Using Personality Measures for Selection Decisions: Predictive Utility and Applicants' Faking Behaviour

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Dissertation

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Author's Declaration

This dissertation is an independent and original academic work prepared and written entirely by myself. Thoughts and ideas that were taken from other sources are indicated as such and cited appropriately. The three publications this publication-based dissertation is based on fulfill the necessary requirements for publication-based dissertations.

Georg Krammer

Acknowledgments

Just like a dissertation, everything in life is a process. Life is movement. It does not stand still. It grows, it flows. The experiences we have chosen for us mold us, form us. This process changes us. It does not occur by itself, it occurs if we let it transform us, and - more importantly - if we play an active part in it. That holds for every review, refusal, and acceptance I have ever gotten; on the small scale in publishing and science, and on the large scale in everything else.

For my dissertation, obviously I want to thank everyone who has accompanied me along the process of my dissertation. Thank you Martin for introducing me to measurement theory and sparking my interest. Thank you Markus for being the best co-author one could imagine. Thank you Barbara, for being my colleague, and becoming my friend; I am looking forward to all the times together that are to come. Thank you Regina for looking after me; I promise I will return the favour.

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Yes, life is a process. On we go...

Abstract

Aim of this publication-based dissertation was to scrutinize the use of personality measures for selection decisions. This was done in three studies examining personality measures used for selection decisions to teacher education. First, *Study 1* evaluated the predictive utility of personality measures for differentiated facets of academic achievement. In accordance with a large body of research, personality measures retained their predictive utility over and above other determinants of academic achievement. With *Study 1* supporting the usefulness of personality measures for predicting future academic achievement in the academic field examined, *Study 2* then addressed the prevalence of applicants' faking behaviour on the personality scales used for selection decisions. *Study 2* showed that applicants increased their raw mean test scores in nearly all personality scales examined, and that applicants' faking behaviour could detrimentally affect applicants' rank order. Based on the findings of *Study 2*, *Study 3* aimed to shed light on the effects of applicants' faking behaviour on the psychometric properties of the personality scales. Measurement invariance analyses revealed that applicants primarily faked by increasing their responses to selected items. The more heterogeneously the responses were increased, the more measurement properties were detrimentally affected. As the synopsis of the three studies, theoretical and practical implications for the use of personality measures for selection decisions are discussed.

Zusammenfassung

Das Ziel dieser publikationsbasierten Dissertation war es die Verwendung von Persönlichkeitsinventaren zu Selektionszwecken kritisch zu hinterfragen. Dafür wurden drei Studien zu der Verwendung von Persönlichkeitsinventaren zur Zulassung zu Lehramtsstudien durchgeführt. *Studie 1* thematisierte die prädiktive Validität von Persönlichkeitsinventaren für differenzierte Facetten von Studienerfolg. Die prädiktive Validität von Persönlichkeitsmerkmalen für Studienerfolg konnte im Einklang mit bisheriger Forschung über andere Merkmale hinaus gezeigt werden. Aufbauend auf der prädiktiven Validität von Persönlichkeitsmerkmalen für Lehramtsstudienerfolg, befasste *Studie 2* sich mit der Prävalenz des Faking-Verhaltens von BewerberInnen bei Persönlichkeitsskalen. *Studie 2* zeigte auf, dass BewerberInnen ihre mittleren Testwerte in fast allen untersuchten Skalen erhöhten, und dass durch dieses Faking-Verhalten die Rangreihen der BewerberInnen verzerrt werden können. *Studie 3* vertiefte diese Befunde, indem die psychometrischen Konsequenzen, die durch das Faking-Verhalten von BewerberInnen entstehen können, untersucht wurden. Die Messinvarianzanalysen zeigten, dass BewerberInnen hauptsächlich Faking-Verhalten zeigten indem sie ihre Antworten auf ausgewählten Items erhöhten; je heterogener diese Erhöhungen waren, umso mehr wurden Messeigenschaften beeinflusst. Abschließend werden die theoretischen und praktischen Implikationen der drei Studien diskutiert.

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1 Introduction

Just like selection processes in general, college admission processes are faced with *faking* (cf. Kuncel & Hezlett, 2010) – a type of response bias wherein respondents distort their responses to self-descriptive items to be viewed more favourably (McFarland & Ryan, 2000). The high stakes involved in selection decisions are likely to prompt some sort of faking behaviour from at least some applicants (cf. Dilchert, Ones, Viswesvaran, & Deller, 2006; Tett & Simonet, 2011). Faking may compromise the fairness of selection decisions, as applicants differ in their propensity and intensity to fake (e.g. Birkeland, Manson, Kisamore, Brannick, & Smith, 2006; Mueller-Hanson, Heggstad, & Thornton, 2003; Rosse, Stecher, Miller, & Levin, 1998; Winkelspecht, Lewis, & Thomas, 2006; Zickar & Robie, 1999). Furthermore, due to faking, individual differences in test scores may not reflect only individual differences in the underlying trait, but also e.g. individual differences in the perceived relevance of items (cf. König, Merz, & Trauffer, 2012; Ziegler, 2011). In line with this reasoning, previous studies indicate that faking can distort applicants' rank-order and thus subsequent selection decisions (e.g. Mueller-Hanson et al., 2003; Rosse et al., 1998; Winkelspecht et al., 2006).

Applicants' faking behaviour begins to pose a challenge for selection decisions as soon as measures are used where faking is possible. Faking could be simply avoided by omitting self-descriptive measures and relying on cognitive measures. For example, general mental ability has proven its predictive utility for academic achievement (e.g. Hell, Trapmann, & Schuler, 2007; Kuncel, Hezlett, Sarah, & Ones, 2001; Kuncel, Hezlett, & Ones, 2004; Kuncel, Wee, Serafin, & Hezlett, 2010). However, personality traits have also proven their predictive utility for academic achievement (e.g. Ackerman & Heggstad, 1997; Farsides & Woodfield, 2003; O'Conner & Paunonen, 2007; Paunonen & Ashton, 2001; Poropat, 2009; Richardson, Abraham, & Bond, 2012). While the predictive utility of personality traits is smaller than that of general mental ability, it prevails even when accounting for general mental ability (e.g. Farsides & Woodfield, 2003; Poropat, 2009). Therefore, it would not seem wise to rely only on cognitive determinants of academic achievement for selection decisions. Consequently, applicants' faking behaviour needs to be addressed.

While the predictive utilities of cognitive and non-cognitive determinants of academic achievement seem well established, it is still necessary to consider their usefulness for specific academic fields. Even for general mental ability, studies can offer mixed results, as some meta-analyses argue for (e.g. Hell et al., 2007) and some against (e.g. Kuncel et al., 2001) its predictive utility for academic achievement being moderated by the academic field. The liter-

ature can also offer an ambiguous view on non-cognitive determinants. For example emotional stability has often been hypothesized to predict academic achievement (cf. Ackerman & Heggestad, 1997; Farsides & Woodfield, 2003), has failed to do so for academic achievement irrespectively of the academic field ($\rho = .02$: Poropat, 2009), while retaining its predictive utility in specific academic fields (e.g. teacher education: Mayr, 2011).

These mixed results for cognitive and non-cognitive determinants of academic achievement may be attributable to tertiary education having elements unique to the respective academic field. Teacher education, for example, is of a highly applied nature world wide due to *student-teaching* (Darling-Hammond & Lieberman, 2012). For the performance in student-teaching, studies have failed to show the predictive utility of general mental ability ($\rho = .04$: Kuncel et al., 2004). However, student-teaching is often experienced as the most influential part of teacher education (Ronfeldt, 2012), and meta-analyses have shown its predictive utility for future teacher performance (e.g. D’Agostino & Powers, 2009). Emotional stability, on the other hand, has shown its predictive utility for the teacher education and profession (cf. Mayr, 2011). The predictive utility of emotional stability for academic achievement in teacher education may also be attributable to the applied nature of this education, as thus the risks of the profession can already be risks during the education (Gold & Roth, 1993). Therefore, the specific academic discipline should be considered, and even the use of well established cognitive and non-cognitive determinants for selection decisions should be tailored to the specific academic discipline.

Following this line of reasoning leads to three basic questions which need to be addressed for college admission processes in specific academic disciplines:

1. Should well established determinants of academic achievement be used, such as general mental ability and personality traits?
2. If personality traits are used, do applicants fake?
3. If applicants fake, does applicants’ faking behaviour detrimentally affect the psychometric properties of the personality measures?

The aim of this publication-based dissertation is to address these three questions for college admission processes in teacher education. To do so, three studies were conducted and published, which subsequently address these three questions. In the following chapters, these three studies will be outlined and the main conclusions drawn. Where appropriate, additional analyses will be presented. For further details, please consult the full texts in Section 6.1, Section 6.2, and Section 6.3, respectively.

2 Study 1: Predicting Academic Achievement in Teacher Education

The aim of *Study 1* (cf. Section 6.1: Krammer, Sommer, & Arendasy, 2016) was to examine the predictive utilities of determinants for academic achievement in teacher education. This was done by studying one cohort of students ($n = 176$) over their entire three year long bachelor's degree of education. Cognitive and non-cognitive determinants of academic achievement in teacher education were considered as predictors. For the academic achievement, all grades of the three year long bachelor's degree were used.

First off, defining and measuring academic achievement in teacher education was revised. Grade point averages (GPA) are often considered the golden standard for measuring academic achievement. Thus, most meta-analyses (e.g. Hell et al., 2007; Kuncel et al., 2001, 2004; Poropat, 2009; Richardson et al., 2012; Trapmann, Hell, Weigand, & Schuler, 2007) resort to GPAs to measure academic achievement. However, GPAs can be differentiated according to different courses types, e.g. courses for general knowledge, domain-specific knowledge, or applied courses (cf. D'Agostino & Powers, 2009; Gustafsson & Balke, 1993). The usefulness of such a differentiation is supported by studies showing that GPAs obtained from different course types exhibit differential structural relations to determinants of academic achievement and occupational performance (cf. D'Agostino & Powers, 2009; Gustafsson & Balke, 1993; Reeve, 2004). Due to student-teaching, teacher education world wide includes applied courses (Darling-Hammond & Lieberman, 2012). GPAs of these student-teaching courses can be seen as an instance of procedural knowledge (the course requirements are similar to future occupational performance: Kuncel et al., 2004). Therefore, it may prove useful to distinguish between courses conveying declarative knowledge and procedural knowledge in teacher education. Consequently, academic achievement in *Study 1* was modeled by GPAs reflecting a differentiation between courses conveying declarative knowledge and procedural knowledge.

Three determinants of academic achievement were considered. The first determinant was general mental ability. General mental ability is one of the most important determinants of academic achievement (e.g. Credé & Kuncel, 2008; Hell et al., 2007; Kuncel et al., 2001, 2004, 2010; Kuncel & Hezlett, 2007). The second determinant was a personality trait, namely a facet of the Big Five domain factor emotional stability. Emotional stability has often been hypothesized to predict academic achievement (cf. Ackerman & Heggestad, 1997; Farsides & Woodfield, 2003). However, this domain factor has failing to do so ($\rho = .02$: Poropat, 2009). Nevertheless, some of its facets are related to academic achievement (e.g. test anxiety: $-.21 \leq \rho \leq -.33$; re-

actions to stress: $-.11 \leq \rho \leq -.14$; Ackerman & Heggstad, 1997; Hembree, 1988; Richardson et al., 2012). Consequently, more specific facets of emotional stability may be useful for predicting academic achievement, such as *reaction to stress* (cf. Ackerman & Heggstad, 1997). This may be especially important for the education to a profession, where 1) stress and burnout are prevalent (Schaarschmidt, 2005; Schaarschmidt & Kieschke, 2007), 2) teachers' stress has been linked to important student behaviours, such as not paying attention in class ($r = .39$) and hostility toward the teacher ($r = .40$: Geving, 2007), and 3) the risks of the profession can already be risks during the education (Gold & Roth, 1993). As a third and final determinant, *realistic job expectations* – the degree to which realistic expectations about prospective education and job are held – were introduced as a new potential predictor of academic achievement. Studies suggest that students may not always have realistic expectations of studying at university, and that providing them with realistic information on what to expect can reduce attrition (e.g. Braxton, Vesper, & Hossler, 1995; Cooke, Sims, & Peyrefitte, 1995; Earnest & Dwyer, 2010). Providing such realistic information increases self-selection, organizational commitment, satisfaction, and possibly even academic achievement, by lowering the initial expectations (e.g. Braxton et al., 1995; Cooke et al., 1995; Earnest & Dwyer, 2010). Unfortunately, studies so far have never examined individual differences in the expectations and their predictive utility for academic achievement.

The hypotheses for *Study 1* were that the examined determinants of academic achievement are differentially linked to the different facets of academic achievement. The predictive utility of general mental ability should be higher for the declarative knowledge facet of academic achievement than for the procedural knowledge facet. In contrast, the predictive utility of the facet of emotional stability should be higher for the procedural knowledge facet than for the declarative knowledge facet. Finally, the predictive utility of realistic job expectations – the newly introduced potential predictor – was examined.

2.1 Method

Measures. Data for all predictors were collected within the college admission process of the University College of Teacher Education Styria. General mental ability was assessed by fluid intelligence with two subtests of the Intelligence Structure Battery (INSBAT: Arendasy et al., 2012): numerical-inductive reasoning (NIR) and figural-inductive reasoning (FIR). The facet of emotional stability was assessed with three subscales forming the higher order factor *health and recreational behaviour* of the Inventory for Personality Assessment in Situations (IPS: Schaarschmidt & Fischer, 2013). The

realistic job expectations were assessed with a structured interview designed specifically for college admission in teacher education.

Sample. In the academic year 2011/12, 237 applicants were accepted and began to study at the University College of Teacher Education Styria. Of these 237 students, 176 finished their bachelor’s degree three years later. These 176 graduates comprised the final sample.

Analyses. Structural equation models were used to evaluate the predictive utilities. The structural relations between the latent predictor and criterion variables were evaluated in a two-step process (cf. Mueller & Hancock, 2008). First, the measurement models for the predictor and criterion variables were evaluated separately. Secondly, the two measurement models were combined into a full structural model to examine the predictive utilities of the latent predictors for the criteria. For parameter estimation a WLSMV estimator (cf. Lei, 2009; Savalei & Rhemtulla, 2013) was used (lack of multivariate normality and some indicators constituted categorical variables). The structural equation models were calculated with *lavaan* (Rosseel, 2012).

2.2 Results & Discussion

The measurement model of the predictor variables ($\chi^2[11] = 17.655$, $p = .09$, $RMSEA < .01$, $CFI > .99$, $SRMR = .042$) and the criterion variables ($\chi^2[8] = 6.651$, $p = .575$, $RMSEA < .01$, $CFI > .99$, $SRMR = .029$) fitted the data well. The final full structural model also fitted the data well ($\chi^2[71] = 76.004$, $p = .319$, $RMSEA = .020$, $CFI = .980$, $SRMR = .054$). The final full structural model with the standardized path coefficients, and explained variances of the criteria is depicted in Figure 1. As Figure 1 shows, all predictors had predictive utility.

As hypothesized, the predictors were differentially linked to the different facets of academic achievement. On the one hand, these findings support that GPAs assessing academic achievement in teacher education should be differentiated between declarative and procedural knowledge facets. On the other hand, these findings suggest that selection decisions in college admission processes for teacher education would profit from not being solely based on cognitive determinants, such as general mental ability. Non-cognitive determinants, such as personality traits, can augment the selection decisions by considering facets of teacher education for which general mental ability fails to show predictive utility. Consequently, it needs to be addressed if applicants’ fake on the personality measures to increase their chances of being admitted to their desired academic field of study.

Figure 1: The final model ($\chi^2[71] = 76.004$, $p = .319$, $RMSEA = .020$, $CFI = .980$, $SRMR = .054$) with standardized path coefficients. For the declarative knowledge, procedural knowledge, and the grade on the bachelor thesis, the amount of explained variance is given.

Note: CRS = emotional stability facet; RJE = realistic job expectations; Gf = fluid intelligence; RE = relaxation after a working day; RB = active recovery behaviour in leisure; HP = health prevention because of warning signals; R1 and R2 = 1st and 2nd Rater of the structured interview; NIR = numerical inductive reasoning; FIR = figural inductive reasoning, GPA = grade point average.

3 Study 2: Do Applicants Fake?

The aim of *Study 2* (cf. Section 6.2: Krammer & Pflanzl, 2015) was to assess if applicants to teacher education fake on the personality scales used in their college admission process. To this end, applicants were re-tested as incumbents ($n = 116$) half a year after they underwent their college admission process. The incumbents were re-tested with the same personality scales. Additionally, the incumbents were randomly assigned to three different response conditions. Aim of one of these response conditions was to obtain honest responses, to offer a comparison for the test scores obtained in the college admission setting.

For the practice of college admission processes, effects of applicants' faking behaviour on the selection decision are crucial. Scholars have argued that in real life selection settings effects of applicants' faking behaviour may be negligible (e.g. Bradley & Hauenstein, 2006; Hogan, Barrett, & Hogan, 2007; Ones, Dilchert, Viswesvaran, & Judge, 2007). This view has been partially supported by a recent meta-analysis indicating only negligible to small differences between applicants' and incumbents' Big Five test scores ($0.11 \leq d \leq 0.45$: Birkeland et al., 2006). It stands to reason, however, if this is applicable to the subsequent selection decisions. After all, selection settings offer the opportunity to distort one's own responses: applicants are motivated to make a good impression, personality measures are quite transparent, and there is no undisputed way to detect faking (Rosse et al., 1998). Moreover, in theoretical models of applicants' faking behaviour, applicants' faking is seen as an individual differences construct, always being at least partially determined by individual differences (e.g. Goffin & Boyd, 2009; Marcus, 2009; McFarland & Ryan, 2000, 2006; Mueller-Hanson, Heggstad, & Thornton, 2006; Snell, Sydell, & Lueke, 1999). Along this line of reasoning, faking behaviour has been shown to affect applicants' rank order, and thus subsequent selection decisions (e.g. Mueller-Hanson et al., 2003; Rosse et al., 1998; Winkelspecht et al., 2006).

Accordingly, it was hypothesized for *Study 2* that applicants will distort their responses to present themselves more favourably than they normally would. Furthermore, there should be individual differences in this distortion of test scores, leading to a distortion of rank-order.

3.1 Method

Sample. In the respective academic year, 286 applicants were admitted to the University College of Teacher Education Styria. 121 thereof participated in *Study 2* as incumbents. Of these 121 incumbents, it was possible

Figure 2: The combined within-subject and between-subject design of *Study 2* and *Study 3*. At selection (t1), all respondents were applicants and went through the same college admission process. At re-test (t2), the same applicants were by now incumbents, and were randomly assigned to one of three response conditions.

to join the data for 116 with their college admission process. These 116 incumbents formed the final sample.

Design & Procedure. All participants filled out the personality scales as part of the college admission process of the University College of Teacher Education Styria (selection, t1). Half a year later, they were asked to fill out the same personality scales a second time (re-test, t2). At re-test (t2), the participants were randomly assigned to one of three response conditions. The response conditions differed only in the instructions under which the personality scales were filled out. In the honest-condition, the standard instructions of the personality scales were used, to collect responses that were as honest as possible. In the other two conditions, participants were either asked to portray themselves as favourably as possible to be admitted to their respective study major (faking-good-condition), or to try to respond to the personality scales like they did half a year ago when they were applicants (reproduce-condition). The complete 2×3 design is depicted in Figure 2. The condition of interest for *Study 2* is the honest-condition.

Measure. The personality inventory used was the Inventory for Personality Assessment in Situations (IPS: Schaarschmidt & Fischer, 2013). The IPS is comprised of three higher-order factors: *social and communicative be-*

haviour (6 subscales), *health and recreational behaviour* (6 subscales), and *performance behaviour* (3 subscales). Additionally, the IPS assesses the satisfaction with one's own behaviour in these three higher-order factors. All together, this yields 18 subscales. All 18 subscales were relevant for the selection decision.

Analyses. If applicants faked and if their rank-orders were affected was analysed by (M)ANOVAs and rank-order correlations, respectively. First, a MANOVA evaluated if there was an overall effect. Given an overall interaction between response condition and measurement point, ANOVAs evaluated the interaction for each subscale. If interactions were significant, post-test first examined the comparability of the response conditions at selection (t1), and then the differences within the response conditions between the measurement points. Finally, rank-order correlations examined the stability of the rank-order across selection (t1) and re-rest (t2). In the following, the focus will lie on the honest-condition.

3.2 Results & Discussion

The MANOVA revealed an overall interaction between the response conditions and the measurement points ($F[36, 194] = 3.87, p < .01, \lambda = .26, \eta^2 = .42$). The subsequent ANOVAs revealed interactions for all subscales (lowest effect: $F[2, 113] = 5.35, p < .01, \eta^2 = .08$; largest effect: $F[2, 113] = 59, p < .01, \eta^2 = .51$). Comparing the mean test scores of selection (t1) and re-test (t2) in the honest-condition yielded effects between $d = -0.27$ and $d = -1.87$. These effects were significant for 16 out of 18 subscales ($-0.58 \leq d \leq -1.87$). The rank-order correlations in the honest-condition were between $r = .17$ and $r = .70$. These rank-order correlations were significant for 14 out of 18 subscales.

The resulting distortion of mean responses showed that applicants increased their responses in the admission testing setting. For the subscales affected by applicants' faking behaviour, the range of the mean response distortion was higher than the range reported by a recent meta-analysis on applicants' faking behaviour in personal selection ($0.11 \leq d \leq 0.45$: Birkeland et al., 2006). This may be attributable to the fact that the meta-analysis considered Big Five domain factor measures for all selection decisions. By contrast, the personality measure used for the selection decision to teacher education in *Study 2* was a measure specifically fitted to the teaching profession (Schaarschmidt & Fischer, 2013). For the latter, the content of the personality measure should therefore be attained as highly relevant, and this perceived relevance is appraised by applicants (e.g. Birkeland et al., 2006; Ziegler, 2011).

The stabilities of rank-orders across applicants and incumbents further corroborated that applicants' faking behaviour should not be considered negligible. In general, the stability of the rank-orders was in line with previous studies, but had a larger range (cf. conscientiousness: $.50 \leq r \leq .62$; Big Five: $.46 \leq r \leq .68$; Griffith, Chmielowski, & Yoshita, 2007; Hogan et al., 2007; Peterson, Griffith, Isaacson, O'Connell, & Mangos, 2011). For some subscales, the stability of rank-order was given (e.g. $r = .7$). However, the rank-order was not preserved for 4 out of 18 subscales, and was still arguably low for some of the other 14 subscales. These results corroborate that applicants' faking behaviour can distort applicants' rank-order. This distortion points towards applicants' faking behaviour being influenced by individual differences of the applicants. Furthermore, the range of distortion across the subscales suggests that differences between the scales also influence applicants' faking behaviour (e.g. transparency of assessment: Marcus, 2009; McFarland & Ryan, 2000, 2006).

In summary, if personality measures are used for selection decisions, applicants will show at least some amount of faking behaviour. To gauge how detrimental the effects of applicants' faking behaviour are, it is necessary to go beyond raw means and correlations, and examine the effects on the psychometric properties of the personality measures used.

4 Study 3: Psychometric Effects of Applicants' Faking Behaviour

The aim of *Study 3* (cf. Section 6.3: Krammer, Sommer, & Arendasy, in press) was to assess if and how applicants' faking behaviour affects the psychometric properties of the personality scales used for selection decisions. To do so, the measurement invariance (cf. Cheung & Rensvold, 2002; Millsap, 2011; Mislevy et al., 2013; Raykov, Marcoulides, & Li, 2012; Vandenberg & Lance, 2000) across applicants' and incumbents was examined for three subscales of the IPS, one for each of the three higher-order factors of the IPS. Additional data ($n = 127$; $N = 243$) was collected following the design of *Study 2* (cf. Figure 2) to provide a sample size allowing the measurement invariance analyses. As in *Study 2*, the honest-condition will also be the focus of *Study 3*, i.e. the comparison between applicants' responses and their honest responses as incumbents half a year later.

Four levels of measurement invariance can be distinguished: 1) *configural*, 2) *weak*, 3) *strong*, and 4) *strict* measurement invariance. These levels of measurement invariance build upon each other, and they are tested level by level. Additionally, the invariance of latent variances, latent means, and latent covariances can be examined.

Applicants' faking behaviour would have different practical implications, depending on the level of measurement invariance which is affected. If applicants' faking behaviour would affect configural measurement invariance, scores of incumbents and applicants could not be directly compared in a meaningful way (e.g. Cheung & Rensvold, 2002; Millsap, 2011; Mislevy et al., 2013; Vandenberg & Lance, 2000). If weak measurement invariance would be affected, then a nonuniform bias would be present (cf. Barendse, Oort, & Garst, 2010; Penfield & Camilli, 2007). Therefore, predictive validities derived from samples of incumbents would not apply to applicants (Chen, 2008). If strong measurement invariance would be affected, a uniform bias would be present (cf. Barendse et al., 2010; Penfield & Camilli, 2007). Consequently, mean scores differences between applicants and incumbents would not only reflect mean individual differences, but also measurement bias (cf. Chen, 2008; Cheung & Rensvold, 2002; Li & Zumbo, 2009; Millsap, 2011). Finally, if only strict measurement invariance would be affected, it could be argued that applicants' faking behaviour could have little practical implications.

Applicants' faking behaviour can theoretically affect measurement invariance on every level of measurement invariance. Consider a questionnaire for the Big Five domain factor extraversion and a teacher vacancy. The questionnaire may measure extraversion for incumbents but extraversion and faking intensity for applicants. Thus, configural measurement invariance would not be given across applicants and incumbents. Now, let on item of the questionnaire be "I like to stay in contact with parents". For applicants, this item content may be an indicator of their extraversion. Incumbents, by contrast, may view this item content as a necessary part of their job. In this example, the item would reflect individual differences in extraversion for applicants, but would do so less for incumbents. Consequently, weak measurement invariance would be affected. Now consider an applicant who would normally endorse "staying in contact with parents" only to a small degree. When applying for the teacher vacancy, however, the applicant may perceive this item as relevant for the teaching profession, and endorse this item to a large degree. Were this the case, strong measurement invariance would be affected. Finally, consider the need of applicants for the job. The individual differences in this need would be a potential source of systematic variance, thus introducing variance unaccounted for by the psychometric model. In this case, strict measurement invariance would be affected.

It was hypothesized for *Study 3* that strict measurement invariance should hold across applicants and incumbents, with only moderate re-test correlations and higher latent means of the applicants compared to the incumbents. Strict measurement invariance was hypothesized due to some scholars (e.g.

Bradley & Hauenstein, 2006; Hogan et al., 2007; Ones et al., 2007) arguing that applicants' faking behaviour should have only negligibly effects on their personality test scores. This line of reasoning is corroborated by one of the very few studies examining measurement invariance across applicants and incumbents (Robie, Zickar, & Schmit, 2001). Moderate re-test correlations were hypothesized because studies indicate a distortion of applicants' rank order (cf. Mueller-Hanson et al., 2003; Rosse et al., 1998; Winkelspecht et al., 2006), and only moderate re-test correlations were found between applicants and incumbents (e.g. Griffith et al., 2007; Griffin & Wilson, 2012; Peterson et al., 2011). Finally, latent mean differences were hypothesized because applicants have shown higher mean scores than incumbents (Birkeland et al., 2006).

4.1 Method

Sample & Procedure. The data was collected in the same way as in *Study 2* (cf. Section 3.1). The final sample was 243 incumbents, for whom it was possible to join the data across selection (t1) and re-test (t2).

Measure. As in *Study 1* and *Study 2*, the Inventory for Personality Assessment in Situations was used (IPS: Schaarschmidt & Fischer, 2013). To study applicants' faking behaviour, one subscale of each of the three higher-order factors of the IPS was selected: *activity in familiar communicative situation* was chosen for the higher-order factor *social and communicative behaviour*, *preventive health behaviour in response to warning signals* for the higher-order factor *health and recreational behaviour*, and *self-confidence in test situation* for the higher-order factor *performance behaviour*.

Analyses. Multi-group means and covariance structure models were computed. Two measurement models formed the basis for the model, one for each measurement point. The residuals of the same items in the measurement models were allowed to correlate between selection (t1) and re-test (t2) to account for the re-administration. This model was specified for each response condition. The full model is depicted in Figure 3.

Even though the respondents were randomly assigned to the three response conditions at re-test (t2), in a first step, the comparability of the three response conditions at selection (t1) was examined. Ideally, strict measurement invariance, and equal latent means and variances across the three response conditions should be given. Accordingly, constraints were imposed for weak ($\lambda_{t1,h,1} = \lambda_{t1,r,1} = \lambda_{t1,f,1}$, etc.), strong ($\tau_{t1,h,1} = \tau_{t1,r,1} = \tau_{t1,f,1}$, etc.), and strict ($\theta_{\epsilon_{t1,h,1}} = \theta_{\epsilon_{t1,r,1}} = \theta_{\epsilon_{t1,f,1}}$, etc.) measurement invariance, followed by constraints for the latent trait variances ($\Phi_{t1,h} = \Phi_{t1,r} = \Phi_{t1,f}$) and means ($\xi_{t1,h} = \xi_{t1,r} = \xi_{t1,f}$).

Figure 3: The multi-group means and covariance structure model, containing one-factor models at selection (t_1) and at re-test (t_2), and the covariances. The λ s denote the factor loadings, the τ s the intercepts, the $\theta\epsilon$ s the residual variances, the rc s the residual covariances, the ξ s the latent trait means, the Φ s the latent trait variances, and r s the latent trait covariance.

After examining the measurement invariance across the three response conditions at selection (t1), the measurement invariance across the measurement points was examined. To this end, constraints were imposed to test for weak ($\lambda_{t1,h,1} = \lambda_{t2,h,1}$, $\lambda_{t1,h,2} = \lambda_{t2,h,2}$, etc.), strong ($\tau_{t1,h,1} = \tau_{t2,h,1}$, $\tau_{t1,h,2} = \tau_{t2,h,2}$, etc.), and strict ($\theta_{\epsilon_{t1,h,1}} = \theta_{\epsilon_{t2,h,1}}$, $\theta_{\epsilon_{t1,h,2}} = \theta_{\epsilon_{t2,h,2}}$, etc.) measurement invariance. Then, the residual covariances were constrained across the response conditions ($rc_{1,h} = rc_{1,r} = rc_{1,f}$, etc.). Afterwards, constraints were imposed on the latent trait variances ($\Phi_{t1,h} = \Phi_{t2,h}$, etc.) and means ($\xi_{t1,h} = \xi_{t2,h}$, etc.).

4.2 Results

Comparability at Selection (t1). The unconstrained models fitted the data well for all three subscales (cf. upper parts of Tables 1-3 in Section 6.3). Imposing the step-by-step constraints across the three response conditions at selection (t1) did not worsen the model fits at any step ($0.083 \leq p(\Delta\chi^2) \leq 0.702$; largest decrease in $CFI = 0.001997$). Therefore, strict measurement invariance, equal latent trait variances, and equal latent trait means at selection (t1) were given across the respondents randomly assigned to the three response conditions at re-test (t2) for all three scales.

