*A disfluent font does not help people to solve math and non-math problems regardless of their
numeracy*

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Abstract

Prior research has suggested that perceptual disfluency activates analytical processing and increases the solution rate of mathematical problems with appealing but incorrect answers (i.e., the Cognitive Reflection Test, hereafter CRT). However, a recent meta-analysis does not support such a conclusion. We tested here whether insufficient numerical ability can account for this discrepancy. In a 2(font: fluent vs. disfluent; between-subjects factor) $\times$ 2 (CRT: Numerical vs Verbal; within-subjects factor) design, 310 participants solved numerical and verbal CRT problems, followed by a measure of numerical ability. We found strong support against the disfluent font effect on the problem-solving rate for math as well as non-math problems regardless of participants' numeracy. The updated meta-analysis ($k = 18$) yielded close-to-zero effect, Hedge's $g = -0.01$, 95% CI[-0.05, 0.03] and decisive evidence against the disfluency effect on math problems, $BF_{0+} = 151.6$. Thus, perceptual disfluency does not help people solve math and non-math problems.

Keywords: Cognitive Reflection Test, Verbal Cognitive Reflection Test, disfluent font, numeracy, fluency

Dual-process theories of cognition postulate two types of cognitive processes: intuitive processes typically described as fast, automatic and frugal, and analytical processes typically described as slow, effortful and reflective (e.g., Darlow & Sloman, 2010; Evans, 2008; Evans & Stanovich, 2013; Morewedge & Kahneman, 2010; Reyna, 2004; Sloman, 1996). Dual-process theories have successfully accounted for various psychological effects in judgment and decision-making, deductive and inductive reasoning, moral reasoning and beliefs (e.g., Frederick, 2005; Liberali, Reyna, Furlan, Stein, & Pardo, 2012; Pennycook, Fugelsang, & Koehler, 2015; Sirota, Juanchich, & Haggmayer, 2014; Toplak, West, & Stanovich, 2011, 2014). The recent theoretical discussion has focused on the mechanisms of activation of the analytical process (e.g., a serial or parallel activation model) and which cues can actually trigger it (e.g., Bhatia, 2017; De Neys, 2012 for a review).

In respect to this research problem, prior research has suggested that meta-cognitive difficulty—the subjective experience of ease or difficulty associated with processing information—might be one of the mechanisms triggering analytical processing when solving problems (Alter, Oppenheimer, Epley, & Eyre, 2007; Oppenheimer, 2008). In the absence of other cues, a feeling of ease associated with processing information will trigger more intuitive processing, whereas a feeling of meta-cognitive difficulty will trigger more systematic, analytical thinking. Some previous studies tested this idea by manipulating perceptual (dis-)fluency: presenting problems in a fluent (i.e., easy-to-read) font and in a disfluent (i.e., difficult-to-read) font. This manipulation relied on the idea that the difficulty associated with reading a problem printed in the disfluent font would be substituted for the meta-cognitive difficulty of the problem itself: as in “if it is hard to read, it must be hard to solve”. For instance, Alter et al. (2007), presented participants with three mathematical problems that had appealing intuitive but incorrect answers, known as the Cognitive Reflection Test, (Frederick, 2005) either in a fluent or disfluent font. The participants

correctly solved more problems in the disfluent font condition than in the fluent font condition. According to the authors, the participants reading the problems in the disfluent font adopted a more systematic processing strategy, which resulted in improved performance. Thus, meta-cognitive experiences of disfluency served as a signal that the initially generated judgment was insufficient and that more elaborate systematic thinking was necessary (Alter et al., 2007).

