Incidental Learning in Music Reading: The Music Contingency Learning Task

Claudia Iorio, Iva Šaban, Bénédicte Poulin-Charronnat, and James R. Schmidt

LEAD - CNRS UMR5022, Université Bourgogne Franche-Comté

Correspondence concerning this article can be addressed to Claudia Iorio, Université Bourgogne Franche-Comté, LEAD - CNRS UMR5022, Pôle AAFE, 11 Esplanade Erasme, 21000 Dijon, France.

E-mail: claudia_iorio@etu.u-bourgogne.fr.

Abstract

The present report investigated whether nonmusicians can incidentally learn musical skills needed for sight-reading. On each trial, participants identified a note name written inside of a note on the musical staff. In Experiment 1, each note was presented frequently with the congruent note name (e.g., “do” with the note for “do”) and rarely with the incongruent names (e.g., “do” with the note for “fa”). With or without deliberate learning instructions, a robust contingency learning effect was observed: faster responses for congruent trials compared to incongruent trials. Participants also explicitly identified the meaning of the note positions more accurately than chance. Experiment 2 ruled out the potential influence of preexisting knowledge on the contingency learning effect by presenting notes most often with an incongruent note name. Robust learning was again observed, suggesting that participants acquired sufficient knowledge of musical notation to produce automatic influences on behavior (e.g., akin to the interference effect previously found in skilled musicians). A congruency effect was additionally observed in Experiment 2, however. Experiment 3 further explored to what extent this congruency effect might be due to prior music knowledge and/or spatial stimulus-response compatibility between note and response locations (analogous to the SMARC effect). Overall, our results open up new avenues for investigating the incidental learning of complex material, musical or otherwise, and for reinforcing learning even further.

Introduction

Music is a complex ability that involves a range of different cognitive processes (e.g., learning, perception, production; Pearce & Rohrmeier, 2012). Not surprisingly, then, during traditional music instruction a wide range of skills need to be learned, such as familiarization with the instrument and musical theory. While traditional training is well adapted to the acquisition of many of these skills, some skills tend to fall behind. One important musical skill, which takes a considerable amount of time to acquire, is *sight-reading* ability. Sight reading refers to the ability to look at a new piece of music for the first time and play it while reading (e.g., without having to memorize or practice the piece beforehand). Typically, explicit tutoring and deliberate practice are used to teach and improve sigh-reading abilities (Ericsson et al., 1993; Ericsson & Harwell, 2019; Hébert & Cuddy, 2006; Lehmann, 1997; Mills & McPherson, 2006; Mishra, 2014). However, even after many years of studying sight reading, these skills are still lacking among many music students (Hargreaves, 1986; Mills & McPherson, 2006; Scripp, 1995). In this paper, we will introduce a novel approach to aiding with sight-reading training, intended as a potential supplement to traditional music instruction. As will be discussed below, our new approach aims to leverage the benefits of incidental learning procedures (e.g., very rapid learning), rather than deliberate practice, to facilitate learning. We note in advance that the current research focuses on one component of sight reading, namely, responding to the note position stimuli with the corresponding actions.

The difficulty of sight reading

Part of the difficulty in learning to sight read may be due to the complexity of the task. Indeed, sight reading is a complex skill that relies on different factors (Kopiez & In Lee, 2006, 2008; Lehmann & Kopiez, 2009) and it involves different processes based on the coding of visual information, motor responses, and visuomotor integration (Gudmundsdottir, 2010). Although the terms "music reading" and "sight reading" are often used interchangeably, the first can be considered as a prerequisite of the second. That is, while music reading mostly refers to the act of reading and decoding musical notation from music sheets, sight reading refers to the complex skill that involves different components such as reading and decoding musical notation (i.e., music reading) and performing (playing) the music directly while reading, that is, without prior practice (Waters et al., 1997; Wolf, 1976). Therefore, it has been defined as a demanding transcription task (Sloboda, 1982, 1985).

Schön et al. (2001, 2002) hypothesized that at least three types of translations are involved when musicians read music: singing-like (visual to auditory transcoding), playing-like (visual to motor transcoding), and note-naming-like (visual to verbal transcoding). Accordingly, Stewart et al. (2003) proposed that musicians automatically generate a sensorimotor translation of a spatial code (written music) into a series of motor responses (keypresses). Reading music requires analyzing visual information. In particular, it is necessary to decode the spatial position of the notes on the music staff. While the horizontal location carries information about the duration, the vertical location indicates the pitch (Sergent et al., 1992). Previous research suggested that timing and pitch information (i.e., the horizontal and the vertical positions of the notes on the staff) are perceived and coded separately (Schön et al., 2001, 2002; Stanzione et al., 1990). Here, we focused on the encoding of the vertical position of the notes, a process that has been investigated in some prior research. Sloboda (1976), for instance, compared the performance in a recall task between musicians and nonmusicians. His results showed that nonmusicians were less accurate in recalling a sequence of notes than musicians, suggesting that naming the visual stimulus can be the first step to encode visual material. Perea et al. (2013) further provided evidence that coding the position of the notes relies on more than just visualization. They used a same/different task, in which participants were asked to judge the similarity between two musical sequences. Nonmusicians had worse performance compared to musicians, suggesting that note position coding is quite approximate at early stages of processing compared to more experienced readers.

In addition to being a complex task, focal study of sight-reading skills is atypical (Hardy, 1998). Instead, a music practice often involves a focus on mastering music scores, frequently with blocked repetition (Barry, 1992, 2007; Maynard, 2006; Rohwer & Polk, 2006), and a music education often focuses on music theory, instrument technique, etc. These are all important skills as well, of course, but sight reading, though a valued skill, is often ignored. One difficulty in teaching sight reading is that students need to automatize the translation of the notes from the page to the actions on the instrument, and for this an enormous amount of novel materials (e.g., music scores) would be needed (Hardy, 1998). For instance, a familiar musical score that the student has already seen and played before is not very useful in practicing the skill of seeing new, unfamiliar material and rapidly playing it while reading.

Automaticity and the Musical Stroop

Though complex, many musicians will eventually automatize their sight-reading skills. Automatizing particular components of a skill is likely to be crucial to learning complex skills and it is often the key for acquiring expertise. For instance, expert chess players are incredible good at reading the board positions, mostly because they can easily and automatically retrieve encoded positions of the chess pieces on the board after years of looking at chessboard configurations (e.g., Saariluoma, 1994).

Similarly, musicians can easily and automatically read music notation. A number of studies using *musical Stroop procedures* (Grégoire et al. 2013; see also, Crump et al., 2012; Drost et al., 2005; Stewart, 2005; Zakay & Glicksohn, 1985, for other musical Stroop procedures), comparing performance between musicians and nonmusicians, provided evidence to support the view of music reading being an automatic process for musicians.

Some authors (Grégoire et al., 2013, 2014b, 2014a, 2015, 2019) proposed that this automaticity in musicians may be due by the learned associations between note-positions and note-names in musicians. In musical Stroop tasks, participants are presented with a note on the musical staff with a note-name written inside of it, as illustrated in Figure 1. On congruent trials, the meaning of the note-position (task irrelevant) and the note-name (task relevant) match (e.g., "ré" written inside of the note for "ré"). On incongruent trials, the meaning of the note-position and note-name mismatch (e.g., "mi" written inside the note for "la"). Analogous to color-word Stroop tasks (see MacLeod, 1991; MacLeod & MacDonald, 2000, for nonmusical Stroop procedures), musical Stroop procedures measure the automatic influences of previously learned associations between note positions and their note names on reading simple written note names. Although the task was to ignore the note-position (i.e., where the note was presented on the musical staff) and simply respond to the note-name written inside of it, musicians processed the note-position and this had an impact on note-name reading, as indicated by slower and less accurate responses to incongruent trials relative to congruent trials. This phenomenon has been termed the Musical Stroop Effect . Contrary to the Musical Stroop Effect observed in musicians, nonmusicians responded just as quickly to incongruent as to congruent name-note pairs (i.e., no Musical Stroop Effect). This is unsurprising, as nonmusicians have not learned the meaning (or "translation") of the note positions (i.e., the association between the note-position and note-name) in the first place and are simply reading the written note names (without any possible influence of the note positions).

[Figure 1]

Previous work with musical Stroop procedures studied the influence of the knowledge acquired before participants entered the laboratory. That is, past work has studied the influence of music knowledge that expert musicians already possessed. Our goal is exactly the opposite: to train nonmusicians to acquire music knowledge that they do not yet possess. Unlike previous research using musical Stroop procedure, here we want to demonstrate that by using an incidental training procedure (discussed shortly) nonmusicians can rapidly acquire such automatic influences of music reading akin to the Musical Stroop Effect previously found in skilled musicians. That is, using an incidental training, nonmusicians should show a Musical Stroop Effect, even after very brief training, supporting the idea of a rapid and incidental acquisition of a complex subskill (i.e., music sight-reading skills). We note that although the term "automaticity" has been used to describe many different features of learning (e.g., the need for awareness, attentional and cognitive resource needs, the stimulus- or goal-driven nature of learning; Moors & De Houwer, 2006), it is certainly not our goal to argue that the learning we observe is automatic in all of these senses. Here, we refer to "automaticity" to describe the "automatic" impact of task-irrelevant note positions on performance of another task (i.e., in the same sense that a color-word produces "automatic" influences on color naming in the traditional Stroop procedure; Augustinova & Ferrand, 2014). That is, we ask whether it is possible that nonmusicians can rapidly acquire similar automatic influences of sight-reading knowledge on behavior as that observed in the Musical Stroop Effect with musicians that have more extensive musical training.

Incidental contingency learning

Our research applies knowledge from cognitive psychology research, and more specifically from work on human contingency learning. Contingency learning refers to the basic human ability to learn the relationship between two or more events in the environment (e.g., Event B tends to follow Event A, making Event A a predictive cue for Event B; for reviews, see MacLeod, 2019; Schmidt, 2021). In an incidental learning procedure, the participant is not given the explicit goal to learn a regularity. Rather, the participant is asked to engage in one task (e.g., identify a target stimulus), but a regularity exists in the task (e.g., an informative secondary stimulus or a predictable sequence of stimuli) that, if learned, allows for anticipation of the likely response. We want to specify that here we used the term “incidental” because we refer to the acquisition of new information without the goal to learn (Kerka, 2000). We note that a separate (albeit correlated) issue from the incidental (vs. deliberate) nature of learning is whether participants are aware of what they have learned. For decades, there has been a heated debate about the nature (implicit or explicit) of the knowledge acquired through “implicit” or incidental learning (Cleeremans et al., 1998). Although we will take some measures of awareness in the present report, it is not our goal to discuss this debate in any detail.

