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Does Achievement Motivation Mediate the Semantic Achievement Priming Effect?

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The aim of our research was to understand the processes of the prime-to-behavior effects with semantic achievement primes. We extended existing models with a perspective from achievement motivation theory and additionally used achievement primes embedded in the running text of excerpts of school textbooks to simulate a more natural priming condition. Specifically, we proposed that achievement primes affect implicit achievement motivation and conducted pilot experiments and 3 main experiments to explore this proposition. We found no reliable positive effect of achievement primes on implicit achievement motivation. In light of these findings, we tested whether explicit (instead of implicit) achievement motivation is affected by achievement primes and found this to be the case. In the final experiment, we found support for the assumption that higher explicit achievement motivation implies that achievement priming affects the outcome expectations. The implications of the results are discussed, and we conclude that primes affect achievement behavior by heightening explicit achievement motivation and outcome expectancies.

Keywords: semantic priming, achievement, achievement motivation, implicit, explicit

When Nisbett and Wilson (1977) asked their subjects why they behaved the way they did, the subjects could not correctly name the experimental stimuli that actually controlled their behavioral decisions. Subjects were simply not aware of the cause of their actions. At least implicitly, in most experimental studies in psychology, researchers rely on this lack of awareness and expect subjects to behave in response to the manipulations without being aware of them. Sometimes, researchers even mislead subjects in order to blur the manipulations induced on them.

Semantic priming studies explicitly demonstrate an influence on behavior without a conscious recognition of the influence by the subjects themselves. For example, reading words that were semantically related to extraversion influenced social judgments in line with the primes, thus unconsciously influencing how the behavior of others was evaluated (Higgins & Bargh, 1987). For the achievement domain, presenting words like *win*, *master*, or *achieve* subsequently leads to higher performance, as has been shown by Bargh, Gollwitzer, Lee-Chai, Barndollar, and Trötschel (2001) and replicated by others (e.g., Engeser, 2009; Hart & Albarracin, 2009; Oikawa, 2004). According to this evidence of the prime-to-behavior effect, we should be permanently influenced by semantic primes in our daily lives. The considerable importance of achievement priming effects has been shown for pictorial achievement primes in experiments and natural settings in the workplace (Shantz & Latham, 2011).

Bargh et al. (2001; see also Bargh, 1990) proposed that the semantic primes activate the "goal of doing well." The primes were related to performance goals in the past, and reading the words subsequently led to a stronger goal of doing well, resulting in an achievement goal-guiding behavior to achieve a goal such as finding as many words as possible in word search puzzles. In the achievement domain, little theoretical and empirical attention has been paid to processes from goal activation to performance. Having more conclusive findings in this respect would be helpful to improve our understanding of the processes leading to differences in the behavioral output. In principle, we favor the explanation that primes increase motivation (alternatively, there could be a change in strategy to attain the achievement goal). A change in motivation (cf. Custers, Aarts, Oikawa, & Elliot, 2009) leads to a higher engagement in the task, subsequently leading to better performance. According to a basic motivational principle, higher motivation could be due to a change in the *incentive* of the achievement goal and/or to the heightened *expectation* to attain the goal (cf. Beckmann & Heckhausen, 2008; Eccles & Wigfield, 2002).

The priming effects in the experiments by Bargh et al. (2001) were considerably strong, and the activation likely took place for most subjects. Nevertheless, the individual differences in the "preexisting" goals do seem to be relevant. For nonachievement goals, Aarts et al. (2005) found that the priming of helpful behavior could only be found in people who had a stronger preexisting goal of helping (see also Fitzsimon & Bargh, 2004; Strahan, Spencer, & Zanna, 2002; Wheeler, DeMarree, & Petty, 2007). In the achievement domain, an early finding reported by Bargh and Gollwitzer (1994) indicated that priming effects do share basic underlying mechanisms with implicit achievement motivation, and Kazén and Kuhl (2005) also found a moderation effect for the implicit

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achievement motive.[1] Additionally, Shantz and Latham (2011) found that achievement primes in pictorial format affect implicit achievement motivation. Hart and Albarracin (2009) found the explicit achievement motive to moderate semantic priming effects in an achievement context, but Engeser (2009) found no moderation by explicit achievement motive.

Implicit achievement motivation reflects a current concern with a standard of excellence, a concern for doing things better and surpassing standards of excellence. More specifically, achievement motivation is a heightened incentive placed on achievement outcomes (Atkinson, 1957; cf. Brunstein & Heckhausen, 2008). Since the first systematic research on achievement motivation (D. C. McClelland, Atkinson, Clark, & Lowell, 1953), a considerable body of research has shown that it predicts achievement behavior such as the level of aspiration and persistence (Brunstein & Heckhausen, 2008; D. C. McClelland, 1985). Implicit achievement motivation is assessed with the picture-story exercise (PSE; Pang, 2010), based on the Thematic Apperception Task by Morgan and Murray (1935). Stories are assumed to express the implicit (i.e., not consciously assessable) concerns of the person writing the story. Studies on the development of a coding manual revealed that experimental variations of achievement context sensitively and reliably changed the concerns for achievement expressed in the stories (D. C. McClelland, Clark, Robey, & Atkinson, 1949). There are small correlations with questionnaire measures of explicit achievement motivation, indicating that these are two different types of motivation, with implicit motivation having an affective-experiential basis and explicit motivation having a cognitive one (D. C. McClelland, Koestner, & Weinberger, 1989; cf. meta-analysis by Spangler, 1992).

Knowing more about which aspects for which individual are affected by achievement primes would deepen our understanding of the processes that mediate prime-to-behavior effects. For example, knowing that prime-to-behavior effects are associated with heightened salience of incentives would allow the prediction that the priming effect will be more likely if a person is sensitive to placing high incentives for achievement goals in general. However, the finding that incentives (or expectations) are not affected by the primes would lend credence to a direct effect as proposed by the ideomotor principle (Dijksterhuis & Bargh, 2001).

The Present Research

In our research, we focus on motivational effects of achievement priming and extend existing models with achievement motivation theory. Focusing on effects of achievement primes on motivation attempts to systematically study the part of the prime-to-achievement effect that has not been focused on in past research. The moderation effects of the implicit achievement motive mentioned above and the study by Shantz and Latham (2011) provide a first indication that implicit achievement motivation is affected by achievement primes. Specifically, the achievement primes are assumed to be closely associated with the experience of past achievement situations, providing a cue for rewards of attaining an achievement goal (Brunstein & Heckhausen, 2008; D. C. McClelland, 1985). The association should provide what Schultheiss (2008) called the referential processing of textual stimuli into a more experiential format to activate implicit achievement motivation. And because achievement motivation is defined by a heightened incentive placed on achievement outcomes, higher implicit achievement motivation should be associated with a higher incentive attached to attaining an achievement goal. A further reason for the involvement of implicit achievement motivation is that the priming effect surpasses conscious recognition.

To simulate a more natural priming manipulation, we embedded the achievement primes in the running text of excerpts of school books. Achievement primes in school books are of great practical significance. Children who read achievement primes in school books will achieve better performance, and cumulating effects should lead to better overall academic achievement. Thus, school books containing more achievement primes should foster children's performance, and first evidence points in this direction (Engeser, Rheinberg, & Möller, 2009). Additionally, the achievement primes in children's books are associated with higher academic performance (Engeser, Hollricher, & Baumann, 2013). To use embedded primes is similar to the priming manipulation with scrambled sentences (e.g., Experiment 2 in Bargh et al., 2001; Fitzsimons & Shah, 2008), which provided a reliable method to find prime-to-behavior effects for semantic primes.

