

Stimulus-specific learning: disrupting the bow effect in absolute identification

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Abstract The *bow effect* is ubiquitous in standard absolute identification experiments; stimuli at the center of the stimulus-set range elicit slower and less accurate responses than do others. This effect has motivated various theoretical accounts of performance, often involving the idea that end-of-range stimuli have privileged roles. Two other phenomena (practice effects and improved performance for frequently-presented stimuli) have an important but less explored consequence for the bow effect: Standard within-subjects manipulations of set size could disrupt the bow effect. We found this disruption for stimulus types that support practice effects (line length and tone frequency), suggesting that the bow effect is more fragile than has been thought. Our results also have implications for theoretical accounts of absolute identification, which currently do not include mechanisms for practice effects, and provide results consistent with those in the literature on stimulus-specific learning.

Keywords Memory · Perceptual categorization and identification · Practice effects

The absolute identification paradigm explores a fundamental limit: that the number of separate categories that can reliably

be identified along a single physical dimension is very small (about 7 ± 2 , according to Miller, 1956). In a typical absolute identification experiment, a participant is presented with a set of stimuli that vary along only one dimension (e.g., lines varying in length or tones varying in intensity). These stimuli are labeled with the numerals 1 to N in order of increasing magnitude. The participant is then shown one stimulus at a time in a random order and is asked to respond with its label. Despite the task's apparent simplicity, absolute identification data reliably exhibit a great many phenomena, some of which are quite complex (for reviews, see Petrov & Anderson, 2005; Stewart, Brown, & Chater, 2005).

In this article, we focus on one of the most fundamental of these phenomena: that performance is better for stimuli at the outer edges of the stimulus range and worse for those in the center. This phenomenon is called the *bow effect* because a U-shaped curve is observed when accuracy is plotted against stimulus magnitude and an inverted U-shaped curve when response time (RT) is plotted. These bow effects are robust phenomena that are consistent across manipulations of stimulus magnitude (Lacouture, 1997), the number of stimuli (*set size*; - Stewart et al., 2005), and sensory modalities (Dodds, Donkin, Brown, & Heathcote, 2011).

However, recent evidence that practice improves absolute identification performance (Dodds et al., 2011; Rouder, Morey, Cowan, & Pfaltz, 2004) implies that the bow effect could be disrupted by practice when the effects of practice are stimulus-specific. Previously, it was widely believed that even extended practice did not lead to much improvement in absolute identification (see, e.g., Miller, 1956; Shiffrin & Nosofsky, 1994), but recent research has shown that improvements can be made for some types of stimuli. For example, Dodds et al. demonstrated that practice improved performance a great deal when the stimuli were lines varying in length or tones varying in frequency, but very little when

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the stimuli were tones varying in loudness. Using tones varying in frequency, Cuddy (1968, 1970) found that presenting some stimuli more often than others resulted in an overall improvement—for all stimuli. This effect was limited, however, to trained musicians and to a task more akin to categorization than to standard absolute identification. Using a more standard paradigm and untrained participants, Cuddy, Pinn, and Simons (1973) demonstrated improved performance across the entire stimulus range when one stimulus was presented more often than the others (but see Chase, Bugnacki, Braid, & Durlach, 1982, for conflicting results).

We generalized these earlier findings in several ways. We examined more than one kind of stimulus dimension (not just tones varying in frequency), and we also used a more standard paradigm in which, during each block of trials, all stimuli were presented equally often. The latter constraint was important because presenting some stimuli more frequently than others encouraged participants to bias their responses. Instead of using unequal presentation frequency within blocks, we employed a different experimental manipulation that encouraged stimulus-specific learning; changing the stimulus set size on a within-subjects basis between blocks of trials. In our design, a participant first would be asked to identify two stimuli (“set size two,” denoted “ $N = 2$ ”) and, in a later phase, would be asked to identify these two stimuli along with six others, in an $N = 8$ condition. The stimulus set for the smaller set size was created from the middle stimuli of the larger set size (e.g., the two stimuli for $N = 2$ were the same as the middle two stimuli from $N = 8$).

In many other paradigms, practice has stimulus-specific effects; that is, extensive practice with some stimuli does not confer a benefit upon other, similar stimuli. For example, there is an extensive literature on perceptual learning that has almost uniformly shown poor generalization (for a review, see Petrov, Doshier, & Lu, 2005). This background makes Cuddy's (1968, 1970) results (general improvement in absolute identification after practice with just one stimulus) quite surprising. However, this contrast is complicated because of Cuddy's nonstandard identification paradigm. If we find, using a standard absolute identification paradigm, that practice improves performance in a stimulus-specific (rather than task-wide) manner, and if the improvements persist across changes in set size, prior exposure to the $N = 2$ condition might improve performance on the middle two stimuli for the $N = 8$ condition. This performance boost would disrupt the bow effect for the larger set size.

Reexamination of existing absolute identification data lends some preliminary support to this hypothesis. As a baseline comparison, first consider Stewart et al.'s (2005) Experiment 1, in which set size was manipulated between

subjects; some participants performed an absolute identification task with six tones of varying frequency, others with eight tones, and still others with ten tones (i.e., $N = 6, 8,$ or 10). The data from this experiment (Fig. 2) exhibit the standard bow effect in each set size, with poorest performance for the middle stimuli.