Measurement Invariance Across the Measurement Points. Next, constraints were imposed to examine the measurement invariance across the measurement points (cf. lower parts of Tables 1-3 in Section 6.3, and Table 4 in Section 6.3). Weak measurement invariance across applicants and incumbents was given in the honest-condition for all three subscales. By contrast, strong measurement invariance was only given for the scale *preventive health behaviour in response to warning signals*, but not for the other two scales. All affected intercepts were higher at selection (t1) than at re-test (t2). In the next step, the residual variances were examined. The residual variances were equal for two scales, but not all were equal for *preventive health behaviour in response to warning signals*. In this scale, the two affected residual variances were lower at selection (t1) than at re-test (t2).

Latent Variances, Means, and Covariances. For no subscale the latent variances and means were equal across applicants and incumbents in the honest-condition (cf. Tables 4 in Section 6.3). Either, the latent means were equal but the variances were not (*activity in familiar communicative situation*), or the latent variances were equal but the means were not (*preventive health behaviour in response to warning signals*), or both were not equal (*self-confidence in test situation*). If the latent means were affected, they were higher for applicants than for incumbents. If the latent variances were affected, they were lower for applicants than for incumbents. Lastly,

the latent re-test correlations were $r = .497$, $r = .387$, and $r = .533$ for the three subscales, respectively.

4.3 Additional Analysis: Adequacy of Sample Size

The sample size in *Study 3* was rather small per group for measurement invariance analyses. Due to this concern, subsequent Monte Carlo simulation studies were conducted. Monte Carlo simulation studies were carried out for each scale to test the stability of the parameters estimates and thereby the adequacy of the sample size for the analyses. The final models of each subscale were used as the population models for the Monte Carlo simulations, which were carried out with 500 repetitions each.

The Monte Carlo simulation studies verified the stability of the parameters estimates. Only for the scale *activity in familiar communicative situation* there were some small biases: there was a small bias in the SEs of the factor loadings of two items and in the latent means. In spite of the slight bias in the SEs of the factor loadings, the factor loadings were significant in 100% of the simulations, and the estimates themselves were unaffected. Furthermore, the latent means may have been also slightly affected, however latent means of the Monte Carlo simulation exhibited the same pattern as the latent means of the data. A detailed summary of the Monte Carlo simulation studies can be found in Table 1, Table 2, and Table 3 for the scale *activity in familiar communicative situation*, in Table 4, Table 5, and Table 6 for the scale *preventive health behaviour in response to warning signals*, and in Table 7, Table 8, and Table 9 for the scale *self-confidence in test situation*.

4.4 Additional Analysis: Use of Fit-Indices

In the current study, the widely used SRMR was not used to assess the model fit. Evaluating the SRMR in the final models showed values exceeding the usual cut-off of .08 (final models: .107, .141, and .141, respectively). However, studies have suggested that the SRMR tends to be higher with smaller sample sizes (e.g. Chen, 2008; Heene, Hilbert, Draxler, Ziegler, & Bühner, 2011; Meade, Johnson, & Braddy, 2008). Therefore, it would be desirable to assess whether or not the SRMR was detrimentally affected by the sample size before rejecting the models due to the SRMR.

To test the effect of sample size on the SRMR, a second Monte Carlo simulation study was conducted. This Monte Carlo simulation study again assumed the final models as population models. Simulations (500 repetitions each) were carried out with varying sample sizes (81, 120, 250, and 500 per group). For each drawn sample, the respective final model of the respective

Table 1:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *activity in familiar communicative situation* in the honest-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE			Bias	Power
		Estimate	M	SD		SE	M	SD		
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.18935	1.19122	0.14316	-0.00187	0.16116	0.13413	0.01895	0.02704	100.00%
	t1.i3	1.18019	1.18951	0.12949	-0.00931	0.12299	0.11746	0.01765	0.00553	100.00%
	t1.i4	1.12855	1.14093	0.15460	-0.01238	0.24416	0.14491	0.01900	0.09925	100.00%
	t1.i5	1.09307	1.09091	0.12734	0.00216	0.20547	0.12316	0.01685	0.08232	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.18935	1.19122	0.14316	-0.00187	0.16116	0.13413	0.01895	0.02704	100.00%
	t2.i3	1.18019	1.18951	0.12949	-0.00931	0.12299	0.11746	0.01765	0.00553	100.00%
	t2.i4	1.12855	1.14093	0.15460	-0.01238	0.24416	0.14491	0.01900	0.09925	100.00%
	t2.i5	1.09307	1.09091	0.12734	0.00216	0.20547	0.12316	0.01685	0.08232	100.00%
Intercepts	t1.i1	3.87176	3.81294	0.03905	0.05882	0.01615	0.01719	0.00093	-0.00104	100.00%
	t1.i2	3.36841	3.29940	0.05088	0.06902	0.02443	0.02551	0.00156	-0.00109	100.00%
	t1.i3	3.74625	3.67989	0.04663	0.06637	0.02030	0.02075	0.00116	-0.00045	100.00%
	t1.i4	3.03134	2.96568	0.05323	0.06567	0.02885	0.03014	0.00197	-0.00129	100.00%
	t1.i5	3.62861	3.56856	0.04740	0.06005	0.02361	0.02458	0.00145	-0.00097	100.00%
	t2.i1	3.71818	3.65843	0.06076	0.05975	0.04551	0.04258	0.00330	0.00292	100.00%
	t2.i2	3.16707	3.09936	0.08371	0.06772	0.07233	0.06684	0.00503	0.00549	100.00%
	t2.i3	3.56032	3.49126	0.07672	0.06906	0.05553	0.05413	0.00429	0.00140	100.00%
	t2.i4	3.03134	2.96568	0.05323	0.06567	0.02885	0.03014	0.00197	-0.00129	100.00%
	t2.i5	3.44296	3.37873	0.07435	0.06422	0.05774	0.06011	0.00461	-0.00236	100.00%
Residual Variances	t1.i1	0.08980	0.08954	0.00909	0.00026	0.01104	0.00821	0.00078	0.00283	100.00%
	t1.i2	0.28790	0.28555	0.02223	0.00236	0.02923	0.02083	0.00190	0.00840	100.00%
	t1.i3	0.15961	0.15889	0.01380	0.00071	0.01957	0.01330	0.00125	0.00627	100.00%
	t1.i4	0.36963	0.36872	0.02863	0.00091	0.02900	0.02703	0.00268	0.00197	100.00%
	t1.i5	0.23551	0.23424	0.01759	0.00127	0.02498	0.01743	0.00163	0.00756	100.00%
	t2.i1	0.08980	0.08954	0.00909	0.00026	0.01104	0.00821	0.00078	0.00283	100.00%
	t2.i2	0.28790	0.28555	0.02223	0.00236	0.02923	0.02083	0.00190	0.00840	100.00%
	t2.i3	0.15961	0.15889	0.01380	0.00071	0.01957	0.01330	0.00125	0.00627	100.00%
	t2.i4	0.36963	0.36872	0.02863	0.00091	0.02900	0.02703	0.00268	0.00197	100.00%
	t2.i5	0.23551	0.23424	0.01759	0.00127	0.02498	0.01743	0.00163	0.00756	100.00%
Res.-cov.	i1	0.01847	0.01806	0.00789	0.00041	0.00810	0.00762	0.00073	0.00047	66.20%
	i2	0.01619	0.01543	0.02009	0.00077	0.01826	0.02022	0.00190	-0.00196	11.00%
	i3	0.01490	0.01488	0.01295	0.00001	0.01421	0.01250	0.00116	0.00172	23.60%
	i4	0.11582	0.11549	0.02779	0.00034	0.02435	0.02642	0.00272	-0.00207	99.60%
	i5	0.04074	0.04111	0.01666	-0.00036	0.01685	0.01683	0.00161	0.00002	69.20%
Latent	t1 ξ	-0.00920	0.04966	0.03715	-0.05886	0.01527	0.01563	0.00111	-0.00036	70.00%
	t2 ξ	-0.00920	0.04966	0.03715	-0.05886	0.01527	0.01563	0.00111	-0.00036	70.00%
	t1 Φ	0.06032	0.06045	0.00969	-0.00013	0.01711	0.00945	0.00118	0.00765	100.00%
	t2 Φ	0.14534	0.14316	0.03219	0.00219	0.04117	0.03026	0.00655	0.01090	100.00%
	r	0.04651	0.04682	0.01381	-0.00031	0.01619	0.01391	0.00245	0.00229	95.00%

Table 2:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *activity in familiar communicative situation* in the reproduce-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE				Power
		Estimate	M	SD		SE	M	SD	Bias	
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.18935	1.19122	0.14316	-0.00187	0.16116	0.13413	0.01895	0.02704	100.00%
	t1.i3	1.18019	1.18951	0.12949	-0.00931	0.12299	0.11746	0.01765	0.00553	100.00%
	t1.i4	1.12855	1.14093	0.15460	-0.01238	0.24416	0.14491	0.01900	0.09925	100.00%
	t1.i5	1.09307	1.09091	0.12734	0.00216	0.20547	0.12316	0.01685	0.08232	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.18935	1.19122	0.14316	-0.00187	0.16116	0.13413	0.01895	0.02704	100.00%
	t2.i3	1.18019	1.18951	0.12949	-0.00931	0.12299	0.11746	0.01765	0.00553	100.00%
	t2.i4	1.12855	1.14093	0.15460	-0.01238	0.24416	0.14491	0.01900	0.09925	100.00%
	t2.i5	1.09307	1.09091	0.12734	0.00216	0.20547	0.12316	0.01685	0.08232	100.00%
Intercepts	t1.i1	3.87176	3.81294	0.03905	0.05882	0.01615	0.01719	0.00093	-0.00104	100.00%
	t1.i2	3.36841	3.29940	0.05088	0.06902	0.02443	0.02551	0.00156	-0.00109	100.00%
	t1.i3	3.74625	3.67989	0.04663	0.06637	0.02030	0.02075	0.00116	-0.00045	100.00%
	t1.i4	3.03134	2.96568	0.05323	0.06567	0.02885	0.03014	0.00197	-0.00129	100.00%
	t1.i5	3.62861	3.56856	0.04740	0.06005	0.02361	0.02458	0.00145	-0.00097	100.00%
	t2.i1	3.87176	3.81294	0.03905	0.05882	0.01615	0.01719	0.00093	-0.00104	100.00%
	t2.i2	3.36841	3.29940	0.05088	0.06902	0.02443	0.02551	0.00156	-0.00109	100.00%
	t2.i3	3.74625	3.67989	0.04663	0.06637	0.02030	0.02075	0.00116	-0.00045	100.00%
	t2.i4	3.03134	2.96568	0.05323	0.06567	0.02885	0.03014	0.00197	-0.00129	100.00%
	t2.i5	3.62861	3.56856	0.04740	0.06005	0.02361	0.02458	0.00145	-0.00097	100.00%
Residual Variances	t1.i1	0.08980	0.08954	0.00909	0.00026	0.01104	0.00821	0.00078	0.00283	100.00%
	t1.i2	0.28790	0.28555	0.02223	0.00236	0.02923	0.02083	0.00190	0.00840	100.00%
	t1.i3	0.15961	0.15889	0.01380	0.00071	0.01957	0.01330	0.00125	0.00627	100.00%
	t1.i4	0.36963	0.36872	0.02863	0.00091	0.02900	0.02703	0.00268	0.00197	100.00%
	t1.i5	0.23551	0.23424	0.01759	0.00127	0.02498	0.01743	0.00163	0.00756	100.00%
	t2.i1	0.08980	0.08954	0.00909	0.00026	0.01104	0.00821	0.00078	0.00283	100.00%
	t2.i2	0.28790	0.28555	0.02223	0.00236	0.02923	0.02083	0.00190	0.00840	100.00%
	t2.i3	0.15961	0.15889	0.01380	0.00071	0.01957	0.01330	0.00125	0.00627	100.00%
	t2.i4	0.36963	0.36872	0.02863	0.00091	0.02900	0.02703	0.00268	0.00197	100.00%
	t2.i5	0.23551	0.23424	0.01759	0.00127	0.02498	0.01743	0.00163	0.00756	100.00%
Res.-cov.	i1	0.01847	0.01806	0.00789	0.00041	0.00810	0.00762	0.00073	0.00047	66.20%
	i2	0.01619	0.01543	0.02009	0.00077	0.01826	0.02022	0.00190	-0.00196	11.00%
	i3	0.01490	0.01488	0.01295	0.00001	0.01421	0.01250	0.00116	0.00172	23.60%
	i4	0.11582	0.11549	0.02779	0.00034	0.02435	0.02642	0.00272	-0.00207	99.60%
	i5	0.04074	0.04111	0.01666	-0.00036	0.01685	0.01683	0.00161	0.00002	69.20%
Latent	t1 ξ	-0.00920	0.04966	0.03715	-0.05886	0.01527	0.01563	0.00111	-0.00036	70.00%
	t2 ξ	-0.09821	-0.03993	0.03770	-0.05828	0.03205	0.02651	0.00234	0.00554	39.20%
	t1 Φ	0.06032	0.06045	0.00969	-0.00013	0.01711	0.00945	0.00118	0.00765	100.00%
	t2 Φ	0.06032	0.06045	0.00969	-0.00013	0.01711	0.00945	0.00118	0.00765	100.00%
	r	0.05418	0.05433	0.01006	-0.00015	0.01738	0.00972	0.00123	0.00766	100.00%

Table 3:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *activity in familiar communicative situation* in the faking-good-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE			Bias	Power
		Estimate	M	SD		SE	M	SD		
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.18935	1.19122	0.14316	-0.00187	0.16116	0.13413	0.01895	0.02704	100.00%
	t1.i3	1.18019	1.18951	0.12949	-0.00931	0.12299	0.11746	0.01765	0.00553	100.00%
	t1.i4	1.12855	1.14093	0.15460	-0.01238	0.24416	0.14491	0.01900	0.09925	100.00%
	t1.i5	1.09307	1.09091	0.12734	0.00216	0.20547	0.12316	0.01685	0.08232	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.18935	1.19122	0.14316	-0.00187	0.16116	0.13413	0.01895	0.02704	100.00%
	t2.i3	1.18019	1.18951	0.12949	-0.00931	0.12299	0.11746	0.01765	0.00553	100.00%
	t2.i4	1.12855	1.14093	0.15460	-0.01238	0.24416	0.14491	0.01900	0.09925	100.00%
	t2.i5	1.09307	1.09091	0.12734	0.00216	0.20547	0.12316	0.01685	0.08232	100.00%
Intercepts	t1.i1	3.87176	3.81294	0.03905	0.05882	0.01615	0.01719	0.00093	-0.00104	100.00%
	t1.i2	3.36841	3.29940	0.05088	0.06902	0.02443	0.02551	0.00156	-0.00109	100.00%
	t1.i3	3.74625	3.67989	0.04663	0.06637	0.02030	0.02075	0.00116	-0.00045	100.00%
	t1.i4	3.03134	2.96568	0.05323	0.06567	0.02885	0.03014	0.00197	-0.00129	100.00%
	t1.i5	3.62861	3.56856	0.04740	0.06005	0.02361	0.02458	0.00145	-0.00097	100.00%
	t2.i1	3.87176	3.81294	0.03905	0.05882	0.01615	0.01719	0.00093	-0.00104	100.00%
	t2.i2	3.36841	3.29940	0.05088	0.06902	0.02443	0.02551	0.00156	-0.00109	100.00%
	t2.i3	3.74625	3.67989	0.04663	0.06637	0.02030	0.02075	0.00116	-0.00045	100.00%
	t2.i4	3.33632	3.27208	0.07748	0.06424	0.07527	0.06556	0.00501	0.00971	100.00%
	t2.i5	3.62861	3.56856	0.04740	0.06005	0.02361	0.02458	0.00145	-0.00097	100.00%
Residual Variances	t1.i1	0.08980	0.08954	0.00909	0.00026	0.01104	0.00821	0.00078	0.00283	100.00%
	t1.i2	0.28790	0.28555	0.02223	0.00236	0.02923	0.02083	0.00190	0.00840	100.00%
	t1.i3	0.15961	0.15889	0.01380	0.00071	0.01957	0.01330	0.00125	0.00627	100.00%
	t1.i4	0.36963	0.36872	0.02863	0.00091	0.02900	0.02703	0.00268	0.00197	100.00%
	t1.i5	0.23551	0.23424	0.01759	0.00127	0.02498	0.01743	0.00163	0.00756	100.00%
	t2.i1	0.08980	0.08954	0.00909	0.00026	0.01104	0.00821	0.00078	0.00283	100.00%
	t2.i2	0.28790	0.28555	0.02223	0.00236	0.02923	0.02083	0.00190	0.00840	100.00%
	t2.i3	0.15961	0.15889	0.01380	0.00071	0.01957	0.01330	0.00125	0.00627	100.00%
	t2.i4	0.36963	0.36872	0.02863	0.00091	0.02900	0.02703	0.00268	0.00197	100.00%
	t2.i5	0.23551	0.23424	0.01759	0.00127	0.02498	0.01743	0.00163	0.00756	100.00%
Res.-cov.	i1	0.01847	0.01806	0.00789	0.00041	0.00810	0.00762	0.00073	0.00047	66.20%
	i2	0.01619	0.01543	0.02009	0.00077	0.01826	0.02022	0.00190	-0.00196	11.00%
	i3	0.01490	0.01488	0.01295	0.00001	0.01421	0.01250	0.00116	0.00172	23.60%
	i4	0.11582	0.11549	0.02779	0.00034	0.02435	0.02642	0.00272	-0.00207	99.60%
	i5	0.04074	0.04111	0.01666	-0.00036	0.01685	0.01683	0.00161	0.00002	69.20%
Latent	t1 ξ	-0.00920	0.04966	0.03715	-0.05886	0.01527	0.01563	0.00111	-0.00036	70.00%
	t2 ξ	-0.00920	0.04966	0.03715	-0.05886	0.01527	0.01563	0.00111	-0.00036	70.00%
	t1 Φ	0.06032	0.06045	0.00969	-0.00013	0.01711	0.00945	0.00118	0.00765	100.00%
	t2 Φ	0.06032	0.06045	0.00969	-0.00013	0.01711	0.00945	0.00118	0.00765	100.00%
	r	0.00113	0.00052	0.00998	0.00061	0.00729	0.00980	0.00176	-0.00251	6.40%

Study 3: Psychometric Effects of Applicants' Faking Behaviour

Table 4:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *preventive health behaviour in response to warning signals* in the honest-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE				Power
		Estimate	M	SD		SE	M	SD	Bias	
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.31382	1.31377	0.10743	0.00005	0.12431	0.11189	0.01723	0.01242	100.00%
	t1.i3	0.93123	0.93173	0.08318	-0.00049	0.10629	0.08280	0.01309	0.02350	100.00%
	t1.i4	0.59878	0.59923	0.07863	-0.00045	0.10741	0.07698	0.01043	0.03043	100.00%
	t1.i5	1.18871	1.19422	0.10298	-0.00551	0.10682	0.10400	0.01561	0.00282	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.31382	1.31377	0.10743	0.00005	0.12431	0.11189	0.01723	0.00005	100.00%
	t2.i3	0.93123	0.93173	0.08318	0.00049	0.10629	0.08280	0.01309	0.00049	100.00%
	t2.i4	0.59878	0.59923	0.07863	0.00045	0.10741	0.07698	0.01043	0.00045	100.00%
	t2.i5	1.18871	1.19422	0.10298	0.00551	0.10682	0.10400	0.01561	0.00551	100.00%
Intercepts	t1.i1	3.23243	3.19750	0.04392	0.03493	0.02566	0.02571	0.00132	-0.00005	100.00%
	t1.i2	3.57689	3.53352	0.04669	0.04337	0.01960	0.02020	0.00110	-0.00061	100.00%
	t1.i3	3.76497	3.73448	0.03548	0.03049	0.01741	0.01783	0.00089	-0.00042	100.00%
	t1.i4	3.68587	3.66788	0.03454	0.01799	0.02689	0.02667	0.00125	0.00021	100.00%
	t1.i5	3.45386	3.41485	0.04952	0.03901	0.02404	0.02312	0.00127	0.00092	100.00%
	t2.i1	3.23243	3.19750	0.04392	0.03493	0.02566	0.02571	0.00132	-0.00005	100.00%
	t2.i2	3.57689	3.53352	0.04669	0.04337	0.01960	0.02020	0.00110	-0.00061	100.00%
	t2.i3	3.76497	3.73448	0.03548	0.03049	0.01741	0.01783	0.00089	-0.00042	100.00%
	t2.i4	3.68587	3.66788	0.03454	0.01799	0.02689	0.02667	0.00125	0.00021	100.00%
	t2.i5	3.45386	3.41485	0.04952	0.03901	0.02404	0.02312	0.00127	0.00092	100.00%
Residual Variances	t1.i1	0.33396	0.33027	0.02407	0.00369	0.03459	0.02415	0.00232	0.01044	100.00%
	t1.i2	0.23232	0.23072	0.02128	0.00160	0.03366	0.02060	0.00178	0.01306	100.00%
	t1.i3	0.10358	0.10326	0.01185	0.00031	0.01618	0.01113	0.00113	0.00504	100.00%
	t1.i4	0.23769	0.23564	0.01710	0.00205	0.03231	0.01771	0.00168	0.01460	100.00%
	t1.i5	0.20814	0.20656	0.01821	0.00158	0.02679	0.01810	0.00157	0.00868	100.00%
	t2.i1	0.33396	0.33027	0.02407	0.00369	0.03459	0.02415	0.00232	0.01044	100.00%
	t2.i2	0.23232	0.23072	0.02128	0.00160	0.03366	0.02060	0.00178	0.01306	100.00%
	t2.i3	0.23605	0.23584	0.03088	0.00021	0.02468	0.02890	0.00417	-0.00422	100.00%
	t2.i4	0.38574	0.38184	0.06269	0.00390	0.06443	0.05847	0.01223	0.00596	100.00%
	t2.i5	0.20814	0.20656	0.01821	0.00158	0.02679	0.01810	0.00157	0.00868	100.00%
Res.-cov.	i1	0.07630	0.07367	0.02363	0.00263	0.02694	0.02373	0.00221	0.00321	88.80%
	i2	0.04014	0.03848	0.01950	0.00166	0.02361	0.01952	0.00175	0.00410	48.80%
	i3	0.03352	0.03271	0.01106	0.00081	0.01395	0.01078	0.00106	0.00316	88.20%
	i4	0.06629	0.06413	0.01718	0.00216	0.01803	0.01717	0.00154	0.00087	97.20%
	i5	0.03724	0.03632	0.01852	0.00092	0.02124	0.01712	0.00159	0.00412	54.40%
Latent	t1 ξ	0.00485	0.03754	0.03109	-0.03269	0.02176	0.02292	0.00166	-0.00116	39.60%
	t2 ξ	-0.29433	-0.25980	0.03868	-0.03453	0.03817	0.03695	0.00252	0.00122	100.00%
	t1 Φ	0.12546	0.12563	0.02088	-0.00017	0.02414	0.02035	0.00261	0.00379	100.00%
	t2 Φ	0.12546	0.12563	0.02088	-0.00017	0.02414	0.02035	0.00261	0.00379	100.00%
	r	0.04859	0.04872	0.01884	-0.00013	0.02403	0.01816	0.00321	0.00587	79.00%

Table 5:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *preventive health behaviour in response to warning signals* in the reproduce-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE				Power
		Estimate	M	SD		SE	M	SD	Bias	
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.31382	1.31377	0.10743	0.00005	0.12431	0.11189	0.01723	0.01242	100.00%
	t1.i3	0.93123	0.93173	0.08318	-0.00049	0.10629	0.08280	0.01309	0.02350	100.00%
	t1.i4	0.59878	0.59923	0.07863	-0.00045	0.10741	0.07698	0.01043	0.03043	100.00%
	t1.i5	1.18871	1.19422	0.10298	-0.00551	0.10682	0.10400	0.01561	0.00282	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.31382	1.31377	0.10743	0.00005	0.12431	0.11189	0.01723	0.01242	100.00%
	t2.i3	0.93123	0.93173	0.08318	-0.00049	0.10629	0.08280	0.01309	0.02350	100.00%
	t2.i4	0.59878	0.59923	0.07863	-0.00045	0.10741	0.07698	0.01043	0.03043	100.00%
	t2.i5	1.18871	1.19422	0.10298	-0.00551	0.10682	0.10400	0.01561	0.00282	100.00%
Intercepts	t1.i1	3.23243	3.19750	0.04392	0.03493	0.02566	0.02571	0.00132	-0.00005	100.00%
	t1.i2	3.57689	3.53352	0.04669	0.04337	0.01960	0.02020	0.00110	-0.00061	100.00%
	t1.i3	3.76497	3.73448	0.03548	0.03049	0.01741	0.01783	0.00089	-0.00042	100.00%
	t1.i4	3.68587	3.66788	0.03454	0.01799	0.02689	0.02667	0.00125	0.00021	100.00%
	t1.i5	3.45386	3.41485	0.04952	0.03901	0.02404	0.02312	0.00127	0.00092	100.00%
	t2.i1	3.23243	3.19750	0.04392	0.03493	0.02566	0.02571	0.00132	-0.00005	100.00%
	t2.i2	3.57689	3.53352	0.04669	0.04337	0.01960	0.02020	0.00110	-0.00061	100.00%
	t2.i3	3.76497	3.73448	0.03548	0.03049	0.01741	0.01783	0.00089	-0.00042	100.00%
	t2.i4	3.43310	3.41519	0.05902	0.01791	0.05799	0.05587	0.00450	0.00212	100.00%
	t2.i5	3.45386	3.41485	0.04952	0.03901	0.02404	0.02312	0.00127	0.00092	100.00%
Residual Variances	t1.i1	0.33396	0.33027	0.02407	0.00369	0.03459	0.02415	0.00232	0.01044	100.00%
	t1.i2	0.23232	0.23072	0.02128	0.00160	0.03366	0.02060	0.00178	0.01306	100.00%
	t1.i3	0.10358	0.10326	0.01185	0.00031	0.01618	0.01113	0.00113	0.00504	100.00%
	t1.i4	0.23769	0.23564	0.01710	0.00205	0.03231	0.01771	0.00168	0.01460	100.00%
	t1.i5	0.20814	0.20656	0.01821	0.00158	0.02679	0.01810	0.00157	0.00868	100.00%
	t2.i1	0.33396	0.33027	0.02407	0.00369	0.03459	0.02415	0.00232	0.01044	100.00%
	t2.i2	0.23232	0.23072	0.02128	0.00160	0.03366	0.02060	0.00178	0.01306	100.00%
	t2.i3	0.23605	0.23584	0.03088	0.00021	0.02468	0.02890	0.00417	-0.00422	100.00%
	t2.i4	0.23769	0.23564	0.01710	0.00205	0.03231	0.01771	0.00168	0.01460	100.00%
	t2.i5	0.20814	0.20656	0.01821	0.00158	0.02679	0.01810	0.00157	0.00868	100.00%
Res.-cov.	i1	0.07630	0.07367	0.02363	0.00263	0.02694	0.02373	0.00221	0.00321	88.80%
	i2	0.04014	0.03848	0.01950	0.00166	0.02361	0.01952	0.00175	0.00410	48.80%
	i3	0.03352	0.03271	0.01106	0.00081	0.01395	0.01078	0.00106	0.00316	88.20%
	i4	0.06629	0.06413	0.01718	0.00216	0.01803	0.01717	0.00154	0.00087	97.20%
	i5	0.03724	0.03632	0.01852	0.00092	0.02124	0.01712	0.00159	0.00412	54.40%
Latent	t1 ξ	0.00485	0.03754	0.03109	-0.03269	0.02176	0.02292	0.00166	-0.00116	39.60%
	t2 ξ	-0.29433	-0.25980	0.03868	-0.03453	0.03817	0.03695	0.00252	0.00122	100.00%
	t1 Φ	0.12546	0.12563	0.02088	-0.00017	0.02414	0.02035	0.00261	0.00379	100.00%
	t2 Φ	0.27954	0.28361	0.06074	-0.00408	0.05993	0.06003	0.01211	-0.00009	100.00%
	r	0.13089	0.13312	0.02808	-0.00223	0.03316	0.02826	0.00443	0.00491	100.00%

Table 6:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *preventive health behaviour in response to warning signals* in the faking-good-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE				Power
		Estimate	M	SD		SE	M	SD	Bias	
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.31382	1.31377	0.10743	0.00005	0.12431	0.11189	0.01723	0.01242	100.00%
	t1.i3	0.93123	0.93173	0.08318	-0.00049	0.10629	0.08280	0.01309	0.02350	100.00%
	t1.i4	0.59878	0.59923	0.07863	-0.00045	0.10741	0.07698	0.01043	0.03043	100.00%
	t1.i5	1.18871	1.19422	0.10298	-0.00551	0.10682	0.10400	0.01561	0.00282	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.31382	1.31377	0.10743	0.00005	0.12431	0.11189	0.01723	0.01242	100.00%
	t2.i3	0.93123	0.93173	0.08318	-0.00049	0.10629	0.08280	0.01309	0.02350	100.00%
	t2.i4	0.59878	0.59923	0.07863	-0.00045	0.10741	0.07698	0.01043	0.03043	100.00%
	t2.i5	1.18871	1.19422	0.10298	-0.00551	0.10682	0.10400	0.01561	0.00282	100.00%
Intercepts	t1.i1	3.23243	3.19750	0.04392	0.03493	0.02566	0.02571	0.00132	-0.00005	100.00%
	t1.i2	3.57689	3.53352	0.04669	0.04337	0.01960	0.02020	0.00110	-0.00061	100.00%
	t1.i3	3.76497	3.73448	0.03548	0.03049	0.01741	0.01783	0.00089	-0.00042	100.00%
	t1.i4	3.68587	3.66788	0.03454	0.01799	0.02689	0.02667	0.00125	0.00021	100.00%
	t1.i5	3.45386	3.41485	0.04952	0.03901	0.02404	0.02312	0.00127	0.00092	100.00%
	t2.i1	3.23243	3.19750	0.04392	0.03493	0.02566	0.02571	0.00132	-0.00005	100.00%
	t2.i2	3.57689	3.53352	0.04669	0.04337	0.01960	0.02020	0.00110	-0.00061	100.00%
	t2.i3	3.76497	3.73448	0.03548	0.03049	0.01741	0.01783	0.00089	-0.00042	100.00%
	t2.i4	3.68587	3.66788	0.03454	0.01799	0.02689	0.02667	0.00125	0.00021	100.00%
	t2.i5	3.61488	3.57963	0.06793	0.03525	0.05624	0.05643	0.00428	-0.00019	100.00%
Residual Variances	t1.i1	0.33396	0.33027	0.02407	0.00369	0.03459	0.02415	0.00232	0.01044	100.00%
	t1.i2	0.23232	0.23072	0.02128	0.00160	0.03366	0.02060	0.00178	0.01306	100.00%
	t1.i3	0.10358	0.10326	0.01185	0.00031	0.01618	0.01113	0.00113	0.00504	100.00%
	t1.i4	0.23769	0.23564	0.01710	0.00205	0.03231	0.01771	0.00168	0.01460	100.00%
	t1.i5	0.20814	0.20656	0.01821	0.00158	0.02679	0.01810	0.00157	0.00868	100.00%
	t2.i1	0.33396	0.33027	0.02407	0.00369	0.03459	0.02415	0.00232	0.01044	100.00%
	t2.i2	0.23232	0.23072	0.02128	0.00160	0.03366	0.02060	0.00178	0.01306	100.00%
	t2.i3	0.10358	0.10326	0.01185	0.00031	0.01618	0.01113	0.00113	0.00504	100.00%
	t2.i4	0.23769	0.23564	0.01710	0.00205	0.03231	0.01771	0.00168	0.01460	100.00%
	t2.i5	0.20814	0.20656	0.01821	0.00158	0.02679	0.01810	0.00157	0.00868	100.00%
Res.-cov.	i1	0.07630	0.07367	0.02363	0.00263	0.02694	0.02373	0.00221	0.00321	88.80%
	i2	0.04014	0.03848	0.01950	0.00166	0.02361	0.01952	0.00175	0.00410	48.80%
	i3	0.03352	0.03271	0.01106	0.00081	0.01395	0.01078	0.00106	0.00316	88.20%
	i4	0.06629	0.06413	0.01718	0.00216	0.01803	0.01717	0.00154	0.00087	97.20%
	i5	0.03724	0.03632	0.01852	0.00092	0.02124	0.01712	0.00159	0.00412	54.40%
Latent	t1 ξ	0.00485	0.03754	0.03109	-0.03269	0.02176	0.02292	0.00166	-0.00116	39.60%
	t2 ξ	0.16078	0.19554	0.04285	-0.03477	0.05009	0.04439	0.00427	0.00570	99.80%
	t1 Φ	0.12546	0.12563	0.02088	-0.00017	0.02414	0.02035	0.00261	0.00379	100.00%
	t2 Φ	0.12546	0.12563	0.02088	-0.00017	0.02414	0.02035	0.00261	0.00379	100.00%
	r	0.04960	0.05000	0.01813	-0.00040	0.01975	0.01755	0.00303	0.00220	82.80%

