Assumptions of the process-dissociation procedure are violated in sequence learning

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Data, code, and materials necessary to reproduce the analyses reported in this article are available at https://github.com/methexp/pdl2.

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Abstract

In sequence learning, a process-dissociation (PD) approach has been proposed to dissociate implicit and explicit learning processes. Applied to the popular generation task, participants perform two different task versions: an *inclusion* condition asking them to re-generate the learned sequence, and an *exclusion* condition asking them to avoid generating the learned sequence. Whereas accurate performance under inclusion may be based on either implicit and/or explicit knowledge, avoiding to generate the learned sequence requires controllable explicit sequence knowledge. The PD approach yields separate estimates of explicit and implicit knowledge that are derived from the same task and therefore avoids many problems of previous measurement approaches. However, the PD approach rests on the critical assumption that the implicit and explicit processes are invariant across inclusion and exclusion conditions. We tested whether the invariance assumptions holds for the PD generation task. Across three studies using first-order as well as second-order regularities, invariance of the controlled process was found to be violated. In particular, despite extensive amounts of practice, explicit knowledge was not exhaustively expressed in the exclusion condition. We discuss the implications of these findings for the use of process-dissociation in assessing implicit knowledge.

Keywords: sequence learning, process-dissociation procedure, invariance assumption

Word count: 14,041

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Introduction

Riding a bicycle is an easy task, but most of us will be hard-pressed to describe in detail the movements necessary for pedaling, keeping direction, and maintaining balance. Capturing this intuition, theories of human learning distinguish two types of knowledge: Explicit knowledge is acquired when a learner becomes aware of an environmental regularity and stores it in episodic memory; in contrast, implicit knowledge reflects regularities in the environment that may have been acquired without becoming aware of them (Shanks & St. John, 1994). A classical paradigm for the study of implicit learning, the serial reaction time task (SRTT; Nissen & Bullemer, 1987) has participants respond to stimuli presented at four horizontal screen locations by pressing the key that corresponds to the stimulus location. Unbeknownst to participants, the stimulus locations follow a regular sequence. With practice, participants learn to respond faster on trials with regular stimulus-location transitions than on irregular transitions. Despite this performance advantage for responses that follow the sequence, participants are often unable to verbalize any knowledge about the sequence.

The measurement of implicit and explicit knowledge in this and other paradigms has been subject of debate. In the domain of sequence learning, the RT advantage of regular over irregular transitions has often been taken to indicate implicit knowledge. To assess explicit knowledge, verbal reports have been used, but they have been criticized as insensitive and potentially distorted by conservative reporting criteria, and they also differ from the RT measure in reliability (i.e., verbal reports rely on only a single data point per participant), and in immediacy (i.e., verbal reports are assessed only after the SRTT) (see Shanks & St. John, 1994). The application of the process-dissociation (PD) approach (introduced by Jacoby, 1991, to dissociate implicit and explicit memory) to sequence learning has therefore been an important improvement (Buchner, Steffens, Rothkegel, & Erdfelder, 1997; Curran, 2001; Destrebecqz & Cleeremans, 2001). In the PD approach, measures of implicit and explicit knowledge are derived from two variants of the same task, thereby largely eliminating the criticized confounds. Specifically, under *inclusion* instructions participants are asked to apply their explicit knowledge when solving the task, so that correct inclusion performance can arise from both explicit and implicit knowledge; in contrast, under *exclusion* instructions they are asked to refrain from using explicit knowledge, so that correct exclusion performance can be attributed only to implicit knowledge.

Process dissociation in the generation task

Destrebecqz and Cleeremans (2001) applied the PD approach to the generation task. Participants were instructed, after finishing the SRTT, to generate a sequence that is either similar (in the inclusion condition) or dissimilar (in the exclusion condition) to that observed during the SRTT. To the degree that participants can generate a similar sequence under the inclusion instruction, they can be said to have acquired knowledge about the sequence; yet, this knowledge may reflect implicit and/or explicit knowledge because both may be used to re-generate the learned sequence. However, only explicit knowledge is assumed to be under participants' control: When asked to generate a sequence that is dissimilar to the learned sequence – that is, to *exclude* their explicit knowledge – participants can avoid generating similar transitions only *to the degree that their sequence knowledge is explicit.* To the degree that their sequence knowledge is implicit, they would still generate a sequence *similar* to the learned sequence despite being instructed to do the opposite. Based on this logic, conclusions about the presence or absence of explicit knowledge can be drawn from performance differences between the inclusion and exclusion conditions; conclusions about the presence or absence of implicit knowledge can be drawn from differences between exclusion performance and a control condition or chance baseline.

The PD generation task has repeatedly been used to investigate sequence learning (e.g., Destrebecqz & Cleeremans, 2001, 2003; Q. Fu, Dienes, & Fu, 2010; Q. Fu, Fu, & Dienes, 2008; Haider, Eichler, & Lange, 2011; Jiménez, Vaquero, & Lupiáñez, 2006; Mong, McCabe, & Clegg, 2012; Norman, Price, & Duff, 2006; Shanks, Rowland, & Ranger, 2005; Wilkinson

& Shanks, 2004), and results showed some convergent validity: Participants who had acquired explicit knowledge – as measured by the PD procedure – were able to drastically reduce their RT during the learning phase by actively predicting the next response; this was not the case for participants who did not show evidence for explicit knowledge in the PD task (Haider et al., 2011). Investigations focusing on the relative contributions of implicit and explicit knowledge in sequence learning have, however, yielded mixed results: While some studies found evidence for implicit but no explicit knowledge, or reported combinations of implicit and explicit knowledge acting together (e.g., Destrebecqz & Cleeremans, 2001, 2003; Q. Fu et al., 2010, 2008; Haider et al., 2011; Jiménez et al., 2006; Mong et al., 2012; Norman et al., 2006), other studies found only evidence for explicit knowledge (Shanks et al., 2005; e.g., Wilkinson & Shanks, 2004). Whereas it is possible that moderating variables (e.g., the response-stimulus interval, cued vs. uncued generation task) may be identified that can account for these discrepancies, they may also (at least in part) arise from unwarranted assumptions of the PD method as discussed next.

Process dissociation and its assumptions. The PD approach has become a popular and versatile tool for measuring the relative contributions of implicit or automatic versus explicit or controlled processes in a variety of tasks (Yonelinas & Jacoby, 2012). It can be formalized as a set of equations describing inclusion (I) and exclusion (E) performance as a function of the probabilities of controlled process, C, and the automatic process, A, as follows:

$$I = C + (1 - C) * A$$

and

$$E = (1 - C) * A$$

These equations express the notions that (1) correct responses under inclusion can arise from either the controlled process (with probability C) or, given that it fails (with probability $1 - C$), from the automatic process A; and (2) correct responses under exclusion are solely due to the automatic process in the absence of the influence of the controlled process,

$(1 - C) * A$. Solving these equations for C and A (or using parameter estimation techniques for multinomial models) yields estimates of the contributions of the controlled and automatic process.

The validity of the PD method and model has been the target of debate since its introduction by Jacoby (1991; see, e.g., Buchner, Erdfelder, & Vaterrodt-Plünnecke, 1995; Curran & Hintzman, 1995). This is because the PD approach is not a theory-free measurement tool but rests on a set of strong and possibly problematic assumptions. First and obviously, it assumes the existence of two qualitatively different—controlled and automatic—processes, and it aims to measure the magnitude of their respective contributions. It is, however, not well-suited for comparing single- and dual-process models: To illustrate, Ratcliff, Van Zandt and McKoon (1995) found that data generated from a single-process model could produce a data pattern that, when analyzed using the PD approach, appears to support the existence – and differential contributions – of two qualitatively distinct processes. This implies that empirical dissociations between the controlled and automatic estimates do not necessarily imply the existence of two qualitatively different underlying processes.

Second, it is assumed that both processes operate independently; that is, on each trial, both the explicit and the implicit process attempt to produce a candidate response in parallel, and their respective candidate responses are not influencing each other. In particular, the response produced by the automatic process is assumed to be uninfluenced by whether the controlled process produces the same or a different response. Relatedly, the model assumes that independence holds across persons and items; when data are aggregated over (potentially heterogeneous) participants and items, a violation can lead to biases in parameter estimates. There has been considerable debate about the independence assumption in applications of the PD to episodic memory paradigms (Curran & Hintzman, 1995, 1997; Hintzman & Curran, 1997; Jacoby & Shrout, 1997). Evidence suggests that aggregation independence may often be violated; hierarchical extension of the PD model have been proposed to address this problem (Rouder, Lu, Morey, Sun, & Speckman, 2008).

Third, it is assumed that both the controlled and automatic processes are *invariant* across the inclusion and exclusion instructions. This is reflected in the PD equations by the use of a single parameter C instead of separate parameters for inclusion and exclusion; in other words, the PD equations represent a simplified model that incorporates the invariance assumption $C = C_{Inclusion} = C_{Exclusion}$. Similarly, the PD equations include only a single parameter A, reflecting the simplifying assumption that the automatic process is invariant across inclusion and exclusion, $A = A_{Inclusion} = A_{Exclusion}$. If the PD instruction affects those cognitive processes, the PD equations do no longer yield valid estimates. Recently, the invariance assumption was indeed found to be violated for the controlled process in three different paradigms (Klauer, Dittrich, Scholtes, & Voss, 2015). The goal of the present study is to test whether the PD model's invariance assumption holds for the generation task.

Invariance assumption: Consequences of violations

Violations of the invariance assumptions may considerably distort parameter estimates and substantive conclusions (e.g., Buchner et al., 1995; Klauer et al., 2015). This is also true for the generation task. Assume first that participants have explicit but no implicit sequence knowledge: In the inclusion task, participants would easily express their knowledge. In the exclusion task, they would strategically generate a different sequence to avoid regular responses in this task. However, they would not notice that this different sequence also contains transitions of the (to-be-avoided) sequence. If this were the case, then explicit knowledge would be more likely to be expressed in the inclusion task than in the exclusion task. This implies that explicit knowledge would successfully lead to improved inclusion performance, but would fail to adequately reduce the rate of regular transitions generated under exclusion instructions. However, because the controlled parameter is assumed to be equal across inclusion and exclusion, this effect would distort estimates of the other parameters; in particular, it would lead to inflated estimates of implicit knowledge. In this case, researchers would erroneously conclude that both explicit and implicit knowledge were present.

Next, assume that participants have acquired implicit but no explicit sequence knowledge. Assume further that they adopt a liberal criterion under the inclusion instruction, allowing them to correctly reproduce a substantial proportion of regular transitions by relying on motor fluency. Under exclusion instructions, lacking explicit and therefore controllable knowledge, they might try to reduce the generation rate of regular transitions by adopting one of several response strategies (e.g., subjective randomness, persevering specific patterns; Stahl, Barth, & Haider, 2015). As a consequence, the acquired implicit knowledge would be expressed to a greater degree under inclusion than under exclusion instructions, a pattern that would typically be interpreted as the presence of explicit knowledge. Taken together, conclusions about the presence or absence of implicit and explicit knowledge, as well as their relative contributions across conditions, may be erroneous if one or both of the invariance assumptions are violated. It is therefore important to test whether these assumptions can be upheld in the investigation of sequence knowledge using the PD approach to the generation task.

Overview of present studies

The present study aimed at testing, in the PD generation task, the invariance assumption for automatic and controlled processes. For this purpose, it was necessary to extend the traditional PD design by orthogonally manipulating explicit and implicit knowledge (see also Klauer et al., 2015). We manipulated *explicit* knowledge by explicitly informing participants, after the SRTT training phase, about a subset of the regular transitions (e.g., 1 out of 6) of the sequence. By presenting information about the transitions *after training* we ensured that participants did not use that information during the SRTT to strategically search for more regular transitions (i.e., we made sure the manipulation did not affect the amount of sequence knowledge acquired during training). We manipulated *implicit* knowledge by varying the amount of regularity present in the SRTT training sequence. For this purpose, we used materials with a mere probabilistic regularity; such materials typically produce robust implicit knowledge in the absence of explicit knowledge.

We then fit an extended process-dissociation model $\mathcal{M}_1$ that allowed for testing the invariance assumption of both the controlled and the automatic process: The model provided us with separate estimates for these processes for both inclusion and exclusion tasks; and we used the differences between these estimates to test the invariance assumption. This model relies on the auxiliary assumptions that each experimental manipulation selectively influenced only one of both processes; these assumptions are tested by goodness-of-fit tests proposed by Klauer (2010). Moreover, in order to justify the auxiliary assumptions, we specified a standard process-dissociation model $\mathcal{M}_2$ that does not enforce the auxiliary assumptions but enforces the invariance assumption; model comparison techniques (DIC; Spiegelhalter, Best, Carlin, & Van Der Linde, 2002) were then used to compare model $\mathcal{M}_1$ and model $\mathcal{M}_2$. If model $\mathcal{M}_1$ is favored over model $\mathcal{M}_2$, this can be taken as evidence in favor of our auxiliary assumptions over the invariance assumption. Finally, instead of aggregating data, we used hierarchical Bayesian extensions of all models (e.g., Klauer, 2010; Rouder & Lu, 2005; Rouder et al., 2008).

The outline of this article is as follows: In Experiment 1, we applied the just-presented method to an SRTT with first-order conditional material. In Experiment 2, we replicated our findings from Experiment 1 and extended them to second-order conditional material. Finally, because Experiments 1 and 2 found a violation of invariance for the controlled process, Experiment 3 explored potential mechanisms underlying this violation of invariance. We discuss the implications of our findings for the validity of the PD model equations as well as ordinal interpretations of findings obtained with the PD procedure as applied to the generation task. Furthermore, we point out directions in which the generation task could be developed to provide an improved measure of implicit and explicit knowledge in sequence learning.

Experiment 1

Experiment 1 tested the invariance assumption for automatic and controlled processes using materials with first-order regularity. We implemented two different levels of implicit knowledge by presenting either random or probabilistic sequences to participants during the SRT task. Orthogonally, we implemented two different levels of explicit knowledge by experimentally inducing such knowledge: After the SRT task, we informed one half of participants about one of the six transitions in the sequence.

Method

Design. The study realized a 2 (*material*: random vs. probabilistic) $\times$ 2 (*explicit knowledge*: no transition revealed vs. one transition revealed) $\times$ 2 (*PD instruction*: inclusion vs. exclusion) $\times$ 2 (*block order*: inclusion first vs. exclusion first) design with repeated measures on the *PD instruction* factor.

Participants. One hundred and twenty-one participants (87 women) aged between 17 and 51 years ($M = 23.7$ years) completed the study. Most were undergraduates from University of Cologne. Participants were randomly assigned to experimental conditions. They received either course credit or 3.50 Euro for their participation.

Materials. We used two different types of material:

- A *random* sequence was randomly generated for each participant anew by drawing with replacement from a uniform distribution of six response locations.
- A *probabilistic* sequence was generated from the first-order conditional sequence $2 - 6 - 5 - 3 - 4 - 1$. With a probability of .6, a stimulus location was followed by the next location from this sequence; otherwise, another stimulus location was randomly chosen from a uniform distribution.

In both materials there were no direct repetitions of response locations. In the random group, there was no "correct" sequence, and transition frequencies varied across persons. To compute the dependent variable in the generation task (i.e., the proportion of rule-adhering or regular transitions), we used the generating sequence for participants who worked on *probabilistic* material; for participants who worked on *random* material, we determined an individual criterion for each participant based on their individual transition frequencies during learning: For each participant, the sequence that best fitted the transitions observed by that participant during the *acquisition* phase served as a criterion for the *generation* phase. For the group that was instructed about a regular transition, this *criterion sequence* also contained the revealed transition.

Procedure. The experiment consisted of three consecutive parts: Participants first worked on a SRTT (the *acquisition task*), followed by a *generation task* and, finally, a debriefing phase. In the acquisition task, participants performed a SRTT consisting of 8 blocks with 144 trials each (for a total of 1,152 responses). SRTT and generation task were run on 17" CRT monitors (with a screen resolution of 1,024 px × 768 px). The viewing distance was approximately 60 cm. A horizontal sequence of six white squares (56 px) was presented on a gray screen. The distance between squares was 112 px. Each screen location corresponded to a key on a QWERTZ keyboard (from left to right Y, X, C, B, N, M). Participants had to respond whenever a square's color changed from white to red by pressing the corresponding key. They were instructed to place the left ring-, middle- and index fingers on the keys Y, X and C. The right index-, middle- and ring fingers were to be placed on keys B, N and M. There was no time limit for responses in the learning phase (nor in the generation phase). A warning beep indicated an incorrect response. The response-stimulus interval (RSI) was 250 ms.

