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Affective Forecasting and Need for Achievement: The Intense Emotion of Mastery Needs and the Features of Focalism

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Preface

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Abstract

When predicting future emotions, i.e. engaging in *affective forecasting*, people tend to overestimate the intensity and duration of emotions, termed the *impact bias*. We investigated the intensity component of affective forecasts, exploring how a student sample (N = 108) predicted the intensity of their own future (un)happiness following an excellent and a poor exam grade. First, we addressed whether impact bias might serve a motivational purpose, by exploring whether individual differences in achievement motivation predicts forecasted intensity. Moreover, we tested the effect of an attentional *focusing* manipulation for a poor or good exam grade, and whether achievement motivation moderated the focusing effect. Individual differences in mastery needs were related to the intensity of forecasted emotions. Performance needs were unrelated to forecasts. The focusing manipulation caused a slight reduction in forecasted intensity for a poor grade, but did not influence forecasts for an excellent grade. No moderation effect was found.

Keywords:

affective forecasting, need for achievement, focalism, achievement goal, impact bias

Sammendrag

Når man forutser fremtidige følelser, eller lager "affective forecasts", har man en tendens til å overestimere følelsens intensitet og varighet, kjent som "impact bias". Vi undersøkte intensitetskomponenten av "affective forecasts" ved å utforske hvordan et studentutvalg (N=108) foutså intensiteten i egen fremtidig (u)lykke etter en svært god eller svært dårlig eksamenskarakter. Først adresserte vi hvorvidt "impact bias" kan ha en motivasjonell funksjon ved å utforske hvorvidt individuelle forskjeller i "achievement"-motivasjon predikerer forutsett intensitet. Videre testet vi effekten av en oppmerksomhetsfokuserende manipulasjon ved en god eller dårlig karakter, og hvorvidt "achievement"-motivasjon modererte effekten av fokuseringen. Individuelle forskjeller i mestringsbehov var relatert til intensiteten i forutsette følelser. Prestasjonsbehov var urelatert til "forecasts". Fokuseringsmanipulasjonen forårsaket en liten reduksjon i forutsett intensitet ved en dårlig karakter, men hadde ingen effekt ved en god karakter. Ingen moderasjonseffekt ble funnet.

Nøkkelord:

affective forecasting, need for achievement, focalism, achievement goal, impact bias

Affective Forecasting and Need for Achievement: The Intense Emotion of Mastery

Needs and the Features of Focalism

Humans frequently make decisions based on the emotional consequences we believe our actions will have. Both trivial and more important choices are informed by these expectations, from deciding how hard to study for an exam, to whom to marry. Gilbert and Wilson (e.g. 2009) developed the theory of *affective forecasting* which attempts to explain how people make these predictions. According to Gilbert and Wilson (2009), people first create a simulation of the event, a *preview*. They then experience an emotional reaction to this preview, a *premotion* (from "pre-emotion"). Predictions of future emotions, *affective forecasts,* are based on this premotion.

Affective forecasts are not always as accurate as people think they are. Research has shown that most people show an *impact bias*: they tend to predict that their emotional reactions will be more intense and longer lasting than they actually are (Gilbert, Driver-Linn, & Wilson, 2002, p. 116).

The present study examined whether individual differences in *motivational orientation* could make some people more prone to impact bias, and we discuss the possible adaptive function of the bias. In addition, we explored how *event consistent* information, *working memory depletion*, and c*onstrual level* could increase or decrease the bias. The study was developed based on the theoretical framework by Gilbert and Wilson (2009). Methodology was inspired by previous studies that have examined individual differences in affective forecasting (e.g. Hoerger, Quirk, Lucas, & Carr, 2010), and studies that have successfully reduced impact bias through experimental manipulation (e.g. Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000). The impact bias has previously been reduced using specific interventions, and we used a manipulation that could potentially differentiate between the effective and redundant components of these interventions. The paper begins by

introducing existing research in the affective forecasting field. We start by describing the mechanisms theorized to be involved in affective forecasting, and go on to describe the link between motivation and affective forecasting.

Affective Forecasting

According to Wilson and Gilbert (2003), people make predictions of emotional valence and specific emotion, in addition to emotional duration and intensity. People are fairly accurate when predicting valence and specific emotion in simple scenarios, and the impact bias only describes the failure to accurately predict intensity and duration (Wilson & Gilbert, 2003). The bias has been demonstrated for a range of both positive and negative events, from failing an exam to winning the lottery (Buehler & McFarland, 2001; Wilson & Gilbert 2003). In this field, making accurate affective forecasts is presumed to be dependent on having the cognitive ability to accurately create previews of the future. In turn, forecasting requires the emotional awareness to accurately recognize and make predictions based on premotions (Gilbert & Wilson, 2009). People have been shown to have sufficient cognitive and emotional capacity to predict whether they will feel good or bad, and whether the "good" will be happy, or maybe proud or amused. The intensity of the happy feeling and how long the feeling will last is more subject to error.

Wilson, Meyers, and Gilbert (2001) suggest that three cognitive tasks are involved when creating previews, and that failure at any one of these may lead to impact bias. First, past experiences must be *compared* to the prospective experience. How alike is the experience of mastering a new skill to the experience of getting a good grade on an exam? Second, people must evaluate which of their previous experiences are *relevant*. When forecasting reactions to getting an A on a physiology exam, are the emotions experienced when winning a game of luck relevant? Third, when completing the two first tasks, people must also be able to *accurately recall* how they have previously felt. Was it happiness, or

maybe pride? If it was happiness; how much happiness exactly? Several sources of error have been found related to these tasks. Memories can be colored by current attitudes, beliefs, and personal goals, and people remember only parts of their emotional experiences. What they do remember is often remembered in abstract terms (Aaker, Drolet, & Griffin, 2008). Additionally, as the perceived *psychological distance* to an event increases, people tend to think in more abstract and decontextualized terms (Trope, Liberman, & Wakslak, 2007). Arguably, if important details of an event or the context are ignored, this may mean neither past nor future events are accurately represented, causing forecasting errors. Based on these and similar findings, Gilbert, Pinel, Wilson, Blumberg and Wheatley (1998) proposed six potential reasons for biased forecasts. In the present study we chose to focus on two of these, *motivated distortions* and *focalism*. Bias may be motivated by a desire to experience the immediate premotion. A biased forecast may also be a way to motivate oneself in order to accomplish or avoid the focal event. Focalism in turn can be summarized as the tendency for people to focus exclusively on the target event. They then fail to correct for the impact other events will have on their emotions.

Specifically, we examined how motivational achievement goals relate to forecasts about achievement events, and how forecasts could be affected by an attempt to focus attention toward specific aspects of the future. Despite previous interest in a connection between motivational goals and affective forecasts (Hoover, 2012; Sheldon, Gunz, Nichols, & Ferguson, 2010), specific achievement goals have not been the focus of affective forecasting studies prior to the current investigation. Knowledge about affective forecasting and the impact bias could in principle be practically applied, for example in clinical work with patients who pathologically under- or overestimate the intensity and/or duration of their future emotions. However, before knowledge about the impact bias and bias reduction can be practically applied, we need to know more about the situations in which biased forecasting

may be adaptive, as well as situations where it could be harmful. It may also be more prevalent, and/or more adaptive for particular individuals. In addition, if one aims to reduce or increase the bias, we need to know more about not just which interventions work, but how they work. The present study was an attempt to start answering these questions.

Focal emotion. To start, we had to choose which specific emotions to measure. One of the most fundamental distinctions in models of emotion has traditionally been whether an emotion is valenced as positive or negative (Watson & Clark, 1994). The *intensity* of the affective experience and the *frequency* of positive versus negative affect appear to be what determines if an experience is "happy" or "unhappy" (Diener, Larsen, Levine, & Emmons, 1985). Affective forecasting research has also focused on the intensity and duration of anticipated (un)happiness in relation to future events (e.g. Ayton, Pott, & Elwakili, 2007; Morewedge & Buechel, 2013; Wilson et al., 2000). Specific emotions such as regret (Sevdalis & Harvey, 2009) and vengefulness (Carlsmith, Wilson, & Gilbert, 2008) have also been studied in affective forecasting research, and specific emotions such as surprise, pride, self-efficacy, shame, fear, and embarrassment may arguably be important in achievement situations. However, we chose happiness and unhappiness, as we theorized that specific emotional states would be encompassed by this more fundamental distinction. Currently, "(un)happiness" is most often conceptualized as an enduring experience of positive or negative affect (Quoi Bach, Mikolajczak, & Gross, 2015). In the present study we focus more on immediate emotional *reactions,* rather than happiness as an enduring state. We also chose to focus on the intensity dimension, as this has been found to be an apt descriptor how "good" or "bad" an emotional experience is judged to be (Diener et al., 1985). High intensity has been associated with specific emotions such as exuberance or depression, while low intensity is more associated with contentment or mild unhappiness (Diener et al., 1985). We wanted to determine whether people with different motivational goals have a tendency to

predict either low or high intensity (un)happiness when imagining achieving a future goal, or experiencing a failure. We also investigated whether the intensity of forecasted happiness could be manipulated experimentally.

Motivation

One of the main arguments for studying affective forecasting is the role forecasts appear to play in motivation (Buehler, McFarland, Spyropoulos, & Lam, 2007). According to Gilbert et al. (1998), affective forecasts are the "[...]guiding star by which people chart their life courses[...]" (p.617). We defined motivation as an experienced general sense of *drive*, being caused by a prospective *end state*. Attribution theories of motivation claim that what motivates people is not the actual end state, but rather their interpretation of the end state (Eccles & Wigfield, 2002). We proposed that this interpretation can be described as an affective forecast, consisting of a specific previewed result and the premotions associated with it, which in turn may lead to a sense of motivation. A person can be motivated to attain a desirable event or avoid an undesirable event, and an event can be neutral, causing no motivation.

