Truth Feels Easy: Knowing Information is True Enhances Experienced Processing Fluency

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Nahon, L. S., Teige-Mocigemba, S., Reber, R., & Greifeneder, R. (2021). Truth feels easy: Knowing information is true enhances experienced processing fluency. *Cognition*, *215*, Advance online publication. https://doi.org/10.1016/j.cognition.2021.104819

Author Note

Declarations of interest: none.

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Abstract

Information is more likely believed to be true when it feels easy rather than difficult to process. An ecological learning explanation for this fluency-truth effect implicitly or explicitly presumes that truth and fluency are positively associated. Specifically, true information may be easier to process than false information and individuals may reverse this link in their truth judgements. The current research investigates the important but so far untested precondition of the learning explanation for the fluency-truth effect. In particular, five experiments (total $N = 712$) test whether participants experience information known to be true as easier to process than information known to be false. Participants in Experiment 1a judged true statements easier to read than false statements. Experiment 1b was a preregistered direct replication with a large sample and again found increased legibility for true statements—importantly, however, this was not the case for statements for which the truth status was unknown. Experiment 1b thereby shows that it is not the actual truth or falsehood of information but the *believed* truth or falsehood that is associated with processing fluency. In Experiment 2, true calculations were rated as easier to read than false calculations. Participants in Experiment 3 judged it easier to listen to calculations generally known to be true than to calculations generally known to be false. Experiment 4 shows an effect of truth on processing fluency independent of statement familiarity. Discussion centers on the current explanation for the fluency-truth effect and the validity of processing fluency as a cue in truth judgments.

Keywords: processing fluency, truth effect

1 Truth Feels Easy: Knowing Information is True Enhances Experienced Processing Fluency

The rising phenomenon of fake news (e.g., Allcott & Gentzkow, 2017; Tasnim et al., 2020) culminating in "infodemics," in which false information spreads like a virus entailing serious consequences (Tedros Adhanom Ghebreyesus, 2020), has put a spotlight on how individuals come to judge information as true or false (e.g., The News Literacy Project, n.d.; World Health Organization, 2021). Yet the actual truth or falsehood of information is not the only factor that influences people's judgments of truth: Independent of its actual truth status, information that feels easier to process is more likely believed to be true (e.g., Dechêne et al., 2010; Reber & Schwarz, 1999). For example, information is more likely believed to be true when repeated (vs. seen for the first time; Begg et al., 1992; Garcia-Marques et al., 2015; Hasher et al., 1977; for a meta-analysis, see Dechêne et al., 2010) or when written in high (vs. low) contrast against its background (Hansen et al., 2008; Reber & Schwarz, 1999).

To explain this *fluency-truth effect*, researchers proposed an ecological learning account and argued that individuals positively associate truth and processing fluency, which is the ease or difficulty with which information can be processed. More specifically, it has been speculated that true information is easier to process than false information (i.e., truth and fluency are positively associated; Scholl et al., 2014; Unkelbach, 2007; Unkelbach & Greifeneder, 2013a). The fluency-truth effect results from reversing this link and extrapolating from processing ease (difficulty) to truth (falsehood). Previous research on the fluency-truth effect has generally focused on this reverse inference (i.e., showing that fluency affects truth judgments) and left the presumed precondition for the explanation of the fluency-truth effect—that true statements can be processed more fluently—untested. The current research investigates this critical precondition of an ecological learning explanation for the fluency-truth effect. Moreover, the current research tests whether it is the actual truth or falsehood of information that is associated with processing fluency or its *believed* truth status.

In what follows, we first turn to the existing evidence and theoretical frameworks of how individuals use processing fluency as a cue in (truth) judgments and discuss how fluency may be a valid cue as well as a bias in (truth) judgments. We then examine different explanations for why individuals use processing fluency in truth judgments. Lastly, we review prior theorizing and evidence for why a positive association between truth and fluency is a plausible assumption and suggest that it is believed truth rather than actual truth that should be positively associated with processing fluency.

1.1 The Use of Processing Fluency as a Cue in (Truth) Judgments

Processing fluency impacts a large variety of human judgments: Fluently processed stimuli are believed to be more familiar (e.g., Jacoby & Whitehouse, 1989; Whittlesea, 1993), more frequent (e.g., Greifeneder & Bless, 2007; Tversky & Kahneman, 1973), of higher quality (e.g., Greifeneder et al., 2010; Oppenheimer, 2006), and are generally preferred (e.g., Reber et al., 1998) to stimuli that are disfluently processed (for reviews, see Greifeneder et al., 2011; Reber & Greifeneder, 2017; Schwarz, 2004a; Schwarz & Clore, 2007). Most importantly for the present research, they are more likely believed to be true rather than false (e.g., Begg et al., 1992; Reber & Schwarz, 1999; for reviews, see Alter & Oppenheimer, 2009; Dechêne et al., 2010). Given these findings, one might argue that the feeling of processing ease itself inherently entails the meaning of familiarity, frequency, quality, truth, and so on. Contrasting this idea, however, prior research indicates that fluency is initially an unspecific feeling of processing ease or difficulty (e.g., Jacoby et al., 1989; Whittlesea, 1993; Whittlesea & Williams, 2000). Once people look for a cause of this unspecific feeling, they *attribute* it to a specific source (attribution account), for example, to the truth status of some piece of information such as "Sivas is in Greece." This process can be further differentiated into attributing processing fluency to a mental process, for example, reading a statement, and then interpreting the meaning of the fluency experience, for example, as telling something about the respective statement's truth status (Unkelbach & Greifeneder, 2013a). More specifically, individuals might interpret processing ease (difficulty) as an indication of truth (falsehood), which implies a decision about a judgment dimension (i.e., truth) and the direction of the association (i.e., processing ease being interpreted as signaling truth and not falsehood). Importantly, the direction of the association is not inherent in the fluency experience itself but needs to be explained by factors apart from this experience. Oftentimes, both directions of influence are conceivable (e.g., Briñol et al., 2006; Corneille et al., 2020; Unkelbach, 2007). For example, although processing ease is generally interpreted as signaling truth, in specific contexts, it may in contrast be interpreted as signaling falsehood (Corneille et al., 2020). This underscores the notion that an unspecific experience is assigned meaning through attribution and interpretation.

Attribution and interpretation of the fluency experience renders fluency susceptible to misattribution or misinterpretation. There are many sources of fluency and fluency is always felt as a feeling of processing ease or difficulty (Alter & Oppenheimer, 2009; Greifeneder & Bless, 2018). Thus, individuals might confuse the source of the fluency experience and misattribute the feeling of fluency to the wrong source. Consider, for example, the availability heuristic which holds that individuals use ease of recall (i.e., retrieval fluency) to judge the frequency of a judgment target (Tversky & Kahneman, 1973). People might use the ease with which they can recall examples of divorced couples, for instance, to estimate divorce rate. Availability is a valid cue to use in frequency judgments since frequent entities are generally easier to recall (Tversky & Kahneman, 1973). In other words, frequency is associated with retrieval fluency. Retrieval fluency, however, can be influenced by factors other than frequency, for example the recency of events. For instance, we may infer that there must be many divorced couples not because we have heard of many divorces, but because we have heard of some divorces recently. Thus, when retrieval fluency is attributed to or interpreted as signaling frequency, these other factors—such as recency—also influence frequency judgments, leading to biases in judgments (Tversky & Kahneman, 1973), even though retrieval fluency in general is a valid cue when judging frequency.

1.2 Why Do Individuals Use Processing Fluency as a Cue in (Truth) Judgments?

1.2.1 Attribution Account

Prior research suggests that the context vastly determines how people interpret the unspecific feeling of processing fluency (Jacoby et al., 1989; Jacoby et al., 1992). For instance, the attribution account of implicit memory (Jacoby et al., 1989) holds that individuals interpret fluency in accordance with the question being asked. If participants are asked which of two things they prefer, people may interpret fluency as a cue for their own preference; if they are asked whether they have already seen a stimulus, people may interpret fluency as a cue for familiarity (Whittlesea et al., 1990), and so on. The attribution account would thus suggest that individuals use fluency as a cue in truth judgments because they are asked to judge the truth of a statement. However, it remains unclear why individuals interpret processing ease as a cue for a statement being *more* and not *less* likely true, suggesting that the question being asked does not determine the direction of influence (Unkelbach & Greifeneder, 2013a). For instance, it has been shown that individuals interpret processing ease as signaling truth irrespective of whether they are asked to judge the *truth* or the *falsehood* of a statement (Corneille et al., 2020).

Other research corroborates the notion that the interpretation of the fluency experience is not completely free. (Mis-)attribution research, for instance, shows that elicited feelings do not influence all judgements equally (for a review, see Greifeneder et al., 2011). Specifically, the trigger of a feeling seems to elicit experiences of some specificity, more relevant to some judgments than others (Ecker & Bar-Anan, 2019).

1.2.2 Ecological Learning Account

A second account follows from Brunswik's (1952) lens model. When looking at the fluency-truth effect from this perspective, truth is the distal criterion, which has to be judged, and processing fluency is the proximal cue, which individuals rely on in their truth judgments. The association between a truth judgment and the fluency experience represents cue utilization; the association between truth and fluency denotes what Brunswik termed "ecological validity." Following from the Brunswikian framework, the ecological learning account assumes that individuals use processing fluency as a cue for truth (fluency-truth effect) because of a learned positive association between the two variables. According to this ecological learning account, a positive association between truth and fluency exists in the environment and individuals may have learned this ecological validity and therefore use fluency as a cue in truth judgments, that is, extrapolating from processing ease (difficulty) to truth (falsehood). This ecological learning account could also explain the various influences that fluency has on different judgements (Unkelbach & Greifeneder, 2013a). The respective ecology sets the stage for the specific influence of fluency. This ecology might be set, for example, by the question an experimenter is asking. The direction of influence should then follow the learnt direction of the existing association in people's environment.

To empirically bolster this ecological learning account, Unkelbach (2007) first exposed participants to a *negative* association between truth and processing fluency: Statements written in high (low) contrast were false (true). In the subsequent test phase, participants then showed a heightened tendency to rate disfluent (i.e., low contrast) compared to fluent (i.e., high contrast) statements as true, which constitutes a pattern of results opposite to those normally observed in fluency-truth studies. Individuals thus seem to be able to learn a specific association between truth and fluency, which they then exploit when judging truth. Interestingly and further fostering this ecological learning account, individuals seem to be able to learn different directions of the association between truth and fluency for different ecologies (Corneille et al., 2020). However, people by default tend to infer truth from processing ease and falsehood from processing difficulty (e.g., Corneille et al., 2020; Reber & Schwarz, 1999). Importantly, for such a default to exist, the necessary precondition holds that people can learn a *positive* association between truth and processing fluency (ecological validity) in their natural environment. In other words, it needs to be assumed that people regularly experience that they can process true (vs. false) information more fluently. Furthermore, if the learning explanation holds true, individuals bring along an extensive learning history with many observations, likely rendering the learnt inference rule reliable and relatively accurate (Greifeneder et al., 2013). Interestingly, the interpretations of processing fluency are socially shared in that individuals express high consensus about the meaning of processing fluency (Greifeneder et al., 2013; Schwarz, 2004b). This suggests that the contingencies in people's environment are similar, leading to similar learning experiences.