Following the SRTT phase, participants were told that stimulus locations during the SRTT followed an underlying sequential structure (but were not informed about the exact sequence). They were then asked to try to generate a short sequence of six locations that followed this structure.

Before working on practice blocks, one transition was revealed to one half of the participants. They were told to memorize that transition and to use this knowledge in the following tasks.

The generation task contained a counterbalanced order of inclusion versus exclusion blocks. Under inclusion (exclusion) instructions, participants were told to generate a sequence as similar (dissimilar) as possible to the sequence from the acquisition task. For both task, participants were instructed to follow their intuition if they had no explicit knowledge about the underlying sequence. Participants who had received information about a transition were instructed to include (exclude) the revealed transition.

To familiarize participants with both inclusion and exclusion instructions, they worked on short practice blocks of twelve consecutive responses. Prior to the inclusion task, two practice blocks involved inclusion instructions; prior to the exclusion task, the first practice block was performed under inclusion instructions and the second practice block was performed under exclusion instructions. If participants who were explicitly informed about one transition failed to include (exclude) the revealed transition in practice blocks, they were informed that they did something wrong; the already revealed transition was again presented and two additional practice blocks had to be performed. This procedure was repeated until the revealed transition was successfully included (excluded) in two consecutive practice blocks. In the main block of the generation task, participants freely generated 120 consecutive response locations. Question marks appeared at all locations and participants' key presses were reflected by the corresponding square's color changing to red. Direct repetitions were explicitly discouraged and were followed by a warning beep.

Upon completing the computerized task, participants were asked to complete a questionnaire containing the following items (translated from German): (1) "One of the tasks mentioned a sequence in which the squares lit up during the first part of the study. In one of the experimental conditions, the squares did indeed follow a specific sequence. Do you think you were in this condition or not?", (2) "How confident are you (in %)?", and (3) "Can you describe the sequence in detail?". Subsequently, participants were asked to indicate, for each of the six response keys, the next key in the sequence on a printed keyboard layout and to indicate how confident they were in this decision. Finally, participants were thanked and debriefed.

Data analysis. All analyses were performed using the R software (R Core Team, 2016) and Stan (Carpenter et al., in press). For the model-based analyses, we used hierarchical Bayesian extensions of the process-dissociaton model (Klauer, 2010; Rouder & Lu, 2005; Rouder et al., 2008). The first level of this hierarchical model extended the traditional process-dissociation model by allowing for a violation of the invariance assumption: The controlled and automatic processes were allowed to vary as a function of instruction (inclusion vs. exclusion),

$$I_{ij} = C_{ijm} + (1 - C_{ijm})A_{ijm}, m = 1$$
$$E_{ij} = (1 - C_{ijm})A_{ijm}, m = 2$$

where i indexes participants, j indexes transition type (i.e., revealed: $j = 1$; nonrevealed: $j = 2$), and m indexes the *PD instruction* condition (inclusion: $m = 1$; exclusion: $m = 2$).

Parameters C_{ijm} and A_{ijm} are probabilities in the range between zero and one; following previous work (e.g. Albert & Chib, 1993; Klauer et al., 2015; Rouder et al., 2008), we used a probit function to link these probabilities to the second-level parameters as follows:

$$C_{ijm} = \begin{cases} \Phi(\mu^{(C)}_{km} + \delta^{(C)}_{im}) & \text{if } j = 1 \text{ (item has been revealed)} \\ 0 & \text{if } j = 2 \text{ (item has not been revealed)} \end{cases}$$

and

$$A_{ijm} = \Phi(\mu^{(A)}_{jkm} + \delta^{(A)}_{ijm})$$

where Φ denotes the standard normal cumulative distribution function, $\mu^{(C)}_{km}$ is the fixed effect of material k (that participant i worked on during the SRTT) and *PD instruction* condition m on controlled processes. $\delta^{(C)}_{im}$ is the ith participant's deviation from his or her group's mean.

Accordingly, $\mu_{jkm}^{(A)}$ is the fixed effect of transition type j, material k, and *PD instruction* condition m on automatic processes, and $\delta_{ijm}^{(A)}$ is the ith participant's deviation from the corresponding mean. Priors on parameters are given in the Appendix.

Note that this specification imposes two auxiliary assumptions to the model: First, it is assumed that controlled processes C are set to zero for nonrevealed transitions (i.e., $C = 0$ for $j = 2$), in other words, we assumed that no explicit knowledge has been acquired during the SRT phase. Second, it is assumed that automatic processes A do not vary as a function of the between-subjects manipulation of explicit knowledge l (i.e., $A_{l=1} = A_{l=2}$). These assumptions allowed us to relax and test the invariance assumption by obtaining separate estimates of both C and A for the inclusion and exclusion conditions (note that a *full* model relaxing all three assumptions cannot be estimated).

To assess goodness of fit, we used posterior predictive model checks as proposed by Klauer (2010): Statistic T_{A1} summarizes how well the model describes the individual category counts for the eight categories (revealed vs. nonrevealed transitions $\times$ correct vs. incorrect $\times$ inclusion vs. exclusion). Statistic T_{B1} summarizes how well the model describes the covariations in the data across participants.

Additionally, we also estimated a model $\mathcal{M}_2$ that does not impose the auxiliary assumptions but enforces the invariance assumptions (i.e., parameters were not allowed to vary as a function of PD instruction condition m):

$$I_{ij} = C_{ij} + (1 - C_{ij})A_{ij}$$

$$E_{ij} = (1 - C_{ij})A_{ij}$$

The second-level equations of model $\mathcal{M}_2$ are then given by:

$$C_{ij} = \Phi(\mu_{jkl}^{(C)} + \delta_{ij}^{(C)})$$

and

$$A_{ij} = \Phi(\mu_{jkl}^{(A)} + \delta_{ij}^{(A)})$$

where i indexes participants, j indexes transition type, k indexes the learning material that participant i worked on during the SRTT, and l indexes the manipulation of explicit knowledge (i.e., whether or not a transition has been revealed to participant i). Note that, given this model specification, separate parameters are estimated for each between-subjects condition kl and each transition type j, while the invariance assumption is maintained (i.e., there is no index m for *PD instruction* in the model equations).

These two models were compared using the deviance information criterion DIC (Spiegelhalter et al., 2002); if model $\mathcal{M}_1$ outperforms model $\mathcal{M}_2$, it can be concluded that the auxiliary assumptions are less problematic than the invariance assumptions. Furthermore, model $\mathcal{M}_1$ yields separate estimates of controlled and automatic processes for both inclusion and exclusion. The invariance assumption can be targeted directly by calculating the posterior differences $A_I - A_E$ and $C_I - C_E$: If the posterior distributions of these differences include zero, it can be concluded that the respective invariance assumption holds; if the posterior does not contain zero, it can be concluded that the respective invariance assumption is violated.

Results

We first analyzed the performance data from the SRT task to determine whether sequence knowledge had been acquired during the task. Next, we analyzed generation task performance using hierarchical PD models.

Acquisition task. If participants acquired knowledge about the (probabilistic) regularity underlying the sequence of key presses, we expect a performance advantage for regular over irregular transitions, reflected in reduced RT and/or error rate. If this advantage is due to learning, it is expected to increase over SRTT blocks.

Reaction times. For RT analyses, we excluded the first trial of each block because the first location cannot be predicted, as well as error trials, trials succeeding an error, reactions faster than 50 ms and slower than 1,000 ms. Figure 1 shows reaction times during the SRTT.

We conducted a 2 (*Material*: Random vs. Probabilistic) $\times$ 8 (*Block number*) $\times$ 2 (*FOC transition status*: regular vs. irregular) ANOVA that revealed a main effect of *material*, $F(1, 119) = 8.11$, $MSE = 39,617.25$, $p = .005$, $\eta^2_G = .055$; a main effect of *block number* $F(4.89, 582.06) = 33.35$, $MSE = 1,032.91$, $p < .001$, $\eta^2_G = .029$; a main effect of *FOC transition status*, $F(1, 119) = 125.46$, $MSE = 714.88$, $p < .001$, $\eta^2_G = .016$; an interaction of *material* and *FOC transition status*, $F(1, 119) = 121.57$, $MSE = 714.88$, $p < .001$, $\eta^2_G = .015$; an interaction of *block number* and *FOC transition status*, $F(6.32, 752.52) = 10.68$, $MSE = 197.96$, $p < .001$, $\eta^2_G = .002$; and a three-way interaction between *material*, *block number*, and *FOC transition status*, $F(6.32, 752.52) = 5.70$, $MSE = 197.96$, $p < .001$, $\eta^2_G = .001$.

Separate ANOVAs for each *material* condition yielded, for random material, only a significant main effect of *block number*, $F(4.38, 258.47) = 13.09$, $MSE = 1,276.78$, $p < .001$, $\eta^2_G = .026$, with RTs decreasing over blocks (all other Fs < 1). For probabilistic material, in contrast, we obtained main effects of *block number*, $F(5.07, 304.28) = 22.09$, $MSE = 891.30$, $p < .001$, $\eta^2_G = .035$; and of *transition status*, $F(1, 60) = 182.32$, $MSE = 976.60$, $p < .001$, $\eta^2_G = .061$ (i.e. responses to regular transitions were faster than those for irregular transitions); importantly, we also obtained an interaction of *block number* and *transition status*, $F(5.93, 356.02) = 15.83$, $MSE = 194.03$, $p < .001$, $\eta^2_G = .007$, showing that the RT difference between regular and irregular transitions increased over blocks, indicating learning of the regularities inherent in the probabilistic material.

Error rates. For analyses of error rates, we excluded the first trial of each block. Figure 2 shows error rates during acquisition. We conducted a 2 (*Material*: Random vs. Probabilistic) $\times$ 8 (*Block number*) $\times$ 2 (*FOC transition status*: regular vs. irregular) ANOVA that revealed a main effect of *block number*, $F(5.83, 693.83) = 6.06$, $MSE = 11.83$, $p < .001$, $\eta^2_G = .016$, indicating that error rates increased over blocks, and a main effect of *FOC transition status*, $F(1, 119) = 38.19$, $MSE = 13.49$, $p < .001$, $\eta^2_G = .019$, indicating that error rates were higher for nonregular transitions. The interaction of *material* and *FOC transition status* was also significant, $F(1, 119) = 27.61$, $MSE = 13.49$, $p < .001$, $\eta^2_G = .014$, reflecting the finding that the effect of the latter factor was limited to the probabilistic material. The three-way interaction of *material*, *block number*, and *FOC transition status* was however not significant, $F(6.55, 778.97) = 1.84$, $MSE = 7.94$, $p = .082$, $\eta^2_G = .004$.

To disentangle these interactions, we analyzed both *material* groups separately. As for RT, an ANOVA for the random material group revealed only a main effect of *block number*, $F(4.94, 291.45) = 2.50$, $MSE = 16.03$, $p = .031$, $\eta^2_G = .013$ (all other Fs < 1). The probabilistic material group showed a main effect of *block number* $F(5.73, 343.65) = 4.63$, $MSE = 10.29$, $p < .001$, $\eta^2_G = .022$, and a main effect of *FOC transition status*, $F(1, 60) = 62.50$, $MSE = 14.23$, $p < .001$, $\eta^2_G = .070$. Importantly, the interaction of *block number* and *FOC transition status* was significant, $F(5.9, 353.81) = 3.23$, $MSE = 7.85$, $p = .004$, $\eta^2_G = .012$, indicating that the difference in error rates between regular and irregular transitions increased across blocks, consistent with the learning effect obtained for reaction times.

Generation task. In a second step, we investigated how learned knowledge was expressed in the generation task. We analyzed generation performance by fitting two hierarchical models, $\mathcal{M}_1$ and $\mathcal{M}_2$. $\mathcal{M}_1$ allows the automatic and controlled processes to vary between inclusion and exclusion, but it assumes that participants acquired only implicit knowledge during the SRTT, and that revealing explicit knowledge after the SRTT did not affect implicit knowledge. $\mathcal{M}_2$ is a hierarchical extension of the classical PD model that enforces the invariance assumption. We computed model fit statistics to test whether each model could account for the means, T_{A1}, and covariances, T_{B1}, of the observed frequencies. We compared both models using the DIC statistic that provides a combined assessment of parsimony and goodness of fit and penalizes models for unnecessary complexity. Parameter estimates from model $\mathcal{M}_1$ were used to address the invariance assumptions, directly. The first trial of a block as well as any response repetitions were excluded from all generation task analyses.

The model checks for model $\mathcal{M}_1$ were satisfactory,

$$T_{A1}^{observed} = 491.06, T_{A1}^{expected} = 469.94, p = .369,$$

$$T_{B1}^{observed} = 9.05, T_{B1}^{expected} = 6.95, p = .366.$$

In contrast, the model checks for model $\mathcal{M}_2$ revealed significant deviations of the model's predictions from the data,

$$T_{A1}^{observed} = 1,092.06, T_{A1}^{expected} = 473.88, p = .002,$$

$$T_{B1}^{observed} = 190.05, T_{B1}^{expected} = 6.93, p < .001.$$

Model $\mathcal{M}_1$ attained a DIC value of 25,293.45 and clearly outperformed model $\mathcal{M}_2$ that attained a DIC value of 25,891.74. This implies that the auxiliary assumptions we introduced to $\mathcal{M}_1$ were much less problematic than the invariance assumption. Moreover, the standard PD model enforcing the invariance assumption was not able to account for the data.

Figure 3 shows the parameter estimates obtained from model $\mathcal{M}_1$. Figure 4 shows that the invariance assumption for the automatic process was violated with $A_I > A_E$, 95% CI [.01, .03], and a Bayesian $p < .001$ ($p = .360$ for revealed transitions). The invariance assumption for the controlled process was also violated with $C_I > C_E$, 95% CI [.08, .54], and a Bayesian $p = .003$.

Discussion

The experimental manipulations had the expected results: Based on the SRTT results, we can conclude that participants acquired sequence knowledge during learning. In addition, explicit knowledge about one of the six transitions had a clear effect on generation performance for that transition.

The extended process-dissociation model $\mathcal{M}_1$ revealed a violation of the invariance assumptions for both the controlled process (i.e., $C_I > C_E$) and the automatic process (i.e., $A_I > A_E$). Model $\mathcal{M}_1$ rested on two auxiliary assumptions: It was assumed that controlled processes were not affected by learning material, and that automatic processes were not affected by the manipulation of explicit knowledge (i.e., revealing a transition). Both assumptions found support in the current data as they did not harm model fit. Comparing model $\mathcal{M}_1$ to a standard process-dissociation model $\mathcal{M}_2$ that did not impose these assumptions but instead imposed the invariance assumption, model $\mathcal{M}_1$ was strongly favored by the DIC.

Despite the fact that the above auxiliary assumptions could be upheld in model comparison, and that the incorporating model was well able to account for the data, it may nevertheless still be the case that violations have biased parameter estimates. To assess this possibility, we used the questionnaire data to assess the assumption that the controlled process was not affected by learning material (i.e., that participants did not acquire explicit knowledge). Specifically, we excluded any transitions that participants reported in their explicit description of the sequence (while keeping the revealed transitions). If participants had in fact acquired explicit knowledge about nonrevealed transitions during learning, they may have used this knowledge to generate more regular transitions under inclusion than exclusion. Because of our assumption that $C = 0$ for nonrevealed transitions, this performance difference would have been reflected in greater estimates of implicit knowledge under inclusion than exclusion, and might account for the observed $A_I > A_E$ pattern. If the acquired explicit knowledge was indeed the cause of the invariance violation, excluding the transitions for which knowledge was reported should make the violation disappear. To the contrary, excluding all correctly reported transitions (9.04% of cases) did not affect the pattern of results.[1] This confirms the above conclusions that the auxiliary assumptions can

[1]Of the reported (nonrevealed) transitions, only approximately 25.47% were indeed regular transitions. After excluding *all* reported transitions regardless of whether they reflect correct knowledge or not (27.55% of cases), the invariance violation was descriptively unchanged but no longer statistically significant, Bayesian

be upheld.

Taken together, these findings suggest that the invariance assumption was violated for both the automatic and the controlled process. Invariance of the automatic process was significantly violated for nonrevealed but not for revealed transitions. This may be due to the small magnitude of the violation effect and the relatively small number of revealed (as compared to nonrevealed) transitions. The magnitude of the invariance violation was much greater for the controlled process: Explicit knowledge was used to a greater degree under inclusion than exclusion instructions.

Experiment 2

Experiment 1 showed that the invariance assumption was violated for both automatic and controlled processes. The main goal of Experiment 2 was to replicate the previous findings and extend them to second-order conditional (SOC) material.