In contrast, Kent and Lamberts (2005) had 3 participants perform absolute identification with dots varying in separation, using four different set sizes ($N = 2, 4, 6,$ and 8), manipulated within-subjects. Each participant experienced all set sizes, one after the other,¹ and each participant showed a disruption (flattening) of the bow effect. Figure 1b illustrates a disrupted bow effect, with much shallower bows than in Stewart et al.'s data. Most pertinent for our study is the result for set size $N = 8$ (bold in Fig. 1b). Kent and Lamberts's data from this condition display a much shallower bow effect than before. Identification of the middle stimuli is enhanced to the point where response accuracy for the central stimuli (4 and 5) is just as good as response accuracy for the next-to-extreme stimuli (2 and 7). In comparison, the standard effect (as in Fig. 1a) exhibits a deep bow, so that there is a large difference between these pairs of stimuli. Our statistical analyses are motivated by this pattern and test for a standard bow effect by assessing performance differences between the central stimuli and the next-to-edge stimuli.²

Although the data in Fig. 1 are suggestive, they must be interpreted with caution. There were many differences between Stewart et al.'s (2005) and Kent and Lamberts's (2005) experiments besides the between- versus within-subjects manipulation of set size—for example, different stimulus modalities, different amounts of training per participant, and different set sizes. Furthermore, the W-shape in Fig. 1b is quite clear in Kent and Lamberts's data when averaged over their 3 participants, but further examination reveals large differences between participants—differences that do not uniformly support the hypothesis that pretraining on smaller set sizes will disrupt the bow effect for larger set sizes. Our Experiments 1 and 2 were an attempt to clarify the evidence that stimulus-specific practice can disrupt the bow effect and to test our hypothesized explanation of this disruption that it is due to stimulus-specific practice effects caused by unequal stimulus presentation frequencies.

¹ Set size was partially counterbalanced between subjects. Participant 1 saw $N = 10, 4, 2, 8,$ and then 6; participant 2 saw $N = 10, 6, 8, 2,$ and then 4; and participant 3 saw $N = 4, 8, 2,$ and then 6. We did not include set size $N = 10$ in the figure, because not all participants experienced this condition.

² Comparing the central stimuli (4/5) against their neighbors (3/6) provides little power to detect a standard bow effect, because the bow curvature is smallest in the center (as in Fig. 1a). On the other hand, comparing the central stimuli (4/5) against the edge stimuli (1/8) will classify all but the most severe disruptions of the bow effect as *standard bows*;—for example, the disrupted bow effect in Fig. 1b still has better performance on edge stimuli than on central stimuli.

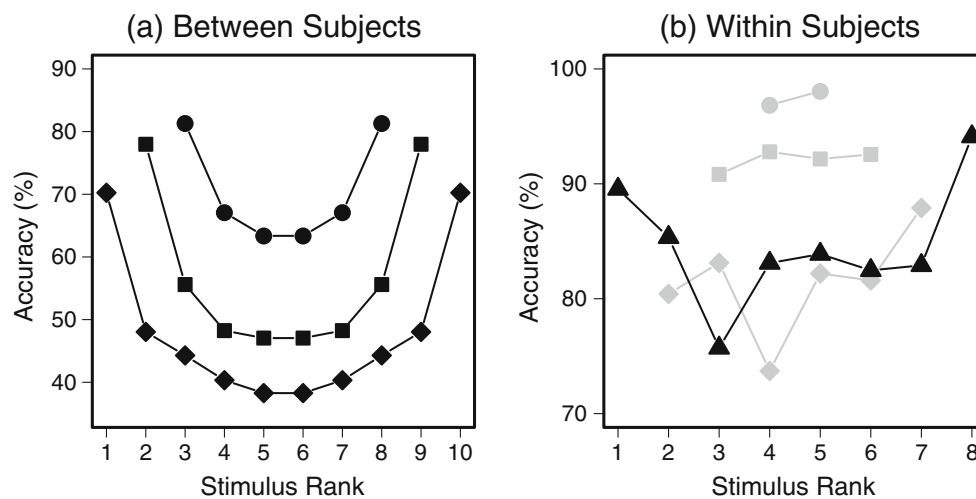


Fig. 1 **a** Between-subjects data from Stewart, Brown, and Chater's (2005) Experiment 1 and **b** within-subjects data from Kent and Lamberts's (2005) Experiment 2. Each line on each graph represents a different set size. All set sizes in panel b are in gray except set size $N = 8$, for ease of comparison. Both plots show accuracy, measured

by the percentage of correct responses, averaged over participants, for different set sizes. Stewart et al. "symmetrized" their data by averaging responses to small and large stimuli in corresponding pairs; we additionally averaged their data over stimulus spacing (wide vs. narrow)

Experiment 1

Dodds et al. (2011) found that practice can improve identification of line length, but not of tone loudness. Hence, our hypothesis makes a clear prediction: If stimulus-specific practice disrupts the bow effect, a within-subjects manipulation of set size should disrupt the bow effect when the stimuli are lines varying in length, but not when they are tones varying in loudness.