Previous research suggests that learning the relationship between events occurs automatically, that is, people are sensitive to frequency of occurrence information (Zacks & Hasher, 2002) and to probabilistic patterns (Kelly & Martin, 1994), and simply attending to events is enough for activating learning of the co-occurrence of these events. Furthermore, people are not just sensitive to the co-occurrences around them, but they can learn this information and use it in a variety of tasks (e.g., in language acquisition; see Aslin et al., 1998; Saffran, Aslin, et al., 1996; Saffran et al., 1997; Saffran, Newport, et al., 1996). We note that we not only have a natural sensitivity in detecting the frequency and probability of events, but this sort of incidental learning can also occur very quickly. Indeed, many learning procedures, such as sequence learning (Nissen & Bullemer, 1987; Turk-Browne et al., 2005), artificial-grammar learning (Reber, 1967; for a review, see Pothos, 2007), the Hebb digits task (McKelvie, 1987; Oberauer et al., 2015; Vachon et al., 2018), and hidden covariation detection (Lewicki, 1985, 1986; Lewicki et al., 1992), produce a rapid learning effect.

We took particular inspiration from the color-word contingency learning procedure of Schmidt et al. (2007; for related learning procedures, see Carlson & Flowers, 1996; Miller, 1987; Mordkoff & Halterman, 2008; Musen & Squire, 1993). Similar to the color-word

Stroop procedure (Stroop, 1935), participants are asked to respond to the color of words by pressing a corresponding button, while ignoring the words. However, the words are neutral (unlike the Stroop) and to induce the acquisition of the contingencies, the words are presented most often in one color (e.g., “move” most often in blue) and rarely in the other colors (“move” rarely in red). Although participants are not informed of the contingencies between colors and words and often do not become aware of the manipulation, they respond quicker and more accurately to *high-contingency* trials, where the word is presented with the expected color (e.g., “move” in blue), than to *low-contingency* trials, where the word is presented with an unexpected color (e.g., “move” in red; Schmidt & De Houwer, 2012b). This contingency learning effect can be explained by the greater familiarization with frequently-presented high contingency trials relative to the rarely-presented low contingency trials (Schmidt & De Houwer, 2016a). The learned regularities allow participants to anticipate the responses based on the presented words (Schmidt et al., 2007), thereby facilitating performance if the anticipated high contingency response is, in fact, required. Interestingly, this effect is extremely robust, with essentially all participants showing a numerical effect, and it is acquired almost instantaneously from the start of acquisition (Lin & MacLeod, 2018; Schmidt et al., 2010; Schmidt & De Houwer, 2016).

A major part of the reason *why* learning is so rapid in this type of incidental learning procedure is probably due to the fact that participants see a very large number of trials in which a stimulus is presented and they rapidly respond to it. In other words, such procedures allow participants to cram substantial amounts of practice with novel stimuli into a very short time period (e.g., several hundred trials in a 10-15 min). As previously indicated, this is one of the difficulties with training sight reading: traditional practice does not involve seeing a large amount of novel materials in a short time period. In any case, given how rapid and easy it is to learn with this type of incidental learning procedure, a similar approach might be equally effective in the automatization of visuomotor integration for sight-reading performance. In particular, we hypothesize that participants may be able to acquire the associations between note positions and note names, along with the corresponding actions (i.e., which note to play) with similar efficiency. Indeed, learning in this type of incidental learning procedure primarily involves the learning of the association between the task-irrelevant stimulus (in the experiments to be described shortly: the note position) and the response to make (e.g., the key to press on a keyboard), or stimulus-response learning (Geukes et al., 2019; Miller, 1987; Schmidt et al., 2007; Schmidt & De Houwer, 2012a, 2016a). This is particularly interesting in the context of sight reading, where automatization of the association between the note position and the action to perform on the instrument is needed. Our studies will therefore follow a similar logic as the color-word contingency learning described above, but with musical materials.

We note that incidental or implicit learning tasks have been used to investigate the learning of music materials in prior work. However, this prior work involved the learning of music that we listen to. For instance, many authors studied the implicit acquisition sequence information linked to melody (Saffran et al., 1999, 2000; Tillmann & Poulin-Charronnat, 2010), timbre (Bigand et al., 1998), harmony (Bly et al., 2009; Loui et al., 2009; Rohrmeier & Cross, 2009), and rhythm (Brandon et al., 2012; Salidis, 2001; Schultz et al., 2013; Tillmann et al., 2011). In particular, the participants listen to music sequences and the learning of the structures underlying these sequences is then tested. However, the role of implicit or even incidental procedures in acquiring music skills useful for performance (e.g., how to play) is not clear yet.

The current research

Our adapted musical contingency-learning procedure is a hybridization of the above-mentioned musical Stroop and color-word contingency learning procedures. Our task follows the same structure of the musical Stroop task of Grégoire et al. (2013), in which a note is presented on a musical staff, which we will refer to as the note-position or simply the note. Written inside the note is the name of a note (e.g., "mi"), or note-name. Critically, as illustrated in Figure 1, the note-name can be either congruent with the position of the note (e.g., "ré" written inside the note for "ré") or incongruent (e.g., "mi" written inside of the note for "la"). However, to induce the learning of the note-name/note-position associations, our task follows the same logic as the color-word contingency learning procedure of Schmidt et al. (2007). In Experiment 1, each note was presented much more frequently with the congruent note-name (18 of 24 presentations, or 75%) than with any of the incongruent note-names (6 of 24 presentations, or 25%). For instance, the note-position for "do" was presented much more often with the note-name "do" than with the note-names "ré", "mi", and so on.

Participants simply respond as quickly and accurately as possible to the task-relevant stimulus (note-name) while ignoring the task-irrelevant stimulus (note-position). Critically, the note-position is informative in our adaptation (i.e., the note-position is predictive of the probable correct response to the note-name). Thus, learning could occur incidentally, and nonmusicians could learn the keyboard actions to perform for the note positions via the contingencies between the note-positions and responses to the note-names. We note that we use an imperfect contingency manipulation (i.e., not all trials are congruent) because this allows us to measure learning while it is occurring (i.e., by contrasting performance on high- and low-contingency trials; see Discussion for further remarks on this point).

Previously in the introduction, music sight-reading has been defined as a transcriptional task, where music symbols are translated into motoric actions (Sloboda, 1982, 1985). To study closely the acquisition of this task, we required our participants to respond to the note-names by pressing an assigned key on a computer keyboard. This type of arbitrary stimulus-response assignment is similar to the learning of playing a new musical instrument, where, for instance, a novice musician must learn which keys to press on a piano keyboard for each note.

It was anticipated that our incidental learning procedure would allow for rapid automatization of sight-reading skills, primarily because participants can experience a relatively large number of randomized trials with the congruent correspondences between note-positions and the keyboard responses to note-names. However, this is not to say that the deliberate intention to learn will not aid learning further. Schmidt and De Houwer (2012a, 2012d) compared the performance in the color-word contingency learning procedure between a deliberate learning group (which was informed of the contingencies present in the paradigm) and an incidental one (which was not informed of the contingencies). Their results showed better performance for the deliberate learning group, suggesting that intentionality plays a role in learning the contingencies (for a similar result in sequence learning, see Destrebecqz, 2004). Therefore, to assess the role of intentionality during learning, in Experiment 1, participants were divided into a deliberate learning group, instructed to pay attention to the contingencies, and an incidental learning group, who received no instructions about the presence of contingencies. It was hypothesized that, most critically, even the incidental learning group would show evidence of learning. However, the deliberate learning group might show even more robust learning.

In addition, subjective and objective awareness measures (see Cheesman & Merikle, 1984) were taken to assess the verbalizable knowledge of the contingencies acquired by participants. *Subjective awareness* is measured by simply asking participants whether they noticed the contingent regularities. *Objective awareness* is measured by asking participants to forced-choice guess the “name” of each note-position, with awareness indicated by above-chance guessing. The objective awareness test also serves as a “test” phase of verbalizable knowledge of the meaning of the note positions.

To summarize, we hypothesized that the incidental contingency learning procedure will help nonmusicians to easily learn the visuomotor translation of music symbols. However, based on previous research (Schmidt & De Houwer, 2012a, 2012d), it is expected that the deliberate intention to learn can help learning even further. Moreover, in a long-term perspective, this research aims to provide the starting point to create a tool that allows nonmusicians (or even experienced musicians) to learn (or improve) sight-reading abilities.

Pilot study

In the interest of full disclosure, we note that we initially conducted a pilot study with 41 participants (undergraduate psychology students from the University of Burgundy). The pilot was identical to Experiment 1 below, with two exceptions. First, there was no deliberate learning group (i.e., all participants learned incidentally). Second, the contingency manipulation was much weaker. Specifically, each note was presented only six times more frequently with the congruent note-name than any of the incongruent note-names (instead of 18 times more frequently in Experiment 1), meaning that congruent pairings occurred on only 50% of trials.

The resulting contingency effect was not significant in response times (RTs), $t(40) = 1.29$, $p = .205$, $d = -.201$, $BF_{10} = .364$, or errors, $t(40) = -1.32$, $p = .195$, $d = .206$, $BF_{10} = .377$, but the difference between low-contingency and high-contingency trials ($M_{\text{low-high}} = 8.28$, $SD = 41.1$) in RTs was encouraging. We thus strengthened the contingency manipulation in Experiment 1, as this should increase the size of the learning effect. For instance, Forrin and MacLeod (2018) showed that the magnitude of the color-word contingency effect is exponentially related to contingency strength. That is, the effect gets much larger the stronger the contingency manipulation is.

Thus, for the present study we decided to (a) increase the strength of the contingency manipulation to elicit a larger congruency effect, (b) increase the sample size for more statistical power, and (c) introduce a deliberate learning group to explore the role of intentionality in a musical notation acquisition context. Supplementary material on our pilot experiment can be obtained by following the link: https://osf.io/fzex7/.

Experiment 1

In Experiment 1, two main hypotheses are investigated: 1) Based on color-word contingency learning research, it is expected that after a very small amount of practice, nonmusicians should incidentally learn which note-name corresponds to which note-position, and should therefore respond faster to the high-contingency (or "congruent") pairings relative to the low-contingency (or "incongruent") pairings, and 2) after a short learning phase, both the participants in the deliberate and incidental learning groups will be able to explicitly read musical notation, performing above chance in the objective awareness test phase.

Method

Participants

We recruited 123 undergraduate psychology students at the University of Burgundy. The participants received course credits for their voluntary participation. Participants were randomly assigned to the deliberate and incidental learning groups. Sixty-two participants (deliberate learning group) were asked to focus on the contingencies occurring during the learning phase. The remaining 61 participants (incidental learning group) did not receive any instructions about the contingencies present in the task. Our inclusion criteria were not being a musician and not being able to read musical notation. These inclusion criteria were indicated in the recruitment advertisement. All the procedures were conducted in accordance with the Declaration of Helsinki. A written consent form was signed by all participants before beginning the study. Participants' anonymization was guaranteed.