In our study, the previewed result was an exam grade. The actual underlying goal may be more general, such as "doing well in school", or "being smart". The level of happiness experienced in response to the preview then leads to a forecast of future happiness (Gilbert & Wilson, 2009), possibly causing a motivated drive to attain or avoid the grade. That is, how you expect to feel about reaching a future goal can determine how much effort you are willing to exert to make it happen.

Motivational consequences of biased forecasts. Two views regarding the consequences of the impact bias are apparent in the literature. Hoover (2012) and Hoerger et al. (2010) have argued that bias should be reduced. They claim that biased forecasts can cause unnecessary anxiety, disappointment, and excessive effort being expended in the

pursuit of fruitless goals. However, most biases and heuristics studied in psychology are extreme cases of generally useful decision making strategies (Gigerenzer, 2008), and this could also be the case for the impact bias. Wilson and Gilbert (2005) have suggested that bias could cause increased experienced motivation. They argued that people will work harder to attain or avoid an outcome when they overestimate the longevity and intensity of the emotional consequences this will have. Subjective well-being has been shown to be largely stable over time, and even uncommon events do not substantially affect long-term happiness (Gilbert et al., 1998). Biased forecasts may thus be more motivating than accurate forecasts that tell you that your happiness will stay pretty equal either way (Gilbert et al., 1998).

Morewedge and Buechel (2013) explored the motivational and behavioral benefits of the impact bias. They found that people made more extreme forecasts after committing to a future event, compared to when they were still deciding. The authors hypothesized that when still deciding, people try to perform impartial cost-benefit analyses. Once decided, they move into an implemental mindset where they focus on achieving the end state. Morewedge and Buechel (2013) also found that people's forecasts are more biased when they believe they can influence an event. In other words, if you don't believe your actions can have an effect, there is no reason to produce more intense and therefore more motivating forecasts. Finally, when the intensity of forecasts was increased experimentally, more intense forecasts resulted in participants working harder, demonstrating that forecasts influence not just experienced motivation, but motivated behavior.

Need for achievement. Many studies on motivation have concerned individual differences. A portion of these have concerned individual differences in *need for achievement* (Eccles & Wigfield, 2002). Need for achievement has been defined as the drive to overcome obstacles and "do something difficult as well and as quickly as possible" (Murray, 1955, p. 64). According to Elliot and McGregor (2001), need for achievement can be divided into two

subcategories, which in this paper will be referred to as *performance* and *mastery* needs. The categories differ in how an achievement result is evaluated. Performance refers to evaluating one's achievements relative to others, also referred to as *competitiveness*, or *ego-involved goals*. Mastery refers to evaluating results relative to absolute or intrapersonal standards. Standards are informed by the requirements of the task, as well as previous achievements, or the highest achievement possible. Mastery can also be referred to as *work-mastery*, *learning,* or *task-involved goals*. (Eccles & Wigfield, 2002; Elliot & McGregor, 2001; Spence & Helmreich, 1983). Elliot and McGregor (2001) conceptualized mastery and performance needs as being valenced as either *approach*, with a focus on winning or mastering, or *avoidance*, with a focus on loss or non-mastery. Approachers have been shown to be more oriented toward positive events, and avoiders toward negative events (Elliot, 2006). Mastery needs have previously been associated with several types of achievements, including academic success. Performance needs, in particular performance avoidance, have been found to be uncorrelated and even negatively correlated with achievement results (Helmreich, Beane, Lucker, & Spence, 1978; Hulleman, Schrager, Bodmann, & Harackiewicz, 2010; Linnenbrink & Pintrich, 2002; Payne, Youngcourt, & Beaubien, 2007).

The need for achievement hypothesis. Several studies have previously examined the various associations between achievement goals, affective forecasts, effort, and success. Morewedge and Buechel (2013) found that a global measure of need for achievement positively correlated with bias in forecasted happiness for winning a game of skill. They also found that participants with more extreme forecasts spent more time working toward an achievement goal (Morewedge & Buechel, 2013). Another study found that participants with more biased forecasts demonstrated greater success at several laboratory tasks (Hoover, 2012). It seems likely that extreme forecasts lead to effortful work, which in turn results in higher academic achievement.

However, need for achievement is not a unidimensional construct, and the effect of forecasts on effort might work differently for different achievement goals. Actual emotional reactions to achieving success through hard work appears to be mediated by achievement orientation. Mastery oriented children report that expending effort leads to more satisfaction, and that low-effort successes lead to boredom and unhappiness. Conversely, children high in performance needs have been shown to associate effort with low ability, and mere effort, even if it leads to success, can cause unhappiness (Dweck & Leggett, 1988). These interpretations of effort appear to have an effect on future achievement behavior. Fisher and Ford (1998) found that mastery oriented participants spent more time working towards a goal, and used more work-intensive strategies than performance oriented participants. In addition, people who are focused on performance have been shown to primarily attempt tasks they know they can successfully complete, while mastery is associated with choosing more challenging tasks (Eccles & Wigfield, 2002). When including impact bias, the association between goals and effort becomes more complicated. Hoover (2012) found that participants who highly valued mastering laboratory tasks had less biased forecasts. She suggested bias may be more prominent for real-world events that presumably are more important to participants, and suggested measuring need for achievement and exam grades in affective forecasting studies (Hoover 2012).

The present study did just that, and examined how specific achievement goals were associated with students forecasted happiness if they got an F or an A on a future exam. If impact bias is not adaptive, there would be no reason for an association between specific goals and forecasts. As mainly mastery needs have been associated with achievement, we predicted that mastery needs alone would be associated with affective forecasts. This would support the theory that affective forecasts form the bridge between achievement needs and academic performance. We also anticipated that approach motives in general would be more

strongly associated with forecasts in the positive scenario (A), and avoid motives in the negative scenario (F).

Focalism

Gilbert et al. (1998) suggested that focalism, an excessively narrow attentional focus when making forecasts, can cause impact bias. They claimed that by focusing on a future event in isolation, people ignore other aspects of their lives that also influence their emotional state. The effect of focalism on people's forecasts has been experimentally demonstrated by Wilson et al. (2000), who manipulated participants forecasts though a *defocusing* manipulation: Before making forecasts, participants in the experimental condition were asked to rate how much time they would spend on a variety of daily activities, and also made a list of self-generated activities. When participants subsequently rated their experienced happiness after the focal event took place, the experimental condition had less biased forecasts.

Several researchers have proposed explanations for how defocusing manipulations reduce bias. Wilson et al. (2000) proposed the *distraction hypothesis*, which states that defocusing works by making people remember that other events also influence their emotional states. People should therefore report spending less time thinking about the focal event after a defocusing intervention. However, Sevdalis and Harvey (2009) found that participants rated emotions as less intense after both writing a diary and solving anagrams, and proposed the *interference hypothesis*. This states that working memory is required for the process that makes biased forecasts, and both anagrams and diaries reduce bias by depleting working memory. Ayton et al. (2007) proposed a third explanation, referred to as the *construal-level hypothesis*, stating that defocusing causes people to move from thinking in high-level construals to low-level construals, i.e. using a less abstract and more contextualized mindset, which causes bias reduction.

The focalism hypothesis. The distraction, interference and construal level hypotheses thus nominate three different components necessary for defocusing. It remains unclear whether the components interact to cause a bias reduction, or if a sole component is responsible for the defocusing effect. We designed one manipulation that enabled us to test all three hypotheses, described more closely in the method section. If the manipulation increased the intensity of forecasts, this would support the distraction hypothesis, as the content of the manipulation was related to the focal event (exam grades). If the manipulation had no effect on forecasts, this would support the interference hypothesis, as the manipulation should minimally tax working memory. If the manipulation decreased intensity, this would support the construal-level hypothesis, as the manipulation used low-level construal language.

The Moderation Hypothesis

We also investigated whether need for achievement affected how sensitive an individual was to the manipulation. A connection between *event-importance* and impact bias has previously been established, where forecasts were more biased for events that were considered more important (Hoerger et al., 2010). We reasoned that achievement goals would be a good proxy for individual differences in event importance, as events related to personal goals (i.e. exam grades and achievement goals) could arguably be more important to an individual than events not related to personal goals. Studies have also shown that there is a positive correlation between event importance and performance on memory tasks, but that this correlation is mediated by attention (Kliegel, Martin, McDaniel, & Einstein, 2001). If attention is not allocated to a task, the importance of the task has no effect on performance. Thus we hypothesized that the forecasts of participants generally high in need for achievement would be more sensitive to manipulations of attention, under the assumption

that achievement goals influence the degree of event importance. We therefore expected a moderation effect if the manipulation successfully affected attentional focus.

Summary: Research Questions and Hypotheses

The present study was designed to answer three different research questions (graphically represented in Figure 1).
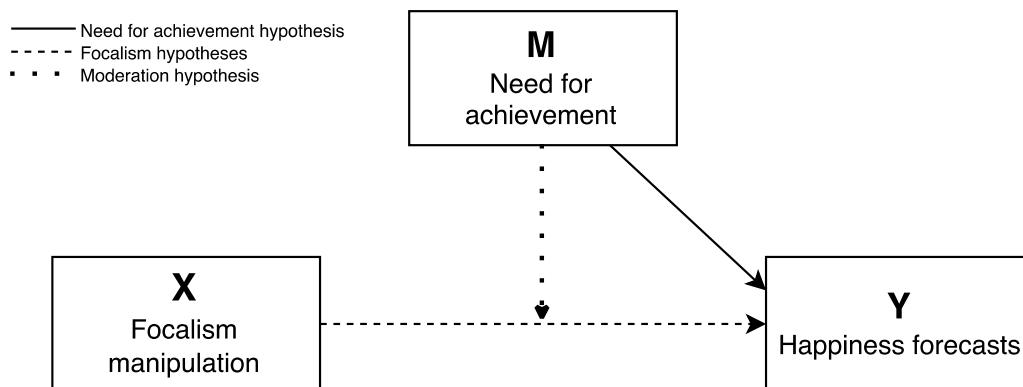
Figure 1. Conceptual model of the three hypotheses examined in the present study.