Thus, a learnt positive truth-fluency association is a plausible and parsimonious explanation for the fluency-truth effect. Furthermore, a learnt positive truth-fluency correlation would be compatible with the attribution account. It could explain why individuals interpret fluency in truth judgements the way they do. Thus, the ecological learning explanation does not exclude but rather complement the attributional explanation for the fluency-truth effect, which emphasizes the importance of context. The role of context (e.g., the question being asked) is acknowledged by the ecological learning explanation and it can further specify the direction of influence by taking into account the context in a broader sense, meaning people's natural environment.

1.2.2.1 Validity

A positive association between truth and fluency in people's environment is also a prerequisite for the validity of processing fluency as a cue in truth judgments. Processing fluency may be a valid cue as well as a bias in judgment (Jacoby et al., 1989). For a cue to be valid, it needs to be associated with the distal criterion (i.e., ecological validity; Hertwig et al., 2008; Tversky & Kahneman, 1973). Furthermore, the direction or sign of the association needs to be the same as for cue utilization. Thus, whether individuals positively associate truth with processing fluency (i.e., whether there exists a positive correlation between truth and fluency) determines whether processing fluency is a valid cue in truth judgments. This truth-fluency correlation constitutes an essential but largely missing piece of knowledge in the current state of the field (Fiedler, 2013; Herzog & Hertwig, 2013). A missing correlation between truth and fluency in people's environment would prevent fluency from being a valid cue for truth and would essentially confine it to an undesirable influence in truth judgments (Unkelbach & Greifeneder, 2013a).

Ecological validity in Brunswik's lens model can be further differentiated by introducing a mechanism for the criterion-cue association (Goldstein & Gigerenzer, 2002) which can explain why the criterion (i.e., truth) is associated with the cue (i.e., processing fluency). In the following, different mechanisms are discussed which could explain the proposed positive truth-fluency association. Additionally, we argue that it is believed rather than actual truth which should be correlated with processing fluency but that this likely leads to actual truth being associated with processing fluency, too.

1.3 The Positive Association Between Truth and Processing Fluency

One possible reason for why truth and fluency might be positively associated may be that individuals might repeat information that they believe to be true in contrast to false more often. Researchers have speculated that the increased repetition of information believed to be true may lead to more fluent processing (e.g., Scholl et al., 2014; Unkelbach, 2007) since repetition increases processing ease.

A second reason for why true as opposed to false information might be easier to process is consistency with prior knowledge. Information that is consistent compared to inconsistent with other information one has already accepted as true, is easier to process (Greifeneder & Schwarz, 2014; Topolinski & Strack, 2009; Winkielman et al., 2012). For verbal statements such as "Rome is in Italy" this would most probably mean that through their relation, "Rome" associatively primes "Italy" (Winkielman et al., 2012). Looking at this line of thought from another angle, this argument corresponds to coherence of true information leading to ease of processing. The statement "The word TIR has three letters," for example, is semantically coherent because it is consistent with individuals' semantic knowledge about language. Semantic coherence in turn has been shown to lead to more fluent processing (Topolinski & Strack, 2009).

For logical or mathematical statements such as "2 x 3 = 6," associative priming of "6" through "2 x 3" would be one explanation for enhanced processing fluency. There is, however, also the possibility that this kind of mathematical relation could lead to more fluent processing via logical coherence (i.e., a higher order form of consistency; Winkielman et al., 2012) because the calculation conforms to the axioms set in mathematics (but see Winkielman et al., 2012).

Strictly speaking, repetition and consistency are connected to *believed* rather than to actual truth or falsehood, and it is thus believed truth versus falsehood that is associated with processing fluency: Regarding repetition, information is repeated when the communicator believes the information is true. Following this line of argument, the more individuals believe a piece of information to be true, the more often should it be repeated, thus becoming easier to process. Likewise, consistency with prior knowledge corresponds to believed rather than to inherent truth of information since prior knowledge reflects people's own personal belief system rather than the objective state of the world. However, if a positive believed truth-fluency correlation exists, it is most likely that actual truth will be positively associated with fluency, too, since we would expect believed truth and actual truth to correlate. Yet this is a question that awaits further research on the ecology of truth (see Reber & Unkelbach, 2010, for a few examples of such studies).

1.4 Preliminary Evidence for a Positive Truth-Fluency Association

Supporting the theorizing that truth and fluency are positively associated, some studies are suggestive at the empirical level. For instance, individuals answer faster to questions about the truth or familiarity of statements if these statements are true as opposed to false (Shtulman & Valcarcel, 2012; Unkelbach & Stahl, 2009), suggesting more fluent processing and thus a positive association between truth and fluency. Yet processing fluency is typically conceptualized as the *subjective experience* of ease or difficulty when processing information and is thus inherently subjective (e.g., Alter & Oppenheimer, 2009; Schwarz & Clore, 2007). Response latencies, by contrast, measure objective answering speed, for which it is uncertain how well it captures the subjective experience of fluency (e.g., Reber et al., 2004). One reason for this uncertainty is that response latencies may be influenced by factors other than processing fluency. Unkelbach (2007), for example, unexpectedly found higher response latencies for repeated as compared to new statements although repetition enhances processing fluency (Dechêne et al., 2010; Whittlesea et al., 1990). Moreover, response latencies can indicate fluency at a specific stage of a cognitive process (Reber et al., 2004). When response latencies as fluency measure and judgments do not correlate, the reason may be that fluency was measured at the wrong perceptual stage (for examples, see Reber, 2012). Evidence with subjective measures—measuring processing fluency more comprehensively than response latencies do—is thus essential to further investigate the association between truth and fluency.

1.5 The Present Research

Against this background, we test the notion that information believed to be true is subjectively experienced as easier to process than information believed to be false. In five experiments, we investigate this assumption with different forms of information and different types of processing fluency. Following major research in the field (e.g., Jacoby et al., 1988; Reber & Schwarz, 1999; Unkelbach, 2007; Whittlesea et al., 1990), we examine judged *perceptual* fluency—specifically judged visual and acoustic fluency—as one prominent form of processing fluency. We understand processing fluency as the superordinate construct, which spans subordinate forms of fluency such as perceptual or conceptual fluency under its umbrella (Alter & Oppenheimer, 2009). However, we do not aim to investigate whether believed truth influences the *objective perception* of information. Instead, our measure assesses how easy or difficult participants *judge* the perception of different information. It has to be noted that besides actual perception processes, *conceptual* fluency—the ease or difficulty with which the meaning of information is processed (Labroo et al., 2008)—may also influence judged readability and audibility. In all experiments, we use a single-item measure for judged perceptual fluency capturing the *difficult-easy* continuum, which has high validity (Graf et al., 2018).

Experiments 1 and 2 test whether true in comparison to false statements and calculations are rated as easier to read. Experiment 3 tests whether true in comparison to false calculations are rated as easier to listen to. Since it is believed rather than actual truth which should be positively correlated with processing fluency, we expected this difference in judged perceptual fluency to be more pronounced for information for which truth versus falsehood is generally known rather than unknown. Experiment 4 aims at excluding the possibility that the sole reason for believed truth leading to higher fluency might be familiarity through repetition. Instead, processing ease of true statements may also stem from semantic coherence. We report all experiments, measures, manipulations, and data exclusions. Data and analysis scripts can be found at http://dx.doi.org/10.23668/psycharchives.4894 and http://dx.doi.org/10.23668/psycharchives.4895, respectively (Nahon et al., 2021).

2 Experiments 1a and 1b

Experiment 1a tested the hypothesis that participants rate true statements as easier to read than false statements. This difference in rated legibility for true versus false statements should be apparent for statements for which truth versus falsehood is generally known but less pronounced (or non-existent) for statements for which truth versus falsehood is generally unknown. To test these assumptions, participants indicated how easy or difficult it was to read true and false statements of the form "City A is in Country B," for half of which the truth or falsehood is generally known (e.g., "Rome is in Italy" vs. "Havana is in Italy"), and for half of which the truth or falsehood is generally unknown (e.g., "Osorno is in Chile" vs. "Viacha is in Chile"; see also Reber & Schwarz, 1999). In addition, we manipulated the visual contrast with which the statements were presented. We introduced this manipulation of visual contrast for the sole reason that the task to rate legibility would make sense to participants. The manipulation of visual contrast was thus an auxiliary methodological means to prevent demand effects and is not related to the focal hypothesis of whether truth is associated with perceived fluency.

Experiment 1b was a preregistered direct replication of Experiment 1a with a large sample. The only difference to Experiment 1a was that we included an attention check. Preregistration protocol can be found at https://aspredicted.org/h6kf6.pdf.

2.1 Method

The pretest which we conducted to construct the materials for Experiments 1a and 1b as well as the listing of these materials can be found in the Supplemental Materials, along with a description of the power analyses.

2.1.1 Participants and Design

2.1.1.1 Experiment 1a

Participants were recruited through Amazon Mechanical Turk. Participation was technically impossible with small screen devices such as iPads, iPhones, or mobile devices running Android operating system. Eighty-one US participants completed the experiment and were offered a compensation of $0.45. Four participants were excluded from the analyses because they indicated not having participated seriously. Two additional participants were excluded because they reported having searched for presented cities or countries externally (e.g., on google), which constitutes a threat to internal validity given that the manipulation of knowledge requires that participants do not know the truth status of unknown cities. Of the remaining 75 participants, 37 were female and 38 male; the average age was 34.63 years (*SD* = 10.10, range: 22-63). Participants were randomly assigned to a 2 (statements set: 1 vs. 2) x 2 (truth: true vs. false) x 2 (knowledge: known vs. unknown) mixed factorial design with the factors truth and knowledge as repeated measures.

2.1.1.2 Experiment 1b

Design and procedure were the same as in Experiment 1a. As preregistered, we put a request for 400 participants on Amazon Mechanical Turk. Four hundred and three US participants completed the experiment and were offered a compensation of $0.50. As preregistered, the inclusion criteria were the same as in Experiment 1a. Ten participants were excluded from the analyses because they indicated not having participated seriously. Additional 70 participants were excluded because they reported having searched for presented cities or countries externally (e.g., on google). Of the remaining 323 participants, 128 were female, 194 male, and one person did not specify; the average age was 38.41 years (*SD* = 11.78, range: 18-71). Thirteen of these participants failed the attention check. As preregistered, we included them in the analyses, but we checked whether the significance and pattern of the reported results stay the same when excluding them, which was the case.

2.1.2 Materials

For the known statements in Experiments 1a and 1b, we selected eight statement packages of the pretest, resulting in 16 true and 16 false statements in total. The true pairs and their false counterparts were separated into two sets (Set 1 vs. Set 2), each set containing four true and four false pairs, with each city and country appearing only once per set, see Table S1.

Besides these known statements, additional four pairs of true and four pairs of false statements were added to each set, see Table S1. These statements were taken from Reber and Schwarz (1999) and contained less familiar cities, for which truth versus falsehood should generally be unknown (unknown statements). Reber and Schwarz (1999) did not find a significant influence of actual truth or falsehood of these unknown statements on truth judgments. Again, the unknown-false statements were constructed by switching countries between statements pairwise.

Overall, the four statement categories known-true, known-false, unknown-true, and unknown-false were each represented by eight statements per set. Additional two true and two false statements for the practice trials were constructed, for which we used different cities and countries, see Table S2. To obtain the two false statements for the practice trials, we switched the countries between two otherwise true statements. All cities and countries were used only once in the practice trials.

