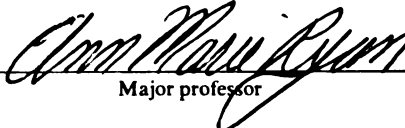This is to certify that the

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APPLICANT FAKING

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TOWARD AN INTEGRATED MODEL OF APPLICANT FAKING

By

Lynn A. McFarland

A DISSERTATION

Submitted to
Michigan State University
In partial fulfillment of the requirements
For degree of

DOCTOR OF PHILOSOPHY

Department of Psychology

2000

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ABSTRACT

TOWARD AN INTEGRATED MODEL OF APPLICANT FAKING

By

Lynn A. McFarland

The present study used the theory of planned behavior to predict faking on a personality selection test. Participants' attitudes toward faking, subjective norms toward faking, perceived behavioral control over faking, and intentions to fake a selection test were all assessed. In addition, participants were asked to take the personality measure under instructions to be honest and also under instructions to respond like an applicant. Their faking behavior was assessed through the use of difference scores and a social desirability scale. The theory of planned behavior predicted both the intention to fake the selection test and actual faking behavior. However, proposed moderators of these relationships, such as valence toward performing well on the test, warning of a lie scale, and knowledge of the constructs assessed by the personality test, were generally not supported. Practical implications of these results and directions for future research are discussed.

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I want to everyone who made this dissertation possible, both directly by providing feedback and advice on the dissertation, and indirectly for providing support and encouragement to me personally throughout the process. First, thanks to Ann Marie Ryan, my chair, for providing guidance and support throughout the dissertation process. I also want to thank my committee members, Drs. Neal Schmitt, Rick DeShon, Dan Ilgen, and Murray Barrick for their insightful comments and recommendations.

A thank you goes to Melissa Kabin for helping me collect my data and to Christine Scheu for helping me to get those tasks done that were so difficult to do from Maryland. I also thank Christine for her general support and friendship along the way.

I want to thank my family, especially my parents, William and Rosanne McFarland. They have always provided me with love and encouragement and have been there for me in the toughest of times cheering me on. Without their constant support I would not have been able to do any of this. I also want to thank my parents for taking the time to learn what industrial/organizational psychology is. This is a true demonstration of their love, concern, and support, and I am sincerely touched.

Finally, most of all, I want to thank my husband, Robert Ployhart, for his advice, support, friendship, and love. He read this dissertation almost as many times as I did and I greatly appreciate it!

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

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INTRODUCTION

Personality tests are increasingly being used for personnel selection because these tests generally yield moderate validities (Barrick and Mount, 1991; Hough, Eaton, Dunnette, Kamp, & McCloy, 1990; Tett, Jackson, & Rothstein, 1991), yet produce less adverse impact than more traditional selection measures (e.g., cognitive ability tests; Bobko, Roth, & Potosky, 1999). Despite this, there is concern that intentional distortion on the part of applicants may make the results of these tests difficult to interpret because it may result in changes in the test mean, reliability, and validity. However, there is still debate about the effects of faking on the results of personality tests. For example, some researchers have found evidence that faking does not negatively influence the validity of these measures (Abrahams, Neumann, & Githens, 1971; Barrick & Mount, 1996; Cunningham, Wong, Barbee, 1994; Hough et al., 1990; McCrae & Costa, 1983; Ones, Viswesvaran, & Reiss, 1996). However, others disagree with these findings and provide their own evidence that faking does attenuate the validity of non-cognitive measures (Douglas, McDaniel, & Snell, 1996; Dunnette, McCartney, Carlson, & Kirchner, 1962; Kluger, Reilly, & Russell, 1991; Pannone, 1984).

There are several reasons that results of research on faking personality and other self-report measures may be inconsistent. An explanation of some of the observed differences between these findings may be that faking is operationalized in different ways across studies. For example, some researchers have used scores on a lie scale as a measure of faking (e.g., McCrae & Costa, 1983; Hough et al., 1990; Rosse, Stecher, Miller, & Levine, 1998), whereas others have used deviations from the mean (e.g.,

Dunnette et al., 1962). It is not clear the extent to which each of these measures of faking actually measure intentional distortion (Paulhus, 1986). Second, some of these discrepancies may exist because situational variables are not taken into account. Some situations may inhibit faking on these measures (e.g., knowledge that a lie scale is included on the measure), while other situations may exacerbate the problem (e.g., high competition for a job). Third, a number of the studies that have investigated the relationship between faking and test validity have assumed that there is no variance in faking (e.g., Becker & Colquitt, 1992; Zickar, Rosse, & Levin, 1996). That is, these studies have assumed that everyone who is faking increases their scores the same amount (i.e., by a constant). As Lautenschlager (1994) suggests, if all applicants increase their scores the same extent (i.e., there is no variance in faking), faking will not change rank orders and therefore should have no effect on criterion-related validity. However, if faking does result in changes in the rank orders of applicants (i.e., some applicants increase their scores through faking more than others), then faking has the potential to distort criterion-related validity. Initial research suggests that there is variance in the extent to which individuals fake (Ellingson, Smith, & Sackett, 1999; McDaniel, Douglas, & Snell, 1997; McFarland & Ryan, in press; Rosse et al., 1998). Therefore, the failure to consider this variability in some studies may explain why these studies typically do not find that faking has an effect on validity, while others do. Finally, the discrepancies within the faking literature draw attention to a larger and more fundamental problem. Currently, there is no theory or model of faking behavior. Understanding what contributes to the extent of faking is needed before we can resolve debates about effects due to distortion. Therefore, the present study will use theory to understand an important

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practical problem so that we may better determine when faking may adversely affect personality test results.

The present study proposes and tests a model of faking behavior. This model is presented in Figure 1. The model incorporates the theory of planned behavior which has been used for several years to predict behaviors as diverse as quitting smoking to stealing (Norman & Conner, 1996; Reinecke, Schmidt, & Ajzen, 1996; Theodorakis, 1992). Specifically, the way in which attitude toward faking, subjective norms about faking, and perceived behavioral control influence the intention to fake will be examined. I will also explore the extent to which the intention to fake predicts faking behavior. Finally, three probable moderators of these relationships will be explored (i.e., valence toward doing well on the test, warning of a lie scale, and knowledge of the constructs assessed by the test). By considering the model of faking presented here, future studies may be able to consider the relevant issues and influences on faking. Only then can we determine under what circumstances faking does matter.

Before describing this model in detail, a definition of faking will first be given, and reasons applicants may fake will be discussed. Second, evidence that applicants can and do fake will be examined. Third, the way in which faking may influence the results of tests will be reviewed. Finally, the model of faking will be described in detail.

Definitions and Why People Fake

A number of labels have been given to describe the tendency of some individuals to select answers on tests that will result in others viewing them in the most favorable way. This has been referred to as response distortion, social desirability,

faking, impression management, self-enhancement, and intentional distortion (Becker & Colquitt, 1992; Douglas et al., 1996; Mumford & Stokes, 1992, Zerbe & Paulhus, 1987).

Paulhus (1984; 1986) suggests that socially desirable responding contains two components: self-deception and impression management. Self-deception refers to the unconscious tendency to see one's self in a favorable light. Impression management is a conscious attempt to present false information to create a favorable impression on others. Therefore, when applicants taking a selection test consciously do not select the most accurate answer (the one that most closely describes them) but instead select the answer that they believe will make them look most favorable, they are using impression management. In the present study, this type of response distortion, whereby an individual consciously distorts answers in order to be viewed favorably, will be referred to as faking.

There are a number of reasons to suspect that people may fake responses on a selection test. Leary and Kowalski (1990) suggest that people are motivated to manage their impressions when the impressions they make are relevant to the fulfillment of their goals and when these goals are greatly valued. Generally, when an applicant takes a selection test, he or she desires to get the job. Individuals may fake in order to obtain a higher score, thus increasing their chances of being hired. Pandey and Rastagi (1979) showed that attempts to manage impressions increased during a job interview when competition for the job became more intense. Therefore, it seems that as the stakes increase, attempts to look the best one possibly can also increase.

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Other less obvious reasons individuals may fake are suggested by Schlenker and

Weigold (1992). They suggest that people regulate the impressions they make to

maintain or enhance self-esteem. People generally act to maximize their self-esteem and

will influence the content of self-presentation to do so. Answering a test in such a way

that others will perceive one positively will help maintain self-esteem.

It is clear that there are reasons applicants may attempt to make themselves look

favorable. The next questions to be addressed are: Can individuals fake such tests? If

they can fake, do they? The following section will review the literature suggesting that

applicants can and do fake personality measures.

Evidence that Applicants Can and Do Fake

For over sixty years research has examined if personality tests can be faked (Bass,

1957; Borislow, 1958; Hunt, 1948; Kelly, Miles, & Terman, 1936; Longstaff, 1948).

Findings are very consistent, showing that people can increase scores on such tests when

they choose to do so. For example, Dunnette et al. (1962) had 62 salesmen take a forced

choice self-description checklist twice in counterbalanced order. In one administration

the subjects were asked to answer honestly, whereas in the other one they were told to

fake (select the best answers). Subjects in the fake condition had mean scores more than

half a standard deviation above those in the honest condition.

Since then several more studies have demonstrated similar findings (e.g., Cohen

& Lefkowitz, 1974; Hough et al., 1990; Hurtz & Bohon, 1997; Krug, 1978; McFarland &

Ryan, in press; Rosse et al., 1998). For example, Douglas et al. (1996) administered a

personality test which contained an agreeableness and a conscientiousness scale to 600

college students. They had a between-subjects design in which half of the subjects were

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randomly placed into the honest condition and the other half were placed into the faking condition (told to make themselves look as good as possible). Results revealed significant group differences of up to .70 standard deviations on the personality scale of agreeableness, and .86 on conscientiousness, with all differences in favor of the fake condition.

However, there are some limitations of this research that should be pointed out. Much of the research that has examined the fakability of personality tests has used within subjects designs which require participants to take the test honestly and also under instructions to fake. A problem with such designs is that they generally result in order effects such that individuals who take the test under instructions to respond honestly first increase their scores when asked to fake more than those who did not take the test before being asked to fake (Klein & Owens, 1965). It is not clear in some of these studies if conditions were counterbalanced. Therefore, the results of these studies should be viewed with caution.

