

$$x_r = \bar{R} + \frac{r + \frac{1}{2} - \bar{R}}{1 - c} \tag{7}$$

where $\bar{R}$ is the mean of the response scale (i.e., $(N + 1) / 2$) and $0 < c < 1$ is a James-Stein estimator. As c becomes larger, criteria are more outwardly displaced.

Fitting the RJM to Existing Data

In this section, we describe the mechanics of fitting the RJM to the data reviewed above. The RJM has a set of α parameters that describe the confusion of the representations of the differences between stimuli, a λ parameter that scales between differences and response scale units, a noise parameter σ, and a James-Stein estimator c. For simplicity, the quantity $1 / (A \ln(r))$ is absorbed into the λ parameter. Thus, when $\lambda = 1$, the size of the difference between stimuli that corresponds to a single unit on the response scale is perfectly estimated. $\lambda < 1$ represents an underestimation of the stimulus spacing and $\lambda > 1$ represents an overestimation.

Except where specified otherwise, the data modeled in this section were collected from experiments where a random sequence of stimuli was used. In modeling these data, the RJM was used to generate predictions of the probabilities of each response for every possible combination of the preceding and current stimuli. The relevant descriptive statistics were then calculated as in the original experiments. For fits to these existing data sets, the MSE between each data point and the predicted value was minimized. The best fitting parameter values were found using both a downhill simplex procedure and Brent's method (Press, Flannery, Teukolsky, & Vetterling, 1992). Fits were repeated with a large set of random starting values.

Best fitting parameters for each model are given in Table 3. Some data sets did not adequately constrain all of the parameters. For example, in fitting the pattern of assimilation and contrast in Figure 5, a wide range of σ parameters was observed in the fits. When this happened, a fit was chosen from the subset of fits with a MSE within 1% of the best fit MSE that had values for the unconstrained parameters similar to those found in fitting other data sets.

The RJM Account of the Bow Effect

In Figure 2, a single fit is presented for all three stimulus spacing conditions in Brown et al.'s (2002) experiment. The data did not constrain the α parameters for less recent stimuli, and so a restricted version of the RJM with $\alpha_3 = \alpha_4 = \alpha_5 = 0$ was fitted. The RJM provides an excellent fit to the characteristic bow.

The primary explanation for the bow effect is that there is a limited opportunity to make errors at the end of the stimulus range. For example, Stimulus 1 can only be mistaken for larger stimuli but Stimulus 5 can be mistaken for smaller or larger stimuli. However, given that the error observed is normally only one, or maybe sometimes two response units (see, for example, the confusion matrices in Figure 2), the restricted opportunity to make mistakes can really only account for the peaks at each end of the accuracy against stimulus magnitude curve, and does not offer an account of the gradual smooth curve over the entire stimulus range.

The gradual bowing is accounted for because the effect of the limited decision capacity is greatest for the central stimuli. When S_n lies in the center of the range, averaging over all possible S_{n-1}, the range of possible R_n constrained by F_{n-1} and $D_{n,n-1}$ is, on average, larger than when S_n lies at an extreme. For example, Table 4 shows the range of possible responses for two cases, $S_n = 1$ and $S_n = 5$. Averaged over all possible S_{n-1}, the range of possible responses is smaller for the extreme stimulus. Thus, the differences leading to extreme stimuli can, on average, be translated more accurately than differences leading to central stimuli within the limited capacity. Optimal use of a limited capacity decision scale, then, acts to reduce accuracy in the center of the range, more than it reduces accuracy at the edge of the range. Together with the limited opportunity for errors at the edge of the range, the effects produce the characteristic smooth bow.

A single set of parameters was used across all of the set sizes in fitting the bow effects in accuracy in Lacouture and Marley's (1995) data (Figure 3). With only the set size differing between fits, the RJM is able to provide a good account of the bows. A key observation is that the limited capacity for representing the range of possible responses can represent two possible alternatives nearly perfectly, but not many more. For this reason, the decision process will not add noise in a task were two stimuli are identified but will add noise when there are more than two stimuli. The RJM is able to account for the dependency of the bow effect on set size because, as the set size increases, the average magnitude of both the differences between stimuli and the range of possible responses on any given trial increases. Both of these increases lead to more variability in responding.

A shortcoming of the RJM in its present instantiation is that it does not make predictions about reaction times. Reaction times are faster for extreme stimuli (e.g., Kent & Lamberts, in press; Lacouture & Marley, 1995). As described above, some models can predict this effect (e.g., Lacouture & Marley, 1995; Nosofsky, 1997) and other models go further and predict reaction time distributions for each stimulus (Karpiuk et al., 1997; Kent & Lamberts, in press; Lacouture & Marley, 2004). Though we have not addressed this issue in this article, it may be possible to extend the RJM to predict response time distributions. In its present form, noise is added to the quantity $F_{n-1} + D^{C}_{n,n-1}/\lambda$ (see Equation 4) and the result $\boldsymbol{R}_n$ is partitioned into response categories (see Equation 6) using fixed criteria. Instead of assuming that response probabilities are given directly by the integrals of the probability density between relevant criteria, a set of leaky competing accumulators (Usher & McClelland, 2001) could be used (following Lacouture & Marley, 2004). By using F_{n-1} and the sign of $D^{C}_{n,n-1}$ to restrict which accumulators from the full set (one for each response category) enter into the competition on each trial, full response time distributions that are conditional upon the current stimulus and previous stimuli could be predicted. Although it would be hard to test the fully conditional distribution predictions because of the large amount of data required (e.g., for absolute identification of 10 stimuli and sequence effects up to a lag of five trials, there would be 10^6 possible sequences, each requiring of the order of 100 repetitions to construct full reaction time distributions), this extension of the RJM could be tested with data conditional upon, say, just the current stimulus.

The RJM Account of the Limit in Information Transmission

In modeling the limit in information transmitted (Figure 1), a single set of best fitting parameters was used across all set sizes. Again, the data did not constrain the α parameters for less recent stimuli, and so a restricted version of the RJM with $\alpha_3 = \alpha_4 = \alpha_5 = 0$ was fitted. Dashed lines represent the fits to the Pollack (1952) and Garner (1953) data. Information transmitted is completely defined by the confusion matrix. Thus, the RJM's account of the limit in information transmitted as the number of stimuli is increased is the same as that given above for the confusion matrix.

The RJM Account of the Restricted Sequence Designs

Fits of the RJM to Luce et al.'s (1982) proportion correct data are shown in Figure 6, using a single set of parameters for all conditions. In fitting these data, the responses that the RJM could generate on each trial were restricted to be those available to participants. There were 10 criteria dividing the response scale into 11 categories positioned according to Equation 7. However, in the small step (3) condition, for example, when each stimulus was either one smaller than, the same as, or one larger than the previous stimulus, only the relevant 2 of these 10 criteria that divided the response scale into the categories F_{n-1} -1, F_{n-1}, and F_{n-1} + 1 were used.

As before, the data did not constrain the α parameters for less recent stimuli, and so a restricted version of the RJM with $\alpha_3 = \alpha_4 = \alpha_5 = 0$ was fitted. The RJM fits the bow effect in mean proportion correct and orders the sequences correctly. Within the RJM, the advantage for conditions with smaller transitions results from the cumulative effect of the smaller transitions, in agreement with Nosofsky (1983a) and Luce et al.'s (1982) conclusions. With smaller transitions, because the range of previous stimuli will be smaller, $\boldsymbol{D}^{C}_{n,n-1}$ is less variable (Equation 3). Together with reduced noise in the mapping process when a stimulus is repeated (Equations 4 and 5), with is more likely with smaller transitions, these two properties of the RJM give higher accuracy for sequences with smaller transitions. (As we noted in the initial presentation of Luce et al.'s data, accuracy is higher in the large step condition than the random condition, because the responses available on each trial are restricted in the large step condition. However, performance measured by *d'* is higher for the random step condition than the large step condition. Immediately below, we show that the RJM predicts this.)

The RJM Account of Bows in Discriminability

Using the parameter set from fitting the accuracy data (above), we also generated predictions for *d'* for Brown et al.'s (2002; our Figure 10) data, Lacouture and Marley's (1995; our Figure 3) data and for Luce et al.'s (1982; our Figure 6) data. Parameters from the accuracy fits were used because the optimization algorithm performed very poorly when fitting *d'* directly. The RJM does predict bows in *d'* because the effect of the limited decision capacity is greatest for central stimuli, as described above. However, the size of this effect is systematically underestimated. The RJM predicts that the confusion between the lowest two stimuli and also between the highest two stimuli is larger than is actually observed. Below, we consider two modifications that should allow the RJM to predict better the bows in *d'*.

In the RJM, we assume that the limit in channel capacity is caused by fixed variability noise in mapping between stimulus differences and the response scale. In Equation 4, we make the perhaps overly simple assumption that the effect of this fixed mapping noise σ on the variability in responding on a given trial is a linear function of the number of available responses ρ (see Equation 5). If one assumes a different relationship (specifically, a convex relationship when the effect on the response scale is plotted against the range of possible responses) then greater bows in *d*' can be predicted. Motivating the functional form of this relationship will require more detailed assumptions about the procedural nature of the mapping from stimulus differences to the response scale. Lacouture and Marley (1995, 2004) assume that the limit results from the range of stimulus magnitudes being represented by the activation of a single noisy unit in a connectionist network. Marley and Cooke (1984, 1986) assume the limit in mapping arises because of a limited rehearsal capacity in maintaining the context against which a stimulus is judged. Either one of these assumptions could be incorporated into the RJM.

An alternative modification would be to assume that the edge stimuli are somehow privileged, as other authors do (e.g., Braida et al., 1984; Marley & Cook, 1984, 1986; Nosofksy 1997). For example, participants may utilize representations of the magnitude of the extreme stimuli if they occur two or three trials ago which they do not utilize for interior stimuli. Alternatively, maybe a limited number of long-term representations of absolute magnitudes can be maintained, and the extreme stimuli are preferentially selected. Though these assumptions would complicate the RJM, they may prove to be necessary in providing a fuller account of the *d*' data.

The RJM Account of Assimilation and Contrast

Fits of the RJM to the sequential effects of Holland and Lockhead (1968), Lacouture (1997), and Ward and Lockhead (1970) are shown in Figures 4 and 5. In the RJM, sequence effects result from the confusion of the representation of the current difference with the representation of other differences. The complex pattern of first assimilation and then contrast emerges naturally from the RJM, as a consequence of the assumption that more recent differences are more likely to be confused or contaminated with the current difference than less recent differences are. Equations 2 and 3 may be substituted into Equation 4 to give

$$\boldsymbol{R}_n = F_{n-1} + \frac{A\ln(r)}{\lambda}\left[\alpha_0(S_n - S_{n-1}) + \alpha_1(S_{n-1} - S_{n-2}) + \alpha_2(S_{n-2} - S_{n-3}) + \ldots\right] + \rho\,\boldsymbol{Z}\,.$$

Recalling that, when feedback is provided, $F_{n-1} = S_{n-1}$ rearranging by collecting together S_{n-i} terms gives

$$\begin{aligned}\boldsymbol{R}_n = {} & \frac{A\ln(r)}{\lambda}\alpha_0 S_n + \left(1 + \frac{A\ln(r)}{\lambda}(\alpha_1 - \alpha_0)\right)S_{n-1} + \frac{A\ln(r)}{\lambda}(\alpha_2 - \alpha_1)S_{n-2} \\ & + \frac{A\ln(r)}{\lambda}(\alpha_3 - \alpha_2)S_{n-3} + \ldots + \rho\,\boldsymbol{Z}\end{aligned} \qquad (8)$$

For assimilation of R_n to S_{n-1} to occur,

$$1 + \frac{A\ln(r)}{\lambda}(\alpha_1 - \alpha_0) > 0 \quad .$$

Given an approximately correct estimate of the stimulus difference corresponding to a single unit on the response scale (i.e., $\lambda = A\ \ln(r)$) and recalling that the availability of the current difference was set at $\alpha_0 = 1$, then this inequality reduces to $\alpha_1 > 0$. α_1, which represents the extent to which $D_{n-1,n-2}$ is confused with $D_{n,n-1}$, is positive and so assimilation is predicted. Assimilation is still predicted if $\lambda \neq A\ \ln(r)$ unless λ is greatly overestimated.

For contrast of R_n to S_{n-2} to occur,

$$\frac{A\ln(r)}{\lambda}(\alpha_2 - \alpha_1) < 0$$

(with similar expressions for less recent stimuli). Given that $A\ \ln(r)$ and λ are both positive, and that $\alpha_1 > \alpha_2$ (reflecting a greater confusion of $D_{n,n-1}$ with $D_{n-1,n-2}$ than with $D_{n-2,n-3}$) contrast is always predicted.

In summary, in the RJM, the awkward pattern of assimilation and then contrast follows in a straightforward way from the assumption that the current difference is confused more with more recent differences. To capture this pattern in a model where absolute stimulus magnitudes are used would be difficult, because one would need to motivate a switch in the sign of the coefficient for S_{n-1} compared to S_{n-2}, S_{n-3}, ...

The RJM Account of the Response Scale Shrinkage

The data in Figure 4 show that, irrespective of S_{n-1}, if S_n is small it is overestimated (i.e., $E_n > 0$) and if S_n is larger it is underestimated ($E_n < 0$). The RJM model accounts for this pattern because optimally-located response criteria x_r are spread outwards from the center of the scale.

Extensions of the RJM

The Effect of Range

Thus far we have accounted for the key phenomena in absolute identification by assuming that the locus of these effects lies purely in the response process. We have assumed that the effects are not perceptual, because all of the effects are seen with very widely spaced stimuli. When stimuli are already widely spaced, increasing their spacing does little to improve performance (e.g., Brown et al., 2002). However, if stimuli are closely-spaced then increasing their spacing does improve performance (e.g., Braida & Durlach, 1972; see our Figure 11), up to an asymptotic limit. So, at least for closely-spaced stimuli, stimulus noise does play some role in limiting performance in absolute identification. The version of the RJM presented above cannot account for this result, because the RJM predicts no effect of increasing the range of the stimuli. Because of the ratio in Equation 1, the RJM is scale free (Chater & Brown, 1999). That is, the magnitude of all of the stimuli could be increased by any factor and the same predictions would be made (see Lacouture, 1997, for an empirical demonstration). However, it is straightforward to extend the model to include a stimulus noise component by replacing Equation 1 with

$$\boldsymbol{D}_{n,n-1} = A \ln\left(\frac{\boldsymbol{X}_n}{\boldsymbol{X}_{n-1}}\right) \tag{9}$$

where stimulus magnitudes are now random variables. Substituting Equation 9 into Equation 3 gives

$$\boldsymbol{D}^C_{n,n-1} = \alpha_0 A \ln \boldsymbol{X}_n + (\alpha_1 - \alpha_0) A \ln \boldsymbol{X}_{n-1} + (\alpha_2 - \alpha_1) A \ln \boldsymbol{X}_{n-2} + \ldots \ .$$

Thus $\boldsymbol{D}^C_{n,n-1}$ is also a random variable, which we assume to be normally distributed (which follows from assuming that the distribution of the logarithm of stimulus magnitudes is normal) with standard deviation σ_s. This version of the model can account for the data in Figure 11 using a single parameter set for all stimulus ranges ($\alpha_1 = 0.271$, $\alpha_2 = 0.253$, $c = 0.109$, $\sigma = 0.025$, $\lambda = 0.892$, and $\sigma_s = 0.129$).

Even with this modification there are still some data that are problematic for the RJM. Nosofsky (1983b) investigated whether stimulus noise or criteria noise was increased when the stimulus range was increased in absolute judgment of the intensity of tones. By fitting a simple Thurstonian model Nosofsky found evidence that both stimulus and criterial noise increased with range. In its present form, the RJM does not predict an increase in stimulus noise and would require a further assumption, such as a limit in the width of an attention band (cf. Luce et al., 1976).

Designs with Uneven Stimulus Spacing

Stimuli are not always evenly spaced in absolute identification designs. For example,Lockhead and Hinson (1986) investigated performance in an absolute identification of three tones differing in intensity. They used three different stimulus sets. In the even-spread condition, stimuli were evenly spaced at 58, 60, and 62 dBA. In the low-spread condition, the lowest stimulus was 4 dB less intense (i.e., a set of 54, 60 and 62 dBA intensities). In the high-spread condition, the highest stimulus was 4 dB more intense (i.e., a set of 58, 60, and 66 dBA intensities). Figure 12 gives the confusion matrices for each condition. Comparing the low- and even-spread conditions, it can be seen that identification performance of the common stimuli (Stimuli 2 and 3) is affected by the location of Stimulus 1. Stimuli 2 and 3 are more likely to be confused when Stimulus 1 is lower. A similar (mirror image) pattern is seen when the the high- and even-spread conditions are compared.

The RJM can capture this pattern. Because stimuli are no longer evenly spaced, we have not used Equation 7 to place criteria. Instead, we have left the criteria as free parameters. In the even condition, we introduced the constraint $x_2 = 4 - x_1$, to ensure that x_2 is displaced away from the center of the scale in exactly the same way as x_1. (Although parameterized slightly differently, this model is exactly equivalent to the RJM described in the Mathematical Specification.) In the low and high spread conditions, x_1 and x_2 were allowed to vary freely, with the constraint that there was symmetry between the low and high spread conditions (i.e., $x_{1,\text{ low}} = 6 - x_{1,\text{ high}}$ and $x_{2,\text{ low}} = 6 - x_{2,\text{ high}}$). Figure 12 shows the best fits that minimize MSE between the data points and the model predictions ($\alpha = 0.073$, $\lambda = 0.373$, $\sigma = 0.285$, $x_{1,\text{ even}} = 0.578$, $x_{1,\text{ low}} = 1.047$, $x_{2,\text{ low}} = 5.886$). The model fits the data reasonably well ($r^2 = .98$). Comparing the low-spread and even conditions, the model correctly predicts that Stimuli 2 and 3 are more likely to be confused when Stimulus 1 is low. Comparing the high-spread and even conditions, the model correctly predicts that Stimuli 1 and 2 are more likely to be confused when Stimulus 3 is high. (Deviations of the data from model predictions are mainly caused by a lack of symmetry in the data. For example, in the data for the even condition, Stimulus 2 is responded to with 3 more often that 1, suggesting that stimuli were not exactly evenly spaced psychologically.)

The Effect of Omitting Feedback

The RJM relies upon the previous feedback in generating a response (see Equation 4). Here we extend the RJM to situations where feedback is omitted. Omitting feedback also alters the sequential effects in absolute identification. When feedback is omitted, information transmitted from the previous response increases, and information transmitted from the previous stimulus is reduced (Mori & Ward, 1995). Note that, in this task, Mori and Ward held accuracy constant across the feedback and no-feedback conditions. The change in sequential effects is thus not due to an overall change in accuracy or task difficulty. In addition to the change in the pattern of information transmission, assimilation, rather than contrast, is observed to stimuli further back in the sequence, and the effect of previous responses is greater than the effects of previous stimuli (see Lockhead, 1984, for a review).