Table 7:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *self-confidence in test situation* in the honest-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE				Power
		Estimate	M	SD		SE	M	SD	Bias	
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.12711	1.12940	0.05648	-0.00229	0.05632	0.05622	0.00519	0.00010	100.00%
	t1.i3	1.04328	1.04417	0.07201	-0.00090	0.06807	0.06963	0.00636	-0.00156	100.00%
	t1.i4	0.78068	0.78300	0.05294	-0.00232	0.05091	0.04774	0.00416	0.00317	100.00%
	t1.i5	0.79442	0.79364	0.06397	0.00078	0.05484	0.06335	0.00636	-0.00851	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.12711	1.12940	0.05648	-0.00229	0.05632	0.05622	0.00519	0.00010	100.00%
	t2.i3	1.04328	1.04417	0.07201	-0.00090	0.06807	0.06963	0.00636	-0.00156	100.00%
	t2.i4	0.78068	0.78300	0.05294	-0.00232	0.05091	0.04774	0.00416	0.00317	100.00%
	t2.i5	0.79442	0.79364	0.06397	0.00078	0.05484	0.06335	0.00636	-0.00851	100.00%
Intercepts	t1.i1	3.98927	3.99129	0.09231	-0.00202	0.07452	0.08796	0.02891	-0.01344	100.00%
	t1.i2	3.95941	3.96224	0.10338	-0.00284	0.08166	0.09999	0.03259	-0.01833	100.00%
	t1.i3	3.72510	3.72720	0.10942	-0.00210	0.10155	0.11082	0.03077	-0.00927	100.00%
	t1.i4	3.93596	3.93576	0.07321	0.00020	0.05958	0.07040	0.02225	-0.01082	100.00%
	t1.i5	4.00351	4.00752	0.11853	-0.00401	0.09644	0.11348	0.03148	-0.01705	100.00%
	t2.i1	3.98927	3.99129	0.09231	-0.00202	0.07452	0.08796	0.02891	-0.01344	100.00%
	t2.i2	3.95941	3.96224	0.10338	-0.00284	0.08166	0.09999	0.03259	-0.01833	100.00%
	t2.i3	3.72510	3.72720	0.10942	-0.00210	0.10155	0.11082	0.03077	-0.00927	100.00%
	t2.i4	3.93596	3.93576	0.07321	0.00020	0.05958	0.07040	0.02225	-0.01082	100.00%
	t2.i5	3.83657	3.83971	0.11926	-0.00314	0.10488	0.11370	0.03168	-0.00882	100.00%
Residual Variances	t1.i1	0.17638	0.17580	0.01653	0.00058	0.01709	0.01635	0.00155	0.00074	100.00%
	t1.i2	0.23764	0.23692	0.02164	0.00072	0.02171	0.02160	0.00201	0.00010	100.00%
	t1.i3	0.27983	0.27791	0.02379	0.00192	0.02506	0.02361	0.00226	0.00145	100.00%
	t1.i4	0.27216	0.27052	0.02099	0.00164	0.02723	0.02072	0.00191	0.00651	100.00%
	t1.i5	0.22238	0.22128	0.01899	0.00109	0.02189	0.01776	0.00177	0.00413	100.00%
	t2.i1	0.17638	0.17580	0.01653	0.00058	0.01709	0.01635	0.00155	0.00074	100.00%
	t2.i2	0.23764	0.23692	0.02164	0.00072	0.02171	0.02160	0.00201	0.00010	100.00%
	t2.i3	0.27983	0.27791	0.02379	0.00192	0.02506	0.02361	0.00226	0.00145	100.00%
	t2.i4	0.27216	0.27052	0.02099	0.00164	0.02723	0.02072	0.00191	0.00651	100.00%
	t2.i5	0.22238	0.22128	0.01899	0.00109	0.02189	0.01776	0.00177	0.00413	100.00%
Res.-cov.	i1	0.00083	0.00116	0.01011	-0.00033	0.00772	0.00970	0.00105	-0.00198	6.80%
	i2	0.02003	0.01912	0.01463	0.00090	0.01306	0.01372	0.00157	-0.00066	30.00%
	i3	0.04185	0.04073	0.01719	0.00112	0.02099	0.01730	0.00167	0.00369	66.80%
	i4	0.02354	0.02350	0.01237	0.00003	0.01512	0.01219	0.00135	0.00294	46.80%
	i5	0.01786	0.01807	0.00949	-0.00021	0.00974	0.00901	0.00112	0.00073	50.20%
Latent	t1 ξ	-0.56906	-0.57280	0.10171	0.00374	0.08634	0.09758	0.02795	-0.01124	100.00%
	t2 ξ	-0.82012	-0.82327	0.11419	0.00314	0.11005	0.11436	0.02524	-0.00431	100.00%
	t1 Φ	0.19298	0.19302	0.02353	-0.00004	0.02416	0.02280	0.00282	0.00136	100.00%
	t2 Φ	0.40326	0.39821	0.07160	0.00504	0.05901	0.07193	0.01491	-0.01292	100.00%
	r	0.14863	0.14975	0.03800	-0.00112	0.03283	0.03550	0.00573	-0.00268	98.40%

Table 8:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *self-confidence in test situation* in the reproduce-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE				Power
		Estimate	M	SD		SE	M	SD	Bias	
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.12711	1.12940	0.05648	-0.00229	0.05632	0.05622	0.00519	0.00010	100.00%
	t1.i3	1.04328	1.04417	0.07201	-0.00090	0.06807	0.06963	0.00636	-0.00156	100.00%
	t1.i4	0.78068	0.78300	0.05294	-0.00232	0.05091	0.04774	0.00416	0.00317	100.00%
	t1.i5	0.79442	0.79364	0.06397	0.00078	0.05484	0.06335	0.00636	-0.00851	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.12711	1.12940	0.05648	-0.00229	0.05632	0.05622	0.00519	0.00010	100.00%
	t2.i3	1.04328	1.04417	0.07201	-0.00090	0.06807	0.06963	0.00636	-0.00156	100.00%
	t2.i4	0.78068	0.78300	0.05294	-0.00232	0.05091	0.04774	0.00416	0.00317	100.00%
	t2.i5	0.79442	0.79364	0.06397	0.00078	0.05484	0.06335	0.00636	-0.00851	100.00%
Intercepts	t1.i1	3.98927	3.99129	0.09231	-0.00202	0.07452	0.08796	0.02891	-0.01344	100.00%
	t1.i2	3.95941	3.96224	0.10338	-0.00284	0.08166	0.09999	0.03259	-0.01833	100.00%
	t1.i3	3.72510	3.72720	0.10942	-0.00210	0.10155	0.11082	0.03077	-0.00927	100.00%
	t1.i4	3.93596	3.93576	0.07321	0.00020	0.05958	0.07040	0.02225	-0.01082	100.00%
	t1.i5	4.00351	4.00752	0.11853	-0.00401	0.09644	0.11348	0.03148	-0.01705	100.00%
	t2.i1	3.98927	3.99129	0.09231	-0.00202	0.07452	0.08796	0.02891	-0.01344	100.00%
	t2.i2	3.95941	3.96224	0.10338	-0.00284	0.08166	0.09999	0.03259	-0.01833	100.00%
	t2.i3	3.72510	3.72720	0.10942	-0.00210	0.10155	0.11082	0.03077	-0.00927	100.00%
	t2.i4	3.93596	3.93576	0.07321	0.00020	0.05958	0.07040	0.02225	-0.01082	100.00%
	t2.i5	3.83657	3.83971	0.11926	-0.00314	0.10488	0.11370	0.03168	-0.00882	100.00%
Residual Variances	t1.i1	0.17638	0.17580	0.01653	0.00058	0.01709	0.01635	0.00155	0.00074	100.00%
	t1.i2	0.23764	0.23692	0.02164	0.00072	0.02171	0.02160	0.00201	0.00010	100.00%
	t1.i3	0.27983	0.27791	0.02379	0.00192	0.02506	0.02361	0.00226	0.00145	100.00%
	t1.i4	0.27216	0.27052	0.02099	0.00164	0.02723	0.02072	0.00191	0.00651	100.00%
	t1.i5	0.22238	0.22128	0.01899	0.00109	0.02189	0.01776	0.00177	0.00413	100.00%
	t2.i1	0.17638	0.17580	0.01653	0.00058	0.01709	0.01635	0.00155	0.00074	100.00%
	t2.i2	0.23764	0.23692	0.02164	0.00072	0.02171	0.02160	0.00201	0.00010	100.00%
	t2.i3	0.27983	0.27791	0.02379	0.00192	0.02506	0.02361	0.00226	0.00145	100.00%
	t2.i4	0.27216	0.27052	0.02099	0.00164	0.02723	0.02072	0.00191	0.00651	100.00%
	t2.i5	0.22238	0.22128	0.01899	0.00109	0.02189	0.01776	0.00177	0.00413	100.00%
Res.-cov.	i1	0.00083	0.00116	0.01011	-0.00033	0.00772	0.00970	0.00105	-0.00198	6.80%
	i2	0.02003	0.01912	0.01463	0.00090	0.01306	0.01372	0.00157	-0.00066	30.00%
	i3	0.04185	0.04073	0.01719	0.00112	0.02099	0.01730	0.00167	0.00369	66.80%
	i4	0.02354	0.02350	0.01237	0.00003	0.01512	0.01219	0.00135	0.00294	46.80%
	i5	0.01786	0.01807	0.00949	-0.00021	0.00974	0.00901	0.00112	0.00073	50.20%
Latent	t1 ξ	-0.56906	-0.57280	0.10171	0.00374	0.08634	0.09758	0.02795	-0.01124	100.00%
	t2 ξ	-0.68493	-0.68707	0.10662	0.00214	0.09935	0.10504	0.02656	-0.00569	100.00%
	t1 Φ	0.19298	0.19302	0.02353	-0.00004	0.02416	0.02280	0.00282	0.00136	100.00%
	t2 Φ	0.19298	0.19302	0.02353	-0.00004	0.02416	0.02280	0.00282	0.00136	100.00%
	r	0.08534	0.08445	0.02601	0.00089	0.02575	0.02511	0.00372	0.00065	93.00%

Table 9:

Comparison of the parameter estimates (and SEs) and the parameter estimates of the Monte Carlo simulation study (and SEs), the magnitude of the resulting absolute bias, and percentage of significant parameter estimates for the scale *self-confidence in test situation* in the faking-good-condition.

		Parameter Estimate			Bias	Parameter Estimate's SE				Power
		Estimate	M	SD		SE	M	SD	Bias	
Factor loadings	t1.i1	1	-	-	-	-	-	-	-	-
	t1.i2	1.12711	1.12940	0.05648	-0.00229	0.05632	0.05622	0.00519	0.00010	100.00%
	t1.i3	1.04328	1.04417	0.07201	-0.00090	0.06807	0.06963	0.00636	-0.00156	100.00%
	t1.i4	0.78068	0.78300	0.05294	-0.00232	0.05091	0.04774	0.00416	0.00317	100.00%
	t1.i5	0.79442	0.79364	0.06397	0.00078	0.05484	0.06335	0.00636	-0.00851	100.00%
	t2.i1	1	-	-	-	-	-	-	-	-
	t2.i2	1.12711	1.12940	0.05648	-0.00229	0.05632	0.05622	0.00519	0.00010	100.00%
	t2.i3	1.04328	1.04417	0.07201	-0.00090	0.06807	0.06963	0.00636	-0.00156	100.00%
	t2.i4	0.78068	0.78300	0.05294	-0.00232	0.05091	0.04774	0.00416	0.00317	100.00%
	t2.i5	1.43143	1.44374	0.12409	-0.01231	0.12817	0.11788	0.02602	0.01029	100.00%
Intercepts	t1.i1	3.98927	3.99129	0.09231	-0.00202	0.07452	0.08796	0.02891	-0.01344	100.00%
	t1.i2	3.95941	3.96224	0.10338	-0.00284	0.08166	0.09999	0.03259	-0.01833	100.00%
	t1.i3	3.72510	3.72720	0.10942	-0.00210	0.10155	0.11082	0.03077	-0.00927	100.00%
	t1.i4	3.93596	3.93576	0.07321	0.00020	0.05958	0.07040	0.02225	-0.01082	100.00%
	t1.i5	4.00351	4.00752	0.11853	-0.00401	0.09644	0.11348	0.03148	-0.01705	100.00%
	t2.i1	3.98927	3.99129	0.09231	-0.00202	0.07452	0.08796	0.02891	-0.01344	100.00%
	t2.i2	3.95941	3.96224	0.10338	-0.00284	0.08166	0.09999	0.03259	-0.01833	100.00%
	t2.i3	3.88115	3.88505	0.09766	-0.00390	0.08189	0.09489	0.03000	-0.01301	100.00%
	t2.i4	3.93596	3.93576	0.07321	0.00020	0.05958	0.07040	0.02225	-0.01082	100.00%
	t2.i5	4.00351	4.00752	0.11853	-0.00401	0.09644	0.11348	0.03148	-0.01705	100.00%
Residual Variances	t1.i1	0.17638	0.17580	0.01653	0.00058	0.01709	0.01635	0.00155	0.00074	100.00%
	t1.i2	0.23764	0.23692	0.02164	0.00072	0.02171	0.02160	0.00201	0.00010	100.00%
	t1.i3	0.27983	0.27791	0.02379	0.00192	0.02506	0.02361	0.00226	0.00145	100.00%
	t1.i4	0.27216	0.27052	0.02099	0.00164	0.02723	0.02072	0.00191	0.00651	100.00%
	t1.i5	0.22238	0.22128	0.01899	0.00109	0.02189	0.01776	0.00177	0.00413	100.00%
	t2.i1	0.04817	0.04764	0.00875	0.00053	0.02011	0.00823	0.00158	0.01188	100.00%
	t2.i2	0.07670	0.07601	0.01317	0.00069	0.02586	0.01249	0.00241	0.01337	100.00%
	t2.i3	0.13732	0.13490	0.02128	0.00241	0.04768	0.02071	0.00401	0.02697	100.00%
	t2.i4	0.05997	0.05866	0.00950	0.00131	0.02139	0.00928	0.00179	0.01210	100.00%
	t2.i5	0.00691	0.00631	0.00788	0.00060	0.00758	0.00733	0.00127	0.00026	17.40%
Res.-cov.	i1	0.00083	0.00116	0.01011	-0.00033	0.00772	0.00970	0.00105	-0.00198	6.80%
	i2	0.02003	0.01912	0.01463	0.00090	0.01306	0.01372	0.00157	-0.00066	30.00%
	i3	0.04185	0.04073	0.01719	0.00112	0.02099	0.01730	0.00167	0.00369	66.80%
	i4	0.02354	0.02350	0.01237	0.00003	0.01512	0.01219	0.00135	0.00294	46.80%
	i5	0.01786	0.01807	0.00949	-0.00021	0.00974	0.00901	0.00112	0.00073	50.20%
Latent	t1 ξ	-0.56906	-0.57280	0.10171	0.00374	0.08634	0.09758	0.02795	-0.01124	100.00%
	t2 ξ	-0.05996	-0.06054	0.09039	0.00058	0.06904	0.08633	0.02898	-0.01729	5.60%
	t1 Φ	0.19298	0.19302	0.02353	-0.00004	0.02416	0.02280	0.00282	0.00136	100.00%
	t2 Φ	0.06936	0.06820	0.01449	0.00117	0.05677	0.01369	0.00281	0.04308	100.00%
	r	0.01396	0.01356	0.01470	0.00041	0.01422	0.01420	0.00251	0.00002	17.40%

scale was fitted, and CFI, RMSEA, and SRMR computed. In these simulations, the fit indices should accept the model, and have only a nominal Type I error.

Figure 4 shows the average fit indices for all three scales. The average CFI increased slightly and the average RMSEA decreased slightly with increasing sample size. More importantly, already with the smallest sample size, both the average CFI and the average RMSEA correctly accepted the models, and were only marginally influenced by the sample size. However, the results were fundamentally different for the SRMR. The average SRMR exceeded its cut-off already with the smallest sample sizes; only with the larger sample sizes the average SRMR began to correctly accept the models.

Figure 5, Figure 6, and Figure 7 complement Figure 4, by showing the percentage of correctly accepted models by the CFI, RMSEA, and SRMR with their usual cut-offs, respectively. The Type I error of the RMSEA seemed the least affected by the sample size. The CFI showed an increase in the percentage of correctly accepted models, while still being adequately high even with the smallest sample size. In sharp contrast, the percentage of correctly accepted models by the SRMR was heavily affected by the sample size: with the lowest sample size, the SRMR almost never (0% – 1%) fell below its cut-off of .08.

To summarize, the SRMR was heavily biased in evaluating the model fits by the sample size. This effect of the sample size yielded Type I error rates of up to 100%, consequently invalidating the use of the SRMR for *Study 3*.

4.5 Discussion

Study 3 suggests that applicants consistently increase their scores on personality scale items to present themselves more favourably than they normally would. This was done primarily by increasing responses to selected items. If this was done homogeneously for all items, strong measurement invariance was preserved, and an increase in the latent mean resulted (cf. *preventive health behaviour in response to warning signals*). If this was done homogeneously on all but a few items, strong measurement invariance was partially given, and additionally an increase in the latent mean resulted (cf. *self-confidence in test situation*). Lastly, if this was done heterogeneously for nearly all items, strong measurement invariance was not given, and the latent means did not differ (cf. *activity in familiar communicative situation*). In summary, applicants' will increase their scores on selected items, and the extent thereof may vary from item to item. The more homogeneously this is done, the more this increase will not be distinguishable from a true difference.

Figure 4: Comparison of the influence of sample size per group on the average CFI, RMSEA, and SRMR.

Figure 5: Percentage of correctly accepted models by $CFI \geq .95$, depending on varying sample sizes.

Figure 6: Percentage of correctly accepted models by $RMSEA < .06$, depending on varying sample sizes.

Figure 7: Percentage of correctly accepted models by $SRMR < .08$, depending on varying sample sizes.

By contrast, the more heterogeneously this is done, the more measurement bias will be introduced.

Applicants' faking behaviour consistently affected at least some psychometric properties of the personality scales. This was contrary to the hypothesis that strict measurement invariance should hold across applicants and incumbents in the honest-condition (cf. Robie et al., 2001). On the one hand, this hypothesis was based on a single study, as there is a paucity of studies examining measurement invariance across applicants and incumbents. On the other hand, there was a key difference between Robie et al. (2001) and the current study. Robie et al. presented a warning in their study, that "distorted self-descriptions would invalidate the ... test results" (p. 195). In the current study, no such warning was presented. This may have caused differences in applicants' willingness to fake between the two studies. Prior research indicates that applicants wrongly fear that their faking behaviour may be exposable (König et al., 2012). Furthermore, studies have shown that such warnings can reduce faking when negative consequences of being exposed are emphasized (Dwight & Donovan, 2003; Goffin & Woods, 1995).

The violation of strong measurement invariance in two out of three scales cautions against interpreting raw mean differences of applicants and incumbents at face value (cf. Chen, 2008; Cheung & Rensvold, 2002; Li & Zumbo, 2009; Millsap, 2011). For *preventive health behaviour in response to warning signals* strong measurement invariance was given. Thus, the latent mean difference between applicants and incumbents corroborates the meta-analytic results derived from raw mean differences (cf. Birkeland et al., 2006). For the other two scales, however, the raw mean differences can be at least partially attributed to uniform measurement bias. Consequently, the raw mean score comparisons of e.g. Birkeland et al. (2006) or *Study 2* (cf. Krammer & Pflanzl, 2015) should be questioned.

However, the results of *Study 3* support that the detrimental effects of applicants' faking behaviour should not be overestimated (cf. Bradley & Hauenstein, 2006; Hogan et al., 2007; Ones et al., 2007). While applicants' faking behaviour consistently affected measurement properties, some measurement properties were also consistently preserved. The consistent preservation of weak measurement invariance implies that validity coefficients from samples of incumbents may be applicable for applicants (cf. Chen, 2008). These findings support studies showing comparable predictive validity coefficients across honest responses and selection settings (for an overview cf. Ones et al., 2007).

Finally, applicants' faking behaviour distorted the rank-orders to a modest extent. While this distortion was in line with prior studies (cf. Griffin & Wilson, 2012; Griffith et al., 2007; Peterson et al., 2011), it corroborates

that applicants' faking behaviour is an individual differences construct (cf. Goffin & Boyd, 2009; Marcus, 2009; McFarland & Ryan, 2000, 2006; Mueller-Hanson et al., 2006; Snell et al., 1999). Therefore, the effects of applicants' faking behaviour on selection decisions should not be taken too lightly.

5 General Discussion

The aim of this publication-based dissertation was to address three questions, which should be addressed for college admission processes in specific academic disciplines:

1. Should well established determinants of academic achievement be used, such as general mental ability and personality traits?
2. If personality traits are used, do applicants fake?
3. If applicants fake, does applicants' faking behaviour detrimentally affect the psychometric properties of the personality measures?

These three questions were addressed in three subsequent studies.

Study 1 (cf. Krammer et al., 2016) addressed the modelling of academic achievement. Studies have suggested that it may be useful to differentiate academic achievement measured by GPAs according to different courses types (e.g. D'Agostino & Powers, 2009; Gustafsson & Balke, 1993; Reeve, 2004). *Study 1* showed that such a differentiation may prove especially useful for academic fields for which the education is of a highly applied nature, i.e. teacher education. This differentiation emphasizes the importance of taking into account if there are elements unique to the education in an academic field. If there are, aggregating GPAs and modelling of academic achievement should reflect such uniquenesses.

Furthermore, *Study 1* examined the predictive utilities of cognitive and non-cognitive determinants of academic achievement. In line with previous studies (e.g. Credé & Kuncel, 2008; Hell et al., 2007; Kuncel et al., 2001, 2004, 2010; Kuncel & Hezlett, 2007), the predictive utility of general mental ability could be shown for academic achievement in teacher education. Also in line with previous studies (e.g. Farsides & Woodfield, 2003; Poropat, 2009), non-cognitive determinants exhibited predictive utility over and above general mental ability. This importance of non-cognitive determinants suggests that in academic fields where the education is of an applied nature, determinants of prospective occupational success can already play an important role in the education. Consequently, it is worth considering not to base selection decisions for educations to these academic fields only on

determinants of academic achievement, but additionally on determinants of occupational success.

With *Study 1* corroborating the predictive utilities of non-cognitive determinants – i.e. personality traits – of academic achievement, *Study 2* (cf. Krammer & Pflanzl, 2015) examined the prevalence of applicants' faking behaviour on personality scales used for selection decisions. While respondents are highly able to distort their scores on personality inventories (cf. Viswesvaran & Ones, 1999), applicants do so only to a negligible to small degree (cf. Birkeland et al., 2006). Furthermore, scholars have pointed out that the detrimental effects of applicants' faking behaviour may be negligible (e.g. Bradley & Hauenstein, 2006; Hogan et al., 2007; Ones et al., 2007). In contrast, *Study 2* showed that applicants increased their mean raw scores in nearly all examined personality scales. Moreover, applicants' faking behaviour affected the rank orders of applicants in line with previous studies (cf. Mueller-Hanson et al., 2003; Rosse et al., 1998; Winkelspecht et al., 2006). In addition to showing the prevalence of applicants' faking behaviour and its effects, *Study 2* also underlined that applicants' faking behaviour varied between applicants and scales. These differences between applicants and scales pose a challenge for the fairness of selection decisions.

Based on the prevalence of faking on personality scales of *Study 2*, *Study 3* (cf. Krammer et al., in press) went beyond the manifest means and correlations of *Study 2*, and examined the psychometric effects of applicants' faking behaviour. This was done by examining the measurement invariance across respondents responding to the same personality scales twice, once while still being applicants and later as incumbents. Such a within-subject design with a real life selection setting has been argued to constitute a golden standard in faking research (cf. Komar, Brown, Komar, & Robie, 2008; Ryan & Boyce, 2006; Peterson et al., 2011). Unfortunately, there is a paucity of studies combining such a within-subject design with the rigor of measurement invariance analyses.

Study 3 showed that applicants' faking behaviour consistently affected at least some of the psychometric properties of the scrutinized personality scales. The psychometric properties were mainly affected due to applicants altering their baseline probabilities of endorsing specific response categories of items. The more heterogeneously applicants increased these baseline probabilities across items of a scale, the more severe measurement properties were detrimentally affected. By contrast, applicants could also present themselves as an idealized version of themselves, by increased their baseline probabilities across items of a scale homogeneously. These results caution against treating applicants' faking behaviour as a one-size-fits-all process. Therefore, future studies should try to shed more light on the process of applicants'

faking behaviour. Understanding the process of applicants' faking behaviour is especially important in the light of the fairness of subsequent selection decisions.

Remedying the consistently affected psychometric properties of the personality scales in *Study 3*, *Study 3* also showed that some psychometric properties were consistently preserved. Most importantly, configural and weak measurement invariance were preserved. Weak measurement invariance is needed to compare correlations within one response condition to correlations within another response condition (Chen, 2008). Therefore, the lack of an effect on weak measurement invariance suggests that the predictive utilities of the personality scales may be preserved. This preservation of the predictive utilities is in line with previous studies, and has been used as the basis for arguing that applicants' faking behaviour may be deemed negligible in real life selection settings (for an overview cf. Ones et al., 2007).

In conclusion, yes, personality measures can and should be used for selection decisions. However, applicants' faking behaviour should not be neglected, and the process further studied. While the detrimental effects of applicants' faking behaviour seem to be limited, applicants' faking behaviour may nevertheless vary between measures *and* applicants.

6 Full Texts & Dissemination

Three publications formed the basis of this publication-based dissertation. These three publications are:

- Krammer, G., Sommer, M., & Arendasy, M. E. (2016). Realistic job expectations predict academic achievement. *Learning and Individual Differences*, 51, 341-348. doi: 10.1016/j.lindif.2016.09.010.
- Krammer, G., & Pflanzl, B. (2015). Faking von Persönlichkeitseigenschaften bei Zulassungsverfahren für Lehramtsstudien [Faking of Personality Measures for College Admission in Teacher Education]. *Zeitschrift für Pädagogische Psychologie*, 29, 205-214. doi: 10.1024/1010-0652/a000161.
- Krammer, G., Sommer, M., & Arendasy, M. E. (in press). The psychometric costs of applicants' faking: examining measurement invariance and re-test correlations across response conditions. *Journal of Personality Assessment*. doi: 10.1080/00223891.2017.1285781

The full texts of these three publication can be found in Section 6.1, Section 6.2, and Section 6.3, respectively. Furthermore, these publications have been the basis for several talks given at national and international conferences. Sorted from oldest to most recent, these talks are:

- Weitlaner, R., & Krammer, G. (2014). *Das Zulassungsverfahren der Pädagogischen Hochschule Steiermark und dessen Validität [The college admission process of the University College of Teacher Education Styria and its predictive utility]*. Paper presented at the BIMM-conference „Diversitätskompetenz in Bildungseinrichtungen“ in Salzburg, AUT, October 2014.
- Krammer, G. (2014). *General mental ability over and above domain-specific measures: predicting academic achievement in a 3 year longitudinal study on college admission*. Paper presented at the 15th conference of the "International Society for Intelligence Research (ISIR)" in Graz, AUT, December 2014.
- Krammer, G., & Pflanzl, B. (2015). *Lehramtsstudienwerber/innen faken für ihren Studienplatz weniger als sie es könnten [Applicants to teacher education fake more than they could]*. Paper presented at the conference of the "Österreichischen Gesellschaft für Forschung und Entwicklung im Bildungswesen (ÖFEB)" in Klagenfurt, AUT, August 2015.

- Krammer, G., & Pflanzl, B. (2015). *Studienerfolg in Lehramtsstudien: Eine 3-jährige Längsschnittstudie zu Intelligenz, Gesundheits- und Erholungsverhalten und realistischen Berufserwartung [Academic achievement in teacher education: a 3 year longitudinal study on intelligence, recreational and health behavior, and realistic job expectations]*. Paper presented at the 80th conference of the "Arbeitsgruppe für Empirische Pädagogische Forschung (AEPF)" in Göttingen, GER, September 2015.
- Weitlaner, R., & Krammer, G. (2016). *Competence-based admission to teacher education*. Paper presented at the ENTEP-Conference "Teacher Education in Europe" in Graz, AUT, March 2016.
- Krammer, G., Sommer, M., Feldhammer, M., & Arendasy, M. E. (2016). *Der Faking-Prozess realer Bewerbungssituationen [The faking process of real-life selection settings]*. Paper presented at the 11th conference of the "Österreichischen Gesellschaft für Psychologie (ÖGP)" in Innsbruck, AUT, April 2016.
- Krammer, G., Pflanzl, B., Sommer, M. & Arendasy, M. E. (2016). *Competence-based admission to teacher education: the predictive utility of realistic job expectations*. Paper presented at the 31th "International Congress of Psychology (ICP)" in Yokohama, JP, July 2016.

6.1 Study 1: Realistic Job Expectations Predict Academic Achievement

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Realistic job expectations predict academic achievement

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ABSTRACT

Individual differences in realistic job expectations – the degree to which realistic expectations about prospective education and job are held – were introduced as a potential predictor of academic achievement. Students in teacher education ($N = 176$) completed a test battery for college admission consisting of general mental ability, reactions to stress and coping with stress, and a structured interview assessing individual differences in realistic job expectations. Grade point averages of the entire three-year long bachelor's degree and students' bachelor thesis grades served as criterion variables. Grade point averages were calculated separately for courses fostering declarative and procedural knowledge. Declarative knowledge and students' bachelor thesis grades were predicted by individual differences in general mental ability, and realistic job expectations. Contrary, procedural knowledge was predicted by individual differences in reactions to stress and coping with stress, and realistic job expectations. The current study is the first to show this predictive utility of realistic job expectations.