However, several studies did not replicate the effect of disfluency on the ability to answer the three mathematical problems (Meyer et al., 2015; Thompson et al., 2013). A meta-analysis of the original study from Alter et al. (2007) and 16 subsequent replication studies found no overall disfluency effect on performance in the Cognitive Reflection Test (Meyer et al., 2015). In fact, none of the replication studies found a statistically significant disfluency effect. Two main explanations could be considered for a possible discrepancy between the original study and the lack of disfluency effect reported in the replication studies. The first explanation would be that one of the two sets of findings is actually incorrect. It might be that the original study reported a false-positive finding (Type I error) or it might be that the subsequent replications reported false-negative findings (Type II error). The false-negative findings explanation does not seem to be a plausible explanation: 7,327 participants amassed in the 16 studies would be sufficient to detect any reasonably small effect (i.e., $d = 0.1$ or bigger) with extraordinary statistical power (.99 or more with the increasing effect size). However, the false-positive finding of the original study remains quite possible. Meyer et al. (2015) noted that, in the original study, the difference in performance between the disfluent and fluent font groups (each having 20 people) was driven solely by an unusually low performance in the fluent font condition of the widget problem (4 out of 20 responses vs 16 out of 20 responses in the disfluent font condition). No difference was observed between the disfluent and fluent fonts in the bat and ball

problem and in the lily pad problem (16/20 vs 15/20, 18/20 vs 18/20, respectively). This means that sampling variation solely could explain the discrepant findings.

The second explanation for the discrepant findings is the existence of hidden moderators. For instance, the authors of the original study suggested that cognitive ability might play a role: disfluency might benefit only those with high cognitive ability (Alter, Oppenheimer, & Epley, 2013). Indeed, some limited evidence suggested that this might be the case: only the participants with higher intelligence benefited from the disfluent manipulation in terms of enhanced performance (Thompson et al., 2013). However, the recent meta-analysis did not support this assertion either. The authors provided several arguments against it: most notably, they showed that intelligence—measured either by selected items from Raven’s Progressive Matrices or by self-reported SAT math scores—did not interact with the font manipulation to predict enhanced performance in the Cognitive Reflection Test (Meyer et al., 2015). In addition, the disfluency effect was not moderated by the experimental setting (in public, in a lab or online), presentation format (paper and pencil vs computer screen) and previous exposure to the problems (Meyer et al., 2015). Finally, it can be argued that the negligible amount of heterogeneity between the studies does not warrant an exhaustive search for hidden moderators (Meyer et al., 2015).

Nevertheless, we believe that one additional moderator should be considered. One obvious candidate—a distinct ability required to solve the Cognitive Reflection Test, which is conceptually and operationally different from cognitive ability—is numeracy. Indeed, prior research found that solving the CRT requires numerical skills over and above cognitive abilities (Liberali et al., 2012; Pennycook & Ross, 2016; Sinayev & Peters, 2015; Sirota, Kostovičová, Juanchich, Dewberry, & Marshall, 2018). The Cognitive Reflection Test has been used widely to measure the extent to which people rely on analytical processing (e.g., Pennycook & Rand, 2018, 2019), but it has also been widely criticised for measuring

numeracy (e.g., Sinayev & Peters, 2015). The disfluent font might increase performance in the Cognitive Reflection Test only to those who might have sufficient mathematical skills to benefit from the triggered analytical processing. Aligned with that proposal, some evidence has suggested that perceptual disfluency enhanced the problem-solving rate of problems that did not require any numerical skills to solve them, but which were similar to those featured in the CRT in terms of offering an intuitive but incorrect first answer; for example, oversight problems such as the Moses illusion (Song & Schwarz, 2008) and the belief-bias syllogism (Alter et al., 2007). This evidence is mixed, however, because some subsequent studies failed to replicate these findings (Meyer et al., 2015; Morsanyi & Handley, 2012; Thompson et al., 2013) and the original studies have their own methodological and analytical limitations (e.g., not correcting for multiple comparisons in the former case; excluding some items due to floor/ceiling effect in the latter case). Hence, sufficient preliminary evidence warrants further investigation of numeracy as a possible moderator of the disfluency effect on solving CRT problems.