In the RJM, an estimate of the difference between the current and previous trials is used together with the feedback from the previous trial to generate a response (see Equation 4). In the absence of feedback, we assume that participants use their response from the previous trial as the best estimate of the correct answer on the previous trial (i.e., F_{n-1} is replaced with R_{n-1} in Equation 4). To test this assumption, we ran 100,000 simulated trials of an absolute identification of 10 stimuli using the RJM with the best-fitting parameter values for the data from Experiment 1. Following Mori and Ward's (1995) design, the presence of feedback was alternated every 20 trials (i.e., 20 trials with feedback followed by 20 trials without feedback). We then calculated the amount of information transmitted from the previous stimulus and from the previous response to the current response separately for feedback and no-feedback trials (see Table 5). Consistent with Mori and Ward's finding, when feedback was omitted, the amount of information transmitted from the previous response was increased and the amount of information transmitted from the previous stimulus was reduced. We also examined sequential effects separately for feedback and no-feedback trials (see Figure 13). Again, consistent with experimental results (Lockhead, 1984), we found that assimilation at larger lags, instead of contrast, was observed when feedback was omitted. In summary, the assumption that participants use their previous response as the best estimate of the correct answer in the absence of feedback and then proceed with a relative judgment strategy correctly predicts the effects of omitting feedback.

Experiment 1

Whilst the RJM is able to provide an account of all of the main phenomena described above, there are no raw data available that will allow these effects to be simultaneously observed and modeled. It is possible, therefore, that whilst the RJM can provide an account of each effect in isolation, the relative sizes of the effects may be such that they cannot be simultaneously modeled. For this reason, a standard absolute identification of frequency experiment was run to provide the raw data necessary to rule out this possibility. Two stimulus spacings were crossed with three set sizes to produce six conditions.

Method

Participants. One hundred and twenty University of Warwick undergraduates participated in this 45-minute experiment for course credit or payment of £5 (approximately U.S. $8). Participants had at most one previous experience of an absolute identification of frequency experiment.[5]

Stimuli. Stimuli were two sets of 10 tones varying in frequency. In the wide-spacing condition, the lowest tone had a frequency of 600.00 Hz, with each subsequent tone increasing in frequency by 12%. Thus the wide tones had a total range of 1063.85 Hz. In the narrow-spacing condition, the lowest tone had a frequency of 768.70 Hz, with each subsequent tone increasing in frequency by 6%. Thus, the total range in the narrow condition was 530 Hz. Because, in both conditions, frequency increased by a constant percentage, tones were equally spaced in log space, and were intended to be approximately evenly spaced psychologically. The center of the range of the wide spacing condition coincided with the center of the range of the narrow condition in log space (i.e., in each condition the tones had the same geometric mean). In the set-size-8 conditions, only the middle 8 tones were used. Similarly in the set-size-6 conditions only the middle 6 tones were used.

Each tone was 500 ms in duration, with the beginning 50 ms ramped linearly from silence to maximum amplitude, and the end 50 ms ramped linearly from maximum amplitude to silence.

Design. Two factors were varied between participants: (a) the spacing of the tones was narrow or wide; and (b) the set size was 6, 8, or 10 stimuli. These two factors were crossed producing six conditions. Participants were assigned to each condition at random, with the constraint that there were an equal number of participants in each condition.

Procedure. Participants were tested individually in a quiet room. Participants experienced seven blocks of 120 tones, with each tone occurring equally often in each block. The ordering of the tones within a block was random. The breaks between blocks were self timed by participants. Tones were delivered to participants using Sennheiser eH2270 headphones. The headphones were of high quality to ensure that tones sounded approximately equally loud over the entire frequency range. At the same time as the tone was played, a "?" prompt appeared on the screen until participants responded. Participants were free to respond from the onset of each tone using the number keys along the top of a standard keyboard. Other key presses were ignored. For half of the participants in each cell, tones were numbered from lowest to highest and, for the other half of the participants, this mapping was reversed.

Participants were told that each tone was one of a set of 6, 8, or 10 varying in frequency, and the ordering of the responses (either low numbers for low stimuli and high numbers for high stimuli, or vice versa). Immediately after their response, the prompt was removed and the correct answer was displayed on the screen for 750 ms. There was a silent pause and blank screen for a duration of 500 ms before the next trial began automatically.

Results

As sequence effects in absolute identification are of concern here, data from the first 10 trials in every block were excluded from analysis so that only data where participants are some way into the sequences are considered. The descriptive statistics presented were calculated for each participant and then averaged across participants.

Average Accuracy. For each of the six cells in the design (three set sizes x two stimulus spacings), the proportion of correct responses made by each participant was calculated. Participants whose proportion was more than two inter-quartile ranges above the upper quartile or below the lower quartile were to be excluded as outliers. In fact, only one participant in the narrow-spacing-set-size-6 cell was eliminated from subsequent analysis (their accuracy was too low).

To test out intention that stimuli would be evenly psychologically spaced, average accuracy on the lower pitched half of the stimuli was compared to average accuracy on the higher pitched half of the stimuli. There was a significant, $t(118) = 5.07$, $p < .0001$, but small

(3%) accuracy advantage for higher pitched stimuli. This suggests that stimuli were not quite psychologically evenly spaced, but that higher pitched stimuli were more widely spaced. To remove this asymmetry from the data in the following analyses data were collapsed across mapping (so that Stimulus 1 was the lowest pitched for half of participants and highest pitched for the other half).

Figure 14 shows how the proportion of correct responses varied through the experiment for each cell of the design. Accuracy was higher for the smaller set sizes, and for the wider stimulus spacings. Accuracy also improved slightly over blocks. This description of results is confirmed by a three-way ANOVA, with three levels of set size (6, 8, and 10), two levels of stimulus spacing (narrow and wide), and seven levels of block. All of the main effects were significant: set size, $F(2, 113) = 45.98$, $p < .0001$; stimulus spacing, $F(1, 113) = 11.13$, $p = .0012$; and block, $F(6, 678) = 26.47$, $p < .0001$. None of the interactions were significant.

In the remainder of this analysis, only data from the last five blocks were used, where performance is approximately at asymptote. Using data from all blocks gives a very similar pattern of results.

Information Transmitted. Table 6 shows the average information transmitted from stimulus to response. Information transmitted is approximately constant across set sizes, and increases with spacing. This pattern is confirmed by a two-way ANOVA with spacing and set size as factors. There was no main effect of set size, $F(2, 113) = 0.49$, $p = .61$. There was a significant effect of spacing, $F(1, 113) = 7.87$, $p = .0059$. There was no significant interaction, $F(2, 113) = 0.39$, $p = .68$.

Central Tendency in Responding. There was a significant central tendency in responding. Figure 15 shows the proportion of times each response was used for each stimulus spacing and set size. The significance was determined by fitting a quadratic to the proportions for each participant. The coefficient of the squared term was, on average, significantly below zero, $t(118) = 6.77$, $p < .0001$.

In addition to the central tendency in responding, there was a tendency for people to press the keys on the right of the keyboard more than those on the left. However, as the assignment of response numbers to keys was counterbalanced across participants, this tendency is not seen in the averaged data.

Bow in the Serial Position Curve. The proportion of correct responses is plotted against stimulus for each stimulus spacing and set size in the top panel of Figure 16. Separate two-way ANOVAs (stimulus x spacing) were run for each set size. For set size 10, there was a main effect of stimulus, $F(9, 342) = 59.19$, $p < .0001$ (Huynh-Feldt $\varepsilon = .86$), no main effect of spacing, $F(1, 38) = 1.72$, $p = .19$, and a significant stimulus x spacing interaction, $F(9, 342) = 2.67$, $p = .0052$. For set size 8, there was a main effect of stimulus, $F(7, 266) = 93.34$, $p < .0001$ (Huynh-Feldt $\varepsilon = .93$), no main effect of spacing, $F(1, 38) = 1.30$, $p = .26$, and no significant stimulus x spacing interaction, $F(7, 266) = 1.44$, $p = .20$. For set size 6, there was a main effect of stimulus, $F(5, 185) = 43.16$, $p < .0001$ (Huynh-Feldt $\varepsilon = .94$), a main effect of spacing, $F(1, 37) = 7.81$, $p = .0082$, and no significant stimulus x spacing interaction, $F(5, 185) = 0.92$, p = .47. Although the effect of spacing was only significant for set size 6, we have already seen that there was a significant main effect of spacing on accuracy (see Average Accuracy).

The bottom panel in Figure 16 plots bows in *d*'. Again, separate two-way ANOVAs were run for each set size. Each of these revealed a main effect of spacing, smallest $F(8, 304) = 3.88$, $p < .0001$. There was a marginal main effect of spacing for set size 6, $F(1, 37) = 1.85$, $p = .06$. The effect was not significant for set sizes 8 and 10, larger $F(1, 38) = 1.85$, $p = .18$. There was a significant interaction between stimulus and spacing for set size 10, with smaller edge effects in the larger set size, $F(8, 304) = 3.89$, $p = 0.0002$. The interaction was not significant for set sizes 6 or 8, larger $F(6, 288) = 1.48$, $p = .18$.

Figure 17 plots the confusion matrices for each of the six cells in the design (3 set sizes x 2 spacings).

Assimilation and Contrast. Figure 18 plots E_n as a function of the S_{n-1} for different S_n. There is a tendency for the responses to small stimuli to be too large, and for the responses to large stimuli to be too small, as shown by the spacing of the lines. The positive slope of the lines indicates that the response given to S_n is biased towards S_{n-1} (i.e., assimilation). Six ANOVAs were run to confirm this description, one for each cell in the design (3 set sizes x 2 stimulus spacings). In every ANOVA, there was a significant main effect of S_n, a significant main effect of S_{n-1}, and no significant interaction.

Figure 19 plots E_n (averaged over all possible stimuli) against the lag k for different S_{n-k}. When S_{n-1} ($k = 1$) is large, a positive error is made, and when S_{n-1} is small a negative error is made (i.e., assimilation). For trials further back, the opposite pattern is seem (i.e., contrast). When $k = 2$, the error dependency is small, showing that the stimulus two trials ago is having little effect on responding. Six ANOVAs were run to confirm this description, one for each cell in the design (3 set sizes x 2 stimulus spacings). Apart from the two set size 8 conditions, there was no main effect of S_{n-k}. Apart from the two set size 6 conditions, there was no main effect of lag, k. Importantly, in every ANOVA there was a significant interaction.

Discussion

Experiment 1 has replicated the standard findings described in the Introduction, demonstrating that all of the effects can occur simultaneously. As set size increased information transmitted remained constant. This limit is below that observed for absolute identification of pitch observed by Hartman (1954) and Pollack (1952), but approximately the same as that observed by W. Siegel (1972). (Hartman used very large inter-trial intervals and Pollack played white noise between each trial.) We found a significant tendency for participants to respond with responses in the center of the scale (see also Balakrishnan, 1997). The bow in the serial position curve was shown in every condition. With larger set sizes, the bow effect is larger (replicating the findings of Alluisi & Sidorsky, 1958; Durlach & Braida, 1969; Lacouture & Marley, 1995; Pollack, 1953; and Weber et al., 1977), and doubling the spacing of the stimuli has only a slight effect on accuracy (replicating the findings of Braida & Durlach, 1972; Brown et al., 2002; Lacouture, 1997; Luce et al., 1976; and Pollack, 1952). For the larger two set sizes (8 and 10 stimuli), increasing the spacing did not improve accuracy significantly, and the actual magnitude of the improvement was slight, if any. This is consistent with Brown et al.'s (2002) findings. The bow effect was evident in every condition even in the last block of this experiment, consistent with previous findings where the bow remains even after extensive practice (Weber et al., 1977; Rouder et al., 2004).

The standard sequential effects were also evident in these data. The current stimulus was assimilated towards the preceding stimulus (replicating the findings of Garner, 1953; Holland & Lockhead, 1968; Hu, 1997; Lacouture, 1997; Lockhead, 1984; Long, 1937; Luce et al., 1982; Purks et al., 1980; Rouder et al., 2004; Staddon, et al., 1980; Stewart, 2001; and Ward & Lockhead, 1970, 1971). R_n was also contrasted away from earlier stimuli (replicating the findings of Holland & Lockhead, 1968; Lacouture, 1997; and Ward & Lockhead, 1970, 1971). Though Holland and Lockhead found the contrast effect to be biggest for S_{n-2}, these data show greatest contrast for stimuli three or four trials back. This pattern is more consistent with Lacouture's data, though perhaps slightly more extreme. Unpublished data from our laboratory suggest that the spacing between trials is likely to be an important factor in accounting for this difference.

In summary, each of the three main types of phenomena reviewed in the introduction - the limit in information transmitted, bow in the serial position curve, and sequential effects - have been demonstrated simultaneously in this experiment.

Modeling

A single fit to average data. As a proof of concept, we fitted the RJM to the average data presented above. The purpose of the modeling described below is to demonstrate that the RJM can fit the main phenomena accurately for a single set of parameter values. We took the slightly ad-hoc strategy of minimizing the SSE between the data points presented in Figures

15, 17, 18, and 19, and the RJM's fits to these data points. The choice of how the SSEs from each figure are combined is necessarily quite arbitrary as the SSEs for different figures are the SSEs of quite different things (probabilities and errors on the response scale). However, the exact weightings (1.0, 1.0, 0.1, and 0.5 for Figures 15, 17, 18, and 19 respectively) are not important as very similar best fits are obtained for different weightings.

The RJM does not predict an effect of changing the spacing of the stimuli in the set. In Experiment 1, there was a significant effect of spacing for only the smallest set size. For this reason, the RJM was fitted to data for each set size averaged across the two different stimulus spacings. We present one fit (see Table 3 for parameter values) as the dashed lines in Figures 20-24. r^2 values for response biases, confusion matrices, assimilation plots and assimilation and contrast plots were .98, .98, .89, and .94 respectively. Fits to *d*' data were poorer (r^2 = .88), and the RJM systematically under predicts the bow in the *d*' data, as described above.

Fits to individual participant's data. The purpose of Experiment 1 was to demonstrate that the main phenomena occur simultaneously. This was observed above in the data averaged across participants. Of the 119 participants, 100 showed bows in the proportion of correct responses plotted against stimulus rank (as defined by greater average accuracy on edge stimuli compared to internal stimuli) and assimilation to the previous stimulus and contrast to those further back (as define by the coefficients of the simple regression model $R_n = a_0 S_n + a_1 S_{n-1} + a_2 S_{n-2} + \ldots$) simultaneously.

To fit the RJM to the data from a single participant, we maximized the likelihood of the model generating the data produced by the participant. The best fitting RJM predicts a bow in accuracy, assimilation to the previous stimulus, and contrast to stimuli at greater lags for every participant. When the predictions for the RJM are generated for each participant, and then averaged across the participants in each condition, plots almost identical to those obtained from fitting the averaged data are obtained. The median best fitting parameter values across participants are given in Table 7, together with the upper and lower quartiles. These values are very similar to those obtained above in fitting the averaged data (and so we do not include them here).

In summary, the average data fit and the individual participant fits provide an existence proof, demonstrating that the RJM is able to provide a good account of all of the main phenomena simultaneously.

A Discrepancy: Conditional Accuracy and d'

There is a discrepancy in the literature that we have deferred discussion of until after we had presented Experiment 1. W. Siegel (1972) found that accuracy was much higher when the current stimulus was a repetition of the previous stimulus (see also Petrov & Anderson, in press; Rouder et al., 2004). In the panels of Figure 25 we plot the bow effect for three different set sizes. The effect is parameterized by the number of stimuli that intervened since the current stimulus was last repeated. When a stimulus is repeated immediately accuracy is practically perfect. With one intervening stimulus performance drops considerably and further intervening stimuli lead to a smaller further drop. This pattern can be fit by the RJM. When a stimulus is repeated $D_{n,n-1} = 0$ and, according to Equations 3 and 4, only the confusion of differences makes a small contribution to E_n. However, when $S_{n-1} <> S_n$, then $D_{n,n-1} <> 0$ and the limited decision capacity will add error to R_n. (The RJM fails to fit the difference between one and more than one trial intervening between repetitions. This is because, as a first approximation, we have assumed no memory for magnitudes other than S_{n-1}. Modifying the model to allow for some memory of S_{n-2} allows the model to predict the effect. Stewart and Brown, 2004, found this modification necessary for modeling unidimensional categorization data.)

In contrast, Purks et al. (1980), Luce et al. (1982), and Nosofsky (1983a) found that there was only a small (though significant) increase in *d'* when, in a random sequence of stimuli, the current stimulus was within one stimulus of the previous stimulus. There are obvious explanations. First, the experiments used different stimuli. W. Siegel used tones varying in frequency and Luce et al. and Purks et al. used tones varying in intensity. Second, the performance measure is different. W. Siegel reports accuracy, Purks et al., Luce et al., and Nosofsky report d'. Third, the partitioning of the data is different. W. Siegel used repetition trials (i.e., $S_n = S_{n-1}$), Purks et al. examined data for every combination of S_n and S_{n-1}, and Luce et al. and Nosofsky had stimuli differing by no more than 1 ($|S_n - S_{n-1}| \leq 1$).

We have examined our own data from the narrow and wide set size 10 conditions of Experiment 1. Consistent with W. Siegel's (1972) data, we found very high accuracy (> 98% correct for every stimulus) on trials then the stimulus was repeated from the previous trial. We also followed Luce et al.'s (1982) analysis and calculated accuracy and d' separately for trials in the random sequence that were preceded by a stimulus no more than one different from the current stimulus (near transitions) and trials that were more than one different from the preceding stimulus (far transitions). Figure 26 shows our results. Unlike Purks et al., Luce et al., and Nosofsky, we found a large advantage in accuracy and d' when the current and previous stimuli were similar. This finding enables us to rule out the accuracy vs. d' difference and the repetition vs. similar stimuli difference as explanations of the differences between the experimental results, and suggest that the difference in findings is due to the use of different stimuli (or other procedural differences). The finding of an d' advantage in the Experiment 1 data also enables us to rule out a response bias account of our accuracy data (and W. Siegel's data), whereby the advantage for repeated stimuli comes from a strong tendency to repeat the previous response. Instead, we think that the reduction in accuracy and d' found when the previous and current stimuli differ occurs because stable, long-term magnitudes to which the current stimulus can be compared are not available, and thus the current stimulus must be compared to only the previous stimulus.

In Figure 26, we have shown the accuracy and d' predictions of the RJM using the parameters from Table 3 for the Experiment 1 data. (Note that these parameters were not chosen to best fit this pattern specifically.) The RJM predicts that accuracy and d' are higher when S_n and S_{n-1} are similar because we assume that there is less noise in the mapping process in this case (ρ is small when $S_n = S_{n-1}$, see Equation 5).

At present, the RJM incorrectly predicts that a difference in accuracy and *d'* for the Luce et al. (1982) random data (as we describe above for the Experiment 1 data). The RJM could possibly be modified to correctly predict only a very small difference between accuracy and *d'* in two ways. First, if the χ parameter (which represents the magnitude below which $D^C_{n,n-1}$ is so small that the S_n is considered a repetition of S_{n-1}) were smaller, reflecting a greater uncertainty in establishing whether a stimulus was a repetition, then accuracy and *d'* would differ much less between the near and far conditions because the low noise mapping for stimulus repetitions would occur less often. Alternatively, the preliminary form of Equation 5 could be altered. Further experimental work is required to constrain these possible extensions.