First, how do affective forecasts relate to mastery and performance aspects of need for achievement? We hypothesized that mastery needs, but not performance needs, would predict the intensity of the happiness/unhappiness participants believe they would feel if they got an F/A on a future exam. This will be referred to as the *need for achievement hypothesis*. Second, can we increase the impact bias, and what does experimentally manipulating forecasts tell us about how focalism functions? The *focalism hypothesis* had three separate stipulations, depending on how the manipulation affected forecasts. Third, does higher levels of need for achievement make people more susceptible to manipulations of forecasts? We predicted that need for achievement would moderate the effect of the manipulation, particularly if the manipulation affects attentional focus, henceforth referred to as the *moderation hypothesis*.

We designed a questionnaire-based experiment with a between-subjects design and two conditions to test the hypotheses. Our sample consisted of students, who were asked to forecast their happiness in response to given exam grades. All participants self-reported their need for achievement, and participants were randomly assigned to either a focalism condition or a control condition.

Methods

Participants

108 participants (76 women, M_{age} = 21.45, SD_{age} = 2.33) were randomly assigned either the *focalism* condition, or the control condition. We used convenience sampling of a specific target population; medical, dentistry, or law students at the University of Bergen. Participants were recruited immediately after a joint lecture for medical and dentistry students[1], and three different lectures for law students[2]. Law students constituted a majority of the sample (71.3 %), followed by medical (23.1 %) and dentistry students (5.6 %). Conditions were matched with respects to gender, age, and course. Women were overrepresented in our sample, but are also overrepresented in the target population (Database for statistikk om høgre utdanning, 2016a, 2016b). Participants were offered a gift card with a value of 33 NOK as compensation for their time.

Procedure

The study was described in the questionnaire as "part of a study of personality, attitudes, and decisions". The text informed participants they could withdraw at any time, and participants gave a written indication of consent. The questionnaire was filled out in the auditorium and took approximately 15 minutes. Participants were instructed not to communicate. Hypotheses were not disclosed in order to avoid bias as a result of demand

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[1] MEDOD1: "Første semester medisin- og odontologistudiet"
[2] JUS111: "Forvaltningsrett I", JUS112: "Arve- og familierett", og JUS123 "Kontraktsrett II".

characteristics (Orne, 2009). A verbal debrief was planned upon completion of the

questionnaire, but proved problematic as participants finished at different times and left the

auditorium. However, experimenters were available if participants had questions or concerns.

No identifying information was gathered, and the risk of harm was judged to be minimal, and

no ethical clearances were deemed necessary.

For both conditions, the questionnaire started with demographic questions, followed

by measures of need for achievement: *Work and Family Orientation Questionnaire*, and

Achievement Goal Questionnaire (Elliot & McGregor, 2001; Spence & Helmreich, 1983). As

both need for achievement measures focus on performance we ran the risk that they would

affect forecasts (Schwarz, 1999). To reduce the effect of these measures we added a

Norwegian translation of the Positive Affect and Negative Affect Scale (PANAS; Jones,

2011) as a filler task before the actual manipulation. The intention was to focus participants'

attention on their present mood, rather than possible future achievements, and to decrease the

transparency of the study to avoid hypothesis guessing.

All participants were then instructed to "picture the following scenario as vividly as

possible", and subsequently asked to imagine that the exam for the class they had just

attended had "gone so poorly that you received an F" (the *F-scenario*). In the focalism

condition this was followed by a short, emotionally neutral text designed to enhance the focus

on academic achievement, which included a reference to two lectures, reading in a study hall,

discussing the grade with classmates, and a study group for a different exam (See Appendix

E for complete focalism questionnaire). We used low-level construal language to make the

manipulation comparable with previous defocusing manipulations (e.g. Wilson et al., 2000).

The manipulation was administered passively, i.e. through a text to be read, which should tax

working memory less than actively generating content. Participants in the control condition

were simply asked to imagine receiving the grade, and received no accompanying text.

Participants in both scenarios then forecasted the intensity of their happiness should they receive an A (the *A-scenario*). Due to an error the focusing text was not included in the A-scenario. As the focusing text in the F-scenario was written on the page immediately preceding the A-scenario, we nevertheless expected an effect, though it may be smaller than in the F-scenario.

Actual emotional reactions were not assessed, so we cannot determine whether the forecasts were in fact biased. However, the intensity of forecasts functioned as a proxy for the impact bias, under the assumption that more intense forecasts are more biased as well.

Measures[3]

Need for achievement. The need for achievement measures were translated from English to Norwegian, see Appendix A for more information about translation and validity.

Work and Family Orientation Questionnaire. The Work and Family Orientation Questionnaire (WOFO) is a 19-item questionnaire designed to measure general achievement motives. The WOFO can been divided into a *work-mastery* component (14 items), and a five item *competition* component (Elliot & McGregor, 2001; Gregor & O'Brien, 2015; Spence & Helmreich, 1983). The present study used a seven-point scale with the anchors "Not at all true of me" and "Very true of me". Two items were deleted from the work-mastery subscale post hoc due to low item-total correlation (.027 and .172), leaving one 12 item work-mastery scale with $\alpha = .762$ (e.g. "I like hard work"), and the five item competitiveness scale (e.g. "I feel that winning is important in both work and games" [Spence & Helmreich, 1983, p. 42], scale $\alpha = .884$).

Achievement Goal Questionnaire. The Achievement Goal Questionnaire (AGQ) is a 12-item measure designed to measure the need to achieve based on a 2 x 2 Achievement Goal

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[3] A measure of expected effort leading up to the exam was excluded from analysis as participants had varying time left until their exam, and as such their responses were not comparable.

Framework (Elliot & McGregor, 2001). We used a seven-point Likert scale with the anchors "Not at all true of me" and "Very true of me". The AGQ has four 3-item factors; *performance approach* (e.g. "It is important for me to do better than other students", scale α=.893), *mastery avoidance* (e.g. "I worry that I may not learn all I possibly could in this class", scale α=.899), *mastery approach* (e.g. "It is important for me to understand the content of this course as thoroughly as possible", scale α=.753), and *performance avoidance* (e.g. "My goal in this class is to avoid performing poorly", scale α=.830; Elliot & McGregor, 2001, p. 504). The AGQ is targeted to a specific course, and participants were instructed to consider the lecture they had just attended while completing the measure. Targeted measures have previously been used when studying task motivation and affective forecasting (Hoover, 2012), and may be a better predictor of forecasts than the more general WOFO.

Happiness forecasts. In both scenarios participants were asked to predict the *intensity*[4] of their happiness on the day they received their grade (the dependent variable) on a scale of 0 ("Very unhappy") to 10 ("Very happy"). Assessing anticipated happiness in this manner is the norm in the field (e.g. Ayton et al., 2007; Gilbert et al., 1998; Hoerger et al., 2010; Levine, Lench, Kaplan, & Safer, 2013; Morewedge & Buechel, 2013; Wilson et al., 2000), and this measure was shown by Gilbert et al. (1998) to have good concurrent validity when compared to two multi-item measures of life satisfaction.

Manipulation check. Whether the manipulation caused an increase in attentional focus was examined by asking participants to indicate "how much time will you spend thinking about your grade that day?" on a scale of 0 ("Very little") to 10 ("Very much"). This is a typical manipulation check in articles based on the distraction paradigm (e.g. Wilson et al., 2000), where the manipulation is considered successful if participants in the *defocusing*

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[4] Forecasted duration of affect was measured, but not analyzed in the present study.

condition score lower than controls. For a successful manipulation of attention in the current study, participants in the focalism condition should score higher than control on this measure.

Statistical Analyses

Analyses were conducted with IBM SPSS version 23, and conducted separately for the F-scenario and the A-scenario. As samples must be approximately normally distributed in order to use many commonly used statistical tests (e.g. t-test, ANOVA, and Pearson's correlation), the Shapiro-Wilk-test was used to assess the distribution of the major study variables, and significantly nonnormal variables investigated (Blanca, Arnau, López-Montiel, Bono, & Bendayan, 2013). Cutoff for acceptable normality was set at skewness and kurtosis greater than ±1.96 when divided by their standard error (Rose, Spinks, & Canhoto, 2014). Significantly nonnormal distributions for several variables were uncovered (see Appendix C for table of descriptives and normality tests). We therefore chose OLS regression to test all our hypotheses, where only the normal distribution of residuals is necessary. Regression is also more sensitive than many nonparametric tests (Cohen, Cohen, West, & Aiken, 2013). To test the moderation hypothesis, we performed moderated OLS regressions using Hayes' (2013) PROCESS macro. Demographic variables (gender, age, course) were controlled for in all regressions. The effect of the manipulation on attention was assessed with a Mann-Whitney U-test, a nonparametric alternative to the t-test. See Appendix C for evaluations of statistical assumptions[5].

Outliers were identified and treated separately for each regression[6]. There were no dropouts, and missing data were imputed (see Appendix B).

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[5] Multiple regression was supplemented by nonparametric tests in the case of unmet statistical assumptions.
[6] No outliers were excluded for non-parametric tests

Results

Normality Tests

All of the major study variables except the WOFO scales and AGQ performance approach were significantly nonnormal. In addition, forecasts in the A-scenario, both manipulation checks, and the AGQ mastery approach subscale showed marked restriction of range. We defined marked restriction of range as scores one standard deviation on either side of the mean falling outside the range of possible responses on the scale. Mild range restriction (scores 2 SD from M falling outside the scale) was also found for forecasts in the F-scenario and all need for achievement variables except WOFO work-mastery (see Appendix C for distribution statistics).

The Need for Achievement Hypothesis

The results confirmed the hypothesis that higher mastery needs, but not performance needs, predicted more intense forecasted (un)happiness in response to exam grades.