The Present Research

In the manuscript presented here, we aimed to test the effect of perceptual disfluency on cognitive reflection, while testing possible boundary conditions of the disfluency effect. Specifically, we made three advancements. First, we measured the numerical ability of our participants using a standard numeracy test, which allowed us to test numeracy directly as a moderator (Lipkus, Samsa, & Rimer, 2001). Second, we also used the Verbal Cognitive Reflection Test, which features items that involve cognitive reflection but do not require any mathematical calculations (Sirota et al., 2018). Finally, we used an extended version of the numerical Cognitive Reflection Test, which contains the three critical items of the original version of the CRT, but also four additional items (Frederick, 2005; Toplak et al., 2014). The extended version of the CRT has better statistical and

psychometric properties, which makes it more likely to detect an effect since it has better score variability and internal consistency (Toplak et al., 2014).

We derived three main hypotheses from the assumption that the disfluency effect exists. First, according to a strong version of the assumption that the disfluency effect exists and operates independently from numeracy, we can expect that the disfluent font will increase the performance in the Numerical Cognitive Reflection Test compared with the fluent font (Hypothesis 1). In fact, we expected, given the overwhelming evidence demonstrating the lack of the disfluency effect, that this hypothesis would not be confirmed. According to a weaker version of the assumption that the disfluency effect exists, the disfluency effect might not occur due to poor numerical skills preventing the activated analytical thinking to be manifested. We derived two hypotheses from this version of the assumption. First, the disfluency will increase performance in the Numerical Cognitive Reflection Test, but only for those who have enough numeracy skills to solve the mathematical problem and not for those with lower numerical skills (Hypothesis 2). Second, disfluency will increase performance in the Verbal Cognitive Reflection Test only since it does not require mathematical skills (Hypothesis 3).

Method

Participants and Design

We recruited 312 psychology undergraduate students from the University of Essex in exchange for course credits. The sample size was based on an a-priori stopping rule to reach a minimal sample size of 300 participants—comprising a sample of 296 participants required to detect a medium effect size Cohen's $d = 0.50$ for an independent samples t -test with high power ($\alpha = .05$, $1 - \beta = .99$ for a two-sided test) and an additional 4 students to account for attrition rate (Cohen, 1988). We collected data from slightly more students

because we let all the students who had signed up for the study participate. We excluded one participant due to an error in materials (i.e., having two Verbal CRTs instead of one Verbal and one Numerical CRT) and one participant due to a failure to fulfil the eligibility criteria (i.e., reported age of 17 years old). The resulting sample size of 310 allowed us to detect a small effect size (Cohen's $d = 0.32$), while using an independent samples t -test and assuming $\alpha = .05$, $1 - \beta = .80$, and a two-sided test (Cohen, 1988). The achieved statistical power of the test was approximating 1.0 for the originally reported effect Cohen's $d = 0.71$ (Alter et al., 2007). Hence, this was a sufficient sample size to assess the effect. Participants were mostly women (79.9%). The participants' ages ranged from 18 to 43 years ($M = 19.4$, $SD = 2.2$ years).

Participants solved the word problems from the Numerical Cognitive Reflection Test (Frederick, 2005; Toplak et al., 2014) and the Verbal Cognitive Reflection Test (counterbalanced for order) either in a fluent, easy-to-read font (i.e., black Myriad Web 12-point font) or in a disfluent, difficult-to-read font (i.e., 10% gray italicised Myriad Web 10-point font)—the same manipulation was used in the original study (Alter et al., 2007). Thus, we had a 2(font: fluent font vs disfluent font) $\times$ 2(CRT: Numerical vs Verbal) mixed-subjects design, with the font as a between-subjects factor and the CRT as a within-subjects factor with a counterbalanced order of presentation. We conducted a manipulation check on a separate sample of 20 participants from the same pool who rated the extent to which the same text (i.e., CRT questions) printed in the two fonts was easy/hard to read using a 5-point Likert scale (anchored as 1: *extremely easy to read*, 5: *extremely hard to read*). The counterbalanced presentation order of the two texts did not significantly alter the ratings for the fluent or disfluent font, $t(18) = 0$, $p = 1.000$, Cohen's $d = 0$; $t(18) = 0.75$, $p = .461$, Cohen's $d = 0.34$, respectively; hence, it was not considered further. Similarly to the prior reported research, the fluent font ($M = 1.2$, $SD = 0.4$) compared with the disfluent font (M

= 3.7, $SD = 1.2$) was judged to be substantially and statistically significantly easier to read, $t(19) = -8.75$, $p < .001$, Cohen's $d = -1.96$. The manipulation was thus very effective.