One question remains. If Luce et al. (1982) did not find an increase in *d'* or accuracy when S_n is similar to S_{n-1} in their random condition, why is performance best in the small step conditions which we described earlier? Nosofsky (1983) found evidence that the advantage comes from the cumulative effect of a series of small transitions between consecutive stimuli (rather than just a single small transition between S_{n-1} and S_n). Nosofsky took the trials where S_n was no more than one stimulus different from S_{n-1} and partitioned the data further depending on whether S_{n-2} was no more than one stimulus different from S_{n-1}. Accuracy on trial *n* was higher when S_{n-2} was more similar to S_{n-1}. The RJM can account for this qualitative pattern, because of the assumption that consecutive differences are confused (see Equation 3). When S_{n-2} is more similar to S_{n-1}, then, $D^C_{n,n-1}$ varies less, and thus responding is also less variable.

Experiment 2

An important question is whether the RJM is falsifiable. We address this issue directly in Experiment 2, which pits the predictions of absolute magnitude models (i.e., the

Thurstonian models, the restricted capacity models, and the exemplar models) and the RJM against each other directly. The models make different predictions regarding the effect of misleading feedback. In the RJM, the feedback from the preceding trial is used together with a judgment of the difference between the preceding and current stimuli to produce a response. If participants are given misleading feedback on the previous trial, then, according to Equation 4, their response on the current trial should reflect this directly. In contrast, the absolute-magnitude-based models described above do not use the feedback from the previous trial in generating the response on the current trial, and so predict no effect of misleading feedback. Below, we consider how, if the misleading feedback is attended to at all, the absolute-magnitude-based models might use the feedback to adjust the mapping between the stimulus scale and the response scale. Even with this adjustment, these models still make different predictions from the RJM.

The experiment was designed such that performance on the final trial of eight critical triplets of trials (defined by S_{n-2}, S_{n-1}, R_{n-1}, F_{n-1}, and S_n) could be compared in the analysis. The triplets differed from one another in two ways. First, F_{n-1} could either be correct or misleading. Second, R_{n-1} could either be correct or incorrect. Four of the triplets are listed in Table 8. The remaining four triplets are the mirror image of the four listed in Table 8 (the values of S_{n-2}, S_{n-1}, R_{n-1}, F_{n-1}, and S_n for these mirror image triplets can be generated by subtracting the values in Table 8 from 11 and ΔE_n predictions can be generated by swapping the sign of the value in Table 8). In the remainder of this article, for simplicity of exposition, we will discuss the triplets in terms of those listed in Table 8.

In each triplet, S_{n-2} was far from S_{n-1} so that, if F_{n-1} were misleading, the deception would not be obvious. When F_n was too great by one, according to the RJM (see Equation 4), E_n should also be too great by one (see the RJM ΔE_n column of Table 8). This prediction can be tested by comparing the first two triplets in Table 8: E_n when F_{n-1} was misleading and E_n when F_{n-1} was not misleading (to provide a baseline measure of the bias due to assimilation and contrast).

However, under the alternative assumption that judgment is absolute, there is an alternative explanation of an increased error when F_{n-1} is misleading. Suppose that, upon being told that their response was incorrect, the participant adjusts their mapping of stimuli to responses. If their response was too small by one, then responses should be remapped onto stimuli so that each stimulus is now mapped onto the response that previously belonged to the next highest stimulus. For example, in a Thurstonian model (e.g., Durlach & Braida, 1969; Luce et al., 1976; Treisman, 1985), all of the criteria should be shifted down the perceptual scale by one stimulus spacing. In an exemplar model (e.g., Brown et al., 2002; Kent & Lamberts, in press; Nosofsky, 1997; Petrov & Anderson, in press), each exemplar should be remapped so that it is associated with the label that was originally associated with the immediately higher exemplar. Thus, after the misleading feedback that is too large by one, each stimulus will now be associated with a response that is also one too large (see the Mapping E_n column of Table 8).

Though the RJM and the mapping alternative make the same predictions when R_{n-1} is correct, they make different predictions when R_{n-1} is incorrect (see the last two triplets listed in Table 8). Take, for example, the case illustrated in the third row of Table 8. Within a Thurstonian framework, if the perception of $S_{n-1} = 4$ is noisy so that the percept falls below the criteria between response categories 3 and 4, then R_{n-1} will be incorrectly underestimated as 3. Truthful feedback (i.e., $F_{n-1} = 4$) will indicate that an error has been made, and criteria might be adjusted in response by shifting them one unit down the response scale. Now, when $S_n = 6$ is presented, R_n will be an overestimate. The same argument can be made for an exemplar framework. If perception of $S_{n-1} = 4$ is noisy so that the percept is more similar to the exemplar for category 3 than category 4, then R_{n-1} will be incorrectly underestimated as 3. Truthful feedback (i.e., $F_{n-1} = 4$) will indicate that an error has been made, and the mapping between exemplars and category labels should be adjusted so that each exemplar is now mapped to the label previously belonging to the next highest exemplar. Now, when $S_n = 6$ is presented, R_n will be an overestimate. When F_{n-1} is misleading and confirms a mistaken R_{n-1} (see the fourth row of Table 8), the match between response and feedback should mean that the participant thinks no error has been made, and does not adjust their mapping. Thus, no error is predicted in identifying S_n.

In summary, the RJM predicts an effect of misleading feedback. Absolute magnitude models predict the same effect if one augments them with the ability to adjust the mapping between stimuli and responses in response to an error. However, the RJM and these augmented mapping models make different predictions in the case when the previous response is wrong but misleading feedback suggests that it was correct.

Method

Participants. Twelve female and seven male students from the University of Warwick aged between 19 and 32 years participated for payment of £6 (approximately U.S. $10).

Stimuli. Ten stimuli were generated. The first was 200 Hz, with a between-stimulus spacing of 25%, giving a last stimulus of 1490 Hz. This spacing is just over twice the spacing of the stimuli used in the wide condition of Experiment 1. The amplitude envelope applied to the stimuli was that used in Experiment 1.

Design and Procedure. The procedure closely follows that of Experiment 1. There were 20 blocks of 40 stimuli. Two of the eight critical triplets differ from another two only in R_{n-1}. Thus, there were only six unique critical triplets (ignoring R_{n-1}, which, obviously, was under the participants' control). In each block, these were randomly assigned to begin on trials 3, 10, 17, 24, 31, and 38. On the remaining trials, the stimulus was selected at random with the constraint that all stimuli appeared equally often in each block. Feedback was always correct, apart from on the middle trial of two of the six triplets, and thus feedback was only misleading on 5% of trials.

Results

Figure 27 shows E_n as a function of whether F_{n-1} was correct or misleading, parameterized by the accuracy of R_{n-1} (see Table 8 for a full description of the conditions). E_n with F_{n-1} and R_{n-1} correct (as in a standard absolute identification experiment) is negative, showing that R_n is assimilated towards S_{n-1} and contrasted away from S_{n-2}. When F_{n-1} is misleading ($F_{n-1} = S_{n-1} + 1$) then E_n is increased, as predicted by the RJM (and the mapping hypothesis). The same pattern is seen when R_{n-1} is incorrect, as predicted by the RJM (the opposite of the prediction of the mapping model). A two-way ANOVA (F_{n-1} correct or misleading x R_{n-1} correct or incorrect) revealed a significant main effect of the accuracy of F_{n-1} [$F(1, 18) = 20.84$, $p = .0002$], no significant main effect of the accuracy of R_{n-1} [$F(1, 18) = 0.31$, $p = .59$], and no significant interaction [$F(1, 18) = 2.95$, $p = .10$]. Though the interaction is approaching significance, it is in the opposite direction predicted by the mapping hypothesis.

The increase in E_n when F_{n-1} is too large by 1 was only .57 (averaged across the accuracy of R_n). The RJM predicts that all of the increase in F_{n-1} should carry over to R_n. However, we have already seen evidence that there may still be some residual memory for the absolute magnitude of not just the preceding stimulus but also the stimulus before that (e.g., Massaro, 1970; W. Siegel, 1972; Stewart & Brown, 2004; Wickelgren, 1966). If S_{n-2} were also used as an anchor against which to judge S_n then one would expect a smaller effect of F_{n-1} on E_n.

Information transmitted. The information transmitted in Experiment 1 was lower than the estimates obtained by Pollack (1952) and Hartman (1954) for absolute identification of pure tones varying in frequency. In Experiment 2, the spacing of the stimuli was greatly increased compared to Experiment 1, but average information transmitted was 1.41 bits - approximately the same as in Experiment 1 and W. Siegel (1972).

Discussion

According to the RJM, the difference between the current stimulus and the previous stimulus is added to the feedback from the previous trial to generate the current response (see

Equation 4). Thus, the RJM makes the strong prediction that, if the previous feedback is inaccurate, this error will be transmitted to the current response. In Experiment 2, we found this to be the case. When participants were misled, and the previous feedback was one too large, then the current response was significantly increased compared to when the previous feedback was accurate.

Of all of the existing models of absolute identification, only Holland and Lockhead's (1968) model, which makes the same assumption about the use of the previous feedback as the RJM, can predict this effect. All of the other models fail to predict the effect in their current forms, because they do not use the previous feedback in generating the current response. Reasonable assumptions can be made to allow the models to adapt the mapping between stimulus magnitudes and response categories after feedback indicates an error has been made (e.g., shifting criteria in a Thurstonian model or relabeling exemplars in an exemplar model). In this way, the models can predict that there will be an error on the current trial when feedback on the previous trial is erroneous, because it will appear that an error has been made on the previous trial and the model can alter the mapping between stimuli and responses to compensate. However, the models then incorrectly predict that there will be no effect of misleading feedback on the previous trial when the previous response was incorrect but the (misleading) feedback indicates that it was correct. In this case, it will appear as if no error has been made, and so no remapping should take place.

There is an alternative way that the absolute-magnitude-based models might be able to account for the Experiment 2 data. Instead of perceptual noise being the cause of an incorrect R_{n-1}, R_{n-1} might be incorrect because the mapping between stimuli and responses is, for some reason, already incorrect. For example, in the case illustrated in the last two rows of Table 8, R_{n-1} might be underestimated because either Thurstonian criteria may be too far up a sensory scale or because each exemplar might be mapped onto a label that is too low. Now, if F_{n-1} is accurate and indicates an error, the adjustment of the mapping would correct the initial mismapping. If F_{n-1} is misleading, and indicates no error when one has been made, then no adjustment will be made. These predictions are now the same as those of the RJM. However, in adopting this explanation though, the idea of relative judgment is incorportated. In assuming that the mapping between stimuli and responses is adjusted after each piece of feedback and, thus, varies from trial to trial, one is abandoning a stable long-term association between particular magnitudes and response categories. There seems to be little difference between, on the one hand, hearing a stimulus two higher than the previous stimulus and so responding with a response two higher than the previous feedback and, on the other, hearing a stimulus and aligning a response scale with it on the basis of the feedback and then hearing another stimulus two higher and thus responding with a response two units up the response scale. In allowing the continual adjustment of the mapping between the stimulus and the response scales, the stable long-term mapping between absolute stimulus magnitudes and response categories is abandoned and these models become models of relative judgment that are really rather similar to the RJM proposed here.

Extending Relative Judgment Models

In this article, our core claim is that a model of relative judgment can provide an account of many phenomena in the absolute identification literature. Our model differs from existing models in assuming that long-term representations of absolute magnitudes are either unavailable or, for some reason, are unused in absolute identification. Here, we outline three ways in which this relative judgment idea might be extended.

The first way would be to test more directly the longevity of absolute magnitude representations. Above, we cited ten reports of same-different judgment tasks in which the memory for a standard decays rapidly as the interval until a comparison item is increased or filled. However, in these experiments, the stimulus chosen as the standard varied from trial to trial. With only one standard, there is little forgetting across intervening tones (D. A. Anderson, 1914, as cited from Massaro, 1970, and Wickelgren, 1966; Irwin, 1937;

Magnussen, Greenlee, Aslaken, & Kildebo, 2003). We think an important step would be to identify the conditions under which long-term representations of absolute magnitudes can and cannot be maintained. The misleading-feedback methodology from Experiment 2 could also be adapted to measure the longevity of absolute magnitude representations by remapping stimuli and responses part way through an absolute identification experiment (e.g., by increasing the feedback by one for the rest of the experiment) and measuring for how long the initial stimulus response mapping persists. The effect of shifting the entire stimulus set between experimental sessions (e.g., Ward & Lockhead, 1970; Ward, 1987) should also be investigated for other stimulus continuums.

A second way in which the idea of relative judgment could be developed would be to extend relative judgment approaches to other psychophysical tasks. We have already had some success in modeling empirical results in unidimensional binary categorization (Stewart & Brown, 2004; Stewart et al., 2002) that cannot be fit by existing absolute-magnitude-based categorization models (e.g., the GCM, Nosofsky, 1986). Laming (1984, 1997) has been able to account for some key results in magnitude estimation and cross-modality matching using his relative judgment model.

Our final suggestion is that absolute and relative judgment models might be integrated into a single theoretical framework. Obviously, at some level, in absolute-magnitude-based models judgment is relative, because the information from previous stimulus-response pairings provides the basis for generating each response. Also, at some level, relative judgment models do assume that a (perhaps peripheral) representation of a stimulus's absolute magnitude can be maintained in the very short term because, without such a representation over the inter-stimulus interval, the following stimulus could not be compared to the previous stimulus. We have already shown that our relative judgment model of unidimensional binary categorization and an absolute-magnitude-based exemplar model (Nosofsky, 1986) are special cases of a more general model (Stewart & Brown, 2005). That is, the relative judgment model can be thought of as occupying one end of a continuum where absolute magnitude representations are very short-lived, with the exemplar model occupying the other end, where absolute magnitude representations are long-lived.

Conclusion

We have presented the RJM, in which, in the assumed absence of stable, long-term absolute magnitudes, the representation of the difference between the stimulus on the current trial and the stimulus on the preceding trial is used in conjunction with the feedback from the previous trial to produce a response. We have demonstrated that a broad class of absolute identification data can be fit by this model. Assuming relative judgment allows an account of the ubiquitous sequential effects observed in absolute identification. By assuming only that the representation of the difference between the current stimulus and the previous stimulus is confused with the representations of earlier differences, the RJM predicts assimilation to the previous stimulus and contrast to those stimuli further back. These effects have been problematic for those existing models assuming that absolute identification is achieved using long-term representations of absolute magnitudes of stimulus values (either as exemplars, anchors, or criteria). Using difference information optimally within a limited capacity provides an account of the bow effect and of the limit in information transmitted. We conclude, therefore, that absolute identification may in fact be achieved by relative judgment.

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Appendix

$d'_{i,\,i+1}$ is calculated as follows. Conditional stimulus-response matrices are drawn up for each possible S_{n-1}. Responding i + 1 or greater to Stimulus i + 1 is considered a hit. Making any of the same responses to Stimulus i (i.e., one stimulus smaller) is considered a false alarm. A value of $d'_{i,\,i+1}$ is then calculated in the normal way (see Green & Swets, 1966) for each conditional matrix. An overall average $d'_{i,\,i+1}$ is formed by averaging a subset of the $d'_{i,\,i+1}$ from each conditional matrix. The subset is selected by choosing only those matrices where S_{n-1} was such that both response i and response i + 1 were available on trial n. In this way, the resulting $d'_{i,\,i+1}$ is controlled so that it is not artificially raised by the restricted opportunity to make responses in non-random sequences.

Figure A1 illustrates the calculation for hypothetical data from an absolute identification of five stimuli with a small step (3) sequence from Luce et al. (1982). Recall that in a small step (3) sequence S_n is constrained to be $S_{n-1} - 1$, S_{n-1}, or $S_{n-1} + 1$. The first column shows five conditional stimulus response confusion matrices, with the top matrix representing the confusions when the $S_{n-1} = 1$, the next matrix down when $S_{n-1} = 2$, and so on. Closed circles represent responses considered hits, and closed squares represent false alarms, for the calculation of $d'_{1,\,2}$. Only the conditional confusion matrices when S_{n-1} = 1 or 2 are used to calculate $d'_{1,\,2}$, as it is only when S_{n-1} = 1 or 2 that the Responses 1 or 2 are available on the trial n. As $d'_{i,\,i+1}$ is a function of the difference between the proportion of hits and the proportion of false alarms after each has been transformed by the cumulative normal distribution function, then $d'_{i,\,i+1}$ will be larger to the degree that the heights of the circles are above the heights of the squares in Figure A1 in each matrix. Compare column A with column B where hits and false alarms are illustrated for $d'_{2,\,3}$. As the curves in Figure A1 are steeper at the edges of the range the differences are larger, and therefore $d'_{i,\,i+1}$ will be larger for extreme stimuli.

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Footnotes

[1] Information transmitted is a measure of the amount of association between the input (stimulus) and output (response) of a channel (the participant). The amount of information transmitted does not describe the nature of the association and the analysis makes no assumptions about the form of the association. Miller (1956) begins with a motivation for using this measure.

[2] It is not clear whether the responses offered to participants were constrained for Luce et al.'s (1982) Experiment 1. This was the case for their Experiment 2.

[3] Gravetter and Lockhead (1973) propose a similar model, except that in their model noise is assumed to be proportional to the criterial range (i.e., the distance between the two most extreme category boundaries), rather than the distance between the two most extreme stimuli. Though these two distances will be highly correlated in most situations, if the lowest and highest stimuli are placed more extremely, criterial range is found to be more appropriate.

[4] The James-Stein estimator was not proved to be the best estimator, just a better estimator than the single observation. In the absence of a best estimator, the James-Stein estimator is used as that which best approximates optimality.

[5] This previous experience is unlikely to matter, as there are only very small practice effects in absolute identification (Alluisi & Sidorsky, 1958; Hartman, 1954; Weber et al., 1977, but see Rouder et al., 2004).

Table 1

The Limit in Information Transmitted for a Variety of Stimulus Attributes

Attribute	Source	Limit/bits
Frequency of a tone	Hartman (1954)	2.3
	Pollack (1952)	2.3
	W. Siegel (1972)	1.6
Intensity of a tone	Garner (1953)	2.2
	Norwich, Wong, and Sagi, (1998)	2.2
	Braida and Durlach (1972, from calculations by Marley & Cook, 1984)	1.9
Saltiness of a solution	Beebe-Center, Rogers, and O'Connell (1955)	1.7
Sweetness of a solution	Beebe-Center, Rogers, and O'Connell (1955)	1.7
Intensity of odor	Engen and Pfaffmann (1959)	1.5
Bisection of a scale	Hake and Garner (1951)	3.2
	Coonan and Klemmer (as reported in Miller, 1956)	3.2 / 3.9
Line length	Baird, Romer, and Stein (1970)	2.4
	Pollack (as cited in Miller, 1956)	2.6 / 3.0
Angle of inclination	Muller, Sidorsky, Slivinske, Alluisi, and Fitts (1955, as cited in Garner, 1962 , and Laming, 1984)	4.5
	Pollack (as cited in Miller, 1956)	2.8 / 3.3
Area	Pollack (as cited in Miller, 1956)	2.6 / 2.7
Area of a circle	Alluisi and Sidorsky (1958)	2.7
Area of a square	Eriksen and Hake (1955a)	2.0
	Eriksen and Hake (1955b)	2.8
Area of complex figure	Baird, Romer, and Stein (1970)	2.1
Hue	Chapanis and Halsey (1956)	3.1
	Eriksen and Hake (1955b)	3.3
	Conover (1959, as cited in Garner, 1962)	3.5
Brightness	Eriksen and Hake (1955b)	2.3
Cutaneous electrical intensity	Hawkes and Warm (1960)	1.7

Note. Limits separated by '/' denote limits for short and long duration stimulus exposure.