A secondary goal was to explore whether different amounts of implicit knowledge are acquired with *mixed* versus *pure* SOC material. Previous studies of the SRTT using a PD generation task have employed a 12-item-sequence of four response locations (e.g., Destrebecqz & Cleeremans, 2001; Wilkinson & Shanks, 2004). Analyzing these sequences more closely, it is evident that they did not only contain second order information (i.e., the last two locations predict the next location), but they also incorporate lower-order information: Direct repetitions never occur, reversals occur below chance (i.e., 1/12, whereas chance level would equal 1/3 given that repetitions are prohibited), and the last location of a triplet L_3 is not independent of the first location L_1 (e.g., for SOC1, $p(L_3 = 2|L_1 = 3) = 2/3$). In other words, in two out of three cases, the third location of a triplet can be predicted by the first location of a triplet alone. It is plausible that participants are able to learn this lower-order information, and that learning effects may not (only) be based on second-order information (cf., Reed & Johnson, 1994). To investigate this

$p = .221$.

possibility, Experiment 2 implemented two types of probabilistic material: A *mixed SOC* material that incorporated both second-order and lower-order types of information, and another *pure SOC* material that only followed a second-order regularity.

Method

Design. The study realized a 3 (*material*: random, mixed SOC, pure SOC) × 2 (*explicit knowledge*: no transition revealed vs. two transitions revealed) × 2 (*PD instruction*: inclusion vs. exclusion) × 2 (*block order*: inclusion first vs. exclusion first) design with repeated measures on the *PD instruction* factor.

Participants. One hundred and seventy-nine participants (120 women) aged between 18 and 58 years ($M = 22.8$ years) completed the study. Most were undergraduates from Heinrich-Heine-Universität Düsseldorf. Data from 8 participants were excluded from generation task analyses because they had received erroneous exclusion instructions. Participants were randomly assigned to experimental conditions. They received either course credit or 3.50 Euro for their participation.

Materials. We implemented three different types of material:

- A *random* sequence was randomly generated for each participant anew by drawing with replacement from a uniform distribution of six response locations.
- A *mixed SOC* sequence incorporated two types of information: First, the third location of a triplet was conditional upon the first two locations. Second, within such regular triplets, given a fixed first-position location, there was one highly probable third-position location and two somewhat less probable third-position locations; the other three response locations never occurred for this first-position location.
- A *pure SOC* sequence followed only the second-order regularity.

In both probabilistic materials (*mixed* and *pure* SOC), 87.5% of trials adhered to the second-order regularity, which was individually and randomly selected for each participant anew. In all conditions, the material adhered to the following (additional) restrictions: (1)

there were no direct repetitions of response locations, and (2) there were no response location reversals (i.e., 1-2-1). To compute the dependent variable in the generation task (i.e., the number of rule-adhering triplets), for both *probabilistic* groups, we used the second-order sequence that was used to generate each participant's materials. For the *random* group, there is no "correct" sequence and we again computed an individual criterion sequence for each participant. For convenience, we did not generate all possible second-order sequences for these participants (as we did for first-order materials in Experiment 1), but chose to use individual criterion sequences that were randomly generated similar to the *pure SOC* material.

Procedure. The experimental procedure closely followed that of Experiment 1: In the acquisition task, participants performed a SRTT consisting of 8 blocks with 180 trials each (for a total of 1,440 responses). The response-stimulus interval (RSI) was 0 ms. Following the SRTT phase, participants were told that stimulus locations during the SRTT followed some underlying sequential structure. They were then asked to try to generate a short sequence of thirty locations that followed this structure.

The generation task followed, with inclusion vs. exclusion block order counterbalanced. Deviating from Experiment 1, we fixed the number of practice blocks that preceded both inclusion and exclusion task: Prior to the inclusion task, three practice blocks involved inclusion instructions; prior to the exclusion task, the first and second practice block involved inclusion instructions, and the third involved exclusion instructions. Before working on practice blocks, two transitions were revealed to one half of the participants.

Upon completing the computerized task, participants were asked to complete a questionnaire containing the following items: (1) "Did you notice anything special working on the task? Please mention anything that comes to your mind.", (2) "One of the tasks mentioned a sequence in which the squares lit up during the first part of the study. In one of the experimental conditions, the squares did indeed follow a specific sequence. Do you think you were in this condition or not?", (3) "How confident are you (in %)?", (4) "Can you describe the sequence in detail?". Subsequently, participants were asked to indicate, for ten first-order transitions, the next three keys in the sequence on a printed keyboard layout. The first-order transitions were individually selected for each participant so that each participant had the chance to express full explicit knowledge about the second-order regularity.

Data analysis

For the model-based analyses, models $\mathcal{M}_1$ and $\mathcal{M}_2$ were analogous to those used in Experiment 1 (see Appendix for detail).

Results

We first analyzed reaction times and error rates during the SRT task to determine whether sequence knowledge had been acquired during the task. Next, we analyzed generation task performance using hierarchical PD models.

Acquisition task. If participants acquired sequence knowledge from probabilistic materials, we expect a performance advantage for regular over irregular transitions, reflected in reduced RT and/or error rate. If this advantage is due to learning, it is expected to increase over SRTT blocks. If participants are able to learn lower-order information that is only present in *mixed SOC* material, the advantage is expected to be greater in *mixed SOC* material compared to *pure SOC*. If participants are able to learn second-order information, a performance advantage is to be expected not only in *mixed SOC* but also in *pure SOC* material.

Reaction times. For all RT analyses, we excluded the first two trials of each block because the first two locations cannot be predicted, as well as error trials, trials succeeding an error, reactions faster than 50 ms and slower than 1,000 ms. Figure 5 shows reaction times during acquisition.

We conducted a 3 (*Material*: random vs. pure SOC vs. mixed SOC) × 2 (*Transition status*: regular vs. irregular SOC) × 8 (*Block number*) ANOVA with repeated measures on the last two factors that revealed a main effect of *block number*, $F(4.46, 780.51) = 41.53$,

$MSE = 1,515.93$, $p < .001$, $\eta^2_G = .020$, reflecting decreasing RT over blocks; a main effect of *transition status*, $F(1, 175) = 40.02$, $MSE = 582.10$, $p < .001$, $\eta^2_G = .002$, reflecting an RT advantage for regular transitions; and an interaction of *block number* and *transition status*, $F(6.39, 1118.42) = 2.81$, $MSE = 439.60$, $p = .009$, $\eta^2_G = .001$, reflecting the finding that the RT advantage for regular transitions increased over block (i.e., the sequence learning effect). We also found an interaction of *material* and *transition status*, $F(2, 175) = 7.40$, $MSE = 582.10$, $p = .001$, $\eta^2_G = .001$, reflecting the finding that the effect of *transition status* was absent in the random material group, $F(1, 58) = 0.44$, $MSE = 380.19$, $p = .510$, $\eta^2_G = .000$; trivially, no sequence knowledge was learned from random material.

The three-way interaction was not significant, $F(12.78, 1118.42) = 0.92$, $MSE = 439.60$, $p = .535$, $\eta^2_G = .000$, suggesting that the sequence-learning effect did not differ across material groups. We conducted separate analyses to probe for sequence-learning effects in each material condition. Analyzing only the random material group revealed only a main effect of *block number*, $F(3.82, 221.55) = 15.74$, $MSE = 1,484.04$, $p < .001$, $\eta^2_G = .020$ (all other ps $> .05$). In the *pure SOC* group, in contrast, a main effect of *block number*, $F(3.96, 229.51) = 12.04$, $MSE = 2,038.65$, $p < .001$, $\eta^2_G = .019$, was accompanied by a main effect of *transition status*, $F(1, 58) = 28.48$, $MSE = 637.73$, $p < .001$, $\eta^2_G = .004$, and an interaction of both factors, $F(6.03, 349.61) = 2.47$, $MSE = 530.13$, $p = .023$, $\eta^2_G = .002$, reflecting a sequence learning effect on RT.

In the *mixed SOC* group, we obtained only main effects of *block number*, $F(4.91, 289.7) = 15.95$, $MSE = 1,334.22$, $p < .001$, $\eta^2_G = .024$, and of *transition status*, $F(1, 59) = 18.83$, $MSE = 725.90$, $p < .001$, $\eta^2_G = .003$, but the interaction of *block number* and *transition status* was not significant, $F(5.74, 338.77) = 1.15$, $MSE = 571.40$, $p = .331$, $\eta^2_G = .001$. This is despite the fact that the effect of transition status is also likely to be a result of sequence learning, and it is of similar magnitude to that obtained in the pure SOC group. The notion that both learning effects are similar was also supported by a joint analysis of the pure SOC and mixed SOC groups: The two-way interaction between block number and transition status was significant, $F(6.39, 1118.42) = 2.81$, $MSE = 439.60$, $p = .009$, $\eta^2_G = .001$, but the three-way-interaction of *material*, *block number*, and *transition status* was not significant, $F(12.78, 1118.42) = 0.92$, $MSE = 439.60$, $p = .535$, $\eta^2_G = .000$. Taken together, we interpret these findings to show that the learning effect in the mixed SOC group was comparable to that observed in the pure SOC group but too small to reach significance in a separate analysis.

Error rates. For all analyses of error rates, we excluded the first two trials of each block. Figure 6 shows error rates during acquisition. We conducted a 3 (*Material*: Random vs. mixed SOC vs. pure SOC) $\times$ 8 (*Block number*) $\times$ 2 (*SOC transition status*: regular vs. irregular) ANOVA with repeated measures on the last two factors that revealed a main effect of *block number*, $F(3.66, 644.87) = 3.78$, $MSE = 39.10$, $p = .006$, $\eta^2_G = .008$, reflecting increasing error rates over blocks, and a main effect of *transition status*, $F(1, 176) = 16.14$, $MSE = 9.08$, $p < .001$, $\eta^2_G = .002$, reflecting an accuracy advantage for regular transitions. The interaction of *material* and *transition status* was not significant, $F(2, 176) = 2.66$, $MSE = 9.08$, $p = .073$, $\eta^2_G = .001$,

Separate analyses yielded no significant effects in the random material group (all ps $>$ .05). Importantly, an effect of *transition status* was clearly absent from the random material group, $F(1, 58) = 0.62$, $MSE = 7.68$, $p = .433$, $\eta^2_G = .000$. In the *mixed SOC* group, a main effect of *block number* was found, $F(5.66, 334.01) = 2.96$, $MSE = 15.46$, $p = .009$, $\eta^2_G = .017$, along with a main effect of *transition status*, $F(1, 59) = 12.88$, $MSE = 11.29$, $p = .001$, $\eta^2_G = .009$, reflecting higher error rates for irregular than for regular transitions. Finally, in the *pure SOC* group, block number did not affect error rates, $F(1.87, 110.6) = 1.72$, $MSE = 133.60$, $p = .185$, $\eta^2_G = .011$; but a main effect of *transition status* was also found, $F(1, 59) = 5.55$, $MSE = 8.24$, $p = .022$, $\eta^2_G = .001$, reflecting higher error rates for irregular than regular transitions.

Taken together, error rates mirror RTs in that they also reflect a performance advantage for regular transitions in the mixed and pure SOC groups that was not evident in the random control group. Deviating from the RT result pattern, this advantage did not reliably increase across blocks.

Generation task. We analyzed generation performance by fitting the two hierarchical models $\mathcal{M}_1$ and $\mathcal{M}_2$ that we introduced above to the data from Experiment 2. For both models, we computed model fit statistics to assess whether each model could account for the data; we then compared both models using the DIC. Parameter estimates from model $\mathcal{M}_1$ were then used to address the invariance assumptions directly. The first two trials of a block as well as any response repetitions and reversals were excluded from all generation task analyses.

The model checks for model $\mathcal{M}_1$ were satisfactory,

$$T_{A1}^{observed} = 692.77, T_{A1}^{expected} = 653.45, p = .291,$$

$$T_{B1}^{observed} = 8.44, T_{B1}^{expected} = 6.04, p = .292.$$

In contrast, the model checks for model $\mathcal{M}_2$ revealed significant deviations of the model's predictions from the data,

$$T_{A1}^{observed} = 1,077.52, T_{A1}^{expected} = 652.79, p = .003,$$

$$T_{B1}^{observed} = 49.97, T_{B1}^{expected} = 6.06, p < .001.$$

Model $\mathcal{M}_1$ attained a DIC value of 38,907.43 and outperformed model $\mathcal{M}_2$ that attained a DIC value of 39,210.66. This implies that our auxiliary assumptions that we introduced to make model $\mathcal{M}_1$ identifiable (i.e., that participants did not acquire explicit knowledge during training, and that revealing explicit knowledge about a transition did not affect implicit knowledge) were less problematic than the invariance assumption. Moreover, the standard PD model enforcing the invariance assumption was not able to account for the data.

Figure 7 shows the parameter estimates obtained from model $\mathcal{M}_1$. Figure 8 shows that the invariance assumption for controlled processes was again violated with $C_I > C_E$, 95% CI [.27, .63], Bayesian $p < .001$. The invariance assumption for automatic processes could be upheld, 95% CI [-.01, .01], Bayesian $p = .638$ for non-revealed transitions and 95% CI [-.10, .05], $p = .763$ for revealed transitions.

Discussion

The experimental manipulations had the expected results: Based on the SRTT results, we can conclude that participants acquired some (albeit weak) sequence knowledge during learning. In addition, generation performance was clearly affected by instructed explicit knowledge.

An extended process-dissociation model $\mathcal{M}_1$ revealed a violation of the invariance assumption for controlled processes with $C_I > C_E$. The invariance assumption for automatic processes could be upheld. Model $\mathcal{M}_1$ rested on two auxiliary assumptions: It was assumed that controlled processes were not affected by learning material, and that automatic processes were not affected by the manipulation of explicit knowledge (i.e., revealing a transition). Both assumptions found support in the current data as they did not harm model fit. Importantly, comparing model $\mathcal{M}_1$ to a standard process-dissociation model $\mathcal{M}_2$ that did not impose these assumptions but left the invariance assumption intact, model $\mathcal{M}_1$ was strongly favored by the DIC.

Regarding our secondary goal to explore whether different amounts of sequence knowledge are acquired from mixed versus pure second-order conditional material, we did not find evidence for a difference between these two types of material in the SRTT. This may well be due to the overall low levels of acquired sequence knowledge in the present study. Clearly, the present data are not strong enough to rule out such differences; this question requires further study.

Experiment 3

Experiments 1 and 2 found a violation of the invariance assumption, suggesting that the interpretation of the parametric PD approach may be problematic. In particular, the results of Experiments 1 and 2 consistently suggest that the PD parameters may not yield an exhaustive measure of explicit knowledge: The degree to which participants made use of their explicit knowledge varied between the inclusion and exclusion tasks. Experiment 3 aimed at obtaining a better understanding of this invariance violation and its effect on the interpretation of generation task performance in the PD framework.

Whereas the invariance violations clearly threaten the validity of the PD model, they may yet turn out to be uncritical for an ordinal interpretation of PD data that has often been used in applications (i.e., a comparison of inclusion versus exclusion performance, and of exclusion versus baseline performance). Even if the (independence and) invariance assumptions do not hold, the general approach of drawing conclusions about the underlying processes by comparing performance between inclusion and exclusion conditions may not be entirely invalidated: It has been formally shown that an ordinal interpretation of PD findings does not rely on parametric assumptions (Hirshman, 2004). However, the ordinal PD approach does assume that baseline performance is identical in the inclusion and exclusion tasks – an assumption that has been shown to be violated at least in some cases (Stahl et al., 2015). More critically for the present question of invariance, it also assumes (not only that inclusion performance increases but also) that exclusion performance monotonically decreases with increasing explicit knowledge. Note that, if (contrary to this assumption) explicit knowledge does not affect exclusion performance at all, the ordinal PD approach may technically still be used. However, the results would be misleading if a difference in explicit (but not implicit) knowledge between two conditions led to a difference in inclusion but not in exclusion performance. In this case, the ordinal PD would suggest that the two conditions differ in explicit *and implicit* knowledge (Hirshman, 2004, Data Pattern I). In other words, for the ordinal PD approach to yield valid results, exclusion performance must fall below baseline when explicit knowledge is present (this would yield Hirshman's Data Pattern IV, which indicates an increase in explicit knowledge). Therefore, a critical empirical test for the ordinal PD approach is whether, and under which conditions, participants are able to use explicit knowledge to suppress generation below baseline levels under exclusion conditions.

