Metamemory and Schema Effects in Source Monitoring

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Marie Luisa Schaper
aus Wuppertal

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aus dem Institut für Experimentelle Psychologie
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1. Prof. Ute J. Bayen, Ph.D.

2. Prof. Dr. Jochen Musch

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Eigenständigkeitserklärung

Ich versichere an Eides Statt, dass die Dissertation von mir selbständig und ohne unzulässige fremde Hilfe unter Beachtung der „Grundsätze zur Sicherung guter wissenschaftlicher Praxis an der Heinrich-Heine-Universität Düsseldorf“ erstellt worden ist.

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Table of Contents

Zusammenfassung

Quellenüberwachung beschreibt das Zuordnen von Information zu ihrer Quelle oder ihrem Ursprung (Johnson, Hashtroudi & Lindsay, 1993). Quellenüberwachungsprozesse werden von Schemata (organisiertes Wissen über die Welt, Alba & Hasher, 1983) unterschiedlich beeinflusst. Einerseits werden schema-inkonsistente, und daher unerwartete Quellen besser erinnert als schema-konsistente, erwartete Quellen (*Inkonsistenzeffekt*, z.B. Küppers & Bayen, 2014). Andererseits ist Quellenraten typischerweise in Richtung der erwarteten Quelle verzerrt (z.B. Bayen, Nakamura, Dupuis & Yang, 2000). Diese Effekte sind gut etabliert, jedoch ist unbekannt, ob Menschen sich des Inkonsistenzeffektes im Quellengedächtnis metakognitiv bewusst sind. Ein Bewusstsein über diesen Inkonsistenzeffekt könnte die schema-konsistente Verzerrung des Quellenratens als strategische Kompensation für das schlechtere Gedächtnis für erwartete Quellen erklären (vergleiche Küppers & Bayen, 2014). Daher untersuchte ich Schemaeffekte auf Gedächtnis, Metagedächtnis und Rateverzerrungen in sieben Experimenten. Während einer Lernphase wurden Gegenstandsworte entweder mit einem erwarteten Raum (z.B. Ofen in der Küche) oder einem unerwarteten Raum (z.B. Mikrowelle im Badezimmer) präsentiert. In der Testphase wurden die gelernten und neue Gegenstände präsentiert und die Versuchspersonen gaben an, ob ein Gegenstand während der Lernphase präsentiert wurde und, falls ja, mit welcher Quelle. Itemgedächtnis- und Quellengedächtniseinschätzungen wurden zu verschiedenen Zeitpunkten in den Experimenten erhoben (vor oder während der Lernphase, und vor, während oder nach der Testphase).

Die Ergebnisse zeigten eine *Konsistenzillusion* im Metagedächtnis: Schemata zeigten keinen Effekt auf das Itemgedächtnis und entweder keinen Effekt oder einen Inkonsistenzeffekt auf das Quellengedächtnis. Im Gegensatz dazu sagten Versuchspersonen einen Konsistenzeffekt im Gedächtnis vorher, das heißt, sie sagten besseres Itemgedächtnis und Quellengedächtnis für erwartete als für unerwartete Quelle-Item-Paare vorher. Dies galt sowohl für item-weise Urteile (Experimente 1.1 und 1.2) als auch für globale Urteile (nur für Quellengedächtnis, Experiment 1.3). Der Effekt war in Quellengedächtniseinschätzungen stärker ausgeprägt.

Die Experimente 2.1, 2.2 und 2.3 zeigten, dass die Konsistenzillusionen im Metagedächtnis auf zwei Faktoren basieren. Erstens moderierten a priori Überzeugungen über den Effekt von Schemata auf Gedächtnis die Konsistenzeffekte in item-weisen Urteilen. Zweitens trugen Erfahrungen, die Menschen im Moment des Lernens machen (z.B.

Verarbeitungsflüssigkeit), zusätzlich zu diesen Konsistenzeffekten bei. Diese Faktoren beeinflussten Itemgedächtnis- und Quellengedächtnisurteile unterschiedlich. Erfahrungen beeinflussten besonders die Itemgedächtnisurteile und Überzeugungen beeinflussten besonders die Quellengedächtnisurteile. Diese Ergebnisse unterstreichen die Unterschiede zwischen den Urteilsarten.

Die Konsistenzillusion im Metagedächtnis hat wichtige Implikationen für die Interpretation der Quellengedächtnisverzerrung. Da die Versuchspersonen sich des Inkonsistenzeffekts im Quellengedächtnis während des Lernens (Experimente 1.1, 1.2), vor oder während des Tests (Experiment 1.3) nicht bewusst waren, konnten sie ihn nicht strategisch kompensieren. Die Metagedächtnisüberzeugungen der Versuchspersonen und Quellenraten korrelierten zu keinem Zeitpunkt. In Experiment 1.4 manipulierte ich die Metagedächtnisüberzeugungen über den Einfluss von Schemata auf das Quellengedächtnis. Diese Manipulation beeinflusste das Quellenraten nicht. Daher reflektiert schemakonsistentes Quellenraten keine kompensatorische Strategie. Stattdessen scheint Raten sowohl von einer verzerrten Kontingenzrepräsentation als auch von einfachem Verlassen auf Schemata abzuhängen.

Die vorliegende Arbeit hat daher drei Schlussfolgerungen: Erstens existiert eine Konsistenzillusion im Metagedächtnis bezüglich des Einflusses von Schemata auf Itemgedächtnis und Quellengedächtnis. Zweitens sind sowohl a priori Überzeugungen als auch Erfahrungen Ursachen dieser Illusion. Drittens schließt diese Illusion eine kompensatorische Strategie als Grundlage für schemakonsistentes Quellenraten aus.

Abstract

Source monitoring involves attributing information to its source, or origin (Johnson, Hashtroudi, & Lindsay, 1993). Schemas (organized knowledge about the world, Alba & Hasher, 1983) influence source-monitoring processes differentially. On the one hand, schema-inconsistent, and thus unexpected, sources are remembered better than schema-consistent, expected sources (*inconsistency effect,* e.g., Küppers & Bayen, 2014). On the other hand, source guessing is typically biased in favor of the expected source (e.g., Bayen, Nakamura, Dupuis, & Yang, 2000). These effects are well established, however, it is unknown whether people are metacognitively aware of the inconsistency effect on source memory. Awareness of the inconsistency effect could explain schema-consistent source guessing as strategic compensation for the worse memory for expected sources (cf. Küppers & Bayen, 2014). Therefore, I investigated the effects of schemas on memory, metamemory and guessing bias in seven experiments. At study, object word items were presented with either an expected room (e.g., oven in the kitchen) or an unexpected room (e.g., microwave in the bathroom). At test, the studied and new objects were presented and participants decided whether an object had been presented during study, and, if so, with which source. Item-memory and source-memory judgments were obtained at different points during the experiments (prior to or during study, and prior to, during or after the test).

The results consistently showed a metamemory *expectancy illusion*: There was no effect of schemas on item memory, and either no effect or an inconsistency effect on source memory. In contrast, participants predicted an expectancy effect on memory, that is, better item memory and source memory for expected versus unexpected source–item pairs. This was the case in item-wise judgments (Experiments 1.1 and 1.2) and global judgments (for source memory only, Experiment 1.3). This effect was more pronounced in source-memory judgments.

Experiments 2.1, 2.2, and 2.3 showed that the metamemory expectancy illusions are based on two factors. First, a-priori convictions about the effect of schemas on memory moderated the expectancy effects on item-wise judgments. Second, experiences people make in the moment of study such as processing fluency additionally contributed to these expectancy effects. These factors influenced item-memory and source-memory judgments differentially. Experiences predominantly influenced item-memory judgments, whereas convictions predominantly

influenced source-memory judgments. These results highlight differences between different judgment types.

The metamemory expectancy illusion has important implications for the interpretation of the source-guessing bias. Because participants were not aware of the inconsistency effect on source memory during study (Experiments 1.1, 1.2), prior to or during the test (Experiment 1.3), they could not strategically compensate for it. Participants' metamemory convictions and source guessing did not correlate at any point. In Experiment 1.4 I manipulated metamemory convictions about the impact of schemas on source memory. This manipulation did not affect source guessing. Thus, schema-consistent source guessing does not reflect a compensatory strategy. Rather, guessing seems to be based on both a biased contingency representation and mere schema reliance.

Thus, the contributions of the work presented here are threefold: First, there exists a metamemory illusion concerning the influence of schemas on item memory and source memory. Second, both a-priori convictions and experiences are causes for this illusion. Third, this illusion excludes a compensatory strategy as explanation for schema-consistent source guessing.

Introduction[1]

Source monitoring involves cognitive processes that contribute to attributing information to its origin (e.g., Johnson, Hashtroudi, & Lindsay, 1993). Remembering not only information itself but also the source of information is critical in daily life. For example, assessing the validity of a remembered statement, say, that climate change is a hoax, requires not only to remember the statement itself (item memory), but also to remember the source of that information (source memory). Source-monitoring processes are differentially influenced by schematic knowledge. Schemas are organized knowledge about the world based on prior experiences (Alba & Hasher, 1983). Whereas schematically unexpected sources are remembered better than expected information (*inconsistency effect*, e.g., Küppers & Bayen, 2014), people tend to guess the expected source if they do not remember the actual source (*schema-consistent guessing*, e.g., Bayen, Nakamura, Dupuis, & Yang, 2000). These effects are well established, however, it is unknown whether people are metacognitively aware of the inconsistency effect on source memory. In the research reported here, I examined the interplay of schemas, source-monitoring processes and metamemory (assessment of one's own memory ability). The contributions of the work presented here are threefold: First, I will establish metamemory illusions concerning the influence of schemas on item memory and source memory. Second, I will determine causes for these illusions. Third, I will show that the illusion about source memory has important theoretical implications for source guessing.

1.1 Measuring Source-Monitoring Processes

In the laboratory, the effects of schemas on source-monitoring processes are typically examined as follows. At study, information is presented with either a schematically expected or unexpected source (e.g., Bayen et al., 2000; Hicks & Cockman, 2003; Mather, Johnson, & De Leonardis, 1999; Sherman & Bessenoff, 1999). For example, Küppers and Bayen (2014) used scenes (kitchen and bathroom) as sources and object words as items. In particular, they presented items either with their expected scene (e.g., oven in the kitchen) or with their unexpected scene (e.g., microwave in the bathroom). At test, the old items and new, not previously presented ones are presented and participants have to decide whether the item been presented at study and, if so, with which of the sources.

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[1]The introduction was partially adopted from Manuscript 1 in the Appendix.

Participants' responses in such source-monitoring tests are influenced by both memory and guessing processes (e.g., Batchelder & Riefer, 1990; Bayen, Murnane, & Erdfelder, 1996; Bröder & Meiser, 2007; Murnane & Bayen, 1996). These memory and guessing processes are differentially influenced by schemas, thus, it is imperative to disentangle these processes to reach valid conclusions about schema effects on memory. However, traditional measures of source-monitoring performance (e.g., correct source identifications conditionalized on item identification) confound source memory and source guessing (Bayen et al., 1996; Bröder & Meiser, 2007; Murnane & Bayen, 1996). Multinomial processing tree (MPT) models of source monitoring solve this problem by allowing separate measurement of source-monitoring processes such as item memory, source memory, old/new guessing, and source guessing (Batchelder & Riefer, 1990; Bayen et al., 1996; Bröder & Meiser, 2007). Thus, MPT models have been frequently used to analyze data from the source-monitoring paradigm (e.g., Bayen & Kuhlmann, 2011; Bayen et al., 2000; Bell, Buchner, Kroneisen, & Giang, 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kuhlmann & Touron, 2011; Kuhlmann, Vaterrodt, & Bayen, 2012; Küppers & Bayen, 2014; Meiser, Sattler, & Von Hecker, 2007). In all experiments reported here I used the two-high-threshold MPT model of source monitoring (Bayen et al., 1996) to analyze data from the source-monitoring test. Thus, I obtained separate measures of item memory, source memory and source-guessing bias.

Figure 1 displays the processing tree structure of the MPT model of source monitoring. The three trees in the model correspond to items presented with the expected source, items presented with the unexpected source, and new items, respectively. For each item, participants have three response options: "expected source", "unexpected source", or "new."

The first tree in Figure 1 represents the processing tree for items that had been presented with the expected source at study. With probability D_{expected}, participants may recognize an item as old (item memory). Additionally, with probability d_{expected}, they may remember that the item had been presented with the expected source (source memory). In this case, participants will give the correct answer that the item had been presented with the expected source. However, with probability $1 - d_{\text{expected}}$, participants may not remember the source of an item. In this case, they must guess the source. They may guess that an item had been presented with the expected source with probability g, resulting in a correct answer. Alternatively, with probability $1 - g$, they may guess that the item had been presented with the unexpected source, resulting in an incorrect answer. With probability $1 - D_{\text{expected}}$, participants may not recognize the item. In this case, with probability *b*, they may guess that the item was old, and then they may either guess that it had been presented with the expected source (with probability *g*, resulting in a correct answer) or with the unexpected source (with probability 1 - *g*, resulting in an incorrect answer). Alternatively, with probability 1 - *b*, participants may guess that an item was new, resulting in a false rejection of the item as new.

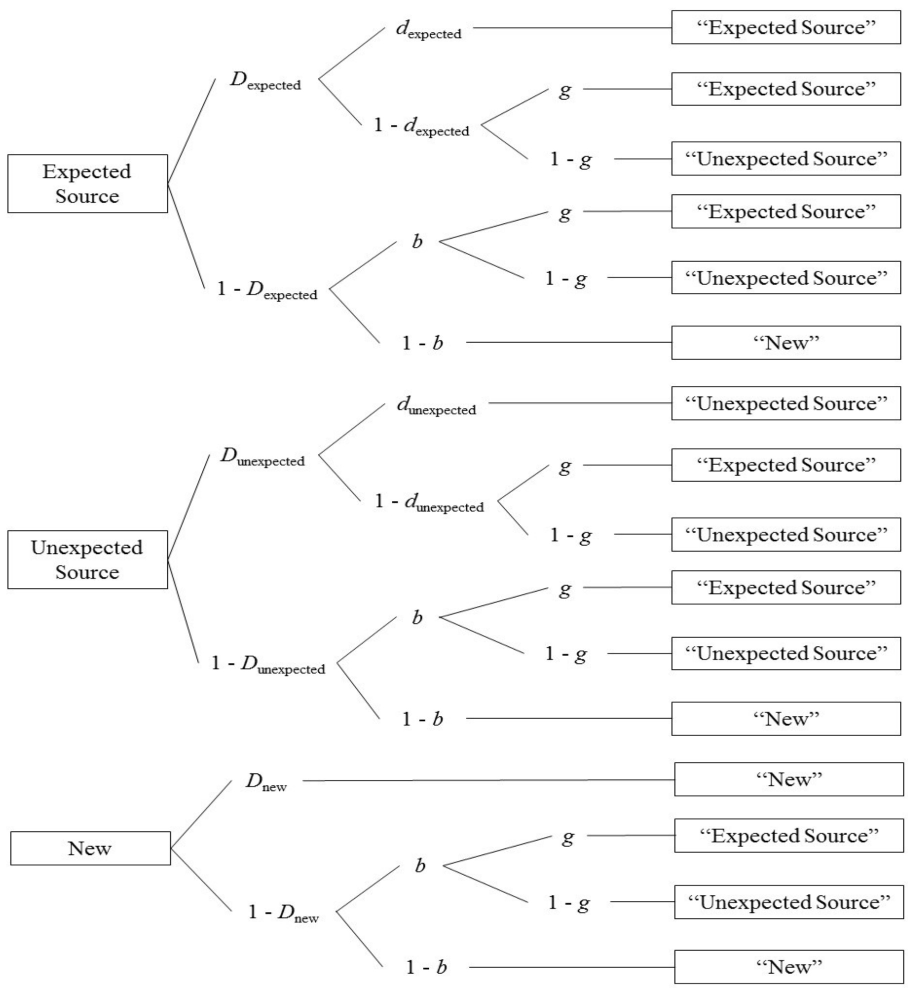

Figure 1. Two-high-threshold multinomial model of source monitoring. $D_{expected}$ = probability of recognizing an item that had been presented with the expected source; $D_{unexpected}$ = probability of recognizing an item that had been presented with the unexpected source; D_{new} = probability of knowing that a new item is new; $d_{expected}$ = probability of remembering that an item was presented with the expected source; $d_{unexpected}$ = probability of remembering that an item was presented with the unexpected source; *g* = probability of guessing that an item had been presented with the expected source; *b* = probability of guessing that an item was old. The original version of the model does not assume source guessing for recognized and non-recognized items to be equal. Adapted from "Source Discrimination, Item Detection, and Multinomial Models of Source Monitoring," by U. J. Bayen, K. Murnane, and E. Erdfelder, 1996, *Journal of Experimental Psychology: Learning, Memory, and Cognition, 22,* p. 202, Figure 3. Copyright 1996 by the American Psychological Association.

Following the same logic, the second tree refers to items presented with the unexpected source. The probability of recognizing such items is $D_{\text{unexpected}}$ and the probability of remembering their source is $d_{\text{unexpected}}$. The third tree refers to items that had not been presented during study. D_{new} is the probability of knowing that such a distractor item is new.

In traditional MPT analyses, data are aggregated over all items and participants and parameters for each of the presented probabilities are estimated from these aggregated data. We used this method in Experiments 2.2 and 2.3. These traditional analyses assume that items and participants are homogeneous in respect to their parameter estimates. However, homogeneity assumptions are often violated and have been shown to lead to biased parameter estimates (Klauer, 2010; Klauer & Kellen, 2010; Smith & Batchelder, 2008, 2010). Further, traditional analyses only provide group-level parameter estimates but not individual estimates, preventing calculation of correlations or regressions between parameters as well as between parameters and covariates. Bayesian-hierarchical approaches address these issues by drawing individual parameters for each participant from an overarching group distribution (Klauer, 2010; Smith & Batchelder, 2010). In particular, the latent trait approach (Klauer, 2010) draws participant parameters from a multivariate normal distribution of probit transformed parameters. Via Bayesian modeling techniques an a-priori distribution is updated to a posterior distribution for each parameter given the data (Bayes' theorem). The Markov Chain Monte Carlo algorithm draws a large number of samples from the posterior parameter distribution. Thus, individual parameter estimates and corresponding parameter distributions are obtained. The parameter distributions are characterized by the Bayesian Credibility Interval which indicates the interval in which the true parameter can be found with 95% confidence. In contrast to the traditional approach, this method has the advantage that correlations between parameters as well as correlations between parameters and covariates can be obtained. In the work presented here, I used hierarchical MPT modeling to disentangle source-monitoring processes in Experiments 1.1, 1.2, 1.3 and 1.4 to regress source-monitoring processes to metamemory.

1.2 Schema Effects on Source-Monitoring Processes

1.2.1 Inconsistency Effect on Source Memory

According to the attention-elaboration account (Brewer & Treyens, 1981; Erdfelder & Bredenkamp, 1998; Friedman, 1979; Küppers & Bayen, 2014; Loftus & Mackworth, 1978), schemas influence memory as follows. Unexpected information (i.e., information that is inconsistent with the schema) attracts more attention than expected information during encoding. Consequently, unexpected information is processed in a more elaborate way. This asymmetrical attention distribution results in better memory for unexpected than expected information, that is, an inconsistency effect.

Tasks that require either item recall or item recognition usually yield an inconsistency effect on item memory, if results are corrected for response bias (for reviews see Rojahn & Pettigrew, 1992; Stangor & McMillan, 1992). In source-monitoring tasks, schemas should primarily influence source memory, because it is the source that renders an item expected or unexpected, not the item per se (cf. Ehrenberg & Klauer, 2005). For example, glasses are not generally unexpected but seeing them in the bread box is. Thus, according to the attention-elaboration account, source memory should be enhanced for items that originated from an unexpected source. Such an inconsistency effect specifically on source memory has frequently been found in source-monitoring studies that obtained unconfounded measures of source memory via MPT modeling (Bell et al., 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kroneisen, Woehe, & Rausch, 2015; Küppers & Bayen, 2014).[2]

1.2.2 Schema-consistent Source Guessing

When individuals have no source memory for a particular item, they will more often guess that it had been presented with the schematically expected than unexpected source (e.g., that the oven had been presented with the kitchen and not the bathroom). That is, people generally show a *schema-consistent guessing bias* (e.g., Bayen & Kuhlmann, 2011; Bayen et al., 2000; Bell et al., 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kroneisen et al., 2015; Kuhlmann et al., 2012; Küppers & Bayen, 2014).

As an explanation for this schema-consistent guessing bias three distinct (but not necessarily mutually exclusive) strategies have been proposed. The *probability-matching account of source guessing* (e.g., Arnold, Bayen, Kuhlmann, & Vaterrodt, 2013; Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002) states that people will match their guessing bias to their perceived source–item contingency at study (*contingency-based* guessing). However, if they do not have a representation of this contingency, they will guess according to their schematic expectations (*schema-based* guessing). Thus, according to this account, schema-

[2]Note that this inconsistency effect on source memory seems to be confined to source–item pairs that strongly violate schematic expectations, whereas it does not seem to occur when items are only somewhat unexpected for their source (Bayen & Kuhlmann, 2011; Bayen et al., 2000; Kuhlmann et al., 2012; cf. Küppers & Bayen, 2014).

consistent source guessing can either be due to a biased contingency representation, and/or reliance on schematic expectations.

As a third, alternative strategy, Küppers and Bayen (2014) proposed that schema-consistent source guessing reflects a metacognitive strategy to compensate for the relatively poor memory for expected sources (see also Ehrenberg & Klauer, 2005). More generally, a *compensatory-guessing hypothesis* states that people strategically adjust their response bias when they are aware of a memory disadvantage (Batchelder & Batchelder, 2008; Meiser et al., 2007). Specifically, they guess the source for which they believe their source memory to be worse. If this belief is valid, this strategy may serve to compensate and to reduce performance differences between the sources. Thus, this strategy can be understood as a form of metacognitive control (Nelson & Narens, 1990). One goal of the current work was to identify which of the three proposed guessing strategies (contingency-based, schema-based, and/or compensatory) contribute to the schema-based guessing bias.

1.3 Metacognitive Monitoring of Item Memory and Source Memory

The work presented here will focus on people's assessments of schema effects on item memory and source memory. In particular, I asked whether people are aware of the inconsistency effect on source memory. Metacognition about item memory is often assessed via *Judgments of Learning* (JOLs, e.g., Rhodes, 2016). After item presentation, participants are asked to judge the likelihood of remembering this item at test. Similarly, metacognition about source memory can be assessed via *Judgments of Source* (JOSs, e.g., Carroll, Mazzoni, Andrews, & Pocock, 1999), independent of item-memory judgments. In JOSs, after the presentation of a source–item pair, participants are asked to predict the likelihood of later remembering the source of this item. These judgments are often assessed item-wise during study, however, some studies have assessed aggregate item-memory judgments after the study phase (e.g., Besken & Mulligan, 2013, 2014; Connor, Dunlosky, & Hertzog, 1997; Koriat, Bjork, Sheffer, & Bar, 2004; Rhodes, 2016). Such ratings differentiate between different trial types (e.g., expected and unexpected source–item pairs) and are commonly referred to as *global differentiated predictions* (GPREDs, Frank & Kuhlmann, 2017; Kornell, Rhodes, Castel, & Tauber, 2011) or *aggregate JOL*s (e.g., Besken & Mulligan, 2013, 2014). I will reserve the terms JOL and JOS for item-wise judgments during study. In contrast, I will use the term item-memory or source-memory GPREDs for aggregate predictions at different points in the experiments. Judgments made after test will be referred to as *global differentiated* item-memory or source-memory *postdictions* (GPOSTs, see Frank & Kuhlmann, 2017).

Research on the influence of conceptual relatedness on metamemory predictions (specifically JOLs) suggests an expectancy effect on metamemory judgments. That is, people should predict better memory for expected source–item pairs versus unexpected pairs. Studies on the paired-associates paradigm have shown that people predict a greater likelihood of remembering an item in response to a cue when this cue is conceptually related to the item (e.g., cat – dog) than when it is unrelated (e.g., cat – spoon; relatedness effect; Hertzog, Kidder, Powell-Moman, & Dunlosky, 2002; Mueller, Tauber, & Dunlosky, 2013; Rabinowitz, Ackerman, Craik, & Hinchley, 1982; Undorf & Erdfelder, 2011, 2013, 2015). However, the effects of conceptual relatedness on metamemory in the source-monitoring paradigm have not been thoroughly investigated yet. There are only few studies on metacognition in schema-based source monitoring. Konopka and Benjamin (2009) found higher JOLs for items presented with their expected source compared to their (somewhat) unexpected source. Similarly, Shi, Tang, and Liu (2012) found that participants predicted source-identification performance (i.e., the likelihood of attributing the item to the correct source) to be better for expected than unexpected source–item pairs. Both studies did, however, not assess JOSs. Likewise, research on conceptual relatedness and metamemory has thus far mostly focused on item-memory judgments and, thus, JOSs are understudied. In the few studies that have examined JOSs for conceptually unrelated source–item material (Carroll, Davis, & Conway, 2001; Carroll et al., 1999; Dutton & Carroll, 2001; Kelly, Carroll, & Mazzoni, 2002; see Kuhlmann & Bayen, 2016, for an overview), JOSs were highly correlated with JOLs, suggesting that people use the same cues for both judgments. Taken together, these results suggest expectancy effects on metamemory judgments in the source-monitoring paradigm. The current work will establish these effects and investigate their causes.

Two factors have been controversially discussed as causes for the relatedness effect on JOLs. First, people hold the a-priori conviction that related items are better remembered than unrelated items even prior to study (Mueller et al., 2013). Second, conceptually related pairs elicit a more fluent processing experience than unrelated pairs, which may further contribute to the higher JOLs for related pairs (Undorf & Erdfelder, 2011, 2013, 2015, see also Alter & Oppenheimer, 2009; Begg, Duft, Lalonde, Melnick, & Sanvito, 1989; Jacoby, Woloshyn, & Kelley, 1989; Kelley & Jacoby, 1996; Koriat, 1997; Koriat & Ma'ayan, 2005). In the source-monitoring paradigm, people may similarly apply their a-priori conviction about relatedness and memory to predict better memory when the item is conceptually related to (i.e., expected for) its source compared to when it is unrelated to its source. Additionally, the conceptual relatedness of items and their sources in expected pairs may result in more fluent encoding (cf. Sherman, Lee, Bessenoff, & Frost, 1998), which may also result in participants' prediction of better memory for expected compared to unexpected source–item pairs. Both accounts (a-priori convictions and in-the-moment experience) thus predict an *expectancy effect* on metamemory judgments in source-monitoring tasks. I will investigate whether a-priori convictions or in-the-moment experiences contribute to JOLs and JOSs in the source-monitoring paradigm.

1.4 The Current Work

Based on the existing literature I derived three main goals for the work presented here. First, there should be an expectancy effect on metamemory judgments in the source-monitoring paradigm similar to the relatedness effect on JOLs in the paired-associates paradigm (e.g., Undorf & Erdfelder, 2015). To anticipate, this was the case for both item-memory and source-memory judgments. Thus, metamemory judgments are dissociated from item memory (showing no effect of schemas) and source memory (showing an inconsistency effect), that is, they show expectancy illusions. Additionally, JOLs and JOSs are differentially influenced by the strength of the schematic relatedness between items and sources.

Second, I aimed to identify the causes of these metamemory expectancy illusions. In particular, as reviewed, a-priori convictions and in-the-moment experiences have been discussed as contributors to JOLs in the paired-associated paradigm (e.g., Mueller et al., 2013; Undorf & Erdfelder, 2011, 2013, 2015). Both factors play different roles in JOL versus JOS formation, with a-priori convictions playing a larger role in JOSs and in-the-moment experiences in JOLs.

Third, the metamemory illusion in source-monitoring has important implications for the nature of the source guessing bias. As established, there are three strategies that may explain schema-consistent guessing: contingency-based guessing, schema-based guessing, and compensatory guessing. The metamemory expectancy illusion rules out compensatory guessing as the driving factor in schema-consistent source guessing. Instead, the current results suggest that the guessing bias is comprised of both contingency reliance and schema reliance.

2 Metamemory Expectancy Illusions

In the following, I will present experiments that show that metamemory convictions about the impact of schemas on memory are dissociated from actual memory.

2.1 Theoretical and Empirical Background

As introduced, unexpected source–item pairs are remembered better than expected pairs (Bell et al., 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kroneisen et al., 2015; Küppers & Bayen, 2014). The first aim of the current work was to establish whether people are aware of this inconsistency effect on source memory at any point during the experiment.

In the paired-associates paradigm, people predict better memory for related than unrelated pairs (i.e., show a relatedness effect on JOLs, e.g., Undorf & Erdfelder, 2015). A similar effect should occur on JOLs in the source-monitoring paradigm. That is, people should predict better item memory for expected source–item pairs than for unexpected pairs (i.e., show an expectancy effect). Because Carroll et al. (1999) showed that JOLs and JOSs are usually correlated and presumably rely on similar cues, the expectancy effect should also be present on JOSs. That is, people may predict an expectancy effect on item memory and source memory, but should show no effect on actual item memory and an inconsistency effect on source memory. Thus, people should show a metamemory expectancy illusion about source memory. To determine changes in metamemory convictions over the course of the experiment, metamemory was assessed during study, as well as prior to, during, and after test.

2.2 Overview of the Studies

2.2.1 Experiment 1.1

The aim of Experiment 1.1 was to establish the proposed metamemory expectancy illusion on JOLs and JOSs. I used the standard source-monitoring paradigm with schematic material and kitchen and bathroom as sources. At study, half of the object-word items were presented with their expected scene (e.g., oven in the kitchen), and the other half with their unexpected scene (e.g., microwave in the bathroom). At test, the studied items and new distractors were presented. Participants decided whether each object had been presented during study, and, if so, with which source. One group of participants provided item-wise JOLs and JOSs during study (*JOL & JOS group*). They also provided GPOSTs at the end of test to capture changes in convictions after test. Because item-wise metamemory judgments have been shown to alter memory (Besken & Mulligan, 2014; Soderstrom, Clark, Halamish, & Bjork, 2015; Susser, Mulligan, & Besken, 2013), a control group that did not provide metamemory judgments during study (*post-only group*) was additionally included.

In the JOL & JOS group, both JOLs and JOSs showed expectancy effects. Notably, the expectancy effect was stronger on JOSs than JOLs. In contrast, item memory was not affected by source–item expectancy in both groups. Source memory, as predicted, showed an inconsistency effect in the post-only group (which was correctly postdicted in the GPOSTs). Interestingly, the JOL & JOS group did not show this inconsistency effect (and postdicted an expectancy effect in the GPOSTs).

Thus, there are two conclusions from this experiment: First, JOLs and JOSs showed at least a single dissociation from item memory and source memory, respectively. Assuming that the post-only group also had the conviction of an expectancy effect, JOSs were even doubly dissociated from source memory. Second, the results suggest that providing metamemory judgments during study changes the encoding of source–item pairs. This is in line with research showing that item-wise JOLs reduce memory differences in general (Besken & Mulligan, 2014; Soderstrom et al., 2015; Susser et al., 2013).

2.2.2 Experiment 1.2

Experiment 1.2 served to replicate the expectancy effects on JOLs and JOSs in Experiment 1.1 while avoiding possible mutual contamination between the judgments. Therefore, a JOL & JOS group (replicating Experiment 1.1) and, additionally, a group that provided only JOLs (*JOL-only group*) and a group that provided only JOSs (*JOS-only group*) were included. The expectancy effects on JOLs and JOSs replicated regardless of whether participants provided single or dual judgments. However, the size of the expectancy effects differed between groups. As in Experiment 1.1, the expectancy effect was stronger on JOSs than JOLs, and it was independent of whether JOLs had been provided alongside or not. For JOLs, the size of the expectancy effect depended on the judgment group: If participants were not able to separately predict source memory (in the JOL-only group), the expectancy effect on JOLs was stronger than when they had this opportunity (in the JOL & JOS group). This suggests that convictions about source memory contaminated JOLs when there was no chance to express these convictions (as, for example, in Konopka & Benjamin, 2009).

As in Experiment 1.1, source memory was influenced by judgment provision. There was no inconsistency effect in the JOL & JOS group and JOS-only group. Notably, the inconsistency effect was intact in the JOL-only group. This suggests that the altering effect of judgment provision on memory is specific to the judgment content.

2.2.3 Experiment 1.3

In Experiments 1.1 and 1.2, item-wise JOLs and JOSs during study showed illusory expectancy effects. However, item-wise judgments might be influenced by in-the-moment experiences in particular (e.g., Undorf & Erdfelder, 2015), because the judgment is provided right in the moment of study. Thus, JOLs and JOSs might fail to reflect a general metacognitive awareness of the effects of schemas on memory. Furthermore, awareness of these schema effects may develop over the course of the test phase (as indicated by the GPOSTs of the post-only group in Experiment 1.1). I therefore sought to replicate the metamemory expectancy illusions with global judgments and observe their development prior to, during (in GPREDs, Kornell et al., 2011) and after test (in GPOSTs). Therefore, Experiment 1.3 included three groups. The *pre-post group* provided GPREDs prior to test and GPOSTs after the test. The *mid-post group* provided GPREDs in the middle of the test and GPOSTs after the test. The *post-only group* provided GPOSTs after the test only (as in Experiment 1.1).

Importantly, the illusory expectancy effect on JOSs replicated in source-memory GPREDs in the pre-post group. Notably, item-memory GPREDs did not show an expectancy effect. The mid-post and post-only groups updated their metamemory convictions over the course of the test. During the test, participants neither showed an expectancy effect nor an inconsistency effect. After test, participants in all three groups showed an inconsistency effect on GPOSTs, suggesting that they learned from test experience.

Concerning source memory, the inconsistency effect replicated in the post-only group (as in Experiment 1.1 and Küppers & Bayen, 2014). However, the pre-post and mid-post groups did not show this effect, suggesting that global judgments influence memory similarly to item-wise judgments. Thus, source-memory convictions were at least single dissociated from source memory prior to test. However, the item-memory dissociation did not replicate with global judgments.

2.3 Discussion

The experiments reported here show a stable metamemory expectancy illusion on item-memory and source-memory judgments in the source-monitoring paradigm. Participants predicted better item memory and source memory for expected versus unexpected source–item pairs (i.e. an expectancy effect). These expectancy effects replicated with item-wise versus global, and single versus dual judgments.