Table 2

A Comparison of Absolute Identification Models

Model	Type	Information transmission limit		Bow				Sequence effects		
		Range constant	N constant	Basic effect	Range constant	N constant	RT	Assimilation	Contrast	Manipulation
Holland and Lockhead (1968)	Regression	No	No	No	No	No	No	Partly[1]	Partly[1]	No
Lockhead and King (1983)	Regression	No	No	No	No	No	No	Yes	Yes	No
Durlach and Braida (1969)	Modified Thurstonian	Yes	Yes	Yes[2]	Yes	Yes	No	No	No	No
Treisman (1985)	Modified Thurstonian	Yes	Yes	Yes	Yes	Yes	No	Partly[3]	Partly[3]	No
Luce, Green, and Weber (1976)	Modified Thurstonian	Yes	Yes	Yes[4]	Yes	Yes	No	No	No	Yes
Marley and Cook (1984, 1986)	Restricted capacity	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Lacouture and Marley (1991)	Restricted capacity	Yes	Yes	No	No	No	No	No	No	No
Lacouture and Marley (1995, 2004)	Restricted capacity	Yes	Yes	Yes	Yes	Yes	Yes	No	No	Yes
Laming (1984, 1997)	Relative judgment	Yes	Yes	No	No	No	No	No	No	Yes
Brown, Neath, and Chater (2002)	Exemplar	No	No	Yes	Yes	Yes[5]	No	No	No	No
Nosofsky (1997)	Exemplar	No	No	Yes[6]	Yes	No	Yes	No	No	No
Petrov and Anderson (in press)	Exemplar	Possibly	Possibly	Yes	Possibly	Possibly	No	Partly[7]	No	Possibly
Kent and Lamberts (in press)	Exemplar	Possibly	No	Yes	Yes[8]	No	Yes	No	No	No
Relative Judgment Model	Relative judgment	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes

Notes. 'Yes' indicates the effect is captured. 'No' indicates the effect cannot be captured. 'Partly' indicates that some aspect of the effect is captured, but that there is a significant shortcoming. [1]Captures the effect only on average. [2]With the additional assumption of an anchor at each end of the range. [3]Incorrectly predicts that assimilation will decrease as $S_n - S_{n-1}$ increases. [4]With the additional assumption that the attention band dwells at the edges of the range. [5]With the additional assumption that discriminability is reduced as stimulus range increases. [6]An account of the bow effect is built into the model's response bias parameters. [7]Predicts response repetition but not increasing assimilation as S_n - S_{n-1} increases. [8]Kent and Lamberts held stimulus spacing rather than stimulus range constant.

Table 3

Best-Fitting Parameter Values of the RJM

Data	Figure	α_1	α_2	α_3	α_4	α_5	c	σ	λ	r^2
Garner (1953)	1	0.161	0.017				0.205	0.078	0.800	.20[1]
Pollack (1952)	1	0.161	0.034				0.162	0.064	0.833	1.00
Brown, Neath, and Chater (2002)	2	0.308	0.187				0.235	0.297	0.816	.98
Lacouture and Marley (1995)	3	0.171	0.138				0.155	0.133	0.808	.95[2]
Ward and Lockhead (1970)[3]	4	0.187	0.152	0.104	0.049	0.000	0.188	0.069	0.962	.89
Holland and Lockhead (1970)	5	0.223	0.174	0.125	0.088	0.058	0.083	0.113	0.885	.98
Lacouture (1997)	5	0.125	0.111	0.079	0.054	0.033	0.159	0.104	0.930	.92
Ward and Lockhead (1970)[3]	5	0.187	0.152	0.104	0.049	0.000	0.188	0.069	0.962	.93
Luce, Nosofsky, Green, and Smith (1982)	6	0.069	0.050				0.103	0.211	0.860	.92[4]
Experiment 1[5]	20-24	0.112	0.101	0.076	0.054	0.035	0.111	0.216	0.961	[6]

W. Siegel (1972)	25	0.160	0.200				0.141	0.144	0.883	.93

Note. [1]This r^2 is low because there is almost no variation in the data to be explained. [2]Fit to accuracy data only; $r^2 = .60$ for d' data. [3]These data were fitted simultaneously. [4]Fit to accuracy data only; $r^2 = .63$ for d' data. [5]Best fitting parameters to data averaged across participants. [6]See text.

Table 4

The Range of Possible Responses Available in an Absolute Identification of 10 Stimuli

	$S_n = 1$		$S_n = 5$	
S_{n-1}	Set of possible responses	Range, ρ	Set of possible responses	Range, ρ
1		1	{2, 3, 4, 5, 6, 7, 8, 9, 10}	9
2	{1}	1	{3, 4, 5, 6, 7, 8, 9, 10}	8
3	{1, 2}	2	{4, 5, 6, 7, 8, 9, 10}	7
4	{1, 2, 3}	3	{5, 6, 7, 8, 9, 10}	6
5	{1, 2, 3, 4}	4		1
6	{1, 2, 3, 4, 5}	5	{1, 2, 3, 4, 5}	5
7	{1, 2, 3, 4, 5, 6}	6	{1, 2, 3, 4, 5, 6}	6
8	{1, 2, 3, 4, 5, 6, 7}	7	{1, 2, 3, 4, 5, 6, 7}	7
9	{1, 2, 3, 4, 5, 6, 7, 8}	8	{1, 2, 3, 4, 5, 6, 7, 8}	8
10	{1, 2, 3, 4, 5, 6, 7, 8, 9}	9	{1, 2, 3, 4, 5, 6, 7, 8, 9}	9

Note. For simplicity in calculating ρ we assume $D^{C}_{n,n-1} = D_{n,n-1}$ in this table.

Table 5

Information Transmitted Predictions from the RJM for Absolute Identification With and Without Feedback

Feedback	$\mathrm{U}(R_n)$	$\mathrm{U}(R_n : S_n)$	$\mathrm{U}(R_n : S_{n-1} \mid S_n)$	$\mathrm{U}(R_n : \mathrm{R}_{n-1} \mid S_n, S_{n-1})$
Yes	3.321	1.229	0.158	0.054
No	3.310	0.729	0.025	0.821

Note. $\mathrm{U}(X : Y \mid Z)$ is information transmitted from Y to X after the effect of Z is removed. See Mori and Ward's (1995) appendix for details of the calculation of these terms.

Table 6

Average Information Transmitted (in Bits) for Each Condition of the Absolute Identification Experiment

	Set Size		
Spacing	6	8	10
Narrow	1.26	1.26	1.26
Wide	1.52	1.39	1.41

Table 7

Summary Statistics for the Best Fitting Parameter Values for Individual Participant Data From Experiment 1

	α_1	α_2	α_3	α_4	c	σ	λ
Median	0.107	0.080	0.051	0.026	0.109	0.227	0.959
LQ	0.088	0.062	0.036	0.011	0.077	0.192	0.920
UQ	0.124	0.100	0.066	0.037	0.130	0.280	1.003

Table 8

Critical Triplets of Trials Used in Experiment 2

R_{n-1} Accuracy	F_{n-1} Accuracy	S_{n-2}	S_{n-1}	R_{n-1}	F_{n-1}	S_n	RJM ΔE_n	Mapping ΔE_n
Correct	Correct	9	3	3	3	5	0	0
Correct	Misleading	9	3	3	4	5	+1	+1
Incorrect	Correct	9	4	3	4	6	0	+1
Incorrect	Misleading	9	3	4	4	5	+1	0

Note. Four additional critical triplets were included, which can be generated by subtracting values of S_{n-2}, S_{n-1}, R_{n-1}, F_{n-1}, and S_n from 11, and reversing the sign of ΔE_n.

Figure Captions

Figure 1. Information transmitted from stimulus to response as a function of stimulus set size. Data are taken from Garner (1953) and Pollack (1952).

Figure 2. Confusion matrices for three different stimulus spacings (ratios 1.037, 1.050, and 1.076) obtained by Brown, Neath, and Chater (2002). Each curve represents the proportion of responses in each response category for a given stimulus: Together the curves show the stimulus-response confusion matrix. The accuracy against stimulus magnitude serial position curve is obtained by joining the apexes of each curve.

Figure 3. Accuracy (top) and d' (bottom) against stimulus rank for five different set sizes (spacing between adjacent stimuli held constant) from Lacouture and Marley (1995).

Figure 4. Average E_n for each S_n as a function of S_{n-1}. Data have been collapsed across pairs of stimuli. Data are from Ward and Lockhead (1970).

Figure 5. Average E_n (across all S_n) as a function of the lag, k, for each possible S_{n-k}. Data have been collapsed across pairs of stimuli. The three data sets are from Holland and Lockhead (1968), Lacouture (1997), and Ward and Lockhead (1970).

Figure 6. Data showing the bow in proportion correct (top) and $d'_{i,i+1}$ (bottom) serial position curves for the four conditions used by Luce, Nosofsky, Green, and Smith (1982).

Figure 7. Sequential effects predicted by the ANCHOR model (Petrov & Anderson, in press). $k_p = 0.04$, $\alpha = 0.3$, $k_m = 0.058$, $T = 0.050$, $H = 0.071$, and $c = 0.036$.

Figure 8. An illustration of how the limited capacity is used. A: $S_{n-1} = 4$, $S_n = 8$. B: $S_{n-1} = 6$, $S_n = 8$. In each case, the same limited capacity is used to represent the range of possible responses, given the sign of $D^C_{n,n-1}$. Noise of the same variance is present in each case, though because responses are more compressed in Case A compared to Case B the variability in responding will be greater in Case B.

Figure 9. The optimal location for response scale criteria for RJM parameters that best fit data from Experiment 1. Dashed lines represent criteria half way between integer values on the

response scale. Solid lines are the optimal criteria.

Figure 10. *d*' against stimulus rank for the three stimulus spacings from Brown et al. (2002). The RJM fit is to data averaged across the three spacings.

Figure 11. Information transmitted in Braida and Durlach's (1972) absolute identification of 10 stimuli as a function of stimulus range.

Figure 12. Confusion matrices for the three different stimulus spacings from Lockhead and Hinson (1986) Experiment 2.

Figure 13. Sequential effects predicted by the RJM for absolute identification of 10 stimuli with and without feedback. For clarity, lines for Stimuli 2 - 9 have been omitted.

Figure 14. Mean proportion of correct responses by block for each condition in Experiment 1.

Figure 15. The response biases for each condition in Experiment 1.

Figure 16. The proportion of correct responses (top) and *d*' (bottom) against stimulus for each condition in Experiment 1.

Figure 17. The confusion matrices for the conditions in Experiment 1.

Figure 18. Average E_n for each S_n as a function of S_{n-1} for Experiment 1. Data have been collapsed across pairs of stimuli.

Figure 19. Average E_n (across all S_n) as a function of the lag k for each possible S_{n-k} for Experiment 1. Data have been collapsed across pairs of stimuli.

Figure 20. The RJM's fits to the response bias for each set size (collapsed across spacing) in Experiment 1.

Figure 21. The RJM's fits to the confusion matrices for each set size (collapsed across spacing) in Experiment 1.

Figure 22. The RJM's fits to *d*' data from Experiment 1 (collapsed across stimulus spacing).

Figure 23. The RJM's fits to the effects of S_{n-1} and S_n on E_n for each set size (collapsed across spacing) in Experiment 1.

Figure 24. The RJM's fits to the effects of S_{n-k} ($k = 1$ to 6) on E_n for each set size (collapsed

across spacing) in Experiment 1.

Figure 25. Accuracy against stimulus rank for three different set sizes (spacing between adjacent stimuli held constant) from W. Siegel (1972). Lines are for different numbers of intervening stimuli between S_n and the last time that stimulus occurred.

Figure 26. Accuracy (top) and d' (bottom) against S_n conditional upon S_{n-1} being either near to S_n (i.e., $|S_n - S_{n-1}| \leq 1$) or far from S_n (i.e., $|S_n - S_{n-1}| > 1$) for data from the set size 10 condition of Experiment 1 (collapsed across spacing). The $d'_{9,10}$ point for the near data is not plotted because discrimination was perfect (i.e., $d'_{9,10} = \infty$).

Figure 27. E_{n+1} as a function of the accuracy of F_n for correct and incorrect R_n for Experiment 2.

Figure A1. The proportion of responses defined as hits (circles) and false alarms (squares) used in calculating $d'_{i,i+1}$, for hypothetical data from a small step (3) absolute identification task with 5 stimuli. The pair of conditional confusion matrices on each row are identical. The matrices are conditional on S_{n-1}, and there is a row for each S_{n-1}. The left column shows the hits and false alarms for $d'_{1,2}$ and the right column shows them for $d'_{2,3}$.

Figure 1

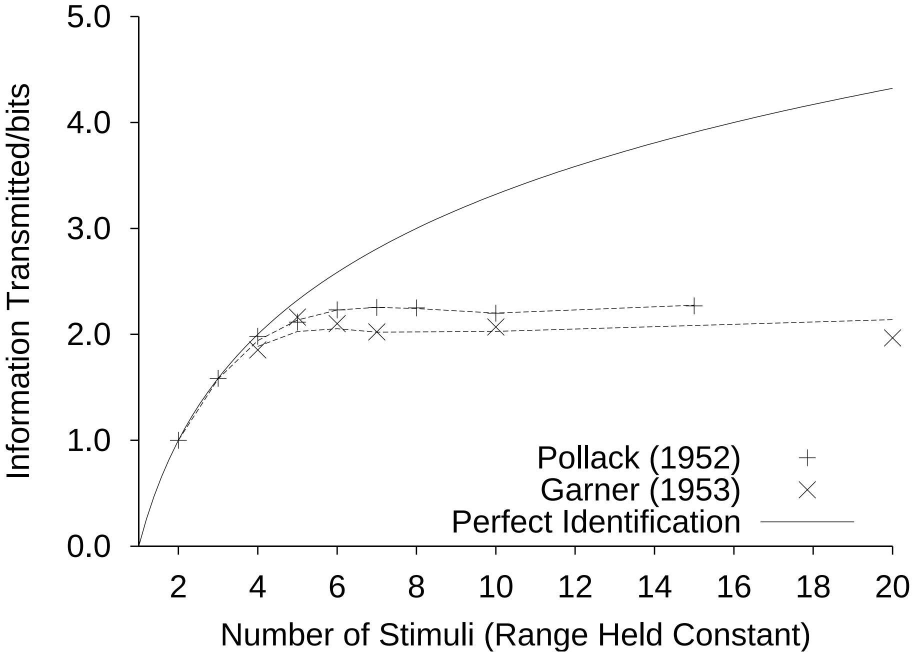
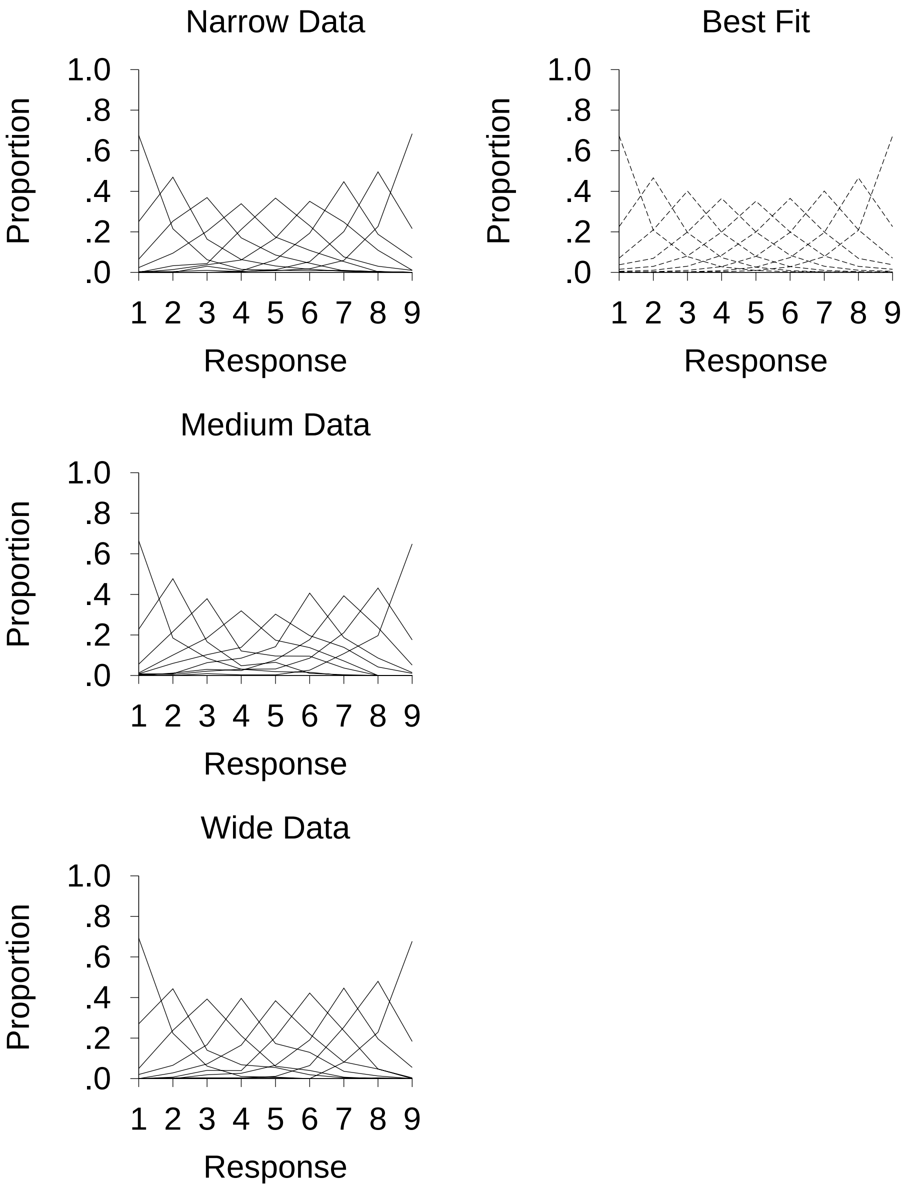

Information Transmitted/bits
5.0
4.0
3.0
2.0
1.0
0.0
2
4
6
8
10
12
14
16
18
20
Number of Stimuli (Range Held Constant)
Pollack (1952) +
Garner (1953) ×
Perfect Identification