A critical precondition for the expression of explicit knowledge may be the opportunity for practice with the generation task. Experiment 3 investigated the effect of practice on the expression of explicit knowledge: In Experiments 1 and 2, participants were given the opportunity to practice inclusion and exclusion of the instructed explicit knowledge, but nevertheless failed to approach ceiling (floor) performance in the inclusion (exclusion) conditions. In Experiment 3, we investigated whether such transition-specific practice can help reduce the invariance violation by comparing practiced with non-practiced transitions. We also wanted to more directly investigate transfer of practice to unpracticed transitions about which participants had explicit knowledge. We therefore manipulated the number of revealed transitions, and whether these revealed transitions were revealed prior to or after practice blocks, both between and within subjects. We realized five between-subjects conditions:

1. In the *Control* group, no explicit knowledge was revealed to participants.

2. In the *No-Practice* group, one transition was revealed immediately before the first generation block, but *after* the practice blocks that preceded the first generation block. To avoid carry-over of practice effects from the first generation block, a different non-practiced transition was revealed after the second practice blocks and immediately preceding the second generation block.

3. In the *Unspecific-Practice* group, one transition was revealed to participants *after* practive, immediately before each generation block (as in the *No-Practice* group). In the third practice block before the exclusion task, participants were asked to inhibit a specific response location (i.e., they were asked *not* to use the 5^{th} location/N key).

4. In the *Practice* group, one transition was revealed to participants immediately *before* the practice blocks. Participants were encouraged to include (exclude) the revealed transition during practice and in the generation block.

5. In the *Transfer* group, information about two transitions was revealed; one of them was non-practiced (as in the No-Practice group), the other one practiced (as in the Practice group). The practiced transition was revealed before the first practice blocks. After these practice blocks, the second (non-practiced) transition was revealed immediately before the first generation block. The practiced transition was again named before participants worked on the practice blocks of the second generation phase. After these practice blocks, a second non-practiced transition was revealed immediately before the second generation block.

The Control and Practice groups were identical to Experiment 1; the other groups extended the previous design. This allowed us to assess generation performance for three main transition types; (1) *non-revealed* transitions, (2) transitions that were revealed but remained *non-practiced*, and (3) transitions that were revealed and *practiced* in the practice blocks.[2]

A comparison of *non-revealed* with (revealed but) *non-practiced* transitions allows us to assess the degree to which participants can spontaneously make use of their explicit knowledge in the generation task. Comparing *non-practiced* with *practiced* transitions allowed us to assess whether specific inclusion/exclusion practice could increase the use of explicit knowledge to a comparable level in the inclusion and exclusion tasks (i.e., eliminate the violation of invariance). We also compared whether performance for revealed but

[2]In the second generation block of the No-Practice, Unspecific-Practice, and Transfer groups, a fourth transition type can be distinguished: Transitions that were revealed but non-practiced before the first generation block. Because participants included (or excluded) these transitions in the previous (i.e., the first) generation block, performance on these transitions should be more similar to practiced than to non-practiced transitions in the second block.

non-practiced transitions differs between the No-Practice and Transfer groups, as would be expected if the effect of specific practice transfers to non-practiced explicit knowledge. Finally, we explored whether, in the Unspecific-Practice group, unspecific inhibition practice affects performance for both revealed but *non-practiced* and/or *non-revealed* transitions.

In sum, we hypothesized that, as in Experiment 1, the invariance assumption for the controlled process would be violated. Relatedly, we aimed at replicating the finding that possessing explicit knowledge would not be sufficient for its expression in the generation task. Specifically, (a) explicit knowledge without practice (*No-Practice* group) should not lead to below-chance exclusion performance, and (b) this should also hold for non-practiced transitions for participants who practiced another transition (*Transfer* group). We had no clear hypothesis regarding the unspecific response-inhibition practice, but wanted to explore whether it would be as effective as transition-specific exclusion practice in improving the validity of the generation task as a measure of explicit knowledge. Finally, Experiment 3 addresses a possible artifactual effect of practice on the invariance violation. Specifically, the practice blocks administered in Experiments 1 and 2 might have *caused* the invariance violation. If this was the case, then invariance should not be violated in the absence of such practice.

Method

Design. The study realized a 5 (*Practice group*: Control, No-Practice, Unspecific-Practice, Practice, Transfer) × 2 (*PD instruction*: inclusion vs. exclusion) × 2 (*block order*: inclusion first vs. exclusion first) design with repeated measures on the *PD instruction* factor.

Participants. One hundred and forty-seven participants (113 women) aged between 17 and 55 years ($M = 23.7$ years) completed the study. Most were undergraduates from Heinrich-Heine-Universität Düsseldorf. Participants were randomly assigned to experimental conditions. They received either course credit or 3.50 Euro for their participation.

Materials and Procedure. The experimental procedure closely followed Experiment 1. During an SRTT consisting of 8 blocks with 144 trials each (for a total of 1,152 responses), participants in all conditions were trained with a *probabilistic* sequence similar to the one used in Experiment 1. After the SRTT, participants were informed about the underlying sequential structure of stimulus locations and asked to generate a short sequence of six key presses that followed this (unspecified) structure.

The generation task followed, with counterbalanced order of inclusion versus exclusion blocks. The number of practice blocks was held constant (in contrast to Experiment 1, where it depended on performance). Upon completing the computerized task, participants answered the same questionnaire as in Experiment 1.

Data analysis. Given that model $\mathcal{M}_2$ failed to fit the data from both Experiments 1 and 2 and model, we fitted only model $\mathcal{M}_1$ and used posterior analyses to evaluate the invariance assumption. For the model-based analyses, we adapted the equations from Experiment 1 to the design of Experiment 3 (which did not contain experimental groups with random material).

In order to accommodate for the more complex design, we used a model specification that allowed for participant and item (i.e., transition) effects and their interactions by estimating fixed effects for each transition type plus individual participants' deviations from these effects. The model equations of model $\mathcal{M}_1$ are given by:

$$C_{ijm} = \begin{cases} \Phi(\mu^{(C)}_{jlm} + \delta^{(C)}_{ijm}) & \text{if } j \epsilon 1, 2 \text{ (item has been revealed \& practiced, revealed \& non-practiced)} \\ 0 & \text{if } j = 3 \text{ (item has not been revealed)} \end{cases}$$

and

$$A_{imt} = \Phi(\mu^{(A)}_{mt} + \delta^{(A)}_{imt})$$

where $\mu^{(C)}_{jlm}$ is the fixed effect of transition type j (non-revealed, revealed & practiced, revealed & non-practiced) in condition l and *PD instruction* condition m on controlled processes, and $\delta^{(C)}_{ijm}$ is the ith participant's deviation from the corresponding mean.

Accordingly, $\mu_{mt}^{(A)}$ is the fixed effect of *PD instruction* condition m and transition t on automatic processes, and $\delta_{imt}^{(A)}$ is the ith participant's deviation from the corresponding mean.

Model $\mathcal{M}_1$ imposes two auxiliary assumptions: First, it assumed that no explicit knowledge has been acquired during the SRT phase (i.e., $C = 0$ for non-revealed transitions). Second, it assumed that revealing sequence knowledge did not affect automatic processes (i.e., A does not vary as a function of the between-subjects manipulation of explicit knowledge, index l). Both auxiliary assumptions were tested by posterior predictive checks. In addition to reporting T_{A1} and T_{B1} as in the previous studies, we calculated additional model check statistics T_{A2}, which summarizes how well the model describes the item-wise category counts (aggregated over participants), and T_{A3}, which summarizes how well the model describes the category counts per participant-item combination; finally, the additional statistic T_{B2} summarizes how well the model describes the variances and covariances introduced by items. We also calculated the posterior differences $C_I - C_E$ and $A_I - A_E$ to more directly test the invariance assumption.

Results

We first analyzed the performance data from the SRT task in a traditional way to determine whether sequence knowledge had been acquired during the task. Next, we analyzed generation task performance using a hierarchical PD model. Finally, to test our predictions regarding the different effects of practice in a model-free manner we analyzed generation performance for transitions that were revealed.

Acquisition task. If participants acquired knowledge about the regularity underlying the sequence of key presses, we expect a performance advantage for regular over irregular transitions, reflected in reduced RT and/or error rate. If this advantage is due to learning, it is expected to increase over SRTT blocks.

Reaction times. For all RT analyses, we excluded the first trial of each block as well as trials with errors, trials succeeding an error, reactions faster than 50 ms and those slower than 1,000 ms. Figure 9 shows reaction times during acquisition.

We conducted a 8 (*Block number*) × 2 (*FOC transition status*: regular vs. irregular) repeated-measures ANOVA. There was a main effect of *block number*, $F(4.26, 622.43) = 96.37$, $MSE = 1,034.51$, $p < .001$, $\eta^2_G = .059$, with RT decreasing over blocks. There also was a main effect of *FOC transitions status*, $F(1, 146) = 573.39$, $MSE = 841.10$, $p < .001$, $\eta^2_G = .066$, reflecting faster responses to regular than to irregular transitions. The interaction of *block* and *FOC transition status* was also significant, $F(6.47, 945.32) = 58.60$, $MSE = 176.46$, $p < .001$, $\eta^2_G = .010$, reflecting the finding that the RT advantage for regular transitions increased over blocks, which indicated successful sequence learning.

Error rates. For all analyses of error rates, we excluded the first trial of each block. Figure 10 shows error rates during acquisition.

The pattern of findings was similar to that obtained for RT. We conducted an 8 (*Block number*) × 2 (*FOC transition status*: regular vs. irregular) repeated-measures ANOVA that revealed a main effect of *block number*, $F(6.29, 917.65) = 8.35$, $MSE = 9.42$, $p < .001$, $\eta^2_G = .015$, reflecting increasing error rates over blocks; and a main effect of *FOC transition status*, $F(1, 146) = 188.88$, $MSE = 11.92$, $p < .001$, $\eta^2_G = .066$, reflecting an accuracy advantage (i.e., lower error rates) for regular transitions. The interaction of *block number* and *FOC transition status* was also significant, $F(6.53, 953.88) = 7.36$, $MSE = 7.09$, $p < .001$, $\eta^2_G = .011$, reflecting an increase of the accuracy advantage for regular (as compared to irregular) transitions over blocks, indicating successful sequence learning.

Generation task. We analyzed generation performance by fitting $\mathcal{M}_1$ and computed model fit statistics to assess whether the model can account for the data. Parameter estimates from model $\mathcal{M}_1$ were used to address the invariance assumptions, directly. The first trial of a block as well as any response repetitions were excluded from all generation task analyses.

The model checks for model $\mathcal{M}_1$ were satisfactory,

$$T_{A1}^{observed} = 35.97, T_{A1}^{expected} = 33.96, p = .322,$$

$$T_{A2}^{observed} = 0.05, T_{A2}^{expected} = 0.05, p = .480,$$

$$T_{A3}^{observed} = 1,763.79, T_{A3}^{expected} = 1,720.63, p = .372,$$

$$T_{B1}^{observed} = 5.31, T_{B1}^{expected} = 4.62, p = .457,$$

$$T_{B2}^{observed} = 3,852.65, T_{B2}^{expected} = 3,393.90, p = .464.$$

Figure 11 shows the parameter estimates obtained from model $\mathcal{M}_1$; while estimates of the automatic process were only slightly above chance in both *PD instruction* conditions, estimates of the controlled process differ strongly between *PD instruction* conditions.

Figure 12 shows that the invariance assumption for automatic processes was violated with $A_I > A_E$, 95% CI [.00, .03], and Bayesian $p = .008$. For revealed and practiced transitions, the invariance assumption was violated with $C_I > C_E$, 95% CI [.19, .63] and a Bayesian $p = .001$. For revealed but non-practiced transitions, the invariance assumption was violated with $C_I > C_E$, 95% CI [.03, .31] and a Bayesian $p = .005$.

Effects of practice on generation of revealed transitions. To test our predictions regarding the different effects of practice in a model-free manner, we analyzed raw generation frequencies for only those transitions about which explicit knowledge was revealed.

Figure 13 shows generation performance for revealed transitions. A 5 (*Condition*: Control vs. No-Practice vs. Unspecific-Practice vs. Practice vs. Transfer) × 2 (*Order*: Inclusion first vs. Exclusion first) × 2 (*PD instruction*: Inclusion vs. Exclusion) ANOVA revealed a nonsignificant main effect of *Condition*, $F(3, 110) = 2.00$, $MSE = 660.29$, $p = .119$, $\eta^2_G = .028$, but a significant main effect of *PD instruction*, $F(1, 110) = 243.88$, $MSE = 575.67$, $p < .001$, $\eta^2_G = .508$, and their significant interaction, $F(3, 110) = 5.59$, $MSE = 575.67$, $p = .001$, $\eta^2_G = .066$. The main effect of *PD instruction* reflects the clear influence of the instructed explicit knowledge depicted in Figure 13. It is present in all practice conditions but modulated by amount of knowledge and type of practice (i.e., greater effects given specific practice): The effect was greatest in the *Transfer* group, $t(29) = 14.84$, $p < .001$, $d = 2.71$; somewhat smaller in the *Practice* group, $t(28) = 9.79$, $p < .001$, $d = 1.82$; it was still smaller and comparable without practice, *No-practice* group, $t(28) = 5.25$, $p < .001$, $d = 0.97$; or with only unspecific practice, *Unspecific-practice* group, $t(29) = 5.13$, $p < .001$, $d = 0.94$.

We investigated this issue more closely in two sets of follow-up analyses. Whereas the above findings support the hypothesis that practice improves the degree to which explicit knowledge is expressed in the generation task, it does not elucidate the mechanism by which this occurs. One mechanism by which practice may improve performance is by boosting the proportion of regular transitions in inclusion blocks.

Inclusion. Inclusion performance for revealed transitions in the *No-Practice* and *Practice* groups was analyzed as a function of practice (practiced vs. non-practiced), as depicted in Figure 14. Results showed no effect of practice on generation performance, $F(1, 56) = 0.21$, $MSE = 696.48$, $p = .652$, $\eta^2_G = .004$. Similarly, when we compared inclusion performance for practiced vs. non-practiced transitions in the *Transfer* group, there was no effect of practice, $F(1, 29) = 1.19$, $MSE = 365.77$, $p = .285$, $\eta^2_G = .014$. We conclude that practice did not affect inclusion performance for revealed transitions.

Exclusion. Next, we analyzed whether practice improves suppressing the regular transition in the exclusion task. We hypothesized that, without training, participants might not be able to suppress their generation of regular transitions below the chance level in the exclusion task. To test this hypothesis, we compared generation performance for the revealed transitions between the *No-Practice* and *Practice* groups, as depicted in the left panel of Figure 15. The expected below-chance performance was not found in the data from both blocks: Whereas the direction of effects was as expected, there was no deviation from chance, neither for the practice condition, $t(28) = -0.79$, $p = .219$, $d = -0.15$, nor for the no-practice condition, $t(28) = 1.60$, $p = .940$, $d = 0.30$. However, the expected pattern was found when only the first block was analyzed: Below-chance performance was found for the practice condition, $t(14) = -4.89$, $p < .001$, $d = -1.26$, but not for the no-practice condition, $t(13) = 0.18$, $p = .569$, $d = 0.05$.

To more directly establish a practice effect, we next turned to the data from the *Transfer* group for a within-subjects comparison of practiced and non-practiced transitions. In addition, we addressed the transfer hypothesis: If specific training is required for each single transition, the finding of at-chance exclusion performance should replicate for non-practiced transitions in participants who practiced another transition. In contrast, if training on one transition transfers to other transitions, we should find below-chance performance for non-practiced transitions in a parallel within-participants comparison in the *Transfer* group. As can be seen from the right panel of Figure 15, generation performance was below chance for practiced, $t(29) = -9.60$, $p < .001$, $d = -1.75$, as well as for non-practiced transitions, $t(29) = -2.04$, $p = .025$, $d = -0.37$, indicating transfer of exclusion practice from practiced to non-practiced transitions.[3]

Discussion

The experimental manipulations had the expected effects on implicit and explicit sequence knowledge: Participants in Experiment 3 acquired knowledge about the sequence, as expressed in RT- and accuracy advantages for regular transition that increased over SRTT blocks. Participants received different amounts of instructed explicit knowledge, and they

[3]Analyzing only the first block revealed the same pattern of results: Generation performance was below chance for practiced, $t(14) = -5.42$, $p < .001$, $d = -1.40$, as well as for non-practiced transitions, $t(14) = -4.56$, $p < .001$, $d = -1.18$.

were able to express this knowledge in the generation task, as revealed by the effect of *PD instruction* on generation of revealed transitions. However, performance differed across groups (i.e., practice conditions), suggesting that specific exclusion practice was beneficial to implementing PD instructions. Finally, even with practice, inclusion performance did not reach ceiling and exclusion performance did not reach floor levels, indicating that participants were not able to exhaustively express their explicit knowledge in the generation task.