Method

Twenty-three participants were randomly allocated to either an absolute identification task using tone loudness (12 participants) or line length (11 participants). The stimuli for the line length task were eight pairs of small white squares, varying in horizontal separation. Each square had sides of length 3.3 mm and was shown at high contrast. The stimuli are referred to as lines varying in length because the participant is essentially making a judgment of length. The eight horizontal separations were 23.5, 26.0, 29.1, 32.2, 35.6, 39.3, 43.4, and 47.4 mm. The viewing distance was not physically constrained but was approximately 700 mm (so the stimuli subtended visual angles ranging from 3.3° to 6.7°). For the tone loudness condition, the stimuli were eight 1000-Hz pure sine tones with loudness varying from 79 to 100db, in increments of 3db. Tones were generated using MATLAB 2009a, with stepped onsets and offsets (although, by definition, the sine waves started and finished at zero amplitude, because their duration was an integer-multiple of their frequency).

Before beginning the experiment, participants were presented with each of the eight stimuli, one at a time, along with the corresponding label. On every trial, participants were first shown a fixation cross for 300 ms, which was removed when the stimulus was presented. In the line-length condition, the stimulus remained on screen for 1 s, after which a mask appeared. The mask consisted of approximately 50 white squares of the same size as that used for the stimuli, randomly scattered across the screen. In the tone loudness condition, the tone played for 1 s, followed by silence. In both conditions, participants were able to respond at any point after the stimulus presentation onset. Responses were made by pressing the appropriate numeral key (from 1 to 8) on the top line of the keyboard. Participants were given one opportunity to respond, after which feedback was provided.

Each participant took part in two 1-h sessions, on separate days, for a total of 20 blocks of 80 trials each. The first 5 blocks in the first session used only the middle two stimuli ($N = 2$), and all subsequent blocks used all stimuli ($N = 8$). When the participants were presented with only the middle two stimuli in the first 5 blocks in the first session, they responded to these with the numerals 4 and 5. In total, every participant received 200 presentations each of stimuli 4 and 5 when $N = 2$ and 150 presentations of each of the eight stimuli when $N = 8$. This meant that, across the whole experiment, each participant received 350 presentations each of stimuli 4 and 5 and 150 presentations of each of the other stimuli.

Results

Responses in the $N = 2$ condition were quite accurate and rapid in both the length and loudness conditions: Mean

accuracy was 78% for length and 86% for loudness, and mean RT was 1.03 s for length and 0.84 s for loudness. Figure 2 shows mean response accuracy and mean RTs for correct responses, both conditional on stimulus rank, for the $N = 8$ condition, separately for line length and tone loudness. Across all stimuli, average accuracy was very similar for loudness and line length (46.8% and 47.1%, respectively), but the pattern of performance was quite different. A typical, deep bow effect was observed for tone loudness: The mean accuracy was significantly higher for stimuli 2 and 7 ($M_{2/7} = 46\%$) than for stimuli 4 and 5 ($M_{4/5} = 33\%$), and the mean RT was significantly shorter ($M_{2/7} = 1.25$ s, $M_{4/5} = 1.37$ s). These differences were statistically reliable according to linear contrasts comparing the two group means [for accuracy, $F(1, 77) = 31.4$, $p < .001$; for RT, $F(1, 77) = 10.9$, $p = .001$].

For the line length condition, however, the bow effect was clearly disrupted, with stimuli in the middle of the range (4 and 5) eliciting responses that were faster and more accurate than those for some other stimuli. In particular, linear contrasts showed that neither the mean accuracy nor the mean RT for stimuli 2 and 7 ($M_{\text{acc}} = 48\%$, $M_{\text{RT}} = 1.64$ s) was significantly different from the mean accuracy or mean RT for stimuli 4 and 5 ($M_{\text{acc}} = 48\%$, $M_{\text{RT}} = 1.68$ s; both F s < 1).

The analyses above show a standard bow effect for tone loudness, but not for line length. Nevertheless, these results do not directly support the conclusion that the bow effect was shallower in the line length condition than in the tone loudness condition (since “the difference between ‘significant’ and ‘not significant’ is not itself statistically significant”; Gelman & Stern, 2006). To directly test this hypothesis, we calculated an interaction contrast comparing the depth of the bow effect between the two conditions, by taking the difference of the two contrasts reported above and testing it against the appropriate error variance term from the mixed ANOVA. These contrasts confirmed that there was a deeper bow effect for tone loudness than for dot separation response

accuracy data, $F(1, 147) = 10.7$, $p < .001$. For RT data, the comparison was not significant, $F(1, 147) = 1.63$, $p = .10$.