2.1.3 Procedure

Participants were welcomed and consented to participation. Instructions stated that participants would be presented with 32 statements of the form "City A is in Country B," that the brightness of these statements would vary, and that participants' task was to indicate how difficult or easy it is to read these statements. After having confirmed that they had read the instructions, participants were presented with the four practice trials. Practice statements were presented in random order and randomly in one of the four contrasts. Visual contrast was manipulated by varying the text color from light grey to black (RGB 210-210-210, 120-120-120, 90-90-90, and 0-0-0) against a white screen background. All statements were horizontally centered at the page's top. Below, on the same page, participants indicated how easy or difficult it was to read the respective statement with a slider ranging from 1 (*very difficult*) to 50 (*very easy*). The slider had no default answer value.

Next, participants completed the main trials with 32 statements from one of the two statement sets presented in random order. For the sake of task plausibility, visual contrast was manipulated on eight levels. The RGB combinations 210-210-210, 180-180-180, 150-150-150, 120-120-120, 90-90-90, 60-60-60, 30-30-30, and 0-0-0, again ranging from light grey to black, were randomly assigned to the statements, separately for each block of a certain kind (e.g., Set 1, unknown-true). Thus, there were always the same number of known-true, known-false, unknown-true, and unknown-false statements in each contrast condition. Everything else was identical to the practice trials. After that, participants indicated gender, age, English language proficiency, and their highest level of education completed. Participants were then asked whether they had participated seriously in the study and whether they had searched for any of the presented cities and countries externally (e.g., on google), and if so, for how many. Only in Experiment 1b, as an attention check, participants were asked to choose the option *not at all* on a scale ranging from 1 (*not at all*) to 9 (*very much*). In both experiments, participants also reported whether there was any reason not to use their data. Lastly, they reported their Amazon Mechanical Turk worker ID, were thanked, and were given the code for receiving compensation for participating in the experiment.

2.2 Results

Experiment 1a. We fitted a linear mixed-effects model with legibility as the dependent variable using the lme4 package version 1.1-17 (Bates et al., 2015) with the default optimizer "bobyqa" of this version and the lmerTest package version 3.0-1 (Kuznetsova et al., 2017) in R version 3.5.1 (R Core Team, 2018). The reported means and standard deviations are descriptive statistics computed in SPSS after calculating four means separately for the four statement categories. As fixed effects in the mixed-effects model, we included truth (true vs. false), knowledge (known vs. unknown), and their interaction. Participants, statements, and visual contrasts were treated as random factors.[1] This mixed linear model has the advantage of accounting for sampling variability of participants, statements, and contrasts (Judd et al., 2012). Within the specified random factors, we aimed for a maximal linear mixed model justified by the design (Barr et al., 2013) and included random intercepts for participants, statements, and contrasts as well as random slopes for truth, knowledge, and the truth X knowledge interaction based on participants and contrasts.[2]

[1] It would also be possible to include visual contrast as a fixed effect rather than as a random factor in the model. This might be interesting to examine the effect of visual contrast on legibility ratings and to see whether the effect of truth on legibility might depend on visual contrast (which we manipulated only to render the procedure plausible to participants). For the interested reader, additional exploratory models with visual contrast (Experiments 1a, 1b, 2, and 4) or noise (Experiment 3) as a fixed effect are reported in the Supplemental Materials. The pattern and significance of the results of these exploratory models were the same as with the main models reported here, except that for Experiment 1a, the interaction of truth X knowledge was significant.

[2] Note that the random factor statements is nested in truth and knowledge and these random slopes and their interaction were therefore not included in the model for the random factor statements.

After specifying and running the mixed-effects model, we used the lmerTest function anova() to calculate *F* statistics (Kuznetsova et al., 2017), so that the results of the fixed effects can be reported equivalent as for a conventional ANOVA but with an underlying mixed-effects model. As expected, the analyses revealed a significant main effect of truth, $F(1, 57.39$[3]$) = 5.94$, $p = .018$, with true statements being rated as easier to read than false statements ($M_{\text{true}} = 35.04$, $SD_{\text{true}} = 7.55$; $M_{\text{false}} = 33.84$, $SD_{\text{false}} = 8.14$, respectively).[4] There was no significant main effect of knowledge, $F(1, 48.30) = 1.88$, $p = .176$ ($M_{\text{known}} = 34.69$, $SD_{\text{known}} = 7.73$; $M_{\text{unknown}} = 34.19$, $SD_{\text{unknown}} = 7.62$) and no significant interaction of truth X knowledge, $F(1, 66.10) = 1.86$, $p = .177$, though a tendency for the hypothesized pattern emerged, see Figure 1 (a).[5]

[3] The R package lmerTest uses the Satterthwaithe's method for approximating denominator degrees of freedom (Kuznetsova et al., 2017). The Satterthwaithe's method for approximating degrees of freedom in mixed effects models is empirically investigated (e.g., Luke, 2017) and used by experts in the field (for more information see, e.g., Bolker, 2020). See Kuznetsova et al. (2017), Appendix A, for the algorithm's details.

[4] The calculation of standardized effect sizes or explained variance statistics bears problems for mixed-effects models because of the variance partitioning in linear mixed models (see, e.g., Rights & Sterba, 2019; Singer & Willett, 2003) and is to date still a topic of debate. We thus do not report standardized effect sizes for fixed effects terms such as main effects or interactions. However, we still decided in favor of mixed-effects models because they are superior to traditional ANOVAs in generalizing to new observations (Judd et al., 2012).

[5] We used newer R, lme4, and lmerTest versions for analyzing Experiments 1b and 4 than in Experiments 1a, 2, and 3. When running Experiment 1a's model with these newer versions,

Experiment 1b. As in Experiment 1a, we fitted a mixed-effects model with legibility as the dependent variable. We used the lme4 package version 1.1-23 (Bates et al., 2015) with the optimizer "bobyqa"[6] and the lmerTest package version 3.1-2 (Kuznetsova et al., 2017) in R version 4.0.0 (R Core Team, 2020). Descriptive statistics were computed as in Experiment 1a. As preregistered, we specified the same fixed effects as in Experiment 1a. In fact, we started with the same maximal model as in Experiment 1a. In contrast to Experiment 1a, however, the maximal model failed to converge. Non-convergence unfortunately constitutes a common problem with linear mixed-effects models (Barr et al., 2013). Attempting to tackle convergence issues by increasing the maximum limit of function evaluations, we derived a simplified model that converged and did not give out a warning message while keeping the goal of our analysis in mind. This resulted in the same final model as for the power analysis. We kept random intercepts for participants and statements as well as the random slope for truth based on participants since the effect of truth was our main effect of theoretical interest. Furthermore, we wanted to control for the variance accounted for by the different contrasts and thus included the random intercept for contrasts.

Running the function anova() to calculate *F* statistics for the specified fixed effects in the mixed-effects model revealed a significant main effect of truth, $F(1, 67.99) = 6.43$, $p = .014$, with true statements being rated as easier to read than false statements ($M_{true} = 36.42$, $SD_{true} = 7.41$;

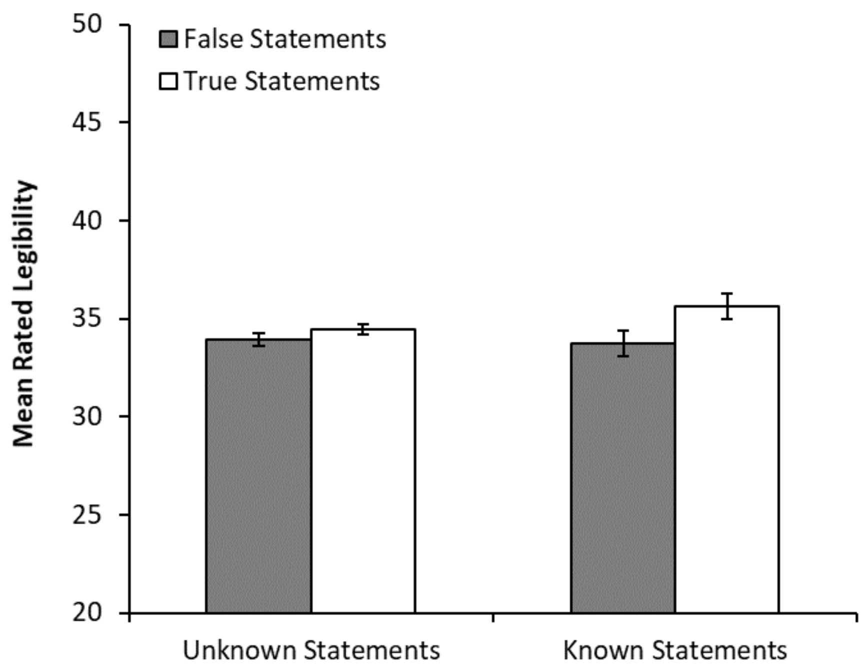
the model gave out a singular fit warning. Simplifying the model to the same specification as for Experiment 1b solved the singularity problem and the hypothesis test conclusions stayed the same.

[6] "Bobyqa" was used to render analysis procedures similar across experiments.

$M_{\text{false}} = 35.86$, $SD_{\text{false}} = 7.94$, respectively), as well as a significant main effect of knowledge, $F(1, 58.70) = 10.10$, $p = .002$, with known statements being rated as easier to read than unknown statements ($M_{\text{known}} = 36.47$, $SD_{\text{known}} = 7.62$; $M_{\text{unknown}} = 35.81$, $SD_{\text{unknown}} = 7.78$, respectively). However, both main effects were qualified by the hypothesized significant truth X knowledge interaction, $F(1, 58.70) = 9.39$, $p = .003$. Inspection of Figure 1 (b) reveals a pattern of results in support of the suggested hypothesis. As preregistered, to specifically test the hypothesis that participants rate known-true statements as easier to read to than known-false statements, simple effects of truth at the two levels of knowledge were analyzed using the R package phia version 0.2.1 (Martinez, 2015). These tests showed that known-true statements were rated as significantly easier to read than known-false statements, $\chi^2(1, N = 323) = 15.57$, $p < .001$ ($M_{\text{known-true}} = 37.07$, $SD_{\text{known-true}} = 7.64$; $M_{\text{known-false}} = 35.87$, $SD_{\text{known-false}} = 8.78$, respectively). Unknown-true statements, however, did not significantly differ from unknown-false statements in their rated legibility, $\chi^2(1, N = 323) = 0.06$, $p = .809$ ($M_{\text{unknown-true}} = 35.77$, $SD_{\text{unknown-true}} = 7.90$; $M_{\text{unknown-false}} = 35.84$, $SD_{\text{unknown-false}} = 7.91$, respectively).