Apparatus, Design, and Procedure

In addition to the contingency manipulation, we made some additional changes to the typical musical Stroop procedure to aid learning. First, the musical staff was presented in the center of the screen in one fixed position. In the original experiments of Grégoire et al. (2013), the location of the staff was pseudorandomly varied in the four corners of the screen to prevent iconic memory of the staff. For the present report, however, we were actively aiming to train participants to learn location-to-response correspondences (i.e., note-position to note-name correspondences), so a fixed staff location was deemed desirable. Additionally, the note-position was presented slightly in advance of the note-name. This was done because it is known that advanced presentation of predictive cues boosts learning (Schmidt & De Houwer, 2016), likely because this gives the cue a “head start” to influence identification of the target. Finally, we used manual (key press) responses rather than oral naming responses. This was done, in part, for convenience and, in part, because a less automatic response modality (i.e., arbitrary stimulus-key assignments are slower than simple reading) allows more time for the cue (note-position) to influence responding to the target (Forrin & MacLeod, 2017; Schmidt, 2018). Moreover, as already suggested in the introduction, arbitrary stimulus-key assignments are similar to the motoric response that novice musicians practice when learning to play an instrument.

The experiment was programmed and ran with E-Prime 2 (Psychology Software Tools, Pittsburgh, PA) and run on laptops with 1080p resolution. During the main parts of the experiment, participants responded with the Z-I keys on a standard AZERTY keyboard. The keys were labelled according to the sequence of the musical scale from the lower to upper position (i.e., fa, sol, la, si, do, ré, and mi, respectively). The “O” and “N” keys were additionally used to answer “Oui” (Yes) or “Non” (No) to the subjective awareness question, and the spacebar was used to begin each phase from the instruction screens.

For stimuli, we used the seven notes from one octave (excluding the repeated octave) but beginning from “fa” (F4) and ending at “mi” (E5), as illustrated in Figure 2. We selected notes from “fa” to “mi” simply to use notes that fit within the main treble staff (i.e., first to fourth space). For instance, the first “do” (C4) falls on one line below the staff and normally is marked with a small strikethrough to indicate the position, which was deemed undesirable. In French, the note names are “do,” “ré,” “mi,” “fa,” “sol,” “la,” and “si”, unlike in English where A-G letter names are typically used. All target stimuli were presented in black 30 pt. Courier New font on a white screen, unless otherwise noted.

[Figure 2]

The experiment involved five phases. The goal of the first two phases was to allow participants to practice and automatize the note name-to-key assignments before proceeding to the actual learning phase. Results for these phases are not analyzed. In these practice phases, participants were not presented with notes or the musical staff, but only the written note names. In the first of these phases, the trial started with a fixation cross (“+”) in the center of the screen for 500 ms. This was followed by one of the seven the French note-names (*fa*, *sol*, *la*, *si*, *do*, *ré*, or *mi*) presented in the center of the screen until the participant pressed the corresponding response key (no time limit). Following correct responses, the next trial began immediately. Following incorrect responses, the note-name changed color to red (255,0,0; or E-Prime/HTML “red”) and stayed on the screen until the participant pressed the correct key. During the entire trial, the seven key labels (*fa* through *mi*), corresponding to the keyboard response keys, were presented at the bottom of the screen in bold 18 pt. Courier New font with five spaces between each, *x*-axis centered and below the target (centered at 600 px. on the *y*-axis). No specific instructions were given on how to use the keyboard responses. Each of the seven note names was presented once per block in random order, with ten blocks total (70 trials). The second practice phase was identical in all aspects, except that the on-screen key reminder was removed, and participants were encouraged to try to respond from memory (though the keys on the keyboard remained labelled in case the participant was particularly lost).

After these two training phases, and to study whether differences occurred between deliberate and incidental leaning, we added an extra instruction screen before the learning phase for half of the participants (deliberate learning group), which instructed them about the contingency manipulation and asked them to try to learn the contingencies. The instructions were (translated from the French version):

> *Note: Each note will be presented more frequently with the correct note name and less frequently with the incorrect note names. Try to learn the note name for each note position.*

The following third phase was the main learning task used to assess learning in response times and errors. On each trial, participants were presented with the musical staff (see Figure 1), an image of 602 × 909 px. (squished slightly to 602 × 902 px. to better align notes with the staff), which remained centered on the screen throughout the whole trial. At the start of the trial, the empty staff was presented for 500 ms. The note (67 × 100 px.) was then added to the staff for 250 ms, *x*-axis centered at 800 px. and *y*-axis centered either on or between one of the lines for the given note-position (522, 482, 442, 402, 362, 322, or 282 px.). The note-name was then written inside the note and participants had 3000 ms to respond. The entire procedure for stimuli appearance during the learning phase is illustrated in Figure 3.

[Figure 3]

If the participant responded correctly, the next trial began immediately. If they responded incorrectly or failed to respond in 3000 ms, the note name was replaced with "XXX" in red for 500 ms. During the learning phase, there were two blocks of 168 trials (336 trials in total), each randomly ordered (without replacement) and there was no break between the two blocks. Each note was presented 18 times per block with the congruent note-name (e.g., “fa” in the note for fa) and once each with the remaining six note names (e.g., “fa” in the note for do). Thus, each congruent name-note pairing was more frequent (high contingency) than each incongruent name-note pairing (low contingency). The congruency (or contingency learning) effect was measured as the difference between low and high contingency RTs (and errors).

Following the main learning phase, we additionally collected contingency awareness data to assess the awareness of participants of the contingency manipulation in the final two phases. In particular, participants were assessed for both subjective and objective awareness (Cheesman & Merikle, 1984). Respectively, *subjective awareness* is defined as a participant’s ability to verbally describe their experience, while *objective awareness* is defined by a participant’s ability to discriminate (e.g., better-than-chance guessing) between experienced and unexperienced events. For the *subjective awareness* measure (i.e., the fourth phase), the on-screen instructions told participants (translated from French):

> *During the third part of this experiment, note names were written inside the notes. Each note was presented more frequently with one note name than the others. That is to say, one note was frequently presented with “do,” another frequently with “re,” etc. Did you notice these regularities?*

Participants could respond “yes” or “no” with a key press.

Directly after, we introduced the *objective awareness* measure test phase (i.e., the fifth and final phase) as a more explicit test of verbalizable knowledge to (a) test whether the association between note-position and note-name was acquired, and (b) investigate whether the information incidentally acquired could be expressed explicitly. The phase began with the following instructions (translated from French):

Now, the task is similar, except that you will only see a note (not a note-name). Try to guess the name of the note by pressing the appropriate key on the keyboard.

The task was similar to the learning phase, except that (a) only the note-positions (without note-names) were presented, (b) the on-screen key reminder was re-added below the musical staff (*y*-axis centered on 775 px.), (c) there was no time limit to respond, and (d) there was no accuracy feedback. Thus, participants had to respond to the notes themselves (previously task irrelevant) rather than to the note-names. There were three blocks of each of the seven notes (21 trials in total), presented randomly without replacement.

Data Analysis

Analyses of the learning phase were conducted on mean correct RTs and error rates. Trials in which participants failed to respond in 3000 ms (i.e., the response deadline) were eliminated. Repeated measures ANOVAs for RTs and for error rates were conducted to assess the overall main effects of contingency, instruction, and the interaction between them. Furthermore, we ran another repeated measures ANOVA for RTs and error rates with the added factor of block (Block 1 and Block 2) to assess the presence of a contingency effect from the start of the acquisition process. If this is the case, we expect no significant interaction between blocks and contingency. One-sample *t* tests were used to assess learning rates between the groups. Pearson's correlations were performed to assess relations between objective and subjective awareness and the contingency effect. All analyses were evaluated at the $\alpha = .05$ level of significance. Additionally, we estimated the Bayes factor for all the data using JASP software (JASP Team, 2019). All the Bayesian analyses were done using the standard noninformative Cauchy prior in JASP with a default width of 0.707. A BF_{10} between 3 and 10 allows us to conclude that we have moderately strong evidence for H_1. The data set and R script are available via the following link: https://osf.io/fzex7/.

Results

Response Times

The RT results for Experiment 1 are presented in Figure 4. A repeated measures ANOVA for RTs with the factors Contingency (high vs. low) and Group (deliberate vs. incidental) showed a significant main effect of Contingency, $F(1, 121) = 32.347$, $p < .001$, $\eta^2 = .211$, $BF_{10} > 100$, indicating faster responses for high-contingency trials (M = 855 ms, SD = 112) than for low-contingency trials (M = 877 ms, SD = 115).

[Figure 4]

The main effect of Group was not significant, $F(1, 121) = .580$, $p = .448$, $\eta^2 = .005$, $BF_{10} = .554$. Interestingly, the interaction between Contingency and Group was also not significant, $F(1, 121) = .797$, $p = .374$, $\eta^2 = .007$, $BF_{10} = .278$, indicating no significant differences between deliberate vs. incidental learning groups for the contingency effect, although the effect was numerically larger for the deliberate learning group (see Figure 4; $M_{\text{low-high}} = 25.7$, $SD = 38.4$; $t(61) = 5.25$, $p < .001$, $d = .667$, $BF_{10} > 100$) compared to the incidental one ($M_{\text{low-high}} = 18.7$, $SD = 47.6$; $t(60) = 3.07$, $p = .003$, $d = .393$, $BF_{10} = 9.320$).

Additionally, a repeated measures ANOVA for RTs with the factors Block (1 vs. 2), Contingency (high vs. low), and Group (deliberate vs. incidental) was computed to analyze the data for rapid acquisition of the contingencies and possible differences across blocks between the two groups. A significant main effect for Blocks was found, $F(1, 121) = 44.053$, $p < .001$, $\eta^2 = .267$, $BF_{10} > 100$, showing significantly faster RTs in Block 2 (M = 849 ms, SD = 117) compared to Block 1 (M = 884 ms, SD = 115), indicating a standard practice effect on mean RT. The main effect for Contingency was also significant, $F(1, 119) = 32.363$, $p < .001$, $\eta^2 = .211$, $BF_{10} > 100$.

[Figure 5]

Block and Contingency did not interact, $F(1, 121) = .543$, $p = .463$, $\eta^2 = .004$, BF_{10} =

.277, suggesting that the learning of contingencies is fast rather than appearing gradually across blocks. On the other hand, the interaction between Block and Group was significant, $F(1, 121) = 9.839$, $p = .002$, $\eta^2 = .075$, $BF_{10} = 95.284$. Specifically, as illustrated in Figure 5, we found a significant difference in RTs for the deliberate learning group ($M_{\text{Block 1-Block 2}} = 50.65$; $t(121) = 6.939$, $p < .001$). This difference was not significant for the incidental learning group ($M_{\text{Block 1-Block 2}} = 18.14$; $t(121) = 2.465$, $p = .091$). Finally, the Contingency × Block × Group interaction was not significant, $F(1, 121) = .279$, $p = .599$, $\eta^2 = .002$, $BF_{10} = .190$

Error Rates

A repeated measures ANOVA for errors with the factors Contingency (high vs. low) and Group (deliberate vs. incidental) did not reveal a main effect of Contingency, $F(1, 121) = .081$, $p = .776$, $\eta^2 = .001$, $BF_{10} = .145$, or Group, $F(1, 121) = .115$, $p = .735$, $\eta^2 = .001$, $BF_{10} = .291$. The interaction between Contingency and Group was also not significant, $F(1, 121) = .015$, $p = .901$, $\eta^2 = 0.00$, $BF_{10} = .186$ (deliberate learning group, $M_{\text{high}} = .976$, $SD = .025$, $M_{\text{low}} = .990$, $SD = .029$; incidental learning group, $M_{\text{high}} = .977$, $SD = 0.027$, $M_{\text{low}} = .980$, $SD = .025$). Given the lack of a contingency effect in errors, a block analysis was not performed.