In contrast to the metamemory results, item memory showed no effect of schemas. Source memory showed no effect of schemas, or an inconsistency effect when no source-memory judgments were provided (in the “naïve” post-only groups in Experiments 1.1 and 1.3, and the JOL-only group in Experiment 1.2). Thus, the expectancy effects on metamemory do not accurately reflect the effects of schemas on memory.

Notably, the expectancy effects reported here are in line with the (non-illusory) relatedness effect on JOLs in the paired-associates paradigm (Hertzog et al., 2002; Mueller et al., 2013; Rabinowitz et al., 1982; Undorf & Erdfelder, 2011, 2013, 2015). Concerning the relatedness effect, two determinants are controversially discussed, namely a-priori convictions about the effects of schemas on memory and in-the-moment experiences people make while studying. The different findings for JOLs versus JOSs suggest that they may differ in their reliance on these factors. In the next section, I will show that both are at play in JOL and JOS formation in the source-monitoring paradigm with schematic material.

3 Determinants of the Metamemory Expectancy Illusion

3.1 Theoretical and Empirical Background[3]

Experiments 1.1, 1.2 and 1.3 demonstrated metamemory expectancy illusions for both item-memory and source-memory judgments. After establishing these illusions I asked why people are not able to correctly monitor their item memory and source memory in schema-based source monitoring. According to the cue-utilization approach by Koriat (1997) people use different cues for their (item-wise) metamemory judgments that may or may not be predictive of later memory. Specifically, Koriat proposed three types of cues: 1) *Intrinsic cues* involve item characteristics (e.g., semantic relatedness of word pairs). 2) *Extrinsic cues* involve characteristics of the study conditions (e.g., list length) and the learner's encoding processes (e.g., forming a mental image of the to-be-remembered information). 3) *Mnemonic cues* involve internal experiences of the learner during study (e.g., the ease with which an item is processed). According to this framework intrinsic and extrinsic cues may influence metamemory judgments either directly, via a-priori convictions about the influence of the respective cue, or indirectly via mnemonic experiences a person makes while studying the material.

In general, both a-priori convictions and in-the-moment experiences have been controversially discussed as determinants for metamemory effects. Some research suggests that a-priori convictions alone explain a multitude of metamemory effects (Mueller, Dunlosky, & Tauber, 2016; Mueller, Dunlosky, Tauber, & Rhodes, 2014; Mueller et al., 2013). However, other research has argued that both a-priori convictions and experiences contribute to these effects (Frank & Kuhlmann, 2017; Kelley & Jacoby, 1996; Koriat, 1997; Undorf & Erdfelder, 2011, 2013, 2015).

In particular, concerning the paired-associates paradigm, Mueller et al. (2013) have shown that participants hold the conviction of an expectancy effect on JOLs. However, they did not show a direct influence of this conviction on JOLs. In contrast, Undorf & Erdfelder (2011, 2013, 2015) have argued that in-the-moment experiences contribute to the relatedness effect in addition to convictions. This research has focused mostly on JOLs, and there is no research on how these factors influence JOS formation.

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[3] Section 3.1 was partially adopted from Manuscript 2 in the Appendix.

Similarly to convictions about relatedness on item memory, participants may already hold convictions about the influence of schemas on source memory. Thus, it is possible that participant's a-priori convictions about expectancy effects on item memory and source memory influence JOLs and JOSs. In particular, a-priori convictions should moderate the expectancy effects on JOLs and JOSs, such that people holding the a-priori conviction of an expectancy effect should also show one on JOLs and JOSs and vice versa (for a similar approach for the volume effect, see Frank & Kuhlmann, 2017).

However, a-priori convictions may not fully account for the expectancy effects on JOLs and JOSs (e.g., Frank & Kuhlmann, 2017), because in-the-moment experiences such as processing fluency may additionally play a role (cf. Undorf & Erdfelder, 2015). Specifically, expected source–item pairs should be more fluently processed that unexpected pairs, resulting in higher judgments. One prominent measure of processing fluency is self-paced study time (e.g., Undorf & Erdfelder, 2015). Study times should be shorter for expected pairs (as shown by Sherman et al., 1998). Further, people should use their study time as indication of processing fluency (Koriat, Ma'ayan, & Nussinson, 2006) and, thus, study time should mediate the expectancy effects on JOLs and JOSs.

At this point, it is unknown whether JOLs and JOSs rely on these factors in the same way. On the one hand, JOLs and JOSs are usually correlated and show similar effects (Carroll et al., 2001, 1999; Dutton & Carroll, 2001). Thus, their reliance on a-priori convictions and experiences may be the same. On the other hand, the research reported earlier shows differences between JOLs and JOSs (for another example of diverging JOLs and JOSs, see Kelly et al., 2002). Thus, it seems more likely that JOLs and JOSs rely on different factors, or that these factors are weighted differently.

3.2 Overview of the Studies

In three experiments, I determined whether a-priori convictions and/or in-the-moment experiences impact the expectancy effects on JOLs and JOSs. I first determined a-priori convictions about item memory and source memory in the source-monitoring paradigm with schematic material (Experiment 2.1). I then asked whether these convictions contributed to the expectancy effect on JOLs and JOSs (Experiments 2.2 and 2.3). In addition, Experiment 2.3 simultaneously determined the influence of in-the-moment experiences on JOL and JOS formation.

3.2.1 Experiment 2.1

In Experiment 2.1 I determined a-priori convictions about the impact of schemas on item memory and source memory. I conducted an online survey in which a former study using the schema-based source monitoring paradigm was explained to participants. Then, participants were asked to predict the results of that former study. As in Experiment 1.2, participants were asked for either item-memory judgments, source-memory judgments, or both. Participants predicted memory separately for expected and unexpected source–item pairs. Participants' a-priori convictions showed a small expectancy effect, however, there were also many participants who predicted an inconsistency effect on item memory or source memory. In addition, convictions did not mirror the stronger expectancy effect on JOSs versus JOLs (see Experiments 1.1, 1.2). Because of these slight differences between a-priori convictions (obtained here) and JOL/JOS convictions (reported earlier), it seems unlikely that a-priori convictions fully account for the expectancy effects on JOLs and JOSs.

3.2.2 Experiment 2.2

Experiment 2.2 tested whether a-priori convictions accounted for the expectancy effects on JOLs and JOSs. In particular, the indicated a-priori conviction should moderate the expectancy effects, that is, participants who show stronger expectancy effects a priori should also show stronger expectancy effects on JOLs and JOSs and vice versa (following Frank & Kuhlmann, 2017). Participants provided their a-priori item-memory and source-memory convictions prior to study. The rest of the experiment followed the procedure of the JOL & JOS group of Experiment 1.1.

The results for JOLs and JOSs replicated previous experiments. Participants showed expectancy effects on JOLs and JOSs, independent of the indicated a-priori conviction. However, as predicted, a-priori convictions moderated the expectancy effect on both JOLs and JOSs. Additionally, this moderating effect was stronger for JOSs than JOLs. That is, JOSs were more dependent on a-priori convictions than JOLs. However, a-priori convictions did not fully account for the expectancy effects on both JOLs and JOSs indicating that other factors, such as in-the-moment experiences, might additionally be at play.

3.2.3 Experiment 2.3

Experiment 2.3 served to replicate the moderating effect of a-priori convictions on the expectancy effects on JOLs and JOSs while simultaneously measuring the influence of in-the-moment experiences. Because study time has been proposed as a measure of in-the-moment experiences (Castel, McCabe, & Roediger, 2007; Koriat, 2008; Koriat & Ackerman, 2010; Koriat et al., 2006; Miele, Finn, & Molden, 2011; Undorf & Erdfelder, 2011, 2013, 2015), I let participants decide how long they wanted to study each source–item pair. The rest of the experiment followed the procedure of Experiment 2.2.

In replication of Experiment 2.2, a-priori convictions moderated the expectancy effect on JOSs. However, this effect did not replicate for JOLs. This shows again that JOSs are more dependent on a-priori convictions than JOLs.

As predicted, study time mediated the expectancy effect on JOLs (replicating Undorf & Erdfelder, 2015). That is, expected items felt more fluently processed than unexpected items, resulting in shorter study times, and, in turn, higher JOLs (see Koriat et al., 2006). However, this effect did not replicate for JOSs. This might indicate that JOSs are not as dependent on in-the-moment experiences as JOLs. Still, JOSs might depend on in-the-moment experiences in some way, because even those participants who initially indicated convictions of an inconsistency effect overall showed an expectancy effect on JOSs. Our results suggest that study time is not an ideal measure of experiences in all cases (cf. Koriat et al., 2006) because participants who initially indicated an inconsistency conviction strategically used study time to compensate for the predicted memory effects. Thus, future research needs to expand on better and more comprehensive measures of in-the-moment experiences.

3.3 Discussion

Both a-priori convictions and in-the-moment experiences contributed to the expectancy effects on JOLs and JOSs. A-priori convictions were not fully reflective of later JOLs and JOSs, but, nonetheless, moderated the expectancy effect on JOSs and, at least in Experiment 2.2, JOLs. That is, participants who showed a stronger expectancy effect on a-priori convictions also showed one on JOSs (and, less so, JOLs). Additionally, in-the-moment experiences influenced JOLs, and, possibly, JOSs (e.g., as indicated by the JOS expectancy effects for inconsistency believers).

Thus, JOLs and JOSs rely on similar factors in line with the cue-utilization approach (Koriat, 1997). However, these factors were weighted differently in JOL versus JOS formation. Specifically, JOSs were more conviction-dependent than JOLs. JOLs on the other hand seemed to be more influenced by in-the-moment experiences. These differences in JOL and JOS formation add more evidence that they are independent judgments and explain, to a part, the stronger expectancy effect on JOSs versus JOLs as a result of the stronger reliance on a-priori convictions in JOSs. Future research should further expand on these differences between metamemory judgments to arrive at a comprehensive theory on judgment formation.

The results thus explain the metamemory expectancy illusion. Both JOLs and JOSs (showing an expectancy effect) were again dissociated from actual item memory (showing no effect) and source memory (showing an inconsistency effect) because JOLs and JOSs rely on factors that are not predictive of future memory. First, a-priori convictions are already erroneous for many participants. Reliance on these wrong convictions thus result in wrong predictions during study. Additionally, processing fluency as a cue for metamemory judgments is not predictive of item memory in this paradigm, and thus further contributes to the illusion in JOLs.

4 Implications for source guessing

4.1 Theoretical and Empirical Background[4]

The established expectancy illusion about source memory has important implications for the interpretation of the schema-consistent source-guessing bias. As introduced, schema-consistent source guessing may be explained by the probability-matching account or a metacognitive, compensatory strategy. The probability-matching account states that people will match their guessing bias to the perceived item-source contingency during study (contingency-based guessing). However, if they lack a representation of this contingency, they will default to guessing the schematically expected source (schema-based guessing; Arnold et al., 2013; Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002). By contrast, compensatory guessing involves guessing the believed-to-be less well remembered source (Batchelder & Batchelder, 2008; Meiser et al., 2007).

People use compensatory guessing in source-monitoring tasks with material that does not elicit schematic expectations. Kuhlmann and Touron (2011) and Meiser et al. (2007) showed that convictions about actual or perceived item-memory difference between sources elicited compensatory guessing, such that participants strategically guessed the source from which (believed-to-be) less memorable items originated. Such metacognitive compensatory source guessing should also occur when participants assume that there are differences in *source* memory (cf. Batchelder & Batchelder, 2008).

It thus seems reasonable to assume that people strategically use schema-consistent guessing to compensate for their relatively poor memory for expected sources. Crucially, however, participants can only strategically compensate for the inconsistency effect in source memory via schema-consistent guessing if they have metacognitive awareness of the inconsistency effect. Based on the research reported so far, this does not seem to be the case. In fact, people assume the opposite, that is, an expectancy effect on source memory. Thus, a compensatory strategy cannot account for the schema-consistent guessing bias. Nonetheless, a compensatory strategy may still contribute to this bias, in that people who are aware of the inconsistency effect may use it in addition to contingency-based or schema-based guessing. This would enhance schema-consistent source guessing for those who believe in an inconsistency

[4] Section 4.1 was partially adopted from Manuscript 1 in the Appendix.

effect on source memory. In addition, people may gain awareness of the inconsistency effect over the course of the test and thus add a compensatory strategy later in addition to, or as a replacement of, contingency-based or schema-based guessing.

We aimed to disentangle the three proposed guessing strategies and determine whether schema-consistent source guessing is a product of a metacognitive control, biased contingency perception, and/or schematic expectations. Therefore, I will focus on further results from Experiments 1.1, 1.2, and 1.3. In addition, Experiment 1.4 involves an experimental test of the compensatory-guessing hypothesis.

4.2 Overview of the Studies

4.2.1 Experiments 1.1 and 1.2

As described, participants in Experiments 1.1 and 1.2 predicted better source memory for expected versus unexpected source–item pairs during study. A compensatory strategy biases guessing towards the source that is predicted to be remembered worse (Batchelder & Batchelder, 2008; Kuhlmann & Touron, 2011; Meiser et al., 2007). Thus, source guessing would have been biased towards the schema-inconsistent source. However, this was not the case. Participants guessed schema-consistently despite an expectancy effect on JOSs. To test whether the metacognitive conviction nonetheless partially predicted source guessing, I regressed source guessing on source-memory convictions during study and after the test. There was no evidence that stronger conviction of an inconsistency effect on source memory resulted in a stronger schema-consistent guessing bias.

Instead, source-guessing was influenced both by a biased representation of the source–item contingency as well as mere schema reliance as predicted by the probability-matching account (Arnold et al., 2013; Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002). Contingency ratings obtained after the test predicted source guessing, but guessing was biased more strongly than the contingency representation was.

4.2.2 Experiment 1.3

In Experiment 1.3, GPREDs were employed at different points during the experiment. As mentioned, participants in the pre-post group predicted an expectancy effect on source memory. I asked whether participants gained awareness of the inconsistency effect on source memory during the course of the test phase. If this was the case, compensatory guessing could be added as a secondary strategy later during the test as a result of the updated metamemory conviction (see Dunlosky & Hertzog, 2000; Hertzog, Price, & Dunlosky, 2008; Koriat & Bjork, 2006). Participants' did update their metamemory conviction as early as the middle of the test. Mid-test GPREDs did not show an expectancy effect. After the test, there was even evidence for an inconsistency effect. However, metamemory convictions prior to, in the middle of, or after the test did not predict source guessing. Thus, again, there was no evidence that schema-consistent source guessing was based on a compensatory strategy. Instead, guessing seems to be based on a combination of a biased contingency representations as well as mere schema reliance (Arnold et al., 2013; Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002).

4.2.3 Experiment 1.4

In Experiment 1.4 I experimentally tested the causal link between source-memory convictions and source guessing as predicted by the compensatory-guessing hypothesis. Peoples' naïve convictions about the influence of schemas on source memory may be too homogeneous to show a meaningful relationship between convictions and source guessing. Thus, I manipulated participants' convictions at the beginning of the experiment. In particular, some participants were told that they would show an expectancy effect on source memory (expectancy-conviction group) and other participants were told that they would show an inconsistency effect on source memory (inconsistency-conviction group). Prior to test, I assessed GPREDs to check whether participants complied with the manipulation. Source-memory convictions differed between the two groups; the expectancy-conviction group showed an expectancy effect and the inconsistency-conviction group showed a marginally non-significant inconsistency effect. Importantly, source guessing did not differ between the groups. Additionally, source-memory convictions prior to or after test did not predict source guessing. These findings again corroborate that schema-consistent source guessing is not based on a compensatory strategy, but rather on strategies proposed by the probability-matching account.

4.3 Discussion

The metamemory expectancy illusion demonstrated in this research has important implications for source-monitoring processes. In particular, I could rule out one explanation for the schema-consistent source-guessing bias proposed in the literature (cf. Küppers & Bayen, 2014). Metacognitive control in form of a compensatory guessing strategy would have biased guessing in favor of the unexpected source. However, all experiments consistently showed biased guessing in favor of the consistent source. In addition, metamemory judgments obtained during study, prior to, in the middle of, and after the test consistently did not predict source guessing. Thus, metacognitive control can be ruled out as a cause for schema-consistent guessing in the source-monitoring paradigm with schematic material.

Of course, this does not imply that people never use compensatory guessing in source-monitoring. In fact, compensatory guessing is used when items and sources do not elicit schematic expectations (Kuhlmann & Touron, 2012; Meiser et al., 2007). However, if they do elicit schematic expectations, other guessing strategies, such as contingency-based and schema-based guessing, seem to be more prevalent.

Overall, there was evidence for both contingency-based and schema-based guessing, as predicted by the probability-matching account (Arnold et al., 2013; Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002). Participants showed a biased contingency representation which partially predicted source guessing. However, consistently, source guessing was even more biased than the contingency bias, indicating that a biased contingency representation alone could not explain schema-consistent source guessing. Rather, it appears that participants additionally defaulted to mere schema reliance. Future studies should disentangle these two strategies further, and determine whether different people use different strategies, or whether both strategies are used in combination by the same person.

5 General Discussion

In seven experiments, I demonstrated a new metamemory expectancy illusion on item memory and source memory. Participants consistently predicted item memory and especially source memory to be better for expected versus unexpected source–item pairs (i.e., expectancy effect). In contrast, item memory was not affected by schematic expectations and source memory was enhanced for unexpected source–item pairs (i.e., inconsistency effect).

The expectancy effects on JOLs, JOSs and GPREDs expands research on the relatedness effect in the paired-associates paradigm (Hertzog et al., 2002; Mueller et al., 2013; Rabinowitz et al., 1982; Undorf & Erdfelder, 2011, 2013, 2015) to source monitoring. Notably, source-memory judgments (JOSs and source-memory GPREDs) showed a stronger expectancy effect than item-memory judgments (JOLs and item-memory GPREDs) prior to test across all experiments. This was the case for both single versus dual judgment groups (JOL vs. JOS, Experiment 1.2). All these novel evidence suggests that item-memory and source-memory judgments differ more than previously assumed (e.g., Carroll et al., 1999, 2001; Kuhlmann & Bayen, 2016), despite their superficial similarities. Apparently, people are aware that schemas influence item memory and source memory differently and are able to express these differences (although their prediction is false). Future research should broaden our understanding of different metamemory judgments.

According to the findings presented here, the illusory expectancy effects on JOLs and JOSs are due to peoples' reliance on two factors in judgment formation. First, in JOSs (and less so in JOLs), participants used their a-priori convictions about the impact of schemas on memory to predict an expectancy effect (Experiments 2.2 and 2.3). Second, in JOLs (and, presumably, in JOSs), participants used in-the-moment experiences (such as an enhanced feeling of processing fluency for expected pairs) as a cue to predict an expectancy effect in item memory (Experiment 2.3). These findings add to the debate whether metamemory judgments are based on a-priori convictions or in-the-moment experiences (Frank & Kuhlmann, 2017; Koriat, 1997; Mueller et al., 2013, 2014, 2016, Undorf & Erdfelder, 2011, 2013, 2015). Both factors play a role in judgment formation. Interestingly, both factors influenced JOLs versus JOSs differently. These results thus offer even more evidence that different judgments capture different aspects of metamemory. Therefore, these studies highlight the importance of examining different judgment types to arrive at a comprehensive theory of metamemory in general.

The metamemory expectancy illusion on source memory rules out the possibility that schema-consistent source guessing may be based on a metacognitive control strategy (i.e., strategically guessing the expected source to compensate for worse memory). Participants were consistently unaware of the inconsistency effect on source memory prior to or during the test. After test, there was some evidence that participants may have gained awareness of the inconsistency effect (Experiments 1.1 and 1.3). However, at no point did their metamemory convictions predict source guessing (Experiments 1.1, 1.2, 1.3, 1.4). Additionally, even direct manipulation of metamemory convictions did not affect guessing bias (Experiments 1.4). Thus, schema-consistent source guessing is not based on a compensatory strategy. Instead, there was consistent evidence that source guessing was based on contingency-based and schema-based strategies as predicted by the probability-matching account (Arnold et al., 2013; Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002). Thus, I could show that assessing metamemory judgments may help understand the causes of memory and guessing processes.

In summary, I demonstrated a new metamemory illusion in schema-based source monitoring. I established in-the-moment experiences and a-priori convictions as determinants of this illusion. The illusion has important theoretical implications regarding source-monitoring processes. Further, the results on different metamemory judgments within the source-monitoring paradigm bring some novel insights on metamemory in general, such as the reactivity of metamemory judgments and contamination between different judgments. Future research should further increase our understanding of the interplay of memory and metamemory in source monitoring.

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Appendix

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Ute J. Bayen

Prof. Ute J. Bayen, Ph.D.

Metamemory Expectancy Illusion and Schema-Consistent Guessing in Source Monitoring

Marie Luisa Schaper

Heinrich-Heine-Universität Düsseldorf

Beatrice G. Kuhlmann

University of Mannheim

Ute J. Bayen

Heinrich-Heine-Universität Düsseldorf

Author note

Marie Luisa Schaper and Ute Johanna Bayen, Institute for Experimental Psychology, Heinrich-Heine-Universität Düsseldorf, Germany. Beatrice G. Kuhlmann, Department of Psychology, School of Social Sciences, University of Mannheim, Germany.

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Correspondence concerning this article should be addressed to Ute J. Bayen, Institut für Experimentelle Psychologie, Gebäude 23.02, Universitätsstr. 1, Heinrich-Heine-Universität Düsseldorf, D-40225 Düsseldorf, Germany. Email: ubayen@hhu.de

Abstract

Source monitoring involves attributing information to one of several sources. Schemas are known to influence source-monitoring processes, with enhanced memory for schematically unexpected sources (inconsistency effect) and biased schema-consistent source guessing. The authors investigated whether this guessing bias reflects a compensatory guessing strategy based on metacognitive awareness of the inconsistency effect or reflects other strategies (as proposed by the probability-matching account). To determine people's awareness of the inconsistency effect, the authors investigated metamemory predictions in a source-monitoring task. Participants studied object word items that were presented with one of two scene labels as sources. Items were either presented with their schematically expected source (e.g., kitchen – oven) or with their schematically unexpected source (e.g., kitchen – toothpaste). In Experiments 1 and 2, participants predicted their item memory and their source memory after each source–item presentation. In Experiment 1, people incorrectly rated both their item memory and, even more so, their source memory to be better for expected than for unexpected source–item pairings. In Experiment 2, this effect replicated with different types of judgment probes. Crucially, item-wise memory predictions did not predict source guessing. In Experiment 3, there were changes in metacognitive awareness of the inconsistency effect on source memory during the test phase. However, metamemory convictions never predicted source guessing. In Experiment 4, we manipulated participants' convictions concerning the impact of schematic expectations on source memory. These convictions did also not predict source guessing. Thus, our results imply that schema-consistent source guessing does not reflect a compensatory strategy.

Keywords: Source monitoring, source guessing, metacognition, schemas, multinomial modeling

Metamemory Expectancy Illusion and Schema-Consistent Guessing in Source Monitoring

Source monitoring involves attributing information to its source or origin such as the episode in which particular information was encountered (Johnson, Hashtroudi, & Lindsay, 1993). Source-monitoring processes are among the many cognitive processes that are influenced by *schemas* (Bayen, Nakamura, Dupuis, & Yang, 2000; Ehrenberg & Klauer, 2005; Küppers & Bayen, 2014; Mather, Johnson, & De Leonardis, 1999; Sherman & Bessenoff, 1999; for a review, see Kuhlmann & Bayen, 2016). Schemas are organized general knowledge about aspects of the world (e.g., scenes, events, or social categories, Alba & Hasher, 1983). In the real world, sources are often related to schemas (e.g., the schema of the *New York Times* as a reliable news source). It is therefore of utmost importance to investigate the processes involved in schema-related source monitoring. As we will explain, the interplay of cognitive and metacognitive processes is of particular theoretical importance in this regard. We conducted the experiments reported in this article to determine whether people are metacognitively aware of effects of schemas on their ability to remember the source of information. We discovered a dissociation of metamemory prior to test and schema-related source memory. As we will show, this dissociation, its development over the test phase, and its manipulability have important theoretical implications regarding source monitoring. In the following paragraphs, we first describe known effects of schemas on source monitoring. We will then review the relevant literature on metacognition and derive hypotheses regarding metamemory predictions of schema-based source monitoring.

Effects of Schemas on Source Monitoring

Source monitoring involves multiple cognitive processes. For example, if you need to find your glasses, different processes may influence how you try to achieve this goal. You may remember the location where you last saw your glasses (i.e., use source memory). Or you may not remember the location but guess that your glasses are most likely where you usually put them (i.e., you guess the source according to your expectations).

In the standard paradigm to investigate effects of schemas on source monitoring, information is presented with one of two sources, either a schematically expected or unexpected source (Bayen et al., 2000; Hicks & Cockman, 2003; Mather et al., 1999; Sherman & Bessenoff, 1999). For example, Küppers and Bayen (2014) presented kitchen and bathroom object items with either a kitchen or a bathroom source. Critically, each item was presented either with its expected source (e.g., toothpaste in the bathroom) or with its unexpected source (e.g., oven in the bathroom). We will henceforth refer to the expectedness of items for their source as *source–item expectancy*. At test, participants then had to decide whether an object had been presented during study and, if so, whether it had been presented with the kitchen or with the bathroom source. Because participants' responses in such a source-monitoring test are influenced by both memory and guessing (e.g., Batchelder & Riefer, 1990; Bayen, Murnane, & Erdfelder, 1996; Bröder & Meiser, 2007; Murnane & Bayen, 1996), measures that disentangle item memory, source memory, and guessing biases are needed. Thus, data from this paradigm are typically analyzed with multinomial processing tree (MPT) models of source monitoring (Batchelder & Riefer, 1990; Bayen et al., 1996; Bröder & Meiser, 2007). These models allow separate measurement of the processes that underlie responses in a source-monitoring task, namely item memory, source memory, and guessing. Separate measurement of these processes is imperative, because, as we will review next, they are differentially influenced by schemas.

Inconsistency Effect on Source Memory

Schemas are thought to influence memory via attention and elaboration (Brewer & Treyens, 1981; Erdfelder & Bredenkamp, 1998; Friedman, 1979; Küppers & Bayen, 2014; Loftus & Mackworth, 1978). According to this account, unexpected information (i.e., information that is inconsistent with the schema) attracts more attention, and is consequently encoded in a more elaborative way than expected information. This asymmetrical distribution of attention thus results in better memory for unexpected than expected information, that is, an *inconsistency effect*.

Tasks that require either item recall or item recognition (if corrected for response bias), usually yield an inconsistency effect on item memory (for reviews see Rojahn & Pettigrew, 1992; Stangor & McMillan, 1992). Ehrenberg and Klauer (2005) proposed that in source-monitoring tasks, schemas should primarily influence source memory, because it is the source that renders an item expected or unexpected, not the item per se. For example, glasses are not an unexpected item per se; however, seeing them in the bread box is. Thus, according to the attention-elaboration account, source memory should be enhanced for items that originated from an unexpected source. An inconsistency effect specifically on source memory, but *not* on item memory, has frequently been found in source-monitoring studies that separately measured memory and guessing via MPT modeling (Bell, Buchner, Kroneisen, & Giang, 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kroneisen, Woehe, & Rausch, 2015; Küppers & Bayen, 2014). It should be noted that this inconsistency effect on source memory appears to be confined to source–item pairings that strongly violate schematic expectations, whereas it does not seem to occur when items are only somewhat unexpected for their source (Bayen &

Kuhlmann, 2011; Bayen et al., 2000; Kuhlmann, Vaterrodt, & Bayen, 2012, cf. Küppers & Bayen, 2014).

Schema-consistent Source Guessing

Source guessing is typically biased in favor of the expected source (Bayen & Kuhlmann, 2011; Bayen et al., 2000; Bell et al., 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kroneisen et al., 2015; Kuhlmann et al., 2012; Küppers & Bayen, 2014). That is, when individuals have no source memory for a particular item, they will likely guess that it had been presented with the schematically expected source (e.g., the oven with the kitchen and not the bathroom).

While this effect of schemas on source guessing is well-established, it is unknown why people apply this schema-consistent bias. Three distinct (but not necessarily mutually exclusive) guessing strategies have been proposed. The probability-matching account of source guessing (e.g., Arnold, Bayen, Kuhlmann, & Vaterrodt, 2013; Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002) states that people will match their guessing bias to their perceived source–item contingency at study (*contingency-based* guessing). However, according to this account, if people do not have a representation of the source–item contingency, they will guess according to their schematic expectations (*schema-based* guessing). Thus, schema-consistent source guessing can be due to a biased contingency representation or due to mere reliance on schematic expectations.

As a third strategy, Küppers and Bayen (2014) proposed that schema-consistent source guessing reflects a metacognitive strategy to compensate for the relatively poor memory for expected sources (see also Ehrenberg & Klauer, 2005). Generally, a *compensatory-guessing hypothesis* states that people adjust their response bias when they are aware of a memory disadvantage for a particular source (Batchelder & Batchelder, 2008; Meiser, Sattler, & Von Hecker, 2007). Specifically, they strategically guess the source for which they believe their source memory to be worse. If this conviction is valid, this strategy indeed serves to compensate and to reduce performance differences between the sources. Thus, this strategy can be understood as a form of metacognitive control (Nelson & Narens, 1990).

Studies have shown that people use compensatory guessing in source-monitoring tasks with neutral material that does not elicit specific schematic expectations. Meiser et al. (2007) and Kuhlmann and Touron (2011) manipulated item memorability between sources and found that source guessing was biased in favor of the source from which the less memorable items had originated. Importantly, item-memory judgments assessed during or after the task revealed participants' awareness of the item-memory difference between sources. Additionally, Meiser et al. showed that *subjective* convictions about item-memory differences between sources are alone sufficient to elicit compensatory source guessing, even when there is no actual item-memory difference. Given this evidence for metacognitive compensatory source guessing, it should also occur when participants assume that there are differences in *source* memory (cf., Batchelder & Batchelder, 2008). It thus seems reasonable to assume that people strategically use schema-consistent guessing to compensate for their relatively poor memory for expected sources. Crucially, however, participants can only strategically compensate for the inconsistency effect in source memory via schema-consistent guessing if they have metacognitive awareness of the inconsistency effect.

In the experiments reported here, we aimed to disentangle the different guessing strategies and to determine whether schema-consistent source guessing is a product of metacognitive control, biased contingency perception, and/or follows pre-existing world knowledge.

Metacognition in Source Monitoring

Metacognition about item memory is often assessed via *Judgments of Learning* (JOLs, e.g., Rhodes, 2016). That is, after item presentation, participants are asked to judge the likelihood of remembering this item at a later test. Similarly, *Judgments of Source* (JOSs) can be used to measure source-memory predictions independent of item-memory predictions (e.g., Carroll, Mazzoni, Andrews, & Pocock, 1999). In such JOSs, after the presentation of a source–item pair, participants are asked to judge the likelihood of later remembering the source of this item. While these judgments are often assessed item-wise during study, some studies have assessed item-memory predictions as an aggregate measure after the completed study phase (e.g., Besken & Mulligan, 2013, 2014; Connor, Dunlosky, & Hertzog, 1997; Koriat, Bjork, Sheffer, & Bar, 2004; Rhodes, 2016). Aggregate ratings that differentiate between different trial types (e.g., expected versus unexpected source–item pairs) are commonly referred to as *global differentiated predictions* (GPREDs, Frank & Kuhlmann, 2017; Kornell, Rhodes, Castel, & Tauber, 2011) or *aggregate JOL*s (e.g., Besken & Mulligan, 2013, 2014). To avoid confusion, we will reserve the terms JOL and JOS for item-wise judgments during study. In contrast, we will use the terms item-memory GPRED and source-memory GPRED for aggregate predictions before completion of the test phase. We will refer to judgments made after the test phase as *global differentiated* item-memory or source-memory *postdictions* (GPOSTs, see Frank & Kuhlmann, 2017).

Based on the compensatory-guessing hypothesis of schema-consistent guessing (Küppers & Bayen, 2014), people are assumed to be aware of the inconsistency effect on source memory. That is, any source-memory judgments for unexpected items should be higher than those for expected items. However, research on the influence of relatedness on metamemory predictions (specifically JOLs) suggests the opposite: Many studies have shown that people predict a greater likelihood of remembering an item (e.g., cat) in response to a cue when this cue is conceptually related to the item (e.g., dog) than when it is unrelated (e.g., spoon; Hertzog, Kidder, Powell-Moman, & Dunlosky, 2002; Mueller, Tauber, & Dunlosky, 2013; Rabinowitz, Ackerman, Craik, & Hinchley, 1982; Undorf & Erdfelder, 2011, 2013, 2015).

There are two factors that contribute to this relatedness effect on metamemory predictions. For one, it has been shown that even before studying the material, people hold the a-priori conviction that related items are better remembered than unrelated items (Mueller et al., 2013). Second, conceptually related pairs evoke the experience of more fluent processing (i.e., processing with greater subjective ease) than unrelated pairs, which further contributes to the larger JOLs for related pairs (Undorf & Erdfelder, 2011, 2013, 2015, see also Alter & Oppenheimer, 2009; Begg, Duft, Lalonde, Melnick, & Sanvito, 1989; Jacoby Woloshyn, & Kelley, 1989; Kelley & Jacoby, 1996; Koriat, 1997; Koriat & Ma'ayan, 2005). In the source-monitoring paradigm, people may similarly apply their a-priori conviction about relatedness and memory to predict better memory when the item is conceptually related to (i.e., expected for) its source compared to when it is unrelated to its source. Additionally, the conceptual relatedness of items and their sources in expected pairings may result in more fluent encoding (cf., Sherman, Lee, Bessenoff, & Frost, 1998), which should also result in participants' prediction of better memory for expected versus unexpected source–item pairings. Both accounts (a-priori convictions and fluency experience) thus predict an *expectancy effect* on metamemory judgments in source-monitoring tasks.

There are only few studies on metacognition in schema-based source monitoring. Konopka and Benjamin (2009) found higher JOLs for items presented by their expected source compared to their (somewhat) unexpected source. Similarly, Shi, Tang, and Liu (2012) found that participants predicted source-identification performance (i.e., the likelihood of attributing the item to the correct source) to be better for expected than unexpected source–item pairings. Both studies did, however, not assess JOSs. Likewise, research on conceptual relatedness and metamemory has thus far mostly focused on item-memory predictions (or, more broadly, text-comprehension predictions, e.g., Jaeger & Wiley, 2014). However, compensatory source guessing should be most strongly connected to convictions about source memory specifically, not about item memory or attribution performance. Thus, in order to test the compensatory-guessing hypothesis, it is crucial to assess source-memory predictions concerning expected and unexpected source–item pairings.