These studies suggest that responses on personality measures can be faked. When asked to do so, subjects can increase their scores by as much as 1 standard deviation above honest respondents. However, just because applicants can fake personality tests does not mean they do fake. It is important to make the conceptual distinction between fakability and actual faking. If applicants can fake, but do not do so, then responses on a fakable measure may not be biased. Hence, it is crucial to examine the literature for any evidence indicating that applicants actually do fake when responding to personality selection measures.

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Dunnette et al. (1962) compared the scores of those in the fake and honest conditions to the scores of 64 applicants applying for a sales job. The applicants achieved a mean score somewhere between the mean scores of the subjects in the fake and honest conditions, indicating that the applicants faked to some extent. However, to determine whether all or only a few applicants tried to create a good impression on the checklist, a fake key was developed. This was done by weighting those responses showing significant differences between the honest and fake conditions. By examining the fake score distribution it was determined that a cut score of 4 successfully identified 90% of the faked answer sheets at the expense of misclassifying only 9% of the honest ones. Through this investigation it was determined that only a few of the applicants actually faked. Therefore, it was the responses of these few that increased the mean for the applicant group.

Pannone (1984) administered a rational biographical questionnaire to over 200 applicants for an electrician position. He included an item which made reference to a piece of electrical equipment that did not exist. Those applicants who said they had used the piece of equipment were considered fakers. It was determined that over one-third of the applicants were fakers and there was a significant mean difference between the scores of fakers and non-fakers such that the mean score of the fakers was over .5 standard deviation units greater than those identified as non-fakers.

This study may be criticized in that it is possible that a number of the fakers were not identified. That is, some people who did fake throughout the test did not say that they had operated the fictional piece of equipment. Similarly, those who did not fake throughout the test may have been identified as fakers by their response to the fake item

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but were actually just confused by the item or were just careless when responding to the item. Therefore, this one item measure of faking may also be tapping carelessness of responding rather than isolating conscious distortion.

Further evidence that applicants may fake selection tests come from Schmit and Ryan (1993). They administered the NEO-FFI, a personality test used to measure the traditional five factor model (extroversion, agreeableness, conscientiousness, emotional stability, and intellectance or openness to experience), to applicants and students. Using confirmatory factor analyses, they found the five factor model fit the data from the student sample but the applicant data contained six factors. The authors suggested this sixth factor (an "ideal-employee factor") that surfaced in the applicant sample may have resulted from applicant faking.

Becker and Colquitt (1992) administered a biodata inventory to 250 real applicants. They also had another group of individuals take the same test under instructions to fake and some under instructs to respond honestly. Results indicate that the scores of the applicants fell between the scores of those in the honest and the fake conditions (they scored lower than the fakers but higher than the honest respondents), indicating that applicants do fake responses.

McDaniel et al. (1997) had 192 subjects complete a conscientiousness and a social influence measure that were mailed to them. Respondents first completed the conscientiousness and social influence measures as they would if they were applying for a job that they really wanted. They were then instructed to respond to these measures a second time honestly. For the conscientiousness measure, 23% of the sample improved their score in the applicant condition and 26% improved their scores in the applicant

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condition for the social influence measure. However, over 65% of the sample obtained the same score across conditions for the social influence scale as compared to 72% for the conscientiousness measure. When responding as applicants, respondents increased their conscientiousness scale score by .26 standard deviations and their social influence scale score by .16 standard deviations.

McDaniel et al.'s (1997) findings may be questioned on the grounds that those who misrepresented themselves may have been less likely to respond to the mail survey for fear that their responses would be traced. However, if this is true then their results probably underestimate the true misrepresentation rate of the sample. This study may also be criticized because the conditions were not balanced; thus, order effects may have been present.

Hurtz and Bohon (1997) instructed subjects to answer a personality-based integrity test as they would if they were applicants for a retail sales position. The mean score of those in this applicant condition was over .5 standard deviations higher than those who took the test under instructions to answer honestly.

Although there is considerable evidence that applicants do fake responses on personality tests, there are some who do not believe this to be true. Hough et al. (1990) administered a temperament inventory to military incumbents that included a social desirability scale to detect intentional distortion in a favorable direction. All respondents were told to answer the inventory honestly but some were later asked to answer so as to look good (fake condition). The inventory was also administered to 125 individuals going through the Military Entrance Processing Station (MEPS) who were told that the test would be used to make decisions about their careers. Results showed that

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incumbents were able to increase scores by faking when told to do so; however, the real applicants did not fake. The social desirability scores of the MEPS were similar to those of the incumbents in the honest condition.

One must keep in mind that the MEPS are not traditional applicants. That is, these were individuals who were already enlisted in the military. Therefore, they were not job applicants and may not have been as motivated as job applicants would have been. Additionally, the researchers used scores on a social desirability scale to identify fakers. Research has shown that these lie scales often fail to isolate conscious distortion because they also measure the self-deception component (i.e., the unconscious part) of socially desirable responding (Paulhus, 1986). Not enough information was provided about the social desirability scale used in this study to determine if it was flawed in this manner. Thus, results should be viewed with caution.

In summary, most of the evidence suggests that at least some applicants do fake their responses on personality measures. There is also evidence that applicants increase their scores by as much as a half of a standard deviation by doing so (e.g., Dunnette et al., 1962; Stokes, Hogan, & Snell, 1993). Now I will examine why applicant faking may be a concern to those using personality measures for selection purposes.

Influence of Faking on the Results of Personality Measures

There is concern that applicant faking may make it more difficult to interpret personality test results due to its effect on selection decisions, criterion-related validity, and construct validity (i.e., factor structures, scale reliabilities, and correlations with other measures). However, others disagree that faking negatively affects test results. In the following section, the research that has been conducted with respect to the effects of

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faking on the results of personality tests will be examined. First I will present the research that has examined how faking may influence selection decisions that are based on personality measures. Second, the possible effects on criterion-related validity will be explored. Finally, the way in which construct validity has been shown to be affected by faking will be examined.

Selection Decisions. As discussed earlier, research has consistently found that faking increases the mean scores of applicant groups (Becker & Colquitt, 1992; Dunnette et al., 1962; McDaniel et al., 1997; Pannone, 1984). Again, findings indicate that when asked to fake, groups can increase scores by as much as a standard deviation through faking, and applicant groups tend to score a half of a standard deviation greater than groups asked to respond honestly (Hough et al., 1990; Hurtz & Bohon, 1997).

Such changes in applicant scores have obvious implications if an organization is using an absolute cut score to select applicants on this type of test. More applicants than would have been selected if everyone responded honestly will be selected if applicants fake. For example, Becker and Colquitt (1992) administered a biodata inventory to over 250 real applicants. To determine the impact of faking on selection decisions, Becker and Colquitt assumed that every applicant distorted his or her score. They then adjusted the score distribution by subtracting three points from everyone's original score (this was the mean difference between the honest and applicant scores). The hiring decision that was made on the original score (hire or not hired) would stand for 83.1% of the applicants when the new distribution of scores was considered. This study provides evidence that, even if we assume that faking is a constant across individuals (everyone

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fakes and increases their scores the same amount through faking), it has the potential to change hiring decisions, such that those who normally would not be hired will be.

However, Becker and Colquitt (1992) based this analysis on the assumption that all applicants faked the same amount. Thus, they failed to consider that faking may result in changes in the rank order of applicants. If rank orders change this would likely change selection decisions more drastically.

A more thorough analysis of how faking influences selection decisions was conducted by Christiansen, Goffin, Johnston, and Rothstein (1994). They administered the 16PF and a response distortion scale to 495 incumbents in a large factory products organization who were participating in an assessment center. Subjects were told that responses to these measures would be used for future operational purposes (e.g., selection, promotion). They then examined the effect of a social desirability correction on selection decisions for different selection ratios. It was determined that, depending on the selection ratio, correction of scores would have resulted in different hiring decisions than those that would have been made on the basis of uncorrected scores. For example, with a 15% selection ratio 12 candidates who would have been selected on the basis of uncorrected 16PF scores would not have been selected on the basis of faking corrected scores. Results indicated that at lower selection ratios there was a greater percentage of discrepant hires. In fact, they observed a change in rank order for over 85% of the applicants with the application of the social desirability correction to the personality scale scores.

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Ellingson, Sackett, and Hough (1999) found similar results. Using the data of Hough et al. (1990), Ellingson et al. determined the percentage of correct top-down hiring decisions that would be made using the fake responses and then using the corrected responses (i.e., lie scale scores partialled from fake scores). The "true" hiring decision was based on the applicant's standing in the honest condition. Results indicated that an increase in the selection ratio corresponded to an increase in the proportion of correct selection decisions in the faking condition. Also, holding everything else constant, as the percentage of individuals faking increased, the proportion of correct selection decisions decreased. For example, given a 10% selection ratio, if 30% of applicants faked, only 34% of those who would have been hired under the honest condition would be hired. Results were similar for the corrected scores. Therefore, even when faking is corrected, the rank orders are not similar to those in the honest condition.

One problem with the majority of these studies is that most have used scores on a lie scale to operationalize intentional distortion. It is not clear how well these scales actually measure faking behavior. However, despite this, there is still considerable evidence to support that there are individual differences in the extent to which individuals fake self-report measures and that this may significantly influence selection decisions (e.g., Dunnette et al., 1962; Hough, 1998; McDaniel et al., 1997; McFarland & Ryan, in press; Rosse et al., 1998). If faking does result in changes in rank orders, it has the potential to alter the criterion-related validity of these measures. Research that has examined how faking influences the criterion-related validity of personality measures will now be reviewed.

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Criterion-Related Validity. In their study described earlier, Dunnette et al. (1962) found that faking exerted an effect on the validity of their personality checklist. Managerial ratings were provided for 45 of the salesmen who had answered the checklist under the honest and the fake conditions. Correlations between ratings and checklist scores under the honest condition were relatively large (reaching .38) and significant. However, when the ratings were correlated with scores of those in the fake condition, the correlations decreased and became non-significant. As described earlier, they determined whether all or only a few applicants tried to create a good impression by developing a fake key on the checklist and determined that only a few of the applicants actually faked. Therefore, the authors concluded that even the presence of a few fakers may decrease the validity of such measures.