The invariance assumption was again found to be violated for both controlled and automatic processes. Explicit knowledge was expressed to a greater degree in the inclusion than in the exclusion blocks of the generation task. Generation practice improved the degree to which explicit knowledge was expressed under exclusion instructions, but invariance was violated despite repeated opportunities for practicing to include/exclude a specific transition.

Results showed that practice increased the magnitude of the invariance violation (i.e., the I-E difference). Importantly, however, this does not imply that our evidence for invariance violation reflect an artifact of practice. First, increasing the overall expression of explicit knowledge (as suggested by parameter estimates, as well as the below-chance exclusion performance under practice conditions) is of course precisely the intended effect of practice. Second, only with practice were participants sometimes able to suppress exclusion performance below chance baselines (as required by the PD model). Third, invariance was also violated in the absence of practice. We conclude that invariance was violated because, overall, participants were not able to refrain from using their explicit knowledge under exclusion conditions (even if practiced). Perhaps more precisely, participants failed to generate a sufficiently high proportion of irregular transitions under exclusion conditions.

General Discussion

Summary of main findings

Process-dissociation assumes that the controlled and automatic process are invariant under inclusion and exclusion instructions. In three sequence-learning experiments, we tested whether this invariance assumption holds in the generation task. The results show a consistent pattern.

Invariance of the controlled process. The invariance assumption for explicit knowledge was consistently violated, in first-order as well as second-order material, and despite extensive opportunity for (as well as without) practice. In all cases, explicit knowledge was expressed to a greater degree under inclusion than under exclusion instructions: Participants succeeded in generating the revealed transition under inclusion conditions, but failed to refrain from generating that transition under exclusion conditions. Specifically, under exclusion conditions, participants generated the revealed transition at chance levels, instead of suppressing its generation altogether as instructed.

Participants were largely unable to use their explicit knowledge to suppress the proportion of regular transitions generated in the exclusion task to levels below chance. Such below-chance generation levels for revealed transitions were robustly found only for material with a first-order regularity, and only in participants who had explicit knowledge about (at least) two transitions and engaged in generation-task practice specific to a given to-be-excluded transition (Exp. 3, Transfer condition). In these participants, there was even some evidence that below-chance exclusion performance transferred to non-practiced explicit knowledge. However, transition-specific practice was (necessary but) not sufficient for successful exclusion: Whereas participants without such practice (i.e., the No-Practice and Unspecific-Practice conditions of Exp.3) failed to reach below-chance levels, participants with practice also failed to attain below-chance levels under exclusion instructions if they worked on the inclusion task first (i.e., Exp.3, Practice condition). Moreover, despite having explicit knowledge as well as transition-specific generation-task practice, participants were not able to exclude their explicit knowledge to below-chance levels with a second-order conditional sequence (Exp. 2). Taken together, across three Experiments we obtained strong evidence for a violation of invariance of the controlled process, and the results of Experiment 3 suggest that this is due to a failure to suppress the generation of regular transitions below chance levels.

Invariance of the automatic process. In Experiments 1 and 3 that used first-order conditional material, we found evidence suggesting a violation of the invariance assumption for implicit knowledge (no such evidence was found for the second-order conditional material used in Experiment 2). If interpreted in a standard PD framework, the inclusion-exclusion performance difference resulting from this violation may lead to erroneous conclusions about the presence of explicit knowledge (if such knowledge is indeed absent), or to overestimation of the contribution of explicit knowledge. We believe these findings of an inclusion-exclusion difference in estimates of the automatic parameter should be interpreted with some caution, for at least three reasons (see also the section Limitations below). First, the finding was inconsistent and there are multiple possible causes of this inconsistency: The lack of a violation in Experiment 2 may be due to specific properties of the material, or it may be due to the fact that sequence knowledge levels in that study were too low for differences in its expression to be measurable.

Second, although robust and replicated, the violation was relatively small (i.e., the $A_I - A_E$ difference ranged between .01 and .03 in Exp.1, and between .00 and .03 in Exp.3). In the absence of controlled influences, this would be equivalent to a difference between inclusion and exclusion performance of approximately 2 percentage points—an effect barely noticeable under typical conditions.

Third, it is unclear whether the observed invariance violation of parameter A reflects implicit knowledge at all. Note that the parameter for the automatic process captures the sum of all non-controlled influences on generation performance. In particular, it might reflect guessing strategies, and these may differ under inclusion versus exclusion conditions (Stahl et al., 2015). In other words, the above effect may reflect a violation of invariance of guessing or response strategies instead of a violation of invariance of the automatic expression of implicit knowledge. Taken together, we interpret the finding as too weak to conclude that the invariance assumption is violated also for the automatic process.

Limitations and open questions

Before turning to the implications of the present findings, we discuss potential limitations and address open questions.

The invariance violation of the automatic process may reflect learned explicit knowledge. Instead of being due to guessing, the inclusion-exclusion difference in estimates of the automatic parameter may be due to explicit knowledge acquired during learning. Such an effect, if present at all, is likely to be small given that (1) the material was probabilistic and therefore difficult to learn explicitly; (2) the model incorporating the assumption that no learned explicit knowledge was learned fitted the data well; and (3) the results were unchanged when we excluded the data from transitions that participants (correctly) reproduced during debriefing. However, we cannot exclude the possibility that small amounts of explicit knowledge, obtained during the SRTT phase, may have distorted our model's parameter estimates. This interpretation could also account for the lack of such an effect in Experiment 2 given that explicit knowledge was less likely to be learned from the more complex second-order conditional material used in that study. If this were true, then any differences between inclusion and exclusion that were attributed by the model to an invariance violation of the implicit process may in fact have been a consequence of residual explicit knowledge that was not reflected in our debriefing questionnaire (perhaps due to participants' conservative reporting criteria). For validating the PD approach, we know of no other ways to address this potential confound other than by controlling for explicit sequence knowledge as assessed by independent measures; successful control is then naturally limited by the validity of these independent measures. This limitation is another reason for caution in interpreting the above finding as evidence for a violation of invariance of the automatic process. Note that it does not limit the interpretation of our main finding of the invariance violation of the controlled process.

The evidence for sequence learning was weak for SOC material in Experiment 2. As expected, second-order conditional material (Exp. 2) was more difficult to learn than first-order conditional material (Expts. 1 & 3). This was reflected here in the finding that (despite a 20% increase in learning trials) there was only weak evidence for sequence learning in Experiment 2. Specifically, responses to regular transitions were clearly faster and more accurate for both variants of the SOC materials, but the interaction between regularity and training block, which is critical for interpreting a performance advantage for regular transitions as an effect of learning, was not significant. Clearly, an even larger amount of SRTT training should be realized in future studies using SOC materials. Yet, it is unlikely that the advantage for regular transitions has any other causes than learning, given that it was absent from the random condition, and that the effect could not be attributed to properties of specific transitions because regularity of a transition was randomized for each participant anew. Nevertheless, because evidence for (implicit) sequence learning was not beyond doubt, it is not warranted to interpret the modeling results as stringent tests of the invariance assumption for the automatic process.

Explicit knowledge learned via instruction may be qualitatively different from acquired explicit knowledge. The present study manipulated explicit knowledge via instruction. Although it is a common method (e.g., Liefooghe, Wenke, & De Houwer, 2012) that has yielded important insights in other domains, one might argue that explicit knowledge acquired via instruction is somehow qualitatively different from explicit knowledge acquired during SRTT training, and that therefore the present results do not speak to the question of interest regarding the invariance of the expression of acquired knowledge. We believe our manipulation to be valid for the following reasons. First, the instructed explicit knowledge communicated the same proposition about the sequence that participants would have acquired during SRTT training (i.e., that a specific location was regularly followed by another location). Second, we took precautions to avoid any inconsistency or conflict with learned sequence knowledge: Transitions that were revealed to participants were part of the regular sequence and therefore compatible with acquired (implicit or explicit) sequence knowledge. Third, we allowed participants to integrate instructed and acquired knowledge during the practice blocks before the generation task.

Given that the instructed and acquired propositions are identical, we would argue that qualitative differences between acquired and instructed knowledge are likely to involve non-propositional forms of knowledge; such non-propositional knowledge is typically considered to be implicit. Indeed, it is likely that strong implicit knowledge is a precondition for acquiring explicit knowledge (Cleeremans & Jiménez, 2002; Haider & Frensch, 2009): Instructed and acquired explicit knowledge are therefore likely to differ in the degree to which they are correlated with implicit knowledge. If participants are better able to control acquired than instructed explicit knowledge, this would then be due, paradoxically, to the presence of acquired implicit knowledge. Finally, even if that was the case, note that this would not salvage the PD method because a strong correlation between explicit and implicit knowledge would violate the independence assumption, thereby posing another problem for its validity.

Implications

We will first discuss implications for the PD model and the ordinal approach before we suggest ways to improve measurement of sequence knowledge using the generation task. We conclude with a few broader implications.

Validity of the PD method. The present findings show that participants fail to suppress generating regular sequences under exclusion instructions. This implies that the controlled process operates less effectively under exclusion than inclusion instructions, violating the invariance assumption. A model that nevertheless incorporates the invariance assumption will likely fail to adequately account for the data, and will yield distorted estimates of the automatic and controlled process. To illustrate, assume that the true values of the parameters are $C_{Inclusion} = .8, C_{Exclusion} = .4$, and $A_{Inclusion} = A_{Exclusion} = .25$. This yields the following generation proportions of regular transitions $I = .8 + (1 - .8) * .25 = 0.85$ and $E = (1 - .4) * .25 = 0.15$. When fitting a traditional PD model enforcing the invariance assumption $C_{Inclusion} = C_{Exclusion}$ to these data, we get $C = .7$ that lies somewhere between the true values of C, and $A = .5$ which is a vast overestimation of the true A. Importantly, note that if the true value of $A = .25$ represents chance level, applications of the traditional PD method might lead to the erroneous conclusion that implicit knowledge had been learned even if such knowledge was in fact entirely absent. In addition, if we are interested in the amount of explicit knowledge learned from the SRTT training phase, it might be argued that the higher estimate obtained from the inclusion condition might be a more valid estimate of learned explicit knowledge; the inability to express this knowledge under exclusion may be of secondary interest. By this argument, applying the traditional PD method also yields an underestimation of explicit knowledge.

We therefore recommend against using the PD method unless separate estimates of $C_{Inclusion}$ and $C_{Exclusion}$ can be obtained, for example as we have done in the present study. To do so, at least two levels of an implicit-knowledge factor are necessary across which the C parameters could be equated to obtain stable parameter estimates. Note that this strategy may not be broadly applicable in typical SRTT studies because of the strong correlation between C and A; the assumption that the level of implicit knowledge does not affect the amount of acquired explicit knowledge will be warranted only in special cases such as realized in the present studies.[4]

Ordinal PD. As argued in the introduction to Experiment 3, it might be the case that, under certain conditions, certain violations of assumptions underlying the validity of the PD model equations might leave the ordinal interpretation of PD data unaffected. This

[4]As another way to obtain separate estimates, instead of assuming equal levels of the controlled process across levels of the automatic process, one might assume that the level of the automatic process does not interact with instruction (i.e., it does not affect the relative magnitude of the invariance violation). In this case, the controlled parameters need not be equated across both levels of implicit knowledge; instead, explicit knowledge in the lower level of the implicit-knowledge factor can be expressed as a proportion of the explicit knowledge in the higher level of that factor (i.e, $C_{Inclusion/low} = w * C_{Inclusion/high}$ and $C_{Exclusion/low} = w * C_{Exclusion/high}$.)

is not true, however, for the specific violation of invariance of the controlled process reported here. To reiterate, given a single experimental condition, it is concluded in the ordinal approach that implicit knowledge is present if exclusion performance is above a (chance or empirical) baseline; and it is concluded that explicit knowledge is present if inclusion performance exceeds exclusion performance. The possible conclusions from comparing two experimental conditions are outlined by Hirshman (2004, Table 1). They depend on the invariance assumption in the sense that a monotonically increasing controlled process should lead to a monotonic increase of inclusion performance and at the same time a monotonic decrease of exclusion performance. The present study shows, however, that exclusion performance cannot be assumed to reliably decrease with increasing explicit knowledge. This implies that the assumptions underlying the ordinal-PD approach proposed by Hirshman are also violated for the generation task as applied to sequence learning. In addition, we have previously shown that another assumption of ordinal PD, namely that baseline performance is identical in the inclusion and exclusion tasks, is also violated at least in some cases (Stahl et al., 2015). We conclude that the ordinal interpretation of PD data cannot be recommended as a fallback option.

Generation task as a measure of sequence knowledge. The generation task has been proposed as a useful and sensitive measure of implicit knowledge (Jiménez, Méndez, & Cleeremans, 1996; Perruchet & Amorim, 1992). Its sensitivity may be called into question by the finding that RT effects obtained during the SRTT were often greater than implicit-knowledge effects in the generation task. In part, this may be attributed to the greater reliability of the RT measure, as it relies on aggregation across a larger number of trials than does the generation task. Another possible reason is that the generation task's sensitivity as a measure of implicit knowledge may be lower than previously thought. For instance, previous findings of implicit knowledge using the generation task may have been overestimates of implicit knowledge due to a violation of invariance for the controlled process with $C_I > C_E$. Note that most studies used much easier-to-learn materials (with four instead of six locations); it is thus plausible that participants acquired more explicit knowledge than they did in our experiments, and that the overestimation bias was more severe in those studies.

Another possible reason for overestimating implicit knowledge is that the regularities in the sequences implemented in previous research were such that the probability of reversals (e.g., 1-2-1) was below chance. Given that participants spontaneously tend to generate reversals at below-chance levels, this implies that they instead generate other regular transitions at slightly above-chance levels even in the absence of any true sequence knowledge. As a consequence of this reversal-avoidance bias, implicit knowledge might be overestimated if one uses chance baselines as a reference. This problem has been discussed before (Destrebecqz & Cleeremans, 2003; Reed & Johnson, 1994; Shanks & Johnstone, 1999), and was solved by comparing performance on the training sequence with performance on a transfer sequence containing a similarly low proportion of reversals. This implies, however, that the PD approach does not provide a measure of the absolute level of implicit or explicit knowledge; instead, by relying on a comparison of performance across two sequences, it yields a difference measure that is associated with reduced reliability. This is because the transfer sequence is selected by the experimenter out of a large set of possible such sequences, and this random choice interacts with participants' partial acquired knowledge, as well as with their individual response tendencies, to introduce additional error into the measurement. In addition, the reversal-avoidance bias may not only mimic implicit knowledge; it may also mimic (or mask) explicit knowledge if it interacted with the inclusion-exclusion instructions, perhaps via different response strategies or criteria adopted under inclusion versus exclusion instructions.

It might be possible to construct a version of the generation task that allows for the separation of automatic and controlled processes but does not depend on exclusion of explicit knowledge and does not induce different response criteria. For example, D'Angelo, Milliken, Jiménez, & Lupiáñez (2013) implemented such a generation task variant in artificial grammar learning in which two different inclusion instructions were compared: After learning about two different grammars, participants were asked, in the first (second) inclusion block to generate exemplars from the first (second) grammar. Under certain assumptions, performance differences between blocks can be interpreted as evidence for explicit controllable knowledge. Exclusion failure and different criteria presumably do not matter in this task: Participants were not instructed to exclude explicit knowledge in this task, and it is plausible that the similarity of instructions for both generation tasks also induced comparable response criteria. As another example, in the domain of source memory, the PD procedure can be replaced by a source-memory task in which, instead of including versus excluding items from one of two study lists (A and B), participants are asked to indicate the source of the word (list A or list B; Buchner, Erdfelder, Steffens, & Martensen, 1997). Perhaps with a similar modification, an improved generation task may prove a useful measure of sequence knowledge.

Conclusion

In light of the present findings suggesting limited validity of the PD generation task, what can we conclude about explicit and implicit sequence knowledge from its previous applications? We have seen that, when the traditional PD approach is used, an invariance violation of the controlled process leads to overestimation of implicit knowledge and to underestimation of the amount of explicit knowledge participants have acquired from the preceding SRTT training phase. In addition, an invariance violation of the automatic process may lead to a (small) overestimation of explicit knowledge. We can take into account these potential distortions in our interpretation of previous findings by distinguishing the two patterns of results found in the literature.