After excluding two outliers in the F-scenario (see Appendix B), multiple regression was conducted with N = 106 in the F-scenario and N = 108 in the A-scenario. See Table 1 for predictor coefficients. Adjusted R^2 signifies the effect size for the total model. As the predictor variables were intercorrelated (see Appendix A for coefficients), significance and effect size for specific predictors were determined by examining the structure coefficient (r_s) and the semi partial correlation coefficient (sr), in addition to standardized beta (ß). When r_s is significant but ß and sr are not, this indicates that the predictor *has* explanatory value, but that this value is shared with other predictors due to multicollinearity (Stellefson, Hanik, Chaney, & Chaney, 2008).

Participants who scored higher on mastery needs forecasted more intense unhappiness in the F-scenario (F $(7,98)$ = 3.843, p = .001, adj. R^2 = .159), in line with the need for achievement hypothesis. All significant correlations were negative (see Table 2), and AGQ

mastery avoidance was the strongest predictor. This means that participants who reported more anxiety about not learning as much as they could, also anticipated more unhappiness if they should receive an F on the exam. AGQ mastery avoidance accounted for nearly all the variance explained by the model. No other variables had significant unique contributions. No coefficients were significant for AGQ performance approach, and it alone was totally unrelated to forecasted unhappiness. Including demographic variables did not lead to an increase in the model's overall predictive power (all R^2 change $p < .413$).

Participants who scored higher on mastery needs forecasted more intense happiness in the A-scenario (F $(7,100) = 5.005$, p $< .001$, adj. $R^2 = .208$), also supporting the need for achievement hypothesis. All significant correlations were positive, and all three mastery subscales were substantial predictors. This means that participants predicted greater happiness at receiving an A on the exam if they also reported they liked to work hard, valued understanding the content of the class, and were anxious about not learning as much as they could. WOFO work-mastery, AGQ mastery approach, and AGQ mastery avoidance explained proportionate amounts of the variance, but only the first two had significant unique contributions. AGQ performance approach and WOFO competitiveness were completely unrelated to anticipated happiness. Including demographic variables did not lead to an increase in the model's overall predictive power (all R^2 change $p < .126$). Not all statistical assumptions were met in the A-scenario, however, supplemental nonparametric analyses resulted in similar findings (see Appendix C for assumptions and Appendix D for nonparametric results).

In sum, we found a significant relationship between AGQ mastery avoidance and forecasted intensity if participants failed their exam. We also found a relationship between forecasted intensity and AGQ mastery approach, AGQ mastery avoidance, and WOFO work-

Table 1

Regression coefficients for individual predictors in the need-for achievement hypothesis

Variable	F-scenario					A-scenario				
	ß	r_s	sr	Explained variance[a]	Unique explained variance[b]	ß	r_s	sr	Explained variance[a]	Unique explained variance[b]
Total model	-	-	-	**15,9 %**	-	-	-	-	**20,8 %**	-
Predictors										
WOFO competitiveness	-,058	**-,297****	-,047	**1,4 %**	0,2 %	-,168	-,086	-,136	0,1 %	1,9 %
WOFO work-mastery	-,015	**-,339*****	-,012	**1,8 %**	0,0 %	**,226***	**,620*****	**,191***	**6,1 %**	**3,7 %**
AGQ performance approach	,018	-,156	,014	0,3 %	0,0 %	,009	,166	,007	0,4 %	0,0 %
AGQ mastery avoidance	**-,406****	**-,942*****	**-,304****	**14,1 %**	**9,3 %**	,193	**,686*****	,146	**7,5 %**	2,1 %
AGQ mastery approach	-,085	**-,574*****	-,062	**5,2 %**	0,4 %	**,234***	**,754*****	**,172***	**9,0 %**	**3,0 %**
AGQ performance avoidance	,040	**-,251****	,033	**1,0 %**	0,1 %	,124	**,306****	,101	**1,5 %**	1,0 %
Condition	,090	**,258****	,089	**1,1 %**	0,8 %	-,043	-,150	-,042	0,4 %	0,2 %

Note.

* $p < .05$, ** $p < .01$, *** $p < .001$

[a] $R^2 * r_s^2$

[b] sr^2

mastery if participants got an A on their exam. AGQ performance approach was completely unrelated to forecasts in both scenarios.

The Focalism Hypothesis

Analyses did not provide consistent evidence that the manipulation led to more or less intense forecasted (un)happiness, and showed that the manipulation did not affect attentional focus.

Results of the Mann-Whitney U-tests indicated that the manipulation check did not differ between the focalism and control group in either scenario, and that participants in the two conditions reported that they would spend an equivalent amount of time thinking about their grade (see Table 2). Multiple regression indicated a small effect of the manipulation on forecasted unhappiness in the event of receiving a failing grade. The positive correlation indicates that forecasts were less intense for participants in the focalism condition. The regression in the A-scenario did not indicate any effect of the manipulation on forecasts[7] (see Table 1). Results of regression thus supported the construal-level hypothesis in the F-scenario, and the interference hypothesis in the A-scenario.

Table 2
Whitney-Mann U-test of the manipulation check

Variable	F-scenario				A-scenario			
	Mdn.	U	r	Explained variance	Mdn.	U	r	Explained variance
Manipulation check								
Focusing	10,0	1217,0	-,016	2,50 %	9,0	1285,5	-,010	1,00 %
Control	10,0	-	-	-	9,5	-	-	-

Note.
$p < .090$

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[7] Nonparametric supplemental tests supported this finding, see Appendix D.

The Moderation Hypothesis

Results indicated that individual differences in achievement goals did not moderate the effect of the manipulation. As analysis regarding the manipulation check indicated no attentional effect, this was not directly contrary to the moderation hypothesis.

After excluding two outliers in the F-scenario (see Appendix B for outliers), moderation analyses were conducted with N = 106 in the F-scenario and N = 108 in the A-scenario. Regressing happiness forecasts on condition, the need for achievement variables, and their interaction term revealed no significant interactions for any of the need for achievement variables in neither the F-scenario nor the A-scenario (all $p > .101$). In the A-scenario, including age significantly increased predictive power in the moderated regression for AGQ performance approach (R^2 change = .056, p = .014) and AGQ mastery approach (R^2 change = .045, p = .017), but this disappeared when subsequently controlling for gender and course ($p > .214$). Controlling for demographic factors did not significantly affect any other analyses in either scenario (all other R^2 change $p < .053$). Several assumptions for regression were markedly violated, and the reader must therefore interpret all results with caution, especially in the A-scenario (see Appendix C for violated assumptions).

Discussion

This study attempted to answer whether individual differences in achievement goals could be related to how intensely people believe they will react when receiving exam results. The results supported our hypothesis that mastery needs, but not performance needs would predict the intensity of forecasted (un)happiness. Specifically, individuals with high (vs. low) mastery needs predicted they would react more intensely to getting both an A and an F. The study also attempted to experimentally manipulate attentional focus, and we made three specific predictions based on whether forecasts became more extreme (supporting the distraction hypothesis), stayed the same (supporting the interference hypothesis), or less

extreme (supporting the construal-level hypothesis). Results were inconclusive. Finally, we addressed whether people with higher achievement needs would be more sensitive to the attention manipulation. Our hypothesis that need for achievement would moderate the effect of the manipulation was not supported, possibly due to the fact the manipulation did not affect attentional focus as intended.

The remainder of this article will first discuss possible interpretations of the results for each hypothesis. We then go on to discuss theoretical and statistical limitations regarding the present study.

The Need for Achievement Hypothesis

Of our three hypotheses, our findings could provide the most unambiguous support for the need for achievement hypothesis. Consistent with our hypothesis, mastery needs, but not performance needs, predicted the intensity of forecasted happiness in response to a top grade, and the intensity of forecasted unhappiness in response a very poor grade. Those who scored highly on mastery needs forecasted more intense emotional reactions, while those lower in mastery needs had more moderate forecasts. This provides tentative support to the proposition that the impact bias could have an adaptive function. If adaptive, forecasts should be influenced by people's motivational goals, and increase motivation to either achieve or avoid forecasted events.

Mastery. We found that mastery needs consistently predicted happiness forecasts. Participants who reported that they were anxious about not learning all that they could (AGQ mastery avoidance) predicted that they would feel unhappier if they failed the exam, possibly because their anxiety would then be confirmed. Participants who claimed to like hard work (WOFO work-mastery) and wanted to learn as much as possible (AGQ mastery approach), as well as being anxious about not learning (AGQ mastery avoidance) believed they would feel more intense happiness if they got an A. We suggest that this increased anticipation of

emotional intensity might lead these individuals to experience more motivation, which in turn will lead to increased effort to achieve or prevent the forecasted event.

A natural next step would be to investigate whether those who score highly on mastery needs are more or less *biased* in their forecasts. As we did not measure how people actually felt when they received their grades, we cannot rule out that people with higher mastery needs are entirely accurate in their predictions. However, previous research has demonstrated that forecasts for exam grades are indeed biased (Buehler & McFarland, 2001), but we cannot be sure that this holds true for the particular students with high mastery needs. For instance, how much a task is valued has previously been associated with less biased forecasts, but biased forecasts have also been associated with greater success at the same tasks (Hoover, 2012). One interpretation is that an overestimation of the impact of future exam grades may lead to more effortful work, which in turn could lead to more academic success. To examine this in a real-world setting, one could examine whether those with more intense forecasts actually spend more time studying for an exam or use more effortful strategies, and whether their forecasts are in fact biased.

People may feel a stronger need for mastery precisely because they believe learning will have a substantial impact on their happiness. Having high mastery needs mean being focused on achieving proficiency in academic settings (Elliot & McGregor, 2001). For mastery oriented students, the grade may be a proxy for their actual goal, i.e. mastering the subject matter, or not having their anxieties confirmed. Highly mastery oriented students may then expect to be rewarded with happy feelings if they get an A, or the lack of unhappy feelings if they avoid failing, and therefore work hard to achieve it. Our own high-mastery participants also explicitly confirmed liking hard work. Research has demonstrated that people indeed do work harder the happier they believe the goal will make them (Morewedge & Buechel, 2013), and that mastery needs are likewise associated with effort (Fisher & Ford,

1998). Mastery needs have also been associated with experiencing more satisfaction after expending effort, even when a goal is not accomplished (Dweck & Leggett, 1988). Consequently, it seems reasonable that the connection between mastery needs and academic achievement could be explained by more extreme forecasts of happiness leading to more effortful work. Whether this actually is the case is an empirical question.