Subjective and objective awareness

For the subjective awareness question, 33 of 62 participants (53%) in the deliberate learning group reported that they noted the regularities, and 27 of 61 participants (44%) in the incidental learning group. Subjective awareness rates were not significantly different between the two groups, $M_{\text{deliberate-incidental}} = 9$ %, $t(121) = .990$, $p = .324$, $d = .179$, $BF_{10} = .300$ (deliberate learning group: $M = 53\%$; incidental learning group: $M = 44\%$).

Using one-sample *t* tests, we found that the rates of objective awareness (test phase accuracy) were above chance (1/7 or 14.3%) in both groups: deliberate learning group ($M = 50.6\%$, $SD = 31.1$), $t(61) = 9.19$, $p < .001$, $d = 1.17$, $BF_{10} > 100$, incidental learning group (M

= 32.0%, *SD* = 27.7), $t(60) = 4.98$, $p < .001$, $d = .637$, $BF_{10} > 100$. Objective awareness was higher for the deliberate learning group than for the incidental learning group, and a Welch two-sample *t* test showed that this 19% differences between the two groups was significant, $t(120) = 3.51$, $p = .001$, $d = .633$, $BF_{10} = 42.530$.

[Figure 6]

The RT-contingency effect (i.e., low minus high contingency) correlated significantly with both subjective awareness, $r(121) = .239$, $p = .008$, $BF_{10} = 3.760$, and objective awareness, $r(121) = .401$, $p < .001$, $BF_{10} > 100$, as shown in Figure 6. Additionally, the contingency effect was significant for both participants who were subjectively aware (M = 32.7, SD = 48.1), $t(59) = 5.28$, $p < .001$, $d = .681$, $BF_{10} > 100$, and for those who declared to be unaware (M = 12.2, SD = 35.5), $t(62) = 2.72$, $p = .009$, $d = .342$, $BF_{10} = 3.941$, suggesting stronger contingency effect for participants with greater awareness. For the objective awareness factor, we also computed the regression intercept at chance guessing (Greenwald et al., 1995). That is, we calculated a regression with objective awareness as the predictor and the RT contingency effect as the dependent variable. Objective awareness was re-centered at chance guessing (1/7, or 14.3%). The intercept therefore indicates the size of the contingency effect when participants are guessing at chance in the objective awareness phase. This intercept was numerically above zero in the sample as a whole (intercept M = 6.989), but not significantly, $t(121) = 1.46$, $SE = 4.78$, $p = .146$, $BF_{10} = 1.0$. Globally, the data show an impact of contingency knowledge on the size of the RT contingency effect, though it remains unclear whether and to what degree implicit learning also contributes to the effect. In contrast to the RT data, the error contingency effect (low minus high contingency errors) was not correlated with subjective awareness, $r(121) = -.018$, $p = .845$, $BF_{10} = .115$, or objective awareness, $r(121) = .001$, $p = .993$, $BF_{10} = .113$, which is not surprising given the lack of a significant contingency effect in errors.

Discussion

As hypothesized, in Experiment 1 we found a contingency effect, suggesting that nonmusicians were able to incidentally learn the associations between note-positions and the keyboard responses to note-names. Furthermore, in line with previous research, the block analysis suggests a rapid acquisition of the contingencies starting from the beginning of the learning phase. Although both groups responded significantly higher than chance in the objective awareness phase, the deliberate learning group was more accurate than the incidental one. This result may indicate an influence of attention in explicitly reporting the new acquired information. Overall, a relationship between the contingency effect and awareness was revealed by the significant correlations.

Experiment 2

Experiment 2 addresses a potential caveat with Experiment 1. It may be argued that the contingency effect in Experiment 1 can be due to previous implicit knowledge about note-name/note-position associations, rather than contingency learning. Although participants claimed that they were not able to read music notation, it is possible that they studied music at school and remember more than they imagined or even that some musicians misrepresented their music reading abilities in order to participate. If this were true, then it could be the case that no actual learning occurred in Experiment 1. Therefore, to address this concern and to also investigate whether previous musical knowledge influences the effect, we ran a second experiment. Experiment 2 was identical to Experiment 1, except that the high-contingency pairings were no longer the congruent pairings. Each note position was presented more often with one of the six incongruent note names (e.g., “ré” written inside the note for “fa”) on high-contingency trials and rarely with the remaining congruent and incongruent names (e.g., “ré” written inside the note for “ré”) on low-contingency trials.

Participants were divided in six groups, such that across participants every note position *except* the congruent note was high contingency for a given note name.

Unlike Experiment 1, in Experiment 2 contingency was different from congruency. The congruent trials (e.g., "ré" written inside the note for "ré") were presented much less often than the high-contingency incongruent trials (e.g., "ré" written inside the note for "fa"). Therefore, in Experiment 2 we speak about both the contingency effect (low minus high contingency trials) and the congruency effect (incongruent minus congruent trials). If previous musical knowledge is not present, the scrambling of the note-name to note-position associations should not be important, and we anticipate that participants will show a contingency effect similar to the one found in Experiment 1 (faster RTs for high-contingency trials compared to low-contingency trials). On the contrary, if participants possess undisclosed previously-acquired musical knowledge, then we should anticipate a congruency effect (faster RTs for congruent trials compared to incongruent ones) despite the high vs. low contingency presentation. Of course, it is also possible that both effects will be observed: a true learning effect within the experiment in addition to a congruency effect due to undisclosed sight-reading knowledge.

Method

Participants

Experiment 2 took place during the Covid-19 pandemic, so to adhere to the general health recommendations that restricted the possibility to recruit new participants to come to the lab, we ran Experiment 2 using the online Prolific.co platform. 132 participants clicked to start the experiment, but we excluded participants who abandoned the experiment before completion or did not actually begin the task. The remaining 60 participants, who received monetary compensation (£2) for their participation, were randomly assigned to each of the six scrambled note-name/note-position conditions, described below. Each condition was composed of 10 participants. The inclusion criteria were the same as those used for Experiment 1 and they were mentioned in the recruitment advertisement. All the procedures were conducted in accordance with the Declaration of Helsinki. A consent form was signed by all participants before beginning the study. Participants' anonymization was guaranteed.

Apparatus, Design, and Procedure

The experiment was programmed and run with Psytoolkit (Stoet, 2010, 2016). The structure of Experiment 2 was basically the same as Experiment 1, with the following exceptions. All participants learned incidentally, as in the incidental learning group of Experiment 1. Thus, no instruction about the contingencies was given. In the learning phase, we used scrambled note-name/note-position associations. That is, each note was presented 18 times per block with one of the incongruent note-name pairing (e.g., "ré" in the note for "fa") and once each with the remaining congruent and incongruent note-name pairings (e.g., "ré" in the note for "ré" and "ré" in the note for "do"). Thus, one specific incongruent name-note pairing was more frequent (high contingency) than the congruent and each remaining incongruent name-note pairings (low contingency). We created six groups by shifting the name-position correspondences by 1, 2, 3, 4, 5, or 6 tones (e.g., the position "ré" most often with the name "mi", "mi" most often with "fa", etc. for Group 1; the position "ré" most often with "fa", "mi" most often with "sol", etc. in Group 2; etc.). Thus, across participants all note positions were high contingency with each note name, except the congruent pairing.

Data Analysis

The same data analysis criteria used in Experiment 1 were applied for Experiment 2 with some exceptions: no block ANOVA was assessed; no instruction factor was used since all participants learned incidentally in Experiment 2. However, we ran a repeated measures ANOVA with Congruency as factor to evaluate the influence of congruent vs. incongruent trials on the learning process. High-contingency incongruent trials were eliminated from this analysis, so that the low-contingency congruent trials were compared only to low-contingency incongruent trials. The data set and R script are available via the following link: https://osf.io/fzex7/.

Results

Response Times

The RT results for Experiment 2 are presented in Figure 7. The repeated measures ANOVA for RTs with Contingency (high vs. low) and Group (1, 2, 3, 4, 5, 6) as factors, showed a significant main effect of Contingency, $F(1, 54) = 55.284$, $p < .001$, $\eta^2 = .506$, $BF_{10} > 100$ (high-contingency trials, $M = 988$ ms, $SD = 207$; low-contingency trials, $M = 1036$ ms, $SD = 206$). The main effect of Group was not significant, $F(5, 54) = 1.05$, $p = .400$, $\eta^2 = .088$, $BF_{10} = .527$, and the interaction between Contingency and Group was also not significant, $F(5, 54) = .565$, $p = .726$, $\eta^2 = .050$, $BF_{10} = .064$, suggesting no differences between groups for the contingency effect.

[Figure 7]

Interestingly, when using Congruency (congruent vs. incongruent) and Group (1, 2, 3, 4, 5, 6) as factors, the repeated measures ANOVA for RTs showed a significant main effect of Congruency, $F(1, 54) = 4.668$, $p = .035$, $\eta^2 = .080$ $BF_{10} = 1.598$ (congruent trials, $M = 996$ ms, $SD = 201$; incongruent trials, $M = 1045$ ms, $SD = 219$). The main effect of Group was not significant, $F(5, 54) = 1.55$, $p = .190$, $\eta^2 = .126$ $BF_{10} =.674$, as was the interaction between Congruency and Group, $F(5, 54) = .411$, $p = .839$, $\eta^2 = .037$ $BF_{10} =.100$.

Error Rates

The repeated measures ANOVA for errors with Contingency (high vs. low) and Group (1, 2, 3, 4, 5, 6) as factors did not reveal a main effect of Contingency, $F(1, 54) = 1.267$, $p = .265$, $\eta^2 = .023$, $BF_{10} = .329$ ($M_{\text{high}} = 9.74$, $SD = 8.70$; $M_{\text{low}} = 10.3$, $SD = 10.5$), or Group, $F(5, 54) = 1.17$, $p = .335$, $\eta^2 = .098$, $BF_{10} = .442$. The Contingency by Group interaction was also not significant, $F(5, 54) = .875$, $p = .504$, $\eta^2 = .075$, $BF_{10} = .137$.