Douglas et al. (1996) obtained job performance ratings from employers of subjects who worked and gave permission to contact their supervisor. Evidence that faking decayed the criterion-related validity of the scales (conscientiousness and agreeableness) was found. The average validity of the scales was .26 for honest subjects and .04 for those who took the tests under instructions to fake. Further analyses revealed results similar to those found by Dunnette et al. (1962). Douglas et al. randomly selected a few of the subjects from the faking condition and added their data to the data of the 97 honest subjects so that the combined sample contained the data of 10 to 25 percent of the faking subjects. Through this procedure they found that as the percent of fakers in the sample increased, the number of fakers scoring within the top ten increased as well. For example, it was found that if 10% of the people faked, five of the top ten subjects were fakers, whereas if 25% of the sample faked, nine of the top ten subjects were fakers. The

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mean validity decreased somewhat as the percentage of fakers in the sample increased. Moreover, it was found that as the number of fakers in the top ten increased, the mean validity of the measures dropped substantially for those who faked. They found that even a few fakers may distort the results of personality measures.

McDaniel et al. (1997) had respondents rate how their supervisor would evaluate their overall job performance (on a four point scale) in addition to the measure of conscientiousness, social influence, and misrepresentation. Results indicated that the extent to which respondents misrepresented themselves was negatively correlated with self-reported job performance. They also found that the validities were higher in the honest condition for both the conscientiousness measure (.14 vs. .06) and the social influence measure (.23 vs. .19). A criticism of this study is that the two measures were validated against a self-reported performance measure. However, one would expect that if the self-reported performance measure was not accurate it would be slanted in the positive direction. This would lead to an underestimation of the decline in validity. Therefore, these results may be conservative.

Although there is some evidence that faking changes the criterion-related validity of personality measures, there is also evidence to the contrary. Hough et al. (1990) developed a temperament inventory called "Assessment of Background and Life Experiences" or ABLE. This inventory consisted of ten content scales that measured six temperament constructs and a social desirability scale that was included to detect intentional distortion in a favorable direction. Hough et al. administered the ABLE to military incumbents who took the test under both honest and fake conditions. All of these respondents were told to answer questions honestly, but half also took the test under

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instructions to fake good and the other half were told to fake bad (possibly so as to not be drafted). The conditions were counterbalanced. Results indicated that the response validity scales did detect the different types of distortion. Moreover, intentional distortion, in a positive direction, did not attenuate the criterion-related validity of the inventory.

The method Hough et al. (1990) used to obtain these results is questionable. The data of the incumbents was skewed so that meaningful moderated regressions could not be performed to determine the impact of faking on the inventory. To deal with this the authors took the mean social desirability scale score of those in the fake good experimental condition and used this to divide the concurrent validity sample into two groups. Those scoring at or above the mean of the fake group were labeled "overly desirable," while those scoring below this mean were labeled "accurate." They then calculated the criterion-related validities of the ABLE for each of these groups separately.

Hough et al. (1990) found that the criterion-related validity did not change substantially for those in the "overly desirable" group; however, the incidence of faking in this group still may not have been very high. As Dunnette et al. (1962) showed, applicants can fake but few actually do. Therefore, it is likely that even those within the top half were not fakers. Instead of splitting the sample in half, Hough et al. should have also examined the top one-third or top ten percent of scorers on the social desirability scale within the incumbent sample. Perhaps if this was done the criterion-related validity would have dropped substantially for the "overly desirable" group. It may also be that the individuals in the "overly desirable" group had true high scores on the measure.

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As discussed earlier, Ellingson et al. (1999) reanalyzed the Hough et al. (1990) data and found that the corrected mean score was lower than the mean score in the fake condition but greater than the mean score in the honest condition. It was also found that as the percentage of individuals faking increased, the proportion of correct selection decisions decreased. Therefore, using the same data as Hough et al., Ellingson et al. concluded that applicants can fake a personality measure and faking will change rank orders. If rank orders change, faking has the potential to influence the criterion–related validity of these measures. They also demonstrated that the effects of faking are not easily corrected.

It should be pointed out that, since the Ellingson et al. (1999) study used the same data as Hough et al. (1990) it is subject to the same criticisms. That is, the Ellingson et al. Study may be flawed because it used a lie scale to assess faking. Therefore, we cannot be sure if the results would hold if an alternative measure of faking was used.

The study discussed earlier by Christiansen et al. (1994) also examined how the criterion-related validity of a personality measure would be affected if corrected for faking. From the 495 participants, 84 were selected for an upper level supervisory position on the basis of the assessment center evaluation. Using the data of the 84 selected applicants for whom criterion data were available, the effect of correction on the criterion-related validities of the traits relevant to the job was examined. Using faking-corrected scores and through using hierarchical regression analyses, it was determined that there was not a significant increase in the criterion variance explained by corrected scores beyond that explained by uncorrected scores. The authors concluded that this

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demonstrated that faking does not influence the criterion-related validity of the personality measure.

Barrick and Mount (1996) administered the Personal Characteristics Inventory (a measure of the FFM of personality) and Paulhus' Balanced Inventory of Desirable Responding (which measures both self-deception and impression management) to two samples of long-haul semi-truck drivers. They found that both impression management and self-deception related to scores on the personality scales. However, they found that faking on the tests did not significantly reduce their predictivness of supervisor ratings or voluntary turnover.

The strongest evidence that faking does not alter criterion-related validity comes from a meta-analysis conducted by Ones et al. (1996). They meta-analyzed the social desirability literature to determine if social desirability operates as a predictor of some criteria, as a mediator, or as a suppressor when personality tests are used as a predictor. They also set out to determine if social desirability is related to real individual differences in personality. They did this by comparing scores on social desirability scales to self and other reports on a personality inventory. The authors found social desirability correlated .37 with emotional stability, .20 with conscientiousness, .14 with agreeableness, and .06 with extroversion. There was no correlation between openness to experience and social desirability. To determine if these correlations represent the susceptibility of these scales to socially desirable responding or if the correlations represent the relation of social desirability to substantive personality constructs, other people's (e.g., friends, relatives) ratings of an individual on the personality measures were examined. There were similar correlations between social desirability scale scores and others' ratings of personality.

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Thus, there is evidence that social desirability is related to real individual differences in personality.

Ones et al. (1996) also found that social desirability did not predict job performance. They calculated the operational validity of social desirability's prediction of five frequently used performance criteria (school success, task performance, counterproductive behaviors, training success, and supervisory ratings of job performance). Only training success was predicted by social desirability scale scores.

To be a mediator social desirability must correlate with both job performance and personality variables. Because it does not correlate with most performance criteria, social desirability does not act as a mediator. In addition, by partialling social desirability from personality measures, Ones et al. (1996) were able to investigate if such responding impacts the criterion-related validities of the Big Five variables. When this was done, the validities did not change; thus, it was concluded that social desirability did not attenuate the criterion-related validity of the personality measures. It was also determined that social desirability did not act as a suppressor.

The authors concluded that the concern with the susceptibility of personality tests to socially desirable responding is needless. Their results indicate that social desirability measures true variance in personality and does not attenuate criterion-related validity. Therefore, controlling for such responding is not necessary. However, Ones et al. (1996) may have overstated their findings because the study contained a major flaw.

The scales used by Ones et al. (1996) to measure socially desirable responding not only measure respondents' conscious attempts to present a positive image (impression management) but also measure the self-deception component of social desirability. For

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example, it has been shown that the Marlow-Crowne scale and the MMPI K Scale, which were used by Ones et al., load on both the self-deception and impression management factors (Paulhus, 1984; 1986). Therefore, these scales are not only measuring conscious attempts to distort, but also the unconscious component.

It is not surprising that measures of self-deception would correlate with personality constructs and not attenuate the criterion-related validity of the personality measures. It has been suggested that if self-deception is controlled for, the usefulness of self-reports may actually decrease because self-deception explains true personality variance (Zerbe & Paulhus, 1987). Therefore, it may very well be that the self-deception component of social desirability was the cause of the correlation between desirability scale scores and personality dimensions. It would be interesting to see if this relationship still existed if self-deception was partialled out of the social desirability scores.

Ones et al. (1996) *can* conclude that the aggregate social desirability scale scores (measuring both self-deception and impression management) used in their analyses correlated with personality dimensions and did not attenuate the validity of personality tests. However, they *cannot* conclude anything about the relationship between respondents' conscious attempts to distort their answers to present themselves positively and personality dimensions or validity. Therefore, Ones et al.'s suggestion that faking should be of no concern to those using self-reports of personality is premature. Perhaps self-deception should be of no concern; however, there is still no conclusive evidence which indicates that applicant attempts to consciously distort responses do not bias personality test results.

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Zickar et al. (1996) used a different approach to analyze the effect that faking has on personality measures. They conducted a Monte Carlo simulation using Item Response Theory (IRT) to model faking on a personality test. Results indicated that across different faking magnitudes (variance of faking between respondents) and faking prevalence rates (percentage of fakers in a validation sample), validities did not decrease a significant amount. Also, the mean observed scale difference between validation samples in which fakers were included and samples with no fakers was less than .25 standard deviations for all but the most extreme faking conditions. Thus, faking was not shown to moderate validity. However, their findings indicated that as the percentage of fakers in the sample increased, the percentage of fakers in the top end of the distribution increased as well. Such findings have implications for top-down selection procedures as this indicates that the fakers may be the ones selected.

Zickar (1997) sought to replicate the findings of Zickar et al. (1996) but based his analysis on a more complex and realistic model of faking. He examined a computer simulation of faking on the Work Orientation scale from the Army's Assessment of Background and Life Events (ABLE). In this simulation, Zickar considered three factors not considered by Zickar et al. First, he varied the variance of faking magnitude or the variance between subjects in the extent of faking. Second, the correlation between faking magnitude and the latent trait was systematically manipulated. Finally, the percentage of items deemed fakable was varied. Results indicated that as the variance of faking increased, the validity decreased; however, this correlation was small and not significant. The percentage of items faked had a curvilinear effect on the validity correlation; that is, the most distortion occurred when 75% of the items were fakable and the least amount of

distortion occurred when 25% and 100% of the items were fakable. The correlation between the squared percentage of items faked with observed validity was significant (r = -.61), while the correlation between faking magnitude and the latent trait and observed validity was not significant. Overall, validity was influenced the most when the variance in faking was large, there were negative correlations between faking and the latent trait, and 75% of the items were faked. Thus, it seems that in specific situations, faking may decrease the criterion-related validity of non-cognitive measures.