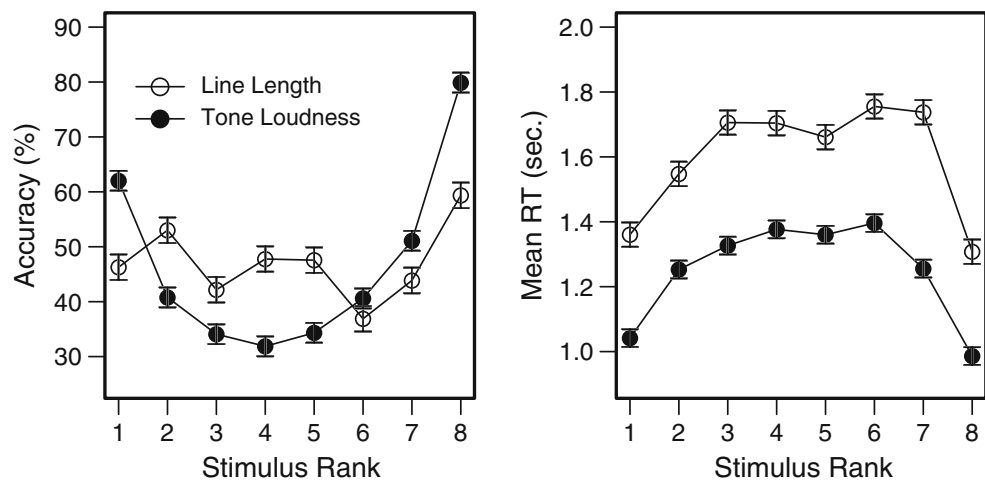
Discussion

Participants in [Experiment 1](#) first practiced identification of two central stimuli (in an $N = 2$ condition) and then identification from the full set of eight stimuli. Data from those participants who identified tone loudness were quite standard, with the poorest performance for middle stimuli and deep bow effects. However, for those participants who identified line lengths, performance for the central stimuli improved, to the point where it was not significantly poorer than performance for the next-to-edge stimuli. A potential weakness of this result is that null findings may be due to limited statistical power. To foreshadow, [Experiment 2](#) addressed this concern and obtained differences similar to those in [Experiment 1](#), using a different design with the same line length stimulus set.

The results of [Experiment 1](#) suggest that the effects of pre-exposure to the central stimuli can, for certain stimulus dimensions, persist for long time intervals on the order of hours, rather than minutes, as other authors have observed for the effects of stimulus presentation frequency (e.g., Petrov & Anderson, 2005). The performance bonus that we found in the line length condition persisted into the second experimental session, which was, on average, a full day after the extra presentations of the two central stimuli (in the $N = 2$) condition. This was true even when we limited analyses to data from session two, during which the $N = 2$ condition was not experienced.

Performance curves similar to those obtained here have also been observed by Kent and Lamberts (2005, Experiment 2) and Lacouture, Li, and Marley (1998, Experiment 1), each of whom observed relatively flattened bow effects even when stimuli were presented equally often in each condition. The main difference between those experiments and those

Fig. 2 Accuracy and mean response times (RTs) as functions of ordinal stimulus magnitude for [Experiment 1](#). The lines represent different stimulus sets: line length or tone loudness. Error bars are 95% confidence intervals calculated in the repeated-measures manner described by Loftus and Masson (1994), separately for the two between-subjects conditions



yielding a typical bow effect (e.g., Stewart et al., 2005) appears to be the within-subjects manipulation of set size. This manipulation results in more frequent presentations of center stimuli than others, across the entire experiment, which could explain the corresponding performance benefit. [Experiment 2](#) tested a potential confound to this presentation-frequency hypothesis present in [Experiment 1](#).

Experiment 2

In [Experiment 1](#), the two central stimuli were both presented *before* the others and were presented *more often* than the others. Either, or both, of these factors could be the cause of the disrupted bow effect observed for line length stimuli in [Experiment 1](#). In [Experiment 2](#), using only line lengths, we balanced the number of presentations per stimulus over the entire experiment. Thus, if the bow effect were to be disrupted in [Experiment 2](#), the results of [Experiment 1](#) could be explained by some stimuli being presented before others. Alternatively, if the typical bow effect were to reappear in [Experiment 2](#), the results could be explained by the differences in presentation frequency.

Method

We used the same procedure and stimuli as in the line length condition in [Experiment 1](#), with 21 new undergraduate participants from the University of Newcastle. The experiment was divided into three sections. Participants, however, were told of only the first two sections. In section 1, participants completed two blocks of 100 trials, each with just the central two stimuli ($N = 2$). In section 2, participants completed 1,000 trials in ten blocks, with all stimuli ($N = 8$). However, the central two stimuli in this section were presented only 50 times each, while the other stimuli appeared 150 times each. Thus, at the end of the first two sections, each stimulus had appeared exactly 150 times. Participants were not explicitly told that the presentation of certain stimuli would be reduced in the second section. The third section reverted to five blocks of 80 trials each, with all eight stimuli appearing equally often. Altogether, the three sections took participants approximately 2 h, which they completed in a single testing session. Throughout the experiment, 1-min breaks were provided regularly. Participants were also given a single, extended 5-min break at the halfway point.