In sum, we propose a model in which the achievement primes in running text arouse implicit achievement motivation (i.e., implicit current concern with a standard of excellence). This leads to a higher incentive for performing well (see upper part of Figure 1). Higher motivation should subsequently lead to a higher engagement in the task, likely leading to better performance. In our experiments, we started out by focusing on the first link of the model (pilot experiments and Experiment 1) and expected achievement primes to lead to a higher implicit achievement motivation (Hypothesis 1a). As we presume that the measurement of implicit achievement motivation does terminate the effect of achievement primes, we did not measure performance within the same experiment. Additionally and more importantly, including an achievement task would most likely increase achievement motivation irrespective of priming (see context sensitivity in the next paragraph). Therefore, we focus solely on the expected process variable of implicit achievement motivation.

The measure of achievement motivation after the priming variable served as the dependent variable. Other researchers have also used implicit measures to assess situational influences on implicit motivation (Hagemeyer & Neyer, 2012; Kuhl & Kazén, 2008; Shantz & Latham, 2009; Wirth & Schultheiss, 2006). These studies showed that the measure of implicit motivation is sensitive to experimental manipulations, just as the first systematic studies of implicit achievement motivation (D. C. McClelland et al., 1949) have already demonstrated. As the effect on a process variable should be stronger than on the outcome (based on previous studies, at least medium-sized effects of prime on performance could be assumed), we expect a strong effect on implicit achievement motivation.

In Experiment 2, in addition to implicit achievement motivation, we included explicit achievement motivation to test the alternative

[1] *Motive* refers to individual differences in motive strength (i.e., as a trait construct). *Motivation* refers to an actual state. This is analog to trait anxiety (the readiness to experience anxiety) and the actual experience of anxiety in a given situation. To avoid misunderstandings, we refer to motives as baseline motivation in our own research (see details in the penultimate paragraph in The Present Research section).

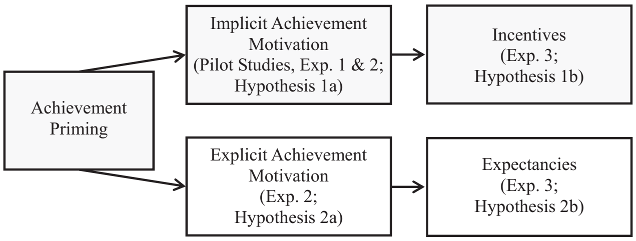

Figure 1. Initial model (upper path) and modified model (lower path) of process variables involved in the prime-to-behavior effect. Exp. = experiment.

hypothesis that priming affects the more cognitive-based explicit achievement motivation (Hypothesis 2a; see lower part of Figure 1). We outlined above that higher implicit achievement motivation should go along with heightened incentive placed on achievement outcomes. In contrast, a stronger explicit achievement motivation should go along with a change in the expectation for performing well, and only to a small degree with higher incentives (cf. Brunstein & Heckhausen, 2008; Nicholls, 1984). At least if incentives and expectations are low, an increase in one or both should lead to higher engagement and likely to higher performance. In Experiment 3, we measured incentives and outcome expectations to test the hypotheses that achievement primes affect incentives (Hypothesis 1b) and expectations (Hypothesis 2b; see Figure 1).

Beside this main research question, we also tested whether priming effects are stronger for subjects higher in implicit achievement motivation before the priming manipulation (Hypothesis 3a). Therefore, we measured implicit achievement motivation before the experimental manipulation, too. As the introduction and procedure for the measure before the experiment are constant for all subjects, this presents the standard procedure when measuring individual differences in motive strength (cf. Pang, 2010). We referred to it as the *baseline measure of implicit achievement motivation* in order to avoid misunderstandings, as it makes clear that it is measured before experimental manipulations and serves as an independent variable. In Experiments 2 and 3, we also measured the baseline explicit achievement motivation to test whether priming effects are stronger for subjects higher in baseline explicit achievement motivation (Hypothesis 3b).

Pilot Experiments

Besides a first test of our hypotheses in respect to the implicit achievement motivation (Hypotheses 1a and 3a), the three pilot studies aimed at examining the priming materials and experimental procedures. The first pilot study was conducted online. Forty subjects participated in the experiment. They were randomly selected to subsequently read either neutral texts or texts with achievement content taken from ninth-grade school textbooks (we did not measure baseline achievement motivation in this pilot study). For the neutral condition, we chose texts with no achievement content, and in the achievement condition, we chose texts with repeated achievement content according to Winter's (1994) manual. To ensure that the textual material is processed more deeply, one sentence was scrambled, with one word having to be reordered to make a meaningful sentence. Afterward, implicit achievement motivation was measured using a PSE, with four pictures in a standard instruction and procedure (see Pang, 2010). The PSE protocols were coded using Winter's (1994) manual.[2] The correlation of the experimental manipulation with implicit achievement motivation was negative and marginally significant ($r = -.28$, $p = .079$). Thus, these results actually indicate the opposite relationship to what we expected in Hypothesis 1a.

In the second pilot study, 30 subjects took part in the experiment in the laboratory. Subjects worked on a five-picture set of the PSE before and on a different five-picture set after the priming manipulation, again in a standard procedure and coded using Winter's (1994) manual. We modified the priming manipulation. We took neutral texts and added words that did not fit into the sentence (mimicking the standard scrambled sentences procedure). In the neutral condition, the words were neutral. In the achievement condition, the words were achievement related. As an additional achievement condition, we probed achievement cloze texts. The achievement-scrambled condition had a marginally significant positive effect ($\beta = .34$, $t = 1.73$, $p = .096$). The achievement cloze condition had no and no marginally statistically reliable effect. The baseline implicit achievement had a positive effect ($\beta = .45$, $t = 2.56$, $p = .018$) on achievement motivation after the priming manipulations, but baseline achievement motivation did not moderate any effects. Thus, our Hypothesis 1a of a positive effect of the achievement condition only revealed some support in the achievement-scrambled condition, and we found no support that baseline implicit achievement motivation moderated the priming effect (Hypothesis 3a).

Seventy-four subjects took part in the third pilot study. The procedure was the same as in the second pilot study, with two further experimental conditions added. We included an additional scrambled sentences condition with different achievement primes and a neutral cloze condition. In contrast to the previous pilot studies, all PSE protocols were coded with the manual provided by Heckhausen (1963; see Pang, 2010). We found no statistically reliable effect of the experimental condition on achievement motivation. All interactions of experimental conditions with the achievement motivation (baseline) were weak and statistically nonsignificant.

Taken together, the first pilot study revealed a negative (marginally significant) effect. In the second pilot study, we found a marginally significant positive effect, but could not replicate this effect in the third pilot study. In terms of the pilot studies, we have to conclude that there is only minimal evidence of a reliable positive priming effect of achievement primes on implicit achievement motivation (Hypothesis 1a) and of a moderation of baseline implicit achievement motivation (Hypothesis 3a). We expected to

[2] The manual has been validated and has shown extensive predictive validity for various applications ranging from speeches by political leaders (Winter, 2000), motive-goal congruence, and well-being (Brunstein, Schultheiss, & Grässmann, 1998; Hofer & Chasiotis, 2003), to experimental studies (Brunstein & Hoyer, 2002; Woike, Mcleod, & Goggin, 2003), and scoring of school and children's books (Engeser et al., 2009; Engeser et al., 2013). Achievement imagery is scored when a concern with a standard of excellence is expressed, such as doing good, reaching an achievement goal, winning or competing, positive and negative evaluation of success and failure, and unique accomplishments. Coders reached more than 90% agreement with the calibration set of Winter's (1994) manual for achievement, power, and affiliation imagery.

find a strong effect that should have been detected with only 10–20 subjects in each condition. Nevertheless, our sample size was too small to reliably detect a possibly small priming effect. Additionally, the reliability of implicit achievement motivation is only moderate (cf. Pang, 2010). To test our hypothesis of an effect of achievement priming on implicit achievement motivation, we increased the sample size in our first main experiment, and combined with the point we discuss next, this would enable us to detect even small effects measured with moderate reliability.