Surprisingly the repeated measures ANOVA for errors with Congruency (congruent vs. incongruent) and Group (1, 2, 3, 4, 5, 6) as factors showed a significant main effect of Congruency, $F(1, 54) = 6.54$, $p = .013$, $\eta^2 = .108$, $BF_{10} = 1.614$ ($M_{\text{congruent}} = 7.86$, $SD = 9.18$; $M_{\text{incongruent}} = 10.8$, $SD = 11.4$). The main effect of Group was not significant, $F(5, 54) = .797$, $p = .557$, $\eta^2 = .069$, $BF_{10} = .095$, nor was the Congruency by Group interaction, $F(5, 54) = 2.12$, $p = .078$, $\eta^2 = .164$, $BF_{10} = .674$.

Subjective and objective awareness

In Experiment 2, more than 50% of the participants (34 of 60) reported to be aware of the regularities. Overall, the rates of objective awareness (test phase accuracy) were above chance (1/7 or 14.3%), ($M = 23.2\%$, $SD = 26.5$) $t(59) = 2.60$, $p = .006$, $d = .335$, $BF_{10} = 3.018$. The correlations between the RT-contingency effect (i.e., low minus high contingency) and subjective awareness, $r(58) = .123$, $p = .350$, $BF_{10} = .247$, and objective awareness, $r(58) = .085$, $p = .519$, $BF_{10} = .197$, were not significant. Additionally, the contingency effect was significant for both participants who were subjectively aware ($M = 42.9$, $SD = 57.5$), $t(33) = 4.35$, $p < .001$, $d = .746$, $BF_{10} > 100$, and for those who declared to be unaware ($M = 55.0$, $SD = 35.5$), $t(25) = 7.90$, $p < .001$, $d = 1.55$, $BF_{10} > 100$.

[Figure 8]

The congruency effect correlated significantly with subjective awareness, $r(58) = .345$, $p = .007$, $BF_{10} = 5.671$, but not with objective awareness, $r(58) = -.057$, $p = .668$, $BF_{10} = .176$, as shown in Figure 8. Moreover, the congruency effect was not significant for participants who were subjectively aware ($M = -2.14$, $SD = 158$), $t(33) = -.079$, $p = .938$, $d = -.013$, $BF_{10} = .148$, but was significant for those who declared to be unaware ($M = 116$, $SD = 165$), $t(25) = 3.56$, $p = .002$, $d = .699$, $BF_{10} = 23.940$. As for Experiment 1, we computed a regression intercept at chance guessing (Greenwald et al., 1995) with the objective awareness factor. The result showed a significant intercept above zero, $t(54) = 6.934$, $SE = 6.735$, p <.001, $BF_{10} = 1.0$; intercept $M = 46.707$, suggesting that implicit learning contributed to the contingency effect.

Not surprisingly, the error contingency effect (low minus high contingency errors) was not significantly correlated with subjective awareness, $r(58) = -.155$, $p = .238$, $BF_{10} = .318$, or objective awareness, $r(58) = .122$, $p = .353$, $BF_{10} = .245$. The error congruency effect was also not significantly correlated with subjective awareness, $r(58) = .217$, $p = .096$, $BF_{10} = .621$, or objective awareness, $r(58) = -.000$, $p = .998$, $BF_{10} = .161$.

Discussion

In Experiment 2, we again observed a contingency learning effect. Unlike in Experiment 1, however, the high-contingency pairings were (specific) incongruent pairings in Experiment 2. Thus, preexisting sight-reading knowledge could not have produced this contingency learning effect. Indeed, any preexisting knowledge would actually work *against* a contingency learning effect, as the congruent pairings were low contingency. Interestingly, we did also observe a congruency effect when comparing the congruent and incongruent low contingency pairings. This is a bit surprising given that past reports have failed to observe a congruency effect in nonmusicians (e.g., Crump et al., 2012; Drost et al., 2005; Grégoire et al., 2013; Stewart, 2005). Similarly, we did not find a robust contingency effect for congruent pairings in our other studies with the present paradigm when the contingency manipulation was too weak (including our pilot study and data from one of the conditions of some of our follow-up work to the present report). The reason for this congruency effect is unclear. One possible interpretation is that some of the participants did have prior sight reading knowledge and failed to disclose this, but Experiment 3 will explore this and another potential interpretation.

Subjective but not objective contingency awareness was poorly correlated with the contingency effect. Some evidence for implicit learning contributions to the contingency effect were observed, including a significant contingency effect for subjectively unaware participants and a significantly positive intercept in the objective awareness data, which contrasts slightly with the results of Experiment 1. Participants also guessed at above-chance rates the interpretations of the note positions. Of course, these were technically the incorrect note interpretations (i.e., consistent with the incongruent contingencies).

Experiment 3

As previously mentioned, we were surprised to find a significant congruency effect in Experiment 2. As mentioned above, this congruency effect may have been due to the inclusion of some participants that did have preexisting sight-reading knowledge that they failed to disclose (e.g., due to underestimation of their knowledge). However, there may be another explanation for the congruency effect that does not assume that some of the participants had preexisting knowledge. Indeed, it is possible that a congruency effect might be observed even if participants do not know the association between note names and note positions. Instead, there may have been an inherent spatial compatibility between the down-to-up organization of the note positions and the left-to-right organization of the response keys.

Previous research (Rusconi et al., 2006) showed the presence of a SMARC (Spatial–Musical Association of Response Codes) effect, defined by the authors as "a variant of the well-known orthogonal stimulus-response compatibility effect, that is a preferential mapping of spatially lower stimuli on left responses and higher stimuli on right responses" (Rusconi et al., 2006, p. 14). For the authors, the SMARC effect reflects the spatial coding of pitches, with the highest pitches represented on the right and the lowest pitches on the left.

Recently, Ariga and Saito (2019) showed the presence of a SMARC effect in the absence of pitch. Although, in their study there was no auditory stimulation, the effect was elicited by written pitch names alone for both trained musicians and musically naïve participants. Overall, this evidence suggested that the human cognitive system automatically codes pitches spatially.

Therefore, regarding our results, it is possible that the congruency effect could be explained by a natural inclination to spatially code pitches. Indeed, the spatially lowest note position (fa) corresponded to the leftmost response (Z) in our prior experiments moving up to the highest note position (mi) with the rightmost response (I). As such, it could be that participants responded faster to the congruent pairings not because they knew the interpretation of the note positions, but because of the spatial compatibility between the stimulus and response locations. To test this hypothesis, we ran a third experiment. It is worth noting that the aim of this third experiment is not to further investigate the contingency learning effect that we observed in the prior two experiments; rather, we aim to test whether the congruency effect found in Experiment 2 was due to preexisting sight-reading knowledge or to a SMARC-like compatibility effect.

Experiment 3 was identical to the previous two experiments, except that no contingency manipulation was used. Each note-name/note-position pairing was presented the same number of times. However, to test the hypothesis of the presence of the SMARC effect we distinguished between congruent trials, compatible trials, and control trials (see Table 1 and the method section for more details). In particular, the response options were reordered such that the congruent response was *not* spatially compatible with the note position. For instance, the bottommost stimulus location (fa) was *not* the leftmost response. Congruent trials were therefore the trials in which the note position was presented with the true note name (e.g., the position for fa presented with “fa”), compatible trials were not congruent but were spatially compatible (e.g., the position for fa with the note name "do"), and all remaining pairings were controls. If participants do not possess undisclosed previously acquired musical knowledge, then we should not find a congruency effect (faster RTs for congruent trials compared to control ones). If participants are influenced by spatial compatibility, however, then we might find a compatibility effect (RTs faster for the compatible trials compared to control trials).

[Table 1]

Method

Participants

Experiment 3 was coded using Psytoolkit (Stoet, 2010, 2016) and run using the online Prolific.co platform. One hundred and seventy-five participants clicked through to the link to the experiment on Prolific, but we again excluded participants that did not complete the study or actually begin it. 119 participants, who received monetary compensation (£2), took part in the experiment. The inclusion criteria were the same used for Experiment 1 and they were mentioned in the recruitment advertisement. All the procedures were conducted in accordance with the Declaration of Helsinki. A consent form was signed by all participants before beginning the study. Participants' anonymization was guaranteed.

Apparatus, Design, and Procedure

Experiment 3 was identical to Experiment 2 with the following exceptions. During the "learning phase", no contingency manipulation was used. That is, each note position was presented equally often with all of the note names. Thus, there was actually no regularity to learn in the present experiment. Instead, we manipulated spatial compatibility and congruency. To dissociate the two, we changed the order of the key mappings. While the down-to-up note positions still went from "fa" to "mi", the key mappings went from "do" to "si". In this way, the leftmost response (e.g., do) did not correspond to the bottommost note position (fa). None of the note positions were spatially compatible with the congruent response. Therefore, we distinguished between: (a) *congruent trials*, in which the note name (e.g., “do”) was written in the congruent note position (e.g., “do”), (b) *compatible trials*, where the note name was spatially compatible with the order of the key mapping (e.g., the leftmost note name “do” written in the bottommost note position “fa”), and (c) *control trials*, which were neither congruent nor spatially compatible (e.g., the leftmost note name “do” written in the topmost position “si”). Concretely, the responses were shifted three places to the left, but otherwise maintained the same relative order (i.e., do, ré, mi, fa, sol, la, si). Hypothetically, it would have been possible to create six such orders (e.g., analogous to Experiment 2). However, we opted for this single ordering because for many of the possible orders the congruent and spatial compatible responses would be very close to each other spatially. The particular response ordering that we used maximally separates the congruent and compatible responses. Furthermore, no subjective awareness phase was shown because of the lack of contingency manipulation. A phase effectively identical to the objective awareness phases of the previous experiments was still included, but was no longer a true “awareness” phase (as there was no contingency to be aware of this time). We will therefore refer to this simply as the “test” phase.

Data Analysis

The same data analysis criteria as those used in Experiments 1 and 2 were applied in Experiment 3. We use t tests to compare RTs and error rates between the different trials: congruency, compatibility, and control. We also ran analyses on both the accuracy for congruency and the accuracy for compatibility in the test phase to study whether participants indicated the congruent and/or compatible responses more often than one would expect by chance. Given the absence of a contingency, participants should only indicate the congruent response more often than chance if they have preexisting sight-reading knowledge and should only indicate the compatible response more often than chance if they are influenced by spatial compatibility. The data set and R script are available via the following link: https://osf.io/fzex7/.

Results

Response Times

The *t*-tests analyses revealed no significant difference in RTs (Figure 9) between congruent and control trials ($M_{congruent\text{-}control}$ = 7.114, *SD* = 58.5), $t(117) = 1.322$, $p = .189$, $d = .121$, $BF_{10} = .329$, or between compatible and control trials ($M_{compatible\text{-}control}$ = -.755, *SD* = 58.9), $t(117) = -.143$, $p = .887$, $d = -.013$, $BF_{10} = .104$.