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1. Introduction

Meta-analyses have already identified several cognitive determinants of academic achievement, such as general mental ability (e.g. Hell, Trapmann, & Schuler, 2007; Kuncel, Hezlett, & Ones, 2001, 2004; Kuncel, Wee, Serafin, & Hezlett, 2010) and several non-cognitive determinants of academic achievement, for example personality traits (e.g. Poropat, 2009; O'Connor & Paunonen, 2007; Richardson, Abraham, & Bond, 2012). Yet, still a large portion of the variance in academic achievement remains unexplained. Therefore, there has been renewed interest in identifying determinants that may explain variance in academic achievement over and above well-established determinants (cf. Ackerman, Chamorro-Premuzic, & Furnham, 2011; Credé & Kuncel, 2008; Kuncel & Hezlett, 2007; Poropat, 2009; Richardson et al., 2012). In the present article we examine the predictive utility of a new potential determinant of academic achievement: the degree to which students hold realistic expectations about their prospective education and occupation.

1.1. Defining and measuring academic achievement

Grade point averages (GPA) are often considered as the golden standard for measuring academic achievement. Thus, most meta-analyses (e.g. Hell et al., 2007; Kuncel et al., 2001, 2004; Poropat, 2009; Richardson et al., 2012; Trapmann, Hell, Weigand, & Schuler, 2007) resort to GPAs to measure academic achievement. Furthermore, GPAs

have been shown to predict occupational status (e.g. Strenze, 2007), salary level (e.g. Roth & Clarke, 1998), and job performance (e.g. D'Agostino & Powers, 2009; Roth, Be Vier, Switzer, & Schippman, 1996). In addition, studies suggest GPAs to be sufficiently reliable ($ICC = 0.94$; Bacon & Bean, 2006) and stable over time ($r_{tt} = 0.83$; Kuncel et al., 2004).

Several authors (e.g. Ackerman et al., 2011; Reeve, 2004; Reeve & Bonaccio, 2011) already noted that sometimes less effort is invested in defining and measuring criterion variables than in defining and measuring predictor variables. For example, GPAs seem to be the predominant criterion variable for academic achievement. Nevertheless, GPAs are not without shortcoming. It has been shown that GPAs obtained from later on in the education do not reflect differences between students as well as GPAs obtained earlier on (Reeve & Bonaccio, 2011). This might be due to individual differences in course choices and/or grading standards across and within institutions, which can lead to an underestimation of predictive validity of determinants of academic achievement (e.g. Ackerman et al., 2011; Berry & Sackett, 2009; Didier, Kreiter, Buri, & Solow, 2006; Elliot & Strenta, 1988; Reeve & Bonaccio, 2011). When considering GPAs, it therefore seems appropriate to examine GPAs of different academic years separately.

Furthermore, GPAs can be differentiated according to different courses types, e.g. courses for general knowledge, domain-specific knowledge, or applied courses (cf. D'Agostino & Powers, 2009; Gustafsson & Balke, 1993). There is evidence that GPAs obtained from different course types exhibit differential structural relations to determinants of academic achievement and occupational performance (cf. D'Agostino & Powers, 2009; Gustafsson & Balke, 1993; Reeve, 2004). Similar results have been obtained for predicting apprentices' training success, where declarative and procedural knowledge have been

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shown to exhibit differential structural relations to cognitive determinants of academic success (e.g. Häusler & Sommer, 2006; Schmidt-Atzert, Deter, & Jaekel, 2004). Distinguishing between declarative and procedural knowledge may be especially relevant when considering GPAs of more applied studies, such as teacher education. Teacher education world-wide involves student-teaching (Darling-Hammond & Lieberman, 2012), and GPAs of student-teaching courses can be seen as an instance of procedural knowledge (the course requirements are similar to future occupational performance: Kuncel et al., 2004). Furthermore, grades in student-teaching courses have a higher predictive utility for future teacher performance than grades of other courses (D'Agostino & Powers, 2009).

In addition to distinguishing between declarative and procedural knowledge, students' bachelor thesis grade could also serve as an additional differentiation of grades. Usually, every student has their own bachelor thesis topic, and a bachelor thesis needs to meet scientific quality standards. This sets bachelor thesis grades apart from course grades, as students need to transfer previously acquired knowledge, need to acquire new knowledge on their bachelor thesis topic, and have to do so autonomously. Additionally, the bachelor thesis constitutes the final and necessary step for degree completion, and degree completion has been treated as an additional measure of academic success (cf. Kuncel & Hezlett, 2007).

1.2. Cognitive determinants of academic achievement

g is one of the most important determinants of academic achievement (e.g. Credé & Kuncel, 2008; Hell et al., 2007; Kuncel & Hezlett, 2007; Kuncel et al., 2001, 2004; Kuncel et al., 2010). Although its predictive utility seems to be highest for academic achievement measured after the first academic year ($0.34 \leq \rho \leq 0.45$: Kuncel & Hezlett, 2007; Kuncel et al., 2001, 2004; Kuncel et al., 2010; Reeve & Bonaccio, 2011), *g* continues to predict academic achievement up until the very end of tertiary education ($0.20 \leq \rho \leq 0.24$: Kuncel & Hezlett, 2007; Kuncel et al., 2010). Several authors (e.g. Ackerman et al., 2011; Reeve & Bonaccio, 2011) argued that this decrease in the predictive utility of *g* may be attributable to an increase in individual differences in course choices and differential grading standards over the academic years. Thus, grades obtained later on in tertiary education are more confounded by construct-irrelevant variance, which subsequently decreases the predictive utility of *g*.

1.3. Non-cognitive determinants of academic achievement

Studies (e.g. Ackerman & Heggestad, 1997; Farsides & Woodfield, 2003; O'Connor & Paunonen, 2007; Paunonen & Ashton, 2001; Poropat, 2009) indicate that personality traits predict academic achievement. Conscientiousness ($\rho = 0.22$), openness ($\rho = 0.12$), and agreeableness ($\rho = 0.07$) have been demonstrated to be such non-cognitive determinants of academic achievement (Poropat, 2009). While the predictive utility of personality traits is smaller than that of *g*, it prevails even when accounting for *g* (e.g. Farsides & Woodfield, 2003; Poropat, 2009).

Facets of conscientiousness and openness even outperform their respective Big Five factor domains in predicting academic achievement (cf. O'Connor & Paunonen, 2007; Paunonen & Ashton, 2001). This indicates that facets of the Big Five domains may be differentially linked to academic achievement. This may also be the case for emotional stability, which has been often hypothesized to predict academic achievement (cf. Ackerman & Heggestad, 1997; Farsides & Woodfield, 2003), while failing to do so ($\rho = 0.02$: Poropat, 2009). Emotional stability comprises several facets such as anxiety, angry hostility, depression, self-consciousness, impulsiveness, and vulnerability or stress tolerance (for an overview: Arendasy, Sommer, & Feldhammer, 2011; Costa & McCrea, 1995). Some of these facets are related to academic achievement (e.g. test anxiety: $-0.21 \leq \rho \leq -0.33$; reactions to stress:

$-0.11 \leq \rho \leq -0.14$; Ackerman & Heggestad, 1997; Hembree, 1988; Richardson et al., 2012), while others are not (e.g. depression: $\rho = -0.03$: Richardson et al., 2012). These different predictive utilities of emotional stability facets may explain why studies failed to support the predictive utility of emotional stability.

Consequently, more specific facets of emotional stability may be useful for predicting academic achievement. For instance, the emotional stability facet *reaction to stress* (cf. Ackerman & Heggestad, 1997) may be a determinant of success in the teaching profession, where stress and burnout are prevalent (e.g. Schaarschmidt, 2005; Schaarschmidt & Kieschke, 2007). Indeed, teachers' stress has been linked to important student behaviours, such as not paying attention in class ($r = 0.39$) and hostility toward the teacher ($r = 0.40$: Geving, 2007). Furthermore, strategies for *coping with stress* have been shown to be related to grades in student-teaching courses (Rieder, 2011). Unfortunately, there are currently no studies examining the predictive utility of selected emotional stability facets for predicting more conventional criteria of academic success, such as GPAs.

1.4. Realistic job expectations

The accuracy of students' expectations of their educational and professional career may be another important determinant of academic success. Students do not always hold realistic expectations of studying at university, and providing them with more realistic information on what to expect reduces attrition (e.g. Braxton, Vesper, & Hossler, 1995; Cooke, Sims, & Peyrefitte, 1995; Earnest & Dwyer, 2010). Providing students with such realistic information lowers the initial expectations, and increases self-selection, organizational commitment, satisfaction, and possibly even academic achievement (e.g. Braxton et al., 1995; Cooke et al., 1995; Earnest & Dwyer, 2010). Actually, this strategy was adopted from a recruitment method used by organizations: *realistic job previews* (cf. Phillips, 1998; Premack & Wanous, 1985; Rynes, 1991). Providing realistic job previews influences how initial expectations are met, which in turn influences job satisfaction, organizational commitment, the intent to leave, job survival, and moderately job performance (e.g. Wanous, Poland, Premack, & Davis, 1992). In the same way, when students have more accurate expectations, it should lead to having better met expectation, which in turn may affect academic achievement. Unfortunately, most studies so far conducted in the academic context used an intervention design and solely focused on increasing the accuracy of students' expectations regarding more general aspects of studying at university. It therefore might be useful to evaluate, whether individual differences in students' expectations about their prospective educational and occupational career – realistic job expectations (RJE) – have predictive utility in explaining individual differences in academic achievement.

1.5. Formulation of the problem

In the present study, the predictive utility of RJE for academic achievement was examined in the context of teacher education. This specific domain was chosen due to the applied nature of its curriculum, which allows differentiating academic achievement into declarative and procedural knowledge. In addition, there are renewed efforts to improve teacher education in Austria and interest in selecting successful teachers rose in the last few years (Mayr, 2012). The next section outlines the research hypotheses with regard to the measurement of the criterion variable *academic achievement* and the predictive utilities of the determinants.

1.5.1. Hypotheses regarding the measurement of academic achievement

Prior research (e.g. Reeve & Bonaccio, 2011) suggested that grades have a higher saturation of construct-relevant variance in the first years of education than later on. This should affect factor loadings of academic achievement indicators. More precisely, GPAs obtained later on

in the education should have significantly lower factor loadings than GPAs earlier on. To our knowledge, this hypothesis has not yet been empirically tested, especially not considering declarative and procedural facets of academic achievement. Testing this hypothesis will contribute to our understanding of the degree to which GPAs of different academic years actually measure academic achievement alike.

1.5.2. Hypotheses regarding the predictive utility of general mental ability

Prior research has shown that *g* predicts academic achievement (Credé & Kuncel, 2008; Hell et al., 2007; Kuncel & Hezlett, 2007; Kuncel et al., 2001, 2004; Kuncel et al., 2010). *g* has shown predictive utility for declarative and procedural knowledge, while it may be less relevant for procedural than for declarative knowledge (cf. Häusler & Sommer, 2006; Schmidt-Atzert et al., 2004). This might be particularly true for teacher education, where Kuncel et al. (2004) reported that *g* does not predict procedural knowledge ($\rho = -0.04$). The authors dismissed their finding, explaining their null-result with sampling error and problems associated with measuring their criterion variable. Alternatively, their null-finding might reflect that *g* possesses less predictive utility for procedural knowledge in this particular domain at a certain level of expertise (for a similar argument: Ackerman, 2003). Therefore, we hypothesized that *g* has a higher predictive utility for declarative than procedural knowledge. Furthermore, *g* should also predict students' bachelor thesis grades, as knowledge acquisition is vital for the bachelor thesis, and in turn *g* should have predictive utility.

1.5.3. Hypotheses regarding the predictive utility of non-cognitive determinants

Studies show mixed results on the predictive utility of emotional stability for academic achievement (cf. Ackerman & Heggestad, 1997; Farsides & Woodfield, 2003; Poropat, 2009). One reason for the inconsistent findings may be that some emotional stability facets predict academic achievement, while others do not (cf. Ackerman & Heggestad, 1997; Hembree, 1988; Richardson et al., 2012). We therefore hypothesized that the emotional stability facet *coping with stress and reaction to stress* (cf. Ackerman & Heggestad, 1997; Geving, 2007; Rieder, 2011; Richardson et al., 2012) should contribute to the prediction of academic achievement, even when controlling for *g*. Furthermore, comparing validity coefficients across studies revealed higher coefficients for procedural knowledge than for declarative knowledge (cf. Ackerman & Heggestad, 1997; Geving, 2007; Richardson et al., 2012; Rieder, 2011). Therefore, *coping with stress and reaction to stress* (CRS) should have a higher predictive utility for procedural knowledge than for declarative knowledge and students' bachelor thesis grades.

In this study, RJE are introduced as a potential determinant of academic achievement. It has not yet been examined whether individual differences in RJE contribute to the prediction of academic achievement. The present study therefore examines, whether RJE can account for individual differences in declarative knowledge, procedural knowledge, and students' bachelor thesis grades.

2. Method

2.1. Measures

2.1.1. Coping with stress and reaction to stress

CRS was measured by means of the domain factor scale *health and recreational behavior* of the Inventory for Personality Assessment in Situations (IPS; Schaarschmidt & Fischer, 2013). The IPS contains three subscales assessing the higher order factor *health and recreational behavior*: *relaxation after a working day* (RE), *active recovery behavior in leisure* (RB), and *health prevention due to warning signals* (HP). The IPS is a German personality inventory that has been specifically developed for professions with high psychosocial demands such as the teacher profession. These scales possess face-validity, as their content is evidently important for teacher education (e.g. Krammer & Pflanzl, 2015b).

Furthermore, several validation studies confirmed that the scales of this inventory measure personality dispositions relevant to the teaching profession, and that teachers and students in teacher education exhibit characteristic personality profiles that are similar across these two groups but differ markedly from students of other disciplines (cf. Mayr & Brandstätter, 1998; Schaarschmidt & Fischer, 2013).

For each scale, a situational context is given. E.g. for *health prevention due to warning signals* the situational context is that you have not been feeling too well for a longer time and that people have already mentioned this to you suggesting a doctor's appointment. The items address personal behavior that may occur in the described situation (e.g. "to act cautious and preventive"). For *relaxation after a working day*, the situational context addresses the evening after the end of a working day, and the personal behavior how this evening is used (e.g. "to enjoy it fully"). For *active recovery behavior in leisure*, the situational context addresses having spare time to use, and the personal behavior how the spare time is used (e.g. "to intensely dedicate myself to leisure activities"). The task of the respondents is to indicate the extent to which they would exhibit the personal behavior in the given situation using a 4 point Likert-scale. Each of these scales comprises five items. Schaarschmidt and Fischer (2013) report satisfactory intern consistency ($0.71 \leq \alpha \leq 0.91$) and good re-test reliabilities ($0.70 \leq r_{tt} \leq 0.88$) for these scales.

2.1.2. Realistic job expectations

RJE were assessed with a structured interview designed specifically for college admission in teacher education. Every applicant was interviewed by two assessors for approximately 15 min, and rated by both assessors independently. The prerequisite for being an assessor was having a pedagogical education.

The structured interview contained eight items. Three items were general items regarding the overall poise of the applicants, i.e. an item addressed the manner of the verbal expression of the applicants. These general items were omitted for the present analysis, as they do not assess RJE. RJE were assessed with the remaining five items, each rated on a 4 point Likert-scale (*definitely true to not true at all*). For example, one item addressed knowledge of current pedagogical affairs, like changes in school reforms (i.e. "the applicant is knowledgeable on current pedagogical issues"), while another item addressed the professional experience of the applicants relevant for the pedagogical context (i.e. "the applicant has experienced pedagogical settings relevant for the teaching profession"). Previous research using CFA indicated that these five items of the structured interview are essentially uni-dimensional for each of the two raters (for further details: Krammer & Pflanzl, 2015a), and exhibited sufficient intern consistencies (R1: $\alpha = 0.831$; R2: $\alpha = 0.850$).

2.1.3. General mental ability

The fluid intelligence (G_f) subtests numerical-inductive reasoning (NIR) and figural-inductive reasoning (FIR) of the Intelligence Structure Battery (INSBAT; Arendasy et al., 2012) were used to measure *g*. The INSBAT is a commonly used German intelligence test battery constructed on the basis of the hierarchical intelligence model of Cattell-Horn-Carroll (Carroll, 1993; Horn & Noll, 1997). Both subtests were administered as computerized adaptive tests (CAT; van der Linden & Glas, 2000) with the target reliability set to an equivalent of Cronbach's $\alpha = 0.70$. As soon as this target reliability was reached, the adaptive testing algorithm terminated.

The subtest *numerical inductive reasoning* consisted of number series problems. Respondents had to identify the rules governing the number series, and to complete the number series by providing the next number. This subtest was constructed by means of automatic item generation and has been shown to exhibit good psychometric properties (Arendasy & Sommer, 2012; Arendasy et al., 2012).

The subtest *figural-inductive reasoning* consisted of classic 3×3 figural matrices problems. Respondents had to identify the rules governing

the matrices and to complete each matrix with one of eight answer alternatives. This subtest was constructed on the basis of a detailed construction rationale, and has exhibited good psychometric properties (Arendasy et al., 2012; Hornke, 2002; Hornke, Etzel, & Küppers, 2000).

2.1.4. Academic achievement

For academic achievement, all grades of an entire bachelor's degree of education were used. Within the curricula of the University College of Teacher Education Styria, the courses can be classified into four major domains: a) social sciences, b) subject matter and teaching methodology, c) complimentary studies, and d) student-teaching. These four domains can be classified into two even broader domains: courses where students study (a, b, and c), and courses where students teach (d). Former conveys declarative knowledge, latter conveys procedural knowledge. For the latter, the students were assigned to schools where they regularly had to hold classes, along with the necessary preparation and post-processing, supervision, and mentoring. Student-teaching is part of the curriculum in each semester and makes up one fifth of the curriculum. Additionally, we obtained students' bachelor thesis grade as a final measure of academic achievement. In summary, the grades separated for declarative and procedural knowledge, and the grade of the bachelor thesis were used as criteria.

2.2. Sample

In the academic year 2011/12, 237 applicants were accepted and began to study at the University College of Teacher Education Styria. Of these 237 students, 176 finished their bachelor's degree three years later. These 176 graduates comprised the final sample. Thereof, 144 (82%) were female and 32 (18%) male (ratio within the 237 applicants: female/male ~74%/26%), and 112 (64%) were training to become teachers in primary and 64 (36%) in secondary education (ratio within the 237 applicants: primary/secondary ~57%/43%). At the beginning of the academic year 2011/12, the sample was between 20.5 and 52.6 years old ($M = 25.4$, $SD = 5.4$). Age did not differ between the graduates and non-graduates ($t(235) = 0.351$, $p = 0.726$, $d = 0.05$); neither did CRS ($t(235) = 0.002$, $p = 0.998$, $d = 0.00$), RJE ($t(235) = -1.797$, $p = 0.074$, $d = -0.27$), nor g ($t(235) = -0.619$, $p = 0.536$, $d = -0.09$), although there was a small effect for RJE.

2.3. Procedure

All the data for the predictors were collected within the college admission process in the academic year 2011/12. For college admission, the applicants completed the measures for intelligence and personality, and finally the applicants were interviewed. The admitted students studied for three years to obtain their bachelor's degree until the end of the academic year 2013/14. Within the last academic year of their education, students had to begin and finish their bachelor thesis for degree completion. All grades are managed electronically in one database for all students by the university administration and were culled from this database.

2.4. Tested models

In line with current best practice standards in structural equation modeling (cf. Mueller & Hancock, 2008) the structural relations between the latent predictor and criterion variables were evaluated in a two-step process. Firstly, the measurement models for the predictor and criterion measures were evaluated separately to rule out a potential misfit of the full structural model being attributable to a misfit in the measurement models. In a second step, the two measurement models were combined into a full structural model to examine the predictive utility of the latent predictors for the criteria.

The indicators for the measurement models of the latent predictors were 1) the three IPS-subscale (RE, RB, and HP) for CRS, 2) the mean

scores of the raters of the standardized interview (R1 and R2) for the RJE, and 3) the two INSBAT-subscale (FIR and NIR) for g . The three latent factors (CRS, RJE, g) were allowed to be correlated to examine potential multicollinearity.

The measurement model of the latent criteria had two factors, one representing declarative and one procedural knowledge. Both factors were measured by GPAs from the 1st, 2nd, and 3rd academic year according to curriculum. Based on previous studies (Häusler & Sommer, 2006; Schmidt-Atzert et al., 2004) the latent declarative and procedural knowledge factors were allowed to correlate.

Factor loadings from the latent declarative and procedural knowledge factors to their respective indicators were examined to test the hypothesis on the saturation of construct-relevant variance within the GPAs. Therefore, these factor loadings were restricted to be equal within each latent knowledge factor for the academic years. If the resulting restricted model fitted the data worse than the unrestricted model (Cheung & Rensvold, 2002), pairwise comparisons followed to determine which academic years differed.

In a final step, the two measurement models were combined into a full structural model and augmented by including the students' bachelor thesis grade as criterion variable. Direct paths lead from all three latent predictors to the correlated latent knowledge factors. In addition, direct paths lead from g , RJE, and declarative knowledge to the bachelor thesis grade. The direct paths from factors CRS and procedural knowledge to the thesis grade were set to zero, as writing a bachelor thesis does not involve teaching of any sort, and should also be independent of individual differences in CRS.

2.5. Evaluation of the model fit

The following cut-offs for the GoF indices were used to evaluate the global model fits: non-significant χ^2 statistic, root mean square error approximation (RMSEA) < 0.08, comparative fit index (CFI) ≥ 0.95 , and standardized root mean square residual (SRMR) < 0.08 (cf. Hu & Bentler, 1999; Marsh, Hau, & Wen, 2004). For local model fit, critical ratio tests tested whether the path coefficients statistically differed from zero ($p < 0.05$). The changes in χ^2 statistic and CFI were used to compare nested models: non-significant changes in χ^2 statistic and $\Delta CFI < 0.01$ indicated equivalent model fit (Cheung & Rensvold, 2002).

3. Results

3.1. Descriptive results

Table 1 provides descriptive statistics and correlations of the manifest variables. Manifest variables of the same latent factor were positively correlated, and correlations between manifest variables of different latent factors were generally smaller. Multivariate normality was given neither for the indicator variables of the predictors, the indicator variables of the criteria, nor for the all manifest variables (Mardia test: all multivariate skewness ≥ 40.05 , all $p < 0.001$). WLSMV (cf. Lei, 2009; Savalei & Rhemtulla, 2013) was used for parameter estimation, due to the lack of multivariate normality and that indicators of the criterion variables constituted categorical variables. All structural equation models were calculated with *lavaan* (Rosseel, 2012).

3.2. Measurement models

The measurement model of the predictor variables fitted the data well ($\chi^2[11] = 17.655$, $p = 0.09$, RMSEA < 0.01, CFI > 0.99, SRMR = 0.042). All factor loadings were of the expected magnitude and statistically significant. Furthermore, g correlated neither with RJE ($r = -0.15$, $p = 0.28$), nor with CRS ($r = 0.02$, $p = 0.86$), and RJE and CRS were also not correlated ($r = 0.15$, $p = 0.09$).

The measurement model of the criterion variables fitted the data well ($\chi^2[8] = 6.651$, $p = 0.575$, RMSEA < 0.01, CFI > 0.99, SRMR =

Table 1
Descriptive statistics and intercorrelations of the manifest variables of the SEM for the predictors, the declarative knowledge, the procedural knowledge, and the grade on the bachelor thesis.

Predictors	Descriptive			Predictors					Declarative			Procedural		
	M	SD	Skew.	Kurt.	RE	RB	HP	R1	R2	FIR	NIR	GPA-AV1	GPA-AV2	GPA-AV3
Predictors	RE	3.47	0.37	−0.43	−0.13									
	RB	3.56	0.32	−0.73	0.41	0.60								
	HP	3.53	0.32	−0.66	−0.23	0.46	0.42							
	R1	3.56	0.46	−0.98	0.33	0.10	0.16	0.07						
	R2	3.51	0.51	−0.92	−0.11	0.07	0.14	0.05	0.84					
	FIR	−0.03	0.80	0.27	0.39	0.00	−0.02	0.09	−0.10	−0.12				
Declarative	NIR	1.23	1.43	−0.38	−0.10	0.04	−0.13	0.17	−0.06	−0.02	0.24			
	GPA-AV1	4.54	0.31	−1.11	1.30	0.10	0.12	0.09	0.23	0.20	0.13			
	GPA-AV2	4.67	0.28	−1.79	3.99	0.05	0.09	0.07	0.16	0.19	0.09	0.71		
Procedural	GPA-AV3	4.75	0.26	−1.86	3.93	0.04	0.07	0.05	0.14	0.13	0.06	0.11	0.71	0.76
	GPA-AV1	4.85	0.28	−2.82	10.68	0.18	0.22	0.15	0.16	0.15	0.06	0.50	0.56	0.58
	GPA-AV2	4.87	0.29	−2.43	5.28	0.19	0.23	0.19	0.11	0.14	0.01	0.04	0.41	0.49
	GPA-AV3	4.93	0.12	−1.82	2.62	0.00	−0.01	0.03	0.04	0.09	0.09	0.29	0.30	0.33
	Thesis	4.56	0.76	−2.12	5.38	0.00	−0.06	−0.04	0.16	0.16	0.04	0.15	0.19	0.16

Note. RE = relaxation after a working day; RB = active recovery behavior in leisure; HP = health prevention because of warning signals; R1 and R2 = 1st and 2nd rate of the structured interview; NIR = numerical inductive reasoning; FIR = figurative inductive reasoning; GPA-AV1, GPA-AV2, and GPA-AV3 = GPA of the 1st, 2nd, and 3rd academic year for the declarative/procedural knowledge; Thesis = grade on the bachelor thesis. All grades can range from 1 to 5, with 5 being the best grade; RE, RB, HP, R1, and R2 can range from 1 to 4, with 4 being the best score. For FIR and NIR, the estimates of the person parameters are given. For $n = 176$, a correlation exceeding $\pm .148$ is sig.

0.029). Again, all factor loadings were statistically significant. In line with theoretical expectations, declarative and procedural knowledge were highly correlated ($r = 0.92, p < 0.001$). However, the latent correlation did not reach 1, suggesting that differentiating between declarative and procedural knowledge is feasible despite this high correlation. Next, factor loadings of the declarative knowledge indicators were constrained to being equal. This restricted model ($\chi^2[9] = 6.476, p = 0.691, RMSEA < 0.01, CFI > 0.99, SRMR = 0.033$) did not fit the data worse than the unrestricted model ($\Delta\chi^2[1] = 0.175, p = 0.676, \Delta CFI < 0.001$). However, a model with equality constraints on factor loadings of the procedural knowledge indicators did not fit the data ($\chi^2[9] = 54.250, p < 0.001, RMSEA = 0.170, CFI = 0.584, SRMR = 0.167$) and also fitted significantly worse than the unrestricted model ($\Delta\chi^2[1] = 47.599, p < 0.001, \Delta CFI = 0.416$). Thus, the hypothesis that GPAs obtained in later years are less saturated by the latent construct only held for the procedural knowledge factor. Pairwise comparisons indicated that constraining the factor loading of the 1st and 2nd academic year to be equivalent ($\chi^2[9] = 11.385, p = 0.250, RMSEA = 0.039, CFI = 0.978, SRMR = 0.041$) resulted in a significantly worse model fit ($\Delta\chi^2[1] = 4.734, p = 0.030, \Delta CFI = 0.022$). Similar results were obtained for the 2nd and 3rd academic year ($\chi^2[9] = 41.966, p < 0.001, RMSEA = 0.145, CFI = 0.697, SRMR = 0.117; \Delta\chi^2[1] = 35.315, p < 0.001, \Delta CFI = 0.303$), and the 1st and 3rd academic year ($\chi^2[9] = 50.499, p < 0.001, RMSEA = 0.162, CFI = 0.619, SRMR = 0.152; \Delta\chi^2[1] = 43.848, p < 0.001, \Delta CFI = 0.381$). An inspection of the standardized factor loading estimates (cf. Fig. 1) indicated that the trait saturation of the year-wise GPAs of the latent procedural knowledge factor gradually decreased from the 1st to the 3rd academic year.

3.3. Full structural model

Next, the fit of the full structural model was examined. This model fitted the data well ($\chi^2[68] = 73.351, p = 0.307, RMSEA = 0.021, CFI = 0.979, SRMR = 0.048$). However, the paths from declarative knowledge to students' bachelor thesis grades ($\beta = 0.066, p = 0.444$), from CRS to declarative knowledge ($\beta = 0.128, p = 0.185$), and from g to procedural knowledge ($\beta = 0.064, p = 0.643$) failed to reach significance. Since this invalidated the initial full structural model (cf. Marsh et al., 2004), a revised full structural model was calculated wherein these paths were fixed to zero. The revised full structural model fitted the data well ($\chi^2[71] = 76.004, p = 0.319, RMSEA = 0.020, CFI = 0.980, SRMR = 0.054$). The standardized factor loadings and regression coefficients of the revised full structural equation model are shown in Fig. 1, details for all regression coefficients in Table 2. All factor loadings and regression coefficients were statistically significant ($p < 0.05$). Given that the paths from CRS to declarative knowledge and from g to procedural knowledge were essentially zero, while the paths to the respectively other knowledge domain differed significantly from zero, there was no need to further test the hypotheses regarding the differentiated predictive utility of CRS and g. In summary, g predicted declarative knowledge and students' bachelor thesis grades, CRS the procedural knowledge, and finally RJE all three criteria.

4. Discussion

In the present study, individual differences in realistic job expectations (RJE) were introduced as a new determinant of academic achievement. The predictive utility of RJE over and above well-established determinants of academic achievement such as g and emotional stability facets was examined and shown. In addition, it was evaluated whether the predictive utility of these determinants generalizes across procedural and declarative knowledge measures of academic achievement.

Fig. 1. The revised full structural model ($\chi^2[71] = 76.004, p = 0.319, RMSEA = 0.020, CFI = 0.980, SRMR = 0.054$) with standardized path coefficients. For the declarative knowledge, procedural knowledge, and the grade on the bachelor thesis, the amount of explained variance is given. Note: CRS = coping with stress and reaction to stress; RJE = realistic job expectations; RE = relaxation after a working day; RB = active recovery behavior in leisure; HP = health prevention because of warning signals; R1 and R2 = 1st and 2nd rater of the structured interview; NIR = numerical inductive reasoning; FIR = figural inductive reasoning; GPA = grade point average.

4.1. Measuring and modeling academic achievement

While GPAs are one of the most commonly used measures of academic achievement (e.g. Hell et al., 2007; Kuncel et al., 2001, 2004; Poropat, 2009; Richardson et al., 2012; Trapmann et al., 2007), it has been suggested that GPAs of different academic years do not reflect academic achievement alike (e.g. Reeve & Bonaccio, 2011). Our results corroborate this potential incomparability of GPAs only for grades of courses conveying procedural knowledge. One should therefore carefully consider from what academic year GPAs of procedural knowledge courses are obtained, as it seems that for these courses GPAs of different academic years may not reflect academic achievement alike. In contrast, we found invariance of the latent trait saturation across academic years of grades from courses conveying declarative knowledge. This invariance attenuates this potential incomparability of GPAs, however only for grades of procedural knowledge courses. These findings also demonstrate a key difference between declarative and procedural knowledge, even if both facets of academic achievement have been shown to be highly correlated as in previous studies (cf. Häusler & Sommer, 2006; Schmidt-Atzert et al., 2004).

Table 2
Unstandardized (b) and standardized regression (β) coefficients of the revised full structural model, with the amount of variance explained for the three criterion variables (R^2).