In the pilot studies, we were unable to determine the effect of single texts. Thus, it might be the case that one text would arouse achievement motivation, whereas others would not. Similarly, the last text presented just before measuring achievement motivation could be the most influential. Therefore, we changed the design of the experiment and measured implicit achievement motivation after the presentation of each text. To ensure that we could test more than just a few priming texts in a reasonable amount of time, we abandoned the measurement of implicit motivation with the PSE and switched to another well-established alternative measure of implicit motivation, the Operant Motive Test (OMT; Kuhl & Scheffer, 1999). The time required to complete a single picture item of the OMT is about 1 min, which is substantially shorter than the standard time of 4–5 min given for writing PSE stories.

As we did not find a consistent pattern of results, the pilot studies did not reveal which priming manipulation is favorable. Given the negative evidence against the previous priming material, we decided to return to the priming manipulation of running text, but without the scrambling of one sentence or using a cloze text. We wanted to simulate a natural priming condition and thereby also avoid that the kind of presentation of the priming text itself represented an achievement task (e.g., unscrambling sentences represents an achievement task). As argued above, an achievement task possibly terminates the priming effect in the achievement prime condition or activates implicit achievement motivation in the neutral condition as well. Taking plain running text without any type of scrambling or cloze text also led us to choose different texts for the neutral and achievement conditions. We did this because taking a textual passage with high-achievement content and deleting the achievement content has the disadvantage that the text would have to be strongly modified. In addition, even after modification, neutralized text would probably have some minimal achievement feature. Thus, an achievement text could hardly be transformed into an achievement neutral text (and vice versa), and we therefore used different texts for the neutral and achievement conditions.

Experiment 1

Method

Subjects. Eighty-nine subjects took part in the study, which was conducted at the University of Trier. There was one missing value for age and one for gender, and we replaced these with the population mean for all analyses. Subjects' age ranged from 18 to 42 years, with a mean of 23.35 years (SD = 3.66); 63 were women. All subjects were students (24 studying psychology), and they were paid 15 Euros for their participation. None of the participants were aware of the experimental manipulation.

Procedure and design. Subjects were informed about the experiment via an e-mail addressed to all students of the University of Trier and via flyers. The experiment was conducted in a separate, small room. As they arrived, subjects were greeted, and written instructions on the computer informed them that the study was about language processing and about the experimental procedure. The experiment began with the measure of baseline implicit achievement motivation. After finishing this measure, subjects were informed that between each session of "imagining stories," they had to read a short text. To ensure careful reading, we informed the subjects that later in the experiment, we would come back to their evaluation of the texts. In total, we administered 10 texts and subsequently 10 measures of implicit achievement motivation with the OMT. They then completed various personality measures and worked on an additional experiment that was not of relevance to the analysis presented here. Finally, subjects indicated their age, gender, and level of education; answered funneled questions regarding the purpose of the study; rated how they liked the texts; and were debriefed.

We had a one-factorial experimental design (neutral vs. achievement) with the baseline implicit achievement motivation as a quantitatively assessed second independent variable. The repeated measurement of implicit achievement motivation is the dependent variable. The neutral condition consisted of neutral texts only. The achievement condition included neutral and achievement texts (five each; randomly selected), with the neutral texts being the same as in the neutral condition (randomly selected out of the 10 neutral texts). Due to the repeated measures design, we ran multilevel regression analyses. We predicted implicit achievement motivation (Level 1) with the baseline implicit achievement motivation, priming condition, and interaction effects of these as between-subject predictors (Level 2 predictors). The baseline implicit achievement motivation was standardized before entered into the analysis. To assess the explained variance of the predictors, we compared the between-subjects variance of an unconditional model (no predictors included) with the model including the predictors (Raudenbush & Bryk, 2002). The analyses were run with the software HLM 7.01 (Raudenbush, Bryk, & Congdon, 2013) using restricted maximum likelihood estimation (with robust standard errors) and assuming normal distribution for the dependent variable.

Materials.

Textual material. The basis of our materials was again excerpts taken from ninth-grade school textbooks (mathematics and language). Subjects worked on 10 texts, with lengths varying between 40 and 84 words, with a mean of 62.53 ($SD = 9.83$) ($M = 62.00$ for neutral and $M = 63.60$ for achievement texts). Achievement texts were selected on the basis of Winter's (1994) scoring manual of motive imagery for running text (see Pilot Study 1 and Footnote 1). Examples for each achievement text are "for many problems you need staying power and a good overview"; ". . . how to attain the best solution for the fast completion of the order"; ". . . a laborious but groundbreaking invention"; ". . . to get a fast and accurate arithmetic solution"; and "the Tyroleans were faster and won the race . . . ." Neutral texts contained no achievement content according to Winter's manual.

OMT administration. The OMT (Kuhl & Scheffer, 1999) uses a modified PSE technique. As in the PSE, the subjects are asked to invent a story for a presented picture. However, in contrast to the

PSE, the subjects did not have to write down the story as such and instead had to respond with a few keywords to the following questions (these are the same questions used as leading questions in the PSE administration to aid the formulation of a complete story): “What is important for the person in this situation and what is the person doing?”; “How does the person feel?”; “Why does the person feel this way?”; and “How does the story end?” In the standard procedure, 15 pictures (drawings) are presented to measure the motives of achievement, affiliation, and power. Five pictures each are used to predominantly arouse the respective motive. As we were focusing on implicit achievement motivation, we used five pictures that predominantly aroused achievement motivation (Pictures 5, 6, 7, 8, and 9) and five of the other pictures that arouse achievement motivation at least on a regular basis (Pictures 10, 11, 12, 13, and 14). For the measurement of implicit achievement motivation before the experimental manipulations, we departed from the standard pictorial stimuli of the OMT. Instead, we used pictures that elicit considerably high-achievement imagery in PSE measures. Pictures were Boy at Desk, Gymnast, Workers, Climber, and Girl at a Blackboard. The first three pictures have been widely used in previous research on implicit motives (cf. Pang, 2010), and the last two pictures were used in our pilot studies.

Coding procedure. The OMT is a modified and extended coding system based on existing coding procedures to measure implicit motivation with the PSE (Kuhl & Scheffer, 1999). Extensive research on the OMT has been reported by Scheffer (2005). With $r = .64$, the scores are strongly correlated with the scores according to Winter’s (1994) coding procedure, pointing to high convergent validity, which is important as we used Winter’s manual in the pilot studies described above. Internal consistency and retest stability proved to be sufficient and are high compared with other measures of implicit constructs, including the PSE. Scheffer, Kuhl, and Eichstaedt (2003) reported estimates of the internal consistency of $\alpha = .70$ and a retest reliability of $r = .72$ (see also Scheffer, 2005). Further, the OMT has been proved to have good validity in different domains and research designs (Baumann & Scheffer, 2010; Hofer, Busch, Chasiotis, Kärtner, & Campos, 2008; Kazén & Kuhl, 2005; Lang, Zettler, Ewen, & Hülsberger, 2012). The OMT focuses on the modes or functional pathways of satisfying needs or motives rather than need strength per se, as in the coding manuals by Winter (1994) and Heckhausen (1963) used in the pilot studies. The OMT differentiates four approach components and one avoidance component for each motive. For the achievement motive, the two positive modes of approach motivation are self-determined flow (learning something, being absorbed, being concentrated) and standard of excellence (doing something well, being proud, being focused on results). The further two approach components are coping with failure (perception of threat associated with active coping) and pressure to achieve (social standards, relief after success). The avoidance component of the achievement motive is a passive avoidance of achievement situations (e.g., fear of failure). For the purpose of the presented studies, the sum of the four approach components of the achievement motive was computed to assess implicit achievement motivation. For the description of affiliation and power coding, see Scheffer et al. (2003). For each picture, exclusively the dominant motive image is scored, although more than one motive image may be mentioned. Moreover, only the dominant component is scored for each motive. Scoring was carried out by well-trained assistants who had attained sufficient reliability across several studies. For 10 subjects, the OMT was double-coded, and the correlations of scores for achievement was $r = .80$. Discrepancies were discussed before the final scores were entered into the analysis.