Performance. The need to perform or compete with others was not a substantial predictor of forecasted (un)happiness in the present study, in line with the need for achievement hypothesis. Performance avoidance, i.e. the need to avoid doing more poorly than others, was only weakly associated with forecasts. Second, performance approach, i.e. the need to do better than others, was completely unrelated to forecasts in both scenarios. This may explain previous findings of limited correlations between performance needs and academic success, as effort is not increased because performance-oriented people do not expect to be rewarded by happy feelings.

However, an exam grade may only be emotionally meaningful for performance oriented students when it can be compared to other people's results. The scenario in the present study did not mention grades received by others, meaning it was not possible for participants to make this comparison. The dimension of mastery may instead have been the main focus for our participants when they simulated the scenario. A way to test this explanation in future studies would be to conduct a similar experiment, and adding a condition where the scenario also included a description of results achieved by others to foster social comparison.

Yet another possibility is that students with high performance needs may have "conceded defeat" earlier in the semester, while mastery focused students were still focused on mastering the subject. Performance-orientation has been associated with perceiving effort as a sign of low ability, even if effort leads to success (Dweck & Leggett, 1988). The

expending of effort alone may therefore lead to feelings of unhappiness for performance-oriented individuals (Dweck & Leggett, 1988). The level of required effort for the level of anticipated success could then possibly determine the level of predicted happiness. For our participants, this could mean that it is not the exam result that is most important for forecasted happiness, but rather how much effort participants expect to have to expend to get there. To determine if this is indeed the case, one could ask participants how much effort they believe they need to put forth each scenario.

Approach and avoidance. As a part of the need for achievement hypothesis, we expected avoidance motivations to be more highly associated with forecasts for receiving an F, and approach motivations to be more highly associated for receiving an A. However, both AGQ mastery avoidance and AGQ performance avoidance significantly predicted forecasts in *both* scenarios. As there was intercorrelation between AGQ subscales (see Appendix A for intercorrelations), this may simply be due to *common method variance* (Drost, 2011). As neither avoidance variable had a significant unique contribution in the A-scenario, the significant correlations do not necessarily contradict our initial assumptions: While approachers work hard to achieve their envisioned result, avoiders put their work into preventing the result they have imagined for themselves if they fail.

The Focalism Hypothesis

The manipulation in the focalism condition used valence-neutral, low-level construal language, with event-consistent content: cues related to school, exams, and studying. It was administered through a text to be read, which was assumed to tax working memory less than actively generating content. The focalism hypothesis had three stipulations, depending on how the manipulation affected forecasts. Statistical analyses of the manipulation check indicated that our manipulation did not successfully focus attention in either scenario. We did not find a significant association between the manipulation and the intensity of forecasted

happiness for a top grade, which corresponds with our expectation that the A-scenario would exhibit a smaller effect than the F-scenario. However, this finding is also in linewith the interference hypothesis, which states that working memory depletion causes bias reduction. However, when imagining a failing grade, results indicated that the manipulation seemed to have a very slight defocusing effect. Our results in the F-scenario may thus be compatible with the construal-level hypothesis, which claims that encouraging low-level construal thinking will cause bias reduction.

Distraction, interference, or construal level? Though the manipulation was intended to focus attention on the exam grade, it may instead have inadvertently *defocused* it. For example, it involved a description of sitting in the cafeteria with fellow students, which may have caused participants to remember the potential distractive or pro-coping effect of social interactions. Our findings would then tentatively support the distraction hypothesis, which states that bias reduction is caused by remembering events that will distract you from the focal event. However, analyses of the manipulation check indicated that participants in the focalism condition were just as focused on their grade as were control participants. An important difference between our study and previous studies on the impact bias is that we attempted to increase bias, whereas other researchers have focused on decreasing it. Perhaps it is not possible to increase the impact bias at all, or perhaps the features of our manipulation were not consistent enough to produce an increase in bias.

According to the interference hypothesis, focusing manipulations should only lead to reduced forecasts if they tax working memory. Results in the A-scenario showed no effect of the manipulation, which supports the interference hypothesis. However, our participants still experienced a slight defocusing effect in the F-scenario. It is unlikely that taxation of working memory in itself produced the defocusing effect we found, and the lack of an effect in the A-

scenario may simply be due to unmet statistical assumptions and insensitive supplemental tests.

The manipulation may have encouraged an increased focus on the details and context of the event, rather than the superordinate features, supporting the construal-level hypothesis. If construal level was responsible for the defocusing, this may also explain why the manipulation check did not detect a change in attentional focus.

Our findings with regard to focalism should be interpreted with caution, and can perhaps best be seen as a starting point for further exploration. If both construal level and distraction plays an equal role in the established defocusing effect, our participants might have been equally influenced by either one, which would result in them cancelling each other out.

One way to follow up these results and test the three hypotheses more explicitly would be an experiment with a 2x2x2 design, where manipulations differ on level of working memory taxation, construal level, and whether cues are event consistent or inconsistent. This would enable us to test the interference hypothesis, the construal-level hypothesis, and the distraction hypothesis respectively. It is possible that different explanatory models apply to the impact bias for positive and negative events e.g. that working memory depletion affected one and construal level the other. It is also possible that different explanations apply to the bias in forecasted intensity and duration. This could be detected through this experimental design provided researchers include measures of both, and a negative and positive scenario.

The Moderation Hypothesis

Finally, no moderation effect was found between need for achievement measures and the manipulation. We hypothesized that participants high in need for achievement would be more sensitive to the manipulation if the manipulation affected attentional focus. As attentional focus was shown to be unaffected, the lack of moderation effect was not

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surprising. Unfortunately, unmet assumptions in the moderated regressions mean that the validity of the results is highly questionable, and we cannot rule out that a moderation does exist despite our nonsignificant findings. Due to range restriction and the nonexistent attentional effect of the manipulation, it was judged improbable that less sensitive nonparametric analyses of moderation would supply more definitive answers.

Limitations

Theoretical limitations. There were a number of theoretical limitations present in the study. First, due to time constraints, we were not able to perform a pilot study to assess the reliability and validity or of our translated measures against the original wordings. The Norwegian translations could possibly be more unreliable and measure a slightly different construct than the original measures. Second, because it was immediately preceded by the F-scenario, a *contrast effect* may have affected responses in the A-scenario (Schwarz & Bless, 1992). The prospect of receiving an A may have functioned as a comfort after imagining failing their exam, causing relief and happiness. This may have inflated happiness forecasts in the A-scenario for the entire sample. Ideally, the order of the scenarios would have been counterbalanced for half the participants, though this would have required a larger sample. Third, using a unidimensional measure may be a methodological weakness common to the field, as unhappiness and happiness does not appear to be opposite anchors on the same scale, but vary independently (Fordyce, 1988; Hahnemann & Krueger, 2006). When forced to merge them into a single response, participants may have given answers that did not accurately represent their forecasts. Fourth, the sample itself limits the interpretability of our results. Students were asked to predict their affective reactions in response to different exams. They might have had divergent expectations about the probability of achieving a high or low grade, and expectations have been shown to play a major role in motivation (Eccles & Wigfield, 2002; Hoover, 2012). However, these differences would likely be equally

distributed across conditions. Fifth, the sampled study programs are highly competitive, and students must have a high GPA from high school to gain admittance (Samordna opptak, 2015). The fact that our sample is fairly homogenous in terms of levels of academic attainment means that results may not be generalizable to a general student population.

Statistical limitations. The present study also had two major statistical limitations: the use of single-item measures, and restriction of range. Single-item measures, used in the present study to measure happiness forecasts, have been shown to have several methodological weaknesses (Gliem & Gliem, 2003), but are nevertheless frequently used in the literature (e.g. Levine, Lench, Kaplan, & Safer, 2012; Morewedge & Buechel, 2013; Wilson et al., 2000). This means that even though our study is vulnerable to these weaknesses, it is not more so than the majority of studies in the field. For an example of a validated single-item measure of happiness forecasting, see Fordyce (1988).

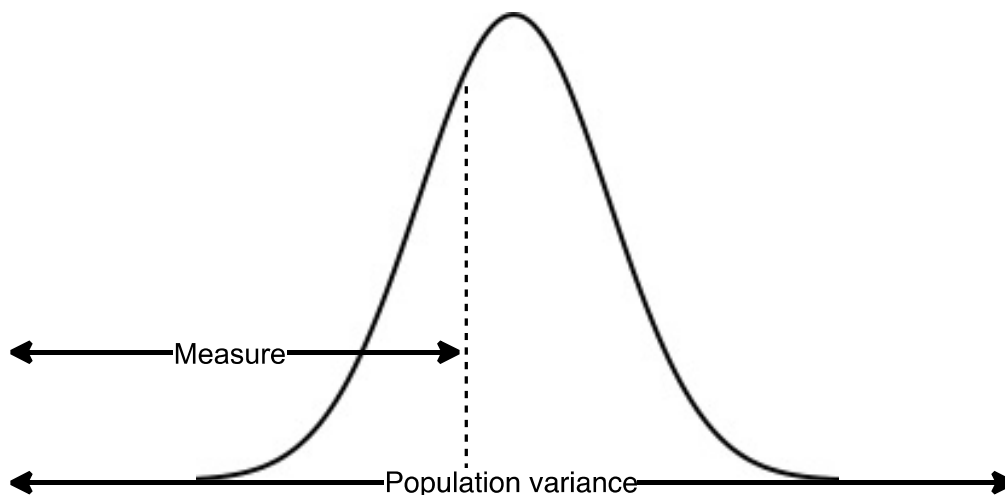A number of the central variables in the current study appeared to be markedly range restricted (see Figure 3).

Figure 2. Graphical representation of a range restricted measure.