Figure 2



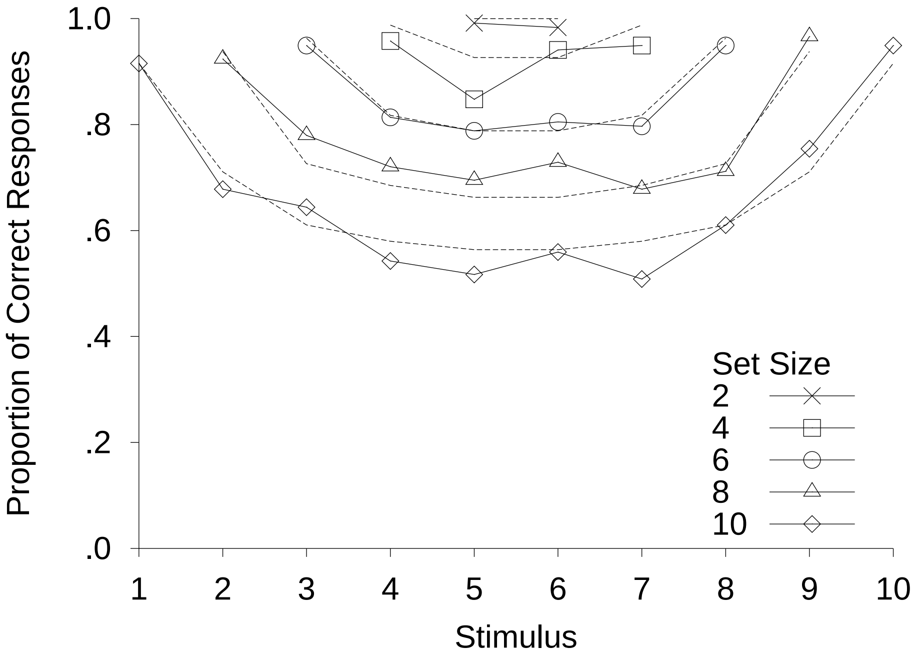
Narrow Data

Best Fit

Medium Data

Wide Data

Figure 3


1.0
.8
.6
.4
.2
.0
Proportion of Correct Responses
1
2
3
4
5
6
7
8
9
10
Stimulus
Set Size
2
4
6
8
10

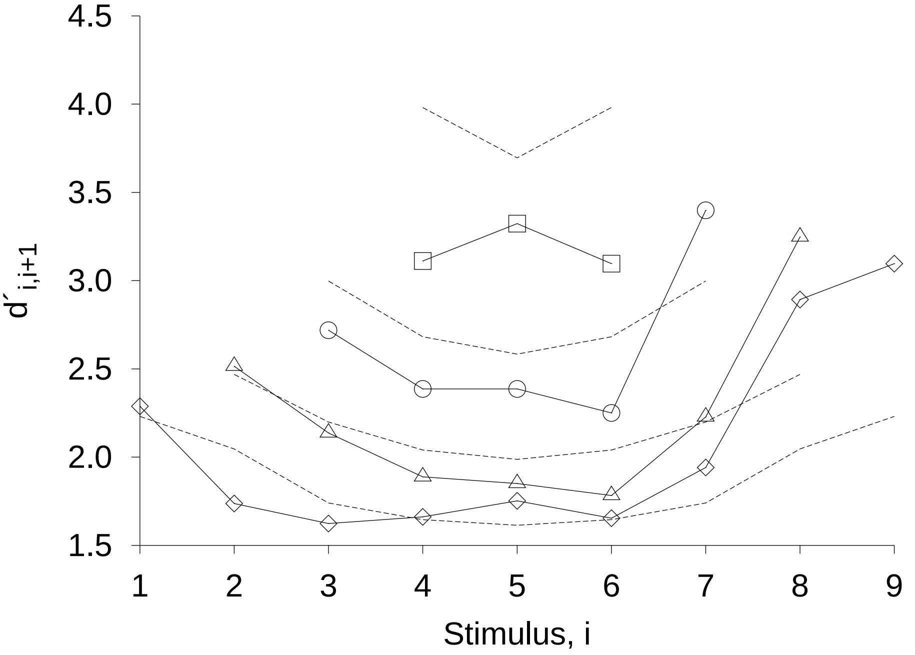

4.5
4.0
3.5
3.0
2.5
2.0
1.5
d′ i,i+1
1
2
3
4
5
6
7
8
9
Stimulus, i

Figure 4

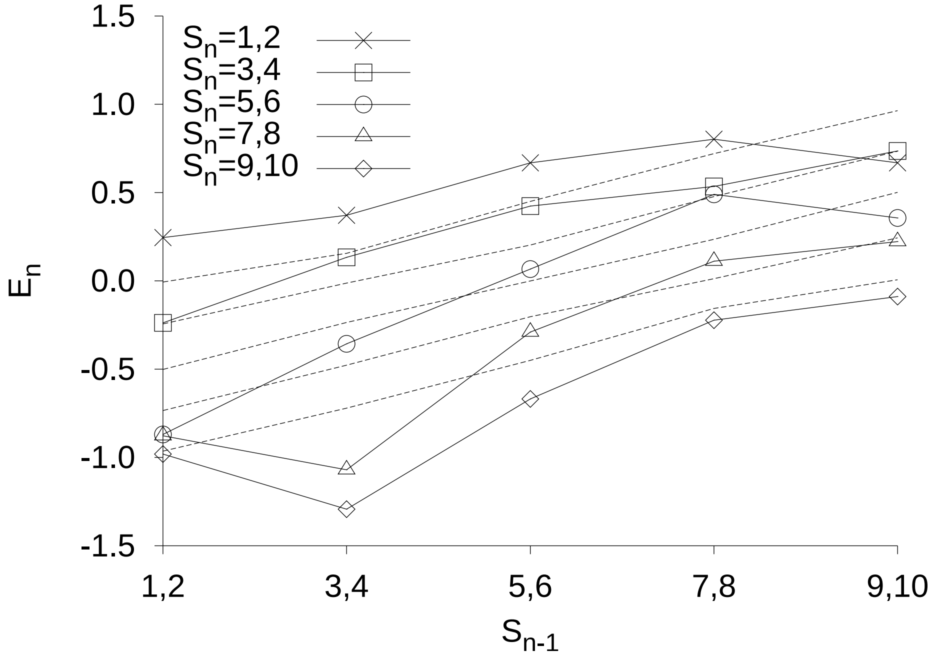

1.5
1.0
0.5
0.0
-0.5
-1.0
-1.5
E_n
1,2
3,4
5,6
7,8
9,10
S_{n-1}
S_n=1,2
S_n=3,4
S_n=5,6
S_n=7,8
S_n=9,10

Figure 5

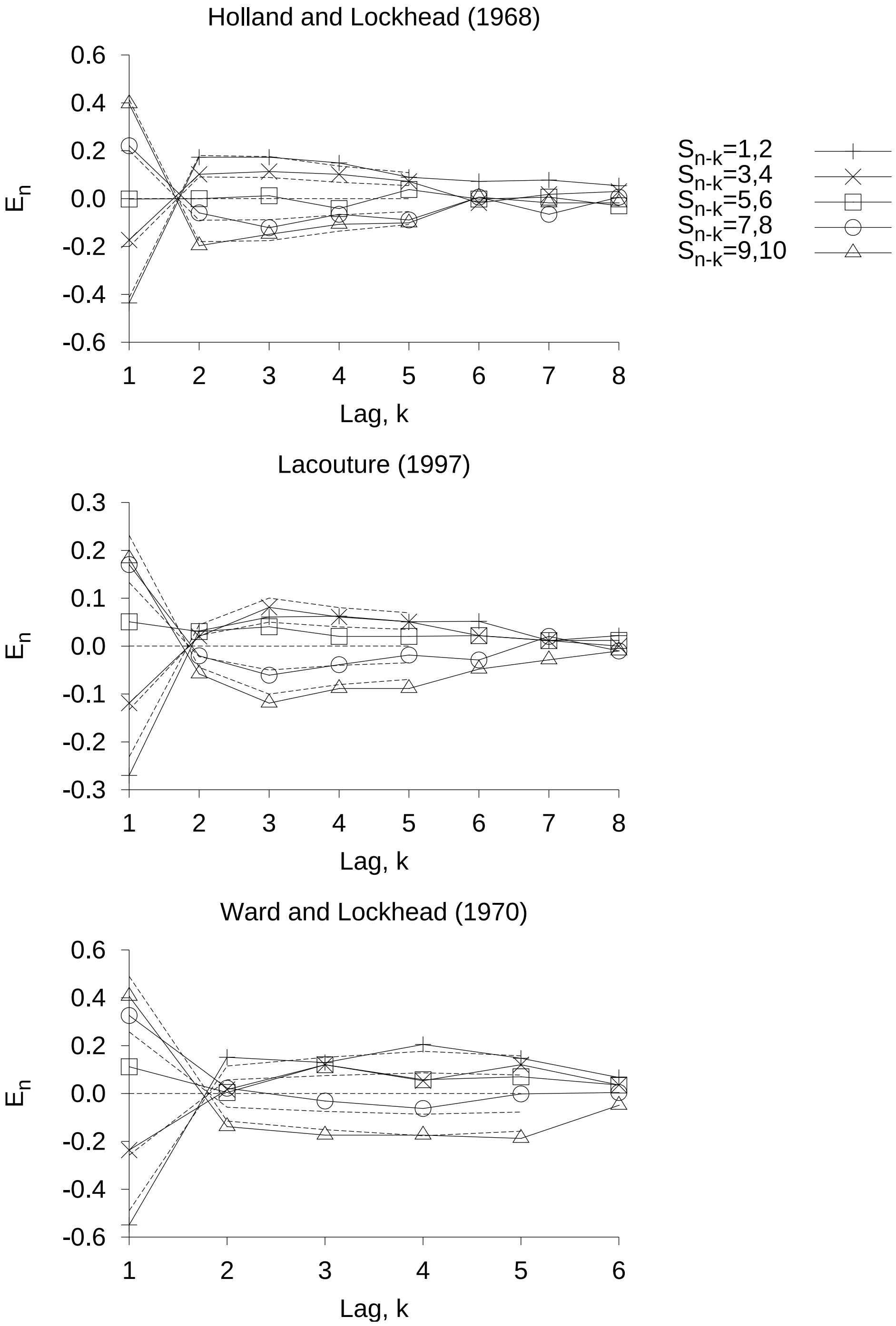

Holland and Lockhead (1968)
E_n
0.6
0.4
0.2
0.0
-0.2
-0.4
-0.6
1
2
3
4
5
6
7
8
Lag, k
S_{n-k}=1,2
S_{n-k}=3,4
S_{n-k}=5,6
S_{n-k}=7,8
S_{n-k}=9,10
Lacouture (1997)
E_n
0.3
0.2
0.1
0.0
-0.1
-0.2
-0.3
1
2
3
4
5
6
7
8
Lag, k
Ward and Lockhead (1970)
E_n
0.6
0.4
0.2
0.0
-0.2
-0.4
-0.6
1
2
3
4
5
6
Lag, k

Figure 6

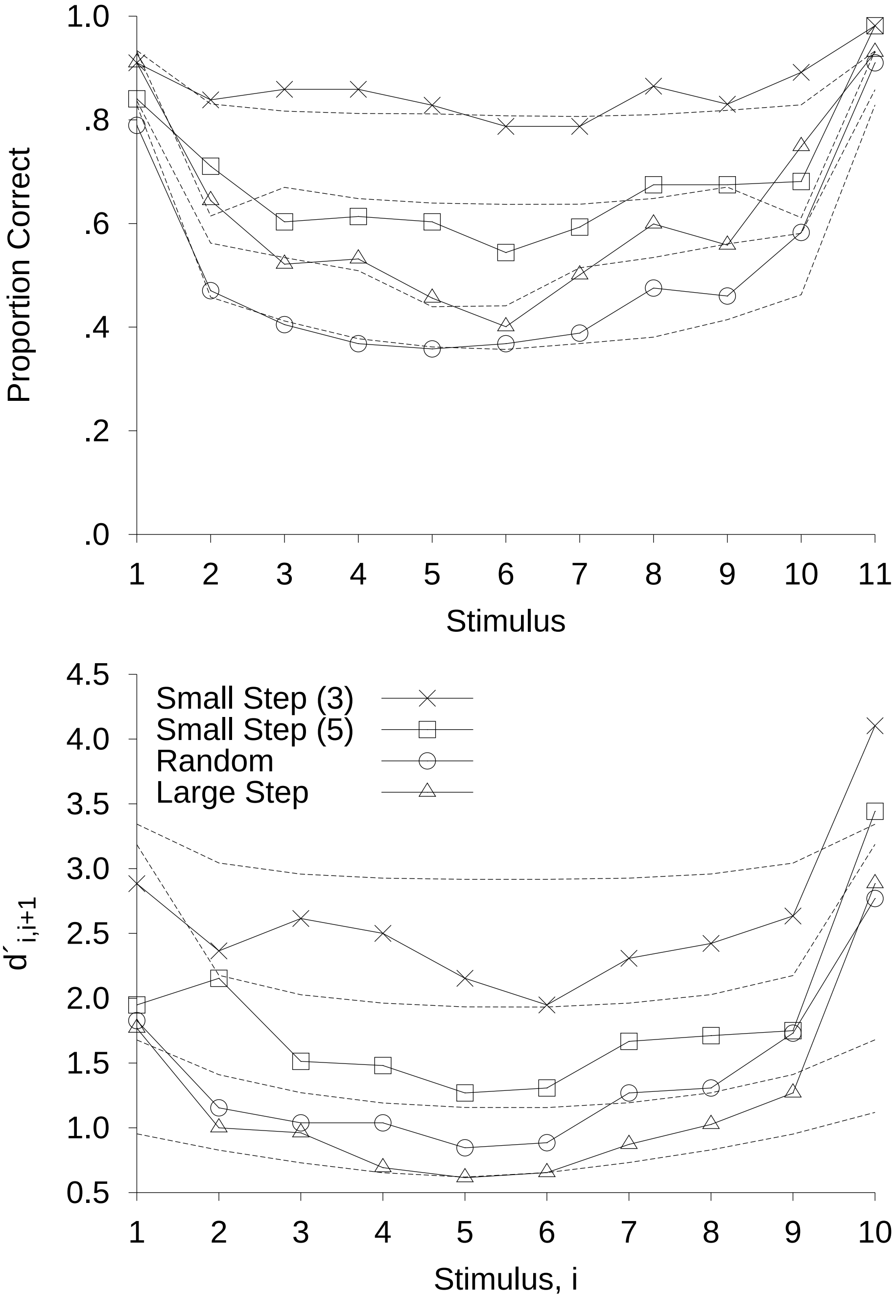
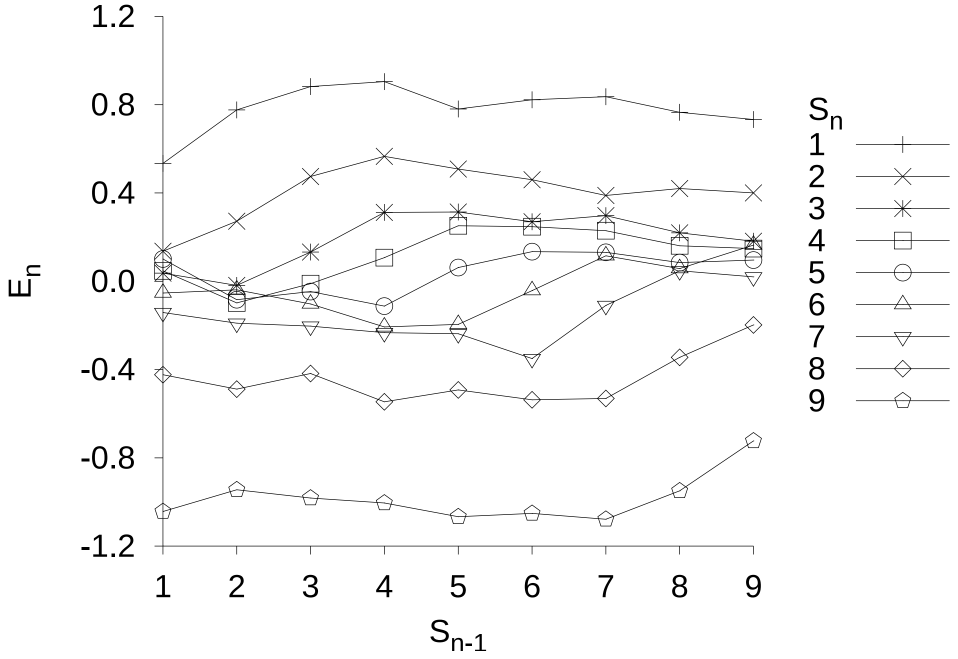

1.0
.8
.6
.4
.2
.0
Proportion Correct
1
2
3
4
5
6
7
8
9
10
11
Stimulus
4.5
4.0
3.5
3.0
2.5
2.0
1.5
1.0
0.5
$d'_{i,i+1}$
Small Step (3)
Small Step (5)
Random
Large Step
1
2
3
4
5
6
7
8
9
10
Stimulus, i

Figure 7

A


1.2
0.8
0.4
0.0
-0.4
-0.8
-1.2
E_n
1
2
3
4
5
6
7
8
9
S_{n-1}
S_n
1
2
3
4
5
6
7
8
9