Results and Discussion

The mean values for baseline achievement motivation was 0.67 ($SD = 0.47$), and for implicit motivation after priming, $M = 0.18$ ($SD = 0.38$). There were no main or interaction effects of age and gender in the analyses conducted.

The fixed effects of the multilevel regression analysis are depicted in Table 1. There is a positive albeit nonsignificant main effect for baseline achievement motivation, a weak and even negative effect for the achievement content of the texts, and no reliable interaction effect. Therefore, we were unable to find support for our expectation that the achievement content increased the achievement motivation (Hypothesis 1a) and that this expected effect should be moderated by baseline implicit achievement motivation (Hypothesis 3a). The comparison of the between-subjects variance of the unconditional model with the conditional model (including prime as the single predictor) revealed that including prime even increased between-subject variance. Thus, the prime could not explain any substantial variance and priming effects. The random effect for the intercept is also nonsignificant, $\chi^2(85) = 64.66$, $p = .951$.

We found that achievement primes do not reliably activate implicit achievement motivation. These results are in line with the pilot studies, and it is reasonable to question our theoretical position that implicit achievement motivation will be aroused

Table 1
Summary of the Fixed Effect on Achievement Motivation (Experiments 1 and 2)

	Implicit achievement motivation (Experiment 1)			Implicit achievement motivation (Experiment 2)			Explicit achievement motivation (Experiment 2)		
Variable	*B*	*SE B*	T^a	*B*	*SE B*	T^b	*B*	*SE B*	T^b
Intercept	.177	.011	16.04**	.281	.011	25.13**	4.273	.037	114.36**
Achievement motivation (baseline)	.013	.011	1.19	.006	.011	.51	.396	.043	9.25**
Priming (neutral vs. achievement)	−.003	.011	−0.24	−.015	.011	−1.37	.080	.037	2.15*
Interaction of motivation and priming	.005	.011	0.45	−.001	.011	−.08	.035	.043	0.81

[a] $df = 85$. [b] $df = 122$.
* $p < .05$. ** $p < .01$.

by semantic achievement primes. This also holds for the expectation that baseline implicit motivation will be a moderator of the semantic priming effect on achievement motivation. We focused on implicit motivation because priming is an implicit process of which a person is unaware. However, implicit motivation is thought to be aroused by "natural" affective-experiential-based stimuli (cf. Brunstein & Heckhausen, 2008; Schultheiss, 2008). Natural stimuli include difficulty of a task or immediate feedback, and the incentives are emotional changes in mastering the task. Semantic primes are clearly not natural stimuli, although we expected them to be highly associated with them through extensive learning processes. Research in the achievement motivation domain has shown that explicit instructions do not consistently lead to higher implicit achievement motivation (see Brunstein, 2008). In a similar vein, the results of Shantz and Latham (2009) for pictorial achievement primes (i.e., affective-experiential based) showed a priming effect on implicit achievement motivation, which did not generalize to semantic primes in our experiments. Explicit achievement motivation, however, is thought to be based on language (cognitive based) and could, therefore, be a good candidate for being affected by achievement primes.

The results from three pilot studies and Experiment 1 did not confirm our expectation concerning the priming effect on implicit measures of motivation. Therefore, we subsequently tested the extent to which achievement priming affects explicit measures of motivation (Hypothesis 2a; see lower part of Figure 1). We did not only measure explicit achievement motivation to test our theoretical expectations, but again implicit achievement motivation as well. This was done in order to contrast both within a single experiment. We also measured baseline explicit and implicit achievement motivation to test whether priming effects are stronger for subjects higher in achievement motivation before the priming manipulation (Hypotheses 3a and 3b).

Experiment 2

Method

Subjects. One hundred thirty-five subjects took part in the study, which was conducted at the University of Trier. Nine subjects were aware of the experimental manipulation or did not provide full information on the achievement motivation measures. These subjects were excluded from the analysis. Thirteen subjects provided no information about age, gender, and subject of study, and for these subjects, missing values were replaced by the population mean. Of the 126 subjects entered into the analyses, 85 were women. Their ages ranged from 19 to 31 years, with a mean of 22.86 years ($SD = 2.53$). All subjects were students (43 of them studying psychology), and they were paid €15 (about $20 U.S.) for their participation.

Procedure and design. The procedure was the same as in Experiment 1, but with the following exceptions. (a) After the measurement of baseline implicit achievement motivation, we measured explicit achievement motivation with three items before the experimental manipulation. We also measured fear of failure with two items. We chose to use this small number of items to avoid the priming of achievement motivation before the experimental manipulation and included 11 filler items as well. (b) We used 16 (instead of 10 in Experiment 1) trials. (c) A recognition text was administered for the priming texts. We selected one sentence from each text and presented sentences that had not been presented previously. The number of correctly classified sentences (whether they had been presented before or not) served as a measure of processing depth of the texts.

We had the same design as in Experiment 1, except that we repeatedly measured two dependent variables (implicit and explicit achievement motivation) and both the baseline implicit and explicit achievement motivation. The neutral condition consisted of neutral texts only. The achievement condition included neutral and achievement texts mixed together (eight each), with the neutral texts being the same as in the neutral condition (randomly selected out of the 16 neutral texts). Due to the repeated measures design, we ran multilevel regression analyses analog to Experiment 1.

Materials.

Textual material. The basis of our materials was again excerpts taken from ninth-grade school textbooks (mathematics and language). Text length varied from 36 to 79 words, with a mean of 57.23 words ($SD = 10.74$) ($M = 55.00$ for neutral and $M = 60.80$ for achievement texts). We used five of the achievement texts from the previous experiments and added three new ones on the basis of the scoring manual by Winter (1994). Examples of new achievement texts are: "The world record for fastest SMS texting is 160 characters in 41.52 seconds"; ". . . with my experience and my attention, I succeeded in my work"; and "I have to study more." Neutral texts had no achievement content according to Winter's manual.

OMT administration and coding procedure. The administration was the same as in Experiment 1, with the exception that only eight pictures instead of 10 were selected for the measurement of implicit motivation (Pictures 6, 7, 8, 9, 10, 11, 13, and 14). Scoring was carried out by two well-trained assistants who had attained sufficient reliability across several studies. For 10 subjects, the OMT was double-coded, and the correlation of scores for achievement was $r = .81$. Discrepancies were discussed before the final scores were entered into the analysis.