Results

The data from 2 participants were removed from analysis due to low accuracy (<25% correct across the entire experiment, which was much lower than that of the other participants in the experiment, $M = 48%$). Mean accuracy and mean RT for the

$N = 2$ condition were 80% and 0.99 s, respectively. [Figure 3](#) shows mean accuracy and mean RT for the $N = 8$ condition. The final section of the experiment, during which each stimulus was presented equally often, is shown in black, and the unequal-frequency (middle) section is shown in gray. Accuracy was poorer and mean RT longer for the central two stimuli than for all others in the critical third section of the experiment (when all stimuli were presented equally often).

Repeated measures ANOVAs on accuracy and mean RT from the final phase of the experiment confirmed main effects of stimulus magnitude [accuracy, $F(7, 126) = 9.37, p < .001$; RT, $F(7, 126) = 4.56, p < .001$]. As would be expected in a standard bow effect, linear contrasts showed that the mean accuracy for stimuli 2 and 7 ($M = 44%$) was significantly greater than the mean accuracy for stimuli 4 and 5 ($M = 31%$), $F(1, 126) = 17.9, p < .001$, and mean RT was significantly shorter ($M_{2/7} = 1.23$ s, $M_{4/5} = 1.35$ s), $F(1, 126) = 11.2, p = .001$. Furthermore, this effect was evident even when middle stimuli were compared with their immediate neighbors [stimuli 3 and 6; $M_{acc} = 45%$, $F(1, 126) = 21.2, p < .001$; $M_{rt} = 1.24$, $F(1, 126) = 8.59, p = .004$].

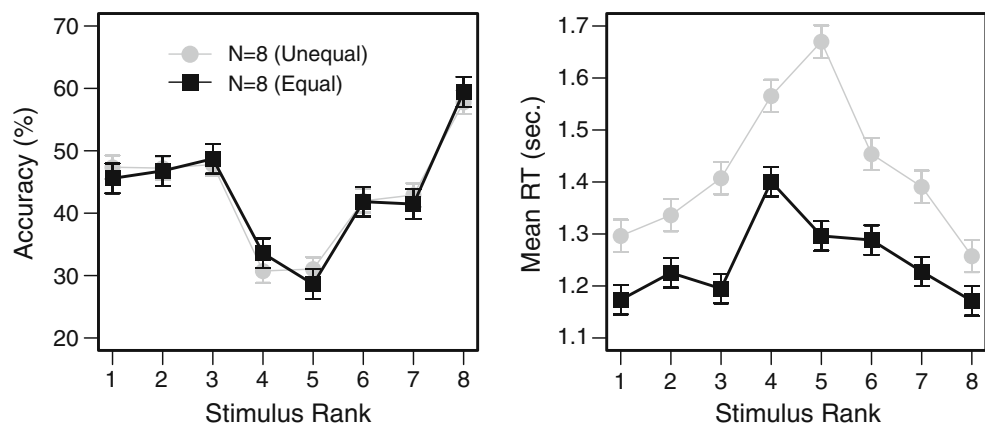
[Figure 3](#) shows very little difference in accuracy between section 2 and section 3 in [Experiment 2](#), $F < 1$. Our hypothesis about overrepresentation of certain stimuli would suggest the occurrence of a W-shape for accuracy in early trials of section 2, due to the overrepresentation of the two central stimuli in section 1; however, we do not see such a pattern, likely due to a lack of power. There was, however, a clear reduction in RT in section 3, relative to section 2 [paired samples t -test: $t(7) = 5.54, p < .001$], which was likely due to a general improvement by practice, with participants trading the possibility of improved accuracy for improvements in speed.

Discussion

When the number of presentations per stimulus was manipulated so that participants were eventually exposed to an equal number of presentations of all stimuli, performance on the central stimuli was poorer than that on all the other stimuli, as in a standard bow effect, for both accuracy and RT. This suggests that it is the overrepresentation of certain stimuli, not presentation order, that leads to improvement in performance for those stimuli.

[Experiment 2](#) also provided an estimate of the difference that might be expected between the central pair of stimuli (4 and 5) and the next-to-edge stimuli (2 and 7), when a standard bow effect is observed. In particular, the central stimuli were correctly identified 13% less often than the next-to-edge pair. A power analysis showed that, if such a difference had been present in the line length condition in [Experiment 1](#), the corresponding linear contrast would have detected a signif-

Fig. 3 Accuracy and mean response times (RTs) as functions of ordinal stimulus magnitude for Experiment 2. Each line represents a different section of the experiment. Gray lines represent section 2 (unequal presentation), and black lines represent section 3 (equal presentation). Error bars are as in Fig. 2



icant difference with almost perfect power (>99%). Indeed, even if the true difference was only half as large (6.5%), the power would still have been close to perfect (>99%). This suggests that the combined null results for both accuracy and RT for the line length condition in Experiment 1 were very unlikely to have been caused by a lack of statistical power.

Experiment 3

Experiment 1 confirmed a prediction arising from Dodds et al.'s (2011) investigation of learning effects in absolute identification: If the bow effect is disrupted in within-subjects designs because of differential stimulus-specific practice, this effect should be modulated by the susceptibility of the stimulus type to learning. As was predicted, Experiment 1 showed a standard bow effect when tone loudness (which does not show strong learning) was judged, but not when line length (which does show a strong learning effect) was judged. A necessary weakness of such an experiment is the comparison of performance on different stimulus types, since these will often have different pairwise discriminability and other characteristics. Experiment 3 remedied this weakness by comparing results for two conditions that both used the same stimulus type: tones varying in frequency. The conditions differed only in the order in which different set sizes were practiced, testing the prediction that one order would enhance the advantage for the central pair and the other would reduce it (i.e., it would disrupts the bow effect).