Materials and Procedure

After providing informed consent, participants were instructed to turn their phones off and were allocated to an individual booth without access to a computer or the internet. This was done to prevent the participants from searching online for the answers to the problems. Participants first attempted to solve the word problems of the Numerical CRT or the Verbal CRT (depending on the random order) in a paper-and-pencil format. We used the extended 7-item version of the Numerical CRT (Toplak et al., 2014) for its good psychometric and statistical properties. It comprised the original three CRT items (Frederick, 2005) and four additional items (Toplak et al., 2014). A summation score (ranging from 0 to 7, transformed into a 0–100% scale for ease of comparison with the Verbal CRT performance) had an acceptable internal consistency (Cronbach's $\alpha = 0.74$); higher scores indicated more cognitive reflection. We used the 10-item version of the Verbal CRT (Sirota et al., 2018) requiring cognitive reflection while solving word problems that do not involve mathematical calculations (e.g., “Mary’s father has 5 daughters but no sons – Nana, Nene, Nini, Nono. What is the fifth daughter’s name probably?”). A summation score (ranging from 0 to 10, transformed into a 0–100% scale) had a good internal consistency (Cronbach's $\alpha = 0.83$); higher scores indicated more cognitive reflection.

Participants then answered numerical ability items using Lipkus et al.'s Numeracy Scale, a very common measure of numerical ability (Lipkus et al., 2001). It consists of 11 simple mathematical word problems that require an understanding of basic probability concepts, the ability to convert percentages to proportions, and the ability to compare

different risk magnitudes (e.g., “If Person A’s chance of getting a disease is 1 in 100 in ten years, and person B’s risk is double that of A’s, what is B’s risk?”) (e.g., Liberali et al., 2012; Sirota et al., 2018). A summation score (ranging from 0 to 11) had a questionable internal consistency (Cronbach’s $\alpha = 0.61$) but similar to the one reported by prior research (e.g., Liberali et al., 2012; Sirota et al., 2018); a higher score indicated better numeracy.

Finally, the participants answered socio-demographic questions about their age and gender and were debriefed. We conducted the study in accordance with the ethical standards of the University of Essex ethical committee and the APA ethical guidelines. We have reported all the experiments, measures, manipulations and exclusions. The data set, R code used for analysis and the materials used in this research are available on the Open Science Framework: <https://osf.io/g65yj/>.

Results

Participants solved correctly, on average, 27.8% of the Numerical CRT problems¹. They solved slightly more word problems in the fluent font compared with the disfluent font condition (Figure 1). Thus, the observed effect was in the opposite direction to the prediction of the disfluency effect hypothesis even though not statistically significantly different, $t(308) = -0.39$, $p = .700$, $d = -0.04$. Since not rejecting the null hypothesis does not logically entail accepting the null hypothesis, we quantified the evidence for the disfluency effect hypothesis relative to the null hypothesis that included the opposite direction effect using a Bayes factor analysis (Lee & Wagenmakers, 2014; Morey & Rouder, 2015). We found strong evidence against the disfluency effect hypothesis, $BF_{0+} = 10.5$ (both using a default

¹We found no evidence for CRT presentation order effect on cognitive reflection, $F(1, 306) = 2.76$, $p = .097$, its interaction with the font, $F(1, 306) = 1.10$, $p = .296$ or with the type of CRT, $F(1, 306) = 1.75$, $p = .186$. Therefore, we did not consider the presentation order factor in further analyses.