[Figure 9]

Error Rates

The *t*-tests analyses revealed no significant difference for error rates between congruent and control trials ($M_{congruent\text{-}control}$ = .730, *SD* = 4.82), $t(117) = 1.645$, $p = .103$, $d = .151$, $BF_{10} = .304$, or between compatible and control trials ($M_{compatible\text{-}control}$ = .307, *SD* = 4.01), $t(117) = .831$, $p = .408$, $d = .076$, $BF_{10} = .164$.

Test phase

The *t* tests on accuracy rates in the test phase (akin to the objective awareness phase in the previous experiments) revealed accuracy rates that were significantly above chance (1/7 or 14.3%) for both the congruent response (*M* = 24.7%, *SD* = 28.1), $t(117) = 4.00$, $p > .001$, $d = .368$, $BF_{10} > 100$, and the compatible response (*M* = 19.6%, *SD* = 20.4), $t(117) = 2.80$, $p = .003$, $d = .258$, $BF_{10} = 8.334$. Both of these effects, especially the congruency effect, seem to be due to a small number of outliers. Figure 10 shows the distribution of the results in the test phase. As can be seen, most of the participants seemed to be guessing (i.e., their results are under or slightly above chance guessing). However, few of them seemed to have enough preexisting knowledge about the congruency between note names and note positions, with some participants "guessing" 100% of the pairings correctly. Given that there was no way to *learn* the congruent pairings in the present experiment, this clearly indicates preexisting knowledge. The compatibility effect seems similar, but weaker, with an even smaller number of participants indicating the spatially compatible response well above chance.

[Figure 10]

Discussion

In Experiment 3, we tested for possible influences of the SMARC effect on the congruency effect we observed in Experiment 2. That is, we wanted to study whether the natural tendency of spatially coding the pitches could influence participants' responses in an incidental Stroop-like task. Our results did not show a significant difference in response times between spatially compatible and control trials. Interestingly, we also did not replicate the congruency effect in response times or errors despite a notably larger sample size. As already discussed, this absence of a congruency effect is actually consistent with a number of prior reports with a similar or (in some cases) near identical procedure. The significant congruency effect observed in Experiment 2 may therefore have been a Type 1 error.

On the other hand, the nonmusicians responded significantly above chance in the test phase with the congruent response. Given that there was no way for participants to learn the congruent pairings without the current experiment, this clearly indicates that some small number of participants *did* have preexisting sight-reading knowledge. The same test phase also revealed elevated numbers of spatially compatible responses. These latter results may suggest that the natural inclination for spatially coding pitches can influence performance in some cases, such as in a more explicit judgement task.

General Discussion

In our study, we were interested in investigating early acquisition of sight-reading skills in an incidental learning procedure. That is, can nonmusicians with no prior familiarity with music reading rapidly acquire knowledge of standard notation that in turn produces automatic influences on performance in a similar way to that observed in skilled musicians? As hypothesized, despite a very short learning phase (336 trials, approximately 15 min) and slightly more complex material than those used in previous incidental learning procedures (e.g., words and colors), nonmusicians produced a robust contingency effect during the learning and subsequent test phases in both the deliberate (Experiment 1) and incidental (Experiments 1 and 2) learning groups.

Musicians can easily read music symbols and Grégoire et al. (2013) pointed out that the Musical Stroop Effect can be explained by the automaticity of the learned association between the note position and note name. Musicians cannot avoid "naming" the note-position just as skilled readers cannot avoid reading color-words in the regular Stroop task. Furthermore, Schön et al. (2001, 2002) proposed that musicians rely on different types of translation when reading music. For instance, playing-like (i.e., visuomotor translation) and naming-like (visual-verbal translation) transcodings are important to automatize the process of sight reading. In general, sight reading seems to be a complex process based on visuomotor integration (Gudmundsdottir, 2010).

In the present report, we showed that recently acquired associations, even if only learned incidentally, can produce the same automatic influences on behavior. Although our predictive stimulus (note-position) was not task-relevant (i.e., not the target stimulus), it produced an effect on performance, anyway. That is, our participants were able to learn the associations between note names and note positions as well as the corresponding actions. As mentioned in the Introduction, it may be the case that learning the contingencies between the predictive stimulus and the target drives the prediction of the motor response. Further, it is likely the case that learning is so rapid because participants can gain extensive practice of the stimulus-response pairings in a short period of time, which often is not the case with more deliberate learning procedures (Logan & Klapp, 1991). Although contingency learning has been observed in numerous learning paradigms (e.g., the color-word contingency learning paradigm), here we show for the first time the presence of the contingency effect in a music-related task. We were able to prove that the same sort of learning observed between simple stimulus pairs (e.g., colors and words) is also observable with more complex (e.g., in terms of the number of stimuli presented and the number of associations to learn) and more ecological musical materials.

The main aim of Experiment 2 was to investigate to which extent previous knowledge can influence the contingency effect found in Experiment 1. We asked for nonmusician participants who do not know how to sight read, though there is always a risk that participants have studied music at school and remember more than they imagined. We therefore scrambled the note-name to note-position correspondences. Reassuringly, a contingency effect was still found in Experiment 2, suggesting once again a rapid incidental learning of the presented associations. As the associations between note positions and responses to the note names were *not* congruent in Experiment 2, this learning effect could not have been due to preexisting sight-reading knowledge. However, in Experiment 2 a congruency effect was also found, suggesting the presence of previous musical knowledge in some participants, possibly due to music training at school. Based on this evidence, it is possible that the contingency effect in Experiment 1 was influenced by the congruency effect (i.e., because in Experiment 1, contingency was confounded with congruency, since all high-contingency trials were congruent and all low-contingency trials incongruent). In any case, our results, though indicating that undisclosed musical knowledge might impact the measure of learning if only congruent associations are used, true contingency learning is still present during the learning phase.

To further elucidate the congruency effect observed in Experiment 2, we ran a third experiment in which we investigated the hypothesis that the congruency effect in Experiment 2 was influenced by the SMARC effect. As previously mentioned, the SMARC effect refers to the natural human tendency for spatially coding pitches (Rusconi et al., 2006), even without the presence of an actual sound (Ariga & Saito, 2019). Based on this premise, in Experiment 3 we dissociated congruency from stimulus-response spatial compatibility. In this way, we distinguished between congruent trials, in which the note name was congruent with the note position (e.g., the note name “do” in the position for “do”) and compatible trials, where the spatial position of the target was compatible to the spatial position of the response key on the keyboard (e.g., the note name “do” in the bottommost “fa” note position when the key responses were ordered from “do” to “si”). Our aim was to measure to which degree the previously observed congruency effect was due to preexisting sight-reading knowledge (as measured by congruency) and/or to a SMARC-like spatial compatibility effect. In response times and errors, we failed to replicate the finding of Experiment 2, with no congruency or compatibility effect. Potentially, this might indicate that the significant congruency effect in Experiment 2 was due to Type 1 error, or that some other seemingly trivial difference between Experiments 2 and 3 was responsible for the different outcomes. However, while in our study participants were engaged in an incidental learning procedure, previous SMARC studies (Ariga & Saito, 2019; Rusconi et al., 2006) asked participants for explicit judgements. It is worth noting that in our study also, when nonmusicians were required to provide an explicit response in the test phase, their performance was significantly above chance level, suggesting the presence of a SMARC effect. We also observed above-chance congruent responses in the same test phase, clearly indicating that some small number of participants did have some preexisting knowledge. This suggests that future studies that aim for a “pure” measure of learning might be best adapted with some form of pretest of preexisting knowledge and/or non-spatially compatible stimulus-response mappings.

Additionally, as previously hypothesized, overall test phase accuracy (objective awareness), in both Experiments 1 and 2, indicates that nonmusicians performed above chance, suggesting that they were able to learn the associations that they were exposed to and even to verbalize this knowledge. However, in Experiment 1, a significant difference in favor of the deliberate learning group in the objective awareness results suggests that deliberate learning boosts learning more than purely incidental learning does. Previous research showed that to learn contingencies, being attentive to the predictive dimension is important (Eitam et al., 2009; Jiang & Chun, 2001). If this is the case for the deliberate learning group, then it is not surprising that they gave more accurate responses in the objective awareness phase than the incidental learning group did. At the same time, the evidence in favor of the deliberate learning group may simply suggest that learning in a deliberate way might aid more during explicit reporting (in the objective awareness phase) than in the case of automatic execution (in the learning phase). In other words, our objective awareness phase specifically required participants to express an explicit judgment, unlike the learning phase where participants were asked for automatic execution. Although, the deliberate learning group reported more accurate response in the objective awareness phase than the incidental one, the nonsignificant Contingency × Group interaction in the learning phase suggests that the two groups were able to automatize the learned contingencies in a quite similar way. Thus, deliberate learning may provide an advantage when it comes to explicit reporting, but perhaps may not confer the same advantage for automatization of contingency knowledge.

Although the observed acquisition of sight-reading knowledge may seem implausibly fast to some readers, such results are not a surprise when considering prior contingency learning work with other stimuli. As previously mentioned in the Introduction, contingency learning paradigms like the present one allow for extremely rapid acquisition of the associations between stimuli in a task (Lin & MacLeod, 2018; Schmidt et al., 2010; Schmidt & De Houwer, 2016), therefore the present results are completely coherent with past work using related, nonmusical learning procedures.

We note that our aim was not to claim that a procedure such as ours can replace other types of deliberate practice, which are more goal-oriented (Ericsson et al., 1993; Ericsson & Harwell, 2019; Mishra, 2014). On the contrary, we believe that the acquisition of complex skills, such as sight reading, can benefit from both deliberate and incidental learning procedures. On one side, more deliberate training can guide the acquisition of instrument-specific skills, such as effortful strategies to improve the technical movements of the bow on the strings to play the violin. On the other hand, an incidental learning procedure such as that used in the present report can help with the automatization of visuomotor integration, favoring sight-reading performance.

As one potential limitation, in the current study participants responded to note-names and learned about the note-positions incidentally. We did this for a few reasons. Most importantly, the current methodology allowed us to study the automatic (i.e., stimulus-driven) influences of note-position knowledge on performance (e.g., akin to the musical Stroop with experienced musicians or the influence of color words on color naming in the traditional Stroop paradigm). Learning may, however, be even stronger and faster if participants respond to the note positions directly (i.e., the note-position is the target, rather than the task-irrelevant but informative stimulus). We are currently investigating this in an ongoing study. Furthermore, as already noted in the Introduction, we used an imperfect contingency manipulation (75% high contingency vs. 25% low contingency). Although this was done to measure learning while it was occurring, a perfect contingency manipulation (e.g., using a

100% congruency between note-names and note-positions) may further help learning, especially in a real-world application (e.g., helping nonmusicians to acquire sight-reading skills with a learning app). This point is the object of another ongoing study we are currently conducting.