In the first case, evidence for explicit but not for implicit knowledge was found (e.g., Wilkinson & Shanks, 2004). In this case, the evidence for explicit knowledge suggests that the distortions due to the invariance violation apply. Obtaining evidence for explicit knowledge despite the underestimation bias implies that explicit knowledge was likely present. Obtaining no evidence for implicit knowledge despite the likely presence of an overestimation bias may indeed reflect the absence of implicit knowledge; alternatively, it may of course reflect lack of statistical power.

In the second case, evidence for both explicit and implicit knowledge was reported (e.g., Destrebecqz & Cleeremans, 2001, 2003; Jiménez et al., 2006; Mong et al., 2012). Here, two scenarios must be distinguished: In the first, the evidence for explicit knowledge also suggests that the distortions resulting from the invariance violation may have compromised the results: Again, the evidence for explicit knowledge obtained despite the underestimation bias should probably be assumed to be reliable; however, the evidence for implicit knowledge may be an artifact of the overestimation bias and should be interpreted with caution. In the second scenario, explicit knowledge is absent, and the explicit-knowledge effect reflects an artifact of the invariance violation of the automatic process (i.e., $A_I > A_E$); the results obtained in the literature would then indicate the presence of implicit but not explicit knowledge.

Of course, different scenarios might underlie different studies, and single studies may also reflect a mixture of scenarios. Taken together, when considering the limitations discovered in our studies, the PD approach to using the generation task as a measure of implicit and explicit sequence knowledge in the SRTT has so far yielded few reliable conclusions. Future research should consider using alternative methods of assessing implicit and explicit knowledge (for a recent overview see Timmermans & Cleeremans, 2015).

Outlook

One of the great benefits of multinomial models such as the PD model is that they are flexibly adaptable measurement models for studying latent cognitive processes using a wide variety of experimental paradigms (Erdfelder et al., 2009). To validate a new model, it is common to assess its goodness of fit, and to empirically demonstrate that its parameters can be selectively manipulated and interpreted psychologically (i.e., parameter estimates reflect targeted experimental manipulations in the predicted manner; Batchelder & Riefer, 1999). In many cases, however, simplifying assumptions need to be made; for instance, latent processes are equated across two or more experimental conditions (e.g., a single controlled process C is assumed to operate under inclusion and exclusion conditions). Whenever such assumptions of invariance are made, we propose that they should also be tested empirically as part of the model-validation effort before a new model is proposed and used to investigate substantive issues.

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Appendix

This appendix provides a complete specification of the models and priors used. Code (R/rstan) is available at https://github.com/methexp/pdl2.

Experiment 1, model $\mathcal{M}_1$

Priors on fixed effects were

$$\mu^{(C)}_{km} \sim N(0,1), k = \{1,2\}; m = \{1,2\}$$
$$\mu^{(A)}_{jkm} \sim N(0,1), j = \{1,2\}; k = \{1,2\}; m = \{1,2\}$$

where j indexes transition type (revealed vs. non-revealed), k indexes learning material presented during the SRTT (random vs. probabilistic), and m indexes *PD instruction* condition (inclusion vs. exclusion).

For participants who did not receive explicit knowledge about a single transition, we assumed that all $C_{ijm} = 0$. Therefore, participant effects are only required for automatic processes ($\delta^{(A)}_{ijm}$). In participants who received explicit knowledge about one transition, two additional participant effects were needed to model controlled processes for revealed transitions ($\delta^{(C)}_{im}$). We thus provide the specification of participant effects for these two groups of participants separately.

Participants who did not receive explicit knowledge about one transition. For participants who did not receive explicit knowledge about one transition, participant effects $\delta^{(A)}_{ijm}$ can be written as vectors $\boldsymbol{\delta}_i$ that were modeled as draws from a multivariate normal

$$\boldsymbol{\delta}_i \sim N_4(0, \Sigma_{kl}), i = 1, ..., I$$

where k indexes the learning material that was presented to participant i and l indexes his or her level of the explicit-knowledge factor. The covariance matrices Σ_{kl} were obtained from the standard deviations of participant effects $\boldsymbol{\sigma}_{kl}$ and correlation matrices Ω_{kl}

$$\Sigma_{kl} = Diag(\boldsymbol{\sigma}_{kl})\ \Omega_{kl}\ Diag(\boldsymbol{\sigma}_{kl}), k = \{1, 2\}, l = \{1, 2\}$$

Each element σ_{klp} of the vectors of standard deviations $\boldsymbol{\sigma}_{kl}$ was drawn from independent half-normal prior distributions.

$$\sigma_{klp} \sim N(0, 1)_{\mathcal{I}(0,\infty)}, k = \{1, 2\}, l = \{1, 2\}$$

For the correlation matrices Ω_k, we used LKJ priors with a scaling factor of 1 (Lewandowski, Kurowicka, & Joe, 2009):

$$\Omega_{kl} \sim LKJcorr(\nu = 1), k = \{1, 2\}, l = \{1, 2\}$$

Participants who received explicit knowledge about one transition. For participants who received explicit knowledge about one transition, participant effects $\delta_{ijm}^{(A)}$ and $\delta_{im}^{(C)}$ can be written as vectors $\boldsymbol{\delta}_i$ that were modeled as draws from a multivariate normal

$$\boldsymbol{\delta}_i \sim N_6(0, \Sigma_{kl}), i = 1, ..., I$$

where k indexes the learning material that was presented to participant i and l indexes his or her level of the explicit-knowledge factor. The covariance matrices Σ_{kl} were specified as above, with the only exception that six instead of four parameters were required.

Experiment 1, model $\mathcal{M}_2$

Priors on fixed effects were

$$\mu_{jkl}^{(C)} \sim N(0,1), j = \{1,2\}; k = \{1,2\}; l = \{1,2\}$$
$$\mu_{jkl}^{(A)} \sim N(0,1), j = \{1,2\}; k = \{1,2\}; l = \{1,2\}$$

Participant effects $\delta_{ij}^{(A)}$ and $\delta_{ij}^{(C)}$ can be written as vectors $\boldsymbol{\delta}_i$ that were modeled by

$$\boldsymbol{\delta}_i \sim N_4(0, \Sigma_{kl}), i = 1, ..., I$$

Priors for the covariance matrix Σ_{kl} were specified as above.

Experiment 2, models $\mathcal{M}_1$ and $\mathcal{M}_2$

For the model-based analyses, we used models $\mathcal{M}_1$ and $\mathcal{M}_2$ analogous to those used in Experiment 1.

Experiment 3, model $\mathcal{M}_1$

Priors on fixed effects were

$$\mu_{jlm}^{(C)} \sim N(0,1), j = \{1,2\}; l = \{1,2\}; m = \{1,2\}$$
$$\mu_{mt}^{(A)} \sim N(0,1), t = \{1, ..., 6\}; m = \{1,2\}$$

where j indexes *transition type* (revealed & practiced vs. revealed & non-practiced), l indexes practice condition (Control, No-practice, Unspecific-practice, Practice, Transfer), t indexes specific items (i.e., transitions), and m indexes *PD instruction* (inclusion vs. exclusion).

Participant effects $\delta_{imt}^{(A)}$ and $\delta_{ijm}^{(C)}$ can be written as vectors $\boldsymbol{\delta}_i$.

For participants in the *Control* group, these were modeled by

$$\boldsymbol{\delta}_i \sim N_{12}(0, \Sigma_l), i = 1, ..., I$$

For participants in the *No-Practice*, *Unspecific-Practice*, and *Practice* groups,

$$\boldsymbol{\delta}_i \sim N_{14}(0, \Sigma_l), i = 1, ..., I$$

For participants in the *Transfer* group

$$\boldsymbol{\delta}_i \sim N_{16}(0, \Sigma_l), i = 1, ..., I$$

The covariance matrices Σ_l were modeled separately and independently for each between-subjects condition. Priors on these matrices were as described above for Experiment 1.

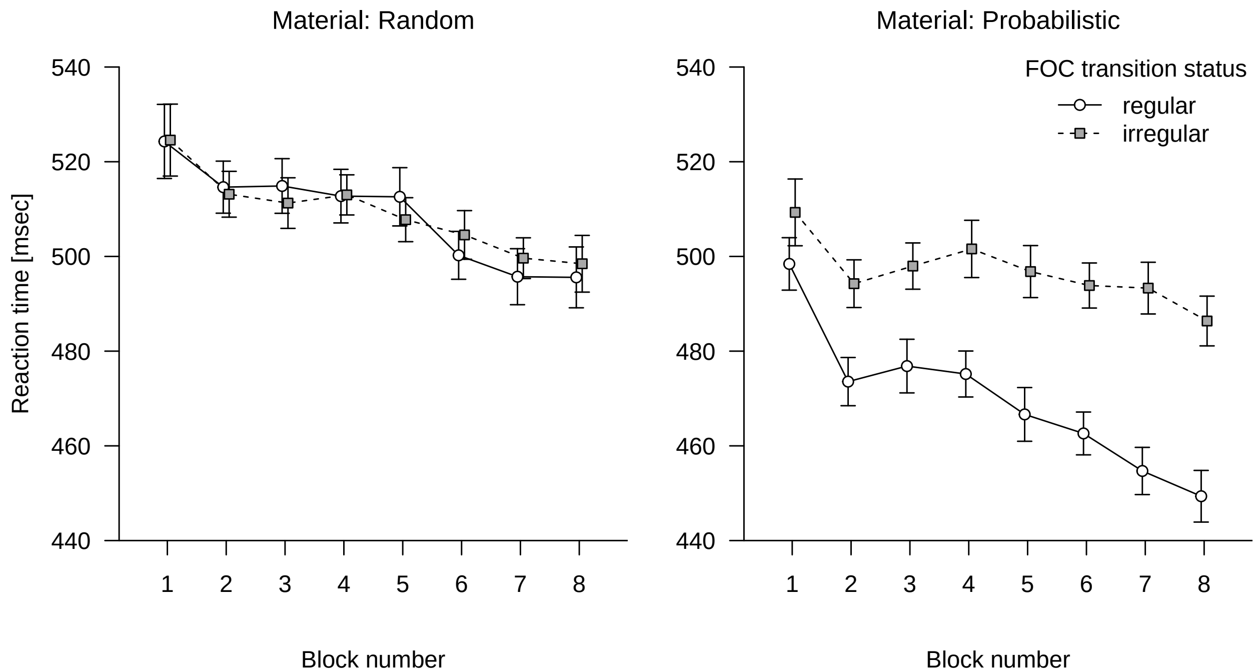

Figure 1. RTs during acquisition phase, split by *material* and *FOC transition status.* Error bars represent 95% within-subjects confidence intervals.

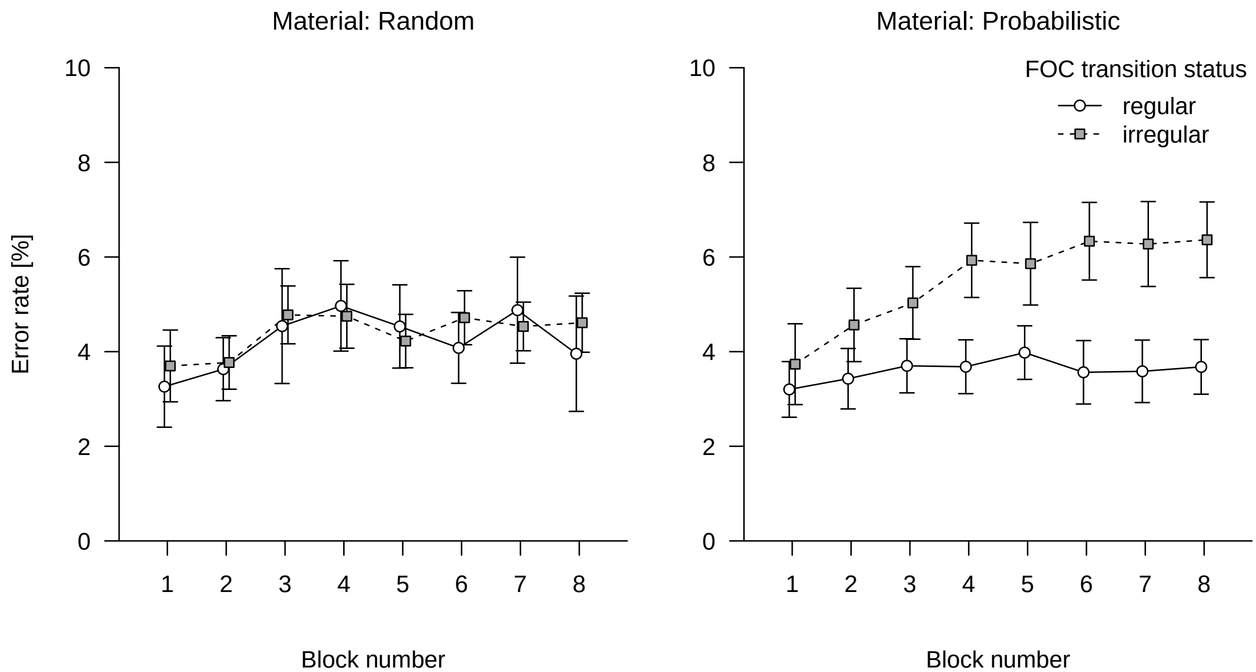

Figure 2. Error rates during acquisition phase, split by *material* and *FOC transition status*. Error bars represent 95% within-subjects confidence intervals.

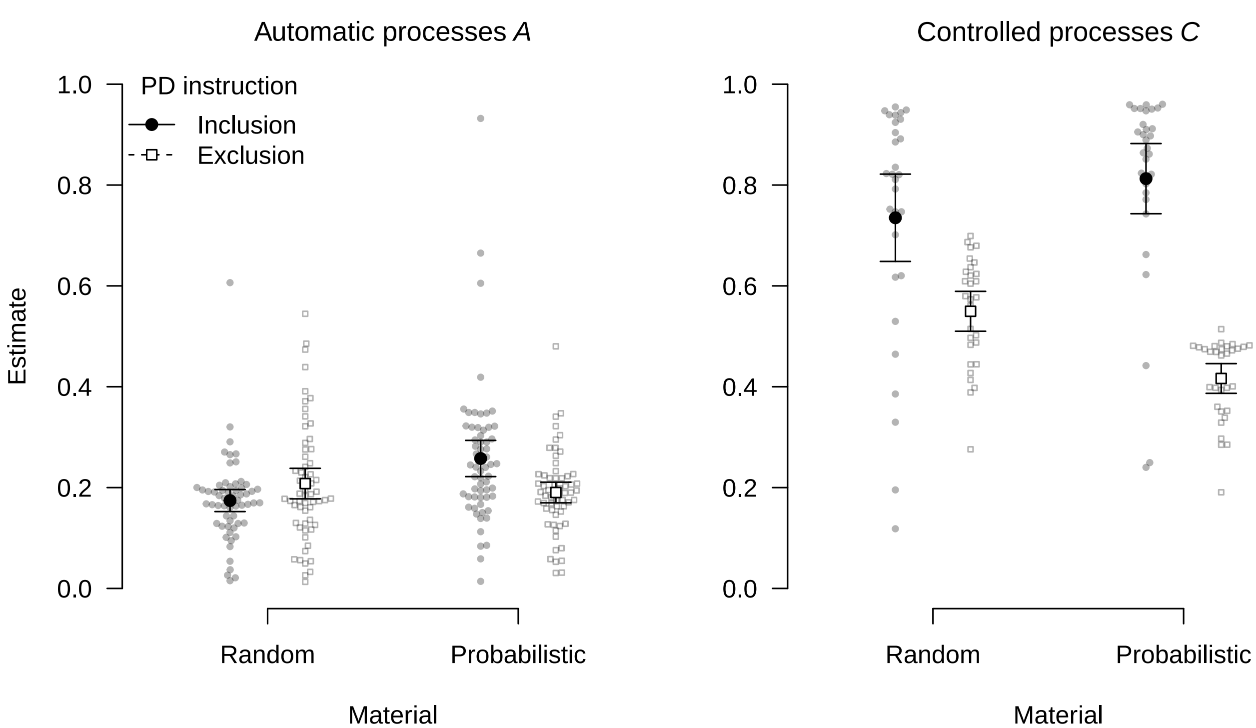

Figure 3. Parameter estimates from Experiment 1. Error bars represent 95% confidence intervals.