Criterion	Predictor	b	SE	β	p	R^2
Declarative knowledge	RJE	0.061	0.024	0.235	0.011	14.2%
	G _f	0.077	0.030	0.295	0.011	
	CRS	–	–	–	–	
Procedural knowledge	RJE	0.045	0.022	0.220	0.041	17.3%
	G _f	–	–	–	–	
	CRS	0.072	0.025	0.353	0.003	
Thesis	RJE	0.150	0.070	0.197	0.033	15.2%
	G _f	0.257	0.080	0.337	0.001	
	CRS	–	–	–	–	

Note. CRS = coping with stress and reaction to stress; RJE = realistic job expectations; G_f = fluid intelligence; SE = standard error of the unstandardized regression coefficients; p = p-value of the unstandardized regression coefficients.

4.2. The predictive utility of general mental ability

g constitutes one of the most important determinants of academic achievement (e.g. Credé & Kuncel, 2008; Hell et al., 2007; Kuncel & Hezlett, 2007; Kuncel et al., 2001, 2004; Kuncel et al., 2010). However, studies suggest that procedural knowledge facets of academic achievement are less well predicted by g than declarative knowledge facets (e.g. Häusler & Sommer, 2006; Kuncel et al., 2004; Schmidt-Atzert et al., 2004). Our results are in line with the hypotheses that g predicts declarative knowledge acquisition (latent declarative knowledge factor), and transfer of acquired declarative knowledge (bachelor thesis), but does not account for procedural knowledge acquisition relevant to teacher education. This finding underlines the usefulness of distinguishing between different facets of academic achievement. Moreover, this finding advises to keep in mind that while g is an important determinant of academic achievement, g does not determine all facets of academic achievement alike.

4.3. The predictive utility of emotional stability facets

One reason for the mixed findings concerning the predictive utility of the Big Five domain factor emotional stability could be that some emotional stability facets predict academic achievement, while others do not (cf. Ackerman & Heggestad, 1997; Farsides & Woodfield, 2003; Hembree, 1988; Richardson et al., 2012; Poropat, 2009). We therefore examined an emotional stability facet relevant for the specific context given, and it showed predictive utility. While it was beneficial for the acquisition of procedural knowledge it was not for the acquisition of declarative knowledge or students' bachelor thesis grades. The present results are in line with the hypothesis that facet-level measures can have a higher predictive utility than domain-level measures (e.g. emotional stability: $\rho = 0.02$; Poropat, 2009). However, this finding seems to be confined to the procedural facet of academic achievement, and in turn demonstrates the importance of occupationally relevant variables for more applied study majors, such as teacher education.

4.4. The predictive utility of realistic job expectations

RJE were introduced as a new potential determinant of academic achievement. While students benefit from interventions increasing the accuracy of their expectations of studying at university (e.g. Braxton et al., 1995; Cooke et al., 1995; Earnest & Dwyer, 2010), no study conducted so far linked individual differences in RJE to more conventional measures of academic achievement, such as GPAs. The present study indicates that RJE have predictive utility for procedural and declarative facets of academic achievement alike. Despite its range restriction, the predictive utility of RJE was similar to the predictive utility reported for conscientiousness ($\rho = 0.22$; Poropat, 2009). RJE also predicted students' bachelor thesis grades over and above g . A potential explanation of these findings might be that RJE are strongly content specific, and partly conceptually linked to domain specific interest (Krapp, 1992; cf. Krapp & Prenzel, 2011, for an overview of domain specific interest).

4.5. Limitations and conclusion

Like all studies, the present one is not without limitations. Firstly, the data were obtained in the teacher education domain and therefore should not be carelessly generalized to other bachelor degree studies. Furthermore, the sample only comprised students, who graduated within the minimum amount of time. The sample therefore cannot be regarded as entirely representative. Future studies should attempt to replicate the present findings with a more heterogeneous student body and students from other domains. A cross-validation on an independent sample also seems appropriate due to post hoc modifications of the full structural equation model.

The second limitation concerns the measurement of the predictor variables. The present study was conducted in a real-life admission testing setting. Therefore, the predictor variables used were also used for admission testing. Although there is evidence that fluid intelligence and g are nearly indistinguishable (e.g. Arendasy, Hergovich, & Sommer, 2008; Gustafsson & Balke, 1993), future studies may consider a broader scope of cognitive abilities. A similar argument can be made for the personality traits. A revised test battery may include measures of openness and conscientiousness, which have been shown to be important determinants of academic achievement (e.g. Farsides & Woodfield, 2003; Poropat, 2009). Ideally, these personality traits would be measured at the domain-level and facet-level. Including domain-level and facet-level measures would allow to compare their predictive utility for different facets of academic achievement.

Despite these limitations, we believe that the present study is a first step in examining the predictive utility of RJE as a potential additional predictor of academic success. Furthermore, the present study outlines the usefulness of distinguishing between different facets of academic success. General mental ability (g), coping with stress and reactions to stress (CRS), and realistic job expectations (RJE) were differentially linked to individual differences in declarative and procedural knowledge, which corroborates and extends previous findings obtained in the field of predicting apprentices' training success (e.g. Häusler & Sommer, 2006; Schmidt-Atzert et al., 2004). In conclusion, realistic job expectations (RJE) were not only a new potential predictor for academic achievement, but are a new predictor with potential.

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6.2 Study 2: Faking of Personality Measures for College Admission in Teacher Education

Originalartikel

Faking von Persönlichkeitseigenschaften bei Zulassungsverfahren für Lehramtsstudien

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Zusammenfassung: Werden selbstbeschreibende Verfahren in Zulassungsverfahren verwendet, stellt sich die Frage, wie sehr Faking diese verzerrt, d. h., wie sehr Personen sich vorteilhafter darstellen, als sie es tatsächlich sind. Bis dato wurde dies für angehende Lehrkräfte nicht untersucht. In der vorliegenden Studie wurde Faking in einem Zulassungsverfahren an einem Persönlichkeitsinventar zum Verhalten und Erleben in sozial-kommunikativen Situationen, Leistungs- und Erholungssituationen untersucht. 116 Lehramtsstudierende bearbeiteten ein halbes Jahr nach ihrer Zulassung dasselbe Persönlichkeitsinventar nochmals. Bei dieser Nacherhebung gab es eine experimentelle Manipulation mit drei Bedingungen, um 1) das tatsächliche Ausmaß an Faking, 2) die Reproduzierbarkeit von Faking und 3) das mögliche Ausmaß an Faking zu bestimmen (= Faking-good). Beim Zulassungsverfahren trat in 16 von 18 Subskalen Faking auf, wobei dieses Ausmaß nur in 11 der 18 Subskalen reproduziert werden konnte. Bei Aufforderung zu faken, zeigte sich in 17 der 18 Subskalen Faking. Die Rangreihen waren umso verzerrter, je stärker Faking auftrat.

Schlüsselwörter: Faking, Persönlichkeit, Selektion, Zulassungsverfahren, IPS

Faking of Personality Measures for College Admission in Teacher Education

Abstract: Personality measures in student admission entail the question if applicants fake, i. e. distort their responses to be viewed more favorably than they actually are. This has not yet been investigated for student admission in teacher education. 116 students were given a personality inventory assessing social-communicative, achievement, and health-recreational behavior twice: once within student admission and half a year later. At this second measurement point, faking was manipulated experimentally to determine 1) the degree of faking within student admission, 2) if the faking of the student admission could be reproduced, and 3) how much faking was possible (= faking-good). Results indicated that students faked within the student admission in 16 out of 18 subscales but could reproduce their degree of faking in only 11 out of 18 subscales. When instructed to fake, faking occurred in 17 out of 18 subscales. The students rank order was negatively affected by the degree of faking.

Keywords: faking, personality, selection, student admission, IPS

1 Theoretischer Hintergrund

Faking ist das Phänomen eigene Ergebnisse in selbstbeschreibenden Verfahren ins Vorteilhafte zu verzerren (McFarland & Ryan, 2000) und stellt damit die Fairness von selbstbeschreibenden Verfahren bei Zulassungsverfahren in mehrerlei Hinsicht in Frage. Durch Faking werden nicht mehr die für die Zulassung intendierten Merkmale gemessen (Ziegler & Bühner, 2009) und nicht alle der sich Bewerbenden verzerren ihre Selbstbeschreibungen in gleichem Maße (Birkeland, Manson, Kisamore, Brannick & Smith, 2006). Dies kann dazu führen, dass nicht jene

ausgewählt werden, welche am geeignetsten sind, sondern jene, welche sich am geeignetsten darstellen können. Deshalb darf Faking für die Vergabe von Lehramtsstudienplätzen nicht außer Acht gelassen werden, wenn selbstbeschreibende Verfahren zum Einsatz kommen.

Faking ist ein komplexer Prozess, der von vielen Faktoren beeinflusst wird (McFarland & Ryan, 2000; Komar, Brown, Komar & Robie, 2008; Tett, Freund, Christiansen, Fox & Coaster, 2012). Diese Faktoren umfassen die eigenen Werte und Moralvorstellungen. Faking setzt Intelligenz und die Fähigkeit zu faken voraus. Darüber hinaus wird Faking von dem wahren Wert auf dem betroffenen

Merkmal beeinflusst, da niedrige Werte weiter nach oben hin verzerrt werden können als bereits hohe. Wie sehr Faking auftritt, hängt auch von der Attraktivität der angestrebten Stelle und der Höhe der Selektionsquote ab. Es ist auch entscheidend, welche Merkmale erhoben werden und für welche Art von Stelle diese erhoben werden. Neben diesen genannten Faktoren gibt es noch weitere, aber zusammengefasst können die Faktoren, die Faking beeinflussen, persönlich oder situativ sein (Ellingson & McFarland, 2011).

Qualitative Studien zu Faking belegen, dass Personen in denselben Situationen unterschiedlich faken (Ziegler, 2011; König, Merz & Trauffer, 2012). König et al. (2012) untersuchten die Kognitionen, die Faking zugrunde liegen, und stellten fest, dass Personen falsche Vorstellungen von selbstbeschreibenden Verfahren haben. U.a. gingen Personen davon aus, dass sich Inkonsistenzen in ihren Einzelantworten negativ auf ihr Gesamtergebnis auswirken würden. Diese Kognitionen legen nahe, dass Personen nicht wissen, dass die Items einer Skala in der Regel einfach summiert werden und, aufgrund mangelnder Reliabilität, keine einzelnen Items miteinander verglichen werden.

Studien belegen, dass es sich bei Faking um ein tatsächliches Problem handelt. Die Meta-Analyse zu Faking von Viswesvaran und Ones (1999) bestätigt, dass Personen in der Lage sind zu faken (abhängig vom Design $0.48 \leq d \leq 0.65$ bzw. $0.47 \leq d \leq 0.93$). Birkeland et al. (2006) zeigten, dass in Bewerbungssituationen Faking ebenfalls auftritt, jedoch in verringertem Ausmaß ($0.11 \leq d \leq 0.45$). Faking tritt auch über verschiedene Erhebungsmethoden hinweg auf. Selbst z.B. Auswahlgespräche – welche zur Studienplatzvergabe akzeptiert (Hell & Schuler, 2005) und aussagekräftig (Hell, Trapmann, Weigand & Schuler, 2007) sind – sind für Faking anfällig (Levashina & Campion, 2006; Levashina & Campion, 2007). Sogar bei Integritätstests und Tests mit biographischen Inhalten konnte Faking nachgewiesen werden (McFarland & Ryan, 2000). Auch wenn Persönlichkeitsmerkmale projektiv oder semi-projektiv statt selbstbeschreibend erhoben werden, tritt Faking auf (Ziegler, Schmidt-Atzert, Bühner & Krumm, 2007).

Der ausschließliche Einsatz von kognitiven Leistungstests könnte Faking in Zulassungsverfahren vermeiden. Allgemeine Intelligenz ist einer der besten Prädiktoren für späteren Berufserfolg (Schmidt & Hunter, 2004). Deshalb werden Intelligenztests und/oder fachspezifische Wissenstests in Zulassungsverfahren eingesetzt und haben sich dafür bewährt (Kuncel, Hezlett & Ones, 2001; Kuncel et al., 2005; Kuncel & Hezlett, 2007). Auch fachspezifische Leistungstests wie Studierfähigkeitstests sind für Zulassungsverfahren anerkannt (Kuncel et al., 2001; Hell, Trapmann & Schuler, 2007).

Diese Praxis der Verwendung von kognitiven Leistungstests in Zulassungsverfahren könnte auf die Vergabe von Lehramtsstudienplätzen übertragen werden. Päßler, Hell und Schuler (2011) weisen darauf hin, dass kognitive Merkmale für die Studien- und Berufsbewährung bei

Lehrkräften relevant sind. Die Bedeutsamkeit von kognitiven Merkmalen für Lehrkräfte zeigt sich bereits beim Studienerfolg (Kaub et al., 2012; Krammer, 2014). Zusätzlich ist das Fachwissen der Lehrkräfte eine notwendige, wenn auch nicht hinreichende, Voraussetzung für eine hohe Qualität des Unterrichts (Baumert & Kunter, 2011).

Persönlichkeitsmerkmale sollten jedoch bei Zulassungsverfahren nicht außer Acht gelassen werden, da sie im Allgemeinen als Prädiktoren für Erfolg im Studium und im Beruf gelten. Einzelne Facetten der Big Five beeinflussen den Studienerfolg (Trapmann, Hell, Hirn & Schuler, 2007; Poropat, 2009) und Berufserfolg (Judge, Higgins, Thoresen & Barrick, 1999). Sie liefern dazu in beiden Fällen einen inkrementellen Beitrag über die Intelligenz hinaus (Judge et al., 1999; Farsides & Woodfield, 2003; Ziegler, Danay, Schölmerich & Bühner, 2010). Persönlichkeitsmerkmale können damit ein Mehrgegnis für Zulassungsverfahren sein, auch wenn durch deren Verwendung Faking eine zu adressierende Problematik wird (Kuncel & Hezlett, 2010).

Während es für den Lehrerberuf fraglich ist, ob es «die Lehrerpersönlichkeit» wirklich gibt (Bromme & Haag, 2004), gelten Persönlichkeitsmerkmale und berufliches Interesse als Prädiktoren für die Studien- und Berufsbewährung bei Lehrkräften (Hanfstingl & Mayr, 2007). Emotionale Stabilität, Extraversion und Gewissenhaftigkeit haben sich als relevant für die Bewährung im Lehramtsstudium und Lehrerberuf gezeigt (Mayr, 2011). Auch berufliches Interesse steht in Zusammenhang mit dem Studienerfolg (Hanfstingl & Mayr, 2007; Kaub et al., 2012). Demnach ist es nicht zielführend, künftige Lehrkräfte nur aufgrund von kognitiven Leistungstests auszuwählen. Bis dato gibt es jedoch noch keine Studien, die Faking bei Zulassungsverfahren zur Vergabe von Lehramtsstudienplätzen thematisieren.

2 Die vorliegende Studie

Klassische Fakingstudien beruhen zumeist auf einer experimentellen Manipulation: Personen werden aufgefordert, sich so ideal wie möglich darzustellen (= Faking-good); dies wird im Anschluss mit einer Kontrollbedingung verglichen. Griffith, Chmielowski und Yoshita (2007) untersuchten Faking in der Personalauswahl, indem dieses Untersuchungsdesign einer tatsächlichen Bewerbungssituation folgte. Für ein ähnliches Design wie Griffith et al. plädierten Komar et al. (2008), um Faking in Bewerbungssituationen zu untersuchen. Griffin und Wilson (2012) verwendeten dieses Design ohne experimentelle Manipulation, um Faking in einem Inventar zu den Big Five bei der Selektion von Medizinstudierenden zu untersuchen. In der vorliegenden Studie wurden diese Ideen aufgegriffen: Es werden hierbei Daten aus einem Zulassungsverfahren zur Studienplatzvergabe für Lehramtsstudien mit Daten einer anschließenden experimentellen Manipulation verglichen.

Als Erstes war es von Interesse herauszufinden, inwiefern Bewerbende für Lehramtsstudien Persönlichkeitsmerkmale faken. Sie sollten sich in den für den Lehrerberuf relevant wahrgenommenen Persönlichkeitsmerkmalen besser darstellen, als sie es sind (Birkeland et al., 2006; Ziegler, 2011). In einer *Faking-good*-Bedingung sollten sie sich besser darstellen als beim Zulassungsverfahren, da in tatsächlichen Bewerbungssituationen weniger Faking auftritt, als möglich wäre (Birkeland et al., 2006). In der vorliegenden Studie war ein Persönlichkeitsinventar zum Verhalten und Erleben in sozial-kommunikativen Situationen, Leistungssituationen und Gesundheits- und Erholungssituationen Teil eines Zulassungsverfahrens für Lehramtsstudien: das Verhalten und Erleben in 1) sozial-kommunikativen Situationen, da sich Extraversion als relevantes Kriterium für die Bewährung im Lehramtsstudium und Lehrerberuf herausgestellt hat (Mayr, 2011); 2) Leistungssituationen, da es im Berufsalltag häufig zu Situationen mit besonderen Leistungs- und Handlungserfordernissen kommt, wie z.B. der Umgang mit schwierigen Schülerinnen und Schülern (Schaarschmidt, 2004); 3) Gesundheits- und Erholungssituationen, da Lehrkräfte stark Burn-out gefährdet sind (Unterbrink et al., 2007). Schaarschmidt und Fischer (2013) zeigten die Wichtigkeit dieser Merkmale für den Lehrerberuf durch einen Vergleich mit für den Lehrerberuf relevanten Persönlichkeitsvoraussetzungen (Mayr & Brandstätter, 1998). Sie zeigten auch, dass Lehrkräfte und Lehramtsstudierende vergleichbare Ausprägungen erzielen, diese sich jedoch von z.B. Psychologiestudierenden unterscheiden. Zudem besteht ein Zusammenhang dieser Merkmale mit der Studienleistung von Lehramtsstudierenden (Krammer, 2014).

Neben den praktischen Implikationen von Faking bedarf es aus wissenschaftlicher Sicht noch eines Erkenntnisgewinns zum Faking-Prozess (Burns & Christiansen, 2011; Griffith & Peterson, 2011). Ist Faking ein Prozess, der reproduziert werden kann, oder ist dieser bei Zulassungsverfahren derart komplex, dass er es nicht werden kann? Als Zweites interessierte daher, inwieweit das Faking eines Zulassungsverfahrens reproduziert werden kann. Aufgrund der Komplexität dieses Prozesses wird angenommen, dass die Bewerbenden dies nicht können.

Die Verzerrung der resultierenden Rangreihen durch Faking ist für die Praxis von Zulassungsverfahren das größte Problem. Diese Verzerrung beeinflusst unweigerlich auch die Selektion (Rosse, Stecher, Miller & Levin, 1998). Als Drittes interessierte daher, inwiefern das Ausmaß an Faking die Rangreihen der Bewerbenden verändert. Da Untersuchungen zufolge Faking die Rangreihen in Selektionsprozessen beeinflusst (Rosse et al., 1998; Winkelspecht, Lewis & Thomas, 2006), sollten diese Rangreihen umso mehr verändert werden, je mehr Faking auftritt.

Zusammengefasst wurde in der vorliegenden Studie das Faking in einem Persönlichkeitsinventar bei einer Studienplatzvergabe von Lehramtsstudien untersucht. Es wurden Daten eines Zulassungsverfahrens mit einer anschließen-

den experimentellen Manipulation verglichen. Dafür wurden folgende Hypothesen aufgestellt: 1) Die sich für Lehramtsstudien Bewerbenden stellen sich besser dar, als sie tatsächlich sind und können sich noch besser darstellen. 2) Sie können ihr Ausmaß an Faking nicht reproduzieren und 3) deren Rangreihen verändern sich umso mehr, je mehr Faking auftritt.

3 Methode

An der Pädagogischen Hochschule Steiermark (PHSt) sind die Studienplätze beschränkt. In der Regel werden pro Studienjahr ca. 60 % der Personen, die sich auf einen Studienplatz bewerben, zugelassen. Einerseits beinhaltet das Zulassungsverfahren Leistungsmerkmale: die Stimm- und Sprechleistung, die Kenntnisse der deutschen Sprache, die allgemeine Intelligenz und ggf. studiengangsspezifische Fachtests. Andererseits beinhaltet es Persönlichkeitsmerkmale: das Verhalten und Erleben in sozial-kommunikativen Situationen, Leistungssituationen und Gesundheits- und Erholungssituationen (Schulz-Kolland, Krammer, Rottensteiner & Weitlaner, 2014). Die Persönlichkeitsmerkmale werden durch das Inventar zur Persönlichkeitsdiagnostik in Situationen (IPS: Schaarschmidt & Fischer, 2013) erhoben.

3.1 IPS

Schaarschmidt und Fischer (2013, S.5) empfehlen das IPS für «Berufsgruppen, die sich durch ein höheres Maß psychosozialer Beanspruchung und durch akzentuierte Anforderungen an das sozial-kommunikative Verhalten auszeichnen», und nennen hierfür explizit Lehrkräfte. In diesem Sinne führen sie auch Reliabilitäten und Validitäten u. a. für Lehrkräfte und Lehramtsstudierende an. Das IPS erhebt drei Faktoren höherer Ordnung: das Verhalten und Erleben in 1) sozial-kommunikativen Situationen (6 Subskalen), 2) Leistungssituationen (6 Subskalen) und 3) Gesundheits- und Erholungssituationen (3 Subskalen). Für jede Subskala wird eine prototypische Situation vorgegeben und es gilt, das eigene Verhalten und Erleben darin einzuschätzen. Eine Subskala des sozial-kommunikativen Verhaltens und Erlebens ist z.B. die *Rücksichtnahme bei sozialer Verantwortung*, wofür eine Situation mit sozialer Verantwortung vorgegeben wird («Mir werden Aufgaben übertragen, bei denen ich ständig mit anderen Menschen zu tun habe und Verantwortung für diese Menschen wahrnehmen muss.») und es gilt, Fragen bzgl. der eigenen Rücksichtnahme zu beantworten (z.B.: «Wahrscheinlich werde ich in solch einer Situation viel Verständnis zeigen.»). Eine weitere Subskala ist z.B. die *Aktivität in vertrauter kommunikativer Situation*: Hier wird eine vertraute und kommunikative Situation vorgegeben und es gilt, Fragen bzgl. der eigenen Aktivität zu beantworten. Eine Subskala für das Verhalten und Erleben in Leistungssituationen ist

z.B. die *Beharrungstendenz bei Umstellungserfordernis*, wofür eine Situation mit Umstellungserfordernis vorgegeben wird und Fragen bzgl. der eigenen Beharrungstendenz zu beantworten sind. Die Items sind auf einer 4-stufigen Skala zu beantworten (*stimmt gar nicht – stimmt genau*). Zusätzlich zu diesen 15 Subskalen wird die Zufriedenheit mit dem eigenen Verhalten und Erleben in jeder dieser Situationen 5-stufig (*sehr unzufrieden – sehr zufrieden*) erfragt. Daraus ergeben sich drei Zufriedenheitsskalen, eine pro Faktor höherer Ordnung. Damit besteht das IPS aus 18 Subskalen (s. Tabelle 1 für eine Auflistung aller Subskalen).

Schaarschmidt und Fischer (2013) berichten von der zusammengesetzten Eichstichprobe für ihre 18 Skalen interne Konsistenzen mit Cronbachs α von .71 bis .91 und für Lehrkräfte und Lehramtsstudierende von .69 bis .91. In Tabelle 1 sind für alle Skalen die internen Konsistenzen der vorliegenden Stichprobe beim Zulassungsverfahren angeführt.¹ Sie fielen geringer als die berichteten aus. Teilweise fielen sie für Persönlichkeitsmerkmale, die mit geringen Itemzahlen erhoben werden, nicht sehr zufriedenstellend aus.

3.2 Design und experimentelle Manipulation

Das IPS wurde ein halbes Jahr nach dem Zulassungsverfahren (t1) nochmals bearbeitet (t2). Zu t2 wurde Faking («Faking-good»), Faking wie beim Zulassungsverfahren («wie-damals») und, als Baseline, kein Faking («honest») experimentell mittels Instruktionen induziert. Bei *Faking-good* wurde instruiert, nicht wahrheitsgetreu zu antworten, sondern sich so geeignet wie möglich darzustellen, um ein Lehramtsstudium an der PHSt beginnen zu dürfen. Bei *wie-damals* wurde instruiert, das IPS wie zu t1 zu bearbeiten. Es wurde dazu aufgefordert, sich in die Situation des damaligen Zulassungsverfahrens hineinzuversetzen und die Items so zu beantworten, als würde jetzt am Zulassungsverfahren teilgenommen werden. Bei der *honest*-Bedingung erfolgte nur eine Instruktion für das IPS und es wurden keinerlei Hinweise auf das Zulassungsverfahren gegeben.

3.3 Stichprobe

Für die Nacherhebung wurde unter jenen Lehramtsstudierenden rekrutiert, die für das betreffende Studienjahr das Zulassungsverfahren durchlaufen hatten. 563 Bewerbenden hatten das IPS vollständig bearbeitet, davon wurden 286 als ordentliche Studierende des Primar- oder Sekundar-Studiengangs aufgenommen. 121 Studierende nahmen an

der vorliegenden Studie teil. Die Analysen beziehen sich auf die 116 Personen, deren Daten mit t1 verknüpft werden konnten.² Dies entspricht einer Rücklaufquote von 40.6 %, bezogen auf das betreffende Studienjahr. Zu t2 wurde, gleich wie zu t1, in Kleingruppen (8–12) getestet. Diese Kleingruppen wurden zufällig den drei Bedingungen zugeteilt. Die Stichprobengrößen der *honest*-, *wie-damals*- bzw. *Faking-good*-Bedingung betrugen 36, 39 bzw. 41 und waren damit vergleichbar groß ($\chi^2[2] = 0.33$, $p = .85$). Da die Kleingruppen den Bedingungen zufällig zugeteilt wurden, wurde geprüft, ob die Kleingruppen in sich überzufällig homogen waren: ICC (one-way, single) wurden für jede IPS-Skala berechnet. 7 dieser ICC fielen über 0 aus, der höchste war .09 in der IPS-Skala *Rücksichtnahme bei sozialer Verantwortung*. Univariate Varianzanalysen mit den Kleingruppen als zufällige Effekte zeigte, dass alle ICC nur zufällig von 0 verschieden waren (alle $p > .07$).

Die Lehramtsstudierenden (davon 72.4 % Studentinnen) waren zu t2 zwischen 18 und 43 Jahre alt ($M = 23.9$, $SD = 4.9$). Die Geschlechterverteilung war vergleichbar mit der Verteilung innerhalb der neu zugelassenen Studierenden des betreffenden Studienjahrs (Studentinnen/Studenten ~ 70.8%/29.2 %, $\chi^2[1] = 1.4$, $p = .71$). Das Geschlecht war nicht mit den Bedingungen ($\chi^2[2] = 1.18$, $p = .56$, $V = .1$) und nicht mit dem Alter ($F[2,113] = 0.44$, $p = .65$, $\eta^2 < .01$) konfundiert. Alle Studierenden waren im 2. Semester ihres jeweiligen Studiums. Der Studienfortschritt war damit konstant gehalten. Schließlich zeigte sich auch für den Studiengang keine Konfundierung mit den Bedingungen ($\chi^2[2] = 0.39$, $p = .82$, $V = .06$): 55 Lehramtsstudierende nahmen aus dem Sekundar-Studiengang (= 47.4 %) und 61 aus dem Primar-Studiengang (= 52.6 %) teil.

3.4 Statistische Auswertung

Das Ausmaß an Faking wurde über die Mittelwertunterschiede operationalisiert. Die Hypothesen dazu wurden varianzanalytisch geprüft. Hierfür war die Wechselwirkung zwischen Zeitpunkt und Bedingung von Interesse. Die Haupteffekte waren weder für die Hypothesen relevant, noch sollten sie bei einer hypothesenkondenformen Wechselwirkung interpretiert werden, da diese hybrid wäre. Die Normalverteilung der Stichprobenmittelwerte wurde wegen der Stichprobengrößen angenommen, die Varianzhomogenität kann bei vergleichbarer Gruppengröße vernachlässigt werden und Sphärizität ist erst ab drei Messzeitpunkten relevant (Bortz & Schuster, 2010).

Um eine α -Fehler-Inflation zu vermeiden, wurde so global wie möglich und bei gegebenen Effekten immer ein Schritt lokaler getestet: zuerst eine MANOVA über alle

¹ Die internen Konsistenzen jeder Bedingung zur Nacherhebung sind im Elektronischen Supplement Tabelle 2–4 angeführt.

² Um Hinweise auf Selektionseffekte zu liefern, wurde verglichen, ob die vorliegenden 116 Personen sich von den 563 Bewerbenden in den IPS-Skalen unterschieden. Die mittlere Stärke des Effekts war $d = 0.08$. Einzig der höchste Effekt ($d = 0.2$) war signifikant ($t[115] = 2.33$, $p = .02$).

IPS-Skalen, bei signifikanter Wechselwirkung anschließend ANOVAs für die einzelnen IPS-Skalen. Für Skalen mit signifikanter Wechselwirkung wurde der Tukey HSD Posttest, ein konservativer Posttest (Field, Miles & Field, 2012), durchgeführt. Im Posttest wurden zuerst die Bedingungen zu t1 verglichen, anschließend die Bedingungen zwischen t1 und t2. Nach diesem Vergleich innerhalb der Bedingungen wurde noch ein Vergleich zwischen den Bedingungen zu t2 vorgenommen. Es wurde anhand der *honest*-Bedingung zu t2 errechnet, wo der Cut-off in den jeweiligen IPS-Skalen für zwei Selektionsquoten (50 % und 70 %) sein müsste. Für diese Cut-offs wurde dann geprüft, wie viel Prozent der Lehramtsstudierenden in den drei Bedingungen aufgenommen worden wären.

Für die Hypothese bezüglich des Einflusses von Faking auf die Rangreihen wurde das Ausmaß an Faking mit den Rangreihenkorrelationen in Beziehung gesetzt. Von Interesse war, wie sehr eine Veränderung im Ausmaß an Faking mit einer Veränderung in der Höhe der Rangkorrelationen einherging. Das Ausmaß an Faking wurde über die Effektstärken der Mittelwertunterschiede zwischen t1 und t2 in den Bedingungen operationalisiert. Diese Effektstärken wurden mittels Korrelation in Beziehung zu den Rangreihenkorrelationen gesetzt.

Das übliche Signifikanzniveau von 5 % wurde verwendet, da es an Studien mangelt, aus welchen zu erwartende Effektstärken von Faking für Lehramtsstudienplätze im IPS ableitbar wären. Das Signifikanzniveau wurde angepasst, um ggf. einem α -Fehler bzw. β -Fehler entgegenzuwirken. Dies betraf den Posttestvergleich zwischen den Bedingungen zu t1 als auch die Rangreihenkorrelationen. Jener Posttestvergleich prüfte, ob keine Unterschiede bestanden. Daher sollte das Signifikanzniveau erhöht werden, um damit den β -Fehler zu verringern. Es wurde auf 10 % gesetzt (Bortz und Schuster [2010] schlagen 25 % vor, jedoch für einen einzigen Vergleich, was bei vorliegender Anzahl an Einzelvergleichen zu liberal wäre). Für die Rangreihenkorrelationen wurde das Signifikanzniveau auf 1 % gesenkt, aufgrund deren Anzahl und dem damit einhergehenden α -Fehler.