In this regard, it is notable that in the few studies that have examined JOSs for conceptually unrelated source–item material (Carroll, Davis, & Conway, 2001; Carroll et al., 1999; Dutton & Carroll, 2001; Kelly, Carroll, & Mazzoni, 2002; see Kuhlmann & Bayen, 2016, for an overview), JOSs were highly correlated with JOLs, suggesting that people use the same cues for both judgments. Therefore, from the metamemory literature, one would predict an expectancy effect on JOSs, much like the one found on JOLs and attribution-performance predictions (Konopka & Benjamin, 2009; Shi et al., 2012). Nonetheless, it remains possible that people are able to monitor source memory separately from item memory and that they accurately predict the inconsistency effect on source memory after all. Although most studies reported similar predictions in JOLs and JOSs (Carroll et al., 1999, 2001), predictions diverged in one prior study (Kelly et al., 2002). Therefore, it is crucial to assess whether the expectancy effect occurs in JOSs (and, more generally, any source-memory prediction) or not. If it does, this would suggest that schema-consistent source guessing is *not* compensatory: Given a conviction that expected sources are better remembered than unexpected sources, the compensatory-guessing hypothesis predicts a bias to guess *inconsistent* with schematic expectations (i.e., to guess the believed-to-be less well remembered unexpected source). Rather, schema-consistent source guessing despite an expectancy effect on source-memory judgments would instead reflect reliance on a biased contingency representation and/or a more general reliance on schematic expectations as predicted by the probability-matching account (Bayen & Kuhlmann, 2011; Kuhlmann et al., 2012; Spaniol & Bayen, 2002). On the other hand, an inconsistency effect on any source-memory judgment would bolster the interpretation of schema-consistent source guessing as compensatory (cf. Küppers & Bayen, 2014).

The Current Experiments

In four source-monitoring experiments with schematic materials, we assessed metamnemonic judgments in addition to source-monitoring processes. Our main objectives were (1) to determine metacognitive convictions about effects of schematic expectations on item memory and source memory, and (2) to determine the relationship of these convictions to source guessing. Generally, we predicted replication of Küpper's and Bayen's (2014) results; that is, no effect of expectancy on item memory, an inconsistency effect on source memory, and schema-consistent source guessing. However, the interpretation of this guessing bias depends on the metamemory findings. An inconsistency effect on source-memory judgments that is related to source-guessing bias would reflect a metacognitive control strategy (i.e., compensatory guessing of the believed-to-be less well remembered source). By contrast, an expectancy effect on source-memory judgments would rather indicate that in their guessing bias, participants rely on biased contingency perception and/or on general schematic expectations. Based on the reviewed metamemory literature, we deemed an expectancy effect on source-memory judgments more likely.

In all experiments, we used scenes as sources (cf. Küppers & Bayen, 2014). Participants studied items that were either expected in a bathroom (e.g., toothpaste) or expected in a kitchen (e.g., saucepan) with either the word *bathroom* or *kitchen*. Later, they received a standard source-monitoring test. We used the MPT model to disentangle item memory, source memory, and guessing. We assessed metamemory at different points in time. In Experiments 1 and 2, we assessed item-wise JOLs and JOSs during study and GPOSTs after test. To test whether metamemory convictions changed during test and whether convictions at any time related to source guessing, we assessed GPREDs immediately before as well as during and after test in Experiment 3.To determine whether source guessing was causally related to metamemory, we manipulated convictions (*expectancy conviction* versus *inconsistency conviction*) and tested for resulting differences in source guessing in Experiment 4.

Experiment 1

The main objective of Experiment 1 was to determine how schemas influence item-wise JOLs and JOSs in a source-monitoring task. A group of participants provided JOLs and JOSs for each source–item pair during study (*JOL & JOS group*). Memory is often altered by item-wise memory predictions (Besken, 2016; Besken & Mulligan, 2013, 2014; Soderstrom, Clark, Halamish, & Bjork, 2015; Susser, Mulligan, & Besken, 2013). Therefore, as a control, we included a group that did not provide any metamemory judgments during study (*post–only group*). In both groups, we assessed GPOSTs, which allowed us to capture changes in metamemory with test experience.

If compensatory guessing was used as a metacognitive control strategy, an inconsistency conviction in JOSs and source-memory GPOSTs should be related to the schema-consistent guessing bias. In addition to assessing metacognitive convictions during study and after test, we therefore investigated how these convictions, specifically source-memory convictions, related to source guessing. In addition or alternatively, source guessing may be (partially) based on a biased contingency representation (as postulated by the probability-matching account, see Arnold et al., 2013). To test this, we obtained contingency judgments after the source-monitoring test and related them to source guessing.

Method

Participants. The research ethics committee of the Faculty of Mathematical and Natural Sciences of the Heinrich-Heine-Universität Düsseldorf declared the experiments exempt from ethics review. We recruited 72 native speakers of German (20 male) at Heinrich-Heine-Universität Düsseldorf. Age ranged between 17 and 32 years ($M = 21.44$, $SE = 0.37$). Participants were randomly and evenly assigned to the two judgment groups (36 each). A power analysis conducted with G*Power 3 (Faul, Erdfelder, Lang, & Buchner, 2007) yielded that 34 subjects per group were needed to detect a medium within-subjects effect (i.e., $d_z = 0.5$) of expectancy on the metamemory judgments with a power of .80 given $\alpha = .05$ and a two-tailed test. In order to fully counterbalance the material (see below), 36 participants per group were tested. The number of participants and items was comparable to that in Küppers and Bayen (2014) and thus should be sufficient for replicating the schema influences on MPT parameters. Participants received course credit or monetary compensation.

Design and Material. We used a 2 × 2 design with the between-subjects factor judgment group (post–only vs. JOL & JOS) and the within-subject factor source–item expectancy (expected vs. unexpected). All materials were in German. We first conducted an online norming study analogous to the ones conducted by Bayen et al. (2000) and Küppers and Bayen (2014) to ensure that items were highly expected for one room and highly unexpected for the other room. We expanded the item pool used by Küppers and Bayen (2014) by generating additional items resulting in 1004 German labels of objects that can be found in either a bathroom or a kitchen. This item pool was split into two random halves. Two-hundred and one participants (49 male; age ranging from 18 to 35 years, $M = 22.50$, $SE = 0.25$) each rated one of the two item lists regarding their expectation of occurrence in a bathroom or a kitchen on a 5-point Likert scale from 1 (*very unexpected*) to 5 (*very expected*). Assignment of the two lists to bathroom versus kitchen ratings was approximately counterbalanced across participants. Assignment of participants to the counterbalanced conditions was random. For the experiments, we selected 96 items that were highly expected for one room (mean expectancy rating of at least 4.00) and highly unexpected for the other room (mean expectancy rating of at most 1.30; cf. Küppers & Bayen, 2014). Bathroom-expected items had a mean expectancy rating of 4.49 ($SE = 0.04$) for a bathroom, and kitchen-expected items had a mean expectancy rating of 4.49 ($SE = 0.04$) for a kitchen. Bathroom-expected items had a mean expectancy rating of 1.11 ($SE = 0.01$) for a kitchen, and kitchen-expected items had a mean expectancy rating of 1.10 ($SE = 0.01$) for a bathroom. There was no difference between bathroom-expected and kitchen-expected items regarding expectancy ratings, number of syllables, nor word frequency (according to norms from the University of Leipzig, 1998).

We split the item pool into three lists containing 16 kitchen and 16 bathroom items each such that mean expectancy ratings, number of syllables, and word frequency did not differ between lists. Each participant studied two of the lists. Items from one of the study lists were presented with the bathroom source, and items from the other study list were presented with the kitchen source. The third list served as the distractor list during test. Across participants, all lists were presented equally often with the kitchen or bathroom source or served as distractors. As a result of this counterbalancing, each participant studied 32 items with their expected source (bathroom items with the bathroom, or kitchen items with the kitchen), and 32 items with their unexpected source (bathroom items with the kitchen, or kitchen items with the bathroom).

Procedure. In each session, we tested up to five participants in individual computer booths. After consent, they received computerized instructions for the upcoming study phase and were informed of the subsequent source-monitoring test. Specifically, they were informed that they would later be asked to remember for each item whether or not it had been presented during study and, if so, with which source. Participants in the JOL & JOS group were also instructed to provide, after each trial, judgments about their perceived likelihood of later remembering the item (JOL) and its source (JOS). Participants were explicitly instructed to make their JOS independent of their JOL. Specifically, they were told to predict their likelihood of remembering the item's source assuming perfect memory for the item.

At study, two of the three item lists were presented. Items were printed above their respective room source (either "in the BATHROOM" or "in the KITCHEN") and were presented one at a time for 4 s each in an order randomized by participant with the restriction of no more than four consecutive trials with the same source. Items and sources were written in white letters on black background. Items were printed with standard German capitalization. Room labels were printed in all capital letters. Four items that were equally expected for a bathroom and a kitchen were presented as primacy buffers, half of them with each source.

Immediately after each source–item presentation, participants in the JOL & JOS group first rated their perceived likelihood of remembering this item (JOL) and then its source (JOS) in the upcoming test on a scale from 0% (*definitely will not remember*) to 100% (*definitely will remember*) using the number keys on the computer keyboard. These judgments were self-paced. Items and sources were not visible during the judgment. To make the two judgments more discriminable, the JOL and JOS judgment screens had a different background color (randomized blue or yellow). After each judgment, there was a blank black screen for 100 ms. Participants in the post–only group did not provide these judgments. In both groups, there was a blank black screen for 500 ms between trials.

Participants were then given instructions for the upcoming source-monitoring test. In a practice test, four equally expected items were presented, two of which had been presented as buffer items prior to the study list (one with each of the sources) and two new ones. Participants then had the opportunity to ask questions before proceeding to the test phase in which all 96 items (both study lists and the respective distractor list) were presented in a new random order in white letters on black background. For each item, participants were instructed to indicate whether it had been presented with the bathroom, or with the kitchen, or was new. Two light gray response boxes were presented beneath each test item, labeled (in black font) “in the BATHROOM” and “in the KITCHEN.” Assignment of response options to the left and right box was counterbalanced across participants. A third gray box was presented in the center beneath the other two and labeled “was not presented.” Participants answered by clicking on the corresponding box with the computer mouse.

Immediately after test, participants of both groups were asked to provide GPOSTs by judging their item memory and their source memory separately for expected versus unexpected pairings on a scale from 0% (*did not remember anything*) to 100% (*remembered everything*). Each question started with one of the statements “some objects were expected for the room in which they were found”/”some objects were unexpected for the room in which they were found.” Participants were asked four questions in total, the first two referring to their perceived item memory (“What percentage of these objects did you correctly remember in the memory test?”), and the other two referring to their perceived source memory (“For what percentage of objects did you correctly remember the room in which they were located?”). The order of the questions regarding expected trials versus unexpected trials was counterbalanced within memory type.

Then, participants provided two source–item contingency judgments. Specifically, they were asked to estimate how many of the 32 bathroom-expected items had been presented with one source at study and how many of the 32 kitchen-expected items had been presented with the respective other source at study. Whether participants made their judgment for the expected or the unexpected source was counterbalanced, as was the order of the two questions. Upon response, the participant‘s numerical answer was presented on the screen (e.g. “17 bathroom-expected items in the bathroom”) along with the remaining item count for the respective other source (e.g. “15 bathroom-expected items in the kitchen”). Participants were asked to verify their answer.

Finally, participants provided demographic information and were asked about their belief concerning the study aim. They were then debriefed and compensated.

Analyses and Results

Analyses: Multinomial process tree modeling. In source-monitoring tasks, several cognitive processes contribute to participants’ responses, namely item memory, source memory, and guessing (Batchelder & Riefer, 1990; Bayen et al., 1996; Bröder & Meiser, 2007; Murnane

& Bayen, 1996). In order to obtain unconfounded results for memory and guessing, we thus need measures that allow us to disentangle these processes. Murnane and Bayen (1996) have shown that all empirical measures of source-identification performance confound source memory and source guessing, and many of them further confound item memory and source memory. The solution to this problem are MPT models of source monitoring, which provide separate and independent measures of item memory, source memory, old/new guessing, and source guessing (Batchelder & Riefer, 1990; Bayen et al., 1996; Bröder & Meiser, 2007). Crucially, our effect of interest (i.e., the inconsistency effect) on source memory can only be investigated with a pure measure of source memory (i.e., not confounded by guessing bias; e.g., Bell et al., 2012; Ehrenberg & Klauer, 2005; Küppers & Bayen, 2014).

We therefore used the two-high-threshold MPT model of source monitoring (2HTSM, Bayen et al., 1996) to analyze the data from the source-monitoring task. This model has been empirically validated by Bayen et al. in 1996, and has since been applied in numerous source-monitoring studies (e.g., Bayen & Kuhlmann, 2011; Bayen & Murnane, 1996; Bayen et al., 2000; Bell et al., 2012; Ehrenberg & Klauer, 2005; Kuhlmann & Touron, 2011). For general overviews of MPT models, see Batchelder and Riefer (1999) or Erdfelder et al. (2009).

Figure 1 illustrates the processing tree structure of the MPT model of source monitoring. The three trees in the model correspond to items presented with the expected source, items presented with the unexpected source, and new items, respectively. For each item, participants have three response options: “expected source”, “unexpected source”, or “new.”

The first tree in Figure 1 represents the processing tree for items that had been presented with the expected source at study. Participants recognize the item as old with probability D_{expected} (item memory). Also, they may remember that the item had been presented with the expected source, with probability d_{expected} (source memory). This will result in the correct answer that the item had been presented with the expected source. Alternatively, participants may not remember the source of a recognized item, with probability $1 - d_{\text{expected}}$. They must then guess a source. With probability g[1], they guess that the item had been presented with the expected source and will thus answer correctly. With probability $1 - g$, they guess that the item had been presented with the unexpected source and will thus incorrectly answer that the item had been presented with the unexpected source. Participants may also not recognize the item, with probability $1 - D_{\text{expected}}$. In this case, they may guess that the item is old, with probability b, and then either guess that it had been presented with the expected source (with probability g, which will result in a correct answer) or with the unexpected source (with probability $1 - g$, which will result in an incorrect answer). Participants may also guess that an item is new, with probability $1 - b$, and thus falsely reject the item as new.

The second and third trees of the model follow the same logic. The second tree refers to items that had been presented with the unexpected source at study. The probability of recognizing such items is $D_{\text{unexpected}}$ and the probability of remembering their source is probability $d_{\text{unexpected}}$. The third tree refers to items that had not been presented during study. D_{new} is the probability of knowing that such a distractor item is new.

We obtained response frequencies from the source-monitoring task for each participant. From these frequencies, we estimated parameters for each participant for item memory (D), source memory (d), old/new guessing (b), and source guessing (g). We used the Bayesian-hierarchical latent trait approach for parameter estimation (Klauer, 2010). In hierarchical MPT modeling, individual parameters are drawn from an overarching group distribution (Klauer, 2010; Smith & Batchelder, 2010). In particular, the latent trait approach draws participant parameters from a multivariate normal distribution of probit transformed parameters. Via Bayesian modeling an a-priori distribution is updated to a posterior distribution for each parameter given the data (Bayes' theorem). The Markov chain Monte Carlo (MCMC) algorithm draws a large number of samples from the posterior parameter distribution. Thus, individual parameter estimates and corresponding parameter distributions are obtained. The parameter distribution is characterized by the Bayesian Credibility Interval (BCI) which indicates the interval in which the true parameter can be found with 95% confidence.

Due to limited degrees of freedom an equality restriction must be placed on parameter D_{new} to obtain a mathematically identifiable base model for parameter tests (Bayen et al., 1996). We set $D_{new} = D_{unexpected}$. Note that if instead we set $D_{new} = D_{expected}$, all reported estimates were within the 95% BCI of one another (largest parameter difference = .063). We will note the few cases where these models led to different conclusions.

We used the *R* package *TreeBUGS* (Heck, Arnold, & Arnold, 2017) for Bayesian-hierarchical parameter estimation in all experiments. *TreeBUGS* uses the MCMC algorithm implemented in JAGS (Plummer, 2003). If not otherwise stated, we used 1,000,000 samples with a burn-in period of 500,000 samples. We retained every 100th sample. Parameter convergence was assessed using the potential scale reduction factor $\hat{R}$. Good convergence is indicated by $\hat{R} < 1.05$. We assessed model fit with the test statistics T_1 and T_2 (Klauer, 2010). T_1 computes the distance between observed and expected mean frequencies using Pearson's χ^2. T_2 computes the summed differences between observed and expected covariances, standardized by the expected standard deviations (see also Heck et al., 2017). Good model fit is indicated by non-significant test results.

Analyses: Hypothesis testing. To test hypotheses regarding differences in parameters between conditions, we sampled parameter differences. A statistically reliable difference is indicated if the 95% BCI for the difference estimate does not contain zero. We report the BCI in brackets.

To test whether source guessing was based on a metacognitive compensatory strategy or a contingency-based strategy, we performed regression analyses within the hierarchical models described above. In these analyses, we entered metacognitive source-memory convictions and contingency ratings as predictors of source guessing (*g*). To obtain individual measures of metacognitive convictions, we subtracted the mean source-memory judgment (JOS or source-memory GPOST) for unexpected source–item pairs from the respective mean source-memory judgment for expected pairs for each participant. We will refer to these difference measures as "conviction." Positive values indicate an expectancy conviction, whereas negative values indicate an inconsistency conviction. To test whether JOS conviction related to source guessing, we entered JOS conviction as predictor of source guessing (*g*) in a model for the JOL & JOS group. Because GPOST conviction and contingency judgments were available for both participant groups, we regressed source guessing onto these variables in a model that included both groups (T_1: $p = .537$, T_2: $p = .487$). A statistically reliable prediction is indicated if the 95% BCI for the regression weight does not contain zero. We will report the regression results below, after reporting the effects of the experimental manipulations on item-wise judgments, GPOST and contingency judgments, respectively.

Results: Source-monitoring processes. Parameter convergence was good as indicated by $\hat{R} < 1.05$ for all parameters. Model fit was good for both the post–only group, T_1: $p = .532$, T_2:

p =.541, and the JOL & JOS group, T_1: p = .483, T_2: p =.462. Table 1 shows the parameter estimates and their 95% BCIs.

Item memory. We computed the differences in the item-memory parameters between unexpected and expected source–item pairs ($\Delta D = D_{\text{unexpected}} - D_{\text{expected}}$). There was no effect of source–item expectancy on item memory in the post–only group, ΔD = –.02, [–.11, .07], nor in the JOL & JOS group, ΔD = –.001, [–.07, .07].

Source memory. For the results regarding source memory, refer to Figure 2. We computed the differences in the source-memory parameters between unexpected and expected source–item pairs ($\Delta d = d_{\text{unexpected}} - d_{\text{expected}}$). In the post–only group, source memory was better for the unexpected than the expected source, Δd = .57, [.16, .83], that is, this group showed an inconsistency effect. In the JOL & JOS group, there was no such effect, Δd = .10, [–.35, .53].

Source guessing. The probabilities to guess the expected source (guessing parameter g) are in Table 1. We computed the differences between the source-guessing parameters and the chance probability of .5 ($\Delta g_{.50} = g - .5$). Additionally, we computed the difference in g between the groups. The probability to guess the expected source was higher than chance in both the post–only group, $\Delta g_{.50}$ = .26, [.15, .34], and the JOL & JOS group, $\Delta g_{.50}$ = .16, [.02, .27]. That is, participants guessed consistent with the schemas in both groups. The groups did not differ in their guessing bias, $\Delta g_{\text{post–only – JOL \& JOS}}$ = .10, [–.06, .26].

Results: Metamemory.

JOLs & JOSs. For each participant in the JOL & JOS group, we averaged JOLs and JOSs separately for expected versus unexpected source–item pairings. Descriptive statistics are shown in Figure 3. We calculated a 2 × 2 within-subjects MANOVA with the factors source–item expectancy (expected vs. unexpected) and judgment content (JOL vs. JOS). Expected source–item pairings were rated as more memorable than unexpected pairings, Pillai's Trace = .64, $F(1, 35) = 60.90$, $p < .001$, $\eta_p^2 = .64$. There was also a main effect of judgment content, Pillai's Trace = .24, $F(1, 35) = 11.07$, $p = .002$, $\eta_p^2 = .24$, which was qualified by a two-way interaction, Pillai's Trace = .49, $F(1, 35) = 33.72$, $p < .001$, $\eta_p^2 = .49$. Paired *t* tests revealed that the perceived difference in memorability between expected and unexpected pairings was more pronounced in JOSs, $t(35) = 7.93$, $p < .001$, $d_z = 1.32$, than in JOLs, $t(35) = 5.08$, $p < .001$, $d_z = 0.85$. Correlations between JOLs and JOSs for expected and unexpected source–item pairs can be obtained from Table A1 in the Appendix. JOLs and JOSs were positively correlated for both expected and unexpected pairs.

Next, we determined the relationship between JOS conviction and source guessing. The regression weight for JOS conviction and its 95% BCI are shown in Table 3. As evident, JOS conviction did not predict source guessing. Thus, source guessing did not depend on the metamemory conviction about source memory during study, suggesting that source guessing was not driven by a compensatory strategy.

GPOSTs. We calculated a 2 × 2 × 2 mixed MANOVA with the between-subjects factor judgment group (post–only vs. JOL & JOS) and the within-subject factors source–item expectancy and judgment content (item memory vs. source memory). Descriptive statistics are shown in Table 2. The results were qualified by a three-way interaction, Pillai's Trace = .07, $F(1, 70) = 5.60$, $p = .021$, $\eta_p^2 = .07$. We therefore analyzed both groups in separate 2 (source–item expectancy) × 2 (judgment content) MANOVAs.

For the post–only group, (item and source) memory was perceived to have been better for unexpected pairings, Pillai's Trace = .19, $F(1, 35) = 7.95$, $p = .008$, $\eta_p^2 = .19$. There was no main effect of judgment content, Pillai's Trace = .02, $F(1, 35) = 0.84$, $p = .367$, $\eta_p^2 = .02$, but a significant two-way interaction, Pillai's Trace = .14, $F(1, 35) = 5.47$, $p = .025$, $\eta_p^2 = .14$. Paired *t* tests revealed that the perceived memory advantage for unexpected pairings was not significant for item-memory judgments, $t(35) = 0.64$, $p = .525$, $d_z = 0.11$, but for source-memory judgments, $t(35) = 3.80$, $p = .001$, $d_z = 0.63$.

For the JOL & JOS group, (item and source) memory was perceived to have been better for expected pairings, Pillai's Trace = .16, $F(1, 35) = 6.52$, $p = .015$, $\eta_p^2 = .16$. Additionally, item memory was perceived to have been better than source memory, Pillai's Trace = .23, $F(1, 35) = 10.43$, $p = .003$, $\eta_p^2 = .23$. There was no interaction, Pillai's Trace = .02, $F(1,35) = 0.58$, $p = .452$, $\eta_p^2 = .02$. For both groups, item-memory and source-memory GPOSTs were positively correlated (see Table A1 in the Appendix).

Next, we determined the relationship between source-memory GPOST conviction and source guessing. Source-memory GPOST conviction did not predict source guessing (see Table 3). Thus, source guessing did not relate to the metamemory conviction about source memory expressed after test, suggesting, again, that source guessing was not driven by a compensatory strategy.

Results: Contingency judgments. We relativized the absolute contingency judgments. We used a one-sample *t* test to compare the relative contingency judgments for expected source–item pairings against the true zero contingency of .5. Also, we tested whether the relative contingency judgments for expected pairings differed from the respective individual source-guessing parameter (via 95% BCIs of the averaged difference estimates *g* – rel. contingency). Contingency judgments and tests against the true contingency of .5 and against source-guessing bias are in Table 4. Participants believed that more items had been presented with the expected source than the unexpected source. Additionally, source guessing was even more biased towards the expected source than the contingency judgment was, as indicated by the 95% BCI for the difference estimate.

Next, we determined the relationship between contingency judgments and source guessing. Arnold et al. (2013) showed that contingency judgments predicted schema-consistent source guessing, suggesting that this guessing bias is at least partially due to a biased contingency representation (as postulated by the probability-matching account, Spaniol & Bayen, 2002). We aimed to replicate this finding Regression weights and 95% BCIs are in Table 4. As expected, contingency judgments related to source guessing. The more contingency judgment were biased towards more expected pairs, the more biased was source guessing towards the expected source.

Discussion

In this experiment, we demonstrated metamemory illusions of expectancy effects on item memory and source memory. Participants predicted better item memory and, especially, source memory for expected source–item pairings, in line with the relatedness effect on metamemory (e.g., Mueller et al., 2013; Undorf & Erdfelder, 2015). In contrast with their predictions, however, participants showed no difference in item memory between expected versus unexpected items. Furthermore, they showed even better source memory for unexpected sources when memory was uncontaminated by the requirement of memory predictions in the post–only group (replicating Küppers & Bayen, 2014). When predictions were made (in the JOL & JOS group), there was no such source-memory difference. This finding is in line with research showing that metamemory judgments can alter memory in other paradigms (Besken, 2016; Besken & Mulligan, 2013, 2014; Soderstrom et al., 2015; Susser et al., 2013) and may extend these findings to source memory. It should be noted however, that the inter-stimulus-intervals and thus the overall length of the retention interval were longer for the JOL & JOS group than for the post–only group; thus, the differences in source memory may alternatively be due to these confounds. We will address this issue in Experiment 2.

There was a single dissociation between predicted and actual source memory in the JOL & JOS group. Assuming that metamemory convictions were the same in the post–only group, there was even a double dissociation in this group. Also, there was a single dissociation between predicted and actual item memory.

A compensatory-guessing strategy (Batchelder & Batchelder, 2008; Ehrenberg & Klauer, 2005; Kuhlmann & Touron, 2011; Küppers & Bayen, 2014; Meiser et al., 2007) would have biased guessing in favor of the believed-to-be less well remembered source. Based on the JOSs provided by the participants in the JOL & JOS group, this would have been the schematically unexpected source. However, we found the opposite: participants guessed in favor of the schematically expected source in both groups. Additionally, the magnitude of neither the JOS conviction nor the source-memory GPOST conviction predicted source guessing. We can, therefore, conclude that the metamemory convictions did not guide source guessing. Thus, we did not find evidence for compensatory guessing in schema-based source monitoring. Schema-consistent guessing thus appears to rather be based on a biased contingency perception or strong schematic expectations (cf. Bayen et al., 2000; Spaniol & Bayen, 2002). Contingency ratings were indeed biased towards more expected pairs, and these ratings did partially predict source guessing. Thus, participants who thought that a larger number of expected source–item pairs had been presented were also more biased in their source guessing (as predicted by the probability-matching account, Arnold et al., 2013; Spaniol & Bayen, 2002). However, source guessing was *even more* biased than this bias in contingency representation, suggesting that a biased contingency representation alone cannot explain schema-consistent guessing. Rather, it appears that people additionally defaulted to their general schematic expectations when guessing the room source.

Experiment 2

In Experiment 2, we sought to replicate the theoretically important dissociation of metamemory judgments and memory, while avoiding possible mutual contaminations between JOLs and JOSs. In Experiment 1, we found a more pronounced expectancy effect on JOSs compared to JOLs, which suggests that people monitored source memory separately from item memory (albeit inaccurately). Yet, since the JOL was always rendered first, it is possible that JOLs influenced JOSs. Thus, we deemed it important to replicate the expectancy effect on JOS (which in turn suggests that the observed schema-consistent guessing is *not* compensatory) independent of whether a JOL was rendered. We therefore included, in addition to a replication of the JOL & JOS group, a JOS-only group in the design of Experiment 2.

As a third group, we included a JOL-only group in order to replicate the JOL-only design chosen by Konopka and Benjamin (2009) who reported an large expectancy effect on JOLs. We asked whether this effect may have been contaminated by participants' strong convictions about an expectancy effect on source memory, as suggested by the stronger expectancy effect on JOSs than JOLs in our Experiment 1. A stronger expectancy effect on JOLs in the JOL-only group compared to the JOL & JOS group would indeed suggest that participants' beliefs about source memory contaminate their JOLs when they are not given the opportunity to provide separate judgments for source memory. The comparisons of single versus dual judgments allowed us to determine to what extent separate metamemory monitoring of item memory versus source memory is possible.

Method

Participants. We recruited 108 new native speakers of German (21 male) at Heinrich-Heine-Universität Düsseldorf. Age ranged between 17 and 34 years (M = 21.93, SE = 0.38). Participants were randomly and evenly assigned to the three judgment groups, with n = 36 per group based on the same power calculations as in Experiment 1. Participants received course credit or monetary compensation.

Design and Material. We used a 3 × 2 design with the between-subjects factor judgment group (JOL & JOS vs. JOL-only vs. JOS-only) and the within-subject factor source–item expectancy. We used the same materials with the same counterbalancing scheme as in Experiment 1.

Procedure. The procedure in the JOL & JOS group was identical to that in Experiment 1. In the JOL-only group, participants were instructed to provide JOLs only. They were explicitly instructed to not judge their likelihood of remembering the respective source, but only their likelihood of remembering each item. In the JOS-only group, participants were instructed to provide JOSs only. They were explicitly instructed to not judge their likelihood of remembering each item, but only their likelihood of remembering the respective source (assuming that they would remember the item). All other procedural details were identical to those in Experiment 1.

Results

Source-monitoring processes. We disentangled memory and guessing via the same hierarchical MPT model as in Experiment 1. Parameter estimates and 95% BCIs are in Table 1. Parameter convergence was good as indicated by $\hat{R}$ < 1.05 for all parameters. Model fit was also good for the JOL & JOS group, T_1: p = .524, T_2: p = .490, the JOL-only group, T_1: p = .460, T_2: p = .351, and the JOS-only group, T_1: p = .500, T_2: p = .529.

As in Experiment 1, we tested whether source guessing was based on a compensatory strategy or a contingency-based strategy using regression analyses. We entered JOS conviction as predictor of source guessing (*g*) in the models for the JOL & JOS group and the JOS-only group. For the JOL-only group, we entered the respective JOL conviction instead (because this group did not provide any JOSs, but instead JOLs that may have been influenced by source-memory convictions). As in Experiment 1, we regressed source guessing on GPOST conviction and contingency judgments in a model that included both groups (T_1: $p = .445$, T_2: $p = .448$). We will report the regression results below, after reporting the effects of the experimental manipulations on item-wise judgments, GPOSTs and contingency judgments, respectively.

Item memory. In all three judgment groups, there was no effect of source–item expectancy on item memory in the JOL & JOS group, $\Delta D = –.01$, [–.08, .07], in the JOL-only group, $\Delta D = –.04$, [–.11, .03], nor in the JOS-only group, $\Delta D = –.07$, [–.14, .01]. Note that if the alternative model (with restriction $D_{\text{expected}} = D_{\text{new}}$) was used, there was an expectancy effect on item memory in the JOS-only group ($\Delta D = –.09$, [–.17, –.01]). However, this effect did not replicate in any group of this or the other experiments, and thus, we refrain from interpretation.

Source memory. For the results regarding source memory refer to Figure 2. There was an inconsistency effect in the JOL-only group, $\Delta d = .48$, [.12, .74]. There was no such effect in the JOL & JOS group (replicating Experiment 1), $\Delta d = .16$, [–.25, .59], nor in the JOS-only group, $\Delta d = .14$, [–.17, .49].

Source guessing. The probabilities to guess the expected source (guessing parameter *g*) are in Table 1. This probability was higher than the chance probability of .5 in the JOL-only group, $\Delta g_{.50} = .22$, [.11, .30], and the JOS-only group, $\Delta g_{.50} = .16$, [.04, .26]. In the JOL & JOS group, this bias was only present numerically, $\Delta g_{.50} = .14$, [–.02, .27]. Notably, however, there was no difference in guessing bias between the three groups ($\Delta g_{\text{JOL-only – JOL \& JOS}}$ = .08, [–.09, .26]; $\Delta g_{\text{JOS-only – JOL \& JOS}}$ = .02, [–.16, .21]; $\Delta g_{\text{JOL-only – JOS-only}}$ = .06, [–.09, .21]).

Metamemory.

JOLs & JOSs. We again calculated mean JOLs and JOSs separately for expected and unexpected source–item pairings. Descriptive statistics are shown in Figure 3. As in Experiment 1, we first calculated a 2 (source–item expectancy) × 2 (judgment content) within-subjects MANOVA for the JOL & JOS group only. Expected information was again rated as more memorable than unexpected information, Pillai's Trace = .59, $F(1, 35) = 50.69$, $p < .001$, $\eta_p^2 = .59$. There was no main effect of judgment content, Pillai's Trace = .10, $F(1, 35) = 3.91$, $p = .056$, $\eta_p^2 = .10$, but a two-way interaction, Pillai's Trace = .58, $F(1, 35) = 48.26$, $p < .001$, $\eta_p^2 = .58$. Paired *t* tests revealed that again the perceived difference in memorability between expected and unexpected source–item pairings was more pronounced in JOSs, $t(35) = 8.08$, $p < .001$, $d_z = 1.35$, than in JOLs, $t(35) = 3.90$, $p < .001$, $d_z = 0.65$. JOLs and JOSs were positively correlated for both expected and unexpected source–item pairs (see Table A1 in the Appendix). Thus, we replicated the metamemory expectancy effect from Experiment 1.

To compare the expectancy effect on the JOSs between the JOL & JOS group and the JOS-only group, we performed a 2 × 2 mixed ANOVA with the within-subjects factor source–item expectancy and the between-subjects factor judgment group (JOL & JOS vs. JOS-only). Expected sources were rated as more memorable than unexpected sources, $F(1, 70) = 144.52$, $p < .001$, $\eta_p^2 = .67$. There was no significant effect of the judgment group, $F(1, 70) = 3.84$, $p = .054$, $\eta_p^2 = .05$, and no interaction, $F(1, 70) = 2.10$, $p = .152$, $\eta_p^2 = .03$. Thus, the expectancy effect on JOSs replicated when no JOLs were rendered beforehand.