This design enabled a confirmation of the findings of Experiment 2 and supported a direct comparison of the bow effects from the two conditions, avoiding the problem of confirming a null hypothesis. It also tested a subtler version of a prediction from Dodds et al.'s (2011) work. Dodds et al. showed that learning effects for tones varying in frequency were smaller than those for line length but larger than those for tones varying in loudness. Thus, if the bow

effect is disrupted by practice, this disruption should also appear for tones varying in frequency, but the disruption should be less marked than for line lengths.

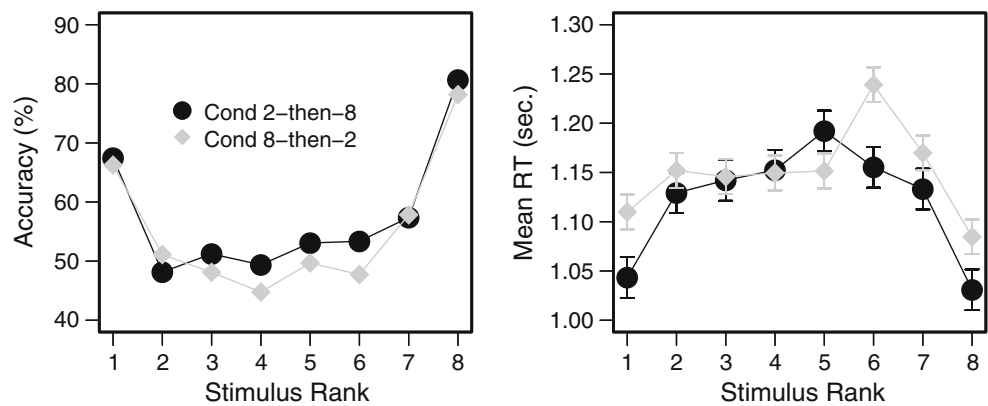
Method

We used the same procedure as in Experiment 1, with 25 participants randomly assigned to one of two conditions that differed only in presentation order. Participants were given stimulus sets of either set size $N = 2$ and then $N = 8$ or the reverse, which we will refer to as the 2-then-8 and 8-then-2 conditions (with 13 and 12 participants, respectively). Each participant took part in 20 blocks of practice, over 2 h. Regular 1-min breaks were provided between blocks, with a compulsory 5-min break after approximately 1 h. Each block consisted of 80 trials. The $N = 2$ condition was practiced for 5 blocks, and the $N = 8$ condition for 15 blocks. In the 2-then-8 condition, the $N = 2$ condition was practiced first, followed by 15 blocks of the $N = 8$ condition. This was reversed in the 8-then-2 condition. The stimuli were eight 1-s 67-dB tones with frequencies taken from Stewart et al. (2005; wide spaced condition): 672, 752.64, 842.96, 944.11, 1057.11, 1,184.29, 1326.41, and 1485.58 Hz. Tones were generated as pure sine waves using MATLAB 2009a and were presented through Sony headphones (model MDR-NC6), with the noise-canceling function turned off.

Results

As before, the data from the $N = 2$ condition showed high accuracy and short mean RTs, both for the 2-then-8 condition (98% and 0.65 s) and for the 8-then-2 condition (98% and 0.58 s). Figure 4 shows mean accuracy and RT as functions of stimulus rank for the $N = 8$ conditions. We extended the linear interaction contrasts employed in Experiment 1 to directly test the difference between conditions by comparing the linear contrast from the two

Fig. 4 Accuracy and response times (RTs) as a function of stimulus magnitude for Experiment 3. Note that there are two conditions: Some participants experienced the $N = 2$ set size first and then $N = 8$ (condition 2-then-8), and others experienced the reverse order (condition 8-then-2). Error bars are as in Fig. 2



groups (against the appropriate pooled error term from a mixed ANOVA with factors of stimulus rank and experimental condition). This contrast showed that the difference between the mean accuracy for stimuli 2 and 7 and for stimuli 4 and 5 was significantly larger in the 8-then-2 condition than in the 2-then-8 condition, $F(1, 161) = 3.86$, $p = .03$. The corresponding test for the RT data was not significant ($p = .07$). Separate linear contrasts for each condition (as in the analysis of Experiment 1) further confirmed these trends for the accuracy data: In the 2-then-8 condition, the mean accuracy for stimuli 2 and 7 was not significantly different from that for stimuli 4 and 5 ($p = .20$), but this comparison was significantly different in the 8-then-2 condition, $F(1, 77) = 10.3$, $p < .001$. The corresponding tests for the mean RT data did not reach significance.