2.3 Discussion

Experiment 1a supports the hypothesis that true statements are subjectively processed more fluently than false statements. In Experiment 1a, descriptive support was obtained for the hypothesis that the difference in rated legibility should be apparent for statements for which truth versus falsehood is generally known but less pronounced (or non-existent) for statements for which truth versus falsehood is generally unknown. Though suggestive, this interaction effect failed to surpass conventional levels of significance, most likely because statistical power was too low. To increase confidence in the obtained findings, we conducted a preregistered and well-powered direct replication of Experiment 1a. Experiment 1b not only replicates the finding that a


False Statements
True Statements
Mean Rated Legibility
50
45
40
35
30
25
20
Unknown Statements
Known Statements

b

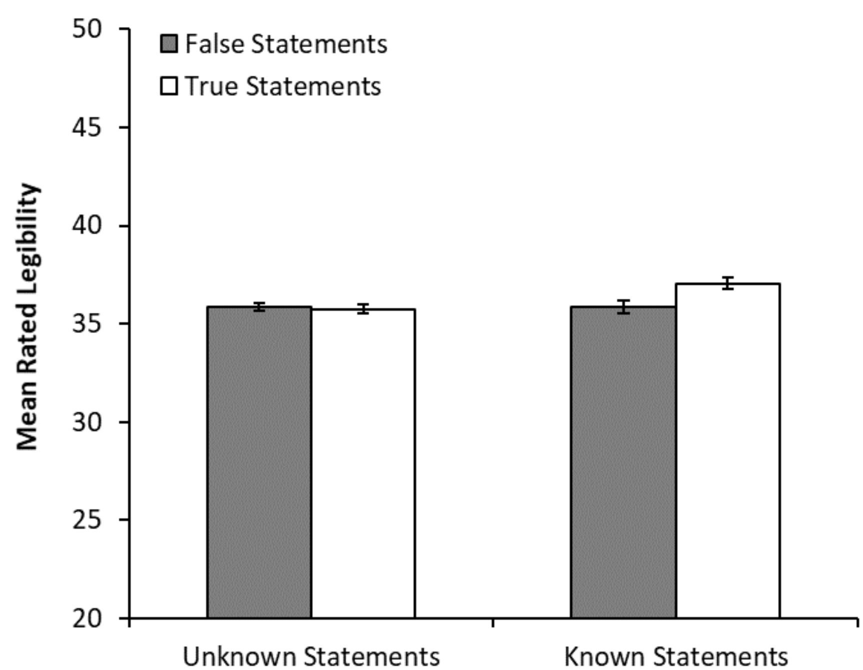

False Statements
True Statements
Mean Rated Legibility
50
45
40
35
30
25
20
Unknown Statements
Known Statements

Figure 1. Mean rated legibility by truth and knowledge in Experiment 1a (a) and 1b (b). Unknown statements are statements for which truth versus falsehood should be generally unknown. For known statements, it is generally known whether they are true or false. Participants indicated with a slider ranging from 1 (*very difficult*) to 50 (*very easy*) how easy or difficult it was to read the respective statement. Error bars represent within-subject standard errors for differences between means according to O'Brien and Cousineau (2014).

the truth of statements enhances processing ease but extends Experiment 1a with clear evidence that the influence of truth depends on knowledge. Consistent with our theorizing, the significant interaction truth X knowledge and the respective simple main effects suggest that the truth or falsehood of information primarily influences subjectively experienced processing fluency when truth or falsehood of the information is known.

Adhering to the preregistered exclusion criteria, we excluded 80 participants in Experiment 1b, resulting in an analyzed sample of 323 participants. Note that a data simulation power analysis for 300 participants, which we conducted *a priori* following the same procedure as for the power analysis reported in the Supplemental Materials for 400 participants, resulted in a power of 1 for the truth main effect and a power of .84 for the truth X knowledge interaction effect, with five of the 1'000 models giving out a singular fit message.

In Experiment 1b, the known statements were rated as easier to read than the unknown statements. The main effect of knowledge is not of primary interest; however, the finding that believed truth as well as knowledge influence judged perceptual fluency is in line with and supports prior theorizing conceptualizing processing fluency as a unitary experience on the continuum of processing ease to difficulty with many possible sources from different cognitive processes (Alter & Oppenheimer, 2009).

3 Experiment 2

In Experiment 2, we aimed to *conceptually* replicate the finding that true statements are subjectively processed more fluently when they are known. We changed the materials in Experiment 2 and asked participants to indicate how easy or difficult it is to read mathematical calculations of the form "A x B = C." For half of these calculations, the presented result was correct (i.e., true calculations) and for the other half false. Half of the calculations are generally known (e.g., 2 x 3 = 6 or 2 x 3 = 10) and it should be obvious whether the presented result is true or false. The other half of the calculations were difficult (e.g., 523 x 148 = 77404 or 523 x 148 = 77024), and it therefore is most probably unknown to participants whether the presented result is true or false. To make sure that unknown-true calculations would not seem more plausible than unknown-false calculations and participants could not easily guess the falsehood of unknown-false calculations, the first and the last digit of unknown-false calculations were correct.

Again, we hypothesized that participants rate correct calculations as easier to read than false calculations. This difference in rated legibility for true versus false calculations should be apparent for calculations for which truth versus falsehood is generally known but less pronounced (or non-existent) for calculations for which truth versus falsehood is generally unknown. As in Experiments 1a and 1b, we manipulated the visual contrast of the calculations in order to create a sensible task for participants, albeit the effect of contrast on legibility ratings is of no theoretical interest here.

3.1 Method

3.1.1 Participants and Design

Participants were recruited through Amazon Mechanical Turk. Since the materials of Experiment 2 were different to those in Experiment 1a, we chose not to base the power analysis for Experiment 2 on Experiment 1a. Instead, we started with the same parameters as in Experiment 1a, suggesting a sample size of 74 to detect a medium to large truth X knowledge interaction effect (for details, see the Power Analysis section for Experiment 1a in the Supplemental Materials).

Eighty US participants completed the experiment for a compensation of $0.45. Three participants were excluded from the analyses because they indicated not having participated seriously. Two additional participants were excluded because they reported having used a calculator to verify calculated results and thus violated instructions. Of the remaining 75 participants, 27 were female, 47 male, and one person did not specify. The average age was 33.60 years (SD = 9.66, range: 20-60). Participants were randomly assigned to a 2 (calculations set: 1 vs. 2) x 2 (truth: true vs. false) x 2 (knowledge: known vs. unknown) mixed factorial design with the factors truth and knowledge as repeated measures.

3.1.2 Materials and Procedure

To construct calculations for which it should be generally known whether the presented result is true or false (known calculations), we first generated 16 multiplications with one-digit numbers with the true result ranging from 1 to 12 (e.g., 2 x 3 = 6, 2 x 5 = 10). We then switched results between calculations pairwise to obtain 16 false calculations (e.g., 2 x 3 = 10, 2 x 5 = 6), resulting in 32 known calculations in total. These calculations were separated into two sets, with one multiplication appearing only once per set, either with a true or with a false result; pairs of switched calculations remained together in a set, either both true or both false. For the calculations for which it should be generally unknown whether the presented result is true or false (unknown calculations), we first generated 16 multiplications with three-digit numbers (e.g., 916 x 358 = 327928), which were randomly generated by sampling three one-digit numbers without replacement between 1 and 9. For each of these calculations, we then generated a false result by replacing all digits of the true result except for the first and the last. To that end, as many one-digit numbers as necessary per result were sampled without replacement from 0 to 9. These 32 unknown calculations were assigned to the two sets, with one multiplication appearing only once per set, either with a true or with a false result.

There was one practice trial for each knowledge X truth category, that is: two one-digit and two three-digit multiplications, of which two were correct and two false. The construction procedure was the same as for the main trials, except that for the known-false calculation, a result was sampled randomly from 1 to 9. All calculations are listed in the Supplemental Materials, Tables S3 to S5.

The procedure was the same as in Experiments 1a and 1b except that the statements were calculations, with the four categories known-true, known-false, unknown-true, and unknown-false remaining the same, each represented by eight calculations per set; again, calculations were presented in random order. Participants were told that they would see mathematical calculations of the form "A x B = C." Participants indicated how easy or difficult it was to read each calculation with a slider ranging from 1 (*very difficult*) to 50 (*very easy*). They were asked not to use a calculator to verify the results and later probed whether they had done so. Participants who reported having used a calculator were asked to write down for how many one-digit and how many three-digit multiplications they had done so, and for how many of the checked calculations they found the printed result to be false.

3.2 Results

The analytical procedure was the same as in Experiment 1a. In contrast to Experiment 1a, however, fitting the maximal model resulted in a convergence warning. Since increasing the maximum limit of function evaluations was not effective, we pursued two separate analyses to test for generalization of the effects of truth and knowledge separately. This procedure is recommended by Barr and colleagues (2013) for studies with multiple fixed effects of theoretical interest and convergence problems. The specified fixed effects were the same in both models as described for Experiment 1a. Again, participants, calculations, and contrasts were treated as random factors, and random intercepts remained the same as before. However, in Model 1, we included the random slopes for truth based on participants and contrasts, whereas in Model 2, we included the random slopes for knowledge based on participants and contrasts. Since the effect of truth constitutes our main theoretical interest, we hereafter report the results based on Model 1. However, the pattern of the reported results and their significance was the same in both models.

Again running the function anova() to calculate F statistics for the fixed effects, Model 1 revealed a significant main effect of truth, $F(1, 52.98) = 4.62$, $p = .036$, with true calculations being rated as easier to read than false calculations ($M_{\text{true}} = 34.95$, $SD_{\text{true}} = 7.64$; $M_{\text{false}} = 34.07$, $SD_{\text{false}} = 8.36$, respectively), as well as a significant main effect of knowledge, $F(1, 2241.00) = 63.43$, $p < .001$, with known calculations being rated as easier to read than unknown calculations ($M_{\text{known}} = 36.05$, $SD_{\text{known}} = 7.91$; $M_{\text{unknown}} = 32.97$, $SD_{\text{unknown}} = 10.59$, respectively). There was no significant interaction of truth X knowledge, $F(1, 2241.00) = 2.20$, $p = .138$, although there was a tendency for the hypothesized pattern, see Figure 2.[7]

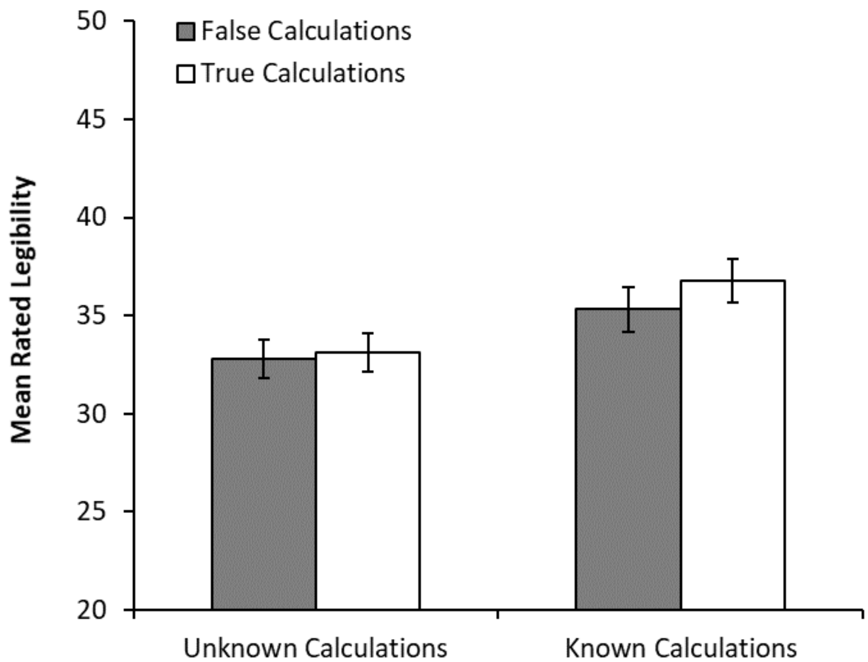


Figure 2. Mean rated legibility by truth and knowledge in Experiment 2. Unknown calculations are calculations for which truth versus falsehood should be generally unknown. For known calculations, it should be generally known whether they are true, i.e. correct, or false. Participants indicated with a slider ranging from 1 (*very difficult*) to 50 (*very easy*) how easy or difficult it was to read the respective calculation. Error bars represent within-subject standard errors for differences between means according to O'Brien and Cousineau (2014).