To compare the expectancy effect on the JOLs between the JOL & JOS group and the JOL-only group, we performed a 2 × 2 mixed ANOVA with the within-subjects factor source–item expectancy and the between-subjects factor judgment group (JOL & JOS vs. JOL-only). Items presented with the expected source were rated as more memorable than items presented with the unexpected source, $F(1, 70) = 40.67$, $p < .001$, $\eta_p^2 = .37$. There was no significant effect of the judgment group, $F(1, 70) = 3.32$, $p = .073$, $\eta_p^2 = .05$, but a two-way interaction, $F(1, 70) = 7.76$, $p = .007$, $\eta_p^2 = .10$. Paired *t* tests revealed that the perceived difference in memorability between expected and unexpected items was more pronounced in the JOL-only group, $t(35) = 5.16$, $p < .001$, $d_z = 0.86$, than in the JOL & JOS group ($d_z = 0.65$, see above). Thus, when participants were not given the opportunity to separately provide both JOLs and JOSs, the expectancy effect on the JOLs was larger than when they provided both judgments.

Next, we determined the relationship between JOL and JOS conviction and source guessing. Results are in Table 3. In the JOL & JOS group, JOS conviction did not predict source guessing (replicating Experiment 1). In the JOL-only and JOS-only groups, JOL or JOS conviction, respectively, predicted source guessing with a positive regression weight. This finding, however, is not compatible with a compensatory strategy, because it indicates that those who believed more strongly in an expectancy effect show *more* schema-consistent guessing, whereas a metacognitive compensation account would predict less of it.

GPOSTs. We calculated a 3 × 2 × 2 mixed MANOVA with the between-subjects factor judgment group (JOL & JOS vs. JOL-only vs. JOS-only) and the within-subjects factors source–item expectancy and judgment content. Descriptive statistics are shown in Table 2. Items were rated to have been better remembered than sources, Pillai's Trace = .12, $F(1, 105) = 14.27$, $p < .001$, $\eta_p^2 = .12$. All other effects were not significant, all $F < 1.76$, $p \geq .177$. For all groups, item-memory and source-memory GPOSTs were positively correlated (see Table A1 in the Appendix).

Next, we determined the relationship between source-memory GPOST conviction and source guessing. GPOST conviction related to source guessing with a positive regression weight (see Table 3). That is, the more participants showed an expectancy effect on source memory (after test), the more they guessed schema-consistently in this group. This finding is again not compatible with a compensatory strategy, because it indicates that those who believed more strongly in an expectancy effect show *more* schema-consistent guessing, whereas a metacognitive compensation account would predict less of it.

Contingency judgments. Contingency judgments and tests against the true contingency of .5 and against source-guessing bias are presented in Table 4. Participants believed that more items had been presented with the expected source. Additionally, source guessing was more biased towards the expected source than the contingency judgment was, replicating Experiment 1.

Next, we determined the relationship between contingency judgments and source guessing. Contingency judgments related to source guessing (see Table 4). The more participants were biased in their contingency judgment towards more expected pairs, the more biased was their source guessing towards the expected source (replicating Experiment 1 and Arnold et al., 2013).

Discussion

The JOL & JOS group in Experiment 2 replicated the metamemory expectancy effects in the corresponding group of Experiment 1. That is, participants predicted item memory and source memory to be better for expected source–item pairings, with a more pronounced effect on

JOSs than on JOLs. As predicted, the expectancy effect on JOLs was stronger when participants did not provide an additional JOS. This suggests that participants confounded item memory and source memory if not prompted to predict both, but were able to differentiate if given the opportunity. Such contamination only occurred from JOSs to JOLs, but not from JOLs to JOSs. The expectancy effect on JOSs replicated independent of whether JOLs were provided alongside or not.

Importantly, this replication of the expectancy effect on metamemory bolsters the interpretation that schema-consistent source guessing does not reflect strategic compensation. Source guessing was biased towards the expected source (albeit only numerically in the JOL & JOS group). However, JOS conviction did not predict the source guessing bias in the JOL & JOS group. In the JOL-only and JOS-only groups, JOL conviction and JOS conviction predicted source guessing. Additionally, source-memory GPOST conviction predicted source guessing. Unexpectedly, these effects indicated that participants who more strongly believed in an expectancy effect on source (and item) memory also showed a stronger schema-consistent bias. Crucially, this unexpected pattern, which did not replicate reliably in our other experiments, is not at all compatible with the compensatory-guessing account (which would predict the opposite).

A biased contingency representation predicted source guessing. Thus, the schema-consistent guessing bias may in part have been contingency-based, as in Experiment 1. However, mere schema reliance likely also played a role, because source guessing was biased more strongly than the contingency representation.

Source memory was again influenced by the provision of metamemory judgments. This expands the findings of Soderstrom et al. (2015) who reported that JOLs modified cued-recall performance. When our participants provided JOSs (in the JOL & JOS and the JOS-only group), the inconsistency effect on source memory was not statistically reliable. However, when only JOLs were provided, there was an inconsistency effect on source memory. This suggests that the effect of providing metamemory judgments on information encoding depends on the judgment content (item versus source). In contrast to Experiment 1, we can, in Experiment 2, attribute the differences in source memory to the requirement of providing JOSs specifically. In particular, both the JOL-only group and the JOS-only group provided one judgment between source–item pairs, and the length of the inter-stimulus-intervals and overall retention intervals were thus comparable. Therefore, the different findings regarding source memory (inconsistency effect vs. no inconsistency effect) were likely due to the different judgments provided.

Regardless of these reactive effects of judgments on memory, there was again a single dissociation between predicted and actual item memory (in all groups) and source memory (in the JOS & JOL and in the JOS-only group). Assuming that metacognitions about source memory were the same in the JOL-only group as in the other groups, there was even a double dissociation between predicted and actual source memory.

Experiment 3

Experiments 1 and 2 provided evidence that metamemory judgments do not reflect the inconsistency effect on source memory. A compensatory-guessing hypothesis predicts a negative relationship between source-memory conviction (expected – unexpected) and source guessing. A conviction of an inconsistency effect on source memory should coincide with a larger schema-consistent source-guessing bias. Neither in Experiment 1 nor in Experiment 2 did we find such a relationship for convictions measured during study or after test. Based on these findings, schema-consistent source guessing does not seem to reflect a metacognitive strategy based on awareness of the inconsistency effect.

In both Experiments 1 and 2, we obtained metamemory judgments during the study phase and after test. Guessing, however, occurs *during* test, and therefore, in order to conclusively decide whether or not participants used guessing as a compensatory strategy, we must consider their metamemory convictions at the time of test. Although there was only a short time between study and test in the first two experiments, one might object that JOLs and JOSs obtained during study are only a proxy for convictions at the time of test. In the following experiments, we therefore assessed GPREDs immediately prior to and during test. After all, it is possible that people become aware of the inconsistency effect on source memory when experiencing their actual memory during test and update their source guessing accordingly (see Dunlosky & Hertzog, 2000; Hertzog, Price, & Dunlosky, 2008; Koriat & Bjork, 2006). In Experiment 1, participants who were not required to provide any judgments during study were able to postdict the inconsistency effect on source memory after test. Thus, based on this knowledge, participants in the post–only group of Experiment 1 may have started using compensatory guessing *in addition* to schema-based guessing at some point during test. In Experiment 2, none of the groups postdicted an inconsistency effect, not even the JOL-only group (which actually showed one). Nonetheless, it seems that these groups somewhat updated their knowledge about their memory, because they did not postdict expectancy effects on item memory nor source memory.

In Experiment 3, we tested whether metamemory convictions change during the test phase and whether compensatory source guessing is added as a secondary strategy after this change. We used three groups and asked participants for metamemory judgments prior to, in the middle of, and after the test phase. In the pre–post group, participants provided a GPRED before and a GPOST after the test. In the mid–post group, participants provided a GPRED in the middle of the test and a GPOST after the test. In the post–only group, participants provided a GPOST after the test only. If compensatory guessing is added as a secondary strategy we should observe the following. First, metacognitive convictions should change during test. That is, participants should gain awareness of the inconsistency effect due to their experience with the test and their own memory. Second, metamemory convictions measured at different times during the test should relate to source guessing, especially after participants gain awareness of the inconsistency effect. The more participants show an inconsistency effect on metamemory, the more they should guess schema-consistently.

Method

Participants. We recruited 144 new native speakers of German (37 male) at Heinrich-Heine-Universität Düsseldorf. Age ranged between 17 and 34 years (M = 22.14, SE = 0.27). Participants were randomly and evenly assigned to the three judgment groups (48 each). We increased the number of participants per group in comparison to Experiments 1 and 2, because we wanted to compare the first and second test halves. Participants received course credit or monetary compensation.

Design and Material. The design was 3 × 2 with the between-subjects factors judgment group (pre–post vs. mid–post vs. post–only) and the within-subjects factor source–item expectancy. We used the same materials as in the other experiments. However, to construct equivalent test halves, we created new study lists and counterbalancing schemes. We split the original item pool into six lists, each containing eight bathroom-expected items and eight kitchen-expected items. Mean expectancy ratings, number of syllables, and word frequency did not differ between lists. Then, we created three sets of two lists each. During study, each participant was presented with two sets, that is, four lists in total. Items from one of the list sets were presented with the bathroom source and items from the other list set were presented with the kitchen source. Items from the third list set were presented as distractors during test. Across participants and groups, all list sets were presented equally often with the kitchen or bathroom source or served as distractors. Due to each list containing eight bathroom and eight kitchen items each, participants again studied equal numbers of expected and unexpected source–item pairings. Thirty-two items were presented with their expected source, and 32 items were presented with their unexpected source. All source–item pairs were presented in an order randomized by participant with the restriction of no more than four consecutive presentations with the same source. During the source-monitoring test, one of the two lists of each set was tested in the first test half and the other one in the second test half. For the test halves, the same three lists (one from each list set) were always grouped together. The order of the presentation of the test halves was counterbalanced across participants within groups. Within test halves, items were tested in an order randomized by participant.

Procedure. The procedure was the same as in the post–only group of Experiment 1 with the following exceptions. After study, participants in the pre-test group were asked to provide item-memory and source-memory GPREDs about their item memory and source memory in the upcoming source-monitoring test separately for expected versus unexpected source–item pairings on a scale from 0% (*will not remember anything*) to 100% *(will remember everything*). Before the GPREDs, they were informed that some objects had been expected and some objects had been unexpected in the room in which they were located. Then, they were asked four questions of which the first two referred to item memory, and the other two referred to source memory. They were asked what percentage of the expected/unexpected objects they would correctly remember in the memory test (item-memory judgment), then for what percentage of the correctly remembered expected/unexpected objects they would correctly remember in which room they had been located (source-memory judgment). They were instructed to make their source-memory prediction independent of their item-memory judgment. The order of the questions regarding expected versus unexpected pairs was counterbalanced within memory type. Participants typed in their answer using number keys on the computer keyboard. In the mid-test and post–only groups, participants did not provide any predictions at this point. All participants then viewed the four practice trials and then proceeded to the first test half.

After the first test half, participants in the mid–post group were asked to provide item-memory and source-memory GPREDs for the second test half. They then proceeded to the second test half. Participants in the pre–post group and the post–only groups did not provide any predictions at this time and proceeded to the second test half without a break. After test, all participants provided GPOSTs and contingency judgments as in the previous experiments.

Results

Source-monitoring processes. We disentangled memory and guessing via the same hierarchical MPT model as in the previous experiments. Table 1 shows the parameter estimates and their 95% BCIs. Parameter convergence was good as indicated by $\hat{R} < 1.05$ for all parameters. Model fit was also good for the pre–post group, T_1: $p = .470$, T_2: $p = .387$, the mid–post group, T_1: $p = .485$, T_2: $p = .481$, and the post–only group, T_1: $p = .497$, T_2: $p = .477$.

To compare source guessing in the first versus the second test half, we fitted joint models with the test halves as within-subjects condition. For these models, we had to increase the number of drawn samples to 10,000,000 with a burn-in period of 5,000,000 samples. We retained every 500th sample. Parameters and model fit statistics for the test halves are listed in Table 5. Model fit was good for all groups. Parameter convergence was good with all $\hat{R} < 1.05$.

Again, we tested whether source guessing was based on a compensatory strategy or a contingency-based strategy using regression analyses. Analogously to the previous experiments, we calculated pre-test and mid-test source-memory GPRED convictions by subtracting the GPRED for unexpected source–item pairs from the GPRED for expected source–item pairs for the pre–post and mid–post groups, respectively. We entered GPRED conviction measures as predictors of source guessing (*g*) in the group-wise models (pre-test GPRED conviction in the pre–post group, mid-test GPRED conviction in the mid–post group, respectively). As in the previous experiments, we regressed source guessing on GPOST conviction and contingency judgments in a model that included all groups all groups in the same model (T_1: $p = .527$, T_2: $p = .483$).

Additionally, we tested whether convictions related to source guessing in any of the test halves. For the joint models with test halves as within-subject condition, we therefore performed the same regression analyses for source guessing in the first and second test halves. That is we regressed source guessing (*g*) in the first and second test halves on pre-test GPRED conviction (in the pre–post group) and mid-test GPRED conviction (in the mid–post group). In addition, we regressed source guessing in the first and second test halves on GPOST conviction and contingency judgments in a model that included all groups (T_1: $p = .582$, T_2: $p = .417$). We will report the regression results below, after reporting the effects of the experimental manipulations on GPREDs, GPOSTs and contingency judgments, respectively.

Item memory. In the mid–post group, there was an inconsistency effect on item memory, ΔD = .08, [.01, .16]. There was no effect of schemas in the pre–post group, ΔD = .01, [–.06, .08], and post–only group ΔD = .04, [–.03, .10].

Source memory. For the results regarding source memory refer to Figure 2. In the post–only group, there was an inconsistency effect on source memory, Δd = .53, [.16, .85], replicating the post–only group from Experiment 1. There was no such effect for the pre–post group, Δd = .21, [–.14, .58], and the mid–post group, Δd = .24, [–.16, .65].

Source guessing. Table 1 shows the probability to guess the expected source (guessing parameter *g*). This probability was higher than the chance probability of .5 in the pre–post group, Δg = .12, [.02, .22], the mid–post group, Δg = .14, [.03, .24], and the post–only group, Δg = .24, [.14, .33]. Notably, there was no difference in guessing bias between the three groups ($\Delta g_{\text{mid–post - pre–post}}$ = .02, [–.12, .16]; $\Delta g_{\text{post–only - pre–post}}$ = .12, [–.02, .26]; $\Delta g_{\text{post–only - mid–post}}$ = .10, [–.04, .24]). Comparisons of g in the first versus second test half are in Table 5. Importantly, the size of the source-guessing bias did not differ between the first and second test half in any of the groups, suggesting no changes in guessing strategy across test.

Metamemory. We first analyzed metamemory development over the course of the test phase. Descriptive statistics are in Figure 4. In all groups, all item-memory and respective source-memory judgments were positively correlated (see Table A1 in the Appendix). We analyzed the first memory judgment each group provided with a 3 × 2 × 2 mixed factorial MANOVA with the between-subjects factor judgment group (pre–post vs. mid–post vs. post–only) and the within-subjects factors source–item expectancy and judgment type. Note that the first judgments were the pre-test GPREDs for the pre–post group, the mid-test GPREDs for the mid–post group, and the GPOSTs for the post–only group. Participants predicted item memory to be better than source memory, Pillai's Trace = .16, $F(1, 141) = 27.16$, $p < .001$, $\eta_p^2 = .16$. Notably, there was an interaction between the factors judgment group and source–item expectancy, Pillai's Trace = .05, $F(2, 141) = 3.55$, $p = .031$, $\eta_p^2 = .05$. None of the other effects was significant, all $F \leq 2.34$, $p \geq .100$. To follow-up on the interaction, we analyzed the three judgment groups separately.

Pre-test GPREDs. In the pre–post group, we analyzed GPREDs with a 2 (source–item expectancy) × 2 (judgment type) within-subjects MANOVA. Participants predicted item memory to be better than source memory, Pillai's Trace = .23, $F(1, 47) = 14.19$, $p < .001$, $\eta_p^2 = .23$. Expected source–item pairs were predicted to be better remembered (expectancy effect), Pillai's Trace = .14, $F(1, 47) = 7.50$, $p = .009$, $\eta_p^2 = .14$. The interaction was also significant, Pillai's Trace = .10, $F(1, 47) = 5.09$, $p = .029$, $\eta_p^2 = .10$. Pairwise *t* tests revealed that there was an expectancy effect on source-memory prediction, $t(47) = 3.15$, $p = .003$, $d_z = 0.45$, but not on item-memory prediction, $t(47) = 1.49$, $p = .143$.

Next, we determined the relationship between pre-test source-memory GPRED conviction and source guessing for the whole test phase and the separate test halves. Pre-test conviction did not predict source guessing, neither across the whole test phase (Table 3) nor within test halves (first half: .002, [–.009, .013], second half: .001, [–.010, .012]). This suggests, again, that source guessing was not driven by a compensatory strategy at any point during the test in the pre–post group.

Mid-test GPREDs. In the mid–post group, we analyzed GPREDs with a 2 (source–item expectancy) × 2 (judgment type) within-subjects MANOVA. Participants predicted item memory to be better than source memory, Pillai's Trace = .21, $F(1, 47) = 12.45$, $p < .001$, $\eta_p^2 = .21$. None of the other effects were significant, all $F \leq 0.12$, $p \geq .733$. In particular, there was neither an expectancy effect, nor an inconsistency effect on memory judgments.

Next, we determined the relationship between mid-test source-memory GPRED conviction and source guessing for the whole test phase and the separate test halves. Unexpectedly, mid-test conviction predicted source guessing in the first test half, .011, [0, .022]. This finding is, as in Experiment 2, not compatible with a compensatory strategy, because it indicates that those who believed more strongly in an expectancy effect showed *more* schema-consistent guessing, whereas a metacognitive compensation account would predict less of it. Mid-test conviction did not predict source guessing across the whole test phase (Table 3) nor in the second test half, .001, [–.012, .014]. This again suggests that source guessing was not driven by a compensatory strategy at any point during the test in the mid–post group.

GPOSTs. Because all judgment groups provided GPOSTs, we included the data from all participants in this analysis. Descriptive statistics are in Table 2. We analyzed GPOSTs with a 3 × 2 × 2 mixed factorial MANOVA with the between-subjects factor judgment group (pre–post vs. mid–post vs. post–only) and the within-subjects factors source–item expectancy and judgment type. Item memory was judged to have been better than source memory, Pillai's Trace = .13, $F(1, 141) = 21.27$, $p < .001$, $\eta_p^2 = .13$. Also, unexpected source–item pairs were judged to have been remembered better than expected pairs (inconsistency effect), Pillai's Trace = .03, $F(1, 141) = 4.40$, $p = .038$, $\eta_p^2 = .03$. There was further a main effect of the judgment group, $F(1, 141) = 5.47$, $p = .005$, $\eta_p^2 = .07$, indicating that the groups differed in their average memory postdiction after test. None of the other effects was significant, all $F \leq 1.30$, $p \geq .275$. Crucially, the strength of the inconsistency effect did not differ between groups as indicated by the absence of an interaction.

Next, we determined the relationship between source-memory GPOST conviction and source guessing for the whole test phase and the separate test halves. GPOST conviction did not relate to source guessing, neither across the whole test phase (see Table 3) nor within test halves (first half: .002, [–.005, .009], second half: .002, [–.006, .010]). This suggests, again, that source guessing was not driven by a compensatory strategy at any point during the test.

Contingency judgments. Table 4 shows contingency judgments and tests against the true contingency of .5 and against source-guessing bias. Participants believed that more items had been presented with the expected source. Source guessing was even more biased toward the expected source than was the contingency judgment, replicating the previous experiments.

Next, we determined the relationship between contingency judgments and source guessing for the whole test phase and the separate test halves Contingency judgment related to source guessing both across the whole test phase (Table 4) as well as within test halves (first half: 1.91, [1.08, 2.75], second half: 2.39, [1.48, 3.33]). The more biased the contingency judgment was towards more expected pairs, the more biased was source guessing towards the expected source (replicating the previous experiments and Arnold et al., 2013).

Discussion

In Experiment 3, we determined the development of metamemory convictions during the test phase. Metamemory judgments regarding effects of schemas on item memory and source memory changed with test experience. Prior to test, there was no effect on item-memory judgments and an expectancy effect on source-memory judgments. The pre-test source-memory GPREDs thus replicated the JOS findings from Experiments 1 and 2; pre-test source-memory GPREDs and actual source memory were dissociated. In contrast, item-memory GPREDs did not show an expectancy effect, and, thus, were not dissociated from actual item memory. In the middle of the test and after the test, source-memory judgments differed from those given before the test. In the middle of the test, we found no expectancy effect on item-memory and source-memory predictions. After test, there was an inconsistency effect. Thus, participants updated their metamemory convictions with test experience.

We then asked whether participants used this updated knowledge to metacognitively control their guessing. Overall, we found no evidence that this was the case. First, there was no change in guessing bias between the test halves. Second, metamemory convictions about source-memory were not predictive of source guessing, neither for the whole test phase, nor for the test halves. The only exception was the first test half of the mid–post group, in which mid-test GPRED conviction predicted source guessing with a positive regression weight. As in Experiment 2, however, this indicates that those who more strongly believe in an expectancy effect show more schema-consistent guessing. Crucially, this finding is not compatible with the compensatory-guessing hypothesis which predicts the opposite. Thus, while participants updated their metamemory convictions about source memory, this knowledge was not strategically used to guide source guessing in a compensatory way. As in the other experiments, the contingency rating was somewhat predictive of source guessing. Source guessing was, however, more strongly biased towards the expected source than the contingency bias would predict. Thus, we conclude again that guessing was based on both a biased contingency representation and mere schema reliance.

Experiment 4

Experiments 1 to 3 suggest that source guessing does not depend on a compensatory strategy. However, so far, the analyses were purely correlational. In Experiment 4, we sought to test the causal link between metamemory convictions at the time of test and source-guessing bias posited by the compensatory-guessing account. We moved beyond correlations, because it is possible that participants' convictions in the previous experiments were too similar to show a correlational relationship between metamemory and source guessing. Even though according to the previous experiments compensation cannot explain the overall schema-consistent guessing bias, it is still possible that compensatory guessing enhances this bias in a group of participants with an inconsistency conviction. We therefore manipulated participants' conviction regarding the influence of schematic expectations on source memory. An expectancy effect on source memory was suggested to one group of participants (expectancy-conviction group), whereas an inconsistency effect on source memory was suggested to the other group (inconsistency-conviction group). If compensatory guessing is used as a metacognitive strategy based on metamemory convictions, the manipulation of said convictions should influence source guessing. Those who believe in an inconsistency effect should seek to compensate via schema-consistent source guessing, whereas those who believe in an expectancy effect should show schema-inconsistent (or, at least, less schema-consistent) source guessing.

Method

Participants. We recruited 72 new native speakers of German (22 male) at Heinrich-Heine-Universität Düsseldorf. Age ranged between 18 and 30 years (M = 20.96, SE = 0.31). Participants were randomly and evenly assigned to two conviction groups, resulting in n = 36 per group as in Experiments 1 and 2. Participants received course credit or monetary compensation.

Design and Material. We used a 2 × 2 design with the between-subjects factor conviction group (expectancy conviction vs. inconsistency conviction) and the within-subject factor source–item expectancy. We used the same materials with the same counterbalancing scheme as in Experiments 1 and 2.

Procedure. The procedure was similar to that in the post–only group of Experiment 1with the following exceptions. At the beginning of the experiment, after the instructions for the study and test phases, we manipulated convictions regarding the impact of schematic expectations on source memory. Given that fluency experience during study may contribute to the expectancy effect (Undorf & Erdfelder, 2015), we decided to give these instructions before study and to explicitly ask participants in the inconsistency-conviction group to discount fluency during study. Participants were informed that they would receive information about the research they were participating in and that they should read the instructions carefully and would be tested on them afterwards. Participants read the following information (translated from German). Group-specific information is in italics. Information for the expectancy-conviction group is followed by instructions for the inconsistency-conviction group in brackets.

> Our studies show that it is easier to remember when an object is located in a room that it is *TYPICAL* [*ATYPICAL*] for. For example, it is easier to remember that an armoire was located in the *bedroom* [*living room*] than in the *living room* [*bedroom*]. We explain this result by an *already existing connection* [*uncommon connection*] between the object and the room *in memory* [*attracting special attention*]. This *existing common* [*uncommon*] connection between an object and its *TYPICAL* [*ATYPICAL*] room is then *strengthened again* [*encoded into memory especially well*]. That way, it is easier later on to remember that the object was located in a *TYPICAL* [*ATYPICAL*] room. Our studies further show that during study, people experience encoding a TYPICAL room for an object as subjectively easier. When you do the experiment later on, you will probably also notice, that

the objects with TYPICAL rooms feel easier to learn. This subjective evaluation thus *concurs with actual memory* [*does not concur with actual memory*].

After that, to test participants' understanding of the respective information, they were asked whether TYPICAL or ATYPICAL information was remembered better. They could answer *typical*, *atypical* (by pressing numbers 1 or 2 on the keyboard, counterbalanced) or *equally well* (by pressing number 3). Their answer was scored as correct if it corresponded to their assigned conviction information. In addition, they were asked whether TYPICAL or ATYPICAL information felt easier to learn. The response options were the same as for the first question, but for all participants, the correct answer was the *typical* option. The instructions were repeated until participants answered both questions correctly, whereupon they proceeded to the study phase.

The study phase was the same as in the post–only group in Experiment 1. After study, participants in both groups were asked to provide GPREDs (as in Experiment 3). Participants then proceeded to the instructions for the source-monitoring test, and the rest of the experiment was the same as in Experiments 1 and 2.

Results

Source-monitoring processes. We disentangled memory and guessing via the same hierarchical MPT model as in previous experiments. Parameter estimates and 95% BCIs of the estimates are in Table 1. Parameter convergence was good as indicated by $\hat{R} < 1.05$ for all parameters. Model fit was also good for the expectancy-conviction group, T_1: $p = .474$, T_2: $p = .548$, and the inconsistency-conviction group, T_1: $p = .501$, T_2: $p = .532$.

Again, we tested whether source guessing was based on a compensatory strategy or a contingency-based strategy using regression analyses. Because both groups provided the same judgments (GPREDs, GPOSTs, and contingency judgments), we regressed source guessing (*g*) on GPRED conviction, GPOST conviction, and contingency judgments in a model that included both groups (T_1: $p = .502$, T_2: $p = .560$). We will report the regression results below, after reporting the effects of the experimental manipulations on GPREDs, GPOSTs and contingency judgments, respectively.

Item memory. In the expectancy-conviction group, there was an inconsistency effect on item memory, $\Delta D = .08$, [.01, .16]. Note that if the alternative model (with restriction $D_{\text{expected}} = D_{\text{new}}$) was used, there was no such effect in this group ($\Delta D = .07$, [–.01, .14]). In the inconsistency-conviction group, there was no effect of schemas, $\Delta D = .05$, [–.05, .15].

Source memory. Figure 1 shows source memory. Neither the expectancy-conviction group showed an effect of source–item expectancy on source memory, $\Delta d = .11$, [–.33, .61], nor did the inconsistency-conviction group, $\Delta d = .42$, [–.09, .81].

Source guessing. The probabilities to guess the expected source (guessing parameter *g*) are in Table 1. This probability was higher than the chance probability of .5 in both the expectancy-conviction group, $\Delta g_{.50} = .18$, [.06, .28], and the inconsistency-conviction group, $\Delta g_{.50} = .18$, [.08, .26]. Notably, there was no difference in guessing bias between the two groups, $\Delta g_{\text{inconsistency conv. – expectancy conv.}} = .00$, [–.14, .15].

Metamemory.

GPREDs. We analyzed GPREDs with a 2 × 2 × 2 mixed factorial MANOVA with the between-subjects factor conviction group (expectancy conviction vs. inconsistency conviction) and the within-subjects factors source–item expectancy and judgment type. Descriptive statistics are in Figure 4. Participants predicted item memory to be better than source memory, Pillai's Trace = .16, $F(1, 70) = 13.34$, $p < .001$, $\eta_p^2 = .16$. Notably, there was a significant two-way interaction between the factors conviction group and source–item expectancy, Pillai's Trace = .11, $F(1, 70) = 8.51$, $p = .005$, $\eta_p^2 = .11$. None of the other effects was significant, all $F \leq 2.18$, $p \geq .144$.

To further test the interaction between the factors conviction group and source–item expectancy, we conducted 2 (source–item expectancy) × 2 (judgment type) within-subjects MANOVAs for each conviction group separately. In the expectancy-conviction group, there was an expectancy effect, Pillai's Trace = .17, $F(1, 35) = 7.27$, $p = .011$, $\eta_p^2 = .17$. Additionally, items were predicted to be better remembered than sources, Pillai's Trace = .24, $F(1, 35) = 10.95$, $p = .002$, $\eta_p^2 = .24$. There was no interaction, Pillai's Trace = .001, $F(1, 35) = 0.04$, $p = .838$. Paired t tests revealed that the expectancy effect was significant for both item-memory judgments, $t(35) = 2.40$, $p = .022$, $d_z = .40$, and source-memory judgments, $t(35) = 2.39$, $p = .022$, $d_z = .40$.

In the inconsistency-conviction group, there was no effect of source–item expectancy on metamemory judgments, Pillai's Trace = .04, $F(1, 35) = 1.55$, $p = .222$. Items were predicted to-be-remembered better than sources, Pillai's Trace = .11, $F(1, 35) = 4.15$, $p = .049$, $\eta_p^2 = .11$. There was no interaction, Pillai's Trace = .05, $F(1, 35) = 1.81$, $p = .187$. Paired t tests showed that there was no effect on item-memory judgments, $t(35) = 0.20$, $p = .842$, and a marginally non-significant inconsistency effect on source-memory judgments, $t(35) = 1.98$, $p = .055$, $d_z = .33$. For both groups, pre-test item-memory and source-memory GPREDs were positively correlated for expected and unexpected source–item pairs (see Table A1 in the Appendix).

Next, we determined the relationship between source-memory GPRED conviction and source guessing. GPRED conviction did not predict source guessing (see Table 3). This again suggests that source guessing was not driven by a compensatory strategy.

GPOSTs. Descriptive statistics for GPOSTs are in Table 2. We analyzed GPOSTs with a 2 (conviction group) × 2 (source–item expectancy) × 2 (judgment type) mixed factorial MANOVA. Item memory was perceived to have been better than source memory, Pillai's Trace = .11, $F(1, 70) = 8.61$, $p = .005$, $\eta_p^2 = .11$. All other effects were not significant, all $F \leq 2.06$, $p \geq .156$. For both groups, item-memory and source-memory GPOSTs were positively correlated for expected and unexpected source–item pairs (see Table A1 in the Appendix).

Next, we determined the relationship between source-memory GPOST conviction and source guessing. GPOST conviction did not relate to source guessing (see Table 3). This suggests, again, that source guessing was not driven by a compensatory strategy.

Contingency judgments. Table 4 presents contingency judgments and tests against the true contingency of .5 and against source-guessing bias. Participants believed that more items had been presented with the expected source. Source guessing was however even more biased towards the expected source than the contingency judgment was, replicating the previous experiments.

Next, we determined the relationship between contingency judgments and source guessing. Contingency judgments related to source guessing (see Table 4). The more biased the contingency judgment was towards more expected pairs, the more biased was source guessing towards the expected source (replicating the previous experiments and Arnold et al., 2013).

Discussion

Experiment 4 provided further evidence that schema-consistent source guessing is not based on a metacognitive compensatory strategy. We manipulated metacognitive convictions prior to the experiment. We found that the expectancy-conviction group indeed showed an expectancy effect on GPRED. However, the inconsistency-conviction group showed only a marginal inconsistency effect. Experiments 1, 2, and especially 3 showed that participants naturally hold the conviction of an expectancy effect. The expectancy-conviction manipulation of Experiment 4 thus concurred with this conviction. By contrast, manipulating participants into believing in an inconsistency effect was more difficult due to a conflicting a-priori conviction of an expectancy effect. Nonetheless, our inconsistency-effect manipulation eliminated the expectancy illusion in this group. Across both groups, this resulted in a wider range of convictions than in the previous experiments. In the hierarchical analyses, we therefore included participants' indicated conviction regardless of group.

Importantly, the GPRED conviction differed between groups, but source guessing did not. That is, the conviction of an expectancy effect prior to test resulted in the same amount of schema-consistent guessing as the conviction of no effect (with a tendency toward an inconsistency effect). In addition, metacognitive convictions prior to and after test were not predictive of source guessing. Thus, the pattern of results again suggests that participants did not use compensatory guessing as a metacognitive control strategy. Schema-consistent guessing occurs regardless of metamemory conviction. Instead, we found evidence for contingency-based guessing (replicating Arnold et al., 2013), and additionally, schema-based guessing, because guessing was more biased than the contingency perception.

General Discussion

Understanding source monitoring requires the understanding of effects of source schemas on source-monitoring processes. In four experiments, we therefore compared the influence of schema-consistent and schema-inconsistent information on metamemory, source memory, and source guessing. There are two main results of our study. First, we found a new metamemory expectancy illusion in item memory and source memory. Second, this source-memory illusion provides novel insights into the mechanisms underlying schema-related source monitoring. Our results show that schema-consistent guessing accompanying the inconsistency effect on source memory is *not* based on a compensatory guessing strategy, but instead reflects perceived source–item contingencies and reliance on schemas. These results will be further discussed in the following.

Metamemory Illusion in Item Memory and Source Memory

Across our experiments, we found consistent evidence that metamemory convictions are dissociated from item memory and source memory in the schema-based source-monitoring paradigm. Participants predicted expectancy effects on both item-wise (item memory and source memory) and global (source memory) judgments, but showed generally no effect on item memory and either no effect or an inconsistency effect on source-memory (replicating Bell et al., 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kroneisen et al., 2015; Küppers & Bayen, 2014). These metamemory illusions are in line with findings across different paradigms showing that under certain circumstances people show strong misconceptions about their own memory and about conditions that hinder or facilitate learning and remembering (e.g., Besken & Mulligan, 2013, 2014; Kassam, Gilbert, Swencionis, & Wilson, 2009; Kornell et al., 2011; Rhodes & Castel, 2008, 2009). The following paragraphs include detailed discussions of our findings with regard to both aspects of the illusion, the inconsistency effect on memory and the expectancy effect on metamemory.

Inconsistency effect on (source) memory. Küppers and Bayen (2014) reported an inconsistency effect on source memory. We replicated this effect under conditions in which participants did not provide source-memory judgments before or during test (i.e., in the post–only groups of Experiments 1 and 3 and in the JOL-only group of Experiment 2). The inconsistency effect occurred likely because unexpected information attracted more attention than expected information and was thus processed more elaborately at encoding (attention-elaboration theory by Brewer & Treyens, 1981, see also Erdfelder & Bredenkamp, 1998; Friedman, 1979; Küppers & Bayen, 2014; Loftus & Mackworth, 1978).