B

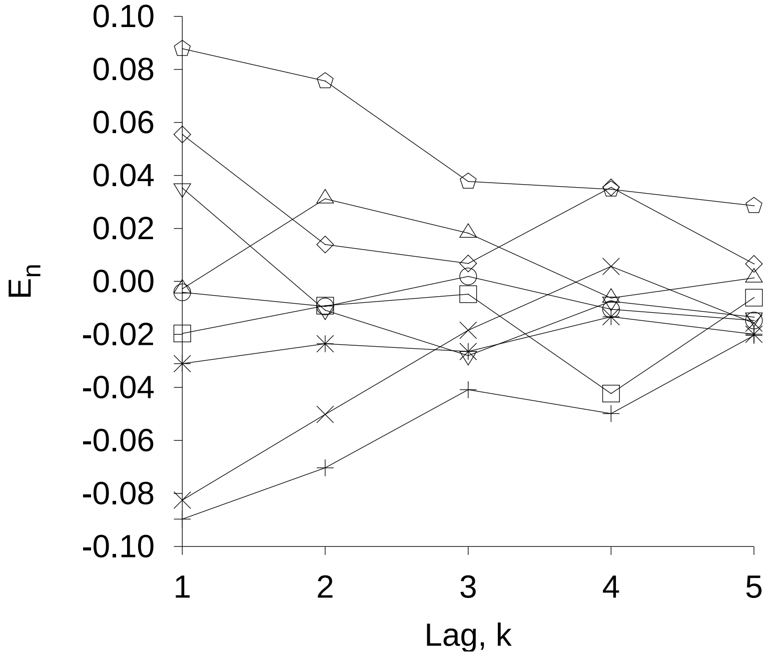

0.10
0.08
0.06
0.04
0.02
0.00
-0.02
-0.04
-0.06
-0.08
-0.10
E_n
1
2
3
4
5
Lag, k

Figure 8

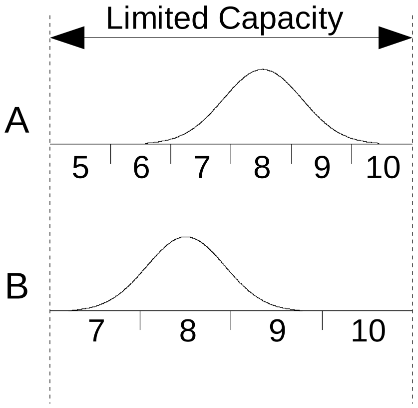


Figure 9

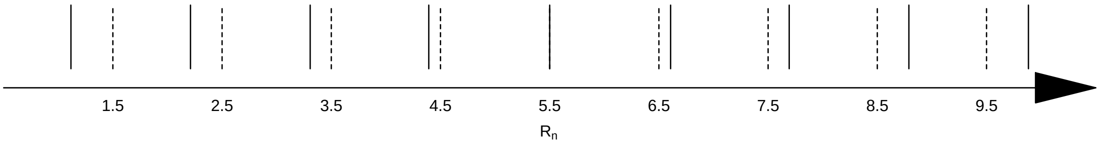


Figure 10

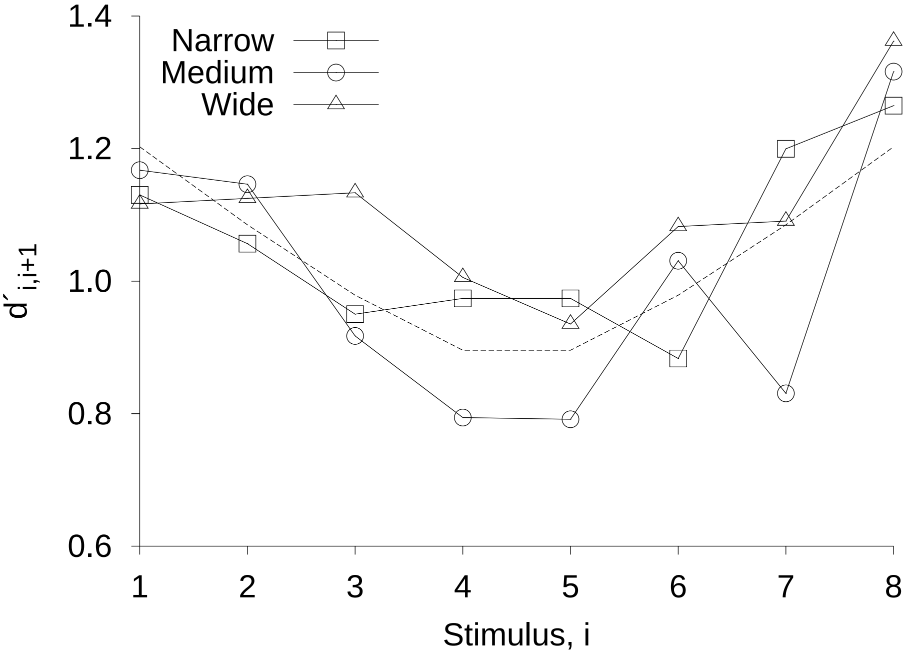


Figure 11

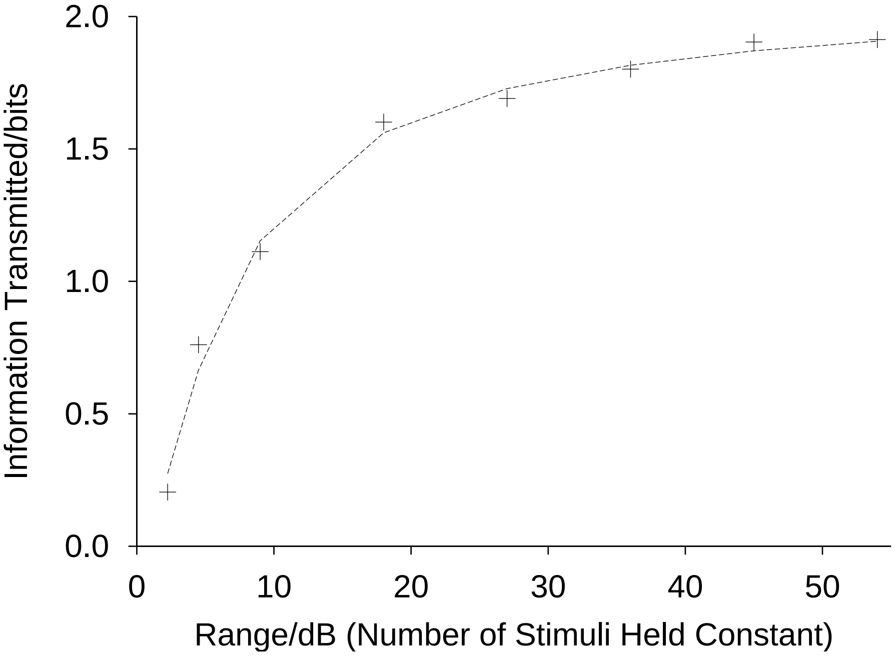

Information Transmitted/bits
2.0
1.5
1.0
0.5
0.0
0
10
20
30
40
50
Range/dB (Number of Stimuli Held Constant)

Figure 12

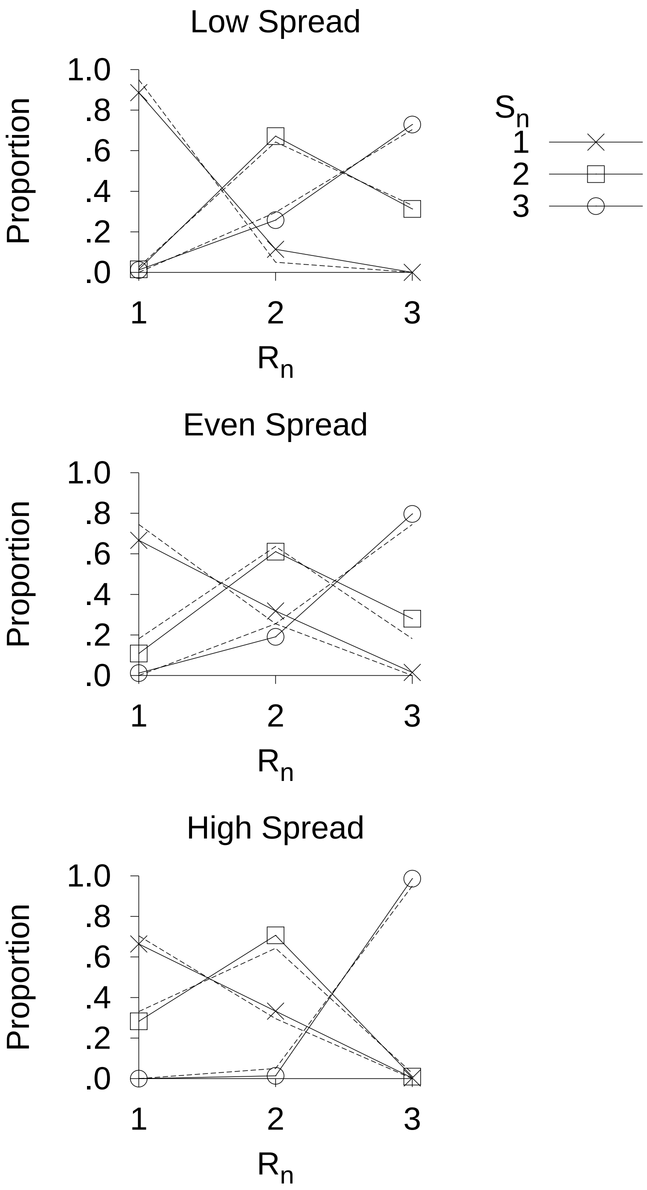

Low Spread
Proportion
1.0
.8
.6
.4
.2
.0
1
2
3
R_n
S_n
1
2
3
Even Spread
Proportion
1.0
.8
.6
.4
.2
.0
1
2
3
R_n
High Spread
Proportion
1.0
.8
.6
.4
.2
.0
1
2
3
R_n

Figure 13

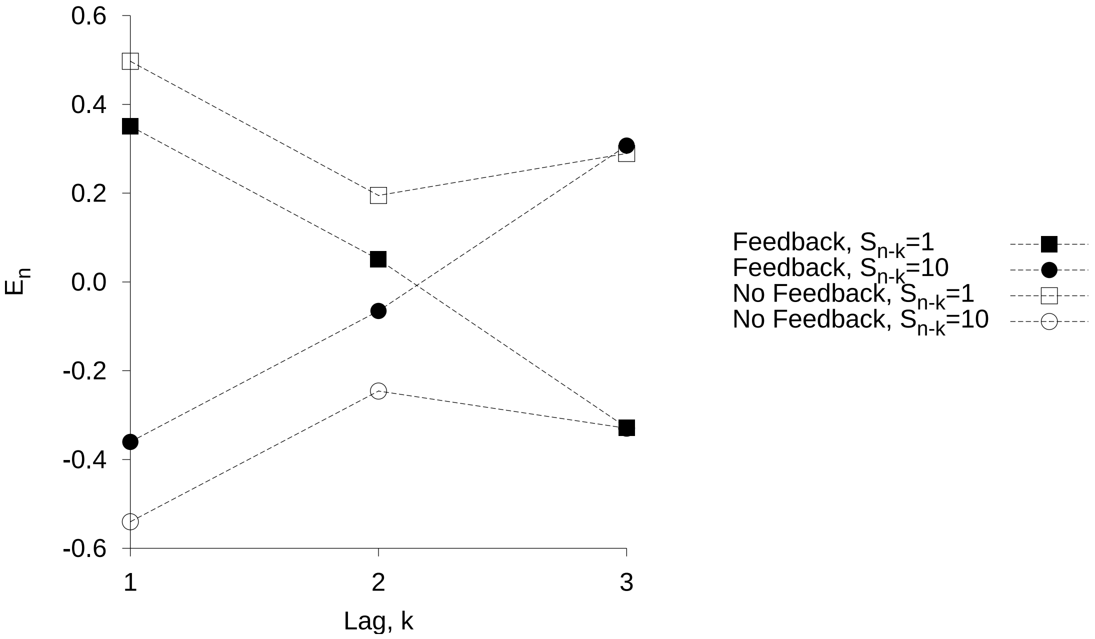

0.6
0.4
0.2
0.0
-0.2
-0.4
-0.6
E_n
1
2
3
Lag, k
Feedback, S_{n-k}=1
Feedback, S_{n-k}=10
No Feedback, S_{n-k}=1
No Feedback, S_{n-k}=10

Figure 14

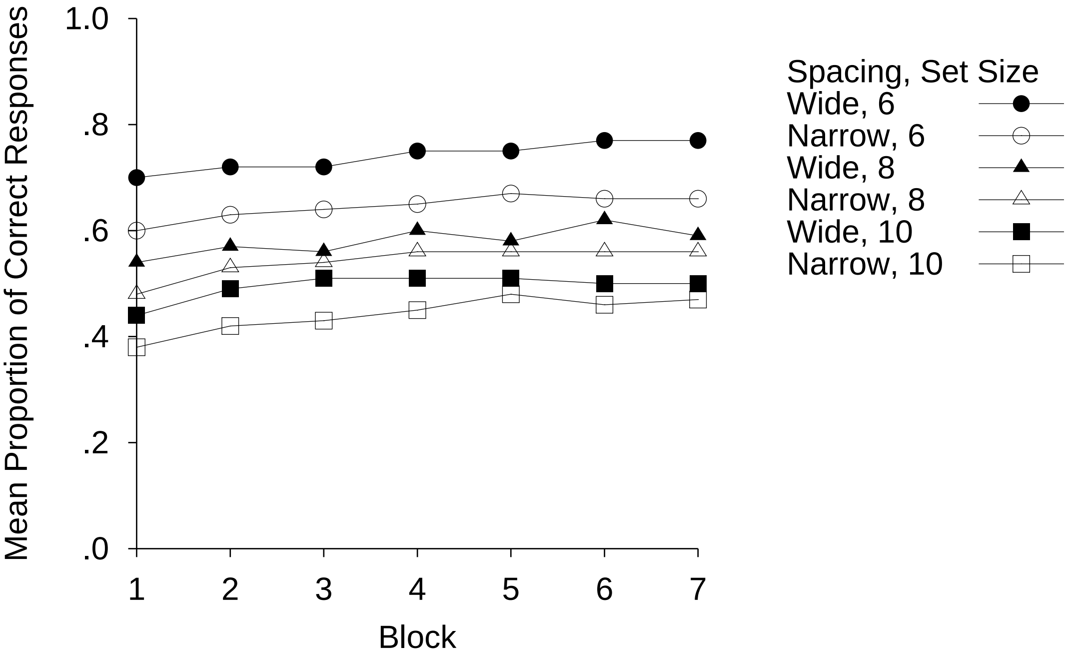

1.0
.8
.6
.4
.2
.0
Mean Proportion of Correct Responses
1
2
3
4
5
6
7
Block
Spacing, Set Size
Wide, 6
Narrow, 6
Wide, 8
Narrow, 8
Wide, 10
Narrow, 10

Figure 15

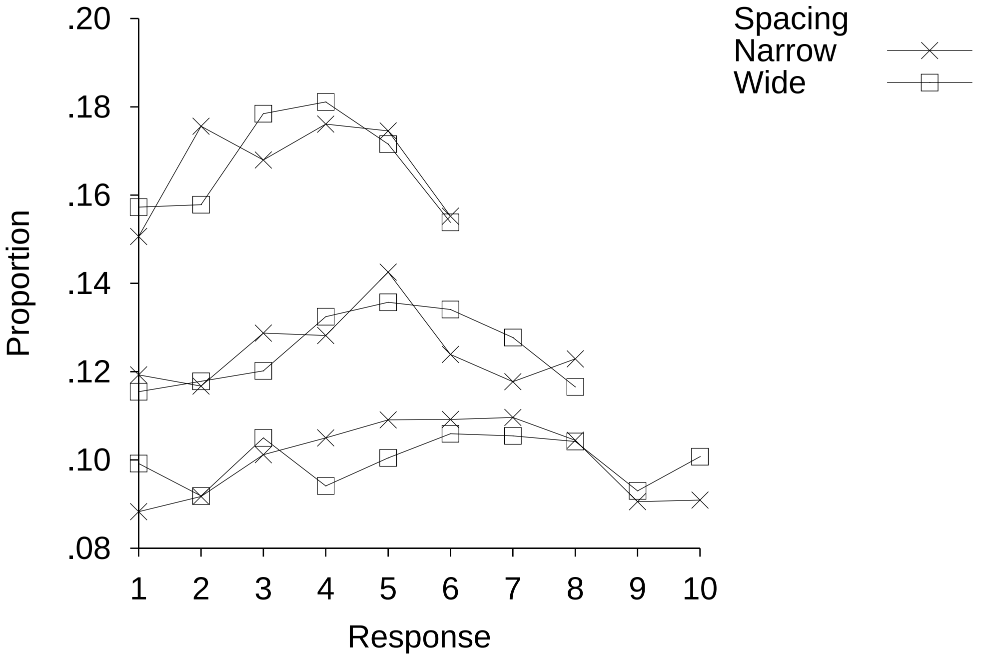
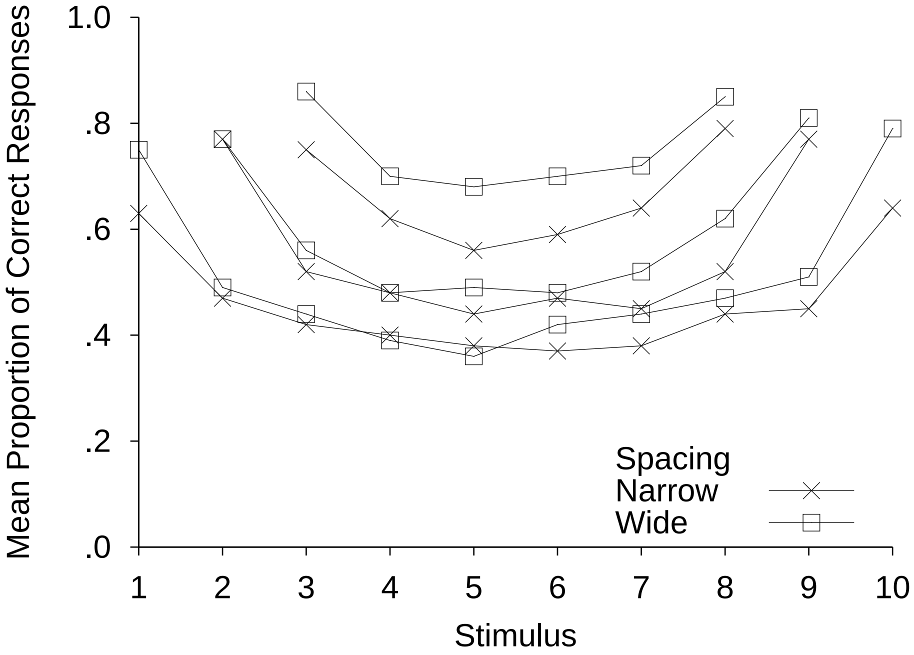

Spacing
Narrow
Wide
Proportion
.20
.18
.16
.14
.12
.10
.08
1
2
3
4
5
6
7
8
9
10
Response

Figure 16

A


Mean Proportion of Correct Responses
1.0
.8
.6
.4
.2
.0
1
2
3
4
5
6
7
8
9
10
Stimulus
Spacing
Narrow
Wide

B

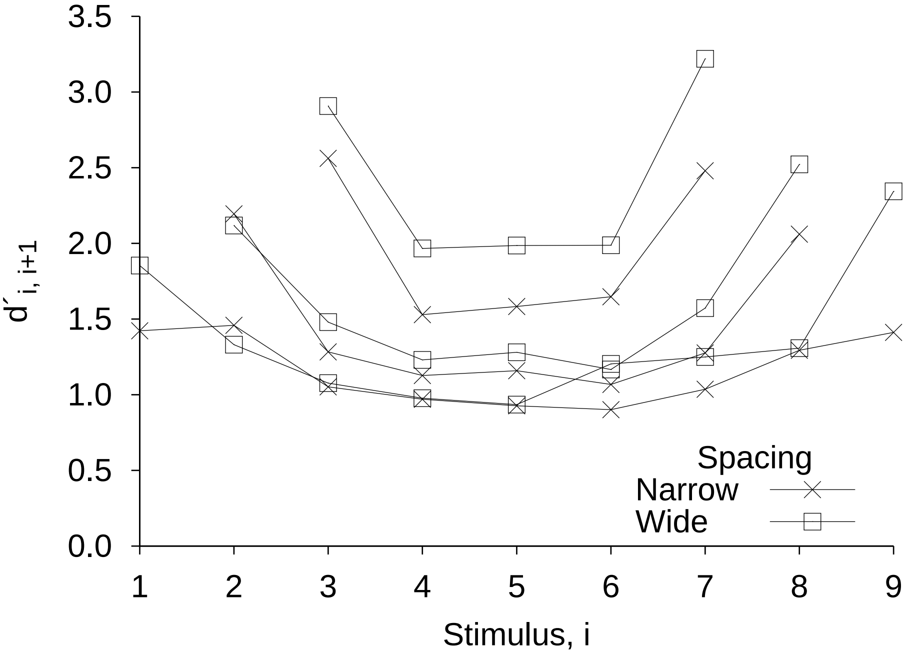

$d'_{i,i+1}$
3.5
3.0
2.5
2.0
1.5
1.0
0.5
0.0
1
2
3
4
5
6
7
8
9
Stimulus, i
Spacing
Narrow
Wide