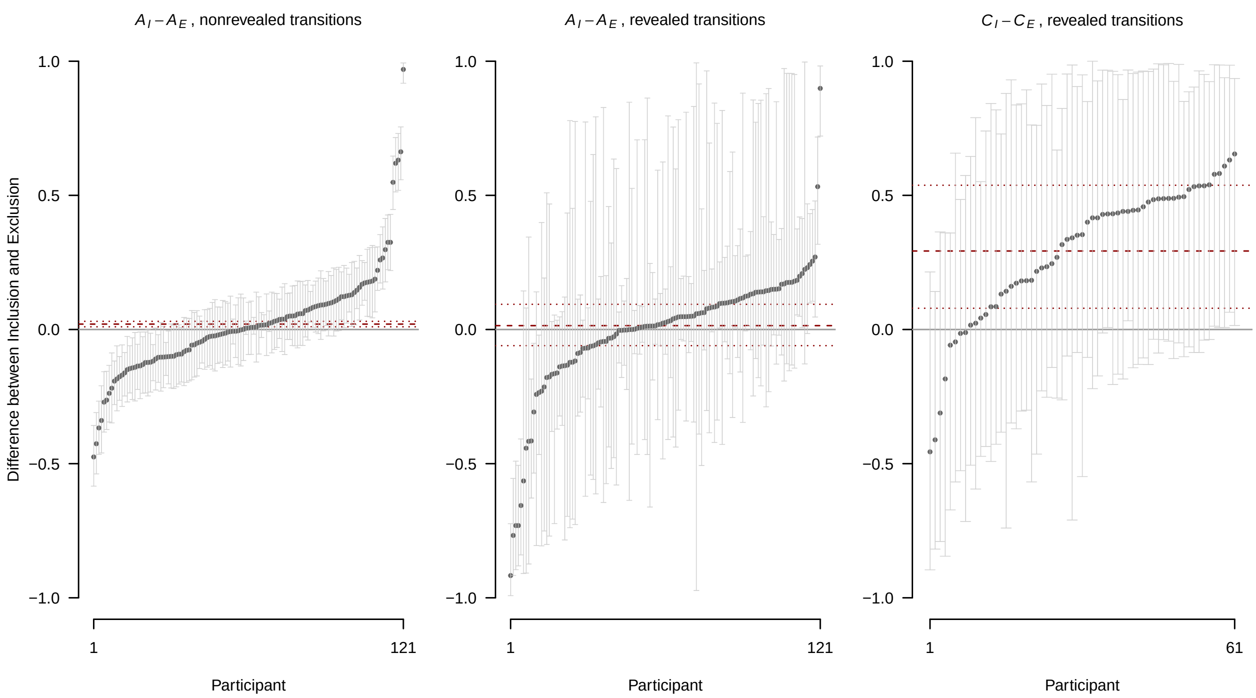

Figure 4. Posterior differences between $A_I - A_E$ and $C_I - C_E$ in Experiment 1, plotted for each participant (gray dots) with 95% credible intervals. Dashed lines represent the posterior means of the differences between mean parameter estimates. Dotted lines represent 95% credible intervals.

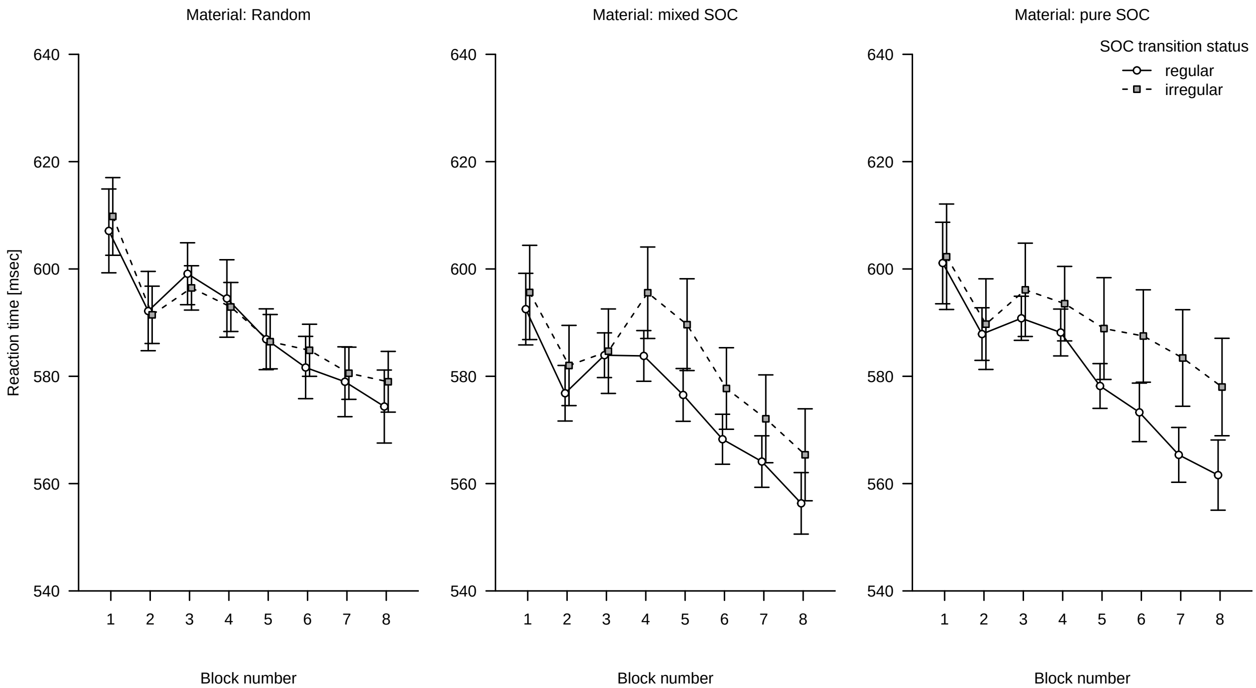

Figure 5. RTs during acquisition phase, split by *material* and *SOC transition status.* Error bars represent 95% within-subjects confidence intervals.

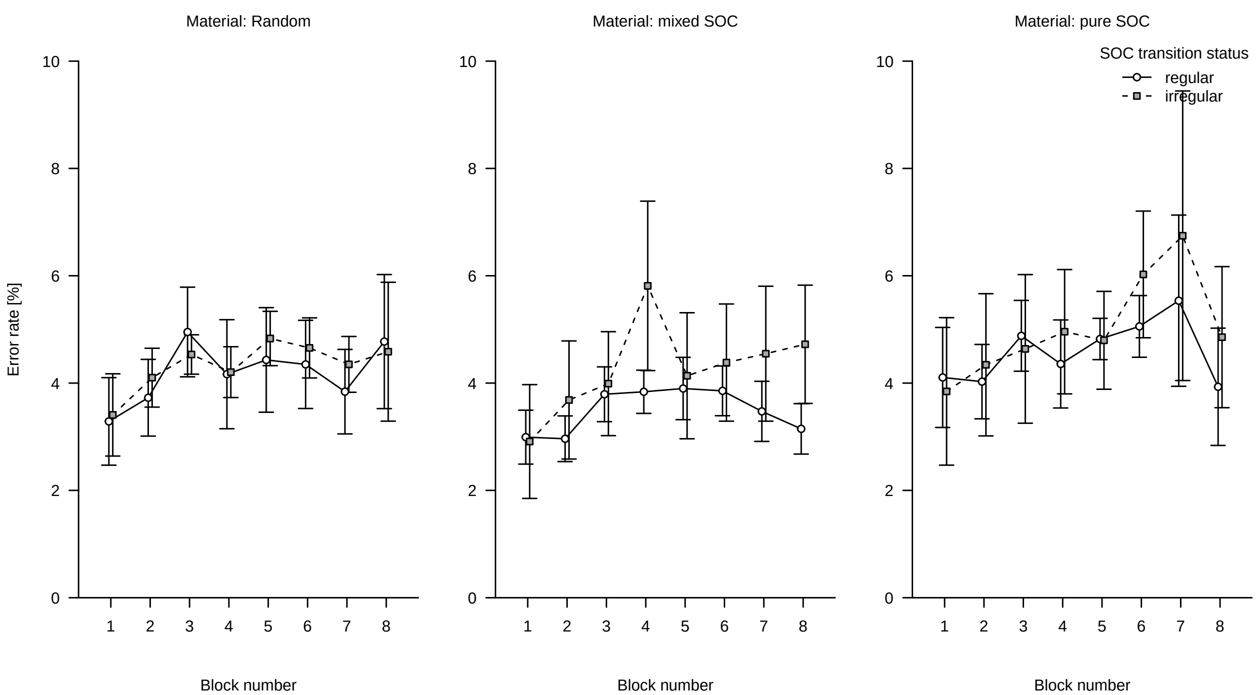

Figure 6. Error rates during acquisition phase, split by *material* and *SOC transition status.* Error bars represent 95% within-subjects confidence intervals.

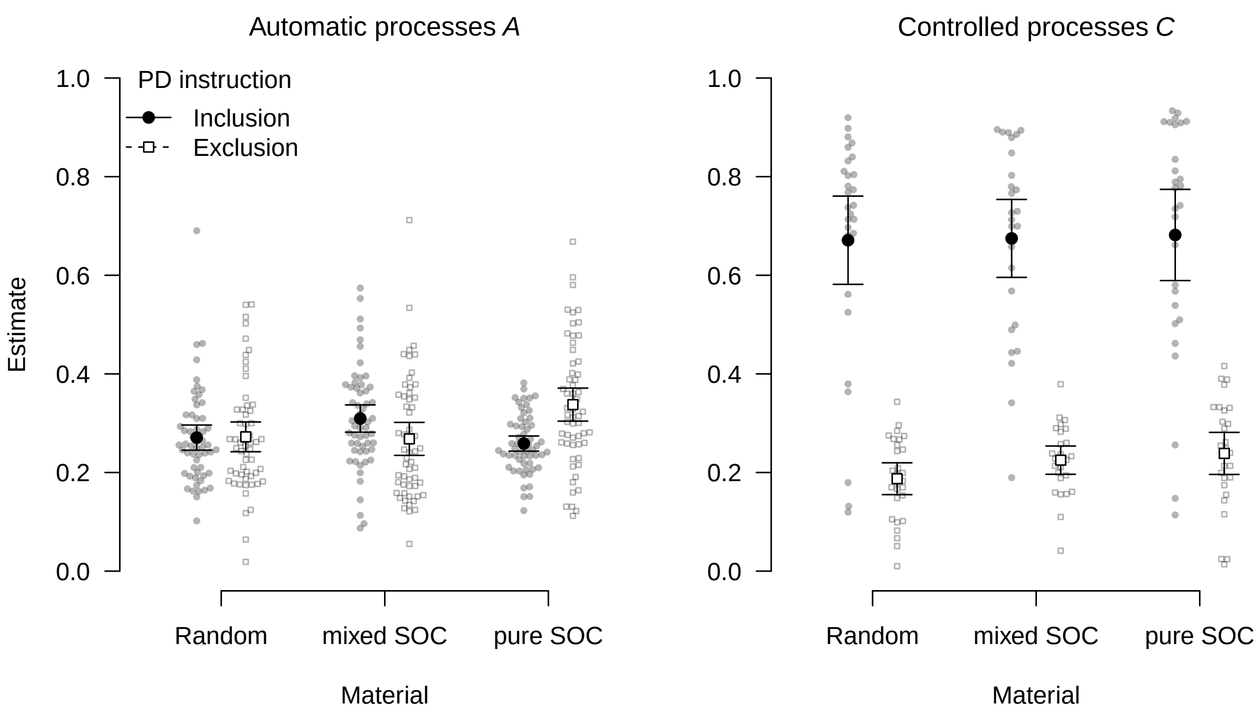

Figure 7. Parameter estimates from Experiment 2. Error bars represent 95% confidence intervals.

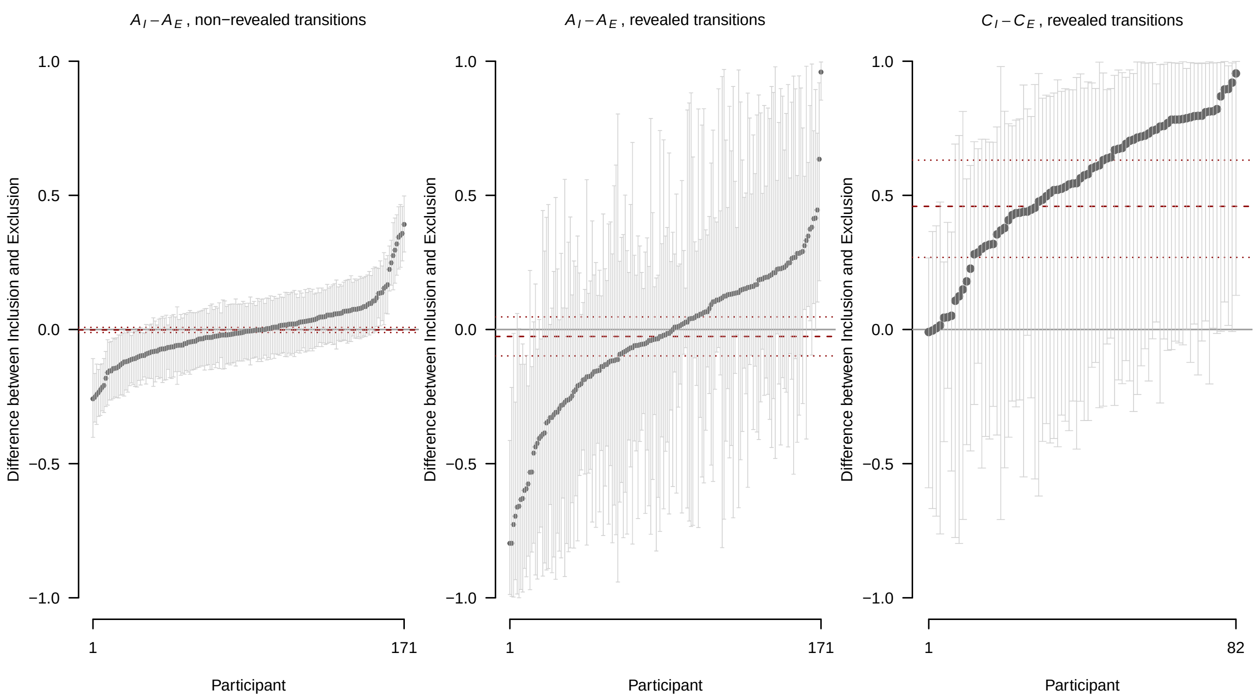

Figure 8. Posterior differences $A_I - A_E$ and $C_I - C_E$ in Experiment 2, plotted for each participant (gray dots) with 95% credible intervals. Dashed lines represent the posterior means of the differences between mean parameter estimates. Dotted lines represent 95% credible intervals.

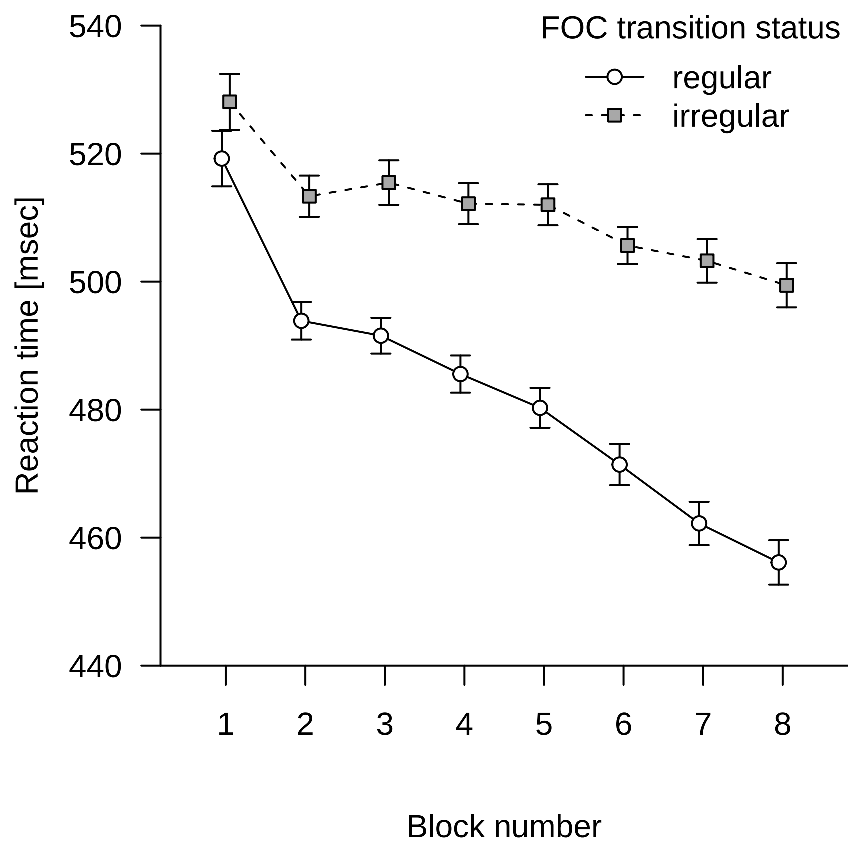

Figure 9. RTs during acquisition phase, split by *FOC transition status*. Error bars represent 95% within-subjects confidence intervals.