Interestingly, the requirement of providing item-wise (Experiments 1 and 2) or global (Experiments 3 and 4) metamemory predictions before or during test decreased the inconsistency effect. Concerning item-wise predictions, this finding corresponds to several studies in which item-wise JOLs reduced effects of item-memory manipulations (Besken, 2016; Besken & Mulligan, 2013, 2014; Soderstrom et al., 2015; Susser et al., 2013) and extends these findings to source memory. Notably, the inconsistency effect on source memory remained intact when only JOLs were provided. This novel finding suggests that the altering effect of metamemory judgments on memory is specific to judgment content. When participants are asked for source-memory predictions, they may attend more equally to both sources. When they are not asked for source-memory predictions and thus process the sources less deliberately, unexpected sources may capture more attention. Additionally, in Experiment 4, the conviction manipulation may have resulted in a more equal attention distribution between the sources. Thus, the manipulation may have eliminated an inconsistency effect on source memory in both groups. Future research should more closely investigate the mechanisms of reactive effects of memory judgments, for example whether asking for JOSs increases attention-allocation to the source and makes participants engage in more effective source–item encoding strategies for all trials (e.g., Kuhlmann & Touron, 2012).

Concerning global predictions, the decrease in the inconsistency effect may be due to the explicit mentioning of the source–item expectancy manipulation before (part of) the test. This may influence the way information is retrieved from memory. Thus, even global judgments after study can alter memory processes, extending the results by Soderstrom et al. (2015). Importantly, however, none of the groups showed an expectancy effect on source memory.

Across four experiments, we generally did not find an effect of source–item expectancy on item memory in the majority of the groups (consistent with a large body of research, e.g., Bayen & Kuhlmann, 2011; Bayen et al., 2000; Bell et al., 2012; Ehrenberg & Klauer, 2005; Kuhlmann et al., 2012; Küppers & Bayen, 2014). However, in Experiments 3 and 4, two groups (the mid–post group and the expectancy-conviction group, respectively) showed a non-predicted inconsistency effect on item memory, in line with research on schema effects on item recall and recognition (Rojahn & Pettigrew, 1992; Stangor & McMillan, 1992). However, we will refrain from further interpretation, because these inconsistency effects only emerged in groups with some mentioning of the expectancy manipulation, and were not replicated in other groups (most notably, in the "naïve" post–only groups).

Expectancy effects on metamemory

Expected information was rated as more memorable than unexpected information in all judgments prior to test (if not manipulated, Experiment 4). As predicted, the expectancy effect on JOLs reported by Konopka and Benjamin (2009) also, and even more so, occurred on JOSs (Experiments 1 and 2). This is in line with research suggesting that people use similar cues for JOLs and JOSs and assume similar processes for item memory and source memory (Carroll et al., 1999, 2001). We also found this expectancy effect on pre-test source-memory GPREDs (Experiment 4).

Compared with schema effects on actual memory performance, as reviewed above, these expectancy effects on metamemory manifest at least single-dissociated metamemory illusions for item memory and source memory. If we assume that participants in the post–only groups of Experiments 1 and 3 held the same expectancy conviction as those groups that judged memory before test (which eliminated the inconsistency effect on source memory), source-memory predictions and source memory may even be doubly dissociated. That is, there is an expectancy effect on source-memory predictions, but an inconsistency effect on actual source memory. Similarly, item-memory predictions were doubly dissociated from item memory in two groups that showed an inconsistency effect on item memory (specifically, the mid–post group in Experiment 3 and the expectancy-conviction group in Experiment 4).

Why do people show this expectancy illusion? Notably, these expectancy effects concur with the (non-illusory) relatedness effect on JOLs for paired associates (Hertzog et al., 2002; Mueller et al., 2013; Rabinowitz et al., 1982; Undorf & Erdfelder, 2011, 2013, 2015). Both a-priori convictions about relatedness and item memory (Mueller et al., 2013) as well as greater encoding fluency of related pairs (Undorf & Erdfelder, 2011, 2013, 2015) may explain this effect on JOLs (and, in extension, on JOSs, Experiments 1 and 2). For one, people may have preexisting convictions about effects of relatedness of item and source on (item and source) memory. Likewise, it is plausible that expected source–item pairings are processed more fluently than unexpected source–item pairings due to their conceptual relatedness (cf. *conceptual fluency*, Alter & Oppenheimer, 2009). First evidence for this stems from studies by Sherman et al. (1998) who found that participants were faster when studying expected than unexpected source–item pairings; self-paced study time is one possible measure of encoding fluency (e.g., Undorf & Erdfelder, 2015). Thus, JOLs and JOSs may have been based on either a-priori convictions, in-the-moment experiences, or both. Similarly, both factors may also have contributed to GPREDs prior to test (Besken, 2016; Koriat et al., 2004). Analogously to JOSs, source-memory GPREDs showed an expectancy effect. However, in contrast to JOLs, item-memory GPREDs did not show an expectancy effect (Experiment 3). Thus, there may be differences in the basis for item-wise versus global judgments. JOLs and JOSs may particularly rely on in-the-moment experiences, because the source–item pair is experienced right at the time of judgment (e.g., Undorf & Erdfelder, 2015). Overall, the illusory convictions concerning source memory were quite strong and even direct manipulation of metamemory convictions did not fully invert memory judgments towards an inconsistency effect (Experiment 4). This may suggest that either participants already held quite strong convictions that were difficult to manipulate, and/or that their fluency experiences were not fully discounted at the time of judgment. Future research should query to what extent participants hold preexisting convictions about expectancy effects on source memory as well as determine whether high encoding fluency of expected source–item pairs contributes to the expectancy effect on metamemory.

Notably, the strength of the expectancy effect on metamemory depended on judgment content. Specifically, people predicted a stronger expectancy effect on source memory than on item memory. This was the case for JOLs and JOSs (Experiments 1 and 2) as well as for GPREDs (Experiment 3). Thus, people may realize that schematic relatedness of items and their sources influences source memory more than item memory (although they judge the direction of this influence incorrectly). Additionally, the strength of the expectancy effect on JOLs increased when there was no possibility of also providing JOSs (Experiment 2). This suggests that the conviction about source memory contaminated JOLs when JOSs were not separately provided. This finding highlights the importance of assessing separate judgments for different memory types to avoid contamination. Future research should further address the influence different metamemory judgments have on one another. Likewise, it should be noted that it is not known how source information, broadly conceived as any information that is present along with the item, influences item-memory judgments, thus this should be investigated systematically to truly understand different metamemory judgments.

As shown in all experiments, test experience updated metamemory convictions, as early as in the middle of the test (Experiment 3). Most groups did not show any expectancy effect on metamemory after test, and in Experiments 1 and 3 we found evidence that this updating of metamemory may even go as far as to reflect the actual inconsistency effect on source memory. Thus, updating knowledge about memory is possible, but it seems to be difficult and not necessarily fully reflective of actual memory.

Schema-consistent Source Guessing

The second main contribution of our research concerns the nature of response bias in source monitoring. We found that when participants did not know the source of a certain item, they tended to guess the schematically expected source (as indicated by MPT model parameter $g > .5$) replicating a large body of existing research (e.g., Bayen et al., 2000; Bell et al., 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kuhlmann et al., 2012; Küppers & Bayen, 2014). Küppers and Bayen (2014) suggested compensatory guessing as explanation for the schema-consistent source-guessing bias (see also Batchelder & Batchelder, 2008; Ehrenberg & Klauer, 2005; Kuhlmann & Touron, 2011; Meiser et al., 2007). However, this metacognitive strategy would require awareness of the inconsistency effect on source memory. With such awareness, schema-consistent guessing could serve as a reasonable strategy to compensate for the relatively poor memory for expected sources. However, as detailed above, we consistently found that participants were not aware of the inconsistency effect prior to test but, in fact, assumed the opposite (i.e., an expectancy effect on source memory). Further, we found no evidence that a conviction of an inconsistency effect led to schema-consistent source guessing in any way. This was consistent over judgments made during study, prior to the test, in the middle of the test, and after the test. In Experiment 2, JOL and JOS convictions (in the JOL-only and JOS-only groups) and GPOST conviction concurred with source guessing. This was also the case for the second test half of the mid–post group in Experiment 3. However, in these cases, an expectancy conviction was related to *more* schema-consistent source guessing. Thus, even these non-predicted results are incompatible with a compensatory-guessing account. Additionally, we showed that while metacognitive convictions are updated during test, this does not relate to source guessing (Experiment 3). Thus, strategic guessing is not added as a secondary strategy with growing metacognitive awareness of the inconsistency effect. In addition, we showed that manipulation of metacognitive convictions did not influence source guessing (Experiment 4). Thus, schema-consistent source guessing does not reflect compensatory guessing as a form of strategic metacognitive control (see Nelson & Narens, 1990). These findings are in line with research on recognition memory showing that people often fail to strategically adapt an optimal response bias (Cox & Dobbins, 2011; Morrell, Gaitan, & Wixted, 2002; Verde & Rotello, 2007).

Crucially, we do not mean to imply that source guessing is never metacognitively guided or that people never strategically update their response bias. In fact, there is evidence for both in memory tasks that do not use schematic material (metacognitive source guessing: Batchelder & Batchelder, 2008; Meiser et al., 2007; strategic updating of response bias: Rhodes & Jacoby, 2007). Therefore, this finding seems rather specific to situations in which schematic expectations come into play and offer alternative guessing strategies.

Instead of metacognitive control, schema-consistent guessing may rather reflect processes predicted by the probability-matching account (Arnold et al., 2013; Bayen & Kuhlmann, 2011;

Kuhlmann et al., 2012; Spaniol & Bayen, 2002). Thus, it may either be based on a biased representation of the contingencies during study (contingency-based guessing), or on mere reliance on schemas in semantic memory (schema-based guessing).

Concerning the distinction between these two types of guessing, our results suggest two things. First, contingency judgments after the source-monitoring task indicated that participants thought that more items had been presented with their expected source than with their unexpected source. Additionally, contingency judgments predicted source guessing. Participants who believed that more items had been presented with the expected source also guessed schema-consistently more often (replicating Arnold et al., 2013). Thus, we found evidence that schema-consistent source guessing is partially due to a biased representation of the source–item contingency.

Second, however, guessing was more biased than the contingency bias would suggest (replicating Bayen & Kuhlmann, 2011). This suggests that contingency judgments can only partially explain the schema-consistent guessing bias. In addition, participants appear to default to general schematic expectations in source guessing. Note that the use of this schema-based strategy is quite adaptive in the real world, because usually, our schematic expectations match the situation we try to remember. Thus, guessing the option that matches the schema if uncertain will often result in the correct response in real life. The exact disentanglement of contingency-based guessing and schema-based guessing needs to be addressed more thoroughly in future research. In particular, the reasons for bias in contingency representations should be determined.

In summary, we demonstrated a new metamemory illusion in schema-based source monitoring. This illusion has important theoretical implications regarding source-monitoring processes. Further, our results on different metamemory judgments within the source-monitoring paradigm bring some novel insights on metamemory in general, such as the reactivity of metamemory judgments and contamination between different judgments. We hope that future research will further increase our understanding of the interplay of memory and metamemory in source monitoring.

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Footnotes

[1] In its most unrestricted (and non-identifiable) version, the model described by Bayen et al. (1996) does not assume that the probability of guessing the expected source is equal for recognized and unrecognized items. To obtain an identifiable model, we did make this reasonable assumption which found support in many source-monitoring studies (e.g., Bayen et al., 1996, 2000; Bell et al., 2012; Ehrenberg & Klauer, 2005).

Table 1

MPT Model Parameter Estimates and 95% Bayesian Credibility Intervals for all Experiments

		Parameter estimates					
Experiment	Group	$D_{expected}$	$D_{unexpected}$	$d_{expected}$	$d_{unexpected}$	b	g
1	Post–only	.57	.54	.18	.75	.24	.76
		[.46, .67]	[.46, .62]	[.01, .52]	[.57, .90]	[.15, .33]	[.65, .84]
	JOL & JOS	.71	.70	.42	.52	.23	.66
		[.64, .77]	[.66, .75]	[.05, .74]	[.33, .65]	[.15, .33]	[.52, .77]
2	JOL & JOS	.75	.74	.45	.61	.24	.64
		[.65, .83]	[.68, .79]	[.09, .73]	[.41, .75]	[.16, .32]	[.48, .77]
	JOL-only	.64	.60	.20	.68	.24	.72
		[.57, .71]	[.53, .68]	[.01, .49]	[.54, .80]	[.17, .31]	[.61, .80]
	JOS-only	.67	.60	.55	.69	.29	.66
		[.59, .74]	[.54, .67]	[.27, .75]	[.54, .82]	[.22, .38]	[.54, .76]
3	Pre–post	.54	.55	.42	.62	.36	.62
		[.47, .61]	[.49, .60]	[.12, .65]	[.45, .76]	[.30, .43]	[.52, .72]
	Mid–post	.52	.60	.44	.68	.34	.64
		[.44, .60]	[.53, .67]	[.08, .78]	[.57, .78]	[.26, .42]	[.53, .74]
	Post–only	.57	.61	.27	.81	.33	.74
		[.50, .64]	[.54, .67]	[.02, .58]	[.65, .93]	[.26, .40]	[.64, .83]
4	Expectancy	.49	.57	.53	.64	.31	.68
	conviction	[.41, .56]	[.50, .64]	[.11, .85]	[.41, .81]	[.23, .40]	[.56, .78]
	Inconsistency	.45	.50	.33	.75	.39	.68
	conviction	[.36, .54]	[.42, .58]	[.02, .77]	[.55, .92]	[.29, .48]	[.58, .76]

Note. Group-level parameters were estimated with the Bayesian-hierarchical latent trait approach (Klauer, 2010) using the R package TreeBugs (Heck et al., 2017). Parameters are on a probability scale from 0 to 1. JOL & JOS = participants provided both Judgments of Learning (JOLs) and Judgments of Source (JOSs); JOL-only = participants provided only JOLs; JOS-only = participants provided only JOSs. Pre–post = participants provided (item and source) pre-test global differentiated predictions (GPREDs) and global differentiated postdictions (GPOSTs). Mid–post = participants provided (item and source) mid-test GPREDs and GPOSTs. Post–only = participants provided (item and source) GPOSTs only. Expectancy conviction = participants were manipulated to believe in an expectancy effect. Inconsistency conviction = participants were manipulated to believe in an inconsistency effect. $D_{expected}$/$D_{unexpected}$ = probability of recognizing an expected/unexpected item; d_{expected}/$d_{\text{unexpected}}$ = probability of remembering that an item was presented with the expected/unexpected source; b = probability of guessing that an item is old; g = probability of guessing that an item was presented with the expected source. D_{new} (probability of knowing that a new item is new) was set equal to $D_{\text{unexpected}}$. 95% Bayesian credibility intervals are in brackets.

Table 2

Descriptive Statistics for GPOSTs

Experiment	Group	Judgment Type	Source–Item Expectancy	
			expected	unexpected
1	Post–only	item memory	49.92 [46.93, 52.90]	51.81 [48.82, 54.79]
		source memory	43.69 [40.91, 46.48]	54.11 [51.33, 56.89]
	JOL & JOS	item memory	58.53 [55.77, 61.29]	53.06 [50.30, 55.82]
		source memory	55.19 [52.29, 58.09]	47.86 [44.96, 50.76]
2	JOL & JOS	item memory	61.17 [57.33, 65.01]	61.61 [57.77, 65.45]
		source memory	58.00 [54.51, 61.49]	55.78 [52.29, 59.27]
	JOL-only	item memory	57.33 [53.61, 61.06]	59.14 [55.41, 62.87]
		source memory	50.25 [46.36, 54.14]	52.14 [48.25, 56.02]
	JOS-only	item memory	64.17 [61.26, 67.07]	60.89 [57.98, 63.80]
		source memory	59.28 [56.64, 61.92]	59.42 [56.78, 62.06]
3	Pre–post	item memory	43.85 [41.96, 45.75]	45.58 [43.69, 47.48]
		source memory	38.81 [36.61, 41.01]	39.40 [37.20, 41.60]
	Mid–post	item memory	50.44 [48.07, 52.80]	53.21 [50.85, 55.57]
		source memory	43.94 [42.09, 45.78]	49.46 [47.62, 51.30]
	Post–only	item memory	53.17 [50.52, 55.81]	55.71 [53.06, 58.35]
		source memory	51.15 [48.19, 54.10]	53.33 [50.38, 56.29]
4	Expectancy conviction	item memory	45.03 [41.68, 48.37]	44.08 [40.74, 47.43]
		source memory	42.83 [39.81, 45.85]	42.03 [39.01, 45.05]
	Inconsistency conviction	item memory	43.08 [40.36, 45.08]	44.39 [41.66, 47.12]
		source memory	35.11 [32.57, 37.65]	40.56 [38.56, 43.09]

Note. Global differentiated postdictions (GPOSTs) were percentage ratings ranging from 0 to 100. JOL & JOS = participants provided Judgments of Learning (JOLs) and Judgments of Source (JOSs), JOL-only = participants provided only JOLs, JOS-only = participants provided only JOSs. Pre–post = participants provided (item and source) pre-test global differentiated predictions (GPREDs) and GPOSTs. Mid–post = participants provided (item and source) mid-test GPREDs and GPOSTs. Post–only = participants provided (item and source) GPOSTs only. Expectancy conviction = participants were manipulated to belief in an expectancy effect. Inconsistency conviction = participants were manipulated to belief in an inconsistency effect. 95% within-subjects confidence intervals are in brackets (Loftus & Masson, 1994).

Table 3

Metacognitive Source-Memory Convictions as Predictors of Source Guessing (g)

Experiment	Group	Time of Judgment	Estimate	95% BCI
1	JOL & JOS	during study	< .001	[–.009, .009]
	Both	post test	–.002	[–.008, .005]
2	JOL & JOS	during study	.002	[–.010, .016]
	JOL-only	during study (JOL)	.007	[.000, .015]
	JOS-only	during study	.011	[.000, .022]
	All	post test	.010	[.005, .015]
3	Pre–post	pre test	.001	[–.009, .012]
	Mid–post	mid test	.006	[–.005, .017]
	All	post test	.002	[–.004, .009]
4	Both	pre test	.003	[–.008, .014]
		post test	.003	[–.007, .013]

Note. Displayed are regression weights for the prediction of source-memory conviction on guessing parameter *g*. Conviction measures were calculated by subtracting the metamemory judgment (scale: 0 to 100) for unexpected source–item pairs from the judgment for expected pairs. JOL & JOS = participants provided both Judgments of Learning (JOLs) and Judgments of Source (JOSs); JOL-only = participants provided only JOLs (in this case, item-wise JOL conviction was used as predictor); JOS-only = participants provided only JOSs. Pre–post = participants provided (item and source) pre-test global differentiated predictions (GPREDs) and global differentiated postdictions (GPOSTs). Mid–post = participants provided (item and source) mid-test GPREDs and GPOSTs. BCI = Bayesian credibility interval.

Table 4

Mean Relative Contingency Judgment, Comparison with True Contingency and Source-Guessing Bias, and as Predictor of Source Guessing

		rCJ vs. zero contingency				*rCJ* vs. source guessing (*g*)	*rCJ* pred. source guessing (*g*)
Group	*rCJ* [95% CI]	*t*	*df*	*p*	*d*	[95% BCI]	[95% BCI]
Experiment 1	.55 [.52, .58]	3.41	71	.001	0.40	.15 [.09, .21]	2.50 [1.47, 3.55]
Experiment 2	.55 [.52, .57]	3.67	107	< .001	0.35	.11 [.05, .16]	1.40 [0.66, 2.15]
Experiment 3	.54 [.52, .56]	4.47	143	< .001	0.37	.10 [.06, .15]	2.14 [1.37, 2.92]
Experiment 4	.54 [.52, .56]	3.39	71	.001	0.40	.13 [.08, .18]	2.24 [0.74, 3.74]

Note. rCJ = mean relative contingency judgment of expected source–item pairings. For rCJs. 95% confidence intervals are in brackets. *t* values refer to one-sample *t* tests with effect size *d*. BCI = Bayesian credibility interval.

Table 5

MPT Model Parameter Estimates, Model Fit, Source-Guessing Comparisons, and 95% Bayesian Credibility Intervals for the Test Halves in Experiment 3

	Time of Metamemory Judgment(s)					
	Pre–post		Mid–post		Post–only	
Parameter	1st half	2nd half	1st half	2nd half	1st half	2nd half
$D_{expected}$	.60	.47	.56	.47	.58	.53
	[.49, .70]	[.37, .56]	[.47, .65]	[.36, .57]	[.50, .66]	[.42, .63]
$D_{unexpected}$	.60	.49	.63	.58	.63	.57
	[.52, .67]	[.43, .55]	[.55, .70]	[.50, .66]	[.56, .71]	[.51, .64]
$d_{expected}$	.36	.51	.56	.30	.50	.22
	[.04, .69]	[.12, .85]	[.10, .93]	[.01, .80]	[.07, .94]	[.01, .66]
$d_{unexpected}$	.68	.58	.65	.72	.78	.80
	[.49, .83]	[.27, .78]	[.44, .80]	[.57, .84]	[.55, .95]	[.61, .94]
b	.36	.37	.35	.34	.42	.28
	[.29, .43]	[.29, .45]	[.26, .45]	[.26, .43]	[.32, .51]	[.21, .36]
g	.64	.60	.58	.70	.66	.77
	[.52, .76]	[.48, .72]	[.46, .70]	[.58, .80]	[.52, .78]	[.65, .86]
Model Fit	T_1: p = .494, T_2: p =.231		T_1: p = .536, T_2: p =.411		T_1: p = .427, T_2: p =.414	
$\Delta g_{2nd\ half - 1st\ half}$	–.04 [–.20, .11]		.12 [–.04, .27]		.11 [–.04, .25]	

Note. Pre–post = participants provided (item and source) pre-test global differentiated predictions (GPREDs) and global differentiated postdictions (GPOSTs). Mid–post = participants provided (item and source) mid-test GPREDs and GPOSTs. Post–only = participants provided (item and source) GPOSTs only. $D_{expected}$/$D_{unexpected}$ = probability of recognizing an expected/unexpected item; $d_{expected}$/$d_{unexpected}$ = probability of remembering that an item was presented with the expected/unexpected source; b = probability of guessing that an item is old; g = probability of guessing that an item was presented with the expected source. D_{new} (probability of knowing that a new item is new) was set equal to $D_{unexpected}$. $\Delta g_{2nd\ half - 1st\ half} = g_{2nd\ half} - g_{1st\ half}$. 95% Bayesian credibility intervals are in brackets.

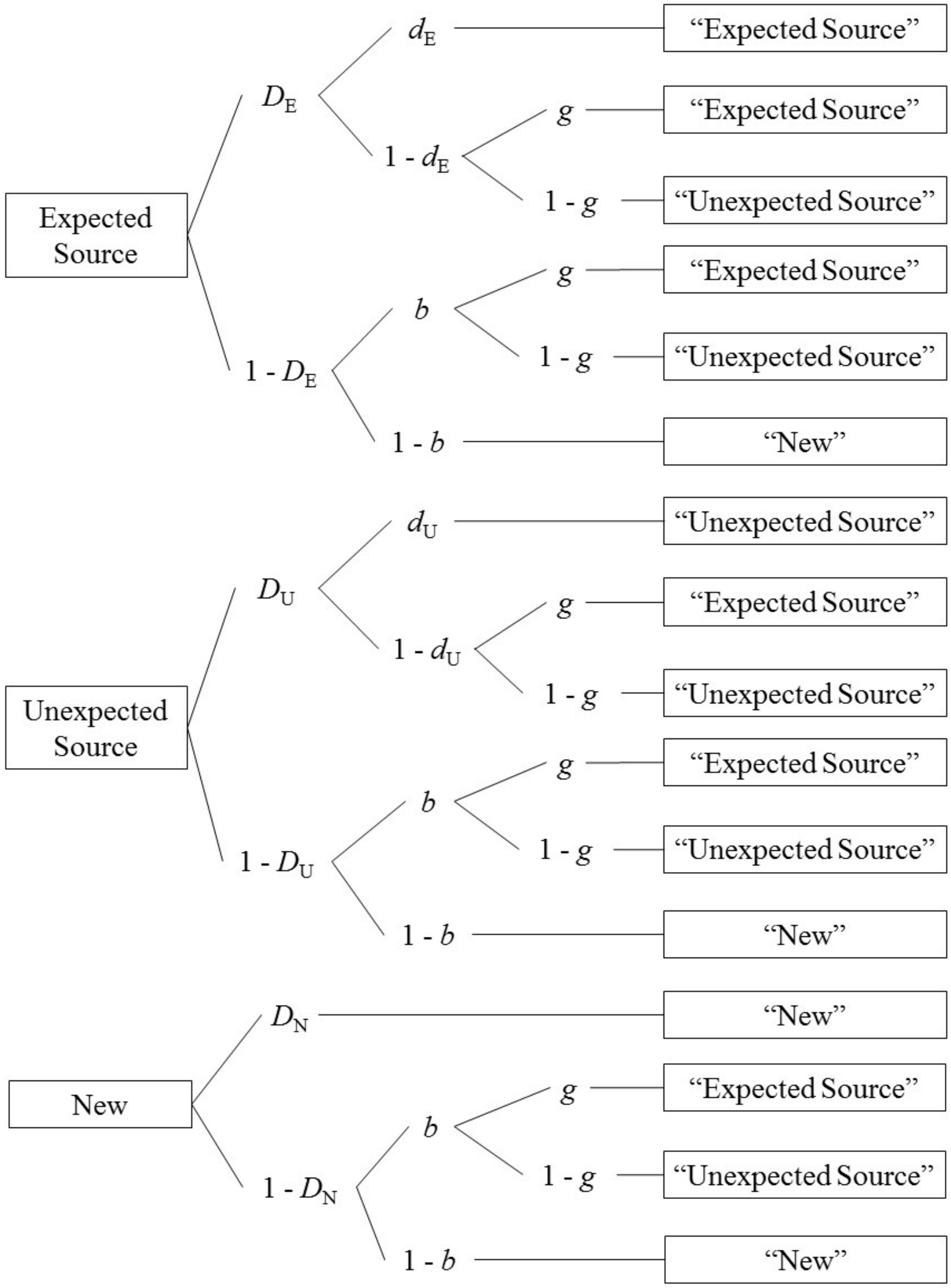

Figure 1. Two-high-threshold multinomial model of source monitoring. D_E = probability of recognizing an item that had been presented with the expected source; D_U = probability of recognizing an item that had been presented with the unexpected source; D_N = probability of knowing that a new item is new; d_E = probability of remembering that an item was presented

with the expected source; d_U = probability of remembering that an item was presented with the unexpected source; g = probability of guessing that an item had been presented with the expected source; b = probability of guessing that an item was old. Adapted from "Source Discrimination, Item Detection, and Multinomial Models of Source Monitoring," by U. J. Bayen, K. Murnane, and E. Erdfelder, 1996, *Journal of Experimental Psychology: Learning, Memory, and Cognition, 22,* p. 202, Figure 3.

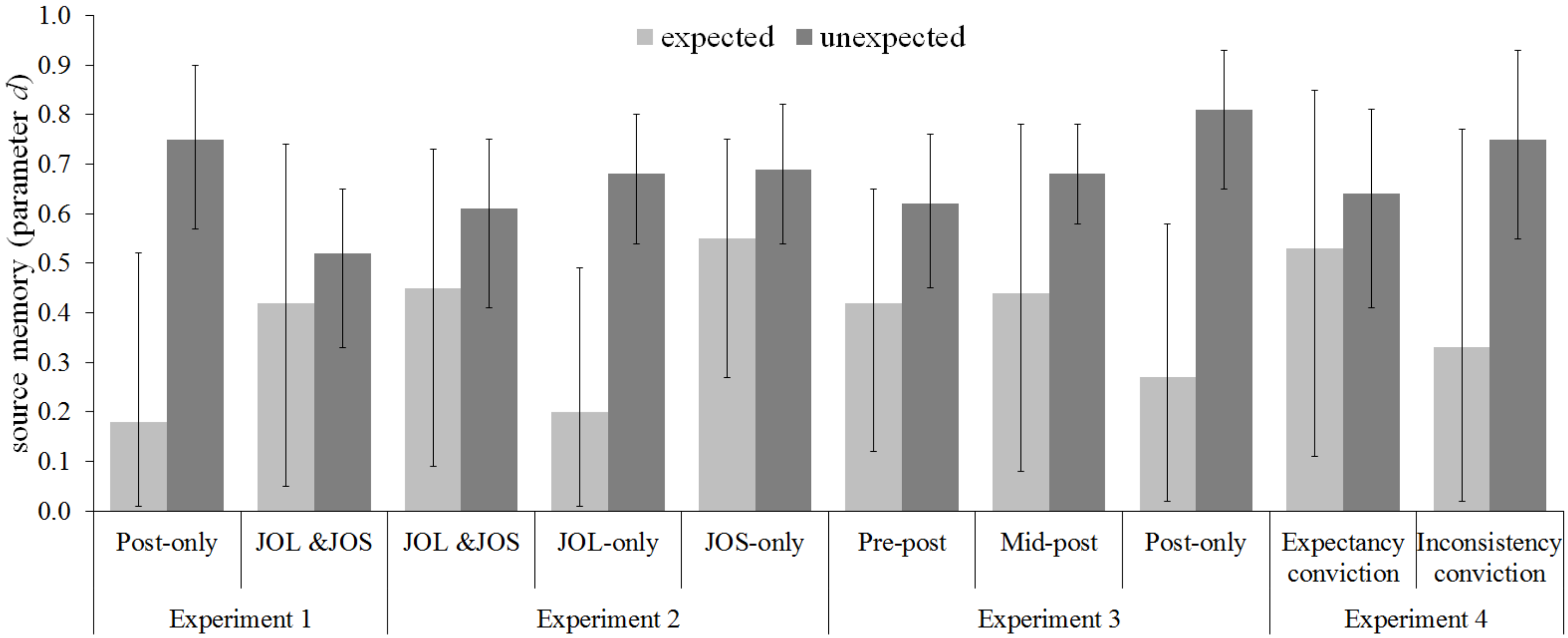

Figure 2. Source memory (*d*) as a function of source–item expectancy and judgment group in all Experiments. JOL & JOS = participants provided both item-wise Judgments of Learning (JOLs) and Judgments of Source (JOSs), JOL-only = participants provided JOLs only, JOS-only = participants provided JOSs only. Pre–post = participants provided (item and source) pre-test global differentiated predictions (GPREDs) and global differentiated postdictions (GPOSTs). Mid–post = participants provided (item and source) mid-test GPREDs and GPOSTs. Post–only = participants provided (item and source) GPOSTs only. Expectancy conviction = participants were manipulated to belief in an expectancy effect. Inconsistency conviction = participants were manipulated to belief in an inconsistency effect. Error bars denote 95% Bayesian credibility intervals.

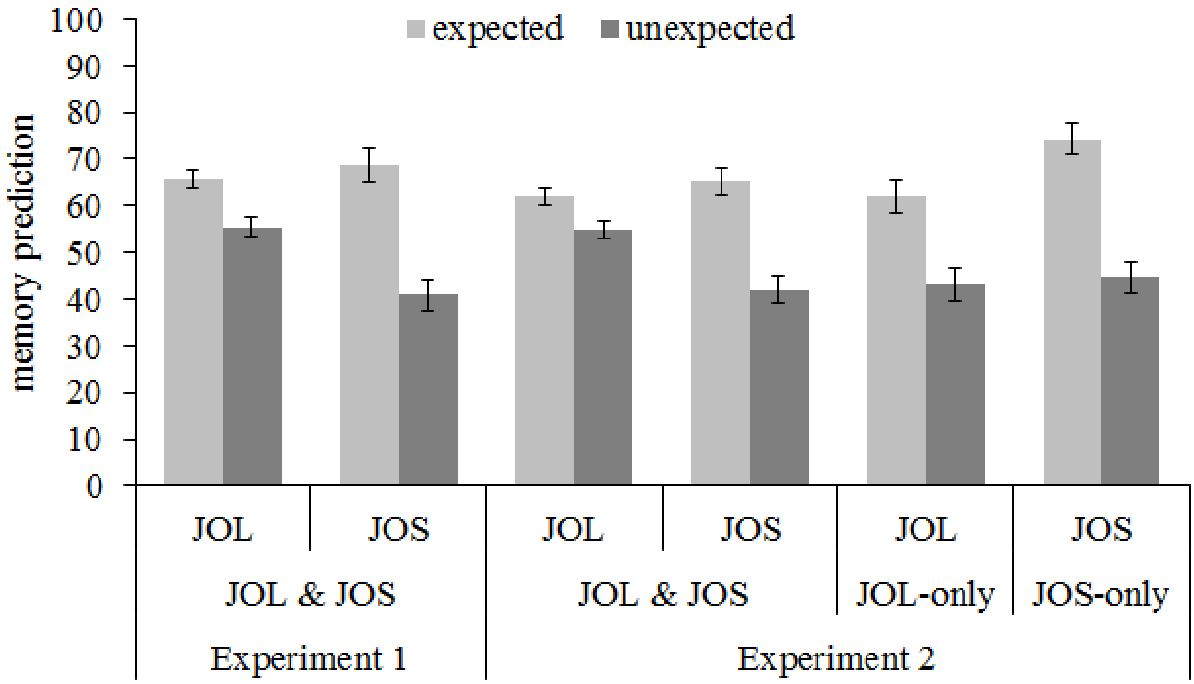

Figure 3. Judgment of Learning (JOL) and Judgment of Source (JOS) as a function of source–item expectancy and judgment group in Experiments 1 and 2. Judgments were percentage ratings ranging from 0 to 100. JOL & JOS = participants provided both JOLs and JOSs, JOL-only = participants provided JOLs only, JOS-only = participants provided JOSs only. Error bars denote within-subjects 95% confidence intervals (Loftus & Masson, 1994).