As another limitation, although we used arbitrary stimulus-key assignments similar to the ones that musicians practice on their instrument (especially piano), we did not use real instruments for learning. In future research, using the same logic of this study, it may be interesting to use a very similar piano response modality, or also other types of instruments (like string or wind instruments). A vocal response modality (e.g., singing) could also be used. Globally, the goal was to show that this type of position-to-action learning can occur rapidly with an appropriately designed learning procedure, but real-world applications to actual instruments remains to be explored. Furthermore, although here we mostly focused on the acquisition and automatization of the associations between spatial positions and motoric responses, previous research suggested that auditory stimuli are important to train sight-reading skills. That is, sight reading benefits greatly from an integration of visual, auditory, and motor components (Brodsky et al., 2003, 2008; Gromko, 2004; Hayward & Eastlund Gromko, 2009), rather than just visuomotor integration (Gudmundsdottir, 2010). In other words, learning what the note positions sound like can facilitate sight-reading skills. In on-going studies, we are investigating the role of auditory stimuli in learning in our task, to further test the facilitative benefit of auditory stimuli in the acquisition of sight-reading skills.

In conclusion, we showed the presence of the contingency effect in an incidental music contingency procedure, as well as the ability to verbalize the knowledge that was incidentally (or deliberately) acquired. Such findings are exciting, because they suggest that a seemingly difficult-to-learn music skill, sight-reading, can be learned much more quickly and easily than previously assumed. In the short-term, we hope that this paper will serve as the starting point for further investigations of the incidental learning of complex material, musical or otherwise, including investigations of ways to reinforce learning even further. In the long-term, this study may open up a new line of research to implement the same or similar approaches in an applied setting to help novices (whether in a musical and nonmusical context) to acquire valued skills with greater ease.

Declaration of Conflicting Interests

The Authors declare that there is no conflict of interest

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Ethics Statement

In accordance with the local legislation and institutional requirements, ethical review and approval for research on human participants in cognitive psychology study was not required.

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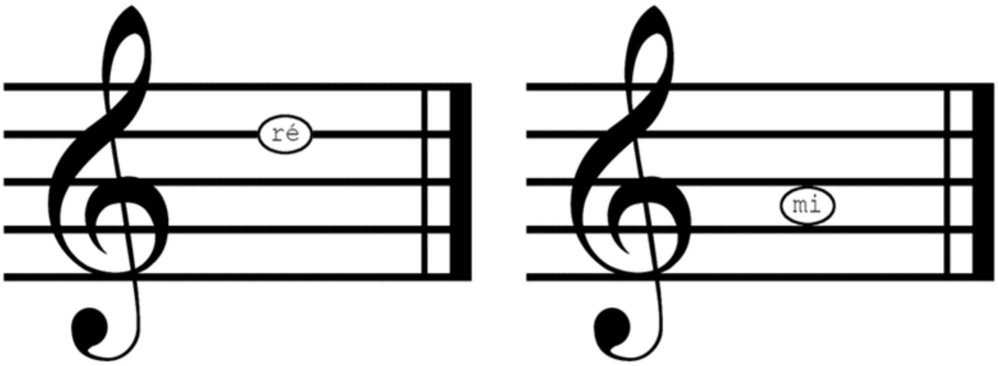
Figures

Figure 1

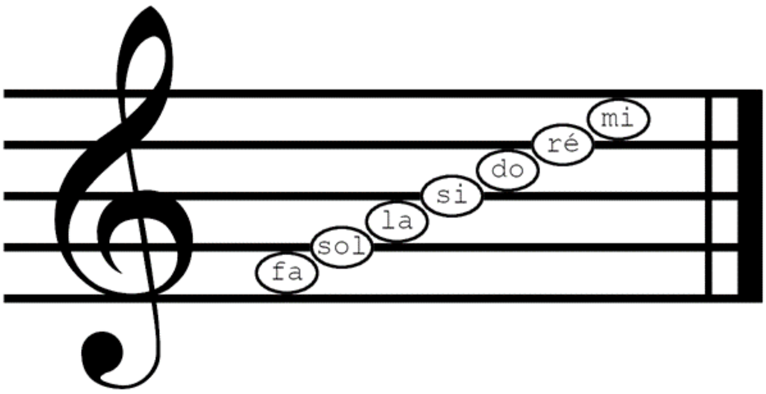
Example stimuli in the musical contingency task.



Note. On the left, a congruent stimulus ("ré" printed in the note for ré). On the right, an incongruent stimulus ("mi" printed in the note for la).

Figure 2

Full range of note positions used in the experiment, with congruent names.



Note. An individual note was horizontally centered on a smaller staff in the actual experiment, as in Figure 1.

Figure 3

Order of stimuli appearance during the learning phase.

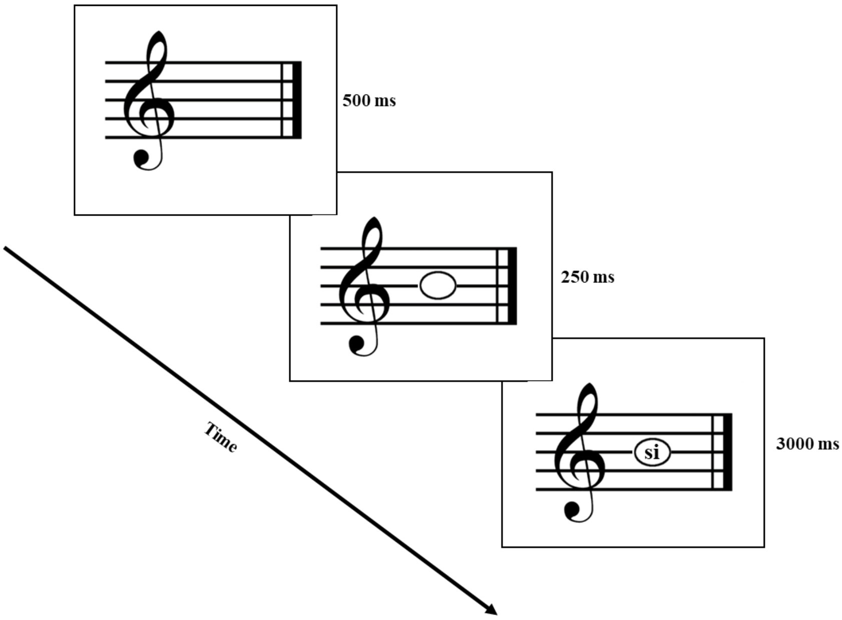
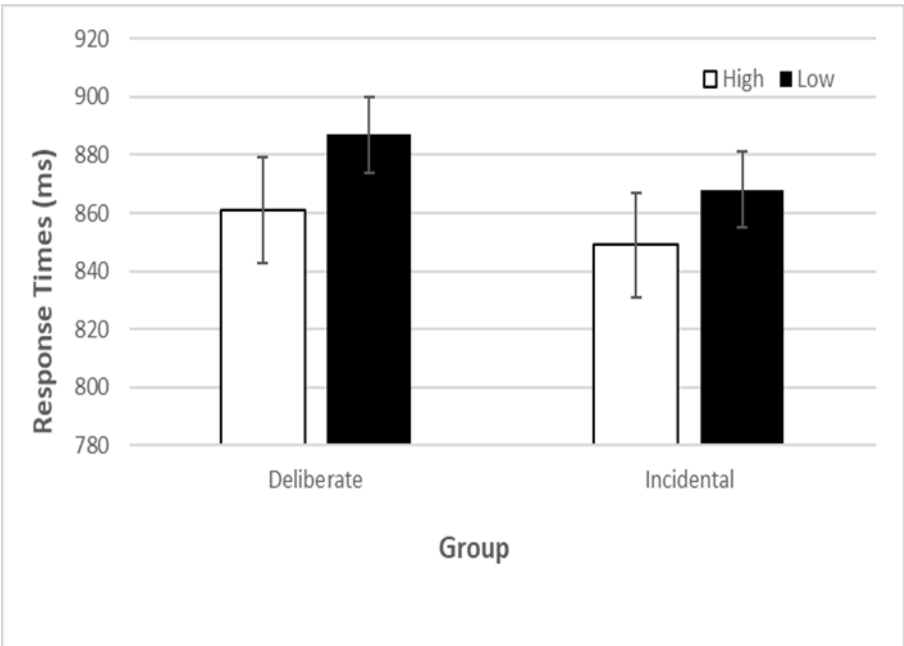


Figure 4

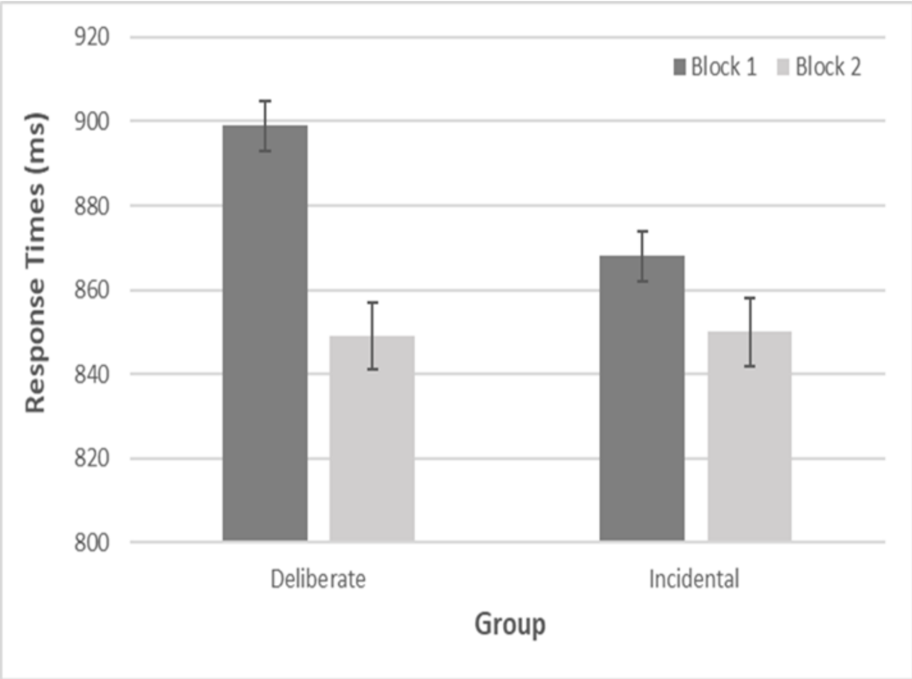
Experiment 1 RTs for deliberate and incidental groups.



Note. Interaction between Contingency (High and Low) and Group (Deliberate and Incidental), standard error bars are shown in the figure.