Hough (1998), in an effort to determine why there are such discrepancies in the literature regarding the relationship between faking and criterion-related validity, sought to determine the cause of these differences. Hough suggested that the different findings were a result of the different samples that were used. By reviewing much of the literature she determined that when incumbents are examined, little faking (as measured by scores on a lie scale) occurs and criterion-related validity is unchanged as a result of this faking. When applicants serve as subjects, more faking (as measured by a lie scale) occurs, therefore the criterion-related validity is slightly compromised, but not substantially. Finally, when research subjects are the focus of the study and are directed to fake (i.e., there is a large amount of faking) criterion-related validity will be substantially reduced. Therefore, Hough concludes that faking in real selection contexts should not be a concern because applicants do not distort responses enough in such situations to reduce the criterion-related validity substantially.

There is one major problem with Hough's (1998) reasoning. She failed to consider that the measure of faking (i.e., the operationalization of faking) may explain these discrepancies between the type of sample used and how faking influences the

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criterion-related validity. For example, in all the studies cited by Hough that used incumbent and applicant samples, a lie scale was used as the measure of faking. However, when subjects are directed to fake there is no need to use the lie scale as an estimation of faking because it is already known which group of people faked (i.e., those instructed to do so). Therefore, again, it may be the failure of these lie scales to isolate conscious distortion that accounts for the discrepancies of these results. The lie scales used in studies involving incumbents and applicants may not really be identifying those who are faking. Thus, we cannot know what the true relationship is between faking and criterion-related validity in these samples.

Hough (1998) went on to conduct a study that investigated how applicant faking related to criterion-related validity. However, once again, a lie scale was used as the measure of faking. Therefore, it was found that faking did not alter the criterion-related validity of the personality measure in question. Again, we cannot be sure to what extent this scale isolates conscious distortion; therefore, these results are not conclusive.

Construct Validity. Studies have also found that faking may influence the construct validity of personality measures through distorting factor structures, the correlations between personality scales, and the reliability of scales. As discussed earlier, Schmit and Ryan (1993) administered the NEO-FFI to applicants and non-applicants (students). They found that factor intercorrelations were higher for the applicant sample when compared to intercorrelations in the student sample. Moreover, the five factor model fit the data from the student sample, but the applicant data contained six factors. The first factor for the job applicant sample was "a large, work-related personality-characteristic dimension" which they labeled the "ideal-employee factor." This factor

contained most of the items from the Conscientiousness scale, but also included items from the other four scales. One possible explanation for this extra factor is that applicants, unlike students, wish to present themselves as a good employee. It may be that applicant faking created this first factor.

A similar study was conducted by Cellar, Miller, Doverspike, and Klawsky (1996). They examined the factor structure of the NEO-PI based on a sample of 423 flight attendant trainees. They also found that the six factor model fit better than the five factor model. However, the sixth factor in their study did not appear to be an ideal employee factor. Instead, this factor appeared to be a method factor. The differences between the findings of Schmit and Ryan (1993) and Cellar et al. may be attributed to the samples used. Schmit and Ryan used actual applicants while Cellar et al.'s sample consisted of trainees who had already passed the selection procedure to become a flight attendant. Therefore, it seems that the sample used by Cellar et al. may have been less motivated to do well or to fake. Cellar et al. also examined applicants for a different type of job. These two influences could have resulted in different factor loadings on the sixth factor.

When Ellingson et al. (1999) reanalyzed the Hough et al. (1990) data they also determined the effect of correcting for social desirability on the construct validity of the measure. Ellingson et al. factor analyzed the faked scores, the corrected scores, and the honest scores for the ten personality scales. Results indicated a one factor solution for both the fake and corrected scores, but a two factor (more complex) solution for the honest scores. The authors concluded that faking dissolved the complex

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multidimensionality of their measure. Thus faking results in scores which no longer reflect personality traits but instead reflect faking behavior.

However, Ellingson et al. (1999) operationalized social desirability by artificially inducing its presence. That is, they specifically asked participants to fake good. Perhaps giving direct instructions to distort responses produced an extremely distorted socially desirable response set consistent with an overpowering social desirability factor. An applied setting may elicit a form of faking that is more refined and may have a different effect (or no effect) on the factor structure of a personality measure. To deal with this limitation of Ellingson et al. (1999), Ellingson, Smith, and Sackett (1999) examined how socially desirable responding influenced the factor structure of personality measures across four different samples. One sample included incumbents, the second examined applicants, the third included applicants and incumbents, and the fourth included applicants, incumbents and students. Additionally, each data set included a different personality measure, and different data collection contexts (i.e., applicants took it within selection context, students within a research context, and incumbents within a developmental context). Within each sample, the fit of the factor structure of the personality test in question was examined separately for those who were labeled "low in social desirability" and those considered "high in social desirability." Note that individuals were categorized as low and high socially desirable responders according to their scores on a lie scale (those who scored in the top and bottom third of the scale were examined).

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Results indicated that correlation matrices computed among the personality scales for the high and low social desirability groups were highly similar. Additionally, across all samples the factor structure models demonstrated good fit when applied to the high socially desirable groups. However, socially desirable responding did seem to affect measurement error as error variances were not equal across the two groups (i.e., those high and low in socially desirable responding). However, the authors concluded that faking did not result in significant changes in factor structures.

In a study discussed previously, Barrick and Mount (1996) found neither self-deception nor impression management altered the criterion-related validity of a personality test. However, they did find that the correlations observed between the personality scales were higher within their applicant sample as compared to non-applicant samples. They suggest that this increase in the relationship among the scales is a result of applicant faking. Therefore, although they find no support that faking results in a decrease in criterion-related validity, they did provide some evidence that the construct validity of personality measures may be compromised by faking.

McCrae and Costa (1983) found evidence that construct validity is not compromised by applicant faking. They administered the NEO inventory and two social desirability scales (the Marlowe-Crowne and the Lie scale from Form A of the Eysenck Personality Inventory) to 215 volunteers. Six months later, the subjects' spouses were asked to rate their spouse's personality using the same personality inventory completed by the subject. All correlations between the self and spouse ratings were significant and ranged from .25 to .61. Partialling social desirability out led to significant correlation increases between self and spouse ratings for only two traits (activity and positive

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emotions). For most traits, the correlation between the self and spouse ratings dropped when social desirability was partialled out. The authors concluded that these findings support the argument that socially desirable responding does not decrease the validity of personality measures. However, McCrae and Costa only examined socially desirable responding, not intentional faking. The subjects in their study were volunteers so they were most likely not motivated to fake their responses. Also, the two social desirability scales used were not designed to detect respondents' attempts to consciously distort answers. These measures tap both self-deception and impression management. Therefore, this study may say little about how faking distorts the validity of personality tests.

Collins and Glese (1998) found that the Five Factor Model held in an applicant sample. Assuming that the applicants in their sample faked, this is evidence that faking did not influence the construct validity of the personality measure.

Ones and Viswesvaran (1998) conducted a meta-analysis to determine the influence of social desirability on the convergent and divergent validity of a personality scale. The number of correlations analyzed exceeded five thousand and the total sample size was over 4 million. Results indicated that both convergent and discriminant validities of the Big Five dimensions remained relatively the same when social desirability was partialled out. The authors conclude that their findings indicate that faking does not adversely influence the construct validity of personality measures. However, again, the meta-analysis included studies that operationalized faking as scores on a lie scale. Therefore, we cannot be sure if results would be the same had intentional distortion been isolated and partialled out of the correlations.

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Douglas et al. (1996) found faking distorted the construct validity of their personality and biodata scales as measured by multi-trait and multi-method analyses and factor analyses. When the data of those in the honest condition were examined using an exploratory factor analysis, four factors were generated. All four were construct factors. The data from the faking applicants generated four factors but two were method factors and two were construct factors. Thus, a measure based on the responses of honest respondents may have a different structure when respondents fake. This is similar to what Schmit and Ryan (1993) found with the five factor model of personality. Although this study tells us that, when asked to fake, the responses of subjects may distort factor structures, it is not known if the extent to which applicants really fake would be enough to distort the factors in this way. Nonetheless, these results provide evidence that if faking does occur it may change the psychometric properties of the measure.

Douglas et al. (1996) also found that internal consistency reliabilities for their four scales were higher for the faking condition than for the honest condition. McFarland, Ryan, and Ellis (2000) found that when individuals were instructed to fake a personality measure, the reliability of the scales increased. These findings suggest that honest responding may hurt the homogeneity of the items. Subjects who fake make more of an effort to report consistently positive responses. In other words, faking creates an artificial inflation of consistency. Honest applicants report inconsistent behavior because their behavior truly is inconsistent. If it is true, and alphas do change as a result of faking, then these measures are tapping both conscious distortion and the construct which they were designed to measure. This is a violation of the unidimensionality assumption

of alpha. This implies that alpha should perhaps not be used as a measure of reliability in settings where some respondents may be faking their answers.

<u>Summary</u>

There is still debate about the way in which faking influences test decisions. Although there is consistent evidence that applicant faking may alter selection decisions, it is less clear how the criterion-related validity and construct validity of these measures is affected. There are several reasons this area may be plagued by such discrepancies.

First, as stated earlier, the operational definition of faking changes across these studies. Some studies use a score on a lie scale as a measure of faking, while others use one item to measure faking, and still others examine increases in scores from an honest condition. Second, it may be that different designs produce different results. Some studies have used within-subject designs, while others have use between subject designs. Third, the samples used may explain these discrepancies. Some studies have used college students and asked them to pretend to be applicants, while others have used applicants or incumbents to determine the effect of faking on validity. Finally, perhaps findings are so discrepant because we still do not understand how various factors may influence the results of each study (e.g., situational cues, knowledge of what the test is measuring). For example, some studies asked participants to fake good but also warned them that a lie scale was included on the measure (Doll, 1971; Hough, 1990), while others have included no such additional instructions (McFarland & Ryan, in press; Pannone, 1984). Some have explained to participants what types of constructs the test in question measures (Cunningham et al., 1994; Dwight & Alliger, 1997), while many have not done this (Hough, 1998). Therefore, it is not surprising that studies have found such

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conflicting results. Before we can really understand how faking may influence test results, we have to understand how these different factors may influence faking behavior. That is, we need a model of faking behavior to understand how and in what way various factors may influence applicant faking.