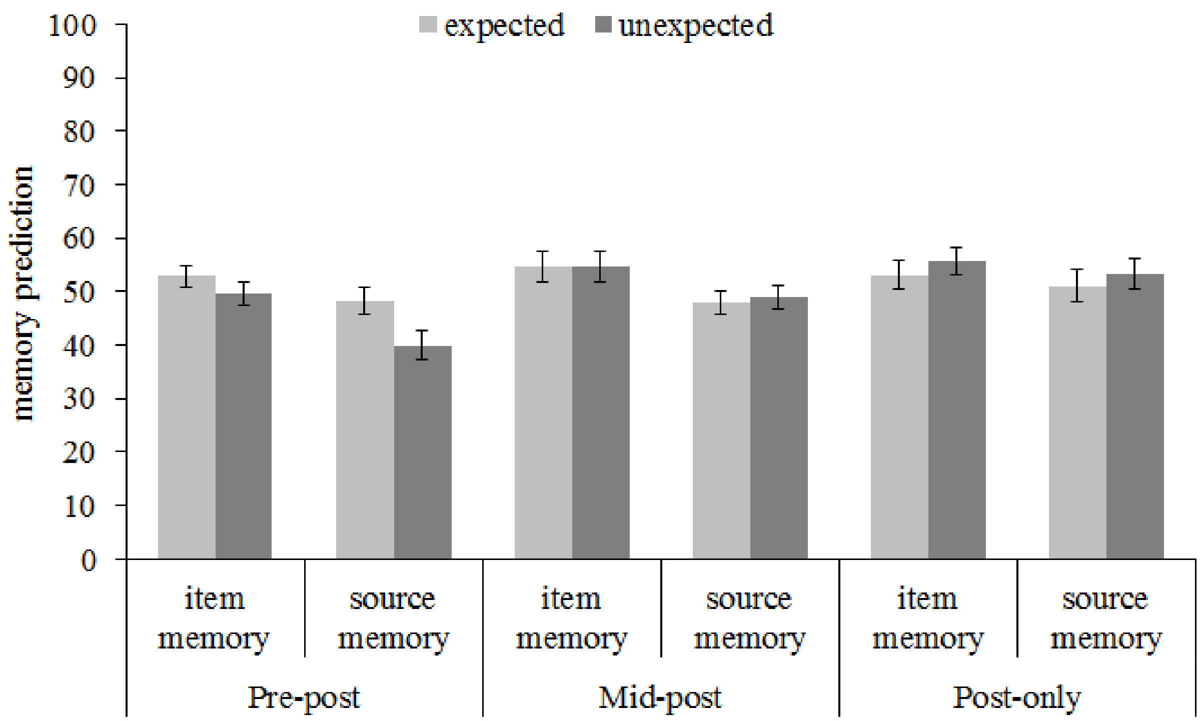

Figure 4. Global differentiated predictions (GPREDs) and global differentiated postdictions (GPOSTs) as a function of judgment timing and source–item expectancy in Experiment 3. Pre–post = participants provided (item and source) pre-test GPREDs and GPOSTs. Mid–post = participants provided (item and source) mid-test GPREDs and GPOSTs. Post–only = participants provided (item and source) GPOSTs only. Error bars denote within-subjects 95% confidence intervals (Loftus & Masson, 1994).

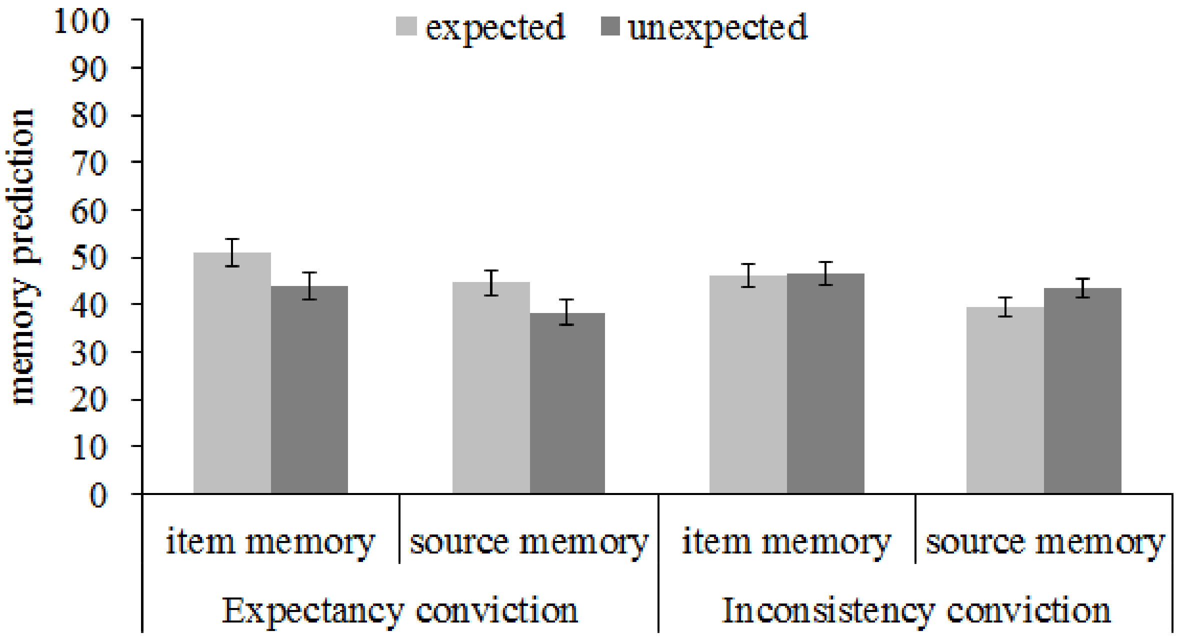

Figure 5. Pre-test global differentiated predictions (GPREDs) as a function of manipulated conviction and source–item expectancy in Experiment 4. Expectancy conviction = participants were manipulated to belief in an expectancy effect. Inconsistency conviction = participants were manipulated to belief in an inconsistency effect. Error bars denote within-subjects 95% confidence intervals (Loftus & Masson, 1994).

Appendix

Table A1

Correlations of Item-Memory and Source-Memory Judgments for Expected and Unexpected Source–Item Pairs in All Experiments

			Source–Item Expectancy	
Experiment	Group	Time of Judgment	expected	unexpected
1	Post–only	post test	.76	.62
	JOL & JOS	during study	.79	.76
		post test	.89	.81
2	JOL & JOS	during study	.60	.46
		post test	.54	.81
	JOL-only	post test	.53	.46
	JOS-only	post test	.77	.77
3	Pre–post	pre test	.74	.68
		post test	.75	.80
	Mid–post	mid test	.61	.65
		post test	.75	.78
	Post–only	post test	.74	.84
4	Expectancy conviction	pre test	.79	.83
		post test	.91	.87
	Inconsistency conviction	pre test	.71	.79
		post test	.76	.77

Note. Judgments were percentage ratings ranging from 0 to 100. JOL & JOS = participants provided Judgments of Learning (JOLs) and Judgments of Source (JOSs), JOL-only = participants provided only JOLs, JOS-only = participants provided only JOSs. Pre–post = participants provided (item and source) pre-test global differentiated predictions (GPREDs) and global differentiated postdictions (GPOSTs). Mid–post = participants provided (item and source) mid-test GPREDs and GPOSTs. Post–only = participants provided (item and source) GPOSTs only. Expectancy conviction = participants were manipulated to believe in an expectancy effect. Inconsistency conviction = participants were manipulated to believe in an inconsistency effect. All correlations were significant, all $p < .01$.

Metacognitive Expectancy Effects in Source Monitoring:

Beliefs, In-The-Moment Experiences, or Both?

Marie Luisa Schaper

Heinrich-Heine-Universität Düsseldorf

Beatrice G. Kuhlmann

University of Mannheim

Ute J. Bayen

Heinrich-Heine-Universität Düsseldorf

Author Note

Marie Luisa Schaper and Ute Johanna Bayen, Institute for Experimental Psychology, Heinrich-Heine-Universität Düsseldorf, Germany. Beatrice G. Kuhlmann, Department of Psychology, School of Social Sciences, University of Mannheim, Germany.

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Correspondence concerning this article should be addressed to Ute J. Bayen, Institut für Experimentelle Psychologie, Gebäude 23.02, Universitätsstr. 1, Heinrich-Heine-Universität Düsseldorf, D-40225 Düsseldorf, Germany. Email: ubayen@hhu.de

Abstract

In metamemory research, a-priori beliefs and in-the-moment experiences are controversially discussed as determinants of metamemory judgments, such as item-memory predictions (Judgments of Learning, JOLs). Both have been found to contribute to JOLs, but their interplay and relative importance have not yet been investigated. Also, prior research mostly focused on JOLs. To understand beliefs and experiences as metamemory determinants more generally, we investigated both JOLs and source-memory predictions (Judgments of Source, JOSs) in a source-monitoring paradigm with schematically expected source–item pairs (e.g., oven in the kitchen) and unexpected pairs (e.g., refrigerator in the bathroom). In this paradigm, JOLs and JOSs are typically higher for expected than unexpected source–item pairs (expectancy effect, Schaper, Kuhlmann, & Bayen, under review). In three experiments, the authors investigated the independent contributions of a-priori beliefs and in-the-moment experiences to this expectancy effect on JOLs and JOSs. Experiment 1 revealed that a-priori beliefs regarding item memory and source memory for expected and unexpected source–item pairs were heterogeneous and only showed a small expectancy effect. In Experiments 2 and 3, these a-priori beliefs moderated, but did not fully account for, the expectancy effect on JOLs and JOSs. In Experiment 3, in-the-moment experiences measured via self-paced study time mediated the expectancy effect on JOLs. Beliefs and experiences were weighed differently in JOL versus JOS formation. The experiments highlight the importance of investigating different processes underlying metamemory and their contributions to different judgments in order to gain a comprehensive understanding of metamemory predictions.

Keywords: metamemory, source monitoring, schemas, in-the-moment experiences, beliefs

Metacognitive Expectancy Effects in Source Monitoring: Beliefs, In-The-Moment Experiences, or Both?

Assessing one's own memory ability is an important aspect of metacognition. In making predictions about their own memory, people use cues that may or may not be predictive of later memory (e.g., Koriat, 1997). Recent research has controversially discussed how such cues influence memory predictions, that is whether people hold a-priori beliefs about effects of cues on memory or whether cues trigger in-the-moment experiences. A-priori beliefs are naïve theories people hold about how cues influence memory and have been shown to contribute to metamemory judgments (Frank & Kuhlmann, 2017; Mueller, Dunlosky, & Tauber, 2016; Mueller, Dunlosky, Tauber, & Rhodes, 2014; Mueller, Tauber, & Dunlosky, 2013). Conversely, in-the-moment experiences during study, such as the fluency of processing, also contribute to metamemory judgments (Undorf & Erdfelder, 2011, 2013, 2015). However, the interplay and relative importance of these two factors has not been studied yet. Also, research has thus far mostly focused on item-memory predictions (e.g., Judgments of Learning, JOLs, Rhodes, 2016). However, in order to arrive at a comprehensive theory of metamemory, other types of judgment must be considered and possible differences must be determined. To broaden the understanding of metamemory formation we obtained both JOLs and source-memory judgments (Judgments of Source, JOSs, e.g., Carroll, Mazzoni, Andrews, & Pocock, 1999) in a source-monitoring paradigm with schematically expected and unexpected sources. Thereby, we examined schematic expectancy as a cue for item-memory and source-memory predictions. While schematically unexpected information enhances source memory, our past research (Schaper, Kuhlmann, & Bayen, under review, Experiments 1 and 2) shows that JOSs do not accurately reflect this effect. Instead, people assume a source-memory advantage for expected sources. We investigated whether a-priori beliefs, or in-the-moment experiences, or both contribute to this illusory expectancy effect on JOLs and JOSs.

In the following paragraphs, we will first review the literature on the influence of schemas on source memory and metamemory. We will then discuss a-priori beliefs and in-the-moment experiences as determinants of JOLs and derive hypotheses for memory judgments in the source-monitoring paradigm.

Effects of Schemas on Item- and Source-(Meta)Memory

Source monitoring refers to the mental processes involved in attributing information to its origin (Johnson, Hashtroudi, & Lindsay, 1993). Sources can be people, presentation media, scenes, the color or font of information, study lists, and so forth. Source-monitoring processes are known to be influenced by schemas, which are organized knowledge structures about aspects of the world (Alba & Hasher, 1983). In the laboratory, the effects of schematic knowledge on both item memory and source memory are often investigated with the following paradigm. During a study phase, information is presented with a source for which it is either schematically expected or unexpected (Arnold, Bayen, Kuhlmann, & Vaterrodt, 2013; Bayen & Kuhlmann, 2011; Bayen, Nakamura, Dupuis, & Yang, 2000; Bell, Buchner, Kroneisen, & Giang, 2012; Dodson, Darragh, & Williams, 2008; Ehrenberg & Klauer, 2005; Hicks & Cockman, 2003; Kroneisen & Bell, 2013; Kroneisen, Woehe, & Rausch, 2015; Kuhlmann, Vaterrodt, & Bayen, 2012; Küppers & Bayen, 2014; Marsh, Cook, & Hicks, 2006; Mather, Johnson, & De Leonardis, 1999; Schaper et al, under review; Sherman & Bessenoff, 1999; Sherman, Lee, Bessenoff, & Frost, 1998; Spaniol & Bayen, 2002; for an overview, see Kuhlmann & Bayen, 2016). In some studies, for example, object labels (e.g., “frying pan”) were presented with either a scene (or scene label) in which the object is expected (e.g., “kitchen”) or a scene in which the object is unexpected (e.g., “bathroom”, Bayen et al., 2000; Küppers & Bayen, 2014; Schaper et al., under review). In the ensuing test phase, items from the study phase and new items were presented and participants had to decide whether each test item had been presented with the scene source in which it was expected, or with the scene source in which it was unexpected, or whether it was a new item. Thus, with this paradigm, item recognition and source memory are simultaneously tested. Research employing (variants of) this paradigm showed better source memory for unexpected than expected items (Bell et al., 2012; Ehrenberg & Klauer, 2005; Kroneisen & Bell, 2013; Kroneisen et al., 2015; Küppers & Bayen, 2014; Schaper et al., under review), whereas item memory in this paradigm seems to be largely unaffected by schematic expectations.[1] Ehrenberg and Klauer (2005) explain this *inconsistency effect* on source memory but not item memory with the fact that it is specifically the source that renders an item expected or unexpected. For example, a frying pan is not *per se* unexpected, but seeing it in the bathroom is.

We will now turn to metacognitive judgments of item memory and source memory in this paradigm. There are two different measures to assess metamemory in the source-monitoring paradigm. JOLs (e.g., Rhodes, 2016) are used to assess metacognition about item memory during study. In a JOL after each source–item presentation, participants predict the probability of remembering the item at test. JOSs (e.g., Carroll et al., 1999), on the other hand, are used to assess metacognition about source memory during study. In a JOS after each source–item presentation, participants predict the probability of later remembering the source of a particular item, assuming that the item itself is remembered.

In the described source-monitoring paradigm with schematic material, an expectancy effect was found on both JOLs (Konopka & Benjamin, 2009; Schaper et al., under review; Shi, Tang, & Liu, 2012) and JOSs (Schaper et al., under review). That is, people predict their item memory and their source memory to be better for expected source–item pairs than for unexpected source–item pairs. Notably, Schaper et al. (under review) who assessed separate JOLs and JOSs from participants found that the expectancy effect was more pronounced on JOSs than on JOLs (regardless of whether the judgments were assessed within or between participants), indicating that, to some extent, people adjust their predictions to the type of memory.

The expectancy effect on metamemory judgments in the source-monitoring paradigm concurs with the relatedness effect on JOLs in the paired-associates paradigm (Hertzog, Kidder, Powell-Moman, & Dunlosky, 2002; Mueller et al., 2013; Rabinowitz, Ackerman, Craik, & Hinchley, 1982; Undorf & Erdfelder, 2011, 2013, 2015). When people are asked to predict their memory on a cued-recall test, they predict better memory for conceptually related word pairs (e.g., dog – cat) than for unrelated word pairs (e.g., dog – spoon). While there is a large body of research demonstrating the relatedness effect on JOLs, there is little research on the effect of source–item relatedness on JOSs. Only one study assessed separate JOLs and JOSs (Schaper et al., under review), and the determinants of the expectancy effect on JOSs are as yet unknown.

Notably, in our prior study (Schaper et al., under review, Experiments 1 and 2), we found memory and metamemory to be dissociated in source monitoring. Specifically, we found a single dissociation of item memory from JOLs: There was (as usually found) no effect of schemas on item memory, whereas JOLs showed an expectancy effect. There was even a double dissociation of source memory from JOSs: There was an inconsistency effect on source memory (replicating many studies, see above), whereas JOSs showed an expectancy effect. Thus, people are unable to accurately predict the effects of schemas on item memory and source memory.

In the current research, we investigated determinants of the expectancy effects on JOLs and JOSs and differences between these judgments. We thus sought to explain why people mistakenly predict an expectancy effect on both item memory and source memory. In the following paragraphs, we review the literature on possible determinants of the illusion.

Beliefs and Experiences as Determinants of Metamemory

When predicting memory (with past research primarily focusing on item memory predictions such as JOLs) people rely on cues that are (sometimes) predictive of memory (Kelley & Jacoby, 1996; Koriat, 1997). Specifically, Koriat's cue-utilization framework distinguishes three types of cues: 1) Intrinsic cues involve item characteristics (e.g., semantic relatedness of words in a pair). 2) Extrinsic cues involve characteristics of the study conditions (e.g., list length) and the learner's encoding processes (e.g., forming a mental image). 3) Mnenomic cues involve internal experiences (e.g., the ease with which an item is processed at encoding). Importantly, according to Koriat's framework, intrinsic, and extrinsic cues may influence metamemory judgments either directly via a-priori beliefs that the person holds about the influence of the cue, or indirectly, via mnenomic experiences in the moment of studying. For example, applied to the relatedness effect on JOLs (which as mentioned above is close to the expectancy effect of interest in the present study) this framework suggests that the effect may be due to people's a-priori beliefs that memory is better for related word pairs and/or due to in-the-moment experiences elicited by related word pairs. There is an ongoing debate about whether the relatedness effect on JOLs is primarily due to beliefs or due to experiences.

On one side of the debate, Mueller et al. (2013) showed that people hold the a-priori belief that word relatedness in a paired-associates paradigm positively influences memory. According to the authors, people apply this belief when making JOLs for word pairs. We think that people may hold similar a-priori beliefs regarding source–item relatedness in the source-monitoring paradigm. That is, they may believe that item memory and source memory for schematically expected source–item pairs will be better than for unexpected source–item pairs. Such a belief would explain the expectancy effect on JOLs and JOSs. At this point, it is not known whether people generally hold this belief and, if so, whether it is specific to item memory (i.e., can only explain the expectancy effect on JOLs) or further generalizes to beliefs about source memory (i.e., can explain the expectancy effect on JOSs as well). Crucially, Mueller et al. only showed that people hold a priori beliefs about relatedness and item memory but did not assess whether or not these beliefs *directly* contribute to item-level JOLs (cf. Frank & Kuhlmann, 2017).

On the other side of the debate, it has been suggested that in the paired-associates paradigm, in-the-moment experiences contribute to JOLs (Alter & Oppenheimer, 2009; Begg, Duft, Lalonde, Melnick, & Sanvito, 1989; Jacoby, Woloshyn, & Kelley, 1989; Kelley & Jacoby, 1996; Koriat, 1997; Koriat & Ma'ayan, 2005; Undorf & Erdfelder, 2011, 2013, 2015). According to this account, people base their metamemory judgments on cues they experience at the very moment when they study the material. One example for a cue that may arise in the moment is processing fluency. Processing fluency refers to a subjective ease with which items are processed during study (for a review, see Alter & Oppenheimer, 2009). In the paired-associates paradigm, related word pairs are presumably processed more fluently than unrelated pairs. According to this account, people use this fluent processing as a cue to predict better memory for the related pairs (e.g., Undorf & Erdfelder, 2011, 2013, 2015). Similarly, in the source-monitoring paradigm, expected source–item pairs may be experienced as being more fluently processed than unexpected source–item pairs. Therefore, memory should be predicted to be better for expected pairs.

As it stands, there is an ongoing debate about whether beliefs, or in-the-moment experiences, or both contribute to the relatedness effect on JOLs in the paired-associates paradigm, and to other effects on metacognitive judgments in general. Some research suggests that a-priori beliefs alone can explain a multitude of effects on JOLs (Mueller et al., 2013, 2014, 2016). Additionally, beliefs that are manipulated at the beginning of the experiment influence

JOLs (Mueller & Dunlosky, 2017). However, other research suggests that a-priori beliefs and in-the-moment experiences both contribute to these effects (Frank & Kuhlmann, 2017; Kelley & Jacoby, 1996; Koriat, 1997; Undorf & Erdfelder, 2011, 2013, 2015). In the experiments reported here, we determined the contributions of both a-priori beliefs and in-the-moment experiences on the formation of metacognitive judgments in a source-monitoring paradigm. We investigated whether one factor alone can fully, or mostly, account for the expectancy effect on JOLs and JOSs, or whether both factors make independent contributions.

Additionally, we sought to investigate possible differences in the weighting of these factors in JOL versus JOS formation. A comprehensive theory of the formation of metamemory judgments must account for different types of memory judgments, not just item-memory predictions. While the factors underlying cue effects in JOL formation have been controversially discussed in the literature, there is little research concerning JOSs. It may well be the case that different factors or a differential weighting of factors contribute to JOSs versus JOLs. Although JOLs and JOSs are usually correlated (Carroll, Davis, & Conway, 2001; Carroll et al., 1999; Dutton & Carroll, 2001; Kelly, Carroll, & Mazzoni, 2002), they diverged in one prior study (Kelly et al., 2002). Additionally, our own research on the influence of schemas on JOLs and JOSs showed that people differentiate between item-memory and source-memory predictions (Schaper et al., under review, Experiments 1 and 2) as indicated by a stronger expectancy effect on JOSs than JOLs. Thus, there is first evidence for separate metacognitive assessment of item memory and source memory, despite the superficial similarity between JOLs and JOSs. If this is the case, JOLs and JOSs may rely on different factors and may be differently affected by them. In the experiments reported here, we thus investigated and compared the use of different factors in JOL and JOS formation.

Overview of the Experiments

In three experiments, we investigated a-priori beliefs and in-the-moment experiences as determinants of the expectancy effects on JOLs and JOSs in a source-monitoring paradigm with schematic materials. Our main objectives were (1) to determine a-priori beliefs about schema effects on item-memory and source-memory (Experiment 1), (2) to determine and compare the effects of these a-priori beliefs on JOLs and JOSs (Experiments 2 and 3), and (3) to determine and compare the independent contributions of both beliefs and in-the-moment experiences, as well as their possible interplay, on JOLs and JOSs (Experiment 3).

In Experiment 1, we conducted an online survey in which we described a source-monitoring experiment with schema-relevant materials. To measure a-priori beliefs, we asked participants for memory predictions for expected versus unexpected source–item pairs in the described experiment. We found a small overall a-priori expectancy effect in beliefs, but participants held quite heterogeneous beliefs.

If a priori-beliefs account for the expectancy effect on metamemory judgments, then these beliefs should moderate the expectancy effect on JOLs and JOSs. However, because some research suggests that beliefs do not fully account for effects on JOLs (e.g., Frank & Kuhlmann, 2017; Undorf & Erdfelder, 2011, 2013, 2015), we predicted a-priori beliefs to only partially moderate the expectancy effect on JOLs and JOSs. To test this hypothesis, we conducted two source-monitoring experiments (Experiments 2 and 3) with schematically expected and unexpected source–item pairs. Prior to study, we asked participants about their a-priori beliefs regarding their item memory and source memory for expected versus unexpected source–item pairs. We then related these beliefs to item-wise JOLs (cf. Frank & Kuhlmann, 2017) and JOSs obtained during study.

Presumably, the expectancy effect on JOLs and JOSs not only depends on a-priori beliefs, but also on in-the-moment experiences. In particular, in-the-moment experiences should mediate the expectancy effects on JOLs (e.g., Undorf & Erdfelder, 2015) and JOSs, but not account for them completely. We thus designed Experiment 3 to additionally determine the effects of in-the-moment experiences on JOLs and JOSs. To our knowledge, we are the first to assess both factors in the same experiment. As study time has been proposed as a measure of in-the-moment experiences (cf. Undorf & Erdfelder, 2015) study time was self-paced in this experiment. We predicted longer study times for unexpected than for expected source–item pairs, because processing unexpected source–item pairs should be experienced as less fluent compared to expected pairs source–item (e.g., Undorf & Erdfelder, 2015). We predicted that study time would mediate the expectancy effect on metamemory judgments, whereas beliefs would moderate the effect. Both beliefs and experiences should thus jointly contribute to the expectancy effect. Based on our prior research demonstrating a stronger expectancy effect on JOSs than on JOLs (Schaper et al., under review) we deemed it possible that beliefs and experiences would play different roles in JOL versus JOS formation.

Experiment 1

The main goal of Experiment 1 was to determine a-priori beliefs concerning item memory and source memory for expected and unexpected source–item pairs. Following Mueller et al. (2014), we conducted an online study in which we explained the source-monitoring paradigm with schema-related materials. Then we asked participants to predict item memory and source memory in this paradigm. Based on the demonstrated a-priori relatedness belief (Mueller et al., 2013) we predicted an expectancy effect on a-priori beliefs. That is, we predicted participants to believe that memory is better when source–item pairs are expected rather than unexpected. If a-priori beliefs do not on average show this expectancy effect, they cannot fully account for the expectancy effect on JOLs and JOSs.

Additionally, we investigated whether a-priori beliefs differentiate between item memory and source memory. As mentioned, our previous research (Schaper et al., under review) on item-wise JOLs and JOSs indicated that predictions for item memory and source memory differed during study (showing a stronger expectancy effect on JOSs). It is unknown if prior to study, people already hold different beliefs concerning item memory and source memory.

Our prior research further showed that item-memory and source-memory predictions may differ depending on whether participants provide both or only one of them (Schaper et al., under review, Experiment 2). We therefore randomly assigned participants to three groups: In the first group, we asked for both item-memory and source-memory predictions (item & source group); in the second group, we only asked for item-memory predictions (item-only group), and in the third group, we only asked for source-memory predictions (source-only group). This allowed us to capture possible differences in beliefs when asked about both or only one memory type.

Method

Participants and Design. The Research Ethics Committee of the Faculty of Mathematical and Natural Sciences of the Heinrich-Heine-Universität Düsseldorf declared the experiments exempt from ethics review. We recruited 108 native speakers of German (27 male, 75 female, 6 other) via social networks. Participants were randomly and evenly assigned to the three judgment groups (36 each). A group sample size of 34 would be sufficient to detect a within-subjects effect size of $d_z = 0.5$ with a power of .80 (with $\alpha = .05$, calculated via G*Power 3, Faul, Erdfelder, Lang, & Buchner, 2007). We recruited 36 participants per group to obtain a perfectly counterbalanced design. Age ranged between 18 and 32 years ($M = 22.54$, $SE = 0.29$). The design was a 3 × 2 mixed-factorial with the between-subjects factor judgment group (item &

source, item-only, source-only) and the within-subjects factor source–item expectancy (expected vs. unexpected).

Material and Procedure. The instructions and materials for all experiments were in German. Experiment 1 was conducted online. After consenting, participants were asked to read the description of a previous experiment (Schaper et al., under review). The description read as follows (translated from German):

> In the prior study, participants saw 64 words one at a time (for 4 seconds each) on the computer screen. The words referred to objects that are either particularly expected for a kitchen or particularly expected for a bathroom. There were 32 kitchen objects and 32 bathroom objects. Alongside each object, a room (kitchen or bathroom) in which it was located was also presented:
>
> - Only 50% of all objects were, however, presented with the room **in which they are typically located**. That is, there were 16 kitchen objects in the kitchen, and 16 bathroom objects in the bathroom.
> - The other 50% of the objects were presented with the room **in which they are typically NOT located**. That is, the remaining 16 kitchen objects were in the bathroom, and the remaining 16 bathroom objects were in the kitchen.
>
> Next, a memory test followed. Participants were again shown the objects from the first part and additionally new (not shown in the first part) objects in random order. They were asked to remember for each object whether or not it had been presented in the first part, and, if it had been, in which room it had been presented.

Participants were then asked to predict the results of the described experiment (following Mueller et al., 2014). In the item-only group, participants were asked to judge the former participants' item memory. They were asked to predict the number of expected/unexpected objects the former participants had correctly remembered in the memory test. In the source-only group, participants were asked to predict for which number of objects the former participants had correctly remembered the room source (assuming that they had remembered all 32 objects). In the item & source group, participants were asked to first judge the former participants' item memory and then to judge the former participants' source memory for expected/unexpected objects. Within each memory type, the order of the judgments regarding expected versus unexpected trials was counterbalanced. Participants were required to type in a number between 0 and 32 after each question.

All participants were then presented a summary of their answers (i.e., that they indicated that they thought item memory or source memory had been better for expected or unexpected source–item pairs, or equal). They were asked to explain their reasoning for their predictions by typing in their answers. Lastly, participants provided demographic information. They were debriefed and could participate in a raffle to win one of four 20€ gift cards.

Results and Discussion

For all analyses, we set alpha to .05. We converted responses (between 0 and 32) to percentages. Descriptive statistics are in Table 1. For the item & source group, we analyzed memory judgments with a 2 × 2 ANOVA with the within-subjects factors source–item expectancy (expected vs. unexpected) and judgment type (item memory vs. source memory). Participants predicted better memory for expected versus unexpected source–item pairs, $F(1, 35) = 4.93$, $p = .033$, $\eta_p^2 = .12$. This expectancy effect must be belief-driven because no source–item pairs were presented (i.e., experienced). This expectancy effect was rather small in terms of Cohen's d (JOLs: $d_z = 0.33$, JOSs: $d_z = 0.34$). There was no effect of judgment type, $F(1, 35) = 1.74$, $p = .196$, and no interaction, $F(1, 35) = 0.01$, $p = .912$. Neither for the item-only group nor for the source-only group was there a difference in predictions for expected versus unexpected pairs, $t(35) = 0.55$, $p = .583$, and $t(35) = 0.07$, $p = .947$, respectively.

We compared the item-memory predictions in the item & source group and the item-only group in a 2 × 2 ANOVA with the within-subjects factor source–item expectancy and the between-subjects factor judgment group (item & source vs. item-only). None of the effects were significant, all $F \leq 3.24$, $p \geq .076$. In particular, there was no difference in the effect of source–item expectancy on item-memory judgments between groups.

Likewise, we compared the source-memory predictions from the item & source group and the source-only group in a 2 × 2 ANOVA with the within-subjects factor source–item expectancy and the between-subjects factor judgment group (item & source vs. source-only). None of the effects were significant, all $F \leq 2.50$, $p \geq .118$. In particular, there was no difference in the effect of source–item expectancy on source-memory judgments between groups.

Examined on an individual level, a-priori beliefs of the participants were quite mixed. Of those participants who provided item-memory judgments, 50.00% showed an expectancy effect (i.e., higher item-memory judgments for expected than unexpected source–item pairs), 2.78% did not show an effect, and 47.22% showed an inconsistency effect (i.e., higher item-memory judgments for unexpected than expected source–item pairs). Of those participants who provided source-memory judgments, 47.22% showed an expectancy effect, 2.78% did not show an effect, and 50.00% showed an inconsistency effect.

Thus, a-priori beliefs about the influence of source–item expectancy on item memory and source memory did not correspond with the strong expectancy effect found on item-wise JOLs and JOSs in previous studies (Konopka & Benjamin, 2009; Schaper et al., under review; Shi et al., 2012). Although there was overall a significant expectancy effect in the item & source group across item and source memory predictions, the single predictions of either item or source memory in the other groups did not show this effect, indicating that the true effect may be rather small. Additionally, in contrast to item-wise judgments in prior research (Schaper et al., under review), a-priori beliefs did not differ between item memory and source memory (as indicated by the non-significant interaction in the item & source group). Notably, and again unlike our research on item-wise JOLs and JOSs, providing single versus dual judgments did not appear to impact item-memory and source-memory judgments (although this may be due to low power of these between-group tests).

These results suggest that beliefs cannot fully account for the expectancy effects on item-wise JOLs and JOSs found in prior studies (Konopka & Benjamin, 2009; Schaper et al., under review; Shi et al., 2012). Nonetheless, a-priori beliefs may partially contribute to the expectancy effect on item-wise JOLs and JOSs, at least for some participants (i.e., moderate this effect; Frank & Kuhlmann, 2017). Thus, people may, to some extent, rely on an invalid belief about the effect of schemas on memory in judgment formation which may explain part of the illusory expectancy effect on JOLs and JOSs. We addressed this possibility in Experiment 2.

Experiment 2

In Experiment 1, a-priori beliefs did not show a homogeneous expectancy effect. There was a substantial proportion of participants who believed in an inconsistency effect challenging the notion that a-priori beliefs fully account for effects on item-level metamemory judgments (cf. Mueller et al., 2013, 2014, 2016). In Experiment 2, we addressed the question whether a-priori memory judgments may nonetheless partially explain the illusory expectancy effect on item-wise JOLs and JOSs. In particular, we hypothesized that a-priori beliefs should moderate the expectancy effect. That is, participants who showed an expectancy effect a priori should show a stronger expectancy effect on JOLs and JOSs than participants who showed an inconsistency effect a-priori. Mueller et al. (2013) argued that the relatedness effect on JOLs in the paired-associates paradigm is mostly explained by a-priori beliefs. Undorf and Erdfelder (2011, 2013, 2015), however, proposed that both a-priori beliefs and in-the-moment experiences such as processing fluency contribute to the relatedness effect on JOLs. JOSs, on the other hand, have never been investigated with regard to this question. Due to the differences between JOLs and JOSs demonstrated in our prior research (Schaper et al., under review) we deemed it possible that a moderating effect of a-priori beliefs on the expectancy effect might also differ between JOLs and JOSs. Thus, we assessed a-priori beliefs about both item memory and source memory from all participants in Experiment 2 (similar to the item & source group in Experiment 1).

The heterogeneity of a-priori (item- and source-) memory beliefs across participants in Experiment 1 is also notable for methodological reasons. Past research has mostly focused on effects for which beliefs were quite homogeneous and in accordance with item-wise JOLs (e.g., the volume effect, Frank & Kuhlmann, 2017, and the font-size effect, Mueller et al., 2014). The heterogeneity of beliefs regarding the current schema manipulation found in Experiment 1 allowed us to examine the impact of a broader range of a-priori beliefs on item-wise metamemory judgments.

To test whether the expectancy effect on JOLs and JOSs is moderated by a-priori beliefs we conducted a source-monitoring experiment in which we presented expected and unexpected source–item pairs at study. Participants predicted their item memory and source memory prior to and during study (item-wise). We investigated whether a-priori item-memory and source-memory beliefs moderated expectancy effects on item-wise JOLs and JOSs using a mixed linear regression model (following Frank & Kuhlmann, 2017).