[7] Running Experiment 2's Model 1 with the newer package versions gave out a singular fit warning. Simplifying the model to the same specification as for Experiment 1b still gave the warning. Additionally dropping the random intercept for calculations resolved the problem and the hypothesis test conclusions stayed the same.

3.3 Discussion

Experiment 2 conceptually replicates Experiments 1a and 1b and supports the hypothesis that true calculations are subjectively processed more fluently than false calculations, supporting our main hypothesis. However, the tendency of a stronger effect of truth for known than for unknown calculations emerged only descriptively. Dovetailing with the results of Experiment 1b, the unknown, more difficult calculations were also rated as more difficult to read, which is in line with the concept of fluency as one integrated experience.

For some of the known-true calculations and one of the known-false calculations, the result of the equation was the same number that was used for one of the multipliers in the equation (e.g., 1 x 2 = 2). To investigate the possibility of numerical priming, we reran our main analysis using an index without these calculations. The main effect that true calculations were rated as easier to read than false calculations remained, thus indicating that numerical priming had not been the main driver of the effect.

4 Experiment 3

Experiments 1a and 2 show that true in comparison to false information is subjectively easier to process. However, both experiments are indecisive concerning the hypothesis that it is not the inherent truth status of information but the believed (i.e., known) truth versus falsehood that influences processing fluency. Experiment 1b supports this assumption. We decided to investigate this interaction hypothesis further in Experiment 3 using a different form of judged perceptual fluency.

Although precautions were taken in Experiment 2 to ensure that what should be unknown is really unknown, some participants may have still guessed the correctness of the unknown equations, for example by applying rules of thumb. In Experiment 3, we therefore took yet a different approach and used a different fluency measure, namely acoustic fluency, that we adopted from Jacoby et al. (1988). Participants were *orally* presented with mathematical calculations of the form "A x B = C," which were read aloud by a computer voice. Participants indicated how easy or difficult it was to listen to the calculations. Equations were presented against a background of white noise, which differed in intensity so that the task to judge how easy or difficult it is to listen to the calculations makes sense to participants. The white noise conceptually parallels the visual contrast manipulation in the previous experiments and serves as an auxiliary methodological means to render the task sensible to participants; it is of no interest for the present hypotheses. For half of the calculations, the presented result was correct, and for the other half false. Half of the calculations were presented in German (the local language) and should be generally known (e.g., 2 x 5 = 10 or 2 x 5 = 6) so that it should be obvious to participants whether the presented result is true or false. The other half of the calculations were read in a language foreign to our participants (Slovak), representing unknown equations. Unlike in Experiment 2, in which some versed participants could have still applied rules of thumb to identify the correctness of the unknown equations despite our precautionary measures, the foreign language manipulation should render it impossible to guess the correctness of the unknown equations. Hence, as long as participants did not understand Slovak, it was guaranteed that the truth status of the unknown equations was truly unknown. Besides these considerations regarding the implementation of the unknown condition, using another form of judged perceptual fluency also allows for testing the generalization and robustness of the effect of believed truth on processing fluency.

We expected that participants rate true calculations as easier to listen to than false calculations. This difference between true versus false calculations should be apparent for calculations read in German for which German-speaking participants can determine truth versus falsehood. In contrast, this difference should be non-existent when calculations are read in Slovak to non-Slovak-speaking participants.

4.1 Method

4.1.1 Participants and Design

Since the materials were similar, we based the power analysis for Experiment 3 on the data of Experiment 2 and used the generic *F* test in G*Power 3, which indicated a power of .80 for a sample size of 175 to detect a truth X knowledge interaction effect ($\eta^2_p = .04$, $\lambda = 7.95$); for further details, see the Supplemental Materials. We aimed at recruiting 175 participants but were only able to collect data for 114 participants due to fixed allotted laboratory time. A post-hoc power analysis based on the observed effect size resulted in an obtained power of .87 ($\eta^2_p = .09$, $\lambda = 9.80$); for details see Supplemental Materials.

Participants were university students participating in the experiment for course credit. Additionally, they received chocolate and had the chance to participate in a raffle. None of the participants indicated not having participated seriously in the experiment. Eleven participants were excluded from the analyses because they indicated speaking a language that enabled them to understand or partially understand the calculations read in Slovak. Of the remaining 103 participants, 92 were female and 11 male. One participant indicated to be 14 years old when asked for demographic information but nevertheless provided written consent to participation in the experiment, which included the explicit statement that the participant is 18 years old or older. Since all participants were students participating for course credit, we decided to consider the answer 14 as a typing error and included this participant in the analyses but excluded the participant from the descriptive statistics of age. The average age thus was 21.49 years (*SD* =

3.95, range: 18-44). Participants were randomly assigned to a 2 (calculations set: 1 vs. 2) x 2 (truth: true vs. false) x 2 (knowledge: known vs. unknown) mixed factorial design with the factors truth and knowledge as repeated measures.

4.1.2 Materials and Procedure

Participants were presented with the 32 one-digit multiplications from Experiment 2. Half of the calculations were read aloud by an automated German computer voice (known calculations) and the other half by an automated Slovak computer voice (unknown calculations). Which calculations were read in German and which in Slovak was switched for the two calculation sets (Set 1 vs. Set 2).

To generate the audio files, the software Text2Speech PRO was used. The German calculations were read aloud by the German Computer voice "Anna," whereas the foreign-language calculations were read aloud by the Slovak Computer voice "Laura." For both voices, the speaking rate was 200 words per minute. All audio calculation files were normalized with the software Audacity to a maximum amplitude of -1 dBFS. We also used Audacity to generate the white noise audio files. We generated eight intensity levels (-40, -35, -30, -25, -20, -15, -10, -5 dBFS) for two lengths of white noise, 2.064 and 2.873 seconds, corresponding to the longest German and the longest Slovak audio calculation recording, respectively. As practice trials, the two practice trials with one-digit multiplications from Experiment 2 were once read in German and once read in Slovak (computer voice "Anna" or "Laura," respectively), resulting in one practice trial for each knowledge X truth category and a total of four practice trials, that is: two German and two Slovak, of which two were true and two false. All calculations are listed in the Supplemental Materials, Tables S3 and S6. For the German practice trials, we generated white noise with the two intensity levels -5 and -25 dBFS, which lasted 2.09 seconds, corresponding to the longest German audio practice calculation recording. For the Slovak practice trials, the white noise intensity was -40 and -20 dBFS and lasted 3.03 seconds, corresponding to the longest Slovak audio practice calculation recording.

The experiment was assessed via computer in 10-15 minutes laboratory group sessions, with minimally one to maximally seven participants per session. After participants had consented to participation, they were asked to put on the headphones which were placed to their left. Starting mute, participants turned up the headphones' volume until they could hear the music that was playing (Chopin, 1834) but were told not to turn up the volume too much. Next, instructions stated that participants would hear mathematical calculations of the form "A x B = C" against a background of noise, which would vary in intensity. Participants were also told that some calculations would be read in German and some in a foreign language, and that their task was to indicate how difficult or easy it was to listen to these calculations. They were asked not to use a calculator to verify the results. Participants were also informed that they could adjust the headphone volume during the practice trials, but they were asked not to do so during the main trials.

Participants were then presented with the four practice trials in random order. The respective white noise intensities were randomly assigned to the German calculations and the Slovak calculations separately and played in parallel to the audio calculation recordings. Similar to Experiments 1 and 2, participants then indicated how easy or difficult it had been to listen to the respective calculation with a slider ranging from 1 (*very difficult*) to 50 (*very easy*). The slider had no default answer value. Next, participants completed the main trials with calculations from one of the two calculation sets presented in random order. The eight white noise intensities were randomly assigned to the calculations, separately for each block of a certain kind (e.g., Set

1, unknown-true), and played in parallel to the audio calculation recordings. Thus, there were always the same number of known-true, known-false, unknown-true, and unknown-false calculations in each noise condition. Everything else was the same as in the practice trials.

Next, participants were asked whether they had understood all German calculations acoustically and if not, how many of them they had not understood. Furthermore, they indicated whether they had adjusted the volume during the main trials and if so, how many times. Participants could then leave comments and were asked how seriously they had participated in the experiment. They also reported whether there was any reason not to use their data. After that, participants indicated their gender, age, whether German was their mother tongue, and in the case that German was not their mother tongue, they indicated for how many years they had been speaking German. Participants also indicated whether they spoke Slovak—with the three options 1 (*yes*), 2 (*no*), or 3 (*a little bit*)—or another language that had enabled them to understand the Slovak calculations—with the three options 1 (*yes*), 2 (*no*), or 3 (*partially*)—and if they had answered *yes* or *partially*, which language that was. They were also asked to indicate their study major and semester. Lastly, participants were thanked and received compensation.

4.2 Results

We used the same analysis procedures as in Experiment 1a with audibility as the dependent variable. The fixed effects in the mixed-effects model were truth (true vs. false), knowledge (known vs. unknown), and their interaction. Participants, stimuli, and noise were treated as random factors. We again aimed for a maximal model and included random intercepts for participants, stimuli, and noise as well as random slopes for truth, knowledge, and the truth X knowledge interaction based on participants and noise.

Running the function anova() over this mixed-effects model revealed a significant main effect of knowledge, $F(1, 13.48) = 86.15$, $p < .001$, with known calculations being rated as easier to listen to than unknown calculations ($M_{\text{known}} = 38.45$, $SD_{\text{known}} = 6.15$; $M_{\text{unknown}} = 24.92$, $SD_{\text{unknown}} = 8.61$, respectively). There was no significant main effect of truth, $F(1, 9.76) = 0.57$, $p = .467$ ($M_{\text{true}} = 31.82$, $SD_{\text{true}} = 6.10$; $M_{\text{false}} = 31.55$, $SD_{\text{false}} = 6.55$). However, the main effect of knowledge was qualified by the hypothesized significant truth X knowledge interaction, $F(1, 15.88) = 5.38$, $p = .034$.[8] Inspection of Figure 3 reveals a pattern of results in support of the suggested hypothesis. To specifically test the hypothesis that participants rate known-true calculations as easier to listen to than known-false calculations, simple effects of truth at the two levels of knowledge were analyzed using the R package phia version 0.2.1 (Martinez, 2015). These tests showed that known-true calculations were rated as significantly easier to listen to than known-false calculations, $\chi^2(1, N = 103) = 4.88$, $p = .027$ ($M_{\text{known-true}} = 38.95$, $SD_{\text{known-true}} = 6.16$; $M_{\text{known-false}} = 37.95$, $SD_{\text{known-false}} = 6.49$, respectively). Unknown-true calculations, however, did not significantly differ from unknown-false calculations in their rated audibility, $\chi^2(1, N = 103) = 0.85$, $p = .357$ ($M_{\text{unknown-true}} = 24.70$, $SD_{\text{unknown-true}} = 8.38$; $M_{\text{unknown-false}} = 25.15$, $SD_{\text{unknown-}}$

[8] In Experiment 2, to investigate the possibility of numerical priming, we reran the main analysis using an index that excluded five items per set. In Experiment 3, 10 items per set needed to be excluded for index computation, resulting in a loss of measurement accuracy. Nevertheless, we reran the main analysis with this index, yielding an interaction of $F(1, 11.44) = 4.12$, $p = .066$.