A Model of Faking

This study is being conducted to test the model of faking behavior presented in Figure 1. This model is being proposed as a way to predict, explain, and ultimately to understand faking behavior. This model does not address all of the possible explanations for the discrepancies in the faking literature (e.g., study design). However, it does address issues of the operationalization of faking and understanding of the various influences on faking behavior.

The current study will examine the usefulness of this model for predicting faking on a personality measure, it may be used to understand faking behavior on any self-report measure (e.g., biodata form, integrity test). Below, each variable in this model and its relationship to faking behavior will be described.
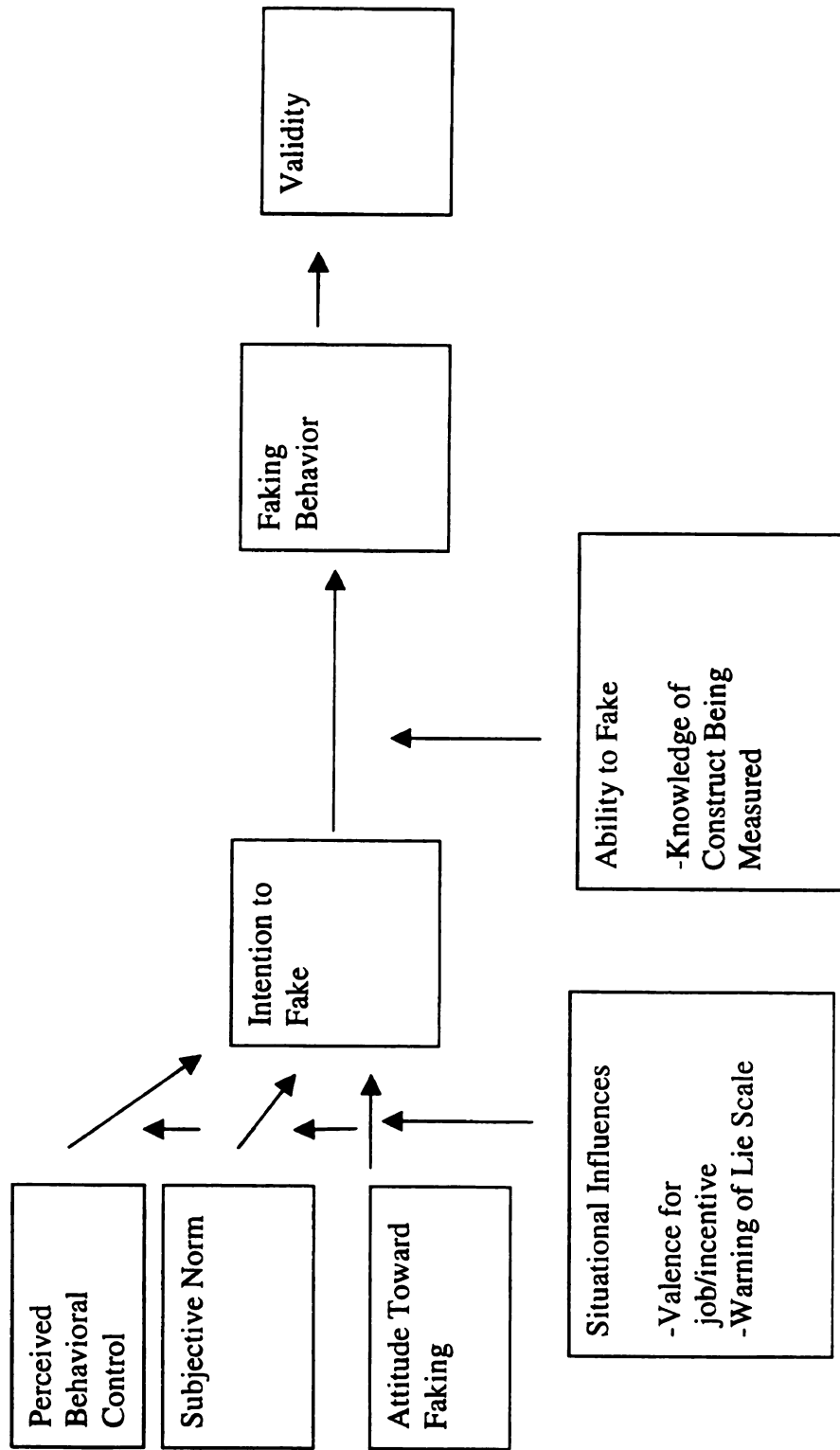
Figure 1. Model of Faking Behavior.

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<u>Attitudes, Beliefs, and Perceptions of Control, and Intention to Fake</u>. The theory of planned behavior, originally proposed by Ajzen (1985), will be used to predict faking behavior. This theory has been used extensively within social psychology to predict intentions, and (through intentions) behavior (Ajzen, 1991; Ajzen & Madden, 1986; Norman & Conner, 1996; Reinecke et al., 1996; Schifter & Ajzen, 1985). For example, the theory of planned behavior has been used to predict condom use (Boldero, Moore, & Rosenthal, 1992; Reinecke et al., 1996), recycling (Boldero, 1995), success quitting smoking (Godin, Valois, Lepage, & Desharnais, 1993), and stealing (Beck & Ajzen, 1991). According to this theory, one's attitude toward the behavior, subjective norms, and perceived behavioral control predict the intention to perform the behavior. In turn, intentions predict behavior.

Attitude toward the behavior refers to the degree to which a person has a favorable or unfavorable evaluation of the behavior in question. More specifically, it is an evaluative appraisal of the consequences of acting or not acting (Ajzen, 1991). This appraisal may be either favorable or unfavorable, and is generally expressed with semantic differential items (e.g., good-bad, pleasant-unpleasant).

Subjective norm is a social factor predicting intentions. This refers to the perceived social pressure to perform or to not perform the behavior. For example, items used to measure subjective norms assess the likelihood that important individuals (e.g., parents, friends) would disapprove or approve of the behavior. Therefore, an individual who thinks friends and family members who are important to them would disapprove of faking on a personality test will have a negative subjective norm for faking behavior.

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Perceived behavioral control is the third factor proposed to predict intentions. This refers to the individual's belief regarding the ease or difficulty with which the behavior can be performed. Individuals who believe they can perform the behavior are more likely to intend to behave in that manner.

The theory of planned behavior is an extension of the theory of reasoned action (Fishbein & Ajzen, 1975). Unlike the theory of reasoned action, the theory of planned behavior does not assume that the behavior in question is completely under one's volitional control. Therefore, the theory of planned behavior goes a step further than the theory of reasoned action by considering an individual's perception of control over the behavior in question. It is believed that the inclusion of this additional variable is why the theory of planned behavior has been shown to predict behavior better than the theory of reasoned action (Ajzen, 1991).

The theory of planned behavior will be used to predict the intention to fake in the following way. Specifically, it is hypothesized that attitudes toward faking personality tests in selection contexts, subjective norms of faking (i.e., perceptions of whether or not important others would approve or disapprove of faking behavior), and perceptions of whether or not one could fake the test, will have a direct effect on the intention to fake.

It has been suggested that attitudes regarding dishonesty (whether it be theft, cheating, lying, etc.) are related to intentions to behave in a dishonest manner. For example, it has consistently been found that positive attitudes toward a behavior lead to greater intentions to perform that behavior (e.g., Ajzen, 1991; Boldero, 1995; Boldero et al., 1992). Beck and Ajzen (1991) found that attitudes toward dishonest actions predicted intentions to cheat on a test, shoplift, and lie. Since positive attitudes toward a behavior

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have consistently been linked to stronger intentions to perform that behavior, the following prediction will be made:

Hypothesis 1: Attitude toward faking will be positively correlated with the intention to fake.

It has been suggested that the belief that certain dishonest behaviors are common or acceptable is a determinant of many forms of dishonesty (Murphy, 1993). For instance, it has been found that an individual is more likely to steal if he or she believes that most people steal (O'Bannon, Goldinger, & Appleby, 1989). Most honesty tests are based on evidence that suggests that individuals who are more likely to commit thefts and other counterproductive acts are more likely to believe that such behavior is accepted by society (Hollinger, 1989).

Research on the theory of planned behavior has consistently found that when subjective norms toward a behavior are positive, the intention to perform that behavior will be greater (Ajzen, 1991; Beale & Manstead, 1991; Schifter & Ajzen, 1985). This means that when individuals perceive that important others would approve or encourage the behavior in question, they are more likely to intend to engage in that behavior. For example, Beck and Ajzen (1991) found that favorable subjective norms of cheating, shoplifting, and lying were positively related to intentions to behave in these ways. That is, individuals who thought their friends and family would approve of such behavior (or not disapprove) would be more likely to intend to engage in these behaviors. It is expected that these results will generalize to faking on a selection test.

Hypothesis 2: Subjective norms toward faking will be positively related to the intention to fake.

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Perceived behavioral control is the third variable in the theory of planned behavior that is hypothesized to influence intentions. Ajzen (1991) suggests perceived behavioral control refers to an individual's perception of the ease or difficulty of performing the behavior of interest. As Ajzen points out, this definition is very similar to Bandura's perceived self-efficacy (Bandura, 1986; 1991). He suggests that the theory of planned behavior places this construct (self-efficacy) within a more general framework of the relations among beliefs, attitudes, intentions, and behavior. Therefore, Ajzen suggests self-efficacy is very similar to, if not the same as, perceived behavioral control. However, since the model in this paper is integrating the theory of planned behavior to predict faking behavior, the term perceived behavioral control will be used instead of self-efficacy because this is the term used within the theory's framework.