Explicit measures. Explicit measures for the achievement domain were selected from the German version (Dahme, Jungnickel, & Rathje, 1993) of the Achievement Motive Scale (AMS; Gjesme & Nygard, 1970) and the Unified Motive Scale (UMS; Schönbrodt & Gerstenberg, 2012). The AMS is widely used in Scandinavia and Germany and has been established as a reliable and valid instrument (e.g., Engeser & Langens, 2010; Lang & Fries, 2006; Rand, 1987). The UMS was established on the basis of simultaneous assessment of explicit motives of several established instruments and the respective psychometric analyses. Therefore, the AMS and UMS do overlap for some items. Both scales measure the hope of success (HS) and fear of failure (FF) component of achievement motivation. Engeser and Langens (2010) showed medium-sized correlations for HS with the facet of achievement striving of the factor conscientiousness and strong correlations of FF with neuroticism of the NEO Personality Inventory—Revised (NEO-PI–R; Ostendorf & Angleitner, 2004). Example items for HS are "I like situations in which I can find out how capable I am" (AMS) and "I am attracted to situations that allow me to test my abilities" (AMS, UMS). For FF, one example is "I feel uneasy doing something if I am not sure of succeeding." Internal consistencies for the AMS in the samples reported by

Lang and Fries (2006) were $\alpha > .80$ for HS and $\alpha > .86$ for FF (15 items each). For the UMS, internal consistencies were $\alpha = .86$ (10 items) for HS and $\alpha = .85$ (14 items) for FF. The retest reliabilities for the UMS were $r = .91$ for HS and $r = .85$ for FF. For the measure of baseline explicit motivation in HS, we used two items of the AMS and one item of the UMS. FF was measured with two items that are both represented in the AMS and UMS. Unrelated to achievement, we included Power, Affiliation, and Intimacy with two items of the UMS, respectively. Seven items were included from two other instruments: the NEO-PI–R and the Personality Style and Disorder Inventory (Kuhl & Kazén, 2009). Explicit achievement motivation (HS) within the experiment was assessed with two items for each trial (for eight trials, i.e., a total of 16 items). FF was measured with one item. Additional items included one item assessing affiliation and power, and two filler items selected from the NEO-PI–R. Items were randomly selected, but HS and FF were presented first. The response format for all items ranged from 1 (*not at all*) to 6 (*completely true*).

Results and Discussion

The mean value for baseline implicit achievement motivation was 0.68 ($SD = 0.19$), and for the implicit motivation after the priming, $M = 0.28$ ($SD = 0.45$). The mean for the HS component of baseline explicit achievement motivation was 4.35 ($SD = 0.79$), for FF, $M = 4.13$ ($SD = 1.10$), and after the priming, the mean was 4.28 (SD 0.91) for HS and 3.52 ($SD = 1.32$) for FF. The correlations between HS and FS were $r = -.19$ and $r = -.10$ (ps $< .036$). There were no main or interaction effects of age, gender, and processing depth for the analyses conducted.

The fixed effects of the multilevel regression analysis for implicit motivation are depicted in Table 1. There is a positive albeit nonsignificant main effect for baseline implicit achievement motivation, a weak and even negative effect for the achievement content of the texts, and no reliable interaction effect. The explained between-subject variance by the conditional model including prime as a single predictor was 3% (indicating a weak effect). As outlined by Masson (2011), we calculated Bayesian posterior probabilities for this negative effect. The explained variance (exactly 3.33%) with 124 degrees of freedom revealed a probability for the hypothesis of a negative effect of $p = .42$ (the probability of the null hypothesis is $p = .58$, respectively). The random effect for the intercept is nonsignificant, $\chi^2(122) = 79.24$, $p = .999$.

The results for the HS component of explicit achievement motivation are depicted in Table 1 (final columns). There is a reliable main effect for baseline explicit achievement motivation and, most importantly, for the achievement content of the text supporting Hypothesis 2b. The interaction indicates that the priming effect is stronger for persons high in baseline explicit achievement motivation, but this effect is not reliable (thus, we could not support our moderation Hypothesis 3b). The explained between-subject variance by the model is very high (57%). Taking only prime as a predictor, the model explains 4% of the variance. An analog analysis for explicit FF reveals a main effect for baseline FF and priming ($B = .555$ [$SE = 0.061$], $t = 9.02$, $p < .001$; and $B = -.119$ [$SE = 0.060$], $t = -2.00$, $p = .048$). The interaction was not significant ($B = -.041$ [$SE = 0.062$], $t = -.66$, $p = .508$). Priming explained 5% of between-subjects variance. The random effects were significant for both HS and FS, $\chi^2(122) = 315.27$, $p < .001$; $\chi^2(122) = 437.04$, $p < .001$. This means that there were variations in HS and FS that were not explained by the model (and could also not have been explained by including age or gender into the model).

In additional analyses, we modeled achievement content of the texts on Level 1 (on the level of each trial); baseline achievement motivation was again modeled at Level 2 (for random effects modeled on the intercept only). We contrasted (a) neutral text versus achievement text and (b) neutral text in the neutral conditions versus neutral text in the achievement condition. For HS, we found positive effects for both contrasts, with the second contrast being marginally significant ($B = .087$ [$SE = 0.059$], $t = 1.46$, $p = .147$; and $B = .156$ [$SE = 0.083$], $t = 1.86$, $p = .062$). One possible mechanism explaining these results is that achievement texts not only influenced the measure in the same trial but also had a lagged effect on the subsequent measures. Otherwise, it is hard to explain why neutral texts in the achievement condition aroused higher levels of HS than in the neutral condition. Therefore, we selected adjacent trials in which an achievement text was followed by a neutral text ($n = 440$) and modeled the lagged effects of the first contrast. As expected on the basis of the results of the previous analysis, a time lag effect was found ($B = .204$ [$SE = 0.093$], $t = 2.20$, $p = .029$). The simultaneous effect is still positive, but not significant ($B = .043$ [$SE = 0.089$], $t = 0.48$, $p = .628$).

In an analog analysis with FF, we found negative effects for both contrasts, with the first contrast reaching significance ($B = -.262$ [$SE = 0.085$], $t = -3.08$, $p = .003$; and $B = -.132$ [$SE = 0.124$], $t = -1.07$, $p = .287$). As could be expected from this result, time-lagged analysis showed a simultaneous effect but no time-lagged effect for the first contrast ($B = -.273$ [$SE = 0.121$], $t = -2.56$, $p = .026$; and $B = .039$ [$SE = 0.114$], $t = 0.34$, $p = .731$). Thus, achievement primes do minimize FF in the same trial exclusively and do not exert a lagged effect as for HS.

Results of this experiment also reveal that achievement primes do not reliably arouse implicit achievement motivation as expected in Hypothesis 1a. However, we did find support for the hypothesis that explicit achievement motivation is affected by semantic achievement primes (Hypothesis 2a). The effect on the explicit measure was found for both components of explicit achievement motivation. Achievement primes increased HS and decreased FF. Post hoc analyses implied that the priming effects prevail to the next trials for HS, but not for FF.

According to our model, a stronger explicit achievement motivation should go along with a change in expectation for performing well (Hypothesis 2b; see lower part of Figure 1). Incentives for performing well should not be affected as we did not find a reliable effect of priming on implicit achievement motivation in the proceeding experiments. In our next experiment, we directly tested the effect of achievement primes on incentives and expectations, assuming an effect of achievement primes on expectations exclusively (Hypothesis 2b). Incentives and expectations were measured along with performance. We included a feedback condition to manipulate the achievement context of the task, as context has been shown to be an important moderator of behavioral priming effects (cf. Gawronski & Cesario, 2013; Hart & Albarracin, 2009).

Experiment 3

Method

Subjects. One hundred one subjects took part in the study conducted at the University of Trier. Five subjects did not follow the instructions (not pressing *enter* after they typed the number), and two subjects obviously did not seriously work on the tasks. These subjects were excluded from the analysis. Of the remaining 94 subjects, 65 were women. Their ages ranged from 18 to 31 years, with a mean of 22.76 years ($SD = 2.80$). All subjects were students (17 studying psychology) and received course credits or were paid for participation. No subject was aware of the experimental manipulation.