Figure 17

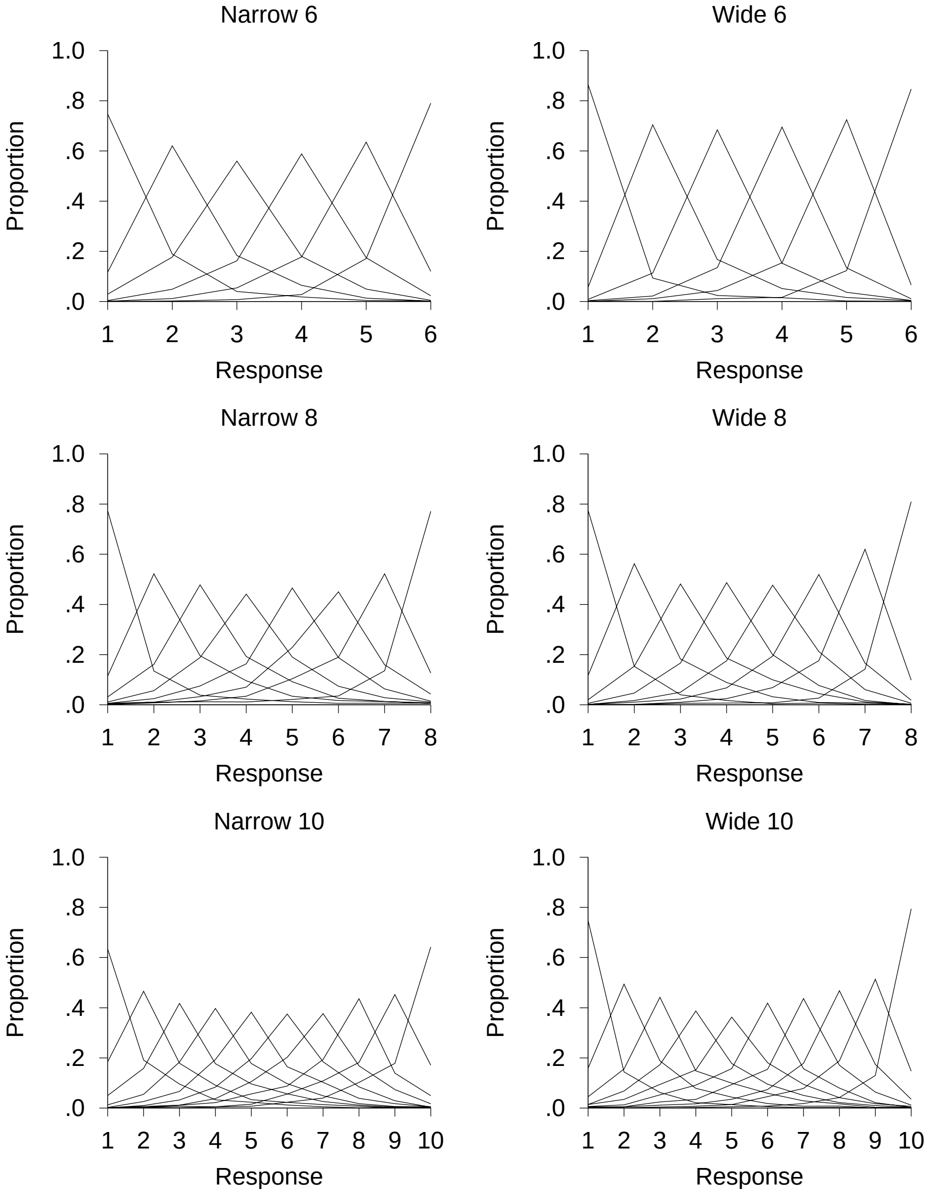

Narrow 6
Wide 6
Narrow 8
Wide 8
Narrow 10
Wide 10
Proportion
1.0
.8
.6
.4
.2
.0
Response
1
2
3
4
5
6
7
8
9
10

Figure 18

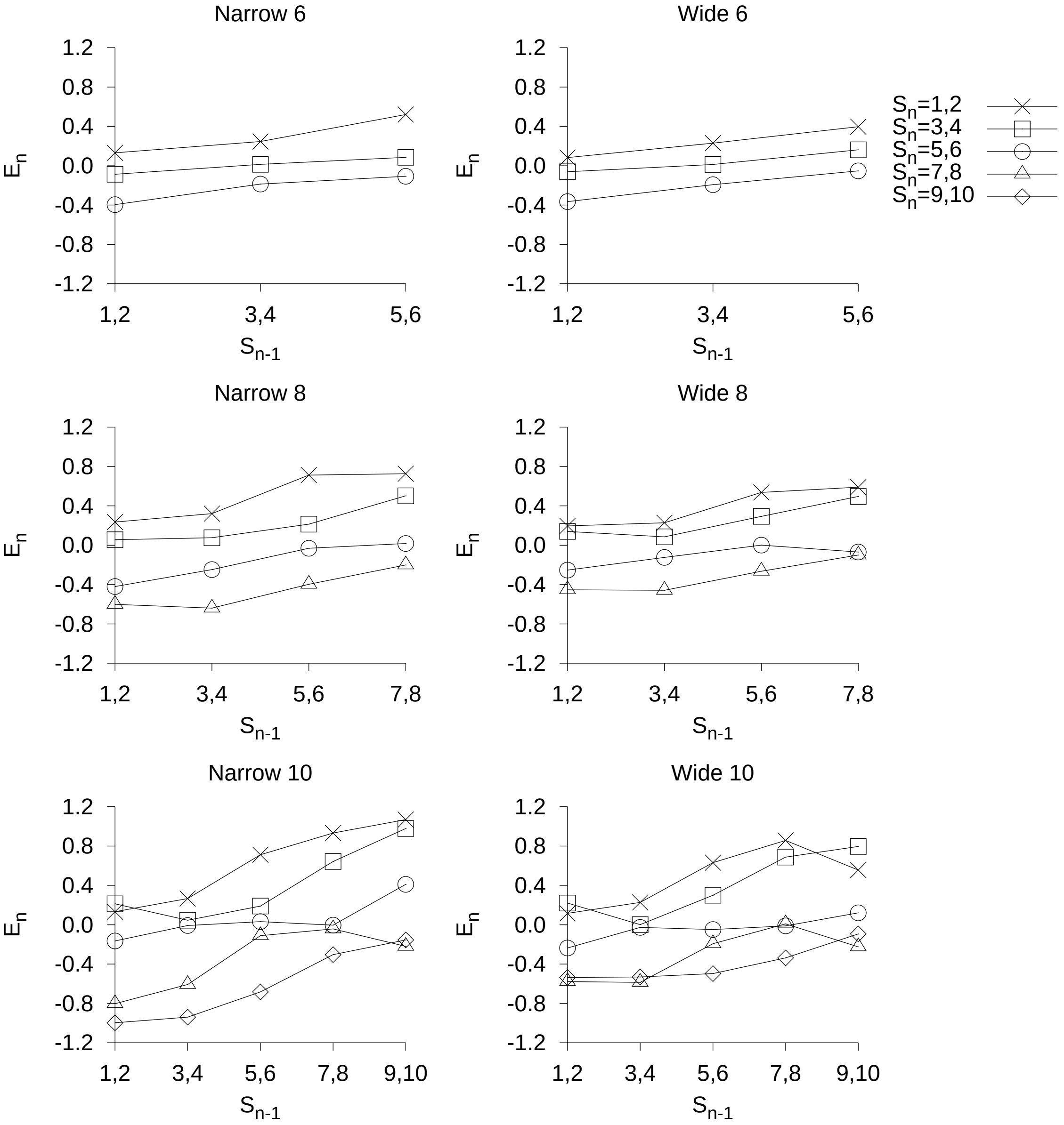

Narrow 6
Wide 6
Narrow 8
Wide 8
Narrow 10
Wide 10
E_n
S_{n-1}
1.2
0.8
0.4
0.0
-0.4
-0.8
-1.2
1,2
3,4
5,6
7,8
9,10
S_n=1,2
S_n=3,4
S_n=5,6
S_n=7,8
S_n=9,10

Figure 19

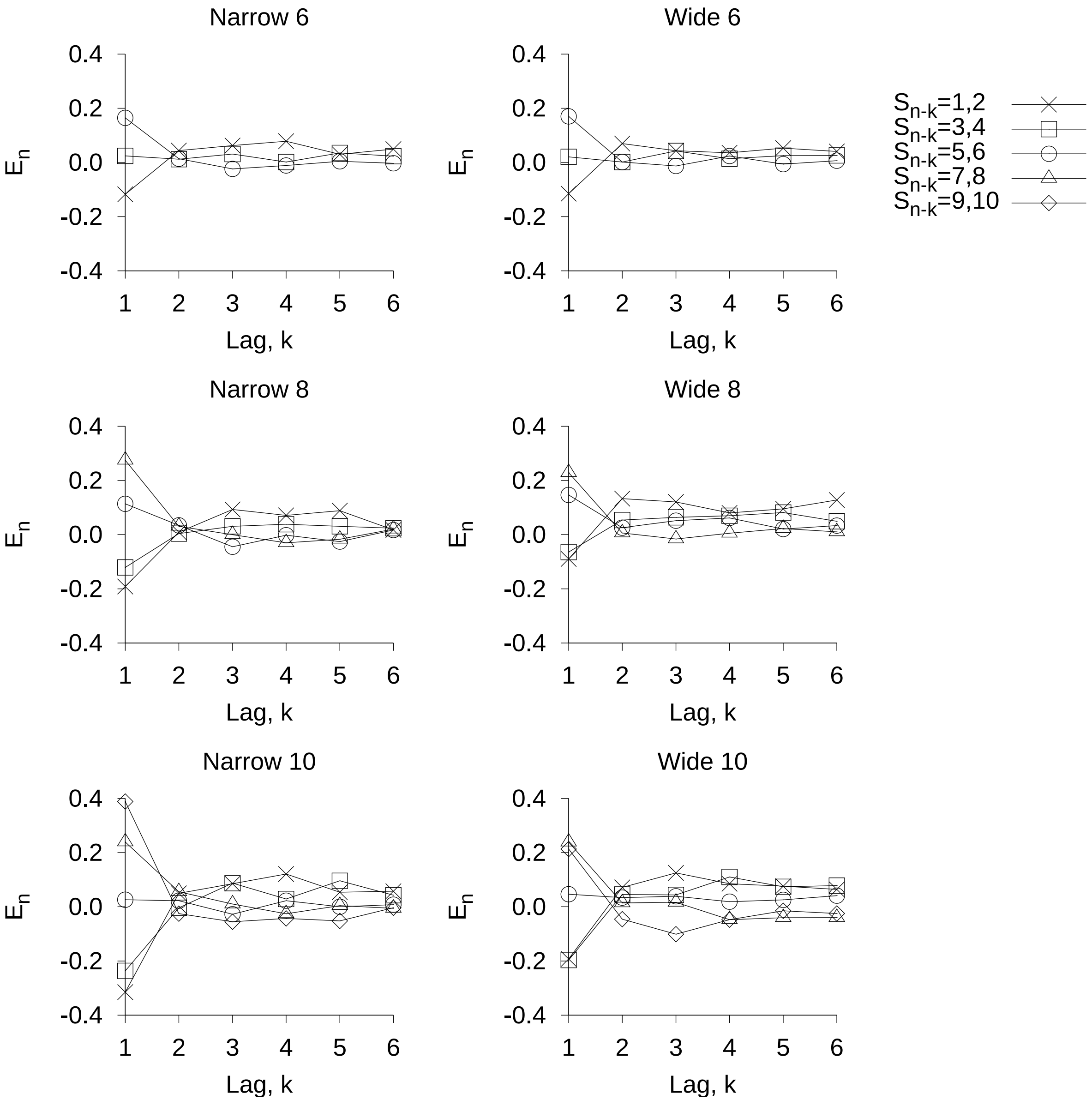

Narrow 6
Wide 6
Narrow 8
Wide 8
Narrow 10
Wide 10
0.4
0.2
0.0
-0.2
-0.4
E_n
1 2 3 4 5 6
Lag, k
S_{n-k}=1,2
S_{n-k}=3,4
S_{n-k}=5,6
S_{n-k}=7,8
S_{n-k}=9,10

Figure 20

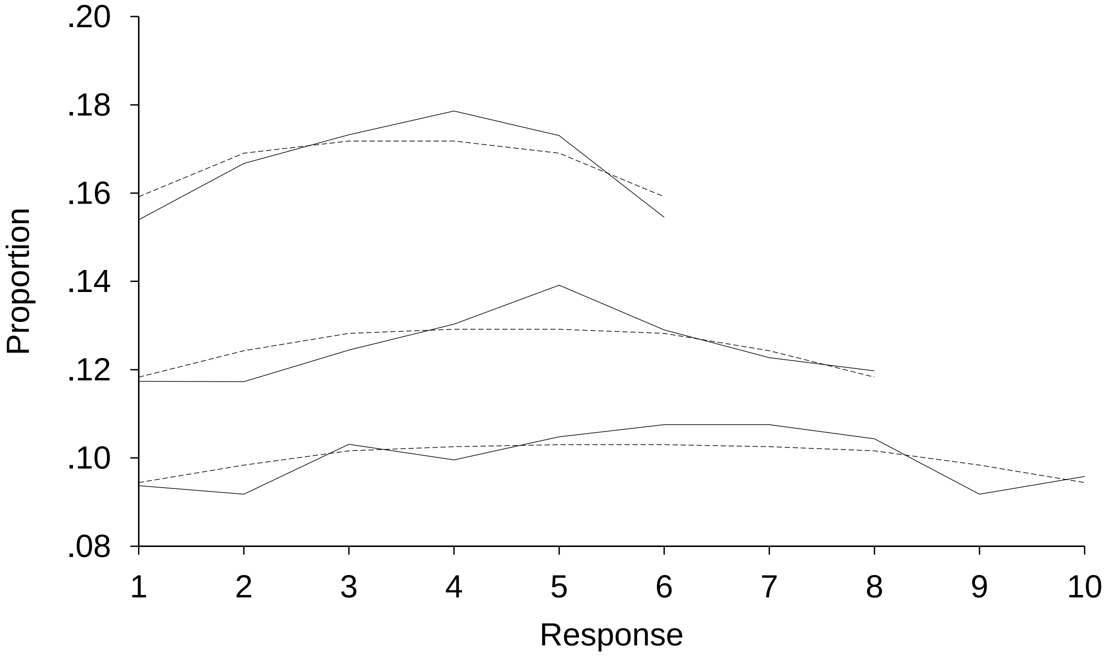


Figure 21

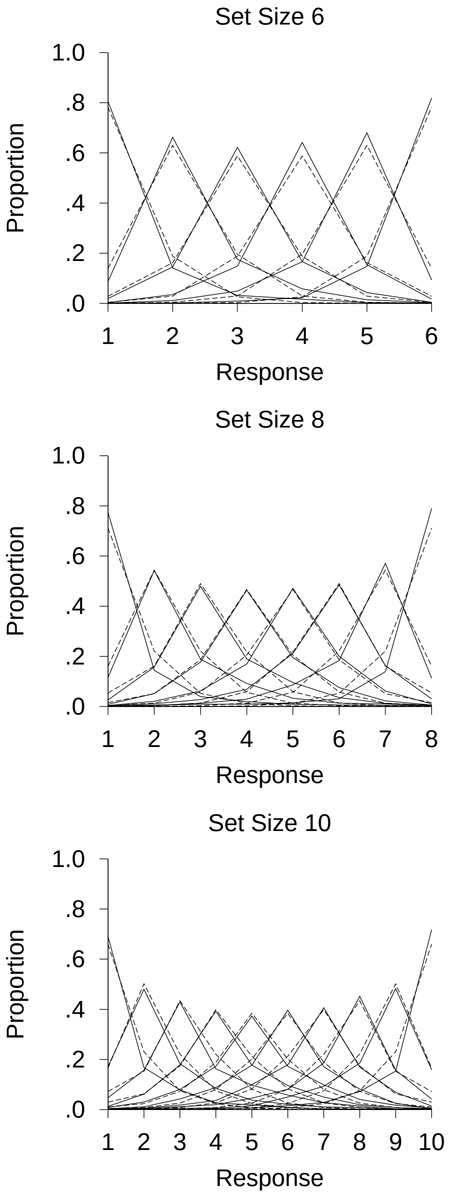

Set Size 6
Proportion
1.0
.8
.6
.4
.2
.0
1
2
3
4
5
6
Response
Set Size 8
Proportion
1.0
.8
.6
.4
.2
.0
1
2
3
4
5
6
7
8
Response
Set Size 10
Proportion
1.0
.8
.6
.4
.2
.0
1
2
3
4
5
6
7
8
9
10
Response