Cauchy prior 0.707) while robustness analysis found similar results using different Cauchy prior widths ($BF_{0+} = 14.8$ for wide and $BF_{0+} = 20.8$ for ultra-wide priors). In addition, we checked the disfluency effect for individual CRT items (given that the effect was mostly driven by differences for one item in the original study), but we did not find any significant differences (Table I). Thus, we found evidence against the disfluency effect hypothesis applied to mathematical CRT problems (Hypothesis 1).

Figure 1: Effect of font and type of Cognitive Reflection Test (CRT) on performance in cognitive reflection (in % of correctly solved problems—cognitive reflection score).

Note. Horizontal lines represent means, boxes represent 95% confidence intervals, beans represent smoothed densities, and circles represent individual responses.

Table 1 The effect of font manipulation on the number of correct responses (in %) for each problem in the Numerical Cognitive Reflection Test and the Verbal Cognitive Reflection Test.

	Correct responses (in %)		Differences between disfluent and fluent font		
	Disfluent font	Fluent font	χ^2	p	ϕ
<i>Numerical CRT</i>					
NCRT 1	29.7	27.1	0.14	.706	-0.03
NCRT 2	27.7	27.7	<0.01	1.000	0.00
NCRT 3	29.7	31.6	0.06	.805	0.02
NCRT 4	14.8	22.6	2.57	.109	0.10
NCRT 5	14.2	14.2	<0.01	1.000	0.00
NCRT 6	33.5	27.1	1.24	.267	-0.07
NCRT 7	40.6	48.4	1.58	.209	0.08
<i>Verbal CRT</i>					
VCRT 1	19.4	21.3	0.08	.778	0.02
VCRT 2	65.8	72.3	1.22	.269	0.07
VCRT 3	65.2	78.7	6.39	.011	0.15
VCRT 4	60.6	68.4	1.71	.192	0.08
VCRT 5	68.4	75.5	1.60	.206	0.08
VCRT 6	42.6	48.4	0.83	.362	0.06
VCRT 7	41.3	46.5	0.61	.423	0.05
VCRT 8	45.2	45.2	<0.01	1.000	<0.01
VCRT 9	39.4	41.9	0.12	.729	0.03
VCRT 10	47.7	54.8	1.29	.256	0.07

Note. Given the number of comparisons (n) the critical p -value (p) of the chi-squared test

(χ^2) was adjusted using the Bonferroni adjustment ($\alpha = .05/n$)—for the Numerical CRT the critical value is $p = .007$ and for the Verbal CRT it is $p = .005$; ϕ indicates phi-coefficient

Participants with higher numeracy performed substantially better with the CRT math problems than those with lower numeracy ($r = 0.44$), $t(308) = 8.62$, $p < .001$; for illustration purposes only: the lower numeracy group (median split)—with a numeracy score smaller than 9 out of 11—solved on average 18.5% of the problems, $SD = 21.7\%$ and the higher numeracy group—those with a numeracy score equal to 9 or more—solved on average 37.9% of the problems, $SD = 29.1\%$. This finding enabled us to persuasively test the second hypothesis that the disfluency effect is more pronounced in those with higher numeracy.

This was not the case. In a multiple linear regression model, the disfluent font did not significantly increase the performance in the Numerical CRT, $b = -0.3$, $t = -0.02$, $p = .982$, while numeracy (used here as a continuous scale variable) did, $b = 6.1$, $t = 6.05$, $p < .001$. Critically, there was no significant interaction between font and numeracy, $b = 0.2$, $t = 0.11$, $p = .912$. Using the Bayes factor framework, the model featuring only the main effects was preferred relative to the full model featuring the main effects and the interaction term, $BF_{12} = 8.4$ and the model featuring only numeracy was preferred relative to the full model, $BF_{12} = 62.6$. In other terms, we found substantive relative evidence against the interaction and very strong relative evidence against the effect of the font and its interaction with numeracy. Thus, we found evidence against the possibility that numeracy moderates the effect of disfluency on the numerical CRT (Hypothesis 2).