This restriction may mean that we are missing a substantial amount of the variation that is present in the population (Sackett & Yang, 2000). When range restriction is caused by measurement issues and not sampling, it may also distort the visible distribution. The portion of the distribution beyond the reach of the measure is not excluded from the sample, but merely stacked at the closest available response alternative. Responses may then be artificially compressed at the end-point of the scale, meaning the visible distribution does not reflect the shape of the actual distribution within the sample itself. Standard deviation and variance would be similarly distorted. Artificial compression of results would therefore mean that the results of any statistical methods using standard deviation or variance in calculations, or that compare the size or shape of distributions, might be inaccurate (Sackett & Yang, 2000). Even mildly range restricted predictor variables in regression reduces the validity of results (Aguinis, 1995), and the regression results in the present study must therefore be cautiously interpreted even when statistical assumptions are met. However, forecasts may simply not be normally distributed in the population, though this would also have statistical implications (Micceri, 1989).

We could not find explicit information about range restriction in the affective forecasting literature. However, when performing a cursory investigation of means and standard deviations, signs of marked and mild range restriction for at least one forecasting variable appeared in all the studies we examined (e.g. Gilbert et al., 1998; Lench, Safer, & Levine, 2011; Wilson, Centerbar, Kermer, & Gilbert, 2005).

Thus, the present study may not be the only one in the field with questionable conclusion validity (Drost, 2011; Mentzer & Flint, 1997). The fact that the varied affective forecasting literature has found consistent evidence of impact bias despite range restriction and single-item measures supports the validity of the results. Forecasts may not be normally distributed, but if they are, measures need to be developed that capture the entire range of the

population distribution. It is encouraging that not all forecast-variables we examined in the literature were restricted, as this indicates that creating such a measure should be possible.

Concluding Remarks

In spite of limitations, we reason that our findings are not purely an illusion caused by measurement issues or unmet assumptions, though the picture may be incomplete. The results suggest a positive relationship between achievement needs/motivation and the intensity of forecasted of emotion, contributing both to the personality literature on individual differences as well as the social psychology literature on affective forecasting. In sum, our findings suggest a definite connection between mastery needs and affective forecasting. We argue that this supports the notion that impact bias serves an adaptive motivational purpose, and that it may cause increased effort and subsequent higher academic achievement for people high in mastery needs. Further research on the relationship between need for achievement and affective forecasts might shed light on both the adaptive function of the biased predictions of emotional reactions, and the cognitive components of mastery needs.

Our result indicated support for both the interference hypothesis and the construal-level hypothesis of focalism. However, restriction of range may have masked the effect of the manipulation. More research is needed to better understand focalism, preferably using validated measures without restriction of range.

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Appendix A[8]

The Need for Achievement Measures

Both the WOFO and AGQ were translated from English to Norwegian. To ensure that the translation was unambiguous and reliable, they were first translated by one researcher, and then translated back to English by an independent researcher who was not familiar with the original measures. "Coworkers" was changed to "fellow students" during translation of the WOFO to make the questions more applicable to our sample. The two questions deleted from the WOFO work-mastery scale were "It is important to me to do my work as well as I can even if it isn't popular with my coworkers" and "I would rather learn easy, fun games, than difficult, thought games" (Spence & Helmreich, 1983, p. 42).

Whether Likert-scales should be defined as ordinal or scale variables is a contentious issue (Boone & Boone, 2012; Carifio & Perla, 2008; Jamieson, 2004; Kuzon Jr, Urbanchek, & McCabe, 1996; Norman, 2010). As the WOFO-scales both consist of four or more Likert-items, we have chosen to treat these as scale. The AGQ subscales, consisting of only three items each, have also been treated as scale, against statistical recommendations (Boone & Boone, 2012), but consistent with the literature (Elliot & McGregor, 2001).

Previous research has found α of .69-.78 for original wording of WOFO competitiveness, and .81 for WOFO work-mastery (de Bruin et al., 2014; Elliot & McGregor, 2001). α for the original wording of the AGQ have also been relatively consistent (performance approach .84-.94, mastery avoidance .74-.88, mastery approach .71-.89, performance avoidance .70-.90 [Elliot & McGregor, 2001; Ganesan, Bt Mamat, Mellor, Rizzuto, & Kolar, 2014; Li, 2013; Van Mierlo & Van Hooft, 2015]). Marked (de Bruin et al.,

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[8] This and subsequent apendices are intended for online publication as supporting information, in line with EJSP guidelines.

2014) and mild (Elliot & McGregor, 2001; Ganesan et al., 2014; Li, 2013) restriction of range are also indicated in previous studies using the measures.

Small to large correlations have been found between subscales within each framework (Elliot & McGregor, 2001; Ganesan et al., 2014; Li, 2013; Van Mierlo & Van Hooft, 2015) suggesting both measures suffer from common method variance (Drost, 2011).

One previous study compared the two frameworks, and found correlations between AGQ mastery approach and WOFO work-mastery (r = .29), as well as between AGQ performance approach and WOFO competitiveness (r = .57). This supports the convergent validity of the two frameworks (Elliot & McGregor, 2001).

Previous studies have investigated the discriminant validity of the original translations of the AGQ using confirmatory factor analysis (Van Mierlo & Van Hooft, 2015), but this was not possible within the time frame of the current study. Instead, we used the correction for attenuation formula as a measure of discriminant validity, and assessed convergent validity by using nonparametric correlation analyses. Results below .85 indicated acceptable discriminant validity (Carmines & Zeller, 1979), and correlations above .50 indicated acceptable convergent validity (Carlson & Herdman, 2012). See Table 3 for validity results.

Table 3

Covergent and discriminant validity of the need for achievement measures

Variables	1.	2.	3.	4.	5.	6.
1. WOFO competitiveness	-	**,516***	-,065	**,270**	,096	,155
2. AGQ performance approach	,742	-	,034	**,233***	**,265**	,072
3. AGQ performance avoidance	,440	,424	-	-,136	-,122	**,430***
4. WOFO work-mastery	,543	,491	,372	-	**,457***	,165
5. AGQ mastery approach	,554	,644	,287	,665	-	**,364***
6. AGQ mastery avoidance	,555	,458	,710	,470	,662	-

Note.

$*p < .05, **p < .01, ***p < .001$

Convergent validity is printed above the diagonal and discriminant validity below.

A large correlation was found between AGQ performance approach and WOFO competitiveness. A below-threshold correlation was found between AGQ mastery approach and WOFO work-mastery. In line with previous findings, AGQ mastery avoidance and AGQ performance avoidance were uncorrelated with the WOFO subscales, indicating that they measured separate constructs. All subscales were discriminately valid, including those thought to be measures of the same underlying construct. In sum, all the translated subscales appeared to measure distinct, though interrelated constructs.

Appendix B

Outliers

The threshold for outliers was set at 3 standard deviations from the mean, and studentized deleted residuals were saved for each case during regression. An outlier was more closely inspected if the value of the studentized deleted residuals exceeded ±1.96. Once an outlier was identified, the decision whether to exclude or keep the outlier was made based on examination of the *actual* versus the *statistically expected* forecasts (Viechtbauer & Cheung, 2010). The original questionnaire was examined to ensure outliers were not a result of coding errors. When conducting multiple regression in the F-scenario, two outliers were identified. After examining the actual and expected responses it was judged probable that these responses were made in error due to participants mentally reversing the scale. They were therefore excluded from analysis. One outlier was also identified in the A-scenario. There was no reason to believe the response, 5, was made in error, as several other participants gave the same score and it was at the midpoint of the scale. It was therefore decided not to exclude the outlier, and analysis was run with and without it. The same outliers identified for the previous regression in the F-scenario were also identified for all moderated regressions in the F-scenario, and excluded. Four outliers were identified for moderated regressions in the A-scenario, all had given a forecast score of 5. It was not believed that the response was erroneous, and so analysis was conducted both with and without the outliers. Our reported results were from analyses including all outliers in the A-scenario, as excluding them did not meaningfully influence assumptions or results.

Missing Data

Five participants had missing values on one or more of the variables, due to skipping a page or a question. These were corrected for with a Markov Chain Monte Carlo (MCMC) Fully Conditional Specification (FCS) imputation method with predictive mean matching.

This is considered best practice in the treatment of missing values (de Bruin, Kok, Leppink, & Camp, 2014). Two completed datasets were generated, and missing values were replaced with the average imputed value across the two datasets. An exception was made for the manipulation check in the A-scenario, where this value exceeded the possible maximum value of the measure. The maximum value was used instead.

Appendix C

Assumptions

See Table 4 for descriptive statistics and normality tests for major study variables[9].

For multiple regression and moderated regression, normality was examined using Shapiro-Wilk on the studentized residuals, as well as inspection of residual histograms, p-p plots, and q-q plots. Autocorrelation was assessed using the Durbin-Watson test. Linearity was examined through visual inspection of scatterplots with added loess-lines ($\alpha = 65\ \%$): a scatterplot of *studentized residuals* against *predicted values,* as well as all partial regression plots. Homoscedasticity was determined using the Koenker test (Koenker & Bassett Jr, 1982), and multicollinearity by examining the Tolerance/VIF values (Cohen et al., 2013). For Kruskal-Wallis H-tests, similarity of distribution was assessed through visual inspection of box-plots. In the case of dissimilar distributions, variables were ranked and tested for homoscedasticity using the Koenker test (Ruxton & Beauchamp, 2008). For Mann-Whitney U-tests, the stochastic equality of distributions was assessed through visual inspection of histograms (Nachar, 2008). Measuring the dependent variables with a single Likert-item means the dependent variables are technically ordinal, but they are usually treated as scale in the literature. We have chosen to treat them as scale for the purpose of regression (otherwise ordinal).

Need for achievement and focalism hypotheses. All assumptions for multiple regression were met in the F-scenario. In the A-scenario, the distribution of residuals was significantly non-normal and plots also indicated a distinctly skewed distribution. The parameter estimates and confidence intervals used in the model are therefore questionable,

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[9] Median and mode were calculated in addition to mean and SD to assess the central tendency of nonnormal variables.

and conclusion validity reduced. In addition, the assumption of homoscedasticity was not met ($p = .004$), which further invalidates the confidence intervals[10].