4 Ergebnisse

Die Mittelwerte und Standardabweichungen aller IPS-Skalen in den drei Bedingungen zu t1 und zu t2 sind in Tabelle 1 angeführt. Die Interkorrelationen zu t1 fielen zwischen $-.49$ und $.63$ aus.³ Die Höhe der Korrelationen wurde nach Bortz und Döring (2006) klassifiziert: 4 Korrelationen waren vernachlässigbar ($r < .1$), 67 klein ($.1 \leq r < .3$), 73 mittel ($.3 \leq r < .5$) und 9 groß ($r \geq .5$). Die Interkorrelationen bestätigten, dass die 18 IPS-Skalen nicht 18 getrennte Konstrukte waren. Es war daher zulässig, über

die IPS-Skalen hinweg multivariat zu testen (Backhaus, Erichson, Plinke & Weiber, 2006). Hierfür zeigte sich eine signifikante Wechselwirkung ($F[36,194] = 3.87, p < .01, \lambda = .26, \eta^2 = .42$) mit signifikanten Haupteffekten (Bedingung: $F[36,194] = 1.9, p < .01, \lambda = .53, \eta^2 = .26$; t1–t2: $F[18,96] = 3.81, p < .01, \lambda = .58, \eta^2 = .42$). Anschließend wurden die einzelnen IPS-Skalen analysiert. Alle Wechselwirkungen waren signifikant mit dem geringsten Effekt bei *Rücksichtnahme bei sozialer Verantwortung* ($F[2,113] = 5.35, p < .01, \eta^2 = .08$) und dem größten bei *Zufriedenheit mit dem Leistungsverhalten* ($F[2,113] = 59, p < .01, \eta^2 = .51$). Bei diesen beiden Skalen war der Haupteffekt der Bedingung einmal nicht signifikant und einmal signifikant ($F[2,113] = 1.48, p = .23, \eta^2 = .03$ bzw. $F[2,113] = 8.28, p < .01, \eta^2 = .13$). Der zweite Haupteffekt war bei beiden Skalen nicht signifikant ($F[1,113] = 1.75, p = .19, \eta^2 = .02$ bzw. $F[1,113] < 0.01, p = .96, \eta^2 < .01$). Die Posttests fanden für 14 der 18 IPS-Skalen keine Unterschiede der Bedingungen zu t1 (alle $p > .19$, alle $d < 0.28$). In den vier IPS-Skalen *Entspannungsfähigkeit nach dem Arbeitstag*, *Stabilität bei stressvoller Anforderung*, *Zufriedenheit mit dem Gesundheits- und Erholungsverhalten* bzw. *Zufriedenheit mit dem Leistungsverhalten* unterschieden sich die Bedingungen *Faking-good* und *honest* bereits zu t1 ($p = .07, = .02, = .08$ bzw. $= .09$); *Faking-good* war stets niedriger als *honest* ($d = 0.42, = 0.60, = 0.42$ bzw. $= 0.37$). Zusätzlich zeigte sich bei *Stabilität bei stressvoller Anforderung* ein Unterschied zwischen der *wie-damals*- und der *honest*-Bedingung ($p = .096, d = 0.43$). Danach wurden t1 und t2 innerhalb der Bedingungen verglichen (s. Tabelle 1 für p -Werte und Effektstärken). Die Vorzeichen der Effektstärken fielen für alle IPS-Skalen hypothesenkonform aus: In der *honest*- und *wie-damals*-Bedingung war t1 vorteilhafter ausgeprägt als t2; in der *Faking-good*-Bedingung war t2 vorteilhafter als t1. In der *honest*-Bedingung gab es Effekte zwischen $d = -0.27$ und $d = -1.87$, mit dem größten Effekt bei *Stabilität bei stressvoller Anforderung*. In der *wie-damals*-Bedingung zeigten sich Effekte zwischen $d = -0.09$ und $d = -0.91$, mit dem größten Effekt bei *Zufriedenheit mit dem Leistungsverhalten*. In der *Faking-good*-Bedingung betrugen die Effekte zwischen $d = 0.35$ und $d = 1.73$, mit dem größten Effekt bei *Selbstvertrauen bei Prüfungsanforderung*. Diese Unterschiede waren in 16, 11 bzw. 17 IPS-Skalen der *honest*-, *wie-damals*- bzw. *Faking-good*-Bedingung signifikant.

Nach diesem Vergleich innerhalb der Bedingungen wurden die Bedingungen zu t2 verglichen. Es wurde für jede IPS-Skala anhand der *honest*-Bedingung ermittelt, wo Cut-offs zu setzen wären, um 50 % bzw. 70 % zu selektieren. Anschließend wurde für diese Cut-offs geprüft, wie viel Prozent der Studierenden in der *wie-damals*- und *Faking-good*-Bedingung aufgenommen worden wären (s. Tabelle 1). Z. B. in der IPS-Skala *Aktivität in vertrauter*

3 Alle Interkorrelationen sind im Elektronischen Supplement angeführt. Siehe Tabelle 1 für die Interkorrelationen zu t1 und Tabelle 2–4 für die Interkorrelationen innerhalb der Bedingungen zu t1 und t2.

Tabelle 1

Mittelwert (M) und Standardabweichung (SD) der IPS-Skalen (Itemanzahl und Cronbachs α zu t1 in Klammer nach der Skalenbezeichnung) zur Zulassung (t1) und Nacherhebung (t2) in den drei Bedingungen (honest, wie-damals und Faking-good). Für den t1–t2-Vergleich sind Cohens d (d) und die Signifikanz nach Tukey HSD ($p(T)$) angegeben, für den t1–t2-Zusammenhang deren Rangkorrelation (r) und Signifikanz ($p(r)$). Für zwei Selektionsquoten (50 % und 70 %) ist angegeben, wie viel Prozent bei einem entsprechenden Cut-off der honest-Bedingung in den drei Bedingungen aufgenommen worden wären.

	t1		t2							
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>d</i>	<i>p</i> (<i>T</i>)	<i>r</i>	<i>p</i> (<i>r</i>)	50 %	70 %
Aktivität in vertrauter kommunikativer Situation (5; $\alpha = .67$)										
honest	3.49	0.31	3.31	0.52	−0.58	< .01	.45	< .01	44	19
wie-damals	3.47	0.34	3.35	0.37	−0.61	< .05	.69	< .01	44	15
Faking-good	3.49	0.34	3.66	0.30	0.57	< .01	.13	.41	73	49
Beharrungstendenz bei Umstellungserfordernis (5; $\alpha = .58$)										
honest	1.88	0.36	2.23	0.45	1.01	< .01	.33	.05	36	28
wie-damals	1.91	0.42	2.13	0.50	0.56	< .01	.24	.14	51	36
Faking-good	1.92	0.48	1.43	0.49	−1.36	< .01	.36	.02	95	88
Durchsetzung in einer Führungsrolle (5; $\alpha = .71$)										
honest	3.41	0.36	3.29	0.43	−0.47	.11	.70	< .01	31	22
wie-damals	3.40	0.43	3.29	0.46	−0.37	.10	.59	< .01	33	26
Faking-good	3.39	0.44	3.66	0.41	0.81	< .01	.38	.02	73	59
Empfindlichkeit bei sozialer Frustration (6; $\alpha = .60$)										
honest	1.96	0.33	2.32	0.50	1.13	< .01	.48	< .01	42	25
wie-damals	2.06	0.49	2.10	0.45	0.15	.54	.57	< .01	69	59
Faking-good	1.93	0.43	1.45	0.39	−1.56	< .01	.39	.01	98	98
Engagement bei hoher Leistungsanforderung (5; $\alpha = .79$)										
honest	3.09	0.50	2.79	0.65	−0.72	< .01	.52	< .01	47	28
wie-damals	3.11	0.47	2.87	0.44	−0.78	.02	.58	< .01	49	15
Faking-good	3.04	0.62	3.65	0.61	1.03	< .01	−.09	.56	93	85
Entspannungsfähigkeit nach dem Arbeitstag (5; $\alpha = .64$)										
honest	3.52	0.36	3.27	0.49	−0.77	< .01	.53	< .01	36	17
wie-damals	3.43	0.41	3.30	0.42	−0.42	.06	.46	< .01	38	10
Faking-good	3.36	0.40	3.81	0.23	1.62	< .01	.31	.05	88	71
Aktives Erholungsverhalten in der Freizeit (5; $\alpha = .56$)										
honest	3.65	0.24	3.57	0.43	−0.27	.21	.35	.04	47	25
wie-damals	3.58	0.37	3.56	0.42	−0.10	.67	.67	< .01	49	28
Faking-good	3.64	0.35	3.91	0.18	1.05	< .01	.25	.12	88	73
Gesundheitsvorsorge bei Warnsignalen (5; $\alpha = .69$)										
honest	3.57	0.37	3.11	0.45	−1.44	< .01	.17	.32	42	19
wie-damals	3.49	0.46	3.21	0.63	−0.83	< .01	.61	< .01	49	36
Faking-good	3.53	0.38	3.82	0.30	1.11	< .01	.39	.01	93	90
Karriere- und Risikobereitschaft bei beruflicher Herausforderung (5; $\alpha = .79$)										
honest	3.57	0.34	3.25	0.60	−0.97	< .01	.60	< .01	42	22
wie-damals	3.48	0.42	3.25	0.47	−0.63	< .01	.45	< .01	36	18
Faking-good	3.52	0.44	3.79	0.35	0.85	< .01	.51	< .01	83	71
Konfrontationstendenz in sozialer Konfliktsituation (9; $\alpha = .66$)										
honest	1.53	0.29	1.90	0.51	1.33	< .01	.67	< .01	47	25
wie-damals	1.50	0.34	1.67	0.35	0.78	< .01	.64	< .01	69	33
Faking-good	1.51	0.32	1.21	0.35	−0.98	< .01	.45	< .01	98	93

Tabelle 1

Mittelwert (M) und Standardabweichung (SD) der IPS-Skalen (Itemanzahl und Cronbachs α zu t1 in Klammer nach der Skalenbezeichnung) zur Zulassung (t1) und Nacherhebung (t2) in den drei Bedingungen (honest, wie-damals und Faking-good). Für den t1–t2-Vergleich sind Cohens d (d) und die Signifikanz nach Tukey HSD ($p(T)$) angegeben, für den t1–t2-Zusammenhang deren Rangkorrelation (r) und Signifikanz ($p(r)$). Für zwei Selektionsquoten (50 % und 70 %) ist angegeben, wie viel Prozent bei einem entsprechenden Cut-off der honest-Bedingung in den drei Bedingungen aufgenommen worden wären. (Fortsetzung)

	t1		t2							
	M	SD	M	SD	d	p(T)	r	p(r)	50 %	70 %
Optimismus gegenüber alltäglicher Anforderung (5; $\alpha = .66$)										
honest	3.72	0.32	3.21	0.47	–1.49	< .01	.27	.11	36	17
wie-damals	3.66	0.34	3.53	0.37	–0.44	.05	.34	.03	77	36
Faking-good	3.62	0.33	3.92	0.18	1.18	< .01	.20	.21	100	85
Rücksichtnahme bei sozialer Verantwortung (5; $\alpha = .66$)										
honest	3.72	0.33	3.58	0.36	–0.68	.01	.57	< .01	42	22
wie-damals	3.62	0.38	3.53	0.43	–0.38	.13	.69	< .01	49	23
Faking-good	3.65	0.32	3.75	0.40	0.35	.06	.50	< .01	76	59
Selbstbehauptung bei Kommunikationserfordernis (5; $\alpha = .84$)										
honest	3.38	0.45	2.99	0.57	–1.17	< .01	.58	< .01	47	14
wie-damals	3.35	0.54	3.09	0.61	–0.63	< .01	.46	< .01	51	31
Faking-good	3.42	0.47	3.88	0.25	1.33	< .01	.19	.24	98	93
Selbstvertrauen bei Prüfungsanforderung (5; $\alpha = .76$)										
honest	3.37	0.39	3.11	0.61	–0.65	< .01	.50	< .01	44	19
wie-damals	3.33	0.50	3.30	0.51	–0.09	.72	.54	< .01	64	23
Faking-good	3.26	0.54	3.90	0.20	1.73	< .01	.19	.23	100	88
Stabilität bei stressvoller Anforderung (5; $\alpha = .77$)										
honest	3.28	0.44	2.56	0.62	–1.87	< .01	.48	< .01	39	25
wie-damals	3.07	0.52	2.77	0.54	–0.71	< .01	.28	.09	56	28
Faking-good	2.99	0.53	3.60	0.56	1.17	< .01	.24	.14	93	90
Zufriedenheit mit dem Gesundheits- und Erholungsverhalten (6; $\alpha = .57$)										
honest	4.50	0.45	4.12	0.42	–1.14	< .01	.43	< .01	44	19
wie-damals	4.44	0.60	4.29	0.63	–0.42	.06	.60	< .01	62	46
Faking-good	4.30	0.49	4.80	0.31	1.47	< .01	.20	.23	93	85
Zufriedenheit mit dem Leistungsverhalten (6; $\alpha = .69$)										
honest	4.22	0.49	3.80	0.49	–1.35	< .01	.58	< .01	42	22
wie-damals	4.16	0.49	3.93	0.49	–0.91	< .01	.74	< .01	49	21
Faking-good	4.03	0.50	4.69	0.45	1.62	< .01	.19	.23	90	85
Zufriedenheit mit dem sozial-kommunikativen Verhalten (3; $\alpha = .69$)										
honest	4.30	0.36	3.98	0.40	–1.12	< .01	.49	< .01	47	28
wie-damals	4.21	0.42	4.04	0.54	–0.58	.02	.64	< .01	51	36
Faking-good	4.20	0.50	4.68	0.48	1.23	< .01	.28	.08	90	85

Anmerkung: Bei den drei Skalen zur Zufriedenheit lag die Spannweite zwischen 1 (sehr unzufrieden) und 5 (sehr zufrieden); bei allen übrigen Skalen zwischen 1 (stimmt gar nicht) und 4 (stimmt genau).

kommunikativer Situation wären mit einem Cut-off um 50 % zu selektieren in der honest-Bedingung 44 %, in der wie-damals-Bedingung 44 % und in der Faking-good-Bedingung 73 % aufgenommen worden. In allen anderen Skalen war dieser Prozentsatz deskriptiv betrachtet gleich hoch oder noch höher. Der höchste zeigte sich bei Opti-

mismus gegenüber alltäglicher Anforderung und Selbstvertrauen bei Prüfungsanforderung, wo 100 % der Faking-good-Bedingung aufgenommen worden wären. Bei einer Selektion von 70 % wären (in den weiteren Skalen) 49–98 % der Faking-good-Bedingung aufgenommen worden. Bei 50 % Selektion lag der Prozentsatz der wie-da-

mals-Bedingung bis auf eine Ausnahme immer zwischen der *honest*- und *Faking-good*-Bedingung. Bei 70 % Selektion lag die *wie-damals*-Bedingung sogar in 5 IPS-Skalen knapp unter der *honest*-Bedingung.

Im letzten Schritt wurde der Zusammenhang zwischen dem Faking und den Rangreihen analysiert. Zuerst wurden Rangkorrelationen zwischen t1 und t2 für jede IPS-Skala und Bedingung berechnet. Diese Rangkorrelationen sind in Tabelle 1 angeführt. Das Quadrat der Rangkorrelationen ist ein Effektstärkenmaß der Rangkorrelationen (Bortz & Döring, 2006). In der *honest*-, *wie-damals*- bzw. *Faking-good*-Bedingung zeigten sich in 14, 15 bzw. 3 IPS-Skalen signifikante Rangkorrelationen zwischen t1 und t2.

Anschließend wurde überprüft, ob es einen Zusammenhang zwischen dem mittleren Ausmaß an Faking und dem Erhalt der Rangreihe von t1 zu t2 gab: Die Rangkorrelationen wurden mit den Effektstärken des Vergleichs von t1 zu t2 in den Bedingungen in Beziehung gesetzt. So zeigten sich z.B. in den drei Bedingungen von *Aktivität in vertrauter kommunikativer Situation* Effektstärken von $d = -0.58$, $d = -0.61$ bzw. $d = 0.57$ bei Rangkorrelationen von $r = .45$, $r = .69$ bzw. $r = .13$. Diese mittleren Ausmaße an Faking (= Beträge der Effektstärken) wurden mit den Rangkorrelationen korreliert. Je höher das mittlere Ausmaß an Faking war, desto geringer fiel auch die Rangkorrelation aus ($r = -.43$, $p < .01$, $R^2 = .19$).

5 Diskussion

In der vorliegenden Studie wurde an einem Zulassungsverfahren für Lehramtsstudien der Einfluss von Faking auf Merkmalsausprägung und Rangreihe eines selbstbeschreibenden Persönlichkeitsinventars, das IPS, untersucht. Die Aufforderung, sich so geeignet wie möglich darzustellen, um ein Lehramtsstudium beginnen zu dürfen, bewirkte bis auf eine Ausnahme in allen IPS-Skalen, dass die Bewerbenden sich noch vorteilhafter als beim Zulassungsverfahren darstellten. Beim Zulassungsverfahren jedoch stellten sie sich bereits in allen bis auf zwei IPS-Skalen vorteilhafter dar, als sie es eigentlich waren. Diese vorteilhaftere Selbstdarstellung beim Zulassungsverfahren trat im Ausmaß von $d = 0.58$ bis $d = 1.87$ auf, was ein höheres Maß an Verzerrung zeigt, als meta-analytisch für die Big Five in der Personalauswahl von Birkeland et al. (2006) berichtet wird ($0.19 \leq d \leq 0.45$). In all diesen 16 IPS-Skalen konnten die Lehramtsstudierenden ihre Werte jedoch bedeutsam noch weiter ins Positive verzerren ($d = 0.35$ bis $d = 1.73$).

Wenn Personen in Bewerbungssituationen zu Faking greifen, berücksichtigen sie, für welche Stelle (Birkeland et al., 2006) oder welches Studium (Ziegler, 2011) sie sich bewerben. Dies legt nahe, dass alle bis auf eine IPS-Skala – *Rücksichtnahme bei sozialer Verantwortung* – von den Lehramtsstudierenden als relevant für den Lehrerberuf angesehen wurden. Während dies für die Verwendung des IPS zur Auswahl künftiger Lehrkräfte spricht, spricht die auftretende Verzerrung gegen die Verwendung von Persön-

lichkeitsinventaren bei Zulassungsverfahren für Lehramtsstudien. Wiederrum dafür spricht, dass weniger stark zu Faking gegriffen wurde, als es möglich gewesen wäre.

Eine Verzerrung der mittleren Merkmalsausprägung ist vor allem relevant, wenn Bewerbende für Lehramtsstudien auf der Basis von Cut-offs ausgewählt werden. Die vorliegende Studie unterstreicht, wie gravierend absichtliches Faking eine Studierendenauswahl auf der Basis von Cut-offs verzerren kann. Betrachtet man nur einzelne Skalen, können Personen sich so vorteilhaft darstellen, dass weit aus mehr Personen als intendiert aufgenommen werden würden. Es ist daher stark davon abzuraten, Studierende nur auf Basis von einzelnen Persönlichkeitsdimensionen zu selektieren.

Für eine faire Auswahl ist das größte Problem eine Verzerrung der Rangreihe durch Faking. In der vorliegenden Studie veränderten sich die Rangreihen umso mehr, je stärker Faking auftrat. Wenn Lehramtsstudierende sich absichtlich so geeignet wie möglich darstellten, gab es nur mehr in 3 von 18 IPS-Skalen einen Zusammenhang zwischen der Rangreihe dieser Darstellung und der des Zulassungsverfahrens. Diese Befunde decken sich mit den Ergebnissen von Griffith et al. (2007) zu Gewissenhaftigkeit unter denselben Bedingungen ($r = .08$). Die Fairness der Zulassung zu einem Lehramtsstudium wäre daher nicht mehr gegeben, wenn Personen sich absichtlich so geeignet wie möglich darstellen. Jedoch zeigte sich in 14 von 18 der IPS-Skalen ein Zusammenhang der Rangreihen und einer ehrlichen Bearbeitung. Faking stellt damit auch bei Lehramtsstudien die Fairness von selbstbeschreibenden Verfahren in Zulassungsverfahren in Frage, jedoch scheint das natürlich auftretende Faking dies nur teilweise zu tun.

Fraglich ist, was in den beiden IPS-Skalen *Durchsetzung in einer Führungsrolle* und *Aktives Erholungsverhalten in der Freizeit* zwischen dem Zulassungsverfahren und der Nacherhebung geschah. In diesen beiden Skalen zeigten sich in der *honest*-Bedingung keine Unterschiede, während es in der *Faking-good*-Bedingung zu Unterschieden kam. Wahrscheinlich ist, dass sich die Wahrnehmung der Wichtigkeit dieser Merkmale verändert hat, z.B. durch die Ausbildung. Dies würde bedeuten, dass die *Durchsetzung in einer Führungsrolle* und das *Aktive Erholungsverhalten in der Freizeit* zum Zeitpunkt des Zulassungsverfahrens noch nicht als relevant für den Lehrerberuf erachtet wurden und sich im Laufe der Ausbildung diese Einschätzung veränderte. Es kann auch nicht ausgeschlossen werden, dass in der *honest*-Bedingung die Werte der Lehramtsstudierenden noch immer nicht die tatsächliche Merkmalsausprägung widerspiegeln. Die dadurch entstehende Unterschätzung des Fakings könnte diese Ergebnisse auch erklären.

Die Befunde zur Reproduzierbarkeit von Faking fielen nicht eindeutig aus. Eindeutig festgestellt werden konnte jedoch, dass Lehramtsstudierende ihr Faking beim Zulassungsverfahren nicht überschätzten. In allen IPS-Skalen, in denen das Faking nicht reproduziert werden konnte, lag eine Unterschätzung der Werte des Zulassungsverfahrens

vor. Diese Ergebnisse betonen den situativen Einfluss, der auch von Ellingson und McFarland (2011) postuliert wurde, und dass Faking ein sehr komplexer Prozess ist, dem viele Kognitionen zugrunde liegen (König et al., 2012).

Zu bedenken ist bei all diesen Befunden, dass Untersuchungen zu Zulassungsverfahren unter Varianzeinschränkung leiden (Linn, 1968). Für prädiktive Validitäten wirkt sich dies validitätsmindernd aus. Dadurch können die vorliegenden Zusammenhänge unterschätzt werden (Bortz & Döring, 2006), da jene Personen nicht Teil dieser Studie sein konnten, welche nicht zugelassen wurden. Es muss offenbleiben, inwiefern die gezeigten Zusammenhänge und Nichtzusammenhänge Unterschätzungen waren.

Die vorliegende Studie ist ein erster Schritt, um Faking von Persönlichkeitsmerkmalen bei Bewerbenden für Lehramtsstudien zu untersuchen. Es wurde nur mit den Persönlichkeitsmerkmalen des IPS, einer gegebenen Selektionsquote und Studierenden in ihrem ersten Studienjahr befasst. Die Ergebnisse können somit nicht darüber hinaus generalisiert werden. Es sollten in diesem Zusammenhang weitere Persönlichkeitsmerkmale untersucht werden. Außerdem sollte festgestellt werden, ob sich die vorliegenden Befunde bei anderen Selektionsquoten und höhersemestrigen Studierenden replizieren lassen. Da sich teilweise fragwürdige interne Konsistenzen zeigten, sollte für die gegebenen Persönlichkeitsmerkmale sichergestellt werden, dass sie in einem Zulassungsverfahren so gemessen werden, wie es intendiert ist. Es sollte auch an größeren Stichproben die Messinvarianz des Persönlichkeitsinventars überprüft werden. Weitere Studien sollten externe Kriterien wie bspw. objektiven und subjektiven Berufs- oder Studienerfolg heranziehen, um zu untersuchen, ob und ggf. wie Faking den Zusammenhang des IPS in Bezug auf diese Kriterien beeinflusst. Auch bleiben die Fragen offen, wie sehr reproduziertes Faking diese Zusammenhänge beeinflusst und ob es Umstände gibt, unter denen tatsächliches Faking besser oder schlechter reproduziert werden kann.

Elektronische Supplemente

Die elektronischen Supplemente sind mit der Online-Version dieses Artikels verfügbar unter <http://dx.doi.org/10.1024/1010-0652/a000161>.

ESM 1. Tabelle.

Die Interkorrelationen der 18 IPS-Skalen zur Zulassung (t1) für die gesamte Stichprobe

ESM 2. Tabelle.

Die Interkorrelationen der 18 IPS-Skalen zur Zulassung (oberes Dreieck) und Nacherhebung (unteres Dreieck) für die honest-Bedingung samt interner Konsistenz (α) zur Nacherhebung

ESM 3. Tabelle.

Die Interkorrelationen der 18 IPS-Skalen zur Zulassung (oberes Dreieck) und Nacherhebung (unteres Dreieck) für

die wie-damals-Bedingung samt interner Konsistenz (α) zur Nacherhebung

ESM 4. Tabelle.

Die Interkorrelationen der 18 IPS-Skalen zur Zulassung (oberes Dreieck) und Nacherhebung (unteres Dreieck) für die Faking-good-Bedingung samt interner Konsistenz (α) zur Nacherhebung

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**6.3 Study 3: The Psychometric Costs of Applicants' Faking:
Examining Measurement Invariance and Re-test
Correlations Across Response Conditions**

The Psychometric Costs of Applicants' Faking

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3 The Psychometric Costs of Applicants' Faking: Examining Measurement Invariance and Re-

4 Test Correlations Across Response Conditions

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Abstract

This study examines the stability of the response process and the rank-order of respondents responding to three personality scales in four different response conditions. Applicants to the University College of Teacher Education Styria (N=243) completed personality scales as part of their college admission process. Half a year later, they retook the same personality scales in one of three randomly assigned experimental response conditions: *honest*, *faking-good*, or *reproduce*. Longitudinal means and covariance structure analyses showed that applicants' response processes could be partially reproduced after half a year, while respondents seemed to rely on an honest response behavior as a frame of reference. Additionally, applicants' faking behavior and instructed faking (faking-good) caused differences in the latent re-test correlations and consistently affected measurement properties. The varying latent re-test correlations indicated that faking can distort respondents' rank-order and thus the fairness of subsequent selection decisions, depending on the kind of faking behavior. Instructed faking (faking-good) even affected weak measurement invariance, while applicants' faking behavior did not. Consequently, correlations with personality scales – which can be utilized for predictive validity – may be readily interpreted for applicants. Faking behavior also introduced a uniform bias, implying that the classically observed mean raw score differences may not be readily interpreted.

Keywords: faking behavior, applicants, incumbents, faking-good, measurement invariance

Introduction

Faking is a type of response bias wherein respondents distort their responses to personality scale items in order to be viewed more favorably (McFarland & Ryan, 2000). Several studies indicated that faking can affect the psychometric properties of personality scales (e.g. Hartman & Grubb, 2011; Miller & Ruggs, 2014; Zickar & Robie, 1999; Ziegler & Bühner, 2009). Furthermore, selection decisions based on personality scales may be detrimentally affected due to applicants differing in their propensity and intensity to fake (e.g. Birkeland, Manson, Kisamore, Brannick & Smith, 2006; Mueller-Hanson, Heggstad, & Thornton, 2003; Rosse, Stecher, Miller & Levin, 1998; Winkelspecht, Lewis & Thomas, 2006; Zickar & Robie, 1999). Therefore, it is important to examine how applicants fake personality scales in real life selection settings (Goffin & Boyd, 2009; Griffith & Peterson, 2011; Kuncel, Goldberg & Kiger, 2011). To this end, the current study examined four different response behaviors to personality scales: (1) a real life admission testing situation, (2) a classic honest-condition, (3) an instructed faking-good-condition, and (4) a condition prompting incumbents to reproduce the response behavior from their admission testing setting. The comparison of the psychometric characteristics of the personality scales across these four response conditions is expected to shed some light on the processes used by applicants to fake personality scales in real life selection settings.

Effects of Faking on Personality Scale Scores

Practitioners are mainly concerned about the effects of faking on applicants' personality scale scores, and the effects on subsequent selection decisions. The high stakes involved in some personnel selection and educational admission settings are likely to prompt some sort of faking behavior from at least some applicants (cf. Dilchert, Ones, Viswesvaran & Deller, 2006; Tett & Simonet, 2011). Several laboratory studies using an instructed faking-good-condition (i.e. instructions to "fake-good") indicate that respondents are capable of increasing their personality scales scores ($0.48 \leq d \leq 0.65$ in within-subject designs, and $0.47 \leq d \leq 0.93$ in between-subject designs: Viswesvaran & Ones, 1999). However, some scholars argue that these detrimental effects of faking are specific to laboratory studies, and that in real life selection settings effects of faking on applicants' personality scale scores are negligible (e.g. Bradley & Hauenstein, 2006; Hogan, Barrett & Hogan, 2007; Ones, Dilchert, Viswesvaran & Judge, 2007). This view has been partially supported by a recent meta-analysis indicating only negligible to small differences between applicants and incumbents ($0.11 \leq d \leq 0.45$; Birkeland et al., 2006). Unfortunately, the results of the meta-analysis cited above are

1 necessarily based on the assumption that strong measurement invariance is given across
2 applicants and incumbents. If strong measurement invariance is given, respondents with the
3 same standing on the latent trait(s) have equal expected item and/or test scores. Thus, mean
4 differences within and between groups of respondents (e.g. applicants vs. incumbents) can be
5 interpreted at face value (e.g. Millsap, 2011; Mislevy et al., 2013). By contrast, if strong
6 measurement invariance is violated, mean score differences reflect not only (latent) true score
7 differences between the groups and/or conditions, but additionally measurement bias (cf. Li
8 & Zumbo, 2009).

9 Studies examining re-test correlations of personality scales completed under different
10 response conditions offer some tentative evidence that faking may affect the psychometric
11 properties of personality scales. For example, Peterson, Griffith, Isaacson, O'Connell, and
12 Mangos (2011) re-tested applicants as incumbents, and reported a re-test correlation for
13 conscientiousness of $r = .62$. Similar results were obtained by Griffith, Chmielowski, and
14 Yoshita (2007), who reported a re-test correlation for conscientiousness of $r = .50$ for
15 applicants responding to a conscientiousness scale with and without motivation to fake. In
16 line with these findings, Hogan et al. (2007) reported re-test correlations for the Big Five
17 ranging from $r = .46$ to $r = .68$. In contrast to these findings, merely re-testing volunteers
18 generally yields higher re-test correlations (e.g. $.70 \leq r \leq .90$: Arendasy, Sommer &
19 Feldhammer, 2011; $.67 \leq r \leq .85$: McCrae, Yik, Trapnell, Bond & Paulhus, 1998; $.50 \leq r \leq$
20 $.85$: Hogan, 1992), as has already been pointed out by scholars (e.g. Hogan et al., 2007).
21 These lower than expected re-test correlations for applicants compared to volunteers may
22 indicate that faking compromises the measurement invariance of personality scales.

24 **Factors Affecting Faking on Personality Scale Items**

25 In order to interpret personality scale scores at face value, individual differences in the
26 respondents' personality scale scores should be entirely attributable to individual differences
27 in the respondents' standing on the latent trait measured (e.g. Millsap, 2011; Mislevy et al.,
28 2013). This implies that respondents' standing on the latent trait should be the only individual
29 differences construct having a causal effect on their responses to the personality scale items.
30 However, several studies indicate that applicants consider a wide range of aspects when
31 deciding which response category to endorse on personality scale items (e.g. Donovan,
32 Dwight & Hurtz, 2003; König, Merz & Trauffer, 2012; Kuncel & Tellegen, 2009; Ziegler,
33 2011). For instance, applicants evaluate the relevance of individual items for a particular
34 position or education when considering faking their responses (cf. König et al., 2012; Ziegler,

2011). Thus, their responses do not only depend anymore on their standing on the latent trait, but also on their perceived relevance of the items, and their willingness to fake.