Participants solved correctly, on average, 52.4% of the Verbal CRT problems. They solved slightly more word problems in the fluent font compared with the disfluent font (Figure 1). Thus, the observed effect was again in the opposite direction to the one predicted by the disfluency effect hypothesis. However, it was not statistically significant, $t(308) = -1.69$, $p = .091$, $d = -0.19$. Using a Bayes factor analysis, we found strong evidence against the disfluency effect hypothesis, $BF_{0+} = 20.7$ (both using a default Cauchy prior 0.707) while robustness analysis found similar results using different Cauchy prior widths ($BF_{0+} = 29.1$ for wide and $BF_{0+} = 41.1$ for ultra-wide priors). In addition, we tested the disfluency effect on individual items, but we did not find any significant differences (Table 1). Thus, even for the Verbal CRT, we found evidence against the disfluency hypothesis applied to non-mathematical problems tapping into cognitive reflection ability (Hypothesis 3).

Finally, to probe the robustness of the analyses reported above, we used a two-way analysis of variance with the font as a between-subjects factor and the Cognitive Reflection

Test as a within-subjects factor and the cognitive reflection performance as the dependent variable. We reached the same conclusion. Consistent with the previous observations and tests, the main effect of the font was not statistically significant, $F(1, 308) = 1.57, p = .210$; the effect of the type of CRT was significant (with the problems of the Verbal CRT being easier), $F(1, 308) = 207.49, p < .001$ and the interaction with the font was not, $F(1, 308) = 1.77, p = .185$. (Adding numeracy as a covariate into the model did not change the conclusions about the results.)

Data Synthesis: Estimating the disfluency effect on cognitive reflection with math problems

To update the meta-analytical disfluency effect on solving math problems reported by Meyer et al. (2015), we computed a meta-analysis across all published studies reported in Meyer et al. (2015) and the findings concerning the Numerical CRT reported here. We used a random effects model as implemented in the R package ‘metafor’ (Viechtbauer, 2010). The overall effect was virtually zero, Hedge’s $g = -0.01$, 95% CI $[-0.05, 0.03]$, $z = -0.44, p = .661$. We observed only minimal between-study heterogeneity in the effect sizes, $\tau^2 < 0.01$ (Cochran Q test was not significant, $Q(17) = 15.60, p = .553$) that occurred due to sampling error variability alone, $I^2 = 0.01\%$. We also computed meta-analytical Bayes factors on the same datasets using the R package ‘BayesFactor’ (Morey & Rouder, 2015). We found decisive evidence against the disfluency effect, $BF_{0+} = 151.6$ (using JSZ Bayes priors with an rscale of 1) relative to the null effect and the opposite effect, while robustness analysis found similar results using different Cauchy prior widths ($BF_{0+} = 107.2$ for rscale = 0.707 and $BF_{0+} = 214.4$ for rscale 1.414).

Discussion

In this well-powered experiment, we found additional evidence against the hypothesis that a disfluent font increases the analytical reasoning that would manifest into improved performance in the numerical and verbal Cognitive Reflection Tests. The disfluent font did not increase performance in the numerical version of the Cognitive Reflection Test and it did not change the proportion of correct responses for any of the problems (Frederick, 2005; Toplak et al., 2014). This finding contrasts with the original findings (Alter et al., 2007), but it is aligned with the reported results from other authors (Meyer et al., 2015; Thompson et al., 2013). Furthermore, the updated meta-analysis, which included the original study (Alter et al., 2007), 16 additional studies (Meyer et al., 2015) and our study, yielded a close-to-zero estimate of the disfluency effect on accuracy and a Bayes factor meta-analysis of these studies allowed us to quantify the strength of this evidence as decisive evidence against the effect of disfluency on cognitive reflection.