Research has consistently found that perceived behavioral control is positively related to intentions such that when individuals perceive that they have control over the behavior in question, the intention to perform the behavior is greater (Ajzen, 1991; Beale & Manstead, 1991; Schifter & Ajzen, 1985). For example, Beck and Ajzen (1991) found that perceptions of behavioral control were highly and positively correlated with intentions such that those who believed they had control over cheating, lying, and shoplifting were more likely to intend to behave in these ways. Similar results are expected in this study, such that individuals who perceive control over faking behavior (i.e., those who think they could fake a personality selection test) will have greater intentions to fake.

Hypothesis 3: Perceived behavioral control of faking will be positively correlated with the intention to fake.

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Intention to Fake and Faking Behavior. It has consistently been found that the intention to engage in a behavior is related to performing the behavior in question. That is, the greater the intention to perform a certain behavior, the greater the likelihood that the behavior will be performed (Ajzen, 1991; Ajzen & Fishbein, 1980; Beale & Manstead, 1991; Beck & Ajzen, 1991; Boldero, 1995; Boldero et al., 1992; Fishbein & Ajzen, 1975; Schifter & Ajzen, 1985). Intentions have been shown to predict everything from losing weight (Schifter & Ajzen, 1985) to stealing (Beck & Ajzen, 1991). Therefore, in the present study it is expected that a similar relationship between the intention to fake and faking behavior will be found.

Hypothesis 4: The intention to fake will be positively related to faking behavior such that those with a greater intention to fake will fake to a greater extent.

Although the relationship between intentions and behavior is strong, it is far from perfect. This indicates that there are other variables at work that are influencing this relationship. The same is true of the relationship between intention to fake and faking behavior. The faking literature suggests some moderators of these relationships. Therefore, the model of faking presented in this paper integrates the theory of planned behavior with some variables that have empirically been shown to affect faking behavior. Specifically, it is expected that situational factors will moderate the relationships between attitudes toward faking, subjective norms toward faking, and perceived behavioral control and the intention to fake (see Figure 1). Additionally, ability to fake (i.e., knowledge of the constructs being measured) will moderate the relationship between the intention to fake and faking behavior. A detailed discussion of these moderators will now be presented.

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<u>Moderators</u>. Although the theory of planned behavior has been successful at predicting intentions and behavior, some have suggested that the theory would explain more variance if moderators were considered in the framework (Bagozzi, 1992). Therefore, other predictors of the intention to fake and faking behavior will be examined within the context of the theory of planned behavior.

Research in the faking literature has found that there are multiple situational factors that may influence faking behavior in applied settings. These situational factors may be situations the applicant is placed in with respect to his or her life circumstances, or may be more directly related to the selection context (e.g., valence for a job, concern with getting caught faking). For example, it has been suggested that when valued outcomes are at stake, applicants will fake more (Mabe & West, 1982). Therefore, an individual in a financial crisis may really need the job in question such that she would fake to ensure she gets the job. It has also been shown that the more desirable a job, the greater the likelihood that the individual will fake responses on a selection test (Fletcher, 1990). Therefore, there is evidence that valence for a job may predict faking behavior. In general, it has been suggested that the more desirable the outcome is (doing well on a test), the more likely an individual is to distort his or her responses (Schlenker & Weigold, 1992). In the present study, although valence for a specific job will not be measured (as this is a lab study), the valence one has for the outcome of doing well on the test will be examined. Specifically, a monetary incentive will be offered to some participants, such that those who perform well on the test will receive money (this will be explained in more detail below).

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As Figure 1 shows, situational factors will moderate the relationship between attitudes toward faking, subjective norms toward faking, and perceptions of behavioral control and the intention to fake. It is expected that even if an individual has a negative attitude toward faking (or negative subjective norm toward faking), the individual may still intend to fake if the outcomes of doing so are very desirable, or if the consequences of not doing so are negative (e.g., not obtaining a job). Likewise, even if an individual does not think he or she can fake a personality test, he or she may still intend to fake if the consequences of doing so are very positive. In the present study it is predicted that those who are offered an incentive will have more positive valence for doing well on the test. Based on the above discussion, the following predictions will be made:

Hypothesis 5a: Valence for doing well on the test will moderate the relationship between attitudes toward faking and intention to fake such that those with high valence will have a greater intention to fake, regardless of attitudes toward faking.

Hypothesis 5b: Valence for doing well on the test will moderate the relationship between subjective norms for faking and intention to fake such that those with high valence will have a greater intention to fake, regardless of their subjective norm of faking.

Hypothesis 5c: Valence for doing well on the test will moderate the relationship between perceptions of behavioral control toward faking and the intention to fake such that those who have high valence will have a greater intention to fake, regardless of their perceptions of behavioral control.

Characteristics of the specific selection situation may make applicants more or less likely to fake. For instance, it has been suggested that concern with getting caught faking may decrease faking behavior on selection tests. Research has consistently found

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that in self-report measures, verifiable and objective items are faked less than items that are unverifiable and subjective (Becker & Colquitt, 1992; Shaffer, Saunders, & Owens, 1986; Stokes et al., 1993). For example, Stokes et al. found that applicants faked verifiable items less than items measuring personal preferences. Research has also found that when applicants are told their test answers will be verified, they fake less than applicants who are not given such information (Hough et al., 1990; Mael, 1991). It has been suggested that this decrease in applicant faking (as measured by both lie scale scores and mean differences from honest groups on the test) on verifiable and objective items (and when told their responses will be verified) is because applicants fear that their attempts to fake will be detected. If applicants are identified as fakers, they fear this would result in decreasing their chances for the job (Lautenschlager, 1994). As faking is generally done to increase one's chances of getting a job, it seems that concern with being caught, and thus being eliminated from the selection process, would decrease faking behavior.

When using personality tests, it is less feasible to verify answers or to create objective items (as compared to many biodata forms or integrity tests) because whether or not an individual has desirable personality characteristics is being measured. A measure of this type is inherently less verifiable and more subjective (Wheeler, Hamill, & Tippins, 1996). Therefore, warning applicants that their responses may be verified (in order to reduce faking) may not work as it will be obvious to the applicants that such verification would be difficult, if not impossible, to obtain. Therefore, other means have been used to deter faking on personality tests and other less verifiable self-report measures.

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Research has shown that telling individuals that a lie scale is included on the test that will identify those who are faking responses may decrease faking (Doll, 1971; Kluger & Colella, 1993; Nias, 1972; Schrader & Osburn, 1977; Wheeler et al., 1996). To determine if warning that faking attempts may be detected decreased faking on a test of mental incompetence, Hiscock, Layman, and Hiscock (1994) administered two forced choice tests - a knowledge test and moral reasoning test - to 105 inmates. A third of the inmates were instructed to take the tests honestly, a second third were told to fake bad, and the last third were told to fake bad but also that there were ways that the inmates may be identified as fakers (e.g., by appearing excessively impaired or answering difficult items correctly and easier ones incorrectly). Results indicated that those asked to fake without being warned scored significantly lower on the knowledge test than those who were told to fake but given a warning. Both groups scored lower than those responding honestly. For the moral reasoning test, although not statistically significant, those in the unwarned fake condition scored lower than those in the warned fake condition.

Similarly, Nias (1972) had 262 twelve and thirteen year old children take the junior Eysenck Personality Inventory. All children were told to take the test honestly, but half of them were also warned that a lie scale was included in the measure to detect individuals who may not be responding honestly. Results indicated that the children who were warned of the presence of a lie scale scored significantly lower on the lie scale and had higher mean scores on psychoticism and neuroticism. Nias also examined if the warning reduced faking more than if one just corrected scores for such responding. To test this, those who had high lie scale scores within the group that was not warned of a lie scale were taken out of the sample and the means for the various scales were recalculated

with those people excluded. Results indicated that mean scores on psychoticism and neuroticism were still much lower compared to the means for the group that was warned a lie scale was included on the measure. Therefore, it seems that warning may be more effective at dealing with faking than correcting scores for faking.

Doll (1971) had 300 college graduates receiving armed service training take a biodata instrument under instructions to respond honestly. Some of the subjects were then told to fake to look good but to be prepared to defend their answers, another group was told to fake to look good but to be aware that a lie scale may be included to detect faking, and the last group was told to fake to look as good as possible. Results indicated that subjects could increase their scores by faking and that the largest score increase was in the "fake to look as good as possible" condition while the least amount of increase was seen by those in the condition which instructed subjects to be aware of a lie scale.

Schrader and Osburn (1977) examined how warning that a lie scale was included on a biodata form influenced faking. They asked participants to fake a biodata form. However, before taking the biodata form, half of the participants were told a lie scale was included in the measure to identify those who were giving false responses. It was found that the group warned that a lie scale may have been included in the measure scored a half of a standard deviation lower than those who were not warned.

Recently, Wheeler et al. (1996) replicated these results, finding applicants who were warned a lie scale may be included on a personality measure scored lower on the lie scale and had lower mean scores then applicants not given the warning. It is believed that this occurs because applicants do not want to be identified as liars and thus decrease their chances of being selected for the job (Kluger & Colella, 1993).

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Therefore, it seems that warning that a lie scale is included in the measure may decrease the intention to fake, even if subjective norms, attitude toward faking, and perceptions of behavioral control are high. Therefore, the following hypotheses will be made:

Hypothesis 6a: Warning that a lie scale is included in the measure will moderate the relationship between attitude toward faking and the intention to fake such that those warned that a lie scale is included in the measure will intend to fake less, regardless of their attitude toward faking.

Hypothesis 6b: Warning that a lie scale is included in the measure will moderate the relationship between subjective norm toward faking and the intention to fake such that those warned that a lie scale is included in the measure will intend to fake less, regardless of their subjective norm toward faking.

Hypothesis 6c: Warning that a lie scale is included in the measure will moderate the relationship between perceptions of behavioral control over faking and the intention to fake such that those warned that a lie scale is included in the measure will intend to fake less, regardless of their perceived behavioral control.

Another moderator considered in the model of faking behavior is the ability to fake. The ability to fake refers to how successful an individual is at increasing his or her score through faking, when he or she chooses to fake. An individual may intend to distort responses and attempt to do so, but not have the resources available to successfully increase his score through distortion. That is, he may not have the ability to distort. There are several factors that may increase the ability of an individual to fake self-report measures. One that has been well researched, and will be examined in this

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study, is knowledge of the construct being measured (Cunningham et al., 1994; Dwight & Alliger, 1997; Holden & Jackson, 1981; Napier, 1979).