Several theoretical models have been proposed to explain why and how applicants fake in real life selection settings (e.g. Goffin & Boyd, 2009; McFarland & Ryan, 2000, 2006; Marcus, 2009; Mueller-Hanson, Heggstad, & Thornton, 2006; Snell, Sydell & Lueke, 1999). Despite various differences, all theoretical models cited above distinguish between dispositional and situational antecedents of applicants' faking behavior. Dispositional antecedents refer to individual differences, e.g. regarding honesty, or to appraisals of the relevance of personality traits for a particular job. On the other hand, situational antecedents refer to more fluctuating characteristics of the assessment situation, such as the perceived attractiveness of the job or the perceived competition level. In principle, these theoretical models can also be applied to an instructed faking-good-condition in a laboratory. For instance, when instructed to make a favorable impression for a particular job, one may expect that respondents consider the perceived relevance of personality scale items for this particular job. However, individual differences in the accuracy of the perceived relevance may be larger in the laboratory than in real life selection settings. By contrast, individual differences in respondents' willingness to fake and its antecedents may be less pronounced in laboratory settings than in a real life selection setting. This raises the question whether personality scales can be assumed to exhibit measurement invariance across various response conditions (faking-good, honest, applicants, etc.).

Effects of Faking on Measurement Properties and Test Scores

Researchers have found it useful to distinguish four levels of measurement invariance (cf. Cheung & Rensvold, 2002; Millsap, 2011; Mislevy et al., 2013; Raykov, Marcoulides, & Li, 2012; Vandenberg & Lance, 2000). Each level builds on the prior level, and they are tested level by level. The lowest level is *configural measurement invariance*. The second level is *weak measurement invariance*. The third level is *strong measurement invariance*. The final level is *strict measurement invariance*. Faking behavior can theoretically affect personality scales on each level of measurement invariance, and personality test scores, which will be outlined below.

Effects of faking on the levels of measurement invariance

Configural measurement invariance only assumes the general factor structure of personality scales to be equal across response conditions. Configural measurement invariance

1 would be violated when a personality scale measures one latent trait in one response
2 condition, but more than one latent trait in another response condition. For example, consider
3 a questionnaire assessing the Big Five domain factor Extraversion. The questionnaire may
4 measure extraversion for incumbents. However, for applicants it may measure extraversion
5 and faking intensity. In this case, configural measurement invariance would be violated.
6 Consequently, scores of incumbents and applicants could not be directly compared in a
7 meaningful way (e.g. Cheung & Rensvold, 2002; Millsap, 2011; Mislevy et al., 2013;
8 Vandenberg & Lance, 2000).

9 *Weak measurement invariance* additionally assumes the items to be equally saturated
10 by the latent trait they have been intended to measure across response conditions. Weak
11 measurement invariance would be violated when an item reflects its latent trait to a different
12 extent across response conditions. For example, consider the extraversion questionnaire again
13 and an applicant for a teacher vacancy. Now, let one item be "I like to stay in contact with
14 parents." Applicants may appraise this item as an indicator of their extraversion. For
15 incumbents by contrast, this item content may be a necessary part of their job, and thus the
16 item may be only moderately related to their extraversion. In this example, the item would
17 reflect individual differences in extraversion for applicants, but would do so less in a sample
18 of incumbents. The violation of weak measurement invariance also indicates that a
19 nonuniform bias is present (for a detailed discussion of nonuniform bias cf. Barendse, Oort &
20 Garst, 2010; Penfield & Camilli, 2007). Furthermore, weak measurement invariance across
21 response conditions is necessary to compare correlations within one response condition to
22 correlations within another response condition (Chen, 2008).

23 *Strong measurement invariance* additionally assumes the baseline probability of
24 endorsing a specific response category to be equal across response conditions. Strong
25 measurement invariance would be violated when respondents with the same standing on a
26 latent trait have unequal item scores across different response conditions. Consider a
27 respondent who moderately likes "staying in contact with parents". As an applicant for a
28 teacher vacancy however, s/he may indicate to like "staying in contact with parents" to a high
29 degree. With faking-good instructions for a teacher vacancy, s/he may even state to extremely
30 like "staying in contact with parents". In this example, strong measurement invariance would
31 be violated. This level of measurement invariance is necessary to interpret mean score
32 differences at face value. If it is not given, a uniform bias is present (for a detailed discussion
33 of uniform bias cf. Barendse, et al., 2010; Penfield & Camilli, 2007). This implies that mean
34 score differences do not only reflect mean individual differences, but also measurement bias

1 (cf. Chen, 2008; Cheung & Rensvold, 2002; Millsap, 2011; Li & Zumbo, 2009).

2 *Strict measurement invariance* finally also assumes both the systematic variance not
3 explained by the psychometric model, and the unsystematic variance of the measurement
4 error to be equal across response conditions. Strict measurement invariance would be violated
5 when responses are affected by their latent trait and situational factors. For example,
6 applicants may also consider how badly they need a job when responding to items. This
7 would introduce systematic variance in the item responses unaccounted for by the
8 psychometric model. An instruction to fake-good could reverse this effect again, as all
9 respondents might respond as if they desperately needed the job.

10

11 **Effects of faking on latent means and variances estimates**

12 Faking may affect the levels of measurement invariance, but also the latent means
13 and/or variances of personality scales. When responding as applicants trying to qualify for a
14 position, respondents may try to elevate their scores by endorsing higher response categories.
15 If this is done equally for all items of a scale, then an increase in the latent mean will result.
16 However, when raising scores, initially lower scores can be inflated by a greater extent. So
17 respondents with a lower standing on the latent trait can raise their scores to a greater extent
18 than respondents with an already higher standing on the latent trait (e.g. Goffin & Boyd,
19 2009; McFarland & Ryan, 2000, 2006). This in turn can lead to a decrease of the variance of
20 the individual differences construct measured.

21 The extent to which faking affects the latent trait means and variances of a personality
22 scale also depends on how homogeneously faking affects the items. Consider for example
23 items of a personality scale differing in their perceived relevance for a position. If
24 respondents choose to fake, the extent to which they will increase their scores will differ
25 across these items. This in turn will lead to a violation of strong measurement invariance due
26 to a uniform measurement bias (cf. Barendse, et al., 2010; Penfield & Camilli, 2007). In
27 contrast, if the responses to all items of one scale are elevated by the same extent, faking
28 would increase the latent trait mean. Such a response behavior could not be distinguished
29 from truly having a higher standing on the latent trait. Note that these two possible outcomes
30 are not mutually exclusive. For instance, respondents may perceive all items of a personality
31 scale as being relevant for a position, and still perceive selected items as being particularly
32 relevant. In this case, the baseline probabilities of endorsing a specific response category
33 would be globally higher, while being even higher for some items. This global increase
34 would generalize to the latent mean, while the additional increase for selected items would

1 violate strong measurement invariance. As a consequence, one would expect to observe a
2 shift of the latent mean, and a lack of strong measurement invariance.

3 In line with these considerations, studies examining the level of measurement
4 invariance across honest- and faking-good-conditions yield results ranging from configural
5 measurement invariance across these two conditions (e.g. Miller & Ruggs, 2014), to weak
6 measurement invariance (e.g. Zickar & Robie, 1999), up to strong measurement invariance
7 (Ferrando & Anguiano-Carrasco, 2009a; 2009b). The inconsistent findings in these studies
8 may be attributable to (1) the specific personality constructs measured and its perceived
9 relevance for the position (e.g. psychoticism: Ferrando & Anguiano-Carrasco, 2009a;
10 impression management: Miller & Ruggs, 2014), (2) the samples of respondents used in these
11 studies (e.g. military: Zickar & Robie, 1999; undergraduate business class students: Miller &
12 Ruggs, 2014), and/or (3) differences in test design characteristics such as item presentation
13 mode (e.g. blocked: Ferrando & Anguiano-Carrasco, 2009a; random: Zickar & Robie, 1999).

14 Unfortunately, studies examining measurement invariance across real life selection
15 settings and other response conditions are sparse. Robie, Zickar, and Schmit (2001) examined
16 measurement invariance across applicants for a sales manager position and job incumbents.
17 The authors reported strict measurement invariance for five out of six personality scales
18 examined. The one scale for which strict measurement invariance was violated exhibited a
19 measurement bias for two items: one item favored applicants, while one item favored
20 incumbents. Based on these mixed results, the authors argued for bias cancelation. Taken
21 together, the currently available empirical evidence suggests that faking may affect the level
22 of measurement invariance of personality scales. However, the extent to which this is the case
23 seems to depend on the types of faking behavior compared (i.e. faking-good or applicants'
24 faking), the position in question, and the characteristics of the personality scales used.

25

26 **Stability of Applicants' Faking Behavior**

27 Although several studies examined measurement invariance and mean raw score
28 differences across various response conditions, little is known about the stability of individual
29 differences in applicants' response behavior. Stability of response behavior has two important
30 aspects, which need to be distinguished: (1) the stability of the response process, and (2) the
31 stability of the rank-order of the respondents. The former can be addressed by examining
32 measurement invariance, while the latter can be addressed by re-test correlations. Both
33 aspects only partially build on each other. When configural or weak measurement invariance
34 have been violated across response conditions, estimates of the re-test correlations within

1 response conditions may be biased and therefore cannot be directly compared to each other.
2 By contrast, when weak measurement invariance across response conditions holds, re-test
3 correlations can be readily interpreted and compared across pairs of response conditions
4 (Chen, 2008). Unfortunately, neither weak nor strong measurement invariance guarantees
5 that the rank-order of respondents is preserved across response conditions. Measurement
6 invariance merely indicates the extent to which the response process can be assumed to be
7 invariant across response conditions (i.e. stability of the response process). For instance,
8 Zickar and Robie (1999) outlined that faking may only result in a shift of the latent trait
9 without affecting strong or strict measurement invariance. According to this model,
10 applicants should fake personality scales in selection settings by imagining a more favorable
11 version of themselves. If this model has merit, one would expect the response process to the
12 personality scales to be identical across e.g. honest responding and real life selection settings.
13 However, since respondents differ in their propensity and intensity to fake (e.g. Birkeland et
14 al., 2006; Mueller-Hanson et al., 2003; Rosse et al., 1998; Winkelspecht et al., 2006; Zickar
15 & Robie, 1999), the re-test correlations of personality scales across these two response
16 conditions may still be detrimentally affected. This may explain why re-testing applicants
17 generally yields smaller re-test correlations than re-testing volunteers (cf. Arendasy et al.,
18 2011; Griffin & Wilson, 2012; Griffith et al., 2007; Hogan, 1992; Hogan et al., 2007; McCrae
19 et al., 1998; Peterson et al., 2011). This may also question the interpretation of test scores
20 obtained from incumbents who were instructed to respond like applicants (e.g. Fell & König,
21 2016). Therefore, it is important to examine both aspects of stability separately.
22 Unfortunately, this has not been done so far.

23

24 **Description of the Current Study**

25 The present study examined the level of measurement invariance of three personality
26 scales in a real life admission testing situation across four different response conditions. This
27 was done in a combined within-subject and between-subject design. The research design is
28 illustrated in Figure 1. At the first time point of measurement (selection, t1), respondents
29 completed the personality scales as part of their admission testing process for studying at the
30 University College of Teacher Education Styria. The personality scales of interest were one
31 part of their admission process. Half a year later (re-test, t2), the personality scales were
32 given for a second time to the applicants, who were by now incumbents. The incumbents
33 were randomly assigned to one of the three response conditions, which only differed in the
34 instructions of the personality scales. The first response condition was the *honest-condition*,

1 for which incumbents were given the personality scales with the standard instructions. The
2 second response condition was the *reproduce-condition*, for which incumbents were
3 prompted to recall the situation of their selection process and to respond to the items as they
4 responded when they first took the admission test. The third response condition was the
5 *faking-good-condition*, for which incumbents were instructed to portray themselves as
6 favorably as possible in order to be admitted to the College University of Teacher Education
7 Styria. This specific research design was chosen because a combination of a real life selection
8 setting and a within-subject design has been argued to constitute a golden standard in faking
9 research (e.g. Komar, Brown, Komar & Robie, 2008; Peterson et al., 2011; Ryan & Boyce,
10 2006). Furthermore, the research design allowed us to examine both aspects of stability.

11 < Insert Figure 1 here >

13 Research Hypotheses

14 For respondents in the honest-condition, we examined measurement invariance across
15 the real life admission testing setting and the laboratory honest responses. Scholars (e.g.
16 Bradley & Hauenstein, 2006; Hogan et al, 2007; Ones et al., 2007) claimed that applicants'
17 faking behavior only negligibly affects their personality test scores. In line with this
18 hypothesis, strict measurement invariance was found across applicants and incumbents
19 (Robie et al., 2001). Therefore, we hypothesized to find strict measurement invariance across
20 applicants and incumbents instructed to respond honestly (hypothesis 1). However, previous
21 studies indicated that faking distorts applicants' rank order (e.g. Mueller-Hanson et al., 2003;
22 Rosse et al., 1998; Winkelspecht et al., 2006). Consistent with this hypothesis, moderate re-
23 test correlations were found between applicants and incumbents (e.g. Griffin & Wilson, 2012;
24 Griffith et al., 2007; Peterson et al., 2011). We therefore hypothesized to observe moderate
25 re-test correlations between the real life admission testing setting and the honest responses
26 (hypothesis 2). Lastly, samples of applicants have exhibited higher mean scores than samples
27 of incumbents, although these effects were usually small (Birkeland et al., 2006). Therefore,
28 we also hypothesized higher latent means of the applicants compared to the incumbents in
29 our study (hypothesis 3).

30 The next condition compared applicants' response behavior in the real life admission
31 testing situation to the response behavior in an instructed faking-good-condition. Instructed
32 faking differs from applicants' faking, as all respondents can be assumed to fake
33 irrespectively of individual differences in faking propensity (cf. Robie et al., 2001). Based on
34 previous studies, we expected to observe at least configural measurement invariance (cf.

1 Miller & Ruggs, 2014). Since some studies (cf. Ferrando & Anguiano-Carrasco, 2009a;
2 2009b) also observed strong measurement invariance, we hypothesized that it may even be
3 possible that strong measurement invariance holds across these two response conditions
4 (hypothesis 4). Furthermore, we hypothesized to find rather low re-test correlations between
5 these two response conditions, because the lack of individual differences in faking propensity
6 in the instructed faking-good-condition can be assumed to alter the rank-order of the
7 respondents (hypothesis 5). This prediction is also in line with previous studies examining the
8 re-test correlations between applicants and incumbents or volunteers instructed to fake-good
9 (cf. Ellingson, Sackett, & Hough, 1999; Griffith et al., 2007). For the same reason, instructed
10 faking-good should also lead to an increase in latent means compared to the applicants
11 (hypothesis 6).

12 The last condition examined the stability of applicants' response behavior across a real
13 life admission testing situation and the reproduced response behavior (reproduce-condition).
14 Based on the finding that the intensity and propensity of applicants' faking did not increase
15 when rejected applicants re-applied for the same job (Hogan et al., 2007), we hypothesized
16 applicants' faking behavior to be relatively stable. On the one hand, this should be reflected
17 in the stability of the response process and would imply strict measurement invariance for
18 incumbents attempting to reproduce their applicants' response behavior (hypothesis 7). On
19 the other hand, the re-test correlation should also be high (hypothesis 8). Lastly, we expected
20 that respondents should be able to reproduce the intensity of their faking behavior. As a
21 consequence, latent mean difference across these two response conditions should be
22 negligible (hypothesis 9).

23

24 **Method**

25 **Measures**

26 Studying applicants' faking behavior requires a personality inventory that is used for
27 selection purposes. The Inventory for Personality Assessment in Situations (IPS:
28 Schaarschmidt & Fischer, 2013) is such a personality inventory, which is used for admission
29 testing processes in teacher education (e.g. Schulz-Kolland, Krammer, Rottensteiner &
30 Weitlaner, 2014). The IPS is suitable for professions with high psychosocial demands, such
31 as the teaching profession. Validation studies have shown that the IPS measures personality
32 dispositions relevant to teaching training and to the teaching profession, and that there are
33 characteristic personality profiles of both teachers and students in teacher education, which
34 differ markedly from students of other courses of study (cf. Krammer, Sommer & Arendasy,

1 in press; Mayr & Brandstätter, 1998; Schaarschmidt & Fischer, 2013).

2 The IPS is comprised of three higher-order factors: social and communicative behavior,
3 health and recreational behavior, and performance behavior. To study applicants' faking
4 behavior, one subscale of each higher-order factor was chosen. *Activity in familiar*
5 *communicative situation* was chosen for the higher-order factor social and communicative
6 behavior, *preventive health behavior in response to warning signals* for the higher-order
7 factor health and recreational behavior, and *self-confidence in test situation* for the higher-
8 order factor performance behavior. Previous research demonstrates that *activity in familiar*
9 *communicative situation* is highly related to the Big Five domain factor Extraversion, while
10 *self-confidence in test situation* is highly related to the Big Five domain factor Emotional
11 Stability/Neuroticism (cf. Koschmieder, Pretsch & Neubauer, 2015). This may be due to the
12 fact that the item contents of the scales represent *sociableness* and *carefreeness* respectively,
13 which are both highly saturated by their respective domain factors Extraversion and
14 Emotional Stability (Arendasy et al., 2011; Costa & McCrae, 1992). Extraversion and
15 Emotional Stability in turn have been shown to be relevant for success in teacher education
16 and the teaching profession (e.g. Mayr, 2011). Lastly, *preventive health behavior in response*
17 *to warning signals* is relevant for the teacher education and the teaching profession, as
18 teachers are a high-risk group for burnout (e.g. Unterbrink et al., 2007), and the pressure of
19 the profession can manifest itself already during the education (cf. Gold & Roth, 1993).

20 All IPS scales consisted of a situational context and five personal behaviors that may
21 occur in the described situation. Respondents had to indicate the extent to which they would
22 exhibit each personal behavior in the described situation using a 4-point Likert scale (*not true*
23 *at all – definitely true*). The situational context for *activity in familiar communicative*
24 *situation* describes the setting of a sociable group of friends and acquaintances, and the items
25 relate to the activity in this situation (e.g. "be very lively"). For the scale *preventive health*
26 *behavior in response to warning signals*, the situational context is beginning to feel
27 incapacitated, people already noticing this, and people suggesting a visit to the doctor. The
28 items in this situation relate to the extent these warning signals are heeded, e.g. "actively do
29 something for my health". Finally, the scale *self-confidence in test situation* describes a test
30 situation and emphasizes the emotional burden of having to pass the test situation
31 successfully. The items in this situation relate to one's own reaction to this pressure, e.g.
32 "stay calm". To facilitate the interpretation, all items were recoded so that higher scores were
33 more favorable. All three scales showed sufficient internal consistency at selection ($t1: .639 \leq$
34 $\alpha \leq .757$). At re-test ($t2$), all three scaled exhibited good internal consistency in the honest-

1 condition and reproduce-condition ($.720 \leq \alpha \leq .875$), while the internal consistencies differed
2 in the faking-good-condition ($\alpha = .529$, $\alpha = .747$, and $\alpha = .862$, respectively).
3

4 **Procedure**

5 The research design is illustrated in Figure 1. The completion of personality scales for
6 selection (t1) was a part of the admission testing process of the University College of Teacher
7 Education Styria (for further details on the admission process: Schulz-Kolland et al., 2014).
8 For the admission testing process, the applicants had to demonstrate adequate vocal function
9 with no vocal chord disorders, followed by a test of their German skills, the measures for
10 intelligence and personality, and finally an interview. All subtests of the admission testing
11 process were relevant for admission. Applicants were ranked according to their final score,
12 which was a weighted linear combination of all subtests. Applicants were aware that every
13 part of the admission testing process would count for their ranking, but were unaware of the
14 respective weights. Typically, approximately 60% of the applicants are admitted every
15 academic year.

16 At re-test (t2), the applicants were re-tested with the same personality scales as
17 incumbents. The incumbents were randomly assigned to the three experimental response
18 conditions. In each experimental response condition (honest, reproduce, or faking-good) the
19 personality scales were completed with differing instructions. Because of the instructions,
20 respondents in the reproduce- and faking-good-condition were necessarily aware of the fact
21 that the study was concerned with faking behavior. In contrast, respondents in the honest-
22 condition were only given the standard instructions of the personality scales, to collect
23 responses that were as honest as possible. These respondents were debriefed afterwards. The
24 data of selection (t1) and of re-test (t2) was matched by a code, which could be generated out
25 of the data at selection (t1) and which respondents were asked to generate themselves at re-
26 test (t2).
27

28 **Sample**

29 A total of 175 (72.02%) females and 68 (27.98%) males aged 18 to 44 years ($M = 23.2$,
30 $SD = 4.8$) participated in the present study. At re-test (t2), all respondents were in their
31 second semester of a bachelor's degree to become either primary ($n = 131 \sim 53.9\%$) or
32 secondary ($n = 112 \sim 46.1\%$) school teachers. Even though the respondents were randomly
33 assigned to the three response conditions, the comparability of the three subsamples was
34 examined. The respondents of the three response conditions ($n = 81$ each) did not differ with

1 regard to gender ($\chi^2[2] = 2.001, p = .368, V = .091$), age ($F[2,240] = 2.932, p = .055$, partial η^2
2 $= .024$), and their study majors ($\chi^2[2] = 0.629, p = .730, V = .051$). The data were collected
3 over a time span of two years. Therefore, the comparability across the two years of data
4 collection was examined. There were no differences in gender ($\chi^2[1] = 0.067, p = .796, V =$
5 $.017$), age ($t[241] = 1.226, p = .222, d = 0.158$), and study major ($\chi^2[1] = 0.141, p = .707, V =$
6 $.024$) across the two years. The data obtained in the first year of data collection had been used
7 in a previous study examining the prevalence of faking and its effect on the raw scores of
8 several personality scales (Krammer & Pflanzl, 2015).

9

10 Tested Models

11 Measurement invariance, latent re-test correlations, and latent mean shifts were
12 analyzed using multi-group means and covariance structure analysis. The multi-group means
13 and covariance structure model is depicted in Figure 2. The three response conditions
14 constituted the three groups. For each response condition, a one-factor model was specified
15 for each measurement point. The latent factor at selection (t1) was hypothesized to correlate
16 with the latent factor at re-test (t2). The unstandardized factor loading of the first item was set
17 to 1 for identification purposes at both measurement points. To account for the re-
18 administration of the same items, the residuals of the respectively same items were allowed to
19 correlate. First, configural measurement invariance was tested, by specifying the described
20 two-factor model in all three response conditions. Next, equality constraints were imposed to
21 (1) test the comparability of the three response conditions at selection (t1) and to (2) test
22 measurement invariance across the measurement points. The model fit was evaluated using
23 the following goodness of fit statistics: non-significant χ^2 statistic, RMSEA $< .06$, and CFI $\geq$
24 $.95$ (Hu & Bentler, 1999; Marsh, Hau & Wen, 2004).¹ Each model fit was also compared to
25 its less restrictive precursor using the following criteria: non-significant $\Delta\chi^2$ statistic and
26 $\Delta\text{CFI} \leq .002$ (Meade, Johnson & Braddy, 2008). Since univariate (skewness and kurtosis
27 $> |1|$) and multivariate (multivariate skewness > 26.668 , all $p < .001$; MVN package:
28 Korkmaz, Goksuluk & Zararsiz, 2014) normality were violated, the parameters were
29 estimated using a maximum likelihood estimator with robust standard errors and a Satorra-
30 Bentler scaled test statistic (MLM: Satorra & Bentler, 1994). This parameter estimation

¹The widely used SRMR was omitted in our study. Studies suggest the SRMR is influenced by the sample size, and advise against using ΔSRMR for measurement invariance analysis (e.g. Meade et al., 2008). In accordance with these studies, Monte Carlo simulation studies on our final models suggested the SRMR to be heavily biased in evaluating our model fits, with the sample size causing Type I errors of 99% - 100%. A detailed summary of the Monte Carlo simulation studies is available from the first author upon request.

method has been shown to perform well under similar conditions in simulation studies (e.g. Curran, West & Finch, 1996). For small sample sizes, this estimator also outperforms the means and variance adjusted weighted least squares estimator (WLSMV) for measurement invariance analysis (cf. Sass, Schmitt & Marsh, 2014). All calculations were carried out in R (R Core Team, 2015) using the lavaan package (Rosseel, 2012).

< Insert Figure 2 here >

Ideally, the admission testing situation should exhibit strict measurement invariance, and equal latent means and variances across the three response conditions. To test this hypothesis, we first constrained the selection (t1) factor loadings to be equal across the three conditions ($\lambda_{t1,h,1} = \lambda_{t1,r,1} = \lambda_{t1,f,1}$, etc.) to test for weak measurement invariance. Then, the selection (t1) item intercepts were constrained to be equal across the three conditions ($\tau_{t1,h,1} = \tau_{t1,r,1} = \tau_{t1,f,1}$, etc.) to test for strong measurement invariance. Afterwards, the selection (t1) residual variances were constrained to be equal across the three conditions ($\theta_{\epsilon_{t1,h,1}} = \theta_{\epsilon_{t1,r,1}} = \theta_{\epsilon_{t1,f,1}}$, etc.) to test for strict measurement invariance. Finally, the latent trait variances and means were constrained to be equal across the three conditions ($\Phi_{t1,h} = \Phi_{t1,r} = \Phi_{t1,f}$ and $\xi_{t1,h} = \xi_{t1,r} = \xi_{t1,f}$, respectively).

After testing the comparability of the response conditions at selection (t1), measurement invariance across the measurement points was examined. Firstly, the factor loadings were constrained to be equal across the measurement points in each response condition to test for weak measurement invariance (i.e. $\lambda_{t1,h,1} = \lambda_{t2,h,1}$, $\lambda_{t1,h,2} = \lambda_{t2,h,2}$, etc.). Secondly, the item intercepts were constrained to be equal across the measurement points in each response conditions to test for strong measurement invariance (i.e. $\tau_{t1,h,1} = \tau_{t2,h,1}$, $\tau_{t1,h,2} = \tau_{t2,h,2}$, etc.). Thirdly, the residual variances were constrained to be equal across the measurement points in each response conditions to test for strict measurement invariance (i.e. $\theta_{\epsilon_{t1,h,1}} = \theta_{\epsilon_{t2,h,1}}$, $\theta_{\epsilon_{t1,h,2}} = \theta_{\epsilon_{t2,h,2}}$, etc.). Afterwards, the equality of the residual covariances across the response conditions ($\text{rc}_{1,h} = \text{rc}_{1,r} = \text{rc}_{1,f}$, etc.) was tested. Finally, the equality of the latent trait variances ($\Phi_{t1,h} = \Phi_{t2,h}$, etc.) and means ($\xi_{t1,h} = \xi_{t2,h}$, etc.) across the measurement points was tested.

If any of the above mentioned equality constraints failed to fit the data, constraints were successively relaxed, and the models again compared (cf. Meredith, 1993; Vandenberg & Lance, 2000). This relaxation of parameters lead to partial invariance models. The partial invariance models were then compared to the last fitting model (e.g. partial strong measurement invariance model vs. weak measurement invariance model). In the case of multiple parameters differing (e.g. item intercepts in more than one response condition at

1 selection (t2)), it was also tested whether additional equality constraints between these
2 parameters could be introduced (e.g. $\tau_{i2,h,1} = \tau_{i2,r,1}$). This was repeated until equivalence was
3 achieved and the final models thereby specified. Within the final models, the equality of the
4 latent trait re-test correlations was then tested (i.e. $r_h = r_r = r_f$).

6 Results

7 Comparison across Response Conditions at Selection (t1)

8 First, measurement invariance at selection (t1) was examined for the comparability of
9 the three response conditions at selection (t1). The global fit statistics and model comparisons
10 of the successively more restricted models are shown in the upper parts of Table 1 (*activity in*
11 *familiar communicative situation*), Table 2 (*preventive health behavior in response to*
12 *warning signals*), and Table 3 (*self-confidence in test situation*). The results indicated that at
13 selection (t1) strict measurement invariance across the three response conditions could be
14 assumed for all scales (model #1a - model #4a, model #1h - model #4h, and model #1s -
15 model #4s, respectively). Furthermore, adding the constraints for equal latent trait variances
16 (model #5a, model #5h, and model #5s, respectively) and equal latent trait means (model #6a,
17 model #6h, and model #6s, respectively) did not worsen the model fit. Therefore, the
18 respondents randomly assigned to the three response conditions at re-test (t2) did not differ in
19 their response processes, latent trait mean, and latent trait variance at selection (t1) in all three
20 scales.

22 Comparison across Measurement Points

23 Next, measurement invariance across selection (t1) and re-test (t2) was tested to
24 examine the extent to which faking behavior in the different response conditions affected the
25 measurement properties of the scales. The global fit statistics of the successively more
26 restricted models for testing measurement invariance across selection (t1) and re-test (t2), and
27 the respective model comparisons are summarized in the lower parts of Table 1 (*activity in*
28 *familiar communicative situation*), Table 2 (*preventive health behavior in response to*
29 *warning signals*), and Table 3 (*self-confidence in test situation*). The final parameter
30 estimates are summarized in Table 4, and the latent re-test correlations in Table 5.

32 Activity in familiar communicative situation

33 For the scales *activity in familiar communicative situation* (cf. Table 1), weak
34 measurement invariance (model #7a) could be assumed across selection (t1) and re-test (t2).

1 However, strong measurement invariance was not given (model #8a): four intercepts differed
2 in the honest-condition and one in the faking-good-condition (model #9a). The item
3 intercepts differed complementarily: they were either higher in the honest-condition at
4 selection (t1) than at re-test (t2), or lower in the faking-good-conditions at selection (t1) than
5 at re-test (t2). This violation of strong measurement invariance in the honest-condition and
6 faking-good-condition was contrary to hypotheses 1 and 4, respectively. Next, the equality of
7 the residual variances was examined. This was the only scale where all residual variances
8 were equal across the measurement points (model #10a). Thus, strict measurement invariance
9 was given in the reproduce-condition, which supported hypothesis 7. The residual
10 covariances were also equal across the response conditions (model #11a).

11 Next, the equality of the latent trait variances, means, and co-variances was examined.
12 The latent trait variances were not equal across the measurement points (model #12a); the
13 latent trait variance in the honest-condition at selection (t2) was higher than all other latent
14 trait variances (model #13a). The latent trait means were also not equal (model #14a), with
15 the latent trait mean in the reproduce-condition at selection (t2) being lower than all other
16 latent trait means (model #15a). The latent trait mean difference in the reproduce-condition
17 was contrary to hypothesis 9, while the equal latent trait means in the honest-condition and
18 faking-good-condition were contrary to hypotheses 3 and 6, respectively. Finally, the latent
19 re-test correlations (cf. Table 5) differed across the three response conditions (model #16a):
20 the latent trait re-test correlation was highest in the reproduce-condition (supporting
21 hypothesis 8), lower in the honest-condition (supporting hypothesis 2), and negligible in the
22 faking-good-condition (supporting hypothesis 5). This was the only scale where no equality
23 of the latent re-test correlations could be established (cf. Table 5).