Discussion

Experiment 3 demonstrated a significant difference in the bow effects observed in the 8-then-2 versus the 2-then-8 condition. The bow effect for response accuracy (but not RT) was deeper in the 8-then-2 condition than in the 2-then-8 condition, which is consistent with the hypothesis that the bow effect was disrupted (flattened) by pre-exposure to the central stimuli in the 2-then-8 condition. The null effects for the RT data are surprising, given the large RT differences in the corresponding test in Experiment 1. This null effect might be due to low power, because the variability we observed in the RT data was much larger than that for the accuracy data (e.g., see the very slow mean RT peaks for some stimuli in Fig. 4). Alternatively, the null effect might have been caused by a speed-accuracy trade-off. If performance is improved for frequently presented stimuli, participants can choose to exhibit that improvement either as improved decision accuracy or as improved RT (or both). Such trade-offs are complex and often accompany improved performance in simple decision tasks (see, e.g., Dutilh, Vandekerckhove, Tuerlinckx, & Wagenmakers, 2009).

Response biases

In most categorization paradigms, increasing the presentation frequency of some stimuli relative to others alters participants' *a priori* response biases in such a way as to improve performance for the frequently presented stimuli (for a general review, see Healy & Kubovy, 1981; or see Petrov & Anderson, 2005, for an example in absolute identification). It is possible that the tendency for our participants to demonstrate improved accuracy for stimuli in the center of the range is due to such response biases. Here, we attempt to rule this explanation out, leaving open the possibility that practice improved discrimination performance itself.

The results from Experiment 1 provide an initial insight into this issue. Experiment 1 provided a direct comparison between the identification of tone loudness and line length. Performance was improved for over-presented lines, but not for over-presented tones, and one would expect that if the results were caused solely by response biases toward over-presented stimuli, performance should have increased for both stimulus modalities. However, there are difficulties comparing such results across different stimulus modalities—in our case, because the dimensions studied do not have identical Weber fractions (i.e., the minimum separation required between adjacent stimuli so that each are equally perceptually discriminable).

To address this issue, we further analyzed the results from Experiment 3, which were ideal for examining issues of response bias. In Experiment 3, both conditions used the same stimuli, so the pairwise discriminability of the stimuli was identical by design. Additionally, Experiment 3 did not include any blocks with unequal stimulus presentation frequencies, so participants were not given any a priori reason to employ unequal response bias. Figure 5 shows the marginal response probability—that is, the probability that each stimulus label is used as a response. Response probability typically demonstrates similar phenomena (including the bow effect) for accuracy and RT (Petrov &

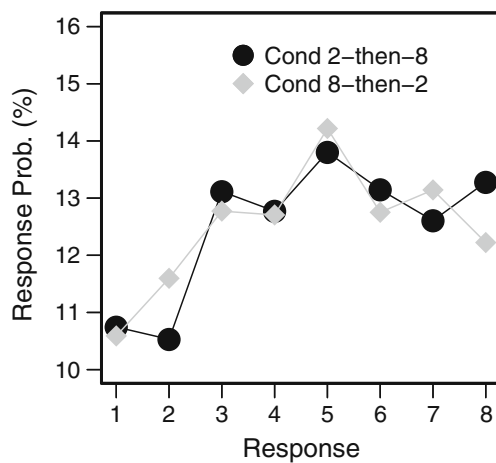


Fig. 5 Response probability for [Experiment 3](#)

Anderson, 2005) but allows examination of bias. Given that the stimuli were presented equally often, increased marginal response probability for a stimulus would indicate a response bias toward that stimulus on the part of the observer. For consistency with earlier analyses, and because of the similarity between patterns in response probability and patterns in response accuracy and RT, we used the same inferential analyses for these data as those used above. Linear contrasts comparing the central stimuli for both conditions with the next-to-edge stimuli (2 and 7), using the appropriate mixed ANOVA error term, showed that there were no significant differences between conditions in the amount of bow observed in response probability ($p = .22$). A power analysis showed that a relatively small difference between conditions (e.g., a difference in the depth of the bow effect for the two conditions of just 2% in marginal probability) would have been detected with a probability of 76%, using a Type I error rate of .05. These results suggest that differences in response biases are unlikely to have been a contributing factor to the significant improvements in performance found for participants in the 2-then-8 condition.

General discussion

[Experiment 1](#) and [2](#) indicated that the bow effect can be disrupted by design factors, such as within-subjects manipulations and stimulus presentation probabilities, at least when the stimuli are line lengths. In [Experiment 1](#), we found that, following the presentation of the $N = 2$ condition, the stimuli in the middle of the range were identified more accurately, as compared with the surrounding stimuli for line lengths, but not for tone loudnesses. [Experiment 2](#) indicated that the disrupted bow effect for line lengths in [Experiment 1](#) was due to unequal stimulus

presentation frequency across the experiment, induced by a standard within-subjects manipulation of set size. [Experiment 3](#) suggested that the modulation of the bow effect depends on stimulus modality, as would be expected from Dodds et al.'s (2011) results.