We supplemented our analysis in the A-scenario with nonparametric tests. The need for achievement hypothesis was examined using the Kruskal-Wallis H-test, a nonparametric test of variance. The Kruskal-Wallis tested the hypothesis from the opposite direction of OLS regression, with the need for achievement variables as test variables, and happiness forecasts as grouping variable. We could then determine whether someone with a high score on a need for achievement measure made more intense forecast (OLS regression), and conversely whether someone with more intense forecast had higher scores on need for achievement measures (Kruskal-Wallis). The focalism-hypothesis was examined using Mann-Whitney U-test, a nonparametric alternative to the t-test, using forecasts as test variable and condition as grouping variable (see Appendix D for results).

For Kruskal-Wallis H-tests, the need for achievement scores were not similarly distributed across all values of forecasts in either scenario, but the assumption of homoscedasticity was fulfilled (all $p > .259$). The test was therefore used to examine differences in *mean ranks* rather than medians. All assumptions for all Mann-Whitney U-tests were met.

Moderation hypothesis. In the F-scenario, the normality assumption was fulfilled only when regressing forecasts on AGQ mastery avoidance, condition, and interaction term. All other regressions suffered from non-normal residual-distributions. Normality plots indicated a multimodal distribution in the WOFO competitiveness and AGQ performance avoidance regressions, and no specific pattern for the others (see Table 5). The main scatterplots indicated linear relationships between predictors and forecasts, but partial regression plots

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[10] When repeating the regression without the outlier, the assumptions remained unmet.

Table 4

Descriptives and normality tests

Variable	M	SD	Min.	Max.	Median	Mode	Shapiro Wilk	Skew.	Kurt.
Forecasts F	1,750	1,624	,00	10,00	2,00	,00	**,844***	**1,654**	**5,612**
Forecasts A	9,093	1,342	5,00	10,00	10,00	10,00	**,714***	**-1,553**	**1,736**
WOFO work-mastery	5,262	,646	3,33	6,67			,990	-,283	-,018
WOFO competitiveness	4,443	1,373	1,40	7,00			,978	-,252	-,687
AGQ Performance approach	4,176	1,527	1,00	7,00	4,00	4,00	**,962**	-,088	-,583
AGQ Mastery avoidance	4,891	1,639	1,00	7,00	5,17	6,00	**,973***	-,447	**-,913**
AGQ Mastery approach	6,148	,859	3,67	7,00	6,33	7,00	**,931***	**-1,020**	,383
AGQ Performance avoidance	3,630	1,668	1,00	7,00	3,33	3,33	**,868***	,167	**-,954**
Manipulation check F	9,278	1,222	2,00	10,00	10,00	10,00	**,630***	**-2,838**	**11,995**
Manipulation check A	8,667	1,680	2,00	10,00	9,00	10,00	**,784***	**-1,446**	**2,338**

Note.

* *p* < .05, ** *p* < .01, *** *p* < .001

Skewness and kurtosis values in bold were above the cutoff when divided by their standard error.

Table 5

Distributions of residuals

Analysis	F-scenario Shapiro Wilk	Skew.	Kurt.	A-scenario Shapiro Wilk	Skew.	Kurt.
Multiple regression	,977	,389	-,085	**,933***	**-1,013**	**,987**
Moderation						
WOFO competitiveness	**,949***	**,544**	-,386	**,808***	**-1,516**	**1,716**
WOFO work-mastery	**,958**	**,502**	-,479	**,862***	**-1,327**	**1,180**
AGQ performance approach	**,951**	**,556**	-,489	**,799***	**-1,465**	**1,535**
AGQ mastery avoidance	,982	,345	-,071	**,873***	**-1,385**	**1,713**
AGQ mastery approach	**,968***	**,512**	-,366	**,896***	**-1,214**	**1,310**
AGQ performance avoidance	**,960**	**,502**	-,351	**,819***	**-1,470**	**1,707**

Note.

$*p < .05, **p < .01, ***p < .001$

Skewness and kurtosis values in bold were above the cutoff when divided by their standard error.

indicated nonlinear partial relationships between all need for achievement variables and happiness forecasts.

None of the regressions in the A-scenario fulfilled the normality assumption, and all plots indicated that the residuals in all regressions were heavily skewed and kurtotic (see Table 5). The main scatterplots indicated linear relationships between predictors and forecasts, but partial regression plots indicated nonlinear partial relationships between forecasts and all need for achievement variables. Assumptions remained unmet when outliers were excluded. Moderation was not tested nonparametrically.

Appendix D

Nonparametric Supplemental Tests

Kruskal Wallis H test examined the need for achievement hypothesis in the opposite direction from the regressions, and supported the regression result (see Table 6). Effect size was determined by computing eta squared (η^2) for the ranked scores (Green & Salkind, 2010).

Table 6

Kruskal-Wallis H-test of the need for achievement hypothesis

	A-scenario			
Variable	χ^2	df	η^2	Explained variance
WOFO competitiveness	4,084	5	,038	3,8 %
WOFO work-mastery	**11,123***	5	**,104***	**10,4 %**
AGQ performance approach	2,157	5	,020	2,0 %
AGQ mastery avoidance	**14,813***	5	**,138***	**13,8 %**
AGQ mastery approach	**11,722***	5	**,110***	**11,0 %**
AGQ performance avoidance	5,647	5	,053	5,3 %

Note.
*p < .05, ***p < .001

See Table 7 for mean ranks across forecasts in the F-scenario. There was an observable general trend for the participants with lower forecasts to have higher scores on AGQ mastery avoidance. See Table 8 for mean ranks across forecasts in the A-scenario. Three general trends were observed, where participants with higher forecasts had higher scores on AGQ mastery approach, AGQ mastery avoidance, and WOFO work-mastery than those whose forecasts are more middling. There were other trends apparent in the distributions of the mean ranks in both scenarios, but these were not statistically significant.

Mann-Whitney U-test was used to supplement regression for the focalism hypothesis in the A-scenario. Results indicated that forecasts were equivalent between conditions in both scenarios (see Table 9 for results).

Table 7

Kruskal-Wallis H-test mean ranks across responses to "How happy would you be that day" in the F-scenario

	Forecasts							
Variables	0	1	2	3	4	5	7	10
WOFO competitiveness	63,3	50,9	59,3	43,9	47,3	69,8	23,0	16,5
WOFO work-mastery	62,6	51,8	51,9	53,4	48,5	30,0	94,0	49,5
AGQ performance approach	59,5	51,0	55,9	53,2	58,1	34,5	14,5	41,0
AGQ mastery avoidance	**73,6**	**52,4**	**61,1**	**31,5**	**33,8**	**24,3**	**9,0**	**74,5**
AGQ mastery approach	69,2	54,0	49,1	46,9	42,0	38,0	92,5	39,5
AGQ performance avoidance	58,4	55,7	61,5	36,8	59,6	48,8	4,5	50,5

Note.
1 = "very unhappy", 10 = "very happy".
No participants gave scores of 6, 8, or 9.
Significant variable in bold.

Table 8

Kruskal-Wallis H-test mean ranks across responses to "How happy would you be that day" in the A-scenario

	Forecasts					
Variables	5	6	7	8	9	10
WOFO competitiveness	81,8	41,2	47,9	50,9	54,8	54,9
WOFO work-mastery	**20,1**	**37,7**	**40,6**	**47,3**	**51,5**	**61,6**
AGQ performance approach	60,0	45,5	39,5	56,1	54,4	55,9
AGQ mastery avoidance	**26,5**	**39,3**	**41,6**	**36,3**	**52,4**	**63,2**
AGQ mastery approach	**26,5**	**22,0**	**38,6**	**48,7**	**54,8**	**60,9**
AGQ performance avoidance	55,3	34,2	52,8	41,1	51,4	59,5

Note.
1 = "very unhappy", 10 = "very happy".
No participants gave scores lower than 5.
Significant variables in bold.

Table 9

Whitney-Mann U-test of the focalism hypothesis

	A-scenario		
Variable	Mdn.	U	r	Explained variance
Forecasts				
Focusing	9,0	1285,500	-,010	1,00 %
Control	9,5	-	-	-

Note.

$p = .350$

Appendix E

Questionnaire

The final pages of the PDF contain the questionnaire for the focalism condition

exactly as it was distributed to the participants, i.e. without page numbers or headings.

Vil du bruke 10 minutter på en <u>anonym</u> spørreundersøkelse?

- Undersøkelsen består av å besvare ulike spørsmål i et kort spørreskjema, og er del av et forskningsprosjekt på personlighet, holdninger og beslutninger ved NHH – Norges Handelshøyskole
- Deltagelse innebærer ingen fare for liv eller helse, og du står fritt til å trekke deg når som helst underveis
- Ansvarlig for forskningsprosjektet er Hallgeir Sjåstad, psykolog fra UiB og PhD-stipendiat ved NHH. Ansvarlig for datainnsamlingen er Maria Wester, student ved profesjonsstudiet i psykologi, UiB.

Informert samtykke
Jeg sier herved Ja ___ (sett kryss) til å delta i denne undersøkelsen.

Bakgrunnsinformasjon

Alder: ___ år

Kjønn: ___ Mann, ___ Kvinne

Studieprogram:_____ Progresjon/Kull: _____

Ønsker du å motta et gavekort fra Kaffemisjonen som belønning etter å ha deltatt i denne undersøkelsen?
___ Ja takk, ___ Nei takk

<u>Kjøreregler: Viktig!</u>
- **Vær <u>stille</u> når du svarer på denne undersøkelsen**
- **Besvar alle spørsmål individuelt; fokuser på deg selv og ikke snakk med andre**
- **Begynn på begynnelsen og besvar alle spørsmål fra start til slutt; ikke bla frem og tilbake underveis**

Hvor godt stemmer hver enkelt påstand for deg?