$_{false}$ = 9.26, respectively).[9]

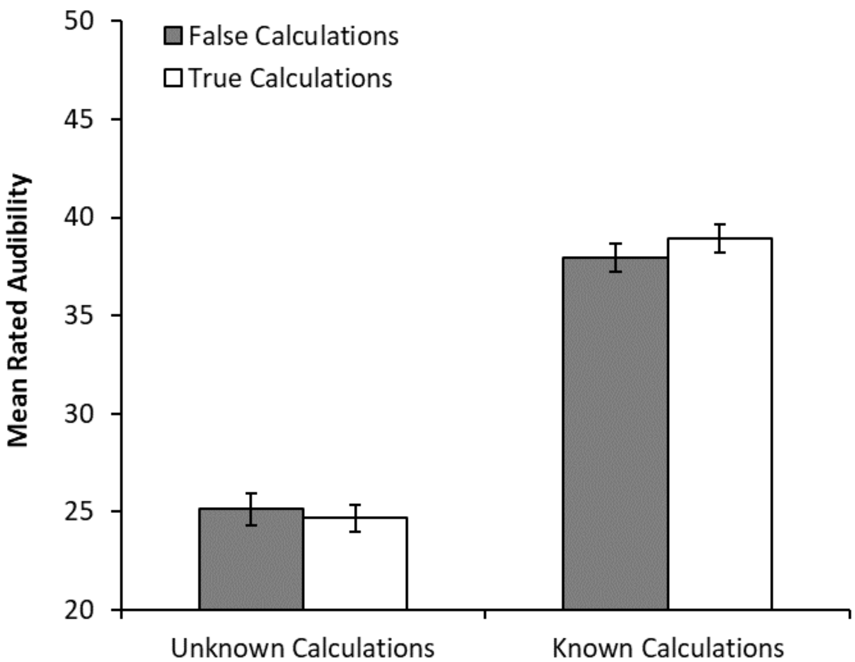


Figure 3. Mean rated audibility by truth and knowledge in Experiment 3. Unknown calculations are calculations that were read aloud in Slovak. Known calculations were read aloud in German. Participants indicated with a slider ranging from 1 (*very difficult*) to 50 (*very easy*) how easy or difficult it had been to listen to the respective calculation. Error bars represent within-subject standard errors for differences between means according to O'Brien and Cousineau (2014).

[9] Experiment 3's model with the newer package versions gave out a singular fit warning. The simplified specification of Experiment 1b still gave the warning. Additionally dropping the random slope for truth based on participants and the random intercept for calculations resolved the singularity problem and the hypothesis test conclusions stayed the same.

4.3 Discussion

Experiment 3 extends Experiments 1 and 2 with a different fluency measure and supports the hypothesis that calculations generally known to be true can be processed more fluently than calculations generally known to be false. Importantly, the same was not true for unknown calculations, which resulted in a significant interaction. Although we used a different methodological approach and a different form of judged perceptual fluency, namely judged acoustic fluency instead of judged visual fluency, the results of Experiment 3 dovetail with those of the previous experiments, suggesting that true information can be subjectively processed more easily than false information, provided that truth and falsehood are known.

We again observed a main effect of knowledge, which presumably represents a main effect of the known in comparison to the unknown language (i.e., a "foreign language" main effect). Moreover, the recordings for the unknown condition were slightly longer than those for the known condition because we adjusted the white noise length to the length of the longest German and the longest Slovak audio calculation recording separately.

5 Experiment 4

The results across Experiments 1 to 3 provide firm evidence that true compared to false information is subjectively easier to process, but only when the information’s truth status is known. This positive association between believed truth and processing fluency may have different reasons. The findings obtained so far seem consistent with the notion that believed true information may be repeated more often and might thus be more familiar. This repetition or familiarity should lead to more fluent processing of information generally believed to be true. Indeed, it seems plausible that the known-true statements used in Experiments 1 to 3, such as "Paris is in France" or "2 x 3 = 6," may be repeated more often in people’s everyday lives and might thus be more familiar than the known-false statements, such as "Paris is in Russia" or "2 x 3 = 10." The subjectively more fluent processing of the known-true compared to the known-false statements in Experiments 1 to 3 might therefore be explained by known-true as opposed to known-false statements being more familiar.

In Experiment 4, we tested whether believed truth affects processing fluency even if familiarity cannot account for this effect. To this end, we constructed statements for which the true compared to false statements should not have been repeated more often in people's lives, so that the true statements are not more familiar than the false statements (e.g., "The word DAZ starts with D" vs. "The word DAZ starts with Z"). This allows investigating whether there is an effect of believed truth on ease of processing irrespective of item familiarity. If such an effect is observed, it may be accounted for by semantic coherence. Indeed, the statement "The word DAZ starts with D," for example, is internally coherent because it is consistent with an individual's semantic knowledge about words and letters. By contrast, the statement "The word DAZ starts with Z" is internally incoherent because it contradicts semantic knowledge. Coherence is a derivative criterion for truth because true statements are supposed to be free from internal contradictions (see Goldman, 1986; Reber & Unkelbach, 2010). Thus, coherence of information indicates truth, and incoherence indicates falsehood, if there is no information at a higher level that could resolve the contradiction. Consistent with this reasoning, Topolinski and Strack (2009) have shown that semantically coherent word triads in the Remote Associates Test are processed faster than incoherent word triads. Against this background, we predict that internally coherent versus incoherent statements indicate truth and will subjectively be processed more fluently.

To test whether coherence of believed true information leads to enhanced processing fluency, we worked with pseudowords and presented participants with statements that were in themselves true or false. Importantly, what sets the new stimuli apart from the stimuli used in Experiments 1 to 3, is that they are not sentences one might hear in the world outside the laboratory, thus keeping familiarity constant. Half of these statements were true (e.g., "The word TIR has three letters," "The word DAZ starts with D," or "The word TUP ends with P") and the other half false (e.g., "The word TIR has two letters," "The word DAZ starts with Z," or "The word TUP ends with T"). Given the focus of Experiment 4, we no longer included statements for which the truth status is unknown, but used only statements for which truth versus falsehood should be clear to participants. Participants rated how easy or difficult it was to read each statement. As in Experiments 1 and 2, we again varied the statements' visual contrast to create a sensible task for participants. We hypothesized that participants judge true compared to false statements as easier to read.

Since the hypothesized interaction between truth and knowledge was not always obtained in Experiments 1 to 3, presumably because statistical power might have been too low in Experiments 1a and 2, we aimed for a power above .95 in Experiment 4. We estimated power through data simulation. To this end, we conducted a pilot study, which is described in the Supplemental Materials.

5.1 Method

5.1.1 Participants and Design

Comprehensive specifications concerning power analysis are outlined in the Supplemental Materials. A request for 140 participants was put on the crowdsourcing platform clickworker. As in the pilot study, participation was technically impossible with small screen devices. One hundred and thirty seven participants living in Germany completed the experiment for a compensation of €1.56. None of the participants reported not having participated seriously in the experiment. Four participants, however, failed the attention check. We included them in the analyses; excluding them did not change the significance and pattern of the reported results. One participant indicated that their data should not be shared or used for other studies and was therefore excluded from analyses to allow open data and reproducibility of our results. Of the remaining 136 participants, 57 were female, 78 male, and one chose the category "other"; the average age was 38.42 years (*SD* = 12.51, range: 18-72). Participants were randomly assigned to a 2 (statements set: 1 vs. 2) x 2 (truth: true vs. false) x 3 (statement type: count vs. first letter vs. last letter) mixed factorial design with the factors truth and statement type as repeated measures.

5.1.2 Materials

To broaden the range of stimuli, we constructed three different types of statements, reflected in the new factor *statement type*: "The word A has B letters" (statement type *count*), "The word A starts with B" (statement type *first letter*), and "The word A ends with B" (statement type *last letter*). Half of these statements were true and the other half false.

For the statement type *count*, there were two, three, four, and five letter pseudowords, six of each per statement set, half of them in a true and half of them in a false statement, resulting in 12 true and 12 false statements per statement set for this statement type. The false statements for the two letter pseudowords stated that the word had three letters, for the three letter pseudowords two, the four letter pseudowords five, and the five letter pseudowords four letters. The pseudowords that were true in one statement set were false in the other and vice versa. To construct the three, four, and five letter pseudowords, we first chose three, four, and five letter monosyllabic nouns from the word lists on https://www.openthesaurus.de by searching through randomly drawn entries. For the two letter pseudowords, we searched for nouns separately. Second, in order to transform these words into pseudowords, we entered them in the pseudoword generator Wuggy (Keuleers & Brysbaert, 2010), specifying German as the language; all other parameters set to default. Wuggy was built for generating nonwords for psychological experiments and ensures that the resulting pseudowords are in line with the orthographic and phonological patterns of a language, in our case German.

For the statement type *first letter*, there were again 12 true and 12 false statements per set. The monosyllabic nonwords were either generated with Wuggy in the described procedure or self-constructed. In order to have nonword pairs for both stimulus sets, we first constructed six monosyllabic nonword pairs, in which the first letter of one word was the last letter of the other and vice versa (e.g., DETZ and ZID), using each letter only once. Using the same first and last letters, we then constructed another six nonword pairs, in which the first letter of one word was the last letter of the other and vice versa (e.g., ZORD and DAZ). For statement Set 1, the first group of nonword pairs was in the true statements (e.g., "The word DETZ starts with D" and "The word ZID starts with Z") and for statement Set 2, the second group of nonword pairs was in the true statements (e.g., "The word ZORD starts with Z" and "The word DAZ starts with D"). To construct the false statements of statement Set 1, we used the second group of nonword pairs and switched the second part of the statements between each statement pair (e.g., "The word ZORD starts with D" and "The word DAZ starts with Z"). To construct the false statements of statement Set 2, we did the same for the first group of nonword pairs (e.g., "The word DETZ starts with Z" and "The word ZID starts with D"). This procedure ensured that the letter in question appeared in the nonword, irrespective of whether the statement was true or false. Furthermore, the letters in question were the same for the true and for the false statements.

For the statement type *last letter*, the construction procedure was the same as for the statement type *first letter,* while using 12 new first and last letters different to those for the *first letter* statements.

For the practice trials, we presented two statements per statement type, one true and one false, containing self-constructed nonwords. All materials are listed in English in the Supplemental Materials, Tables S7 and S8, but were presented in German (the local language of the participants).

5.1.3 Procedure

After being welcomed and consenting to participation, participants were instructed that they would see 72 statements about words and letters, that the brightness of these statements would vary, and that their task was to indicate how difficult or easy it is to read these statements. After having confirmed that they had read the instructions, participants were presented with the six practice trials. Practice trial statements were presented in random order and randomly in one of six contrasts from grey to black (RGB 165-165-165, 135-135-135, 105-105-105, 60-60-60, 30-30-30, and 0-0-0), and were horizontally centered at the top of the page. On the same page, participants indicated how easy or difficult it was to read the respective statement with a slider ranging from 1 (*very difficult*) to 50 (*very easy*). The slider had no default answer value.