Procedure and design. Subjects were informed about the experiment via an e-mail addressed to all students of the University of Trier and via flyers. The experiment was conducted in separate small rooms. Subjects were told that the study is about language processing and concentration. Written instructions on the computer informed subjects about the experimental procedure, and then the arithmetic task with a practice block of six tasks started. When subjects solved fewer than four of the six tasks correctly, they were again provided the instructions and had to solve the practice block again. Then, subjects completed two blocks of 15 arithmetic tasks to assess baseline performance. After the baseline assessment, the presentation of the task changed. Each task had to be solved within a time limit (see detailed description of the task below). If subjects were not able to solve a task within the time limit, the next task appeared. Additionally, subjects were informed that they should carefully read a text before each block of tasks because it would become relevant later in the experiment. After they read the text, a block of 10 tasks with a time limit was presented, and subjects received feedback about how many tasks they solved correctly. Then, the experimental blocks started. Subjects in the neutral priming condition read only neutral texts, and subjects in the achievement priming condition were presented six achievement priming texts and four neutral texts (neutral texts were the same as in the neutral condition). After reading the text and before each experimental block of 10 tasks each, they had to indicate the incentives of and expectations about the next block of tasks. After each block, subjects received feedback about how many tasks they solved correctly. Half the subjects (randomly selected) received additional feedback regarding their performance compared with others (normative feedback). A plus sign indicated performing better than half the other subjects, and a minus sign indicated performing worse than average. Independent of their actual performance, subjects received five plus and five minus signs selected at random. After completing five of the total 10 blocks of tasks, subjects were informed that the time limit for completing the task would be slightly shorter than before. After completion, subjects worked on the recognition task (see Experiment 2) and indicated their age, gender, and whether they had any insight into the experimental manipulations.

We had a between-subjects design with 2 (neutral vs. achievement text) $\times$ 2 (no normative feedback, normative feedback) experimental conditions, with repeated measures of the dependent variables of incentives, expectations, and performance. Due to the repeated measures, we again conducted multilevel regression analyses. If not mentioned otherwise, we predicted the dependent variable (Level 1) with experimental condition and the interaction of the experimental condition as between-subjects predictors (Level 2 predictors). Control variables differed with respect to the dependent variable and entered as between-subjects predictors.

Materials.

Textual material. The basis of our materials was again excerpts taken from ninth-grade school textbooks (mathematics and language). Subjects worked on 10 texts, with lengths varying between 44 and 65 words, with a mean of 56.63 words ($SD = 7.02$) ($M = 56.80$ for neutral and $M = 56.33$ for achievement texts). The texts were taken from Experiment 2, although some texts were omitted due to fewer rounds than in Experiment 2 (selection was based on restricting variance of word count).

Arithmetic task. We used the arithmetic tasks from the Concentration Performance Test (Düker, Lienert, Lukesch, & Mayrhofer, 2001). The test is highly standardized, with time per task and difficulty being the same for each block. The tasks consist of simple arithmetic tasks, with an additional rule applied, thus making the task more demanding. Specifically, subjects see two lines of three single numbers. Subjects have to add up the two lines, and if the sum of the first line is greater than the second one, the sums of the lines have to be subtracted. If the sum of the first line is lower than the second one, both sums have to be added. Subjects must enter the results, and then the next two lines of numbers appeared on the screen. For baseline assessment, two blocks of 15 arithmetic tasks were presented with no time limit. The individual speed in baseline tasks was subsequently used to calculate the time limit for the experimental tasks. We tried to set the limit in such a way that subjects were only able to solve little more than half of the tasks. In preliminary testing, we first set the time limit to mean baseline time multiplied by .80 for the first five blocks and multiplied by .72 for the final five blocks. The first six subjects solved about seven tasks with this time limit, and we therefore reduced the time limit and multiplied the mean baseline time by .76 and .66. With the new time limit, the following seven subjects solved $M = 5.80$ ($SD = 2.12$) tasks, and we decided to use this time limit in our main study.

Incentives. We based the assessment of incentives on the studies of incentive gradients by Halisch and Heckhausen (1988). Before each block, we asked the subjects to indicate the number of tasks they felt they should complete to begin to feel satisfied with their performance. This value indicates the minimum number of tasks it takes for the subjects to start to feel satisfied. Additionally, subjects also indicated how satisfied they actually were with the number of tasks they had previously indicated on a scale ranging from 1 (*not satisfied*) to 10 (*totally satisfied*). Finally, subjects were asked to indicate the number of completed tasks that would make them feel twice as satisfied. The difference between the first question and this question allowed the calculation of an incentive gradient. The smaller the number, the steeper the incentive gradient, thus indicating that the subjects are more sensitive to achievement incentives (see Brunstein & Heckhausen, 2008).

Expectations. We asked the subjects to predict how likely they would solve three, five, seven, and nine tasks. For each number of tasks, subjects rated their expectation on a continuous scale ranging from 0% to 100%.

Results and Discussion

Subjects solved $M = 6.29$ ($SD = 2.42$; for the first five blocks, $M = 6.14$, $SD = 2.25$; for the final five blocks, $M = 6.44$, $SD = 2.23$). Subjects began to feel satisfied with their performance when they solved $M = 5.48$ ($SD = 1.83$) tasks and indicated that they were actually $M = 4.24$ ($SD = 1.83$) satisfied with solving this number of tasks. Subjects reported they would be twice as satisfied if they would solve $M = 7.70$ ($SD = 1.83$) tasks. The expectation for solving three, five, seven, and nine tasks decreased from 81% to 65% to 44% to 21%, respectively. There were no main or interaction effects of age, gender, and processing depth for the analyses conducted.

Incentives. First, the number of tasks the subjects began to feel satisfied with was used as a dependent variable. Beside the experimental priming condition and feedback as well as the interaction of the priming and feedback, the number of solved tasks was included as a predictor (variable centered). The number of solved tasks significantly influenced the dependent variable, indicating that subjects began to feel satisfied with a higher number of tasks when they already solved a higher number of tasks ($B = .555$ [$SE = 0.146$], $t = 5.81$, $p < .01$). Priming had a positive albeit nonsignificant influence ($B = .118$ [$SE = 0.146$], $t = 0.81$, $p = .422$). Feedback and the interaction of prime and feedback was not significant ($B = .119$ [$SE = 0.146$], $t = 0.81$, $p = .421$; and $B = .069$ [$SE = 0.152$], $t = 0.456$, $p = .649$). Taking priming as the sole experimental predictor, prime explained less than 1% of the variance, indicating a very weak effect. Using Bayesian statistics to estimate the posterior probability of the null hypothesis, as suggested by Masson (2011), revealed a probability of $p = .883$ that the null hypothesis is true (assuming that the null hypothesis and alternative hypothesis are equally likely before data collection). Thus, the null hypothesis was quite likely based on the data and provided positive evidence that primes do not influence incentives.

Next, we performed analyses with the measure of how satisfied subjects would actually be if they solved the number of tasks they had indicated they would begin to feel satisfied with. We used the same predictors as before and additionally included the number of tasks subjects indicated to begin to feel satisfied with (variable centered). The level of satisfaction was negatively related to the number of tasks subjects solved ($B = -0.355$ [$SE = 0.108$], $t = -3.29$, $p < .01$). This indicates that subjects who solved more start to feel satisfied with higher numbers of tasks. However, the higher they set their number of tasks they begin to feel satisfied with, the more they are actually satisfied by this number ($B = 0.433$ [$SE = 0.112$], $t = 3.86$, $p < .01$). Priming had a positive albeit nonsignificant influence ($B = .170$ [$SE = 0.151$], $t = 1.28$, $p = .263$). Feedback and the interaction of priming and feedback was also not significant ($B = -0.052$ [$SE = 0.148$], $t = -0.35$, $p = .726$; $B = .157$ [$SE = 0.151$], $t = 1.04$, $p = .301$). Taking priming as the sole experimental predictor, it explained less than 1% of the variance, indicating a very weak effect. Bayesian statistics revealed a probability of $p = .904$ that the null hypothesis is true. Thus, the null hypothesis was quite likely and provided positive evidence that primes do not influence incentives.