Figure 5

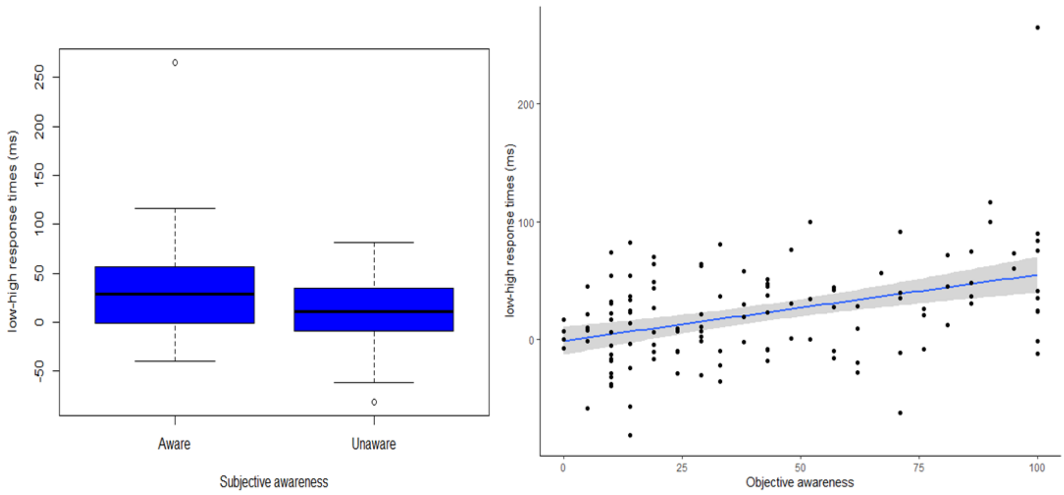
Experiment 1, interaction between Block and Group.



Note. Averaged response times across high and low contingency trials for block (Block 1 and Block 2) for the deliberate and incidental learning groups (standard error bars are shown).

Figure 6

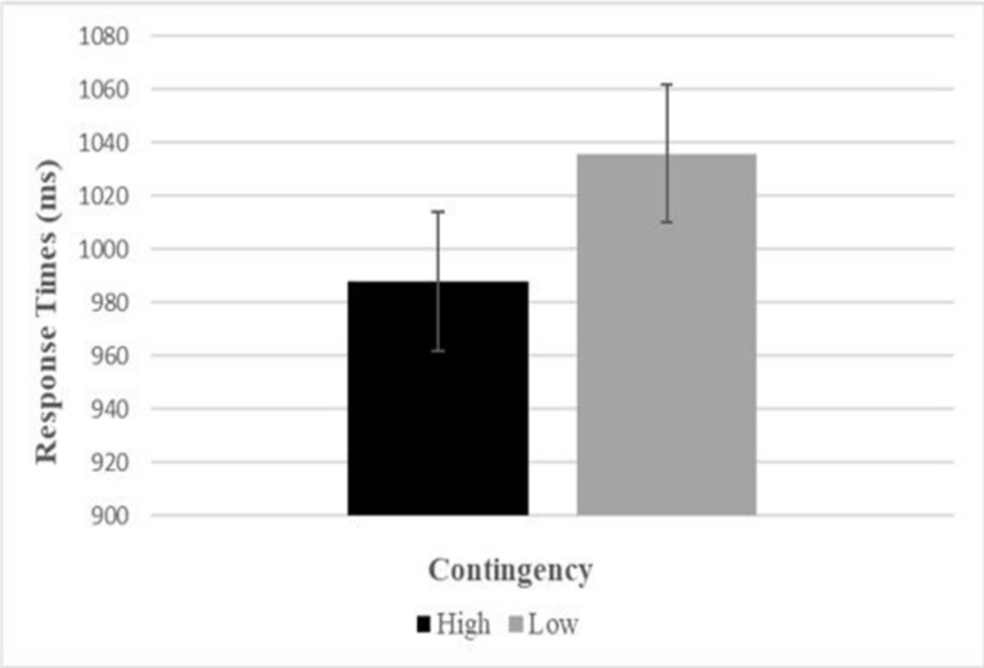
Experiment 1, correlations between contingency effect and subjective and objective awareness.



Note. In the left panel, the correlation between the contingency effect and subjective awareness is shown. In the right panel, the correlation between the contingency effect and objective awareness (test phase) is shown.

Figure 7

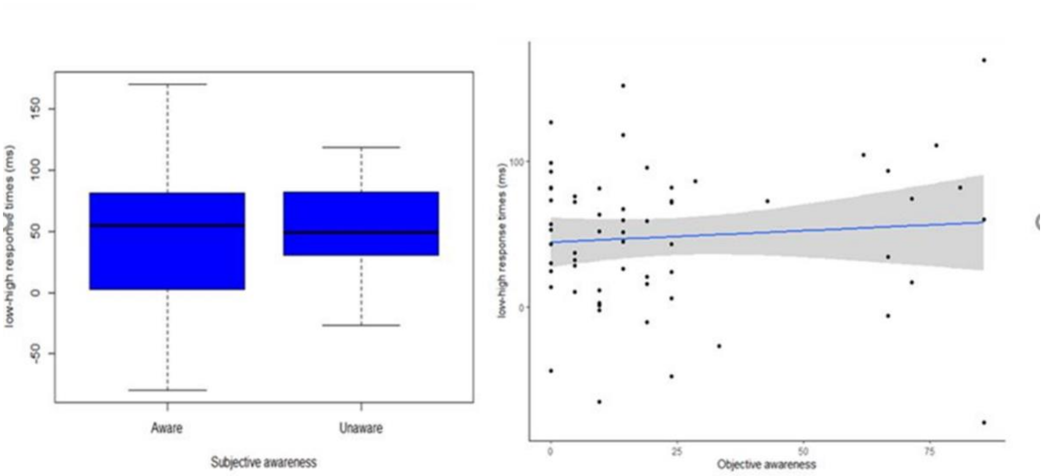
Experiment 2, avaraged mean for Contingency effect.



Note. Averaged mean scores between groups for high- and low-contingency trials. Error bars represent standard errors.

Figure 8

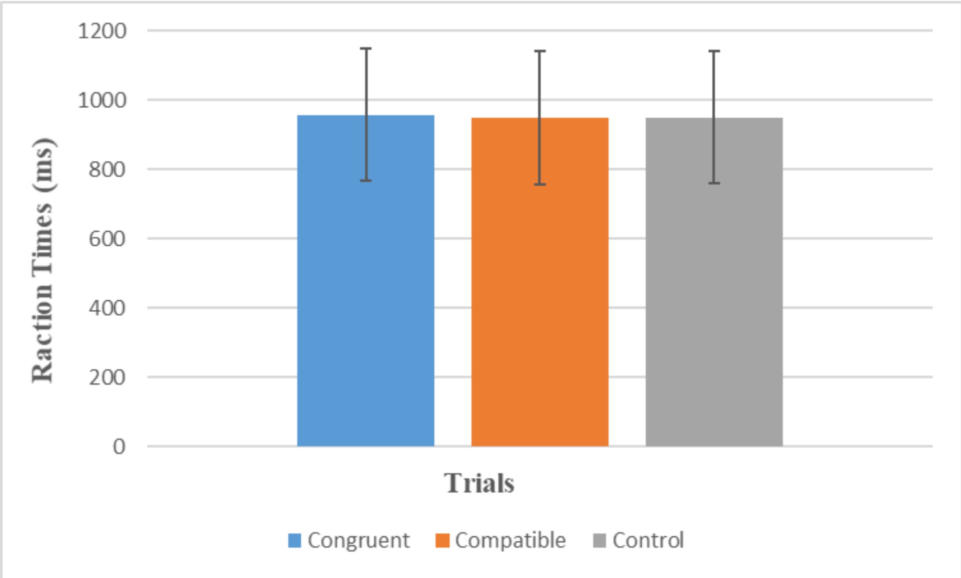
Experiment 2, correlations between contingency effect and subjective and objective awareness.



Note. In the left panel, the correlation between the contingency effect and subjective awareness is shown. In the right panel, the correlation between the contingency effect and objective awareness (test phase) is shown.

Figure 9

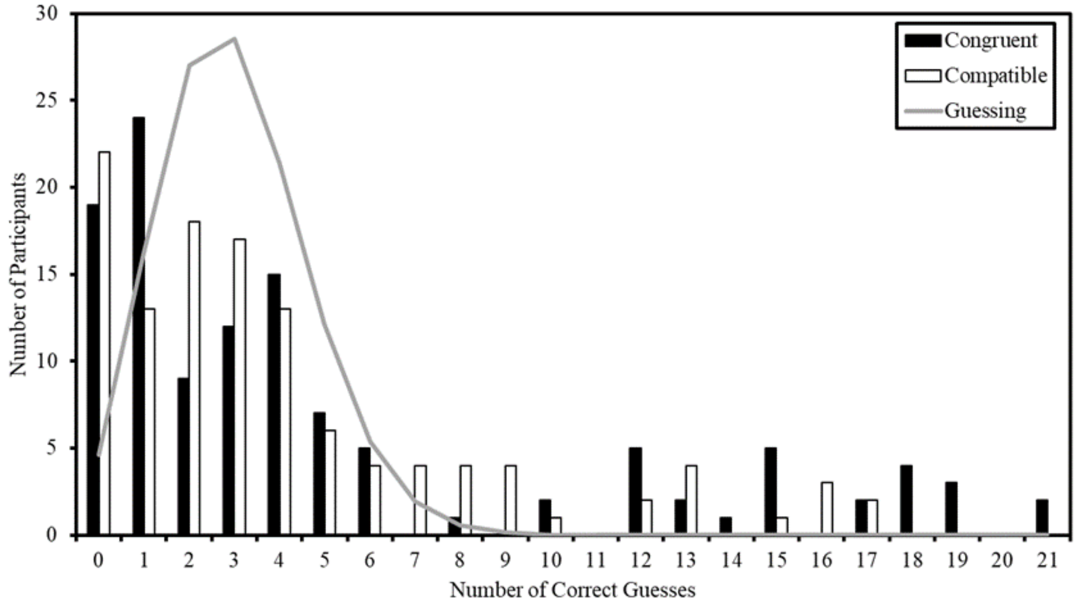
Experiment 3, RTs for the different trials.



Note. Mean RTs scores for the different trials: congruent, compatible and control.

Figure 10

Experiment 3, distributions of the number of congruent and compatible guesses (out of 21) along with the expected number of correct responses if guessing alone.



Note. The guessing curve assumes that participants do not have a bias to repeat the same response to the same stimulus. The distribution would be flatter if participants have said bias, probably explaining the larger number of participants with a score near zero and multiples of three along with the smaller number of participants near the expected peak of the distribution.

Table 1

Experiment 3, Musical Stroop contingency learning manipulation.

Note Name	Note position						
	Do	Ré	Mi	Fa	Sol	La	Si
Do	3	3	3	*3*	3	3	3
Ré	3	3	3	3	*3*	3	3
Mi	3	3	3	3	3	*3*	3
Fa	3	3	3	3	3	3	*3*
Sol	*3*	3	3	3	3	3	3
La	3	*3*	3	3	3	3	3
Si	3	3	*3*	3	3	3	3

Note. Numbers of repetition for each trial. Congruent trials in **bold**, compatible trials in *underlining italic* and control trials in standard font.