Method

Participants. We recruited 96 native speakers of German (35 male, 61 female) at Heinrich-Heine-Universität Düsseldorf. We increased the sample size in comparison to the group sample size in Experiment 1 to obtain a sufficient number of participants who a priori believed in either an inconsistency effect or an expectancy effect. In Schaper et al. (under review), the smallest expectancy effect on item-wise judgments had an effect size of $d_z = 0.65$. An a-priori power analysis showed that 21 participants would be sufficient to detect this effect with a power of $1 - \beta = .80$ (with $\alpha = .05$). We recruited participants until we had found at least 21 participants for both belief categories (expectancy belief vs. inconsistency belief) for both a-priori item-memory and source-memory beliefs. We then continued data collection until all control variables in the experiment were perfectly counterbalanced (which required a number divisible by 12). Age ranged between 17 and 34 years ($M = 22.09$, $SE = 0.36$). Participants received course credit or monetary compensation.

Design and Material. We investigated the influence of the within-subjects factor source–item expectancy (expected vs. unexpected) and of measured a-priori beliefs on JOLs and JOSs in a laboratory experiment. We used the same materials and counterbalancing scheme as Schaper et al. (under review). That is, we selected items that were highly expected for one room and highly unexpected for the other room based on the results of a prior norming study. In this norming study, we randomly split 1004 German labels of objects that can either be found in a bathroom or a kitchen into two lists. Two-hundred and one participants (49 male, 152 female; age ranging from 18 to 35 years, $M = 22.50$, $SE = 0.25$) each rated the objects of one of these lists regarding their expectation of occurrence in a bathroom or a kitchen on a scale from 1 (*very unexpected*) to 5 (*very expected*). Assignment of lists to bathroom or kitchen ratings was approximately counterbalanced, and participants were randomly assigned to the counterbalanced conditions. Based on the participants' responses we selected 96 items that received high expectancy ratings for one room (mean rating of 4.00 or higher) and low expectancy ratings for the other room (mean expectancy ratings of 1.30 or lower; cf. Küppers & Bayen, 2014). The mean expectancy rating for bathroom-expected items was 4.49 (*SE* = 0.04) for bathroom and 1.11 (*SE* = 0.01) for kitchen. The mean expectancy rating for kitchen-expected items was 4.49 (*SE* = 0.04) for kitchen and 1.10 (*SE* = 0.01) for bathroom. Bathroom-expected items and kitchen-expected items did not differ in expectancy ratings, number of syllables, nor word frequency (according to norms from the University of Leipzig, 1998).

For the laboratory experiment, we split the 96 selected items into three lists with 16 kitchen items and 16 bathroom items each. Mean expectancy ratings, number of syllables, and word frequency did not differ between lists. Participants studied two of the lists. Items from one study list were presented with the bathroom source, and items from the other study list were presented with the kitchen source. Items from the third list were presented as distractors during test. Assignment of lists to conditions was counterbalanced across participants. Each study list thus contained 16 bathroom items and 16 kitchen items and participants studied equal numbers of expected and unexpected source–item pairs resulting in 32 items being presented with their expected source and 32 items being presented with their unexpected source.

Procedure. Up to five participants at a time were tested in individual computer booths. After consent, participants received computerized instructions for the upcoming study phase, that is, they were told that they would be presented object labels alongside rooms. They were also informed of the subsequent source-monitoring test. Before study, they were asked to provide a-priori judgments about their item memory and source memory separately for expected versus unexpected source–item pairs on a scale from 0% (*will not remember anything*) to 100% (*will remember everything*). They were informed that some objects would be expected for the room in which they would be located, and that some objects would be unexpected for the room in which they would be located. Then, they answered four questions of which the first two referred to item memory and the other two referred to source memory. They predicted what percentage of the expected/unexpected objects they would correctly remember in the memory test (item-memory judgments), then for what percentage of the expected/unexpected objects they would correctly remember the room (source-memory judgments). As in Experiment 1, participants were instructed to make their source-memory predictions assuming that they would remember all items. The order of the questions regarding expected versus unexpected objects was counterbalanced within memory type. Participants typed in their answer using number keys on the computer keyboard.

The rest of the experiment was identical to the JOL & JOS conditions of Schaper et al. (under review). After each study trial, participants provided item-wise memory judgments of their likelihood of later remembering the item (JOL) and its source (JOS). For JOSs, they were instructed to predict their likelihood of remembering the source assuming perfect memory for the item.

Participants were then presented two of the item lists. Items were printed above their respective room source (“in the BATHROOM” or “in the KITCHEN”). Items were presented for 4 s each in random order with the restriction of no more than four consecutive trials with the same source. Items (in standard German capitalization) and room labels (in all capital letters) were printed in white letters on black background. Four items that were equally expected for a bathroom and a kitchen were presented as primacy buffers, half of them with each source.

After each source–item pair, participants judged their perceived likelihood of remembering this item (JOL) and then its source (JOS) in the upcoming test on a scale from 0% (*definitely will not remember*) to 100% (*definitely will remember*). Judgments were provided via the number keys and were self-paced. During judgments, items and sources were not visible. JOLs and JOSs were made more discriminable via different background screen colors (randomized blue or yellow). Between judgments, there was a blank black screen for 100 ms. After each complete trial, there was a blank black screen for 600 ms.

Participants were then instructed for the source-monitoring test. They first completed a practice test in which four equally expected items were presented (two of them from the primacy buffer and two new items, one of each with each source). Then, participants proceeded to the test phase in which both study lists and the distractor lists (96 items in total) were presented in a new random order. Items were again printed in white letters on black background. Participants were instructed to indicate for each item whether it had been presented with the bathroom, with the kitchen, or was new. Beneath each test item, two gray response boxes were presented with the labels "in the BATHROOM" and "in the KITCHEN" in black font. Across participants, assignment of the response options to the left and right box was counterbalanced. A third box was presented in the center beneath the other two labeled "was not presented". Participants indicated their answer by clicking on the respective box with the computer mouse.

After test, participants were asked to provide post-test judgments about their item memory and source memory separately for expected versus unexpected source–item pairs on a scale from 0% (*did not remember anything*) to 100% *(remembered everything*). They were asked the same four questions as in the a-priori memory judgments (in the same counterbalanced order), but were now asked to estimate the percentage they had correctly remembered instead of predicting it.[3] Again, they typed in their answers using number keys.

Participants then provided source–item contingency judgments (cf. Bayen & Kuhlmann, 2011). They were asked how many of the 32 bathroom-expected items had been presented with one source at study, and how many of the 32 kitchen-expected items had been presented with the respective other source at study. The order of these questions was counterbalanced as was the order of the judgments for the expected versus unexpected source. Participants received feedback of their numerical answer (e.g. “17 bathroom-expected items in the bathroom”) along with the remaining item count for the respective other source (e.g. “15 bathroom-expected items in the kitchen”). Participants confirmed their answers.

Lastly, participants provided demographic information and were asked about their belief concerning the study aim. They were then debriefed and compensated.

Results and Discussion

Memory, guessing, and contingency judgments. We analyzed responses in the source-monitoring test with the two-high threshold multinomial processing-tree model of source monitoring (Bayen et al., 1996). This model allows us to obtain separate and independent parameter estimates for item memory, source memory, and guessing biases. Parameter estimates, fit indices, and results of hypothesis tests are shown in Table 2. Technical details of the modeling are explained in Bayen et al. (1996). Results showed no schema effect on item memory but an inconsistency effect on source memory and schema-consistent source guessing, replicating prior results (Bell et al., 2012; Ehrenberg & Klauer, 2005; Küppers & Bayen, 2014; Schaper et al., under review). As also found in prior studies (Schaper et al., under review), we found that in their contingency judgments after test, participants assumed that more than half of the items had been presented with their expected source, $M = 34.88$, 95% CI [33.52, 36.22], $t(95) = 4.24$, $p < .001$, $d = 0.43$.

A-priori memory judgments. We analyzed the a-priori memory judgments with a 2 × 2 ANOVA with the within-subjects factors source–item expectancy (expected vs. unexpected) and judgment type (item memory vs. source memory). Descriptive statistics are in Table 1. Participants predicted better (item and source) memory for expected versus unexpected source–item pairs, $F(1, 95) = 20.25$, $p < .001$, $\eta_p^2 = .18$. This expectancy effect was small to medium sized in terms of Cohen’s d (JOLs: $d_z = 0.48$, JOSs: $d_z = 0.37$). Like in Experiment 1, the expectancy effect must be belief-driven, because at the time of these judgments, participants had not yet seen any items or sources. There was no effect of judgment type, $F(1, 95) = 2.06$, $p = .155$, and no interaction, $F(1, 95) = 0.14$, $p = .705$. Thus, there was, as in Experiment 1, no difference in a-priori beliefs regarding item memory and source memory.

As in Experiment 1, a-priori beliefs were quite heterogeneous. Regarding item memory, 58.33% of participants predicted an expectancy effect, 16.67% predicted no effect, and 25.00% predicted an inconsistency effect. Regarding source memory, 53.13% of participants predicted an expectancy effect, 16.67% predicted no effect, and 30.21% predicted an inconsistency effect.

Item-wise memory judgments (JOLs and JOSs). For each participant, we averaged item-wise JOLs as well as item-wise JOSs provided during study. We analyzed these item-wise memory judgments with a 2 × 2 ANOVA with the within-subjects factors source–item expectancy (expected vs. unexpected) and judgment type (JOL vs. JOS). Descriptive statistics are in Table 1. While studying, participants gave higher memory judgments for expected versus unexpected source–item pairs, $F(1, 95) = 78.76$, $p < .001$, $\eta_p^2 = .45$. There was also a main effect of judgment type, $F(1, 95) = 4.51$, $p = .036$, $\eta_p^2 = .05$, which was qualified by a significant two-way interaction, $F(1, 95) = 69.05$, $p < .001$, $\eta_p^2 = .42$. Paired t tests revealed a significant medium-sized expectancy effect on JOLs, $t(95) = 6.15$, $p < .001$, $d_z = 0.63$, and a large effect on JOSs, $t(95) = 9.44$, $p < .001$, $d_z = 0.96$. These findings replicate Schaper et al. (under review). Thus, in contrast to a-priori memory predictions, we found differences in item-wise JOLs versus JOSs. This shows that a-priori beliefs and item-wise predictions do not fully concur. Replicating Schaper et al. (under review), memory predictions and actual memory were dissociated; that is, participants predicted an expectancy effect on both item memory and source memory, but actually showed no effect on item memory, and an inconsistency effect on source memory.

In their JOLs, 66.67% of participants predicted an expectancy effect, 2.08% did not predict an effect, and 31.25% predicted an inconsistency effect. Of those participants who had a priori expressed an inconsistency belief in item memory, 54.17% showed an expectancy effect on JOLs, indicating that an a-priori expectancy belief is not necessary for the occurrence of an expectancy effect on JOLs. In contrast, only 26.79% of participants who had a-priori expressed an expectancy belief showed an inconsistency effect on JOLs. That is, more participants changed from an a-priori inconsistency effect to an expectancy effect during study than vice versa, $z = 2.35$, $p = .019$.

In their JOSs, 84.38% predicted an expectancy effect, and 15.63% predicted an inconsistency effect. Of those participants who had a priori expressed an inconsistency belief in source memory, 72.41% showed an expectancy effect on JOSs, again indicating that a-priori expectancy beliefs are not necessary for the occurrence of an expectancy effect on JOSs. In contrast, only 7.84% of participants who had a-priori expressed an expectancy belief showed an inconsistency effect on JOSs. That is, more participants changed from an a-priori inconsistency effect to an expectancy effect during study than vice versa, $z = 5.99$, $p < .001$.

This is an important finding concerning the main question whether a-priori beliefs explain the expectancy effect on JOLs and JOSs. A large proportion of participants who showed an inconsistency effect on the a-priori belief assessment showed an expectancy effect on both JOLs and JOSs. Thus, it is *not* necessary to hold an a-priori belief in an expectancy effect to express one in item-wise JOLs and JOSs.

A-priori belief as moderator of the expectancy effect on JOLs and JOSs. We tested whether a-priori beliefs about source–item expectancy fully or partially accounted for the expectancy effects on item-wise JOLs and JOSs. To obtain measures of a-priori beliefs, we subtracted each participant's a-priori memory judgment for unexpected source–item pairs from their a-priori memory judgment for expected source–item pairs (following Frank & Kuhlmann, 2017), separately for item-memory judgments and source-memory judgments.. This provided measures of how much better or worse participants a priori believed their (item or source) memory to be for expected versus unexpected source–item pairs. A positive difference indicates that participants believed a priori that memory would be better for expected source–item pairs (expectancy effect). A negative difference indicates that participants believed a priori that memory would be better for unexpected source–item pairs (inconsistency effect). We will hereafter refer to this measure as “belief”.

Beliefs should moderate the expectancy effect on JOLs and JOSs. This would be indicated by a significant interaction between source–item expectancy and beliefs as predictors of item-wise JOLs and JOSs (e.g., Baron & Kenny, 1986). In addition, we were interested in whether this moderation effect differed between JOLs and JOSs. To test this, we entered source–item expectancy, judgment type (JOL vs. JOS), the respective (item-memory or source-memory) belief score, and their interactions as predictors of item-wise judgments into a mixed linear regression model (cf. Kenny, Korchmaros, & Bolger, 2003; Krull & Mackinnon, 2001) using the R packages lme4 and lmerTest (Bates, Maechler, Bolker, & Walker, 2015; Kuznetsova, Brockhoff, & Christensen, 2014; R Core Team, 2017), with participants as random effects. For source–item expectancy, we set unexpected pairs as the reference condition (unexpected = 0, expected = 1). For judgment type, we set JOL as the reference condition (JOL = 0, JOS = 1). Thus, the intercept was the average JOL for unexpected source–item pairs when the participant a priori predicted equal memory for both unexpected and expected source–item pairs (belief = 0). Unstandardized regression weights and inference statistics are in Table A1 in the Appendix. Importantly, there was a significant three-way interaction between source–item expectancy, judgment type, and belief, $t(1.218*10^4) = 4.18$, $p < .001$. This indicates that the moderating effect of a-priori beliefs on the expectancy effect on metamemory judgments differed between JOLs and JOSs. Due to this three-way interaction, we refrained from further interpretation of the other effects and instead calculated separate analyses for JOLs and JOSs.

For JOLs and JOSs separately, we entered the respective item-memory or source-memory belief and source–item expectancy as predictors into a linear mixed regression model with participants as random effects. The other settings were the same as described above. Thus, the intercept was the average JOL or JOS for unexpected source–item pairs when the participant a priori predicted equal memory for both unexpected and expected source–item pairs (belief = 0). Unstandardized regression weights and inference statistics are presented in Table 3.

We found significant positive main effects of source–item expectancy on both JOLs and JOSs. This indicates that both JOLs and JOSs were higher for expected than unexpected source–item pairs when belief = 0. Thus, these results revealed that the main effect of source–item expectancy on both JOLs and JOSs was significant even when a-priori beliefs were taken into account. There were no main effects of belief, indicating that JOLs and JOSs for unexpected (i.e., source–item expectancy = 0) source–item pairs did not differ with the magnitude of beliefs. Critically, for both JOLs and JOSs, we found significant interactions between belief and source–item expectancy indicating that a-priori beliefs moderated the expectancy effect on both JOLs and JOSs. There was a positive slope between belief magnitude and expectancy effect for both JOLs and JOSs. Thus, the more strongly participants believed a priori that expected source–item pairs would be remembered better, the higher were the JOLs and JOSs they gave to expected relative to unexpected source–item pairs during study. Notably, this moderating effect of beliefs was stronger for JOSs than JOLs as indicated by the significant three-way interaction reported first. That is, JOSs relied more on a-priori source-memory beliefs than JOLs relied on a-priori item-memory beliefs. This finding is first evidence that different types of metamemory judgments may differ with regard to the weight assigned to beliefs versus experiences in judgment formation. It is thus a first step toward expanding existing theories of JOL formation into a more comprehensive theory of metamemory judgments.

Importantly, however, a-priori beliefs did not fully account for the expectancy effect on item-wise JOLs nor JOSs. Thus, experiences that participants had in the moment when studying the source–item pair appear to have additionally contributed to the expectancy effect on JOLs and JOSs (e.g., Koriat, 1997). This may explain why many participants who expressed an inconsistency belief a priori changed to an expectancy effect on memory judgments during study. Therefore, we simultaneously examined the contributions of both a-priori beliefs and in-the-moment experiences in Experiment 3.

Experiment 3

In Experiment 2, a priori beliefs moderated the expectancy effect on both item-wise JOLs and JOSs. At the same time, a-priori beliefs could not fully explain said effect, suggesting that in-the-moment experiences play a role as well (cf. Frank & Kuhlmann, 2017). In Experiment 3, we aimed to demonstrate that in-the-moment experiences also contribute to the expectancy effect on item-wise JOLs and JOSs, while simultaneously replicating the moderating effect of a-priori beliefs. So far, studies have assessed either beliefs or in-the-moment experiences (e.g., Frank & Kuhlmann, 2017; Mueller et al., 2013, 2014, 2016, Undorf & Erdfelder, 2011, 2013, 2015), but not both simultaneously. We deemed it important to assess both in the same experiment to also test for mutual influences.

There are different types of in-the-moment experiences and many ways to measure them. One prominent type of in-the-moment experience is processing fluency (Alter & Oppenheimer, 2009), which has been measured via self-paced study time (Castel, McCabe, & Roediger, 2007;

Koriat, 2008; Koriat & Ackerman, 2010; Koriat, Ma'ayan, & Nussinson, 2006; Miele, Finn, & Molden, 2011; Undorf & Erdfelder, 2011, 2013, 2015).

Study time may, in addition to in-the-moment experiences, also reflect beliefs. Koriat et al. (2006) provided a theoretical framework for the roles study time may serve in different situations, and we used this framework to disentangle the roles of in-the-moment experiences and beliefs in study time and metamemory judgments. According to Koriat et al., self-paced study time can serve a bottom-up monitoring and/or a top-down control function. According to the monitoring function, items that are experienced to be processed with more difficulty "call for" longer study times than items that are processed more easily. People thus use their study time to monitor the in-the-moment experience of processing difficulty and, in turn, as a cue for their metamemory judgment. Thus, items with longer study times should receive lower judgments than items with briefer study times (i.e., a negative relationship between study time and judgment). In line with these ideas, study time has been shown to mediate the effect of relatedness on JOLs in the paired-associates paradigm (e.g., Undorf & Erdfelder, 2015). That is, people studied unrelated pairs longer than related pairs, presumably because the former were more difficult to process, and study time was negatively related to JOLs (i.e., pairs studied longer received lower JOLs). Thus, part of the relatedness effect on JOLs was indirect, mediated via study time. This reasoning can be transferred to the source-monitoring paradigm: unexpected source–item pairs should call for longer study times than expected pairs (as found by Sherman et al., 1998), which should be used as an indication of more difficult processing, resulting in lower JOLs and JOSs. Thus, study time should (partially) mediate the expectancy effect on JOLs and JOSs.

However, we propose that study time, although often interpreted this way, may not measure in-the-moment experiences exclusively, but may also be influenced by beliefs. According to Koriat et al. (2006), study time may also fulfil a control function such that people may first assess an item as difficult or important and then strategically allocate more study time to it. In this case, after study, people predict that they will remember longer-studied items better because of more time invested (i.e., a positive relationship between study time and judgment). We suggest that, in the source-monitoring paradigm with schematic material, people may be motivated to compensate for their believed-to-be worse memory by allocating more study time to believed-to-be worse remembered source–item pairs. Thereby, a-priori beliefs may influence study-time allocation. Specifically, people holding an expectancy belief may allocate more study time to unexpected versus expected source–item pairs (compensating for believed-to-be worse memory for unexpected pairs). In contrast, people holding an inconsistency belief may allocate more time to expected pairs (compensating for believed-to-be worse memory for expected pairs). Therefore, we will test whether a-priori beliefs moderate study-time allocation to expected versus unexpected source–item pairs or whether these study-time differences reflect differences in in-the-moment experiences such as processing fluency. To our knowledge, we are the first to test, by ways of a moderator analysis, whether a-priori beliefs exert a top-down influence on study-time allocation, in line with Koriat et al.'s framework.

Because study time may serve a monitoring or a control function, Koriat et al. (2006) further proposed an attribution process. On a given task, learners may attribute the length of their study times to item characteristics (such as processing difficulty) and/or to their own goals (which may be motivated by beliefs). We thus tested whether (in addition to and/or independent of any direct influence on study times) a-priori beliefs may influence this attribution process. For example, depending on their a-priori belief, participants may be more or less surprised by the difficulty of processing the expected versus unexpected source–item pairs, and may thus be more or less motivated to actively control their study time. To answer this question, we tested whether a-priori beliefs moderated the relationship between study time and JOLs/JOSs. Such a moderation would qualify a general role of in-the-moment experiences in influencing study times and point to a role of beliefs in the study-time contribution to metamemory judgments.

Thus, based on Koriat et al.'s (2006) framework, we tested three not mutually exclusive hypotheses about the role of study time and beliefs in judgment formation: (1) Unexpected source–item pairs may call for longer study times due to in-the-moment experiences such as processing difficulty (cf. Undorf & Erdfelder, 2015). If so, these experiences should contribute to the expectancy effects on metamemory judgments (monitoring function). We tested this hypothesis via a mediation analysis: The relationship between expectancy and metamemory judgment may be mediated by study time. However, the interpretation of this mediation crucially depends on the effect of beliefs on study time. As outlined above, beliefs could moderate study-time allocation to expected versus unexpected source–item pairs or could moderate the study time-judgment relationship (cf. Koriat et al., 2006). If either of these were the case, a mediation of the expectancy effects by study time would not exclusively reflect a contribution of experiences in judgment formation. Rather, study time would serve a control function for beliefs. Therefore, the mediation would, at least partially, reflect an indirect contribution of beliefs. By contrast, if an effect of beliefs on study-time allocation or on the study time-judgment relationship can be excluded, a mediation of the expectancy effect on metamemory by study time would point to a belief-independent contribution of study time in judgment formation (i.e., study time would exclusively serve a monitoring function, and thus measure in-the-moment experiences). Therefore, to clearly distinguish the roles of experiences and beliefs in the study-time contribution to judgments, we additionally tested the following two hypotheses: (2) A-priori beliefs may influence study-time allocation to expected versus unexpected source–item pairs (control function). We tested this hypothesis via a moderator analysis: A priori beliefs may moderate the relationship between expectancy and study time. (3) Beliefs may influence the function that study time serves (attribution process). We tested this hypothesis via a moderated mediation: A-priori-beliefs may moderate the mediating effect of study time on the relationship between expectancy and memory judgment. To our knowledge, this is the first test of such influences of a-priori beliefs on the role of study time in judgment formation. Note that even if the study-time mediation postulated in hypothesis (1) does not hold, hypotheses (2) and (3) may still apply. For one, beliefs may influence study-time allocation as postulated in hypothesis (2), even though study time may then not be relied on in metamemory judgments. Second, even if there is no mediation for the full sample, a moderated mediation as postulated in hypothesis (3) might nonetheless reveal a study-time effect on metamemory judgments for a specific belief group.

We used the same source-monitoring paradigm as in Experiment 2 with the exception that study time was self-paced instead of fixed. We assessed a-priori beliefs via memory predictions before the study phase (as in Experiment 2). The heterogeneity of a-priori beliefs in the schema-relevant source-monitoring paradigm allowed us to subgroup participants by their indicated a-priori beliefs (i.e., expectancy belief, inconsistency belief, no belief) for moderated mediation analyses, which would not be feasible for those metamemory effects that occur in a large majority of people (e.g., the volume effect, Frank & Kuhlmann, 2017, and the font-size effect, Mueller et al., 2014).

Method

Participants. We recruited 120 new native speakers of German (25 male, 95 female) at the same university. As in Experiment 2, we aimed for at least 21 participants per belief category, and a sample size divisible by 12 for perfect counterbalancing. In Experiment 3, this was achieved with 120 participants. Age ranged between 17 and 31 years ($M = 21.23$, $SE = 0.30$). Participants received course credit or monetary compensation.

Design and Material. We manipulated source–item expectancy (expected vs. unexpected) within participants. A-priori belief and self-paced study time were measured variables. JOL and JOS were dependent variables. We used the same materials with the same counterbalancing scheme as in Experiment 2.

Procedure. The procedure was the same as in Experiment 2 with the exception that instead of studying each source–item pair for 4 seconds, study time was self-paced. Prior to study, participants were instructed to study each source–item pair as long as they needed to, but as briefly as possible. They were instructed to press the space bar when they were finished studying a source–item pair (which was also stated at the bottom of the screen during each trial). Upon pressing the space bar, they were asked to provide the JOL and JOS for each trial.

Results and Discussion

With the data from Experiment 3, we first replicated all analyses performed for Experiment 2. We will first describe results for memory, guessing, and contingency judgments followed by results for a-priori memory judgments and JOLs and JOSs. Then, we will present the test of the moderating effect of a-priori beliefs on the expectancy effect on JOLs and JOSs. Beyond these replications, we will test the three hypotheses about the role of in-the-moment experiences and beliefs in self-paced study time.

Memory, guessing, and contingency judgments. Like in Experiment 2, we analyzed responses in the source-monitoring test with the multinomial model of source monitoring (Bayen et al., 1996). Table 2 shows the parameter estimates for item memory, source memory, and guessing biases. Results showed no effect of expectancy on item memory, an inconsistency effect on source memory, and schema-consistent source guessing, replicating the results of Experiment 2 and prior research (e.g. Bell et al., 2012; Ehrenberg & Klauer, 2005; Küppers & Bayen, 2014; Schaper et al., under review). As in Experiment 2, we found that after test, participants assumed that more than half of the items had been presented with their expected source, $M = 35.15$, 95% CI [33.98, 36.32], $t(119) = 5.35$, $p < .001$, $d = 0.49$.

A-priori memory judgments. We again analyzed a-priori memory judgments with a 2 × 2 ANOVA with the within-subjects factors source–item expectancy and judgment type (item memory vs. source memory). Descriptive statistics are in Table 1. Participants predicted better memory for expected versus unexpected source–item pairs, $F(1, 119) = 32.03$, $p < .001$, $\eta_p^2 = .21$, replicating Experiments 1 and 2. This expectancy effect was medium sized in terms of Cohen's d (JOLs: $d_z = 0.48$, JOSs: $d_z = 0.49$). Like in the prior experiments, this expectancy effect must have been belief-driven because participants made the prediction prior to seeing any items or sources. Also, item-memory judgments were higher than source-memory judgments, $F(1, 119) = 21.58$, $p < .001$, $\eta_p^2 = .15$. There was no interaction, $F(1, 119) = 0.04$, $p = .837$. Thus, as in Experiments 1 and 2, the a-priori belief in an expectancy effect did not differ between item memory and source memory.

In their item-memory judgments, 62.50% of participants predicted an expectancy effect, 10.83% predicted no effect, and 26.67% predicted an inconsistency effect. In their source-memory judgments, 58.33% of participants predicted an expectancy effect, 20.83% predicted no effect, and 20.83% predicted an inconsistency effect.

Item-wise memory judgments (JOLs & JOSs). Like in Experiment 2, we analyzed item-wise memory judgments with a 2 × 2 ANOVA with the within-subjects factors source–item expectancy and judgment type (JOL vs. JOS). Descriptive statistics are in Table 1. While studying, participants again gave higher memory judgments for expected versus unexpected source–item pairs, $F(1, 119) = 98.09$, $p < .001$, $\eta_p^2 = .45$. Again, there was no main effect of judgment type, $F(1, 119) = 0.41$, $p = .524$, but a significant two-way interaction, $F(1, 119) = 63.40$, $p < .001$, $\eta_p^2 = .35$, indicating a stronger expectancy effect on JOSs than on JOLs. Paired t

tests revealed a significant expectancy effect on both JOLs, $t(119) = 7.49$, $p < .001$, $d_z = 0.68$, and JOSs, $t(119) = 9.88$, $p < .001$, $d_z = 0.90$. Thus, we replicated the illusory expectancy effect on JOLs and JOSs found in Experiment 2 and prior research.

In their JOLs, 80.83% of participants predicted an expectancy effect, and 19.17% predicted an inconsistency effect. Of those participants who had a priori expressed an inconsistency belief regarding item memory, 75.00% showed an expectancy effect on JOLs, indicating that a-priori beliefs again did not fully account for the expectancy effect on JOLs. In contrast, only 18.67% of participants who had a-priori expressed an expectancy belief showed an inconsistency effect on JOLs. That is, like in Experiment 2, more participants changed from an a priori inconsistency effect to an expectancy effect during study than vice versa, $z = 5.58$, $p < .001$.

In their JOSs, 82.50% of the participants predicted an expectancy effect, 0.83% did not predict an effect, and 16.67% predicted an inconsistency effect. Of those participants who had a priori expressed an inconsistency belief regarding source memory, 60.00% showed an expectancy effect on JOSs, again indicating that a-priori beliefs did not fully account for the expectancy effect on JOSs. In contrast, only 11.43% of participants who had a priori expressed an expectancy belief showed an inconsistency effect on JOSs. That is, more participants changed from an a priori inconsistency effect to an expectancy effect during study than vice versa, $z = 4.87$, $p < .001$. Thus, the results from Experiment 2 replicated. These results show that there must have been factors other than belief that contributed to the expectancy effect on item-wise JOLs and JOSs.

A-priori belief as moderator of the expectancy effect on JOLs and JOSs. We performed the same moderator analysis as in Experiment 2 to test the moderating effect of a-priori belief on the expectancy effect on item-wise JOLs and JOSs via the interaction of a-priori beliefs and source–item expectancy. That is, we again entered source–item expectancy, judgment type (JOL vs. JOS), and (item-memory and source-memory) belief as predictors of item-wise memory predictions into a linear mixed regression model with participants as random effects. Unstandardized regression weights and inference statistics are in Table A1 in the Appendix. Importantly, there was again a significant three-way interaction of source–item expectancy, judgment type, and beliefs, $t(1.523*10^4) = 5.34$, $p < .001$, indicating differences in the moderating effect of a-priori beliefs on metamemory judgments for JOLs versus JOSs. As in Experiment 2, we refrained from further interpretation of the other effects and conducted separate analyses for JOLs and JOSs. That is, we entered source–item expectancy, (item-memory or source-memory) belief, and their interaction as predictors of JOLs or JOSs into two separate linear mixed regression models with participants as random effects. Unstandardized regression weights and inference statistics for these analyses are in Table 3.

We found significant positive main effects of source–item expectancy on both JOLs and JOSs. Critically, these expectancy effects emerged while a-priori beliefs were taken into account indicating that beliefs did not fully account for the expectancy effects.

The other results differed for JOLs versus JOSs. On JOLs, there was no main effect of belief nor an interaction. Thus, the moderating effect of a-priori beliefs on the expectancy effect on JOLs found in Experiment 2 did not replicate.

On JOSs, by contrast, there was a main effect of beliefs indicating that JOSs decreased with the extent to which participants had a-priori predicted a memory advantage for expected versus unexpected source–item pairs. Critically, we also found a significant interaction of beliefs and source–item expectancy. As in Experiment 2, there was a positive slope between belief magnitude and expectancy on JOSs indicating that the more strongly participants believed a priori that expected source–item pairs would be remembered better, the higher was the expectancy effect on JOS. Thus, belief moderated the expectancy effect.

In sum, in Experiment 3, there was again an expectancy effect on item-wise JOLs and JOSs, even when we accounted for a-priori beliefs. Further, the moderating effect of a-priori beliefs on this expectancy effect was only present for JOSs, but not for JOLs. Experiments 2 and 3 convergingly demonstrated a stronger (or selective) moderating effect on JOSs than on JOLs, indicating a stronger dependency on a-priori beliefs of JOSs compared to JOLs. A-priori beliefs seem to play only a minor (or no) role in JOL formation.

Notably, however, the expectancy effect on JOSs was not fully explained by a-priori beliefs in the mixed model analyses. Thus, there must be in-the-moment experiences that reflect a higher ease of processing for expected source–item pairs and thus contribute to an expectancy effect on JOLs and JOSs.

The role of in-the-moment experience and belief in self-paced study time. Next, we tested the hypotheses based on Koriat et al.'s (2006) framework. First, we tested for a main effect of expectancy on study time: study times should be longer for unexpected than expected source–item pairs. We asked whether study time fulfilled a monitoring function of in-the-moment experiences by testing for a mediating effect of study time on the expectancy effect on JOLs and JOSs (Hypothesis 1). Then, we asked whether study time fulfilled a control function based on a-priori beliefs by testing for a moderating effect of beliefs on study time allocation (Hypothesis 2). Finally, we asked whether beliefs influenced the attribution of study time by testing whether the indicated belief moderated the mediating effect of study time on the relationship between expectancy and JOLs and JOSs (Hypothesis 3).

Study time as mediator of the expectancy effect on JOLs and JOSs. We eliminated trials in which self-paced study times were shorter than 200ms or longer than a participant's individual mean plus three individual standard deviations, which were a total of 1.6% of all trials. All results replicated when all trials were included.

Unexpected source–item pairs (*M* = 9.22 s, 95% CI_{WS} [7.82, 8.22][2]) were studied longer than expected source–item pairs, *M* = 8.02 s, 95% CI_{WS} [9.02, 9.41], $t(119) = 6.03$, $p < .001$, $d_z = 0.55$, replicating Sherman et al. (1998). Next, we examined the influence of in-the-moment experiences on JOLs and JOSs. To test whether the expectancy effect on JOLs and JOSs in source monitoring was mediated by self-paced study time, we conducted mixed model mediation analyses (following Undorf & Erdfelder, 2015). Like Undorf and Erdfelder (2015), we logarithmized study times to render their distributions closer to normality, then centralized them to the grand mean. The settings for all other variables were the same as in Experiment 2.

Before analyzing the mediating effect of study time, we conducted linear mixed regression analyses to establish the direct effects between source–item expectancy, study time, and metamemory judgments with participants as random effects. For both JOLs and JOSs, we fitted two models each. For the first model, we entered source–item expectancy (0 = unexpected, 1 = expected) as a predictor of self-paced study time. For the second model, we entered source–item expectancy and self-paced study time as predictors of the respective metamemory judgment (JOL or JOS). We then conducted mediation analyses using the R package mediation (Tingley, Yamamoto, Hirose, Keele, & Imai, 2014; see also Imai, Keele, & Tingley, 2010). Indirect effects of source–item expectancy on JOLs/JOSs mediated by study time and their 95% confidence intervals were estimated using Tingley et al.'s (2014) nonparametric bootstrapping procedure with 5000 bootstrap samples.