Finally, we used the incentive gradient as the dependent variable with priming, feedback, and their interaction as predictors (we did not control for the number of tasks solved, as the gradient is based on a difference score). We found that the gradient is steeper (i.e., smaller) in the achievement priming condition, but the effect is weak and nonsignificant ($B = -0.054$ [$SE = .102$], $t = 0.53$, $p = .598$). Feedback and the interaction with feedback were also nonsignificant (ps $> .365$). Using priming as a single predictor, priming explained less than 1%, indicating a weak effect. Bayesian statistics revealed a probability of $p = .903$ that the null hypothesis is true. Thus, the null hypothesis was quite likely and provided positive evidence that primes do not influence incentive gradients.

Expectations. The expectancy to solve three, five, seven, and nine tasks was predicted by priming, feedback, and their interaction as well as the number of solved tasks. We found that priming effects on expectancies increased with the number of tasks and reached statistically significant levels for nine tasks. Feedback and the interaction of prime and feedback had no statistically significant effects. To test whether the priming effect increases with higher number of tasks and to avoid presenting four different analyses, we conducted an analysis in which we predicted expectations with priming, number of tasks (within subject; coded with 1, 2, 3, and 4), and the cross-level interactions. Results revealed a positive but nonsignificant main effect of priming ($B = -.183$ [$SE = 2.309$], $t = -0.79$, $p = .430$). The number of tasks had a strong effect, thus indicating that expectations decrease for higher numbers ($B = -20.160$ [$SE = 2.309$], $t = -26.24$, $p < .01$). Most importantly, there was a cross-level interaction with prime and number of tasks ($B = 1.571$ [$SE = 0.770$], $t = 2.04$, $p = .044$). Figure 2 illustrates this interaction and shows that the effect of priming increases from three to nine tasks. Comparing the between-subjects variance without and with priming revealed that priming explained 4% of the variance. Thus, we found support for our Hypothesis 2b, at least for more difficult tasks. The random-effects intercept and slope were significant, $\chi^2(91) = 549.45$, $p < .001$; $\chi^2(91) = 447.41$, $p < .001$.

In an additional analysis (parallel to Experiment 2) with the dependent variable for the expectation to solve nine tasks, we

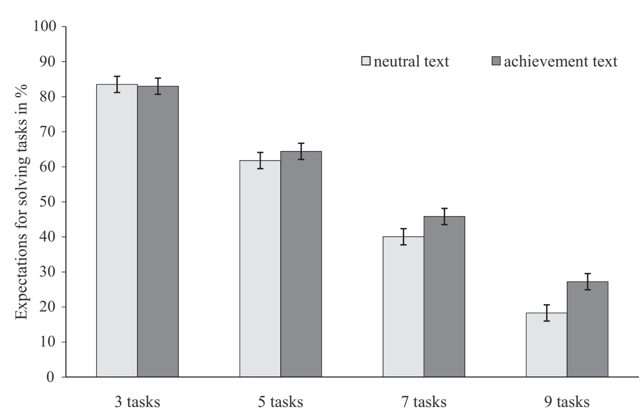

Figure 2. Expectations for solving three, five, seven, or nine tasks as a function of the number of tasks and achievement priming (Experiment 3).

modeled achievement content of the texts on Level 1 (on the level of each trial), and number of solved tasks was modeled at Level 2 (for random effects on the intercept only). We contrasted (a) neutral text versus achievement text and (b) neutral text in the neutral conditions versus neutral text in the achievement condition. We found positive effects for both contrasts, with the first contrast being significant ($B = 5.808$ [$SE = 2.612$], $t = 2.22$, $p = .026$; and $B = 7.023$ [$SE = 4.815$], $t = 1.46$, $p = .145$). This implies an immediate effect of achievement priming on expectations.

Performance. The number of tasks solved by the subject was predicted by priming, feedback, and their interaction as well as baseline performance. We found that subjects who performed fast in the baseline condition (i.e., subjects requiring less time for solving the tasks) solved even more tasks ($B = 0.108$ [$SE = 0.022$], $t = 4.85$, $p < .01$). Priming had a weak and nonsignificant influence on performance ($B = -0.161$ [$SE = 0.162$], $t = -0.99$, $p = .324$). Feedback had a significant influence, and subjects receiving feedback performed more poorly ($B = -0.311$ [$SE = 0.153$], $t = -2.03$, $p = .045$). Additionally, the interaction between priming and feedback was significant ($B = -0.328$ [$SE = 0.152$], $t = -2.16$, $p = .034$). Inspection of the data showed that the negative effect of feedback was exclusively found in the achievement priming condition. In the no-feedback condition, priming led to higher performance than in the feedback condition, but the difference was not significant.

In line with our expectation (Hypothesis 2b), we found that achievement primes increased the expectation for solving (high numbers of) tasks. Incentives were not reliably predicted by achievement primes, and Bayesian statistics provided positive evidence for the null hypothesis of no priming effects, which contradicts Hypothesis 1b but is in line with the finding that priming did not affect the implicit achievement motivation in the previous experiments. That expectations only increased for high numbers of tasks was not specifically predicted by our model. We suspect that for the low number of tasks, the expectation was already very high and priming could not increase this further (i.e., ceiling effect). However, priming may specifically increase expectations for very challenging tasks. Stajkovic, Locke, and Blair's (2006) results give credit for this conclusion, as priming effects were not present for easy goals, but for "do your best" and for very difficult goals.

We found no main effect of priming on performance in our final experiment. At first glance, this may question the results found for the priming effects on motivational variables and that they play a crucial role in the prime-to-behavior effect. However, higher motivation does not necessarily lead to higher performance for all tasks and contexts (Brunstein & Hoyer, 2002; Humphrey & Revelle, 1984; VandeWalle, Cron, & Slocum, 2001). We found a negative effect of feedback on performance. In general, feedback increases motivation and performance (see Kluger & DeNisi, 1996). As we found a negative effect of feedback, supposing higher motivation indeed did not lead to higher performance in the task format we used. Subjects had to solve tasks within a given time limit, which is very challenging, and increased motivation may have pushed motivation beyond an optimal level (Anderson, 1990). On a descriptive level, performance was even worse in the feedback and achievement priming condition, which may have pushed motivation away from the optimal level even further. Only in the no-feedback condition did achievement priming go along with higher performance (on a descriptive level).

General Discussion

We found no support that achievement primes arouse implicit achievement motivation, leading to a higher incentive for performing well. In three pilot studies and two main experiments, we were unable to find a reliable and consistent effect of achievement primes in running text on implicit achievement motivation and no priming effect on incentives in the final experiment. Therefore, we were unable to find support for our Hypotheses 1a and 1b that implicit achievement motivation and incentives are affected by achievement primes. Moreover, we found no empirical support that baseline implicit achievement motivation is a moderator of the achievement priming effect (Hypothesis 3a). However, we did find support that explicit achievement motivation and outcome expectancy were higher when subjects read achievement primes in running text (Hypotheses 2a and 2b). Achievement priming affected both components of explicit achievement motivation. Achievement primes increased HS, indicating that subjects see themselves as more confident and prone to achievement situations and their challenges, and decreased FF, indicating that subjects have less fear of failing in a challenging situation.

Taken together, we see substantial evidence that the achievement primes change explicit rather than implicit motivation. The priming effect is limited to explicit pathways, although priming effects do bypass conscious recognition of the effect for the subjects. Taken that explicit achievement motivation has a cognitive representation based on language (D. C. McClelland et al., 1989; Schultheiss, 2008), the semantic priming effects most likely affected these kinds of cognitive representations. Semantic meaning of the primes does not translate into a more affective-experiential format as we expected it would. We found some evidence that semantic primes even inhibit implicit achievement motivation. In line with Schultheiss (2008), language-based stimuli do not affect implicit achievement motivation, at least if not stimulated with additional experimental manipulations that translate it into an affective-experiential format (Baumann & Scheffer, 2010; Kazén & Kuhl, 2005; Schultheiss & Brunstein, 1999). Shantz and Latham (2011) found achievement priming effects on implicit achievement motivation with pictorial primes. Pictorial primes represent a more experiential format, implying that achievement priming is associated with the implicit pathway depending on the prime stimuli used.