Figure 22

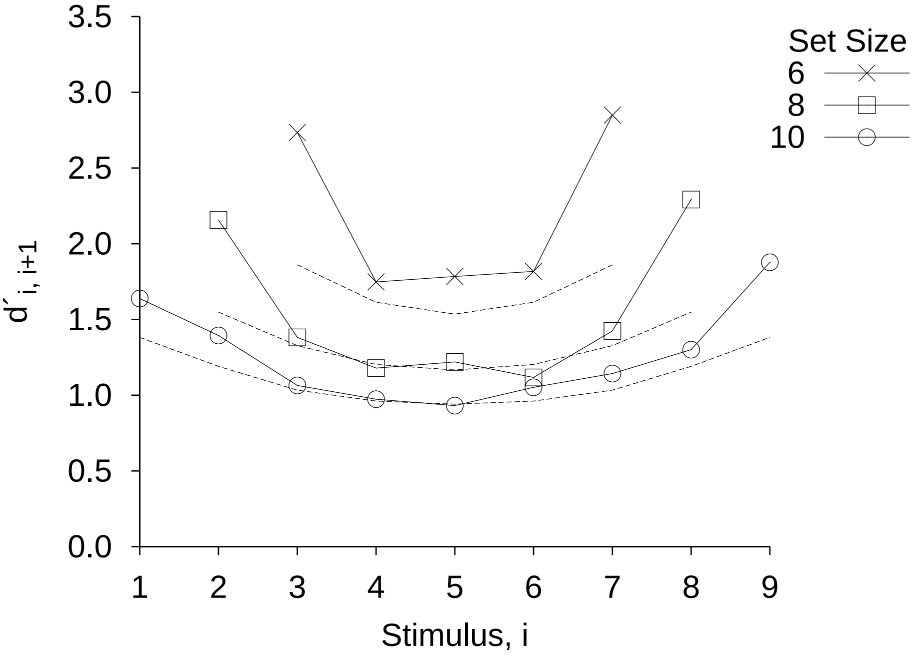


Figure 23

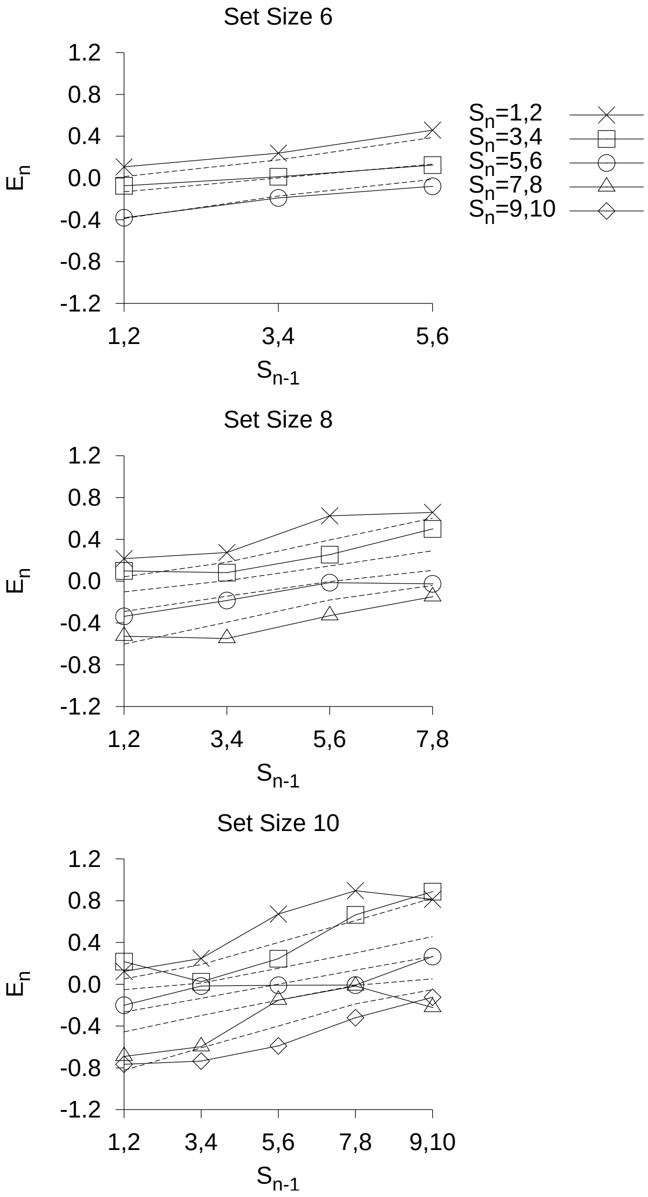

Set Size 6
Set Size 8
Set Size 10
E_n
S_{n-1}
1.2
0.8
0.4
0.0
-0.4
-0.8
-1.2
1,2
3,4
5,6
7,8
9,10
S_n=1,2
S_n=3,4
S_n=5,6
S_n=7,8
S_n=9,10

Figure 24

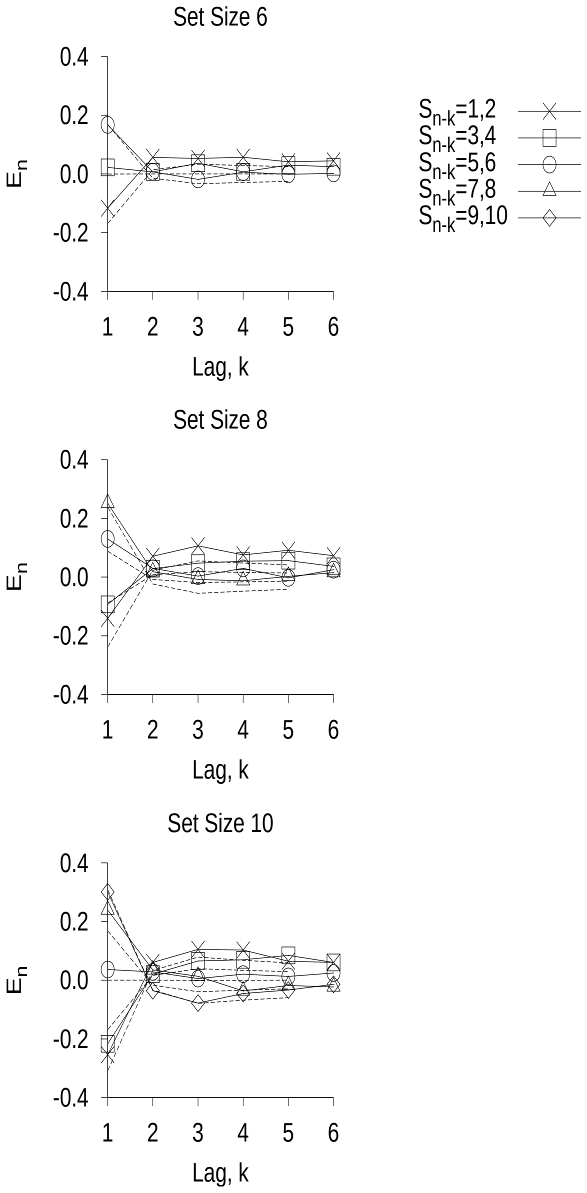

Set Size 6
Set Size 8
Set Size 10
E_n
0.4
0.2
0.0
-0.2
-0.4
1
2
3
4
5
6
Lag, k
S_{n-k}=1,2
S_{n-k}=3,4
S_{n-k}=5,6
S_{n-k}=7,8
S_{n-k}=9,10

Figure 25

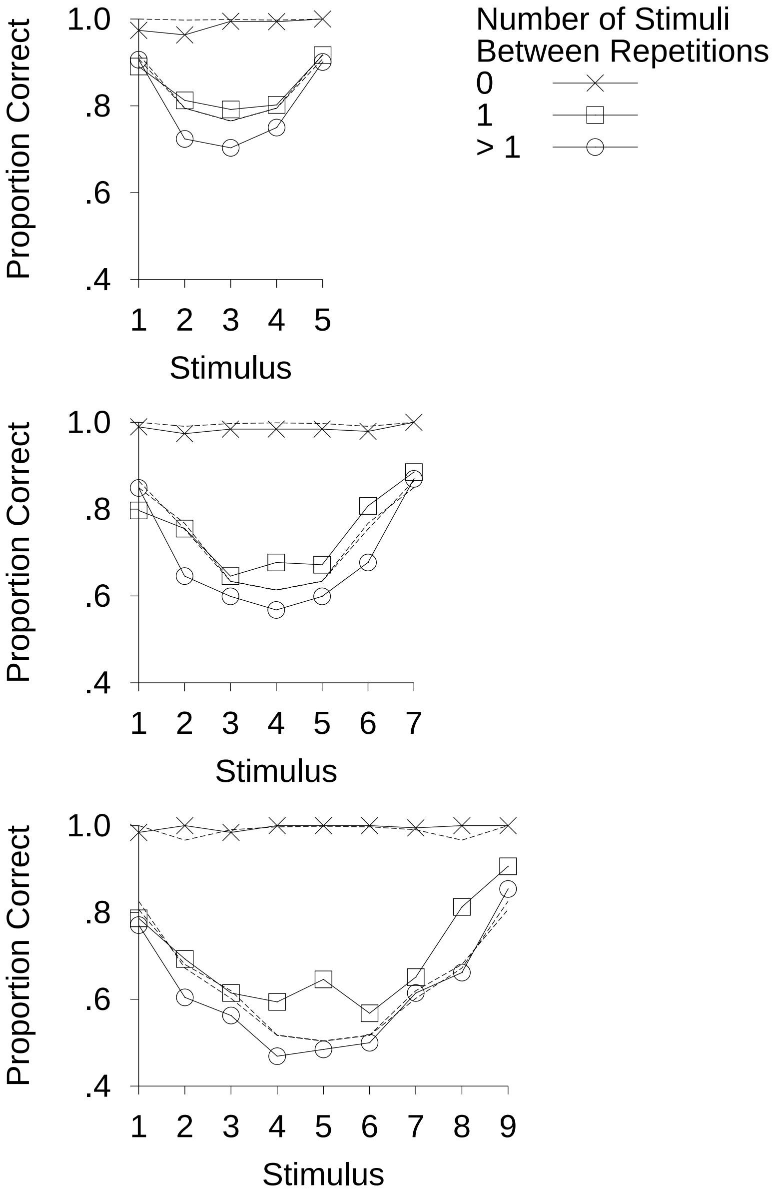

Number of Stimuli
Between Repetitions
0
1
> 1
Proportion Correct
1.0
.8
.6
.4
1 2 3 4 5
Stimulus
Proportion Correct
1.0
.8
.6
.4
1 2 3 4 5 6 7
Stimulus
Proportion Correct
1.0
.8
.6
.4
1 2 3 4 5 6 7 8 9
Stimulus

Figure 26

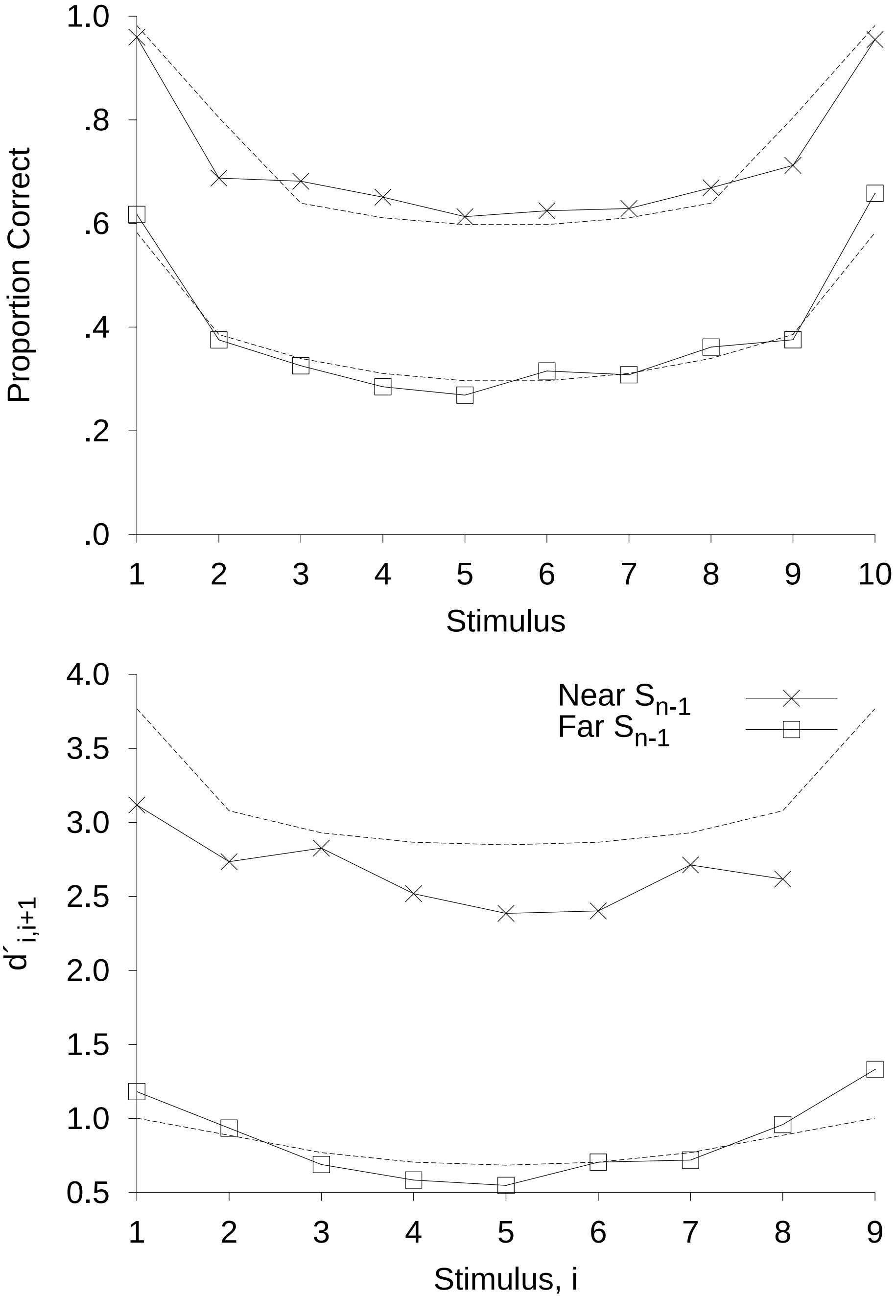

1.0
.8
.6
.4
.2
.0
Proportion Correct
1
2
3
4
5
6
7
8
9
10
Stimulus
4.0
3.5
3.0
2.5
2.0
1.5
1.0
0.5
$d'_{i,i+1}$
Near S_{n-1}
Far S_{n-1}
1
2
3
4
5
6
7
8
9
Stimulus, i

Figure 27

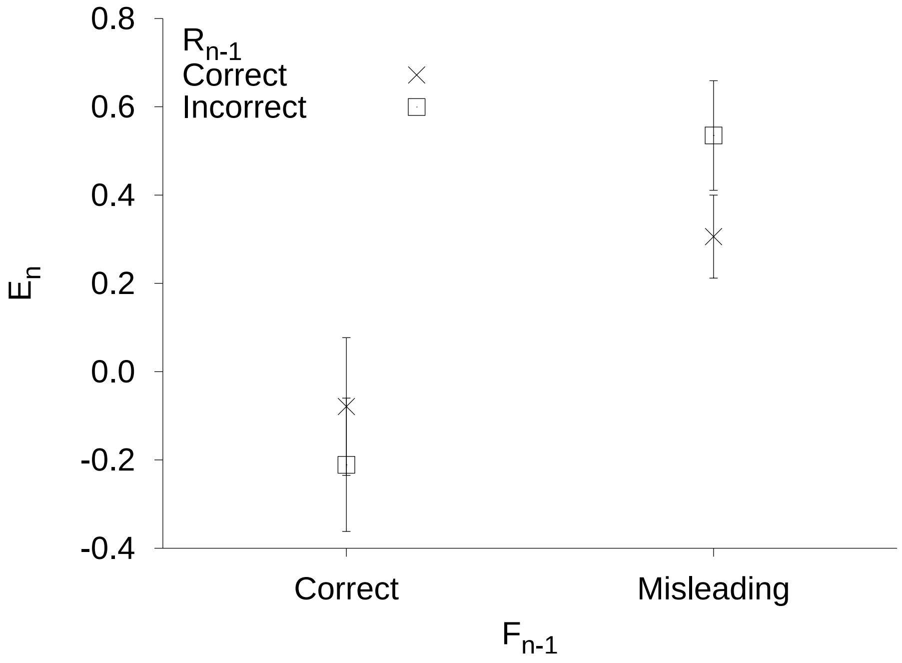
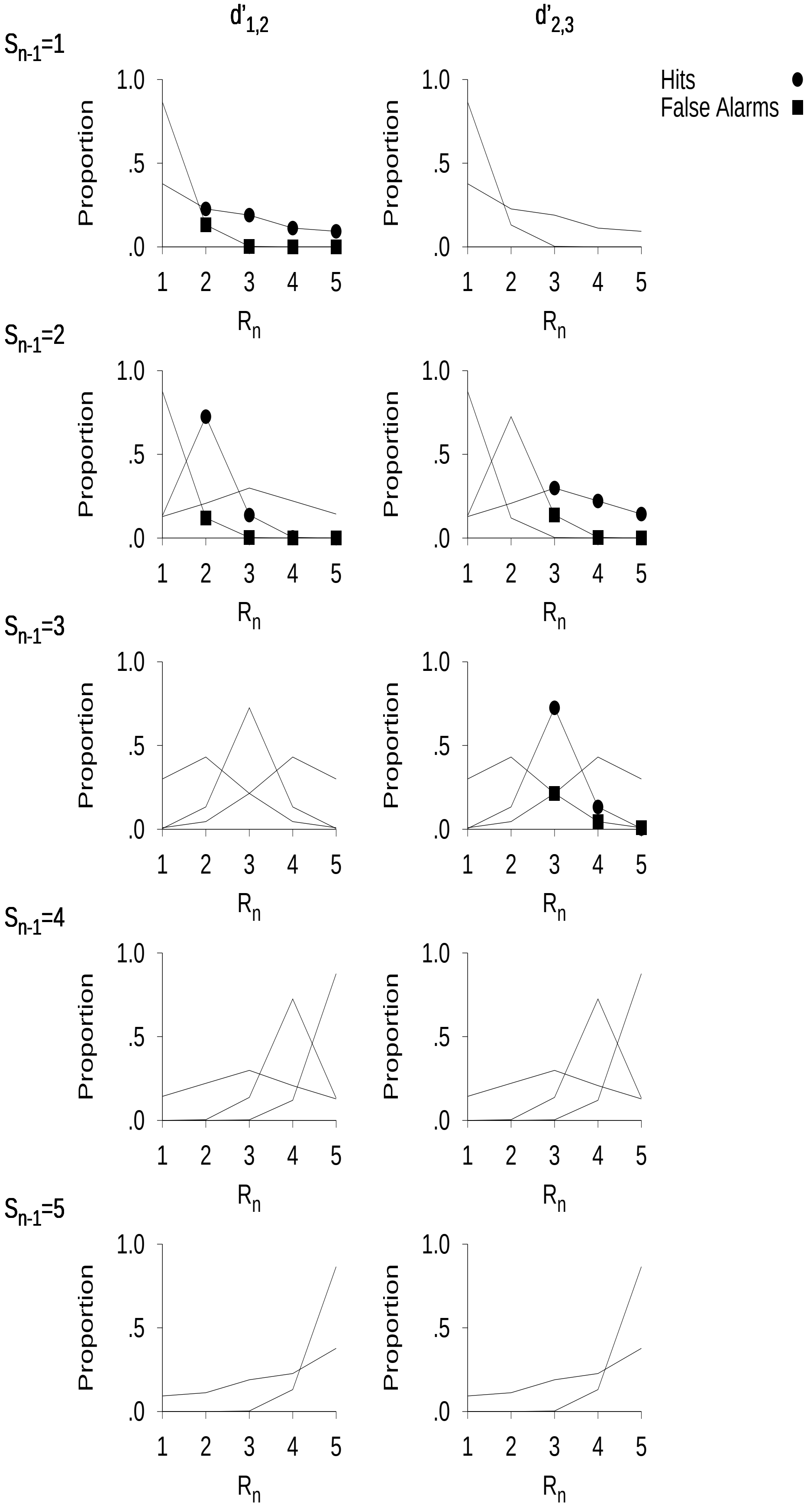

0.8
0.6
0.4
0.2
0.0
-0.2
-0.4
E_n
R_{n-1}
Correct
Incorrect
Correct
Misleading
F_{n-1}

Figure A1


d'$_{1,2}$
d'$_{2,3}$
S_{n-1}=1
S_{n-1}=2
S_{n-1}=3
S_{n-1}=4
S_{n-1}=5
Hits ●
False Alarms ■
Proportion
1.0
.5
.0
1
2
3
4
5
R_n