24 < Insert Table 1 here >

25

26 **Preventive health behavior in response to warning signals**

27 For the scale *preventive health behavior in response to warning signals* (cf. Table 2),
28 weak measurement invariance was given across the measurement points (model #7h), while
29 strong measurement invariance was not (model #8h). Two intercepts differed (model #9h),
30 one in the faking-good-condition ($t1 < t2$; not supporting hypothesis 4), and one in the
31 reproduce-condition ($t1 > t2$; not supporting hypothesis 7). Next, equality constraints on the
32 residual variances were imposed (model #10h), which showed that the residual variances
33 differed across the measurement points. Overall, three residual variances were lower at
34 selection (t1) than at re-test (t2); the residual variances of one item in the honest-condition

1 and reproduce-condition to the same extend, and additionally another residual variance in the
2 honest-condition (model #11h). These differences in the residual variances in the honest-
3 condition were contrary to hypotheses 1. The covariances of the residuals were equal across
4 all three response conditions (model #12h).

5 Afterwards, equality constraints for the latent trait variances, means, and co-variances
6 were imposed. The latent trait variances were not equal across the measurement points
7 (model #13h), with the variance of the latent trait in the reproduce-condition at re-test (t2)
8 being the highest (model #14h). The latent trait means also differed across the measurement
9 points (model #15h): in the honest-condition and the reproduce-condition they were equally
10 higher at selection (t1) than at re-test (t2), while being lower in the faking-good-condition
11 (model #16h). The latent trait mean differences in the honest-condition and faking-good-
12 condition offered support for hypotheses 3 and 6, respectively. However, the latent trait mean
13 difference in the reproduce-condition was contrary to hypothesis 9. Finally, the latent re-test
14 correlations were also not equal across all three conditions (model #17h), but were across the
15 honest-condition and faking-good-condition (model #18h). As can be seen in Table 5, the
16 latent re-test correlations were moderate in the honest-condition and faking-good-condition
17 (supporting hypothesis 2, while not supporting hypothesis 5, respectively), and higher in the
18 reproduce-condition (supporting hypothesis 8).

19 < Insert Table 2 here >

21 **Self-confidence in test situation**

22 The scale *self-confidence in test situation* (cf. Table 3) differed from the other two
23 scales, as weak measurement invariance was not given (model #7s). Subsequent analyses
24 indicated that the factor loading of one item of this scale was higher in the faking-good-
25 condition than in all other response conditions (model #8s). This violation of weak
26 measurement invariance in the faking-good-condition contradicted hypothesis 4. Similar to
27 the other two scales, constraining the item intercepts to be equal across the measurement
28 points decreased the model fit (model #9s). One intercept differed per response condition
29 (model #10s): one intercept was equally higher in the honest-condition and reproduce-
30 condition at selection (t1) than at re-test (t2), while in the faking-good-condition one item
31 intercept was lower at selection (t1) than at re-test (t2). The differences in intercepts in the
32 honest-condition and reproduce-condition were contrary to hypotheses 1 and 7, respectively.
33 The residual variances were also not equal across measurement points (model #11s). No
34 residual variances were affected in the honest-condition nor the reproduce-condition, while

1 all residual variances in the faking-good-condition were higher at selection (t1) than at re-test
2 (t2). Regarding the residual covariances, they were equal across all three response conditions
3 (model #13s).
4 Finally, the equality of the latent trait variances, means, and co-variances was
5 examined. As with the other two scales, the latent trait variances were not equal across the
6 measurement points (model #14s). The latent trait variance was lower at selection (t1) than at
7 re-test (t2) in the honest-condition, while it was higher in the faking-good-condition (model
8 #15s). The latent trait means also differed (model #16s): the latent trait means in the honest-
9 condition and reproduce-condition were lower at selection (t1) than at re-test (t2), while it
10 was higher at selection (t1) than at re-test (t2) in the faking-good-condition. Similar to the
11 results of the scale *preventive health behavior in response to warning signals*, these latent
12 trait mean differences supported hypotheses 3 and 6, while being contrary to hypothesis 9.
13 This was the only scale where no further equality of the latent trait means could be established.
14 The latent re-test correlations were also unequal (model #17s), but were equal across the
15 honest-condition and reproduce-condition (model #18s). The latent re-test correlations (cf.
16 Table 5) were moderate in the honest- condition and reproduce-condition (supporting
17 hypothesis 2, while not supporting hypothesis 8 respectively), and lower in the faking-good-
18 condition (supporting hypothesis 5).

19 < Insert Table 3 here >
20 < Insert Table 4 here >
21 < Insert Table 5 here >
22

23 **Discussion**

24 While it has been examined whether, and to what extent, applicants can fake
25 personality scales, more research is needed to evaluate the effect of different kinds of faking
26 behavior on the response process involved (Goffin & Boyd, 2009; Griffith & Peterson, 2011;
27 Kuncel et al., 2011). The current study is expected to shed some light on applicants' faking
28 behavior by re-testing applicants as incumbents in three response conditions: (1) honest
29 responses, (2) faked-good responses, and (3) responses given when attempting to reproduce
30 one's own responses from when applying. Measurement invariance, re-test correlations, and
31 latent trait mean differences across these three response conditions and applicants' response
32 behavior in a real life selection setting were examined.
33

1 Honest, Applicants', and Instructed Faking Responses

2 It was hypothesized that the response behavior of applicants is comparable to the
3 response behavior of incumbents completing the personality scale in an honest-condition (cf.
4 Robie et al., 2001). The findings of the current study only partially support this hypothesis.
5 None of the scales exhibited strict measurement invariance across applicants in a real life
6 selection setting and incumbents responding honestly (contradictory to hypothesis 1). Faking
7 in a real life selection setting caused respondents primarily to increase their responses to
8 selected items. As this was not the case for all items to the same extent, strong measurement
9 invariance was violated for two out of three scales. For the one scale where strong
10 measurement invariance was given, strict measurement invariance was nevertheless violated.
11 This decrease of unaccounted for variance from honest response behavior to applicants'
12 response behavior seems to be attributable to real life selection settings reducing the potential
13 ambiguity of item contents. If an item content is ambiguous, this would introduce variance
14 unaccounted for by the psychometric model. However, being in a real life selection setting
15 could reduce this ambiguity, because respondents will less likely relate the item content to all
16 possible settings, but rather to the job relevant settings.

17 Nevertheless, our results corroborate that the detrimental effects of applicants' faking
18 behavior should not be overestimated (e.g. Bradley & Hauenstein, 2006; Hogan et al., 2007;
19 Ones et al., 2007), as some measurement properties were consistently preserved (configural
20 and weak measurement invariance). The preservation of weak measurement invariance also
21 indicates that predictive validity coefficients may not be detrimentally affected (Chen, 2008).
22 This may explain why the effects of applicants' faking behavior has been deemed to be
23 negligible in real life selection settings based on a comparison of predictive validity
24 coefficients across honest responding conditions and real life selection settings (for an
25 overview cf. Ones et al., 2007).

26 The finding of a lack of strict measurement invariance across applicants' response
27 behavior in a real life selection setting and the honest response behavior contradicts our
28 hypothesis and the results of Robie et al. (2001) on which this hypothesis was based.
29 However, there was one main difference between the present study and the study conducted
30 by Robie et al. (2001). In the selection situation of Robie et al. (2001), a warning was
31 presented "that distorted self-descriptions would invalidate the ... test results". No such
32 warning has been presented in the current study. Prior research indicated that applicants
33 wrongly fear their faking behavior may be exposed (König et al, 2012). Furthermore, it has
34 been shown that a warning that faking can be detected reduces faking when the negative

1 consequences of being detected are emphasized (e.g. Dwight & Donovan, 2003; Goffin &
2 Woods, 1995). Therefore, applicants may have been more reluctant to fake in the study
3 conducted by Robie et al. (2001) than in our present study.

4 In summary, the effects of applicants' faking behavior were not negligible in our study.
5 Applicants' faking behavior compromised either strong or strict measurement invariance for
6 all three scales. Furthermore, applicants' faking behavior distorted the rank-orders
7 (hypothesis 2) to a modest extent ($r = .497$, $r = .387$, and $r = .533$). This distortion was in line
8 with previous research (e.g. Griffin & Wilson, 2012; Griffith et al., 2007; Peterson et al.,
9 2011) and our hypotheses (hypothesis 2), and corroborates the effect of detrimental
10 applicants' faking behavior on selection decisions.

11 The response behavior in an instructed faking-good-condition was hypothesized to be
12 different to other response behaviors, such as applicants' response behavior and honest
13 response behavior (e.g. Miller & Ruggs, 2014; Robie et al., 2001). The findings of the current
14 study corroborate this hypothesis. This difference could be seen in the distortion of rank-
15 order (hypothesis 5). This distortion was in line with previous studies (e.g. Ellingson, Sackett,
16 & Hough, 1999; Griffith et al., 2007), and showed that the rank-order could be completely
17 distorted ($r = .019$), but was at its highest still only modestly preserved ($r = .395$).
18 Furthermore, instructed faking compared to applicants' response behavior consistently
19 violated strong measurement invariance (hypothesis 4). This was always the case for items
20 that were not yet affected by applicants' faking behavior, underlining that applicants engage
21 in less faking than they could.

22 For one scale, weak and strict measurement invariance were additionally violated
23 across applicants' response behavior and instructed faking. This was the scale (*self-*
24 *confidence in test situation*) where instructed faking lead to a ceiling effect. Due to the
25 limited variance in the items of this scale, the unaccounted variance of all items was also
26 decreased. For the item with the most pronounced restriction of variance, even the factor
27 loading was affected. This was also the item that had the highest average responses at
28 selection (t1). The item content of this item was related to being stable, a trait for which even
29 an overly high endorsement cannot be viewed negatively for teachers.

30 Taken together, our findings suggest that instructed faking will always detrimentally
31 affect the measurement properties of scales. Moreover, this detrimental effect will be even
32 more pronounced when instructed faking leads to strong ceiling effects. Consequently,
33 caution is advised when making selection decisions: when faking behavior causes a ceiling
34 effect – e.g. applicants are highly motivated to fake by very high selection ratios – the

1 selection decisions may be heavily biased.

2 We hypothesized to find the lowest latent means for honest response behavior, higher
3 latent means for applicants' response behavior, and even higher latent means for instructed
4 faking-good response behavior (cf. Birkeland et al, 2006; Viswesvaran & Ones, 1999).
5 However, these hypotheses were only partially confirmed (hypotheses 3 & 6). The latent
6 means of two of the three scales showed this expected pattern, while the latent means of the
7 remaining scale (*activity in familiar communicative situation*) did not differ significantly
8 across response conditions. Because of this, a post-hoc comparison of this remaining scale's
9 manifest raw means was done additionally. This post-hoc comparison revealed the honest
10 response behavior having a lower raw mean than the applicants' response behavior ($d =$
11 0.36), which in turn had a lower raw mean than the instructed faking-good response behavior
12 ($d = 0.33$). Like the aforementioned meta-analyses (Birkeland et al, 2006; Viswesvaran &
13 Ones, 1999), most studies on mean differences due to faking behavior only examine the mean
14 difference of raw scores. Unfortunately, our finding that strong measurement invariance was
15 not consistently given indicated that such mean raw score comparisons may be biased (Li &
16 Zumbo, 2009). If strong measurement invariance is unaffected or affected only partially
17 however, the latent trait means will nevertheless show that mean honest responses are lower
18 than mean applicants' responses, which are lower than mean instructed faking responses.
19 Therefore, raw mean score differences due to faking behavior may reflect true differences,
20 but, in the most detrimental case, may only reflect a uniform measurement bias.

21

22 **Stability of Applicants' Faking Behavior**

23 To try to shed even more light on applicants' faking behavior, the current study also
24 addressed the stability of applicants' faking behavior over half a year. Based on previous
25 findings, we hypothesized applicants' faking behavior to be relatively stable (cf. Hogan et al.,
26 2007). Our results demonstrated that half a year later, incumbents could partially reproduce
27 their response process from when they were applying (partial support for hypothesis 7). One
28 scale exhibited strict measurement invariance (*activity in familiar communicative situation*),
29 but for the other two scales, only partial strong measurement invariance was given.
30 Additionally, an increase in unaccounted for variance by the psychometric model could be
31 observed. For these affected items, the effects were consistent with an honest response
32 behavior. Incumbents also consistently underestimated their faking intensity from when
33 applying, i.e. their latent means decreased (contradictory to hypothesis 9). By contrast, the
34 rank-orders (hypothesis 8) could remain remarkably stable ($r = .90$, and $r = .699$), but were at

1 least equally high as when compared to honest response behavior ($r = .442$). Consequently,
2 studies using test scores obtained from incumbents which were instructed to respond like
3 applicants may underestimate the effects of applicants' faking behavior (e.g. Fell & König,
4 2016).

5 In summary, our results suggest that incumbents seem to be able to reproduce their
6 response behavior from a real life selection setting, and when in doubt, they rely on an honest
7 responding behavior as a frame of reference. Given that half a year had passed between the
8 real life selection setting and the attempt to reproduce one's own faking behavior, it is
9 remarkable that only a few measurement properties could not be reproduced. Therefore, these
10 findings suggest that the individual differences determinants of faking behavior may be
11 predominantly dispositional, as was already postulated by Snell et al. (1999). However,
12 respondents underestimated their faking intensity when asked to reproduce their faking
13 behavior from a real life selection setting. In consequence, situational factors may also play a
14 non-negligible role.

15

16 **Limitations and Conclusion**

17 While the current study aimed to provide insights into applicants' response behavior, it
18 is of course not without limitations. Firstly, only one personality inventory was used in the
19 present study. Future research may consider using more than one personality inventory to
20 examine the effects of faking in cases where scales differ in structure, response format, but
21 also in their perceived relevance for the particular job. The perceived relevance of scales for a
22 particular job may also be directly assessed in future studies, as the appraisal of relevance
23 may differ between applicants and scholars. Also, more traditional Big Five inventories could
24 be used, which do not relate the items to a situational context. However, it should be noted
25 that the personality inventory used in this study behaved consistently with faking effects
26 shown for more widely used personality inventories.

27 Future study may also wish to experimentally manipulate the selection ratio to examine
28 the effects of the selection ratio on the propensity and intensity to fake. However, differences
29 in selection ratios could affect the extent of range restrictions due to the selection process
30 (Linn, 1968). This may lead to an underestimation of the correlations (Bortz & Döring,
31 2006). To avoid such selection effects, future studies may also want to include rejected
32 applicants in their samples. We do not believe that a range restriction significantly biased our
33 results, since all three response conditions should be equally affected by this problem.

34 It would also be favorable to conduct future studies with larger sample sizes. The

1 sample size in this study was rather small per group for measurement invariance analysis.
2 Due to this concern, subsequent Monte Carlo simulation studies were conducted to examine
3 if our parameter estimates were biased. Based on the final models, the Monte Carlo
4 simulation studies suggested that the parameter estimated were largely unbiased, and that
5 their standard errors were only slightly biased for two items of the *scale activity in familiar*
6 *communicative situation*. A detailed summary of the Monte Carlo simulation studies is
7 available from the first author upon request.

8 Furthermore, it remains unclear what caused respondents' underestimation of their own
9 faking intensity when attempting to reproduce their applicants' response behavior. Future
10 research should examine whether there are differences in the selection settings and individual
11 differences in the applicants by which the faking intensity can be reproduced with more or
12 with less accuracy. Finally, it can always be possible that responses in an honest-condition
13 may still not be entirely honest.

14 Despite these limitations, the current study improved our understanding of applicants'
15 response behavior in real life situations. The current study indicates that applicants' response
16 behavior in a real life situation is quite stable and faking primarily affects strong
17 measurement invariance by inducing a uniform bias as compared to an honest responding
18 behavior, which in turn detrimentally effects the stability of the rank-order of the respondents
19 according to their standing on the latent trait. However, it should be noted that these effects
20 are by far less severe than the effects of an instructed faking.

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Tables

Table 1

Model fits and model comparisons for the comparability of the three conditions at selection (t1), the measurement invariance (MI) across selection (t1) and re-test (t2), and the comparison of latent re-test correlations of the scale activity in *familiar communicative situation*.

	#	Model	Model fit			Model comparison					
			χ^2	df	p	RMSEA	CFI	vs.	$\Delta\chi^2$	Δdf	p
Comparability at selection (t1)	1a	Configural MI	85.213	87	.534	.000	1.000	-	-	-	-
	2a	Weak MI ($\lambda_{d1,h,1} = \lambda_{d1,r,1} = \lambda_{d1,f,1}$, etc.)	91.857	95	.572	.000	1.000	1a	6.659	8	.574
	3a	Strong MI ($\tau_{t1,h,1} = \tau_{t1,r,1} = \tau_{t1,f,1}$, etc.)	93.634	99	.633	.000	1.000	2a	2.616	4	.624
	4a	Strict MI ($\theta_{\xi d1,h,1} = \theta_{\xi d1,r,1} = \theta_{\xi d1,f,1}$, etc.)	109.648	109	.465	.009	.998	3a	16.617	10	.083
	5a	4a + equal variances ($\Phi_{t1,h} = \Phi_{t1,r} = \Phi_{t1,f}$)	111.301	111	.474	.004	.999	4a	1.733	2	.421
	6a	5a + equal means ($\xi_{t1,h} = \xi_{t1,r} = \xi_{t1,f}$)	113.176	113	.478	.004	.999	5a	1.849	2	.397
MI across selection (t1) and re-test (t2)	7a	Weak MI ($\lambda_{d1,h,1} = \lambda_{d2,h,1}$, etc.)	124.725	125	.490	.000	1.000	6a	11.807	12	.461
	8a	Strong MI ($\tau_{t1,h,1} = \tau_{t2,h,1}$, etc.)	155.095	140	.181	.036	.953	7a	46.733	15	.000
	9a	Partial strong MI	135.094	135	.482	.003	1.000	7a	10.720	10	.380
	10a	9a + equal residual variances ($\theta_{\xi d1,h,1} = \theta_{\xi d2,h,1}$, etc.)	144.528	150	.611	.000	1.000	9a	12.336	15	.653
	11a	10a + equal residual covariances ($\tau c_{1,h} = \tau c_{1,r} = \tau c_{1,f}$, etc.)	154.305	160	.612	.000	1.000	10a	9.825	10	.456
	12a	11a + equal variances ($\Phi_{t1,h} = \Phi_{t2,h}$, etc.)	171.275	163	.313	.025	.974	11a	8.770	3	.033
MI across selection (t1) and re-test (t2)	13a	11a + partial equal variances	156.415	162	.609	.000	1.000	11a	1.970	2	.374
	14a	13a + equal means ($\xi_{t1,h} = \xi_{t2,h}$, etc.)	169.040	165	.398	.017	.988	13a	25.940	3	<.001
	15a	13a + partial equal means	158.993	164	.596	.000	1.000	13a	3.048	2	.218
	16a	15a + equal latent covariances ($\tau_h = \tau_r = \tau_f$)	170.974	166	.379	.019	.985	15a	5.992	2	.049

Note. The final model is bold faced.

1 Table 2

2 Model fits and model comparisons for the comparability of the three conditions at selection (t1), the measurement invariance (MI) across
 3 selection (t1) and re-test (t2), and the comparison of latent re-test correlations of the scale *preventive health behavior in response to warning*
 4 *signals*.

	#	Model	Model fit			Model comparison						
			χ^2	df	p	RMSEA	CFI	vs.	$\Delta\chi^2$	Δdf	p	ΔCFI
Comparability at selection (t1)	1h	Configural MI	77.624	87	.754	.000	1.000	-	-	-	-	-
	2h	Weak MI ($\lambda_{d1,h,1} = \lambda_{d1,r,1} = \lambda_{d1,f,1}$, etc.)	85.315	95	.752	.000	1.000	1h	7.666	8	.467	.000000
	3h	Strong MI ($\tau_{d1,h,1} = \tau_{d1,r,1} = \tau_{d1,f,1}$, etc.)	88.794	99	.759	.000	1.000	2h	3.510	4	.476	.000000
	4h	Strict MI ($\theta_{\xi d1,h,1} = \theta_{\xi d1,r,1} = \theta_{\xi d1,f,1}$, etc.)	97.468	109	.778	.000	1.000	3h	8.716	10	.559	.000000
	5h	4h + equal variances ($\Phi_{d1,h} = \Phi_{d1,r} = \Phi_{d1,f}$)	98.092	111	.804	.000	1.000	4h	1.116	2	.572	.000000
	6h	5h + equal means ($\xi_{d1,h} = \xi_{d1,r} = \xi_{d1,f}$)	99.067	113	.822	.000	1.000	5h	0.707	2	.702	.000000
MI across selection (t1) and re- test (t2)	7h	Weak MI ($\lambda_{d1,h,1} = \lambda_{d2,h,1}$, etc.)	120.555	125	.596	.000	1.000	6h	17.337	12	.137	.000000
	8h	Strong MI ($\tau_{d1,h,1} = \tau_{d2,h,1}$, etc.)	157.368	140	.150	.039	.948	7h	53.666	15	<.001	.052192
	9h	Partial strong MI	136.375	138	.523	.000	1.000	7h	17.772	13	.166	.000000
	10h	9h + equal residual variances ($\theta_{\xi d1,h,1} = \theta_{\xi d2,h,1}$, etc.)	178.211	153	.080	.045	.924	9h	37.509	15	.001	.075762
	11h	9h + partial equal residual variances	144.888	151	.625	.000	1.000	9h	10.269	13	.672	.000000
	12h	11h + equal residual covariances ($\tau_{c1,h} = \tau_{c1,r} = \tau_{c1,f}$, etc.)	158.733	161	.536	.000	1.000	11h	14.119	10	.168	.000000
MI across selection (t1) and re- test (t2)	13h	12h + equal variances ($\Phi_{d1,h} = \Phi_{d2,h}$, etc.)	173.696	164	.287	.027	.971	12h	9.101	3	.028	.029137
	14h	12h + partial equal variances	158.230	163	.591	.000	1.000	12h	1.326	2	.515	.000000
	15h	14h + equal means ($\xi_{d1,h} = \xi_{d2,h}$ etc.)	228.389	166	.001	.068	.813	14h	94.058	3	<.001	.187485
	16h	14h + partial equal means	160.633	164	.560	.000	1.000	14h	2.623	1	.105	.000000
	17h	16h + equal latent covariances ($\tau_{h1} = \tau_{r1} = \tau_{f1}$)	168.583	166	.430	.000	.992	16h	8.010	2	.018	.007763
	18h	16h + partial equal latent covariances	160.430	165	.586	.000	1.000	16h	0.001	1	.972	.000000

Note. The final model is bold faced.

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Table 3

Model fits and model comparisons for the comparability of the three conditions at selection (t1), the measurement invariance (MI) across selection (t1) and re-test (t2), and the comparison of latent re-test correlations of the scale *self-confidence in test situation*.

	#	Model	Model fit			Model comparison					
			χ^2	df	p	RMSEA	CFI	vs.	$\Delta\chi^2$	Δdf	p
Comparability at selection (t1)	1s	Configural MI	89.598	87	.403	.019	.991	-	-	-	-
	2s	Weak MI ($\lambda_{t1,h,1} = \lambda_{t1,r,1} = \lambda_{t1,f,1}$, etc.)	96.012	95	.452	.011	.996	1s	6.442	8	.598
	3s	Strong MI ($\tau_{t1,h,1} = \tau_{t1,r,1} = \tau_{t1,f,1}$, etc.)	98.745	99	.488	.000	1.000	2s	3.241	4	.518
	4s	Strict MI ($\theta_{\xi_{t1,h,1}} = \theta_{\xi_{t1,r,1}} = \theta_{\xi_{t1,f,1}}$, etc.)	106.656	109	.546	.000	1.000	3s	7.634	10	.665
	5s	4s + equal variances ($\Phi_{t1,h} = \Phi_{t1,r} = \Phi_{t1,f}$)	109.475	111	.523	.000	1.000	4s	3.256	2	.196
	6s	5s + equal means ($\xi_{t1,h} = \xi_{t1,r} = \xi_{t1,f}$)	110.787	113	.541	.000	1.000	5s	1.210	2	.546
MI across selection (t1) and re-test (t2)	7s	Weak MI ($\lambda_{t1,h,1} = \lambda_{t2,h,1}$, etc.)	137.937	125	.202	.036	.955	6s	21.971	12	.038
	8s	Partial weak MI	120.463	124	.573	.000	1.000	6s	9.817	11	.547
	9s	8s + equal intercepts ($\tau_{t1,h,1} = \tau_{t2,h,1}$, etc.)	148.558	139	.274	.029	.966	8s	36.997	15	.001
	10s	8s + partial equal intercepts	134.578	137	.543	.000	1.000	8s	15.309	13	.288
	11s	10s + equal residual variances ($\theta_{\xi_{t1,h,1}} = \theta_{\xi_{t2,h,1}}$, etc.)	335.831	152	.000	.122	.355	10s	278.787	28	<.001
	12s	10s + partial equal residual variances	144.253	147	.549	.000	1.000	10s	24.177	23	.394
MI across selection (t1) and re-test (t2)	13s	12s + equal residual covariances ($\tau\zeta_{t1,h} = \tau\zeta_{t1,r}$, etc.)	152.073	157	.596	.000	1.000	12s	7.622	10	.666
	14s	13s + equal variances ($\Phi_{t1,h} = \Phi_{t2,h}$, etc.)	193.861	160	.035	.051	.881	13s	79.337	3	<.001
	15s	13s + partial equal variances	153.800	158	.580	.000	1.000	13s	1.909	1	.167
	16s	15s + equal means ($\xi_{t1,h} = \xi_{t2,h}$, etc.)	284.375	161	<.001	.097	.567	15s	158.876	3	<.001
	17s	15s + equal latent covariances ($\tau_h = \tau_r = \tau_f$)	167.275	160	.331	.024	.974	15s	13.207	2	.001
	18s	15s + partial equal latent covariances	154.015	159	.597	.000	1.000	15s	0.280	1	.597

Note. The final model is bold faced.

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Table 4
Comparison of selected unstandardized parameter estimates of selection (t1) to the three conditions at re-test (t2) for all three scales. For selection (t1), the unstandardized parameters are given. For re-test (t2), either the unstandardized parameters are given, or =t1 in case of invariance across measurement points.

Activity in familiar communicative situation									
	t1 all conditions	t2 honest	t2 reproduce	t2 faking-good	t1 all conditions	t2 honest	t2 reproduce	t2 faking-good	
i1-intercept ($\tau_{\text{tx},c,1}$)	3.872	3.718	=t1	=t1	=t1	i1-res.-var. ($\theta_{\text{tx},c,1}$)	0.090	=t1	=t1
i2-intercept ($\tau_{\text{tx},c,2}$)	3.368	3.167	=t1	=t1	=t1	i2-res.-var. ($\theta_{\text{tx},c,2}$)	0.288	=t1	=t1
i3-intercept ($\tau_{\text{tx},c,3}$)	3.746	3.560	=t1	=t1	=t1	i3-res.-var. ($\theta_{\text{tx},c,3}$)	0.160	=t1	=t1
i4-intercept ($\tau_{\text{tx},c,4}$)	3.031	=t1	=t1	=t1	3.336	i4-res.-var. ($\theta_{\text{tx},c,4}$)	0.370	=t1	=t1
i5-intercept ($\tau_{\text{tx},c,5}$)	3.629	3.443	=t1	=t1	=t1	i5-res.-var. ($\theta_{\text{tx},c,5}$)	0.236	=t1	=t1
latent trait mean ($\xi_{\text{tx},c}$)	-0.009	=t1	-0.098	=t1	latent trait variance ($\Phi_{\text{tx},c}$)	0.060	0.145	=t1	=t1
preventive health behavior in response to warning signals									
i1-intercept ($\tau_{\text{tx},c,1}$)	3.232	=t1	=t1	=t1	=t1	i1-res.-var. ($\theta_{\text{tx},c,1}$)	0.334	=t1	=t1
i2-intercept ($\tau_{\text{tx},c,2}$)	3.577	=t1	=t1	=t1	=t1	i2-res.-var. ($\theta_{\text{tx},c,2}$)	0.232	=t1	=t1
i3-intercept ($\tau_{\text{tx},c,3}$)	3.765	=t1	=t1	=t1	=t1	i3-res.-var. ($\theta_{\text{tx},c,3}$)	0.104	0.236	0.236
i4-intercept ($\tau_{\text{tx},c,4}$)	3.686	=t1	3.433	=t1	=t1	i4-res.-var. ($\theta_{\text{tx},c,4}$)	0.238	0.386	=t1
i5-intercept ($\tau_{\text{tx},c,5}$)	3.454	=t1	=t1	=t1	3.615	i5-res.-var. ($\theta_{\text{tx},c,5}$)	0.208	=t1	=t1
latent trait mean ($\xi_{\text{tx},c}$)	0.005	-0.294	-0.294	=t1	latent trait variance ($\Phi_{\text{tx},c}$)	0.125	=t1	0.280	=t1
Self-confidence in test situation									
i1-intercept ($\tau_{\text{tx},c,1}$)	3.989	=t1	=t1	=t1	=t1	i1-res.-var. ($\theta_{\text{tx},c,1}$)	0.176	=t1	0.048
i2-intercept ($\tau_{\text{tx},c,2}$)	3.959	=t1	=t1	=t1	=t1	i2-res.-var. ($\theta_{\text{tx},c,2}$)	0.238	=t1	0.077
i3-intercept ($\tau_{\text{tx},c,3}$)	3.725	=t1	=t1	=t1	3.881	i3-res.-var. ($\theta_{\text{tx},c,3}$)	0.280	=t1	0.137
i4-intercept ($\tau_{\text{tx},c,4}$)	3.936	=t1	=t1	=t1	=t1	i4-res.-var. ($\theta_{\text{tx},c,4}$)	0.272	=t1	0.060
i5-intercept ($\tau_{\text{tx},c,5}$)	4.004	3.837	3.837	=t1	=t1	i5-res.-var. ($\theta_{\text{tx},c,5}$)	0.222	=t1	0.007
latent trait mean ($\xi_{\text{tx},c}$)	-0.569	-0.820	-0.685	=t1	latent trait variance ($\Phi_{\text{tx},c}$)	0.193	0.403	=t1	0.069

Note. The subscript tx denotes t1 or t2, and the subscript c the response conditions honest, reproduce or faking-good. res.-var. = residual variance.

1 Table 5
2 The latent re-test correlations of the final models of all three scales. Per scale, the *not differing* latent re-test correlations are marked (*).

Scale	Condition		
	honest	reproduce	faking-good
Activity in familiar communicative situation	.497	.898	.019
Preventive health behavior in response to warning signals	.387*	.699	.395*
Self-confidence in test situation	.533*	.442*	.121

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Figures

Figure 1 (cf. Figure 2 in Section 3.1):

The combined within-subject and between-subject design. At selection (t1), all respondents were applicants and went through the same admission testing situation. At re-test (t2), the same applicants were by now incumbents, and were randomly assigned to one of three response conditions.

Figure 2 (cf. Figure 3 in Section 4.1):

The multi-group means and covariance structure model, containing one-factor models at selection (t1) and at re-test (t2), and the covariances. The λ s denote the factor loadings, the τ s the intercepts, the θ s the residual variances, the ρ s the residual covariances, the ξ s the latent trait means, the Φ s the latent trait variances, and ρ s the latent trait covariance.

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