Next, participants completed the main trials with statements from one of the two statement sets presented in random order. The 12 RGB combinations 165-165-165, 150-150-150, 135-135-135, 120-120-120, 105-105-105, 90-90-90, 75-75-75, 60-60-60, 45-45-45, 30-30-30, 15-15-15, and 0-0-0, again ranging from grey to black, were randomly assigned to the statements, separately for each block of a certain kind (e.g., Set 1, statement type *count*, true). Thus, there were always the same number of known-true, known-false, unknown-true, and unknown-false statements in each contrast condition. Everything else was identical to the practice trials. After the main trials, participants indicated gender, age, their highest level of education completed, whether German was their mother tongue, and in case German was not their mother tongue, they indicated for how many years they had been speaking German. They could then leave comments and were asked how seriously they had participated in the study. As an attention check, participants were asked to choose the option *not at all* on a scale ranging from 1 (*not at all*) to 9 (*very*). They also reported whether there was any reason not to use their data. Lastly, they were thanked and were given the code for receiving compensation.

5.2 Results

We fitted a linear mixed-effects model with legibility as the dependent variable using the same R and package versions as for analyzing Experiment 1b, again using the optimizer "bobyqa." Descriptive statistics were computed as in Experiments 1 to 3. Proceeding from a maximal linear mixed model justified by the design and attempting to tackle convergence issues by increasing the maximum limit of function evaluations, we derived a simplified model that converged and did not give out a warning message. We included truth (true vs. false), statement type (count vs. first letter vs. last letter), and their interaction as fixed effects in the mixed-effects model. Participants, visual contrasts, and stimuli (i.e., pseudowords) were treated as random factors. We included random intercepts for participants, visual contrasts, and stimuli as well as random slopes for truth based on participants and stimuli[10]. Consistent with the effect of interest,

[10] Since we had more data than in the pilot study, we were able to fit a more complex model including the random slope for truth based on stimuli. Note that the pattern of the reported results and their significance did not change when fitting the simpler model of the pilot study.

we were able to include random slopes for truth. We were not able to include random slopes for statement type, but the effect of statement type is not of theoretical interest here.

The function anova() revealed the expected significant main effect of truth, $F(1, 135.60) = 10.82$, $p = .001$, with true statements being rated as easier to read than false statements ($M_{\text{true}} = 42.38$, $SD_{\text{true}} = 6.88$; $M_{\text{false}} = 40.33$, $SD_{\text{false}} = 9.84$, respectively). There was no significant main effect of statement type, $F(2, 67.80) = 2.41$, $p = .098$ ($M_{\text{count}} = 41.31$, $SD_{\text{count}} = 7.71$; $M_{\text{first letter}} = 41.56$, $SD_{\text{first letter}} = 7.58$; $M_{\text{last letter}} = 41.19$, $SD_{\text{last letter}} = 7.87$) and no significant interaction truth X statement type, $F(2, 67.91) = 0.22$, $p = .804$. Figure 4 suggests that true statements were rated as easier to read for all three statement types. To explore this assumption, simple effects of truth at the three levels of statement type were analyzed using the same R package as in Experiment 1b. These tests showed that true statements were rated as significantly easier to read than false statements for all three statement types, count: $\chi^2(1, N = 136) = 9.20$, $p = .002$, first letter: $\chi^2(1, N = 136) = 11.15$, $p < .001$, last letter: $\chi^2(1, N = 136) = 9.47$, $p = .002$.

5.3 Discussion

Experiment 4 provides further evidence for a reliable and substantial effect of believed truth on processing fluency and again supports the assumption that believed true information is subjectively easier to process than believed false information. Finding this effect for all three statement types and using mixed-effects models analyses heightens the probability of replicating the same effect with new observations and different stimuli.

Most notably, the effect of believed truth on processing fluency persisted even when the true and false statements did not differ in familiarity. Working with pseudowords, there is no reason to believe that a true statement such as "The word FI has two letters" is more familiar than a false statement such as "The word FI has three letters." Thus, the findings of Experiment 4

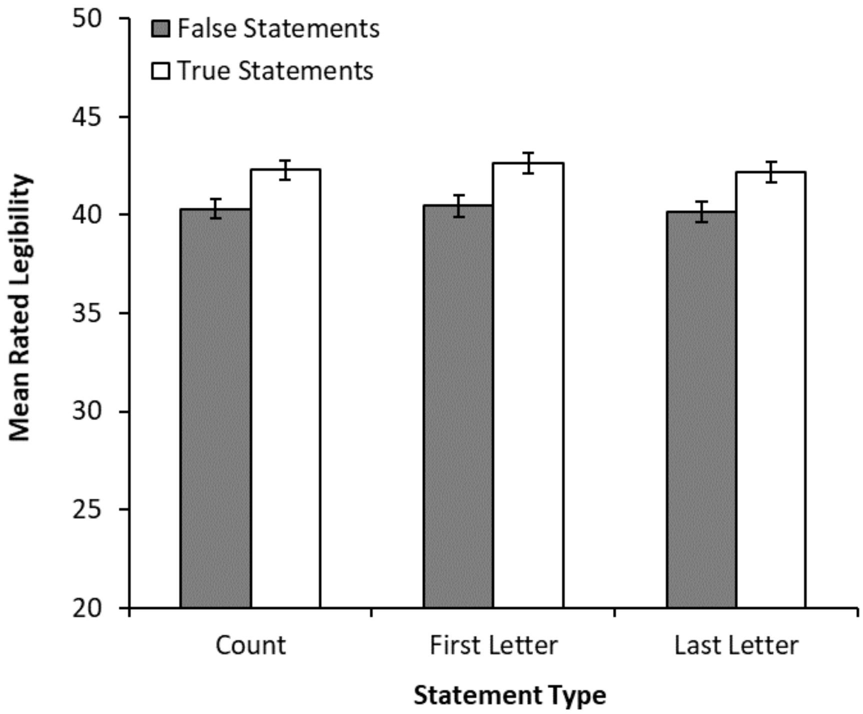


Figure 4. Mean rated legibility by truth and statement type in Experiment 4. The statement type *count* refers to statements concerning the number of letters in a pseudoword. *First letter* statements concern the first letter of a pseudoword and *last letter* statements the last letter. Participants indicated with a slider ranging from 1 (*very difficult*) to 50 (*very easy*) how easy or difficult it was to read the respective statement. Error bars represent within-subject standard errors for differences between means according to O'Brien and Cousineau (2014).

presumably do not stem from differences in item familiarity. By contrast, the heightened fluency of true statements may stem from semantic coherence. Coherence as a derivative criterion for truth leads to closer associative links between the nodes of a proposition, which increases speed and ease of processing. The semantic coherence explanation corresponds to an explanation referring to consistency with prior knowledge, since coherence implies consistency with semantic knowledge.

In Experiment 4, we used statements that differed in topic area and format from Experiments 1 to 3. We thereby again broadened the range of stimuli used in this line of research. It is interesting and important to test the effect of believed truth on processing fluency in diverse information ecologies to learn more about the reliability and generalizability of the effect.

Experiment 4 shares some features with experiments conducted in the context of the discrepancy-attribution hypothesis (e.g., the use of nonwords; Whittlesea & Williams, 1998, 2001a). However, it should be noted that the discrepancy-attribution hypothesis focuses on the (in-)coherence in the *processing experience* that results from the fact that orthographically regular nonwords may be processed more fluently than expected. In contrast, in Experiment 4, semantic inconsistency as defined in the present contribution pertains to the within-item truth status. According to Whittlesea and Williams (2001a, 2001b), inconsistency as manipulated in Experiment 4 should not produce an incoherent processing experience, because the disfluent processing fits the obvious semantic inconsistency.

6 General Discussion

When individuals encounter a piece of information, they need to determine whether it is true or false. In a world of fake news and post-truth politics, this task may have become more important than ever (see contributions in Greifeneder et al., 2021). Prior studies suggest that individuals rely on experienced processing fluency when solving this task. The current explanation for this fluency-truth effect assumes that people learn an existing positive association between truth and fluency in their environment. We set out to investigate this precondition that people positively associate believed truth with processing fluency.

In support of this main hypothesis, we found that true compared to false information is subjectively easier to process. Furthermore, Experiments 1a and 2 offer preliminary evidence that for this effect to unfold, individuals need to be able to discern truth or falsehood, afforded by knowledge. This notion is supported by Experiments 1b and 3, in which we found that only information generally known to be true (vs. false) is subjectively experienced as easier to process but not information for which truth (vs. falsehood) is unknown. The findings of Experiments 1 to 3 are in line with the assumption that believed true information may be repeated more often and is hence more familiar, thereby facilitating processing. Experiment 4 extends these findings by showing that the association between believed truth and processing fluency is independent of information familiarity, but could be explained by semantic coherence as the source of fluency. To be sure, familiarity may still be one of multiple explanations for the influence of believed truth on fluency in general; but it is not the only factor that contributes to the observed effect. Indeed, examining Experiment 4 further with a conventional 2 (truth: true vs. false) x 3 (statement type: count vs. first letter vs. last letter) within-participants ANOVA, revealed a medium sized effect of truth on subjective fluency ratings, $F(1, 135) = 10.87$, $p = .001$, $\eta^2_p = .07$, thus clearly bolstering the importance of factors other than familiarity in the believed truth-fluency association.

One might wonder whether it is familiarity or coherence rather than believed truth that leads to experienced processing ease? Indeed, we argue that believed truth is subjectively fluent *because* it is familiar and/or coherent. Put simply, if coherence leads to fluency, truth will be fluent, since truth "has to be" coherent. Thus, because familiarity and coherence lead to fluency, individuals may learn that believed truth is fluent. This assumption could be inferred with some plausibility. However, as earlier studies did not manipulate truth and knowledge, the current experiments provide the empirical test of this hitherto unexamined assumption.

The present results are first to empirically establish the precondition that is implicit or explicit to the current ecological learning explanation of the fluency-truth effect. This perspective holds that individuals use processing fluency as a cue for truth (fluency-truth effect) because individuals have learned a positive association between the two. This notion rests on the precondition that truth and fluency are positively associated. With our experiments, we empirically support this assumption. Using visual as well as acoustic stimuli and different forms of statements as well as calculations, we established the effect of believed truth on processing fluency for different forms of judged perceptual fluency and different types of information. Moreover, using linear mixed-effects models for analyses, there is good reason to assume that the results generalize across participants *and* stimuli (Judd et al., 2012).

In what follows, we will first discuss how actual versus believed truth relate to processing fluency. Then, we will turn to the question what the observed believed truth-fluency association might imply for the validity or biasing influence of fluency as a cue in truth judgments. Furthermore, we will examine our measure of subjectively experienced processing fluency more closely, and we will explore whether individuals may have learned that falsehood is especially disfluent or rather that truth is especially fluent. Lastly, we will discuss the information ecologies used in our experiments and the importance of stimulus variation.

6.1 The Association of Believed Versus Actual Truth With Processing Fluency

We found that truth is positively correlated with fluency primarily if individuals knew whether information is true or false. Crucial is thus *known* or *believed* truth rather than actual truth; indeed, the mechanisms of the truth-fluency association (e.g., repetition, coherence) act on the level of believed truth or falsehood. Information is repeated more often when it is generally *believed* to be true. Likewise, the degree of consistency with prior knowledge pertains to information *believed* to be true or false since prior knowledge reflects individuals' learnt subjective belief system rather than objective truth. As believed and actual truth are likely to be positively correlated, actual true (vs. false) information is also likely to gain in ease from being repeated, from being more coherent, and so on, presumably leading to an actual truth-fluency association. Admittedly, the actual truth-believed truth correlation is unlikely to be perfect. The distinction becomes apparent with statements such as "Sydney is the capital of Australia." Albeit objectively false, such statements might be processed quite fluently if many people believe in the false proposition and repeat it.