We also did not find support for the version of the disfluency hypothesis that considers numeracy as a boundary condition of the disfluency effect. According to this hypothesis, the effect of disfluency might not translate into higher cognitive reflection performance because people also needed to have sufficient numerical skills to benefit from increased analytical reasoning. Two pieces of evidence presented here go against this hypothesis. First, one would expect the disfluency effect to occur for those with higher numerical skills. However, we found no significant interaction of the font manipulation with numeracy on cognitive reflection. This is aligned with the evidence that self-reported SAT math scores did not moderate the disfluency effect across five different studies (Meyer et al., 2015). Our results extend these findings as we measured numeracy objectively, which circumvents possible issues with inflated self-reported SAT scores for which there is mixed

evidence (Cassady, 2001; Shepperd, 1993). Second, one would expect the disfluency effect to occur for the problems that do not require numerical skills. However, we found no disfluency effect on the Verbal Cognitive Reflection Test (Sirota et al., 2018), and only found evidence against the disfluency effect on the performance with the non-mathematical items of the Verbal CRT. This undermines the idea of lack of numeracy as a possible reason for the absence of the disfluency effect since one does not require numerical skills to answer these items. Furthermore, aligned with previous robust findings (Meyer et al., 2015), it undermines the reasoning that cognitive ability moderates the disfluency effect, since participants here solved over 50% of the problems correctly. Finally, our results also add more weight to the disconfirming evidence that a disfluent font activates analytical processing in the non-numerical tasks requiring deliberation such as belief bias syllogism or oversight problems (Meyer et al., 2015; Morsanyi & Handley, 2012; Thompson et al., 2013).

Taken together, this evidence indicates that a disfluent font does not trigger analytical processing when solving cognitive reflection problems. A statistical variation can account for the fact that the original study reported a significant effect on CRT problems. As noted by Meyer et al. (2015), the disfluency effect was driven by unusually low performance in the control, fluent condition for the “widgets problem”; the rest of the problems had very similar performances. This, in combination with very small samples used in the original research (20 participants per condition), can account for the discrepancy between the original study and the remaining 17 studies trying to replicate the finding. Although such an explanation seems the most parsimonious, we should also consider alternative explanations. The most obvious one is that solving problems featured in either of the Cognitive Reflection Tests does not require extensive analytical processing. Recent evidence coming from thinking aloud protocols and dual-response paradigms indicates that most of the participants who answered the CRT problems correctly provided the answers relatively quickly and

without extensive corrective deliberation (Bago & De Neys, 2019; Szaszi, Szollosi, Palfi, & Aczel, 2017). Putting methodological issues with this evidence aside, if true, this can mean that a disfluent font perhaps activated analytical processing but this was not required to solve the problems. However, such an explanation still cannot explain why the disfluency effect was observed in the first place in the original study: if analytical processing is not required it should not manifest in the original study. Hence, regardless of whether the CRT requires extensive analytical reasoning or not, one needs to assume that the original study reported a false-positive finding.

Several limitations of our research deserve more attention here. First, our study was not an exact replication of the original study, because it included additional problems. We cannot exclude the possibility that presenting more items is eroding the disfluency effect. One could counter-argue, however, that longer exposure to a disfluent font should activate a stronger—not a weaker—effect. One would also expect at least some disfluency effect on the first set of items since the order of the items within each instrument was fixed. This was not the case, despite the same font manipulation as in the original research, which reached remarkably similar manipulation check ratings. Second, our sample consisted of undergraduate psychology students who performed substantially lower in the Cognitive Reflection Test than the students of Princeton University participating in the original research. Finally, we did provide a manipulation check but we did not include a positive control condition to test the efficacy of the disfluency manipulation. Future research should therefore further test the disfluency effect by including a positive control condition. For example, several studies have found that disfluent fonts improve performance on memory tasks, so including a memory task might provide an effective positive control (e.g., Diemand-Yauman, Oppenheimer, & Vaughan, 2011; French et al., 2013).

To summarise, we found several pieces of evidence *against* the hypothesis that a disfluent font activates analytical processing that would, in turn, increase the solution rate of the mathematical problems used in the Cognitive Reflection Test. Such a disfluency effect was not moderated by numeracy since it was absent from the performance of people with lower as well as higher numerical ability. Finally, the disfluent font did not increase the solution rate of the non-mathematical and easier problems used in the Verbal Cognitive Reflection Test. The evidence presented here, along with prior failed replications, corroborates the null effect of disfluent fonts on cognitive reflection.

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