Goldman and Olczak (1976) examined the effect of knowledge about self-actualization on faking the Personal Orientation Inventory (a measure of self-actualization). All participants first took the inventory under instructions to respond honestly. Then, half of the participants were given a class that described and discussed self-actualization. After this, one-third of the participants were told to take the inventory again but were told to fake bad, one third were told to fake good, and the last third were told to take the test honestly again (in each condition half of the participants had been given information about self-actualization while the other half had not). Results indicated that both groups (those not given information about self-actualization and those given the information) asked to fake good were able to significantly increase their scores and both groups were also able to decrease their scores by a significant amount when asked to fake bad. However, those given knowledge of self-actualization were able to increase their scores more than those not given this information when asked to fake good and decreased their scores significantly more than those not given this information when asked to fake bad. Thus, when provided with information about the construct the test was measuring, participants were able to fake to a greater extent.

Cunningham et al. (1994) explored the effects on test scores of providing specific content information about the major constructs involved in the Reid Report, a popular overt integrity test. The authors instructed all participants to respond to the measure as they would if they were taking the test as a job applicant. However, some participants were given information about the punitive scale (what it was trying to measure and how it

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measured it), some about the projective scale, and some about both dimensions of the test, while another group was given no information about any test dimensions. In an attempt to motivate subjects, they were told that those scoring within the top five percent would receive ten dollars. Results indicated that those who scored lowest on the test were subjects who received no information about what the test measured. Those given information on a dimension of the test had significantly higher mean scores on that dimension as compared to those who did not receive this information. The groups receiving information on both dimensions had the highest mean total scores (.5 standard deviations greater than the control group). However, there were some generalized effects on the non-targeted dimensions such that information on one dimension increased the mean score on the other dimension slightly. These results suggest that applicants can improve scores through faking *if* they are provided with information about the test. These findings may be exaggerated given that students served as subjects. Real applicants may give more thought (due to greater motivation) to what the test is trying to measure and not need to get information on the nature of the test because they would be motivated to figure it out on their own. However, such information may have helped students who have less experience with selection tests and who are less motivated to increase scores through faking. Nonetheless, this study indicates that the ability to figure out what the test is measuring may be a good predictor of faking.

Dwight and Alliger (1997) replicated the findings of Cunningham et al. (1994). They had 120 undergraduates take the Employee Integrity Index (EII) which is an overt integrity measure developed by Ryan and Sackett (1987). Subjects were placed in one of three conditions: fake good, honest, or a coaching condition. Those in the coaching

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condition were told what integrity tests are designed to measure and taught how to respond to such items so as to obtain a high score. Results indicated that the mean score of the coaching condition was significantly higher (by almost .5 standard deviation units) than the mean score of those in the fake good and honest conditions. The mean score of the fake good group was also significantly greater (by over one standard deviation) than those in the honest condition. Cunningham et al. (1994) and Dwight and Alliger (1997) demonstrated that coaching individuals about the nature of the construct being measured increased individuals' ability to increase their scores.

It should be noted that no research could be found that has examined knowledge of constructs and faking on personality measures. There is no reason why the results pertaining to integrity tests should not also apply to personality testing. Both are self-report measures. Also, although not empirically tested, it has been suggested that applicants will be more successful at increasing their scores on personality measures through faking if they can tell what the test is measuring (Anastasi & Urbina, 1997).

Therefore, it is predicted that knowledge of the constructs that the test measures will moderate the relationship between the intention to fake and faking behavior. Again, an individual may intend to distort responses, but not have the necessary ability to do so. Hypothesis 7: Knowledge of constructs measured will moderate the relationship between the intention to fake and faking behavior such that those who are given information about the constructs measured on the personality test and have a high intention to fake the test will increase scores more than those with similar intentions but not given this information.

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It should be noted that the present study will operationalize faking in two ways: scores on a lie scale and difference scores (i.e., change in scores from the honest to applicant condition). As discussed previously, one of the reasons findings may be so discrepant in this literature is a failure to have one operationalization of faking across studies. Some studies use a difference score to measure faking, while others use lie scale scores. To determine if the results are consistent across different measures of faking, the present study will explore the effects on faking and the effects of faking, using both a lie scale and difference scores.

Faking and Validity. As discussed, one of the main reasons faking is even a concern to those using selection tests is the potential impact faking may have on the validity of personality measures. Therefore, in addition to testing the hypotheses above, the extent to which faking influences the validity of the personality measure will also be examined using GPA as the criterion measure.

Research has demonstrated that conscientiousness is a valid predictor of job performance. Additionally, conscientiousness has been shown to predict educational criteria such as GPA. Therefore, to determine if the validity of the test changes as a result of faking, GPA will be correlated with conscientiousness test scores from both the honest and the applicant conditions. If the correlations change substantially across conditions, we can be fairly certain that faking is changing the validity. However, given that the empirical results are mixed with regards to how faking may influence the results of such tests, no hypotheses will be made, but the issue will be explored.

Sample

Participants were 547 undergraduates from a large university in the Midwest. This sample size results in considerable power to detect even small effects (Cohen, 1988; 1992). Table 1 contains specific information about the demographic makeup of the participants. As the table shows, the majority of the participants were White females under the age of 20.

Table 1. Demographic Characteristics of Study Participants

Total Sample Size	570
Sex	
Male	171 (30%)
Female	376 (66%)
Race	
White	426 (74.7%)
African American	45 (7.9%)
Hispanic	11 (1.9%)
Asian	40 (7.0%)
Other	21 (3.7%)
Age	19.80 (2.96)
Past Experience With Personality Tests	
Yes	405 (71.2%)
No	43 (7.5%)

Design

A 2 (incentive vs. no incentive) x 2 (warning of lie scale vs. no warning) x 2 (knowledge of constructs vs. no knowledge) between-subjects design was used. For the first factor, participants were asked to respond to a personality measure as if they were an applicant for a job they would like to have. However, approximately half of the

participants (n = 253) were also given an incentive such that they were told that those who scored in the top 15% would receive $20 while the other participants (n = 294) were not offered this incentive. Other studies have shown that $20 is seen as very desirable to undergraduates (McFarland & Ryan, in press; McFarland et al., 2000). Therefore, it is reasonable to expect that those offered this incentive would have significantly higher valence toward doing well on the test than those not offered the incentive. The second factor was warning of a lie scale. Approximately half of the participants (n = 285) were told that a lie scale was included in the measure so that fakers could be identified, while the other half (n = 262) were not told a lie scale was included in the measure. The third factor was knowledge of constructs being measured. Half of the participants (n = 276) were told what constructs the personality test measured and these constructs were defined for them prior to taking the personality test. The other half of participants was not given this information (n = 271).

It should be pointed out that in addition to taking the personality test under instructions to respond like an applicant, participants also took the personality test under instructions to respond honestly. These honest responses were only collected so that difference scores could be created (these difference scores serve as one measure of faking behavior). However, the honest and applicant conditions were counter-balanced to determine if the order of the conditions effected results.

Pre-questionnaire. Approximately one week before attending the experiment, participants took the pre-experiment questionnaire (as a pre-questionnaire on the Internet). This measure included items that measured participants' attitudes toward faking on selection tests, subjective norms for faking, perceptions of behavioral control

over faking, and intentions to fake in the future on selection tests (pre-intentions to fake). In the study conducted by Beck and Ajzen (1991), participants were asked to respond to similar items (e.g., items that asked about attitudes, subjective norms, and perceived behavioral control toward stealing and cheating). The results indicated that individuals were very willing to indicate what would be perceived as undesirable responses (e.g., positive attitudes toward theft). However, to increase the likelihood of honest responses to this measure, participants were told that their responses would be kept completely anonymous and that the answers they provided would not affect them in any way. Additionally, they were encouraged to respond honestly. All items were adapted from Beck and Ajzen who found adequate reliabilities for the above measures (all greater than .65). See Appendix A for a reproduction of these measures.

Attitude Toward Faking. Participants indicated their attitudes toward faking a selection test on five, 5-point semantic differential-type response scales: good-bad, pleasant-unpleasant, foolish-wise, useful-useless, unattractive-attractive. The internal consistency of this scale was .86.

Subjective Norms. Four items were used to measure subjective norms toward faking. These items were answered on a 5-point scale from strongly disagree to strongly agree. An example of an item on this scale is "Most people who are important to me would look down on me if I lied on a selection test." The internal consistency of this scale was .73.

Perceived Behavioral Control. Three items were used to measure perceived behavioral control over faking. These items were on a 5-point scale from strongly disagree to strongly agree. An example of an item on this scale is "It would be easy for me to lie on a selection test." The internal consistency of this scale was .79.

Pre-Intentions to Fake. Generally, intention measures are given prior to the performance of the behavior that is to be examined (Ajzen, 1991). Therefore, ten items were used to measure intentions to fake in the future on selection tests. Items were on a 5-point scale from strongly disagree to strongly agree. An example of an item on this scale is "I would NEVER lie on a selection test." The internal consistency of this scale was .89.

Personality Test. The NEO-FFI (Costa & McCrae, 1989), a measure of the five factor model of personality, was used to assess personality. This measure is a short version of Costa and McCrae's (1985) NEO-PI. The five factors measured are neuroticism, extroversion, openness to experience, agreeableness, and conscientiousness. Each of the five factors are measured by 12 items with a Likert response format ranging from 1 (strongly disagree) to 5 (strongly agree). The internal consistencies for each of the scales across both the honest and applicant conditions are reported in Table 2.

Table 2. Reliabilities of Personality Scales Across the Honest and Applicant Conditions,
and Reliabilities of Difference Scores

Scale	Honest	Applicant	Difference Score
Neuroticism	.88	.88	.61
Extroversion	.84	.84	.42
Openness	.76	.72	.21
Agreeableness	.77	.78	.24
Conscientiousness	.86	.88	.75

Post-Questionnaire. A post questionnaire was handed out after the participants took the personality test under the applicant condition. Most of the items were used as manipulation checks (i.e., to assess if the manipulations were salient to participants). Intentions to fake the personality measure were also assessed. See Appendix B for a reproduction of this questionnaire.