Vurder deg selv ved å rangere de følgende påstandene på en skala fra 1 (Stemmer ikke i det hele tatt) til 7 (Stemmer perfekt for meg). Bruk det åpne feltet etter hver enkelt påstand til å skrive inn ditt svar (ett av tallene: 1-2-3-4-5-6-7). Vær så ærlig og presis som du kan.

Stemmer ikke i det hele tatt   1   2   3   4   5   6   7   Stemmer perfekt for meg

Det er viktig for meg å jobbe så godt jeg kan, selv om det ikke er populært blant medstudentene mine. _____ (1-7)

Jeg synes det er tilfredsstillende å jobbe så godt jeg kan. _____ (1-7)

Det er tilfredsstillende med godt utført arbeid. _____ (1-7)

Jeg synes det er tilfredsstillende å overgå tidligere prestasjoner, selv om jeg ikke presterer bedre enn andre. _____ (1-7)

Jeg liker å jobbe hardt. _____ (1-7)

En del av det jeg liker ved å gjøre ting er å forbedre tidligere prestasjoner. _____ (1-7)

Jeg vil heller gjøre noe som jeg føler meg trygg og avslappet med, enn noe som er utfordrende og vanskelig. _____ (1-7)

Når en gruppe jeg er med i planlegger en aktivitet vil jeg heller lede den selv enn å bare hjelpe til og la noen andre organisere det. _____ (1-7)

Jeg vil heller lære enkle, morsomme spill enn vanskelige, utfordrende spill. _____ (1-7)

Hvis jeg ikke er flink til noe vil jeg heller fortsette å streve for å mestre det enn å gå videre til noe jeg kan være god i. _____ (1-7)

Når jeg først begynner på en oppgave så fortsetter jeg. _____ (1-7)

Jeg foretrekker å jobbe i situasjoner som krever et høyt ferdighetsnivå.
_____ (1-7)

Jeg prøver oftere å gjøre oppgaver som jeg ikke er sikker på om jeg vil klare, enn oppgaver som jeg er sikker på at jeg vil klare. _____ (1-7)

Jeg liker å ha det travelt hele tiden. _____ (1-7)

Hvor godt stemmer hver enkelt påstand for deg?

Stemmer ikke i det hele tatt   1   2   3   4   5   6   7   Stemmer perfekt for meg

Jeg liker å jobbe i situasjoner som involverer konkurranse med andre.
____ (1-7)

Det er viktig for meg å prestere bedre enn andre i oppgavene jeg gjør. ____ (1-7)

Jeg føler at det å vinne er viktig både i spill og studier/arbeid. ____ (1-7)

Det irriterer meg når andre mennesker presterer bedre enn det jeg gjør.
____ (1-7)

Jeg prøver hardere når jeg konkurrerer med andre mennesker. ____ (1-7)

Hvor godt stemmer hver enkelt påstand for deg?

Ta utgangspunkt i det faget som du nettopp har hatt forelesning i, og vurder deg selv ved å rangere de følgende påstandene på en skala fra 1 (Stemmer ikke i det hele tatt) til 7 (Stemmer perfekt for meg). Bruk det åpne feltet etter hver enkelt påstand til å skrive inn ditt svar (ett av tallene: 1-2-3-4-5-6-7). Vær så ærlig og presis som du kan.

Stemmer ikke i det hele tatt   1   2   3   4   5   6   7   Stemmer perfekt for meg

Det er viktig for meg å gjøre det bedre på eksamen enn andre studenter. _____ (1-7)

Det er viktig for meg å gjøre det bra på eksamen, sammenlignet med andre i dette kullet. _____ (1-7)

Mitt mål i dette faget er å få en bedre karakter enn de fleste andre studenter. _____ (1-7)

Jeg bekymrer meg for at jeg ikke skal lære alt jeg kan i dette faget. _____ (1-7)

Av og til er jeg redd for at jeg ikke skal forstå innholdet i dette faget så grundig som jeg skulle likt. _____ (1-7)

Jeg er ofte bekymret for at jeg ikke skal lære alt som er å lære i dette faget. _____ (1-7)

Jeg vil lære så mye som mulig i dette faget. _____ (1-7)

Det er viktig for meg å forstå innholdet i faget så godt som mulig. _____ (1-7)

Jeg ønsker å fullstendig mestre materialet som presenteres i dette faget. _____ (1-7)

Jeg vil bare unngå å gjøre det dårlig i dette faget. _____ (1-7)

Målet mitt i dette faget er å unngå å gjøre det dårlig. _____ (1-7)

Frykten min for å gjøre det dårlig i dette faget er ofte det som motiverer meg. _____ (1-7)

Hvordan føler du deg akkurat nå?

Denne skalaen inneholder en del ord som beskriver ulike følelser og emosjoner. Les hvert ord i boksen under og gi en passende respons til hvert av ordene i de åpne plassene ved siden av, på en skala fra 1 (Ikke i det hele tatt) til 5 (Ekstremt mye). Indiker i hvilken grad du føler det slik akkurat her og nå. Bruk den følgende skalaen til å svare:

Ikke i det hele tatt   1   2   3   4   5   Ekstremt mye

Hvordan føler du deg akkurat nå? (1-5)

_____ interessert   _____ irritabel

_____ bekymret   _____ kvikk

_____ begeistret   _____ skamfull

_____ opprørt   _____ inspirert

_____ iherdig   _____ engstelig/nervøs

_____ skyldbevisst   _____ målbevisst

_____ skremt   _____ oppmerksom

_____ fiendtligsinnet   _____ skjelven

_____ entusiastisk   _____ aktiv

_____ stolt   _____ redd

Stopp opp et øyeblikk, og se for deg så klart og levende som du kan at du står i den følgende situasjonen:

Ta utgangspunkt i det faget som du nettopp har hatt forelesning i, og tenk to måneder frem i tid til sensuren er klar. Se for deg at det har gått så dårlig på eksamen at du får **karakteren F.**

Du er på lesesalen mellom to forelesninger når du finner ut hvilken karakter du har fått. Du sjekker karakteren din fordi flere av medstudentene som du møter i lunsjen diskuterer hvordan det gikk på akkurat denne eksamenen, og sier at resultatene sikkert blir lagt ut snart. Det neste som skjer etter at du har sjekket karakteren din er at du må gå tilbake til auditoriet for å rekke neste forelesning. Etter denne forelesningen er ferdig har du avtalt eksamenskollokvie i et annet fag, slik at du må bli på skolen i noen timer til før du kan gå hjem.

Dersom dette skjedde deg:

Hvor mye ville du tenkt på eksamenskarakteren din den dagen?

Tenkt svært lite   Tenkt svært mye

0   1   2   3   4   5   6   7   8   9   10

Hvor lykkelig ville du følt deg den dagen?

Svært ulykkelig   Svært lykkelig

0   1   2   3   4   5   6   7   8   9   10

Stopp opp et øyeblikk, og se for deg så klart og levende som du kan at du står i den følgende situasjonen:

Ta utgangspunkt i det faget som du nettopp har hatt forelesning i, og tenk to måneder frem i tid til sensuren er klar. Se for deg at det har gått så dårlig på eksamen at du får **karakteren F.**

Du er på lesesalen mellom to forelesninger når du finner ut hvilken karakter du har fått. Du sjekker karakteren din fordi flere av medstudentene som du møter i lunsjen diskuterer hvordan det gikk på akkurat denne eksamenen, og sier at resultatene sikkert blir lagt ut snart. Det neste som skjer etter at du har sjekket karakteren din er at du må gå tilbake til auditoriet for å rekke neste forelesning. Etter denne forelesningen er ferdig har du avtalt eksamenskollokvie i et annet fag, slik at du må bli på skolen i noen timer til før du kan gå hjem.

Dersom dette skjedde deg:

I hvor lang tid etterpå ville eksamenskarakteren påvirket lykkefølelsen din?

Ingen tid   Svært lang tid

0   1   2   3   4   5   6   7   8   9   10

Hvor lang tid ville det tatt før eksamenskarakteren ikke lenger ville hatt noen betydning for lykkefølelsen din?

___ Måneder, ___ Uker, ___ Dager, ___ Timer

Stopp opp et øyeblikk, og se for deg så klart og levende som du kan at du står i den følgende situasjonen:

Ta utgangspunkt i det faget som du nettopp har hatt forelesning i, og tenk to måneder frem i tid til sensuren er klar. Se for deg at det har gått så bra på eksamen at du får **karakteren A**.

Dersom dette skjedde deg:

<u>Hvor mye ville du tenkt på eksamenskarakteren din den dagen?</u>

Tenkt svært lite   Tenkt svært mye

0	1	2	3	4	5	6	7	8	9	10

<u>Hvor lykkelig ville du følt deg den dagen?</u>

Svært ulykkelig   Svært lykkelig

0	1	2	3	4	5	6	7	8	9	10

Stopp opp et øyeblikk, og se for deg så klart og levende som du kan at du står i den følgende situasjonen:

Ta utgangspunkt i det faget som du nettopp har hatt forelesning i, og tenk to måneder frem i tid til sensuren er klar. Se for deg at det har gått så bra på eksamen at du får **karakteren A**.

Dersom dette skjedde deg:

I hvor lang tid etterpå ville eksamenskarakteren påvirket lykkefølelsen din?

Ingen tid   Svært lang tid
0   1   2   3   4   5   6   7   8   9   10

Hvor lang tid ville det tatt før eksamenskarakteren ikke lenger ville hatt noen betydning for lykkefølelsen din?

___ Måneder, ___ Uker, ___ Dager, ___ Timer   (fyll ut)

I det faget som du nettopp har hatt forelesning i:

Hvor stor arbeidsinnsats er du villig til å legge ned i eksamensforberedelsene dine, fra i dag og frem til eksamensdagen?

Ingen innsats   Svært stor innsats
0   1   2   3   4   5   6   7   8   9   10

Hvor mye tid per uke er du villig til å bruke på å forberede deg til eksamen, fra i dag og frem til eksamensdagen?

___ timer