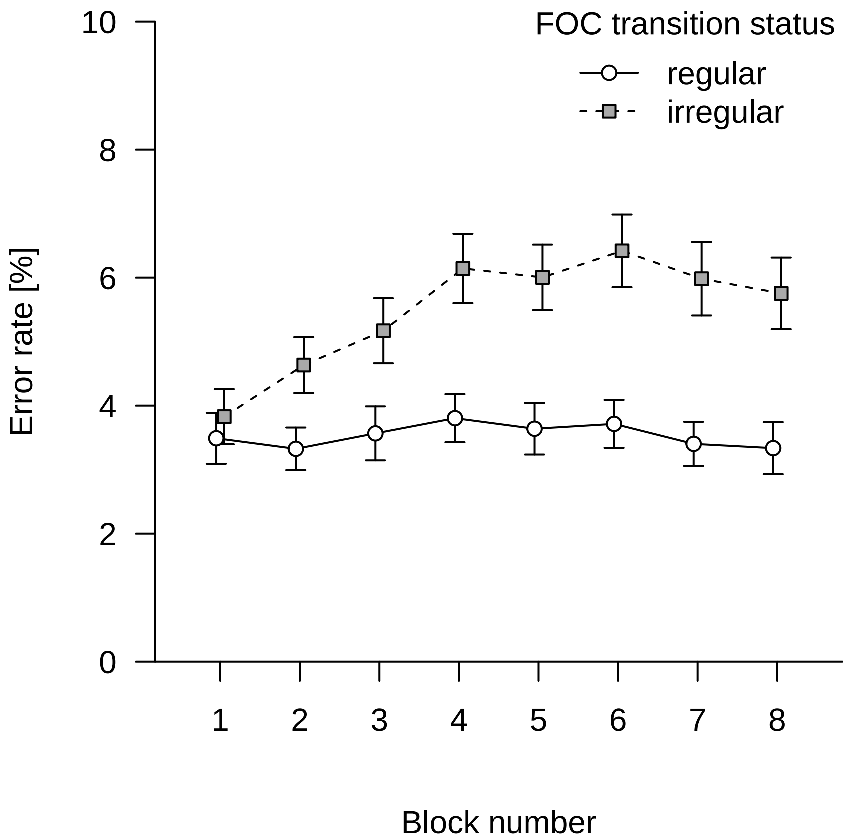

Figure 10. Error rates during acquisition phase, split by *FOC transition status.* Error bars represent 95% within-subjects confidence intervals.

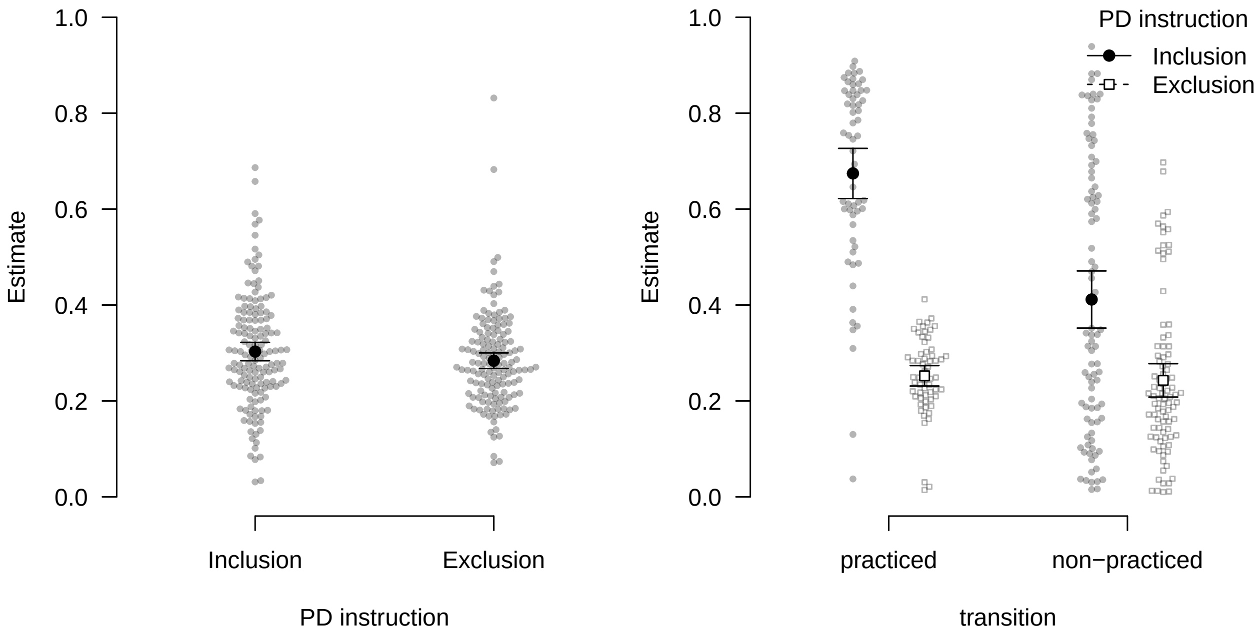

Figure 11. Parameter estimates from Experiment 3. Error bars represent 95% confidence intervals.

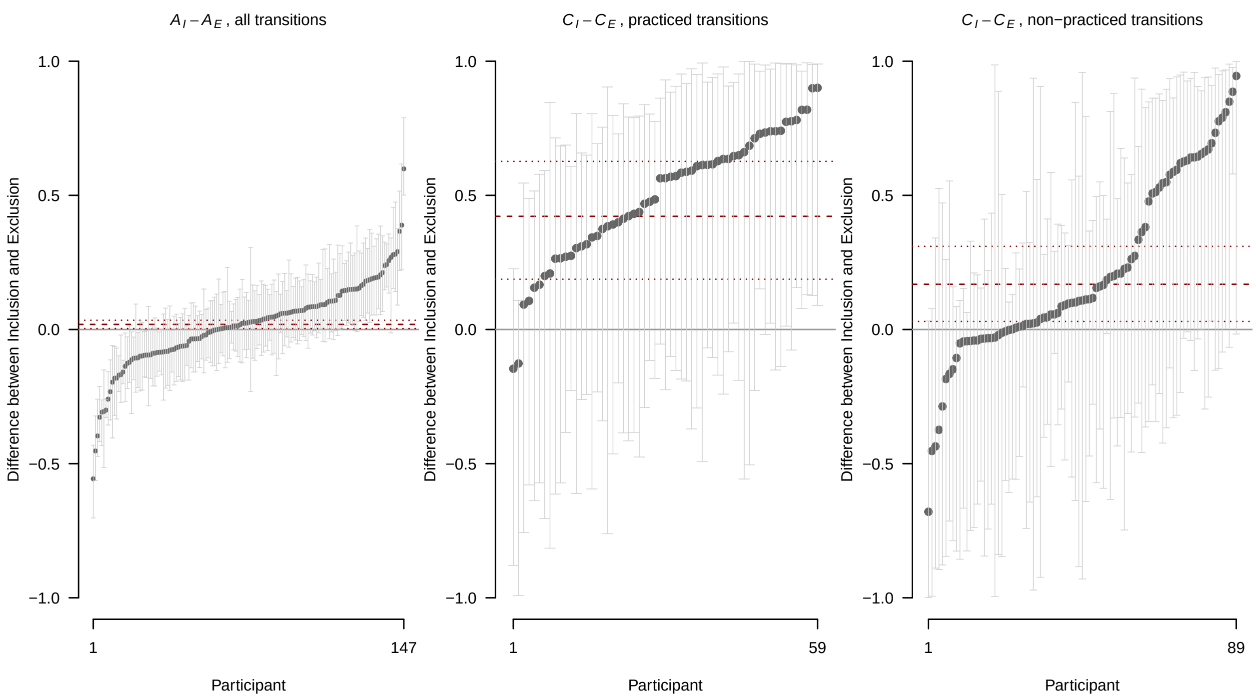

Figure 12. Posterior differences between $A_I - A_E$ and $C_I - C_E$ in Experiment 3, plotted for each participant (gray dots) with 95% credible intervals. Dashed lines represent the posterior means of the differences between mean parameter estimates. Dotted lines represent 95% credible intervals.

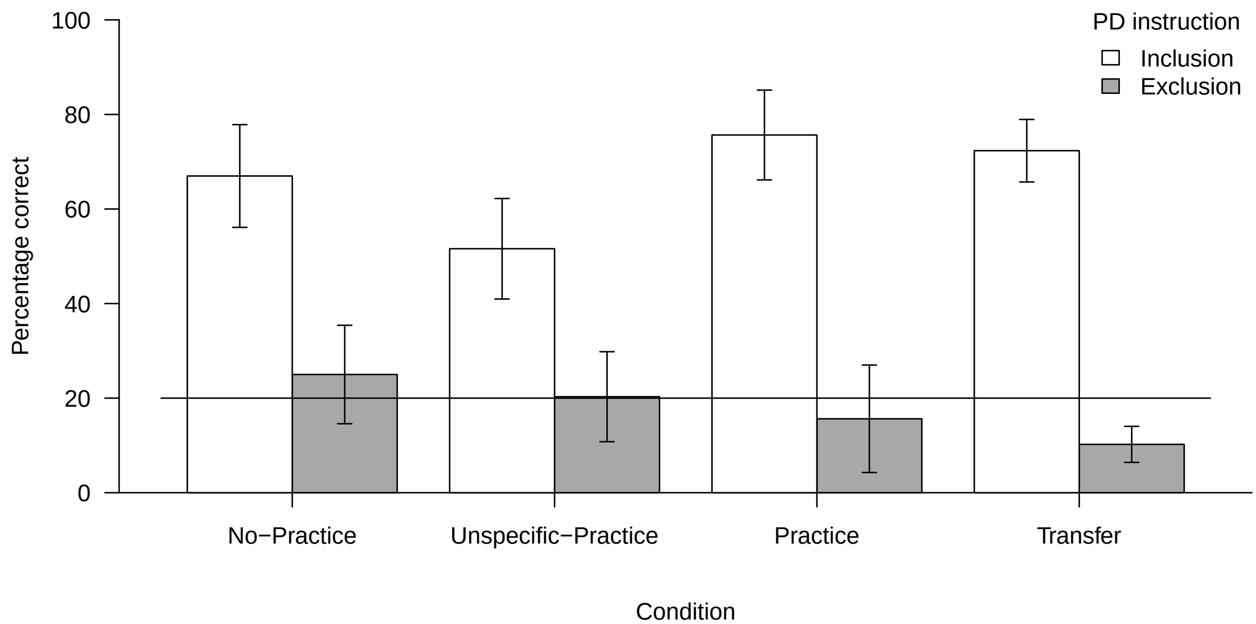

Figure 13. Generation performance for revealed transitions. Error bars represent 95% confidence intervals. Horizontal lines represent chance baseline.

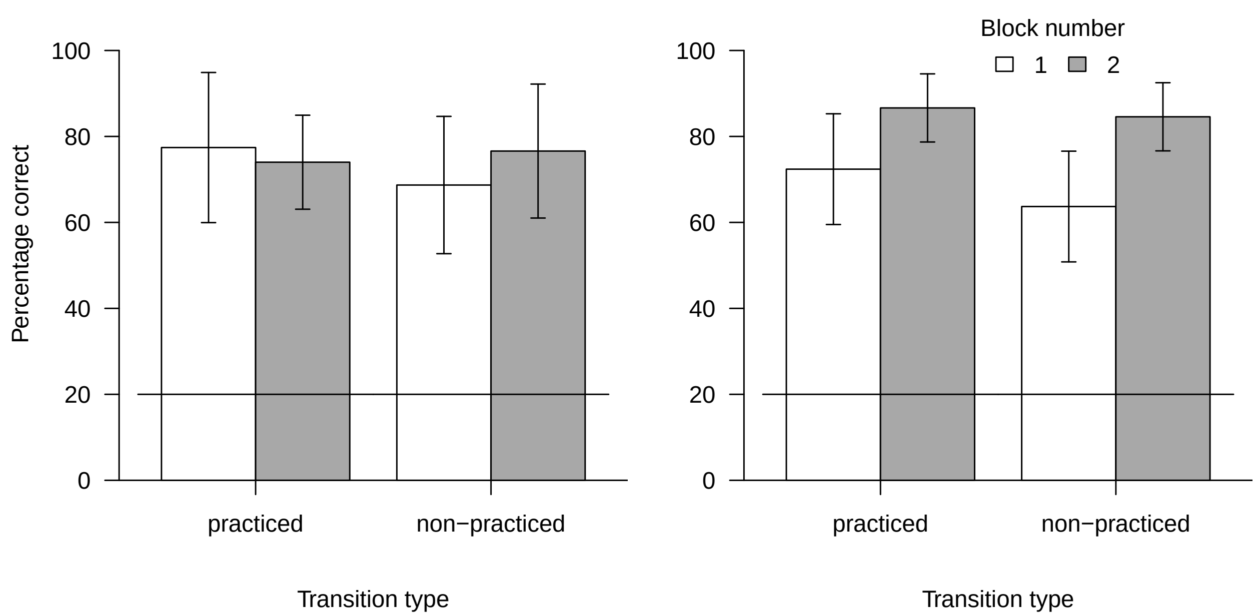

Figure 14. Inclusion performance for revealed transitions. Left: Between-subjects comparison between *No-Practice* and *Practice* groups. Right: Within-subjects comparison in *Transfer* group. Horizontal lines represent chance baseline.

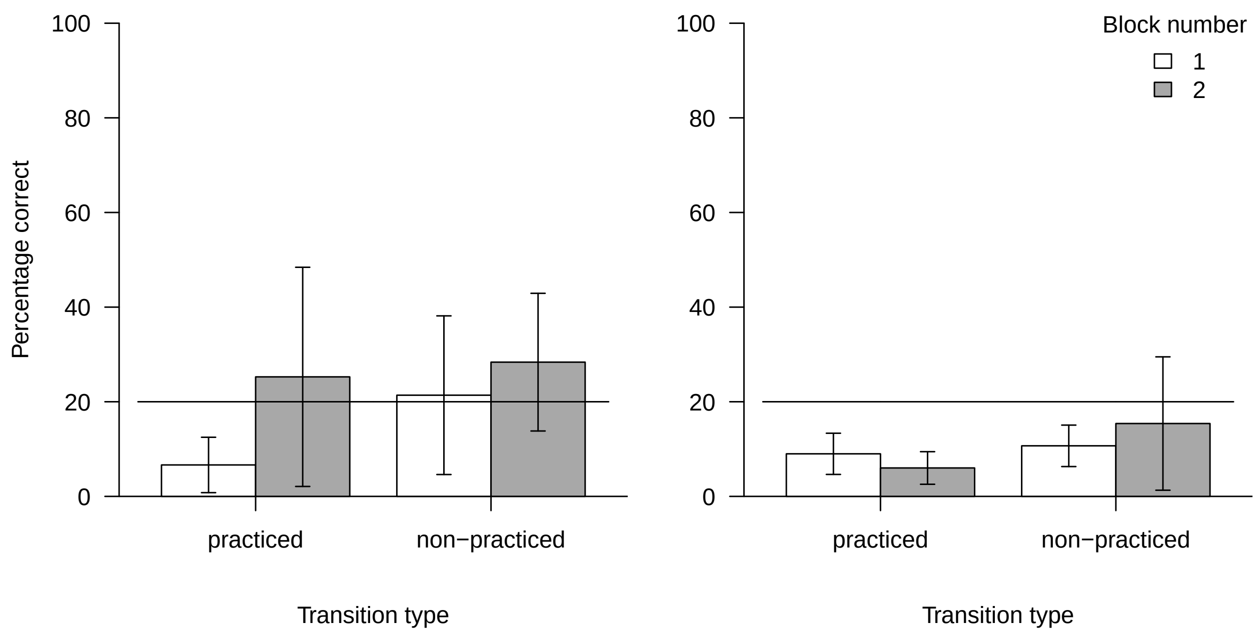

Figure 15. Exclusion performance for revealed transitions. Left: Between-subjects comparison between *No-Practice* and *Practice* groups. Right: Within-subjects comparison in *Transfer* group. Horizontal lines represent chance baseline.