In our pilot studies and in Experiment 1, we focused on one process variable, implicit achievement motivation. In Experiment 2, we assessed implicit achievement motivation along with explicit achievement motivation. Besides the objectives for such an approach, the drawback is that the priming effect reported in the literature occurs within an achievement task. However, writing stories or answering questions is not necessarily regarded as an achievement task. It could be argued that implicit achievement motivation increases only if words are presented *within* an achievement context. Nevertheless, the evidence does not speak in favor of implicit achievement motivation being affected by achievement primes.

First, implicit achievement motivation should be heightened by achievement primes alone, and this heightened motivation should then be relevant in achievement contexts. It is like the smell of good food will make us hungry regardless of the opportunity to eat. Second, explicit achievement motivation increased in the presence of achievement primes. We see no reason why explicit achievement motivation should be heightened by achievement primes alone (i.e., and not exclusively by achievement primes in an achievement context), whereas implicit achievement motivation would exclusively be heightened if words *and* context are achievement related. Third, the studies by Custers et al. (2009) showed that the expectation of success is heightened by achievement primes, but the incentive for achievement outcomes is not. This is in line with research showing that explicit achievement motivation is associated with higher outcome expectancy or self-efficacy (cf. Brunstein & Heckhausen, 2008), and the outcome expectancies have been affected by achievement primes in our final experiment. Implicit achievement motivation, however—as outlined in the introduction—should lead to higher incentives for doing well. As Custers et al. (2009) did not find an increase in incentives due to achievement primes; a mediation of implicit achievement motivation is unlikely for this reason, too. Fourth, Shantz and Latham (2011) found an effect of pictorial achievement primes on implicit motivation in a design conceptually similar to the one we used here.

One could further argue that implicit measures may not be sufficiently reliable to test our expectations. Although reliability is not as low as sometimes presented (cf. Lang et al., 2012), it is indeed a problem (Schultheiss et al., 2008). However, also in the case of low reliability, the overall results do not provide support for the expected effects on implicit achievement motivation. We expected a strong effect that should still be found in the case of low reliability. At least in Experiments 1 and 2, we had fairly high numbers of subjects per condition, which allowed small and unreliable effects to be detected. Additionally, implicit measures may not be as reliable as explicit ones because they are highly sensitive to context factors. Context sensitivity is actually one reason that lowers reliability when measuring individual differences, but favors the detection of priming effects on implicit achievement motivation. Finally, we found a negative albeit nonsignificant effect in Experiments 1 and 2. Looking at the results from a posteriori perspective, this makes an even negative effect of priming on implicit achievement motivation more likely than a positive one.

We measured explicit achievement motivation with items of a trait measure of personality (cf. Engeser & Langens, 2010; Schönbrodt & Gerstenberg, 2012). We obviously did not change personality, but tapped on the state component of the questionnaire. Primes may facilitate retrieval processes of self-relevant episodes of achievement situations, leading to higher scores when subjects answer the items after achievement priming. Less subtle situational or context factors may even have a stronger influence. Even long-term changes in personality measures due to major changes in life (e.g., Neyer & Lehnart, 2007) may be partly explained by a facilitated retrieval process of recent experiences instead of completely representing a personality change. Varying situations would provide an estimate of how important the state component of the measure is and whether subjects on all levels of traits respond in the same or in different ways to situational changes (Schmitt et al., 2013). Taken this way, the sensitivity of measures to context factors could be used to validate the measure (Borsboom, Mellenbergh, & van Heerden, 2004). If we specify the factors that theoretically influence the measure (e.g., Lang et al., 2012; Tuerlinckx, De Boeck, & Lens, 2002; Steyer, Schmitt, & Eid, 1999) and empirically validate this, we could be reassured that we are measuring what we want to. Thus, the broader implication is that what is now error variance in personality measures could be used to understand the process of measuring personality in more depth as well as how personality may translate into behavior (cf. Baumert & Schmitt, 2012).

A challenge of semantic priming studies is that connotations of semantic primes are generally not one-dimensional. To address this challenge, it is helpful to understand the processes of semantic priming effects in more detail. This allows the ambiguity to be minimized and enables the formulation of more specific hypotheses. According to our data, achievement primes do increase explicit achievement motivation and outcome expectancies (cf. Custers et al., 2009). Taking this result into consideration, we could be more specific regarding which aspects of the semantic primes do foster motivation and should foster subsequent performance. Widely used primes are, for example, *win*, *achieve*, and *master* (e.g., Bargh et al., 2001; Crusius & Mussweiler, 2012; Engeser, 2009; Hart & Albarracin, 2009; Oikawa, 2004), and these words imply being successful instead of “proud,” “satisfied,” or “happy,” which would highlight the incentives of achievement. The primes used in our experiments also highlight being successful, and thus using primes with an incentive connotation might alter the process of the prime-to-behavior effect. To further validate the explicit pathway of semantic priming, measures of explicit measures should include items that assess the chronic concern for achievement incentives as well.

We do know that affect influences outcome expectancies (e.g., Gendolla & Krüsken, 2002), and most of the achievement primes used in the literature have positive connotations. We should rule out affective valence as an alternative explanation (e.g., Kazén & Kuhl, 2005) in order to be able to attribute the effect to the achievement content. Priming research could learn from affective priming studies, which are more sophisticated in this respect (cf. Gable & Harmon-Jones, 2010). In light of our discussion regarding connotation and context sensitivity, we are skeptical about “direct” prime-to-behavior effects, as have been proposed in the perceptual–behavioral link by Dijksterhuis and Bargh (2001). The achievement primes are quite abstract, which should not allow an immediate imagination of an action. However, some aspects of the words may nevertheless have the potential to affect action quite directly, such as simply activating behavior, which in itself could account for higher performance (Albarracin et al., 2008; Gendolla & Silvestrini, 2010). The position that semantic primes activate goals should be more precise regarding the nature of the goals and whether all people share the same goals.

We already discussed the fact that we found no main effect of priming on performance in our final experiment, reasoning that priming may have pushed motivation beyond the optimal level. We focused on motivational variables in our experiments and found that achievement primes change motivation. As higher motivation does not always lead to better performance, we still see that changes in motivation are the process variables of the prime-to-behavior effect. In experiments with mostly identical priming material and procedures, Engeser, Baumann, and Baum (2014) found achievement priming effects on performance. In one experiment, the achievement task was the same as the one used here, but without setting a time limit per single task (in a second experiment, achievement texts fostered performance for solving anagrams, again without a time limit per task). Thus, mostly identical priming materials and procedures led to prime-to-behavior effects in another presentation of tasks. Therefore, we are quite confident that the process variables studied here do reflect the process underlying the prime-to-behavior effects. Furthermore, we would like to point out that, apart from a mediation of the prime-to-behavior effect through motivation, studying the priming effects on motivation itself is justified regardless of whether motivation will lead to the respected behavioral changes.

Finally, semantic behavior priming effects in domains other than achievement started to be called into question recently (Abbott, 2013; Yong, 2012). This is primarily due to failed replications of classical behavioral priming studies (e.g., Doyen, Klein, Pichon, & Cleeremans, 2012; Shanks et al., 2013). Shanks et al. argued that traditional priming research on lexical or semantic processing had assumed a narrow context-specific priming effect that does not generalize very broadly. In the light of our results, we would encourage focusing on the proposed process variables of the priming effect in order to understand both the specific, probably narrow effects and under what individual and situational aspects those effects lead to behavior changes.

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