Mediation of the expectancy effect on JOLs. Table 5 shows the direct effects of source–item expectancy on study time, study time on metamemory judgment, and source–item expectancy on metamemory judgment (for both JOLs and JOSs). For JOLs, the direct effects of source–item expectancy on study time, study time on metamemory judgment, and source–item expectancy on metamemory judgment were significant. There was a negative slope for the effect of source–item expectancy on study time indicating that unexpected source–item pairs were studied longer than expected pairs. There was also a negative slope for the effect of study time on JOLs indicating that items that were studied more briefly received higher JOLs. Lastly, there was a positive slope for the effect of source–item expectancy on JOLs indicating that unexpected pairs received lower JOLs than expected pairs even when study time was controlled.

For the mediation analysis, indirect effects and proportions mediated by study time are shown in Table 6. We replicated self-paced study time as a mediator of the expectancy effect on JOLs, which Undorf and Erdfelder (2015) found with a different paradigm. In JOL formation, our participants thus used study time as a cue to the processing difficulty of an item and thus related longer study times to poorer memory for unexpected items. Study times thus served a monitoring function (Koriat et al., 2006) implying that in-the-moment experiences such as processing fluency contributed to the expectancy effect on JOLs. Thereby, Experiment 3 revealed another determinant of the expectancy illusion on JOLs in addition to a-priori beliefs. Expected source–item pairs seemed to elicit a stronger feeling of fluency or processing ease than unexpected pairs (as indicated by the study-time differences). This experience contributed to item-wise JOLs although it was not predictive of later item memory (which was independent of source–item expectancy).

Mediation of the expectancy effect on JOSs. As shown in Table 5, for JOSs, there were direct effects of source–item expectancy on study time and on metamemory judgment, replicating the results for JOLs. However, the direct effect of study time on JOSs was not significant.

A mediation analysis analogous to the one described for JOLs confirmed this result. As shown in Table 6, study time did not mediate the expectancy effect on JOSs. Thus, for JOSs, participants did not consider study time a cue to source–item pair difficulty and, thus, study time did not serve a monitoring function. Thus, the mixed model analyses revealed different mediation patterns for JOSs versus JOLs, with a different relationship of self-paced study time to JOSs versus JOLs.

Belief as a moderator of study-time allocation. Next, we tested whether a-priori beliefs moderated study-time allocation to unexpected versus expected source–item pairs. This would be indicated by an interaction of belief and source–item expectancy on study time. We thus calculated two linear mixed models with source–item expectancy, a-priori (item-memory and source-memory) beliefs, and their respective interactions as predictors of study time. Table 4 shows the unstandardized regression weights and inference statistics. For both item-memory and source-memory beliefs, we found main effects of source–item expectancy with negative slopes indicating again that unexpected pairs were studied longer than expected pairs. There were no main effects of beliefs, and importantly, there were no interactions between expectancy and beliefs. This suggests that a-priori beliefs did not control study-time allocation. Thus, importantly, the mediating effect of study time on the expectancy effect on JOLs described in the previous paragraph can be interpreted as a bottom-up contribution of in-the-moment experience and not by a-priori beliefs influencing study-time allocation top-down.

Influence of belief on the attribution process of study time. Finally, we tested whether a-priori beliefs moderated the mediating effect of study time on the expectancy effects (Hypothesis 3). To this end, we conducted separate mediation analyses for the three a-priori belief subgroups (expectancy effect, inconsistency effect, no effect).

Effect of beliefs on the mediation of the expectancy effect on JOLs. Table 5 shows the direct effects of source–item expectancy on study time, study time on metamemory judgment, and source–item expectancy on metamemory judgment (for both JOLs and JOSs) for the participant groups broken down by their indicated a-priori belief (expectancy effect, inconsistency effect, no effect). For JOLs, all direct effects of source–item expectancy on study time, study time on metamemory judgment, and source–item expectancy on metamemory judgment were significant for each belief group, replicating the results for the full sample. Notably, the subgroup analyses revealed that even those who a priori believed in an inconsistency or no effect showed an average expectancy effect on JOLs (when study time was controlled).

We then performed mediation analyses separately for each belief group. Indirect effects and proportions mediated by study time are shown in Table 6. Study time was a significant mediator of the expectancy effect on JOLs in each belief group, replicating the result for the full sample. Thus, importantly, the mediating role of study time for the expectancy effect on JOLs was independent of a-priori belief. That is, for JOLs, a-priori beliefs did not influence participants' interpretation of their study time differences between expected and unexpected source–item pairs. Thus, for JOLs, study time was used as a monitoring cue to item difficulty independent of a-priori belief.

Effect of beliefs on the mediation of the expectancy effect on JOSs. As shown in Table 5, for JOSs, the direct effects of source–item expectancy on study time and on metamemory judgment replicated the results of the full sample. Notably, as with JOLs, subgroup analyses of the expectancy effect on JOSs revealed that even those who a priori believed in an inconsistency or no effect showed an average expectancy effect on JOSs (when study time was controlled). However, the direct effect of study time on JOSs was neither significant for those who indicated a belief in an expectancy effect nor for those who indicated that they believed in no effect. Only those who indicated a belief in an inconsistency effect showed a significant effect of study time on JOSs. Notably, this effect showed a positive slope. That is, the longer source–item pairs were studied, the higher JOSs they received.

Mediation analyses confirmed this result. As shown in Table 6, study time neither mediated the expectancy effect on JOSs for those who indicated a belief in an expectancy effect nor for those who indicated that they believed in no effect (replicating the result of the full sample). For those who believed in an inconsistency effect, study time did mediate the expectancy effect, however, with a negative slope (i.e., the expectancy effect on JOSs increased when study time was controlled).

Implications of the moderated mediation analyses. Like the mediation analyses for the full sample, the mediation analyses moderated by belief subgroups revealed different mediation patterns for JOSs versus JOLs. For JOLs, study time served a bottom-up monitoring function (i.e., study time was used as an indication of source–item pair fluency), independent of a-priori belief. For JOSs, by contrast, a-priori beliefs influenced how participants interpreted the function of study time. Specifically, for those participants who a priori believed in an inconsistency effect on source memory, study time served a top-down control function (i.e., compensating for differences in source–item pair difficulty), indicated by a positive relationship between study times and JOSs. This control function of study time on JOSs stands in contrast to the monitoring function it appears to serve for JOLs, but is in line with prior research showing both functions of study time (Koriat et al., 2006).

Why did participants with an a-priori inconsistency belief attribute their study times to a control function? Presumably, the difficulty of unexpected source–item pairs was particularly surprising to these participants, who had initially predicted better memory for these unexpected pairs. They may have interpreted their increased study times for unexpected pairs as a compensation for this surprising difficulty. Although participants integrated this potential compensation into their item-wise JOSs (i.e., higher JOSs for pairs studied longer), they apparently did not fully trust it as they nonetheless showed an overall expectancy effect on item-wise JOSs, contrary to their a-priori beliefs and their extended study times for unexpected pairs. In contrast, those participants who initially believed in an expectancy effect or no effect may not have been as surprised by the difficulty of unexpected source–item pairs, because they had either predicted it, or had no strong belief on the matter. Therefore, these participants may have ignored study time as a cue for JOSs altogether. Taken together, the moderated mediation results reveal that in JOS formation, participants did not use study time as a monitoring cue as they did in JOL formation. By contrast, for those participants who initially believed in an inconsistency effect study time seemed to serve a control function (Koriat et al., 2006).

Notably, the expectancy effect on JOSs was not explained by study time, but also not fully explained by a-priori beliefs. Thus, possibly, there are in-the-moment experiences not captured by self-paced study time that reflect a higher subjective ease of processing or ease of imagery for expected source–item pairs. Reber, Wurtz, and Zimmermann (2004), for example, showed that different aspects of processing contribute to an overall subjective feeling of fluency. Thus, in-the-moment experiences may still contribute to the illusory expectancy effect on JOSs, but they appear to be different from those contributing to JOLs.

General Discussion

The development of a comprehensive theory of metamemory requires the understanding of different metamemory judgment types, different factors that contribute to judgment formation, and differential weighting of such factors. In three experiments, we therefore examined a-priori beliefs and in-the-moment experiences as determinants of the expectancy effect on two metamemory judgments (JOLs and JOSs) in a source-monitoring paradigm with schema-related materials. In two ways, our results contribute to the ongoing debate whether metamemory judgments are mainly based on a-priori beliefs or in-the-moment experiences. First, we found that both a-priori beliefs and in-the-moment experiences independently contributed to the expectancy effect on JOLs and JOSs. Thus, Koriat's (1997) framework on the utilization of both a-priori beliefs *and* experiences at the moment of study is useful to explain the formation of JOSs in addition to JOLs (and other item-focused metamemory judgments such as FOKs, see Koriat, 2000). Second, we nonetheless found differences in the weighing of these factors in JOL versus JOS formation, with in-the-moment experiences playing a larger role for JOLs, and a-priori beliefs playing a larger role for JOSs. That is, when discussing determinants of metamemory, specific judgment types (in this case, JOLs and JOSs) must be considered. Thus, our findings highlight the importance of a broad approach in compiling a comprehensive account of metamemory determinants. Further, to date, the approaches taken in the debate whether beliefs or experiences play the more prominent role in metamemory judgment formation have been limited, with most studies assessing only one of the two (Frank & Kuhlmann, 2017; Mueller et al., 2013, 2014, 2016, Undorf & Erdfelder, 2011, 2013, 2015). We advocate the assessment of both beliefs and experiences in the same studies to determine their common and/or separate contributions.

In Experiments 2 and 3, we found an expectancy effect on item-wise JOLs (replicating Konopka & Benjamin, 2009; Mueller et al., 2013; Schaper et al., under review; Shi et al., 2012; Undorf & Erdfelder, 2011, 2013, 2015) and JOSs (replicating Schaper et al., under review). This effect was more pronounced in JOSs than in JOLs, indicating that participants thought that schemas influenced source memory more than item memory. In replication of Schaper et al. (under review), these metacognitive judgments were dissociated from actual item memory (which showed no effect of schemas) and source memory (which showed an inconsistency effect). In the following, we will summarize the contributions of a-priori beliefs and in-the-moment experiences to this illusory expectancy effect on JOLs and JOSs.

A-priori beliefs moderate the expectancy effect

The experiments reported here are the first to measure a-priori beliefs about the effect of schematic knowledge on item memory and source memory. Across all three experiments, participants did not homogenously hold an a-priori belief in expectancy effects on item memory nor source memory. Although many of the participants did express this belief, there was a substantial proportion (between 37% and 50%, depending on judgment type and experiment) who believed in either no effect or in an inconsistency effect. This is different from other studied metamemory effects (such as the font-size effect, Mueller et al., 2014, and the volume effect, Frank & Kuhlmann, 2017), which usually show quite homogeneous a-priori beliefs. The source-monitoring paradigm with schematic material thus helps us investigate the impact of beliefs, because we can study the impact of different types of beliefs (with sufficiently large sample sizes) instead of just different degrees of belief in the same effect.

In contrast to item-wise judgments, a-priori beliefs were the same for item memory and source memory. That is, participants did not a priori assume a stronger effect of schemas on source memory than on item memory as found in item-wise judgments. Additionally, in Experiments 2 and 3, a large proportion (between 54% and 75%, depending on judgment type and experiment) of participants who had initially indicated a belief in an inconsistency effect predicted the contrary (i.e., an expectancy effect) during study. All this evidence underscores that JOLs and JOSs do not directly mirror a-priori beliefs and, thus, these beliefs do not fully account for the expectancy effect on JOLs and JOSs.

A-priori beliefs did, however, partially moderate the expectancy effect on metamemory judgments (see Frank & Kuhlmann, 2017, for similar results for the volume effect). Participants who a priori strongly believed in an expectancy effect predicted a stronger expectancy effect during study than those who a priori believed in no effect or in an inconsistency effect. In

Experiment 2, this was the case for both JOLs and JOSs; however, in Experiment 3, this effect replicated for JOSs only. In both experiments, the moderation of the expectancy effect was stronger on JOSs than on JOLs.

JOSs were thus more belief-dependent than JOLs. These two types of metamemory judgments thus differed in the weight different contributing factors received during judgment formation, even though both judgments were elicited one after the other from the same participants. It may be relatively easy to apply general beliefs to only two sources rather than to many individual items. This reasoning is speculative, however, and should be addressed in future research, for example by employing more than two sources. As mentioned, the question whether metamemory judgments are primarily based on a-priori beliefs is controversially discussed in the literature (Frank & Kuhlmann, 2017; Koriat, 1997; Mueller et al., 2013, 2014, 2016, Undorf & Erdfelder, 2011, 2013, 2015). Thus far, differences in the contribution of beliefs to different judgment types have mostly been ignored. However, based on our results, we deem it imperative to consider judgment-specific effects of a-priori beliefs in order to develop a general account of judgment formation.

Our findings explain in part why metamemory and memory are dissociated in this paradigm. Many participants already hold an illusory expectancy belief prior to study. This false belief influences JOLs and especially JOSs, and thus biases these judgment towards false memory predictions. Different weights beliefs receive in JOL versus JOS formation may, to an extent, explain the differences we observed between the two types of judgments (Schaper et al., under review). JOSs may show a larger expectancy effect than JOLs because they rely more on this illusory a-priori belief.

Importantly, we found expectancy effects on both JOLs and JOSs even when a-priori beliefs were taken into account, indicating that beliefs only explained part of the expectancy effect on item-wise metamemory judgments. Thus, while metamemory judgments (JOSs especially) are in part belief-dependent, there must be factors that contribute to them in the moment (see Frank & Kuhlmann, 2017).

In-the-moment experiences mediate the expectancy effect

Concerning in-the-moment experiences, we also found different results for JOLs and JOSs. In Experiment 3, we showed that self-paced study time, as a suggested measure of in-the-moment experiences, mediated the expectancy effect on JOLs (replicating Undorf & Erdfelder, 2015). Thus, in-the-moment experiences do play a role in JOL formation in the source-monitoring paradigm. This mediation effect was present regardless of the indicated a-priori belief, showing that the impact of in-the-moment experiences on JOLs is not belief-dependent, but an independent contribution. Note, however, that this contribution was rather small. Only 4% of the direct effect of source–item expectancy on JOLs was mediated by study time. Thus, a large portion of the direct expectancy effect on JOLs remained unexplained (see Undorf & Erdfelder, 2015, for a comparably small mediation effect for the relatedness effect on JOLs). While it is possible that in-the-moment experiences only weakly contribute to judgment formation, it is also possible that self-paced study time did not fully capture the fluency experiences during study. Thus, future research should develop more comprehensive measures to fully capture the fluency experience. The reliance of JOLs on study time shows another reason for the dissociation of metamemory and memory in the source-monitoring paradigm: Processing fluency as a cue for metamemory judgments is not predictive of item memory in this paradigm, and thus, in addition to beliefs, contributes to the illusion in JOLs.

In JOSs, we did not find this mediation of the expectancy effect by study time. Over all participants, there was no relationship between study time and JOSs. Only for those who a-priori believed in an inconsistency effect, we found a *positive* relationship between study time and

JOSs, indicating that these participants gave higher JOSs to source–item pairs that they studied longer. These results show that participants did not use their study times as a fluency cue in order to predict their source memory. Rather, study time seems to sometimes serve a control function (Koriat et al., 2006) in form of a compensatory reaction to surprisingly difficult source–item pairs instead of a monitoring function of source–item difficulty. Although study times were not directly controlled by a-priori beliefs, the relationship between study times and JOSs depended on beliefs, indicating that beliefs influenced the attribution process of study time proposed by Koriat et al.

Considering this finding, one might want to conclude that, in contrast to JOLs, JOSs are generally not affected by in-the-moment experiences. However, we deem this conclusion premature. While for JOSs, self-paced study time was not used as a cue to the difficulty of a source–item pair, we still deem it plausible that in-the-moment experiences play a role in JOS formation. This is especially evident if we once again consider the effects of a-priori beliefs on JOSs explained above. Even when beliefs were taken into account, the expectancy effect on JOSs was still present. Additionally, more participants who expressed a belief in an inconsistency effect a priori showed an expectancy effect during study than vice versa. Likewise, participants who initially expressed belief in an inconsistency effect also showed an overall expectancy effect during study. Thus, there must be other influences that occurred right in the study situation that made these participants change their prediction during the course of the experiment. Thus, possibly in-the-moment experiences not captured by study time contributed to the illusory expectancy effect on JOSs in addition to invalid beliefs.

Our data thus suggest that self-paced study time may not be the ideal measure of in-the-moment experiences in all circumstances. However, other measures of in-the-moment experiences are not feasible in this paradigm. In particular, trials-to-acquisition has been suggested as an alternative measure (cf. Undorf & Erdfelder, 2015), but given the high chance of guessing the correct source (out of only two options) many source–item pairs likely would be answered correctly on the first trial without actual learning. Thus, we deemed trials-to-acquisition an inadequate measure of processing fluency in this paradigm. Several studies have used lexical decision times (Connor, Balota, & Neely, 1992; Mueller et al., 2013, 2014; Yaniv & Meyer, 1987), however, in the present paradigm, we would only be able to assess lexical decision time for the item or the source, but not for the (critical) combination of both. Future research needs to find better measures of ease of processing in order to better explain metamemory effects.

In summary, we found that both a-priori beliefs and in-the-moment experiences contributed to the expectancy effects on two different metamemory judgments. Future research may further investigate the relative contributions of beliefs versus in-the-moment experiences to a multitude of metamemory judgments to integrate these findings towards a comprehensive theory of metamemory.

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Footnotes

[1]Note that a measure of source memory that is unconfounded by response bias (e.g., a multinomial processing tree parameter estimate, Batchelder & Riefer, 1990; Bayen, Murnane, & Erdfelder, 1996; Bröder & Meiser, 2007) must be used to show this effect because mere performance measures confound source memory and source guessing (Bayen et al., 1996; Murnane & Bayen, 1996). In the described paradigm, source guessing is usually biased towards the expected source. That is, if participants do not remember the source of a certain item, they tend to guess that it was presented with the expected source during study (e.g., Bayen et al., 2000; Bell et al., 2012; Küppers & Bayen, 2014; Schaper et al., under review). This guessing bias affects performance measures and thus counteracts the inconsistency effect on source memory.

[2]For within-subject comparisons, we computed 95% confidence intervals as described by Loftus and Masson (1994). These are interpreted like the standard between-subjects confidence intervals but provide concordant information to within-subject tests specifically. We will label these as CI_{WS} to distinguish them from CI for between-subjects comparisons.

[3]The results regarding post-test memory judgments replicated those reported by Schaper et al. (under review). Because these results had no direct relevance to the current main research questions, we report them in an online supplement only.

Table 1

Descriptive Statistics for Metamemory Judgments in all Experiments

			source–item expectancy	
Experiment	Judgment timing	Judgment type	expected	unexpected
1	a priori (item & source)	item memory	56.86 [52.38, 61.34]	48.09 [43.61, 52.57]
		source memory	60.24 [55.61, 64.88]	51.04 [46.41, 55.68]
	a priori (item-only)	item memory	53.13 [48.67, 57.58]	55.56 [51.11, 60.01]
	a priori (source-only)	source memory	61.55 [56.30, 66.79]	61.20 [55.95, 66.44]
2	a priori	item memory	64.71 [63.01, 66.41]	56.70 [55.00, 58.40]
		source memory	62.92 [60.89, 64.95]	55.50 [53.47, 57.53]
	during study	item memory (JOL)	60.57 [59.57, 61.57]	54.36 [53.36, 55.36]
		source memory (JOS)	63.34 [61.56, 65.12]	46.40 [44.62, 48.18]
3	a priori	item memory	65.51 [63.80, 67.21]	56.39 [54.69, 58.10]
		source memory	61.48 [59.84, 63.13]	52.61 [50.96, 54.26]
	during study	item memory (JOL)	58.31 [57.56, 59.07]	52.60 [51.84, 53.35]
		source memory (JOS)	63.96 [62.41, 65.50]	48.57 [47.03, 50.11]

Note. Mean memory judgments with 95% within-subjects confidence intervals in brackets.[2] Judgments were percentage ratings ranging from 0 to 100. Judgments from Experiment 1 were originally frequency ratings from 0 to 32, and were transformed to percentages. A priori = judgments rendered prior to (or without) presentation of source–item pairs. During study = item-wise Judgments of Learning (JOLs) and Judgments of Source (JOSs) rendered during study. Item & source = participants rendered both item-memory and source-memory judgments. Item-only = participants rendered item-memory judgments only. Source-only = participants rendered source-memory judgments only.

Table 2

Parameter Estimates, Model Fit, and Parameter Tests of the Multinomial-Processing-Tree Model Analyses for Experiments 2 and 3

Model Parameter	Experiment 2	Experiment 3
D	.68 [.67, .79]	.78 [.77, .79]
b	.30 [.28, .33]	.31 [.29, .34]
d_{expected}	.37 [.24, .51]	.53 [.43, .64]
$d_{\text{unexpected}}$	.62 [.57, .68]	.71 [.67, .75]
g	.67 [.61, .72]	.66 [.60, .71]
Model Fit	$G^2(1) = 2.00, p = .157$	$G^2(1) = 0.74, p = .391$
Hypothesis Tests		
d_{expected} vs. $d_{\text{unexpected}}$	$\Delta G^2(1) = 9.00, p = .003$	$\Delta G^2(1) = 8.12, p = .004$
g vs. .50	$\Delta G^2(1) = 33.07, p < .001$	$\Delta G^2(1) = 26.99, p < .001$

Note. D = probability of recognizing an item; $d_{\text{expected}}/d_{\text{unexpected}}$ = probability of remembering that an item was presented with the expected/unexpected source; b = probability of guessing that an item is old; g = probability of guessing that an item was presented with the expected source. 95% confidence intervals are in brackets. G^2 = maximum-likelihood chi-square goodness of fit statistic. Model fit is evaluated by minimizing the approximately chi-square distributed maximum-likelihood statistic G^2 which measures the deviation of the model prediction from the empirical data. A good model fit is indicated by a non-significant test result. Parameter tests can be performed by implementing additional parameter restrictions ($d_{\text{expected}} = d_{\text{unexpected}}$ to test source-memory differences; g =.50, to test for expectancy-related guessing). The thus restricted model is tested against the base model. A decrease in model fit (indicated by a significant ΔG^2 statistic) indicates a difference.

Table 3

Linear Mixed-Model Results Regarding Effects of Source–Item Expectancy and A-priori Belief on JOLs and JOSs in Experiments 2 and 3

Experiment	Judgment type		Estimate	95% *CI*	*df*	*t*	*p*
2	JOL	Intercept	53.59	[49.78, 57.40]	98	27.56	< .001
		Expectancy	5.69	[4.65, 6.73]	6046	10.70	< .001
		Belief	0.10	[–0.11, 0.30]	98	0.91	.364
		Expectancy × Belief	0.07	[0.01, 0.12]	6046	2.26	.024
	JOS	Intercept	47.35	[44.08, 50.62]	100	28.40	< .001
		Expectancy	15.18	[12.07, 16.29]	6046	26.87	< .001
		Belief	–0.13	[–0.28, 0.03]	100	1.64	.105
		Expectancy × Belief	0.24	[0.18, 0.29]	6046	8.90	< .001
3	JOL	Intercept	52.62	[49.02, 56.22]	122	28.63	< .001
		Expectancy	5.48	[4.59, 6.38]	7558	11.97	< .001
		Belief	–0.003	[–0.18, 0.17]	122	0.03	.976
		Expectancy × Belief	0.03	[–0.02, 0.07]	7558	1.18	.239
	JOS	Intercept	50.31	[47.23, 53.38]	124	32.07	< .001
		Expectancy	13.49	[12.50, 14.47]	7558	26.83	< .001
		Belief	–0.20	[–0.35, –0.04]	124	2.52	.013
		Expectancy × Belief	0.21	[0.16, 0.26]	7558	8.58	< .001

Note. Unstandardized regression weights and inference statistics were computed using the R procedures lme4 and lmerTest with restricted maximum likelihood estimation. JOL = Judgment of Learning. JOS = Judgment of Source. For source–item expectancy, 0 = unexpected, 1 = expected. Beliefs indicate the predicted memory difference between expected source–item pairs and unexpected pairs. CI = confidence interval.

Table 4

Linear Mixed-Model Results Regarding the Influence of A-priori Belief and Source–Item Expectancy on Study Time in Experiment 3

Item-memory belief	Estimate	95% *CI*	*df*	*t*	*p*
Intercept	0.11	[–0.03, 0.24]	121	1.55	.123
Expectancy	–0.15	[–0.18, –0.13]	7435	10.63	< .001
Belief	–0.003	[–0.01, 0.003]	121	0.99	.322
Expectancy × Belief	–0.0004	[–0.001, 0.001]	7435	0.54	.587
Source-memory belief					
Intercept	0.12	[–0.01, 0.26]	121	1.80	.075
Expectancy	–0.16	[–0.19, –0.13]	7435	10.75	< .001
Belief	–0.01	[–0.01, 0.001]	121	1.53	.128
Expectancy × Belief	0.0002	[–0.002, 0.001]	7435	0.21	.833

Note. Unstandardized regression weights and inference statistics were computed using the R procedures lme4 and lmerTest with restricted maximum likelihood estimation. Beliefs indicate the predicted memory difference between expected source–item pairs and unexpected pairs. Study times were trimmed, logarithmized and centralized. CI = confidence interval.

Table 5

Linear Mixed-Model Results for the Direct Effects between Source–Item Expectancy, Study Time, and JOLs and JOSs in Experiment 3

Judgment Type	Belief group	Effect	Estimate	95% *CI*	*df*	*t*	*p*
JOL	All participants (*n* = 120)	Expectancy on Study Time	–0.16	[–0.18, –0.13]	7436	12.08	< .001
		Study Time on JOL	–1.62	[–2.34, –0.91]	7533	4.47	< .001
		Expectancy on JOL	5.45	[4.63, 6.27]	7441	13.01	< .001
	Expectancy effect (*n* = 75)	Expectancy on Study Time	–0.16	[–0.10, –0.13]	4653	9.83	< .001
		Study Time on JOL	–1.26	[–2.15, –0.37]	4723	2.78	.005
		Expectancy on JOL	4.76	[3.74, 5.77]	4656	9.18	< .001
	Inconsistency effect (*n* = 32)	Expectancy on Study Time	–0.11	[–0.15, –0.06]	1983	4.4.48	< .001
		Study Time on JOL	–1.79	[–3.29, –0.29]	1994.3	2.34	.020
		Expectancy on JOL	4.46	[2.86, 6.07]	1983.1	5.45	< .001
	No effect (*n* = 13)	Expectancy on Study Time	–0.27	[–0.36, –0.18]	798	5.69	< .001
		Study Time on JOL	–2.38	[–4.35, –0.41]	803	2.37	.018
		Expectancy on JOL	12.02	[9.32, 14.72]	798.5	8.73	< .001
JOS	All participants (*n* = 120)	Expectancy on Study Time	–0.16	[–0.18, –0.13]	7436	12.08	< .001
		Study Time on JOS	0.74	[–0.04, 1.52]	7348	1.85	.064
		Expectancy on JOS	15.49	[14.58, 16.39]	7445	33.55	< .001
	Expectancy effect (*n* = 70)	Expectancy on Study Time	–0.16	[–0.19, –0.12]	4343	9.33	< .001
		Study Time on JOS	0.38	[–0.63, 1.39]	4257	0.74	.461
		Expectancy on JOS	15.68	[14.52, 16.83]	4348	26.64	< .001
	Inconsistency effect (*n* = 25)	Expectancy on Study Time	–0.11	[–0.17, –0.06]	1548	3.95	< .001
		Study Time on JOS	4.08	[2.44, 5.71]	1556.1	4.88	< .001
		Expectancy on JOS	7.12	[5.22, 9.02]	1547.9	7.35	< .001
	No effect (*n* = 25)	Expectancy on Study Time	–0.21	[–0.26, –0.15]	1543	6.87	< .001
		Study Time on JOS	–0.84	[–2.58, 0.90]	1518.9	0.95	.343
		Expectancy on JOS	23.25	[21.14, 25.36]	1545.1	21.59	< .001

Note. Unstandardized regression weights were computed using the R procedures lme4 and lmerTest with restricted maximum likelihood estimation. JOL = Judgment of Learning. JOS = Judgment of Source. For source–item expectancy, 0 = unexpected, 1 = expected. Study times were trimmed, logarithmized and centralized. CI = confidence interval.

Table 6

Linear Mixed-Model Results regarding the Mediating Effect of Study Time on the Expectancy Effect on JOL and JOS in Experiment 3

Judgment Type	Belief group	Effect	Estimate	95% *CI*	*p*
JOL	All participants (*n* = 120)	Indirect effect	0.26	[0.14, 0.38]	< .001
		Proportion mediated	.04	[.02, .07]	< .001
	Expectancy effect (*n* = 75)	Indirect effect	0.20	[0.06, 0.36]	.004
		Proportion mediated	.04	[.01, .07]	.004
	Inconsistency effect (*n* = 32)	Indirect effect	0.19	[0.03, 0.39]	.018
		Proportion mediated	.04	[.01, .09]	.018
	No effect (*n* = 13)	Indirect effect	0.63	[0.10, 1.25]	.016
		Proportion mediated	.05	[.01, .10]	.016
JOS	All participants (*n* = 120)	Indirect effect	–0.12	[–0.25, 0.01]	.065
		Proportion mediated	–.01	[–.02, .0004]	.065
	Expectancy effect (*n* = 70)	Indirect effect	–0.06	[–0.23, 0.10]	.472
		Proportion mediated	–.004	[–.01, .01]	.472
	Inconsistency effect (*n* = 25)	Indirect effect	–0.47	[–0.80, –0.20]	< .001
		Proportion mediated	–.07	[–.14, –.03]	< .001
	No effect (*n* = 25)	Indirect effect	0.17	[–0.19, 0.54]	.346
		Proportion mediated	.01	[–.01, .02]	.346

Note. Displayed are the indirect effects of source–item expectancy on Judgment of Learning (JOL) and Judgement of Source (JOS) mediated by study time and the respective proportions mediated by study time. CI = Quasi-Bayesian confidence interval. Mediation analyses were carried out using the R package mediation (Tingley et al., 2014). Estimates are presented for the full sample and separately for participants who a priori expressed a belief in an expectancy effect, an inconsistency effect, or no effect (on item memory or source memory, respectively). For source–item expectancy, 0 = unexpected, 1 = expected. Study times were trimmed, logarithmized and centralized.

Appendix

Table A1

Linear Mixed-Model Results regarding the Influence of Source–Item Expectancy, Judgment Type, and A-Priori Beliefs on Metamemory Judgments in Experiments 2 and 3

Experiment		Estimate	95% *CI*	*df*	*t*	*p*
2	Intercept	54.50	[51.40, 57.60]	109	43.42	< .001
	Expectancy	5.69	[4.55, 6.83]	$1.218*10^4$	9.77	< .001
	Judgment Type	–6.57	[–7.70, –5.44]	$1.220*10^4$	11.38	< .001
	Belief	–0.02	[–0.08, 0.05]	$1.048*10^4$	0.54	.587
	Expectancy × Judgment Type	9.49	[7.91, 11.08]	$1.218*10^4$	11.75	< .001
	Expectancy × Belief	0.07	[0.003, 0.13]	$1.218*10^4$	2.07	.039
	Judgment Type × Belief	–0.19	[–0.25, –0.13]	$1.224*10^4$	6.13	< .001
	Expectancy × Judgment Type × Belief	0.17	[0.09, 0.25]	$1.218*10^4$	4.18	< .001
3	Intercept	51.91	[49.10, 54.73]	137	36.14	< .001
	Expectancy	5.48	[4.49, 6.48]	$1.523*10^4$	10.77	< .001
	Judgment Type	–2.03	[–3.03, –1.02]	$1.524*10^4$	3.96	< .001
	Belief	0.07	[0.02, 0.13]	$1.071*10^4$	2.64	.008
	Expectancy × Judgment Type	8.01	[6.59, 9.42]	$1.523*10^4$	11.11	< .001
	Expectancy × Belief	0.03	[–0.2, 0.07]	$1.523*10^4$	1.06	.289
	Judgment Type × Belief	–0.22	[–0.27, –0.17]	$1.526*10^4$	8.68	< .001
	Expectancy × Judgment Type × Belief	0.19	[0.12, 0.26]	$1.523*10^4$	5.34	< .001

Note. The estimates represent unstandardized regression weights. Analyses were performed with the R procedures lme4 and lmerTest with restricted maximum likelihood estimation. For source–item expectancy, 0 = unexpected, 1 = expected. For judgment type, 0 = JOL, 1 = JOS. CI = confidence interval.

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Table B1

Descriptive Results for Post-Test Memory Judgments in Experiments 2 and 3

Experiment	Judgment type	expected	unexpected
Experiment 2	item memory	59.40 [58.00, 60.79]	56.84 [55.45, 58.24]
	source memory	55.17 [53.52, 56.82]	50.68 [49.03, 52.33]
Experiment 3	item memory	66.15 [64.80, 67.50]	66.53 [65.17, 67.88]
	source memory	61.19 [59.81, 62.57]	59.17 [57.79, 60.55]

Note. Judgments were percentage ratings ranging from 0 to 100. Means and 95% within-subjects confidence intervals (in brackets, Loftus & Masson, 1994) are presented for item-memory and source-memory judgments for expected and unexpected source–item pairs. Significance tests are in Table B2.

Table B2

Inference Statistics for Post-Test Memory Judgments in Experiments 2 and 3

Experiment		$F(1, 95)$	p	η_p^2
Experiment 2	Expectancy	6.65	.011	.07
	Judgment type	12.02	.001	.11
	Expectancy × Judgment type	1.85	.177	.02
Experiment 3	Expectancy	0.46	.501	.004
	Judgment type	37.76	< .001	.24
	Expectancy × Judgment type	3.64	.064	.03

Note. Results of a 2 × 2 ANOVA with the within-subjects factors expectancy (expected vs. unexpected) and judgment type (item memory vs. source memory) with post-test memory judgments as the dependent variable. Inference statistics are reported for both factors and their interaction. Descriptive statistics are in Table B1.

References

Loftus, G. R., & Masson, M. E. J. (1994). Using confidence intervals in within-subject designs. *Psychonomic Bulletin & Review*, *1*, 476–490. doi: 10.3758/BF03210951