Previous results on unequal stimulus presentation frequency

Petrov and Anderson (2005) manipulated the presentation frequency of stimuli (dots varying in separation) in an absolute identification experiment and found that correct responses were more likely for stimuli presented more frequently. However, Petrov and Anderson's presentation frequency manipulation was counterbalanced over short time scales, such that long-term learning effects (as we observed) would not be expected in longer time-scale averages. Cuddy (1968, 1970) found that training participants on just one particular stimulus, out of a set of nine tones varying in frequency, resulted in improvement for the entire set. However, this result was limited only to highly trained musicians; regular participants showed little improvement. In the work most similar to our own, Cuddy et al. (1973) found that regular participants were able to greatly improve their performance when trained by presenting three tones out of a set of nine more frequently than others. However, Chase et al. (1982) replicated Cuddy et al.'s experiment and found very small improvements (8%, as opposed to Cuddy et al.'s 50% improvement).

Our experiments extended these earlier findings in several ways. First, we examined performance in conditions where all stimuli were presented equally often (after having manipulated presentation frequency earlier). This more faithfully represents performance in standard absolute identification tasks. Cuddy (1968, 1970) used a similar procedure but found changes in performance only for musically trained participants. Second, we demonstrated effects of the presentation frequency for stimuli across the entire experiment. That is, stimuli that were presented more often over the entire experiment were identified more accurately, even when every block of trials contained equal presentation frequencies for all stimuli in the block ([Experiments 1](#) and [3](#)). This manipulation mirrors standard within-subjects manipulations of stimulus set size and avoids creating a situation that rewards response biases in favor of more frequent stimuli. Third, our experiments systematically examined different stimulus types, which have predictable and large effects on results.

Absolute identification can be thought of as a variant of categorization, in which each stimulus defines its own category. In standard categorization tasks, where many different stimuli are mapped to the same response (a single category), there have been many investigations of the effect

of unequal stimulus presentation frequency, with results that are consistent with ours. For example, Nosofsky (1988) found that frequently presented category exemplars were classified more accurately and were rated as more typical of the category than were less frequently presented exemplars. This effect generalized to unseen exemplars that were very similar to the more frequently presented exemplars, but not to less similar ones, analogous to our stimulus-specific findings.

Theoretical implications

Our results are indicative of long-term learning. This adds weight to recent findings that practice can improve performance in absolute identification (e.g., Rouder et al., 2004) and that these effects are larger for line length and tone frequency than for tone loudness (Dodds et al., 2011). An additional theoretical implication from our results is that learning is stimulus-specific. For example, suppose that learning effects were, instead, driven by time-on-task (or the total number of absolute identification decisions). Under that assumption, additional presentations of some stimuli would not lead to improved performance for those particular stimuli above others, contrary to our results. Furthermore, improved performance for frequently presented stimuli was observed to last for hours or days, long after uniform presentation frequencies were re-established. This suggests that theoretical accounts of improved performance for frequently presented stimuli based on short-term biasing mechanisms (e.g., Petrov & Anderson's, 2005, ANCHOR model) are not sufficient.

Although current theories for absolute identification do not include mechanisms by which practice can improve performance, there are several obvious candidate mechanisms. Some of these candidates seem better suited to meeting the challenges described above than do others. For example, exemplar-based models (e.g., Kent & Lamberts, 2005) naturally predict that increased exposure to some stimuli enriches the representation of those stimuli above others. Kent (2005, Chap. 9) suggested a precise mechanism that would have this effect: a particular relationship between the number of exemplars and the associated psychological distances.

The selective attention component of both the SAMBA (Brown, Marley, Donkin, & Heathcote, 2008) and ANCHOR (Petrov & Anderson, 2005) models explain the phenomenon known as *contrast* (the tendency for a response on the current trial to be biased away from those presented more than one trial previously) by assuming that recently presented stimuli have privileged representations in memory; psychological space effectively expands around these representations, increasing their distances from other stimulus representations. Such mechanisms might

naturally accommodate improved performance due to extra stimulus presentations, because extra presentations of a stimulus usually lead to a higher probability that that stimulus was presented in the recent past. However, both SAMBA and ANCHOR assume that these changes are very short-lived (lasting only a few trials, or perhaps on even shorter time scales; see Matthews & Stewart, 2009). This assumption would have to be altered to allow the contrast mechanisms to explain our results.

One further theoretical constraint—the observed differences between stimulus types—has interesting implications for these possible accounts based on contrast mechanisms. Standard contrast effects occur for all stimulus types (e.g., Ward & Lockhead, 1971), so it is not immediately clear why a contrast mechanism (in SAMBA or ANCHOR) should allow for disrupted bow effects for line length and tone frequency, but not for tone loudness. An intriguing possibility was raised by Dodds et al.'s (2011) finding that, when extended practice improves performance, the standard contrast effect disappears. It is possible that extra practice with frequently presented stimuli alters the contrast mechanism, to the extent that learning occurs, by fixing in place the expanded psychological representation.

It is a matter for future research to identify why this might occur for some stimulus sets (such as line lengths and tone frequencies) but not for others (such as tone loudness). This account might be tested in future work by examining contrast effects in paradigms that, as in ours, involve differential stimulus presentation frequencies. Existing experiments, including ours, are not suitable for such analyses, because the frequently presented stimuli were always the central stimuli and contrast effects are not observed for those stimuli (Ward & Lockhead, 1971).

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