6.2 The Validity of Processing Fluency as a Cue in (Truth) Judgments

Processing fluency may be a valid cue in judgment (Hertwig et al., 2008; Jacoby et al., 1989). A good example for fluency used as a valid cue was investigated by Kelley et al. (1989): Participants tried to read briefly presented words and had to decide whether they had heard or read these words previously in the experiment. Participants used ease of reading (i.e., visual fluency) as a cue for having read as opposed to heard a word previously (cue utilization). Indeed, having read a word previously facilitates reading this word again more than having heard the word before (ecological validity). This ecological validity allows visual fluency to be a valid cue when remembering modality of presentation. Similarly, it has been argued that fluency may be a valid cue for judging truth (Reber & Unkelbach, 2010; Unkelbach & Stahl, 2009).

What does the observed positive truth-fluency association tell us about the validity of processing fluency as a cue in truth judgments? From the perspective of Brunswik's (1952) lens model, the observed correlation between truth and processing fluency constitutes the ecological correlation of the judgment cue (i.e., processing fluency) with the criterion (i.e., truth), reflecting ecological validity. Since people's reliance on fluency as a cue for truth is a robust and ubiquitous finding (e.g., Dechêne et al., 2010; Hansen et al., 2008; Reber & Schwarz, 1999; Scholl et al., 2014), this insight is crucial because it proposes that processing fluency is not only a cue used but a valid cue in truth judgments (Unkelbach & Greifeneder, 2013b).

Yet processing fluency may also bias judgments. Individuals judge whether they have seen a word previously by relying on fluency (Kelley et al., 1989). However, this fluency may not be caused by prior exposure but instead by a matching context word (Jacoby & Whitehouse, 1989) or by visual clarity or contrast (Whittlesea et al., 1990). Here, processing ease caused by visual clarity is misattributed to or misinterpreted as stemming from prior exposure. Note that this example does not question the possibility that fluency may be a valid cue for judging prior exposure *in general*. A biasing influence arises because fluency is attributed to or interpreted as stemming from the wrong source. Thus, using fluency in judgments might still lead to biases, even when fluency in general is a valid cue for a certain criterion.

Applying this logic to judgments of truth, even when a positive truth-fluency association exists, individuals might infer truth from misattributed or misinterpreted processing ease. However, given awareness (e.g., Jacoby & Whitehouse, 1989), enough cognitive resources (Gilbert, 1989; Jacoby et al., 1992), and motivation (Schwarz, 2004b), individuals may be able to correct rapid and automatic attributions or interpretations in order to debias judgments. Furthermore, individuals more strongly rely on fluency as a cue to judge truth when they have learned from past experience that using fluency as a cue resulted in valid truth judgments (Scholl et al., 2014). People unwaveringly inferring truth from fluency thus further supports the assumption that a biasing influence of fluency in truth judgments might be the exception to the rule of fluency being a generally valid cue for truth. This again points to a positive truth-fluency association in people's environment (ecological validity).

6.3 Measuring Subjectively Experienced Processing Fluency

On the measurement level, we focused on judged perceptual fluency as one particularly prominent kind of processing fluency. It is important to note, however, that we do not claim that believed truth or falsehood alter the actual *perception* of the presented information. Our experiments rather measure the *judgments* participants made concerning their perception. Although perception might be the basis for such a judgment, our measures also entail an *interpretation* of this perception (for this important distinction, see Firestone & Scholl, 2016). It may well be and it would be consistent with our argument that believed truth increases conceptual fluency, which then influences individuals' judgments of perceptive ease (see, e.g., Labroo et al., 2008, for the influence of conceptual on perceptual fluency). Again, please note that although we manipulated perceptual fluency via contrast as an auxiliary methodological means, we were not interested in the judgment differences caused by this perceptual fluency manipulation. It may be interesting to investigate the effect of believed truth on objective measures of processing fluency, too. It has to be noted, however, that the concept of processing fluency is inherently subjective since it is defined as the subjective feeling of processing ease or difficulty (e.g., Alter & Oppenheimer, 2009; Schwarz & Clore, 2007) and it is uncertain how well objective measures capture this subjective experience of processing fluency (e.g., Graf et al., 2018; Reber, 2012; Reber et al., 2004). Dovetailing with this conceptualization of fluency, subjectively experienced compared to objectively manipulated processing fluency seems to have a greater impact on judgments (Forster et al., 2013).

One might worry that participants might have suspected that truth was expected to lead to higher legibility and audibility ratings, answering accordingly. We guarded against such demand effects and varied the contrast or background noise level in order to create a sensible task for participants. The manipulation of contrast or noise should direct participants' attention to the perceptual readability or audibility rather than to the statements' truth or falsehood.

Another concern might be that halo effects may account for the observed effects. Halo effects denote that a global evaluation and/or a salient attribute influences evaluations of other attributes of the same target (Dagger et al., 2013; Nisbett & Wilson, 1977). Typically, such halo effects are observed in person perception, with one attribute (e.g., attractiveness) influencing other attributes (e.g., perceived intelligence or competence; "what is beautiful is good" stereotype; Dion et al., 1972; Miller, 1970; Talamas et al., 2016). In our studies, one might argue that the truth of a statement translates to readability judgments not because truth is fluent but because both truth and readability are positive attributes. Halo effects seem to have, however, some specificity (selective halo effects; Dagger et al., 2013; Dermer & Thiel, 1975; Kaplan, 1978; Lucker et al., 1981) in that they are not generalizable to just any target or attribute. Specifically, an important qualification constitutes the evaluability of an attribute in that easy-to-evaluate attributes should not be prone to being influenced by halo effects. Given the experimental setup with the variation of contrast or background noise level, judging the attribute of readability or audibility is quite straightforward and should not have proven difficult to participants. Furthermore, to ensure some experience with the task, we included practice trials in all five experiments. The to-be-judged attribute should thus not have been complex and participants should not have lacked knowledge for their judgments, the criteria for an attribute to be easy versus difficult to evaluate (Dagger et al., 2013), which is why we consider halo effects an unlikely explanation of our results.

6.4 The Association of Truth Versus Falsehood With Processing Ease Versus Difficulty

When investigating the effect of fluency on judgments, one may ask whether it is experienced processing *ease*, experienced processing *difficulty*, or *both* that is/are informative in judgment (Hansen et al., 2008; Schwarz, 2004a; Wänke et al., 1996). One answer to this question focuses on the relative baseline level of experienced fluency (Schwarz, 2004a, 2004b) since deviations from this baseline level are most informative and influential in judgments (Wänke & Hansen, 2015). In addition, for judgments of truth, the answer to this question likely depends on whether individuals have learned that falsehood is especially disfluent, or that truth is especially fluent. If falsehood is especially disfluent, difficulty of processing will be particularly diagnostic; in contrast, if truth is especially fluent, ease of processing will be particularly diagnostic; finally, it is also conceivable that fluency and disfluency are equally informative. Prior theorizing suggests that believed true information is more likely to be repeated (e.g., Unkelbach, 2007). As repetition leads to processing ease, one may thus argue that it is especially believed true information that should differ from the baseline level of experience fluency, in being especially fluent. Data from Experiments 1a and 1b seem to offer preliminary evidence for this theorizing. The mean experienced fluency of the statements for which truth or falsehood is generally unknown could serve as a proxy for the baseline level of experienced fluency. This assumption seems reasonable for Experiments 1a and 1b because participants have probably never heard these statements before, but the statements' semantic elements are not especially difficult to process. The mean experienced fluency was significantly higher for statements known to be true than for unknown true statements in both experiments; ts > 2.50, dfs > 73, ps < .015, dz > 0.27.

In contrast, the mean experienced fluency did not significantly differ for statements known to be false and unknown false statements in both experiments; *t*s < 0.36, *df*s > 73, *p*s > .72, *dz* < 0.05. It has to be noted, however, that our experiments were not designed to answer this question and taking the unknown statements as the baseline level is a speculative assumption. Nevertheless, dovetailing with this preliminary evidence that believed truth may be especially fluent, and processing ease may hence be particularly informative, Hansen and colleagues (2008) found that processing ease leads individuals to more probably judge a statement as true, whereas processing difficulty does not seem to lead to a higher probability to judge a statement as false.

6.5 Information Ecologies and Stimulus Variation

A note is warranted concerning the information ecologies we used in our experiments. In particular, in Experiments 1 to 3, we looked at statements of the form "City A is in Country B" and "A x B = C," and we were able to find the hypothesized effect in these ecologies. In Experiment 4, we aimed at broadening the range of stimuli and used new material with a different topic and in a different format (e.g., "The word A has B letters"), again finding the hypothesized effect, indicating its reliability. Furthermore, in Experiment 4, we additionally varied the stimuli by constructing three different types of statements. Despite all these variations, it needs to be acknowledged that we investigated specific information ecologies. It appears interesting to examine the observed effect in other information environments, too. That being said, the effect being found over all stimulus sets and the present statistical analyses suggest that it is unlikely to depend on the specific stimuli selected (Judd et al., 2012) so that there is good reason to believe that the observed effect will generalize to even broader and more diverse information ecologies.

Also in respect to stimulus selection and presentation procedure, the effects of processing fluency seem to be stronger when items with low fluency and items with high fluency are presented intermixed, increasing variability in processing fluency (Dechêne et al., 2009, 2010). This speaks in favor of manipulating the truth of statements within-subjects and mix true and false statements, as done in all experiments, as well as varying stimuli within one experiment, as implemented in Experiment 4.

One important specification of our information ecologies was the status of common knowledge. We found that true information is easier to process than false information, provided that truth and falsehood are known. This might explain recent evidence reported by Unkelbach and Greifeneder (2018), who observed reliance on fluency in truth judgments even when individuals had 100% valid declarative information at their disposal. Furthermore, fluency influences truth judgments even when individuals hold prior knowledge about the truth status of the information (e.g., Fazio, 2020). According to our results, truth and fluency are perceived as associated exactly in situations in which individuals have explicit knowledge about truth or falsehood. Against the background of our results, it makes sense that people rely on fluency in truth judgments even if diagnostic declarative information is available.

7 Conclusion

We examined the implicit but so far unexplored precondition for the current explanation of the well-known fluency-truth effect: Believed truth and fluency are associated in that believed truth is subjectively processed more fluently than believed falsehood. From this perspective, it makes sense for individuals to use fluency as a cue when judging truth. Yet it will be interesting to explore how far fluency may be influenced by sources other than truth so that fluency leads to biased truth judgments.

Acknowledgements

We would like to thank Sofia Thai, Rebekka Holzer, Christiane Büttner, and Daniela Sutter for their assistance in preparing the materials and collecting data, Mirella Walker, Selma Rudert, and Leonie Reutner for their support and for discussing the project, Rui Mata and Andrea Meyer for their introduction to mixed-effects models, and Henrik Singmann and Daniel Heck for their advice.

Funding: Rolf Reber was supported by "Literature, Cognition, and Emotions" at the Faculty of Humanities, University of Oslo; and Lea S. Nahon and Sarah Teige-Mocigemba were supported by the Deutsche Forschungsgemeinschaft at the Philipps University of Marburg [grant number 290878970-GRK 2271].

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