Valence Toward Doing Well on the Test. This measure assessed how much the participants valued the outcome of doing well on the test. It was anticipated that those who were offered the incentive for doing well on the test would have higher valence toward doing well on the test than those not offered the incentive. Thus, this measure served as a manipulation check to determine if those who were offered the incentive felt more positively toward doing well. Five items were included on this measure. Responses to all items were made on a Likert type response scale ranging from 1 (strongly disagree) to 5 (strongly agree). An example of an item on this measure is "doing well on the test is very desirable to me." The internal consistency of this scale was .90.

Concern With Being Caught Faking. This measure served as a manipulation check. It was used to estimate how concerned individuals were that they would be identified as a faker and whether or not they believed such faking attempts would be detected by the scale. It was expected that those who were warned about the lie scale being included on the test would have greater concern that their false responses would be detected by the lie scale. This measure contained nine items. These items had a Likert type response format ranging from 1 (strongly disagree) to 5 (strongly agree). An

example of an item included on this scale is, "I was concerned that the lie scale would identify me as a liar." The internal consistency of this scale was .77.

Knowledge of Constructs Being Measured. Eight items were used to measure knowledge of constructs being measured. This measure served as a manipulation check to determine if those who were given information about the constructs the personality test was measuring were better able to determine which items related to the constructs. This measure contained items that asked participants to indicate if they could successfully identify the constructs that were being assessed on the test. Items were on a 5-point scale from strongly disagree to strongly agree. An example of an item on this scale is, "Some items measured how reliable and responsible a person is." The internal consistency of this scale was .80.

Post-Intention To Fake. A questionnaire assessing the extent to which participants intended to fake the personality test under the applicant condition was administered. As stated earlier, intention measures are generally given prior to the performance of the behavior that is to be examined (Ajzen, 1991). This is why the present study administered an "intentions to fake in the future" measure on the pre-questionnaire. However, this pre-intention to fake measure was administered about a week before the participants actually took part in the experiment. It has been suggested that, for it to predict behavior, intentions should be measured as close to the time at which the individual has the opportunity to perform the behavior in question (i.e., faking; Ajzen, 1991). In this study, if participants were asked their intentions to fake immediately prior to the administration of the test, it might have influenced their responses on the test (i.e., some people may have never even thought about faking, but since the experimenter asked

about faking right before the test, some participants may have been likely to fake; Feldman & Lynch, 1988). Also, the design is such that post-intentions must be measured to test two of the moderation effects (valence and lie scale). That is, an intention to fake measure must be administered *after* participants are offered the incentive and after they have been told that a lie scale is included on the test. Otherwise, it cannot be determined if these manipulations influenced intentions to fake. For these reasons, intention to fake was also measured immediately following the test administration.

However, just because the post-intention measure was given after the test does not mean that this is simply a measure of faking behavior on the test. Post-intention items asked if individuals intended to fake the test. Some individuals may have intended to do so, but may not have been successful. Therefore, other measures of faking behavior (i.e., difference scores and impression management scale scores) serve as the actual measures of the extent of faking.

This post-intention measure included seven items that used a Likert type response format ranging from 1 (strongly disagree) to 5 (strongly agree). An example of an item on this scale is, "I intended to lie on this test." The internal consistency of this scale was .91.

Demographic Questionnaire. A questionnaire assessing various participant demographics was also included and assessed factors such as race, sex, years in college, and GPA. This measure also had participants indicate the amount of experience they have had with personality tests. This measure also asked participants whether or not they were thinking of any specific job for which they were applying when they took the personality test under instructions to behave like an applicant. If so, they were asked to

indicate what kind of job they imagined they were applying for. See Appendix C for a reproduction of this questionnaire.

Faking Behavior. Across the faking literature, two operationalizations of faking are most often used: lie scale scores and difference scores. Therefore, in this study, faking behavior was measured in these two ways. First, within the NEO-FFI, items from Paulhus' (1984; 1991) BIDR scale were included. This is a lie scale that is used to measure both self-deception (unconscious distortion) and impression management (conscious distortion). The self-deception scale contained 20 items. An example of an item on this scale is "I have not always been honest with myself." The internal consistency of the self-deception scale was .74 in the honest condition and .86 in the applicant condition.

The impression management scale also contained 20 items. Scores on the Impression Management scale were used to measure intentional distortion (i.e., faking; Paulhus, 1986; 1991). An example of an item on this scale is "When I hear people talking privately, I avoid listening." The internal consistency of the impression management scale was .76 in the honest condition and .90 in the applicant condition. Although both self-deception and impression management are measured, we are most concerned with impression management scores because this is the measure used to assess intentional distortion.

Faking behavior was also assessed by examining differences in scale scores across the honest and applicant conditions. Several studies have used changes in test scores from honest to applicant or fake conditions to operationalize faking (i.e., the difference between the two administrations is considered to be due to faking; e.g., Doll,

1971; Ellingson et al., 1999; McDaniel et al., 1997; McFarland & Ryan, in press).

Therefore, a measure of faking behavior (in addition to the impression management scores) was derived for each of the personality scales by subtracting the score received in the honest condition from the score received in the applicant condition. This resulted in 5 difference scores for each individual. These difference scores were used as one measure of faking behavior. By using the difference score we can be more confident that intentional distortion (i.e., faking) is being isolated. Self-deception may occur in both the honest and applicant conditions, but as described earlier, self-deception remains constant across situations (Paulhus, 1986) and so should not influence the difference score.

Table 2 contains the reliability of the difference scores. The following formula was used to calculate the reliability of the difference scores: $r_{dd} = (\sigma_d^2 - \sigma_{ed}^2) \setminus \sigma_d^2$; where $\sigma_{ed}^2 = \sigma_h^2 (1 - r_{hh}) + \sigma_a^2 (1 - r_{aa})$; with h representing the measure in the honest condition, a for the applicant condition, and σ_d^2 representing the variance of the difference score (Rogosa, Brandt, & Zimowski, 1982; Tisak & Smith, 1994). Only two of the difference scores achieved adequate reliabilities (neuroticism and conscientiousness). Thus, further analyses that include the other three difference scores (agreeableness, openness, and extroversion), should be viewed with caution.

Procedure

Participants signed-up on-line via the Internet to participate in the experiment. They were directed to an Internet site and could select to participate in a number of experiments for credit. Before participants could sign-up to participate in the present study, they were required to fill out a 22 item questionnaire. This was the pre-questionnaire containing the attitude toward faking, subjective norms toward faking,

perceptions of behavioral control, and pre-intention to fake measures. Therefore, on average, this survey was taken one week before the participants came to the experiment.

The experiment session began by having participants read and sign an informed consent form (see Appendix D for a reproduction of the consent form). Half of the participants were randomly assigned to take these tests under the honest condition first and the other half were asked to complete the tests under the applicant condition first. In the honest condition participants were given the following instructions:

Please answer the following questions on the personality test as honestly as possible. Your answers will remain completely confidential. Your answers will be used for research purposes only, and will not be used to evaluate you in any way. The nature of the project requires that you answer the following questions as honestly as possible so please provide as accurate answers as you can.

Half of the participants were also told the five constructs the personality test was created to measure. A definition of each construct was also provided (see the protocol in Appendix E for specific instructions).

Before taking the personality test within the applicant condition, all participants were given the following instructions:

When answering the questions on the personality test imagine that you are a job applicant. Please answer as you would if you were really taking the test to get a job. Keep in mind that your answers will be kept completely anonymous.

After these instructions were given (for the applicant condition), half of the participants were given the following instructions:

To make this situation more like an applicant situation, we are offering an incentive. Those of you who score in the top 15% on this test will receive $20.

Also prior to taking the test in either condition (depending on which administration was given first, and after the general instructions (honest or applicant) were given), half of the participants were told that a lie scale was included in the measure. These individuals were told the following:

Sometimes people try to make themselves out to be better than they really are. Therefore, this test includes a scale to check on this. Such scales are used to identify individuals who give inaccurate or false responses. Individuals who have a high score on this scale (i.e., those individuals that the lie scale identifies as faking responses) will be eliminated from the selection process (and from consideration for the incentive if they are in that condition). Please keep this fact in mind as you take this selection test.

Therefore, it was made clear to those that were in the incentive and warning condition that those who were caught faking would not be eligible for the money.

After the necessary instructions were given, the personality test was administered. After taking the test under the applicant condition, participants were given the Post-Questionnaire that contained the items for the final four measures (knowledge of construct being measured, intention to fake, valence toward doing well on the test, and concern with being identified as a faker). This was followed by the administration of the demographic form. After the experiment was complete, participants were given a debriefing form (see Appendix F for a reproduction of this form) and were debriefed.

<center>Results</center>

<u>Descriptive Analyses</u>

The equivalence of the eight between-subject conditions was examined to ensure that differences between these groups were a result of the manipulations. ANOVAs and chi-squares were conducted to ensure that the eight groups were equivalent in terms of age, race, sex, GPA, past experience with personality tests, and all pre-questionnaire scales (e.g., pre-intentions to fake). When controlling for the number of analyses that were conducted (e.g., controlling for Type I error with a Bonferroni correction), no significant differences were found, indicating that the eight groups did not differ on these variables.

Table 3 presents the means and standard deviations for each of the measures across the two conditions and the correlations among all the measures. There were moderate intercorrelations between the different personality scales. However, the intercorrelations of the personality scales in the honest condition are similar to normative information provided by the test publishers (Costa & McCrea, 1989).

<center>59</center>

Table 3. Correlations Among Demographic Variables and Measures.

	Mean	S.D.	1	2	3	4	5	6	7	8	9	10	11	12	13	14
DEMOGRAPHICS																
1. Age	19.80	2.96														
2. GPA	3.21	.47	-.16													
3. Experience with Personality Tests	2.24	.80	.12	-.01												
PRE-QUESTIONNAIRE																
4. Attitude Toward Faking	2.18	.68	-.05	.02	.00	.86										
5. Subjective Norms	2.30	.80	.05	-.03	.03	.58	.73									
6. Perceived Behavioral Control	2.84	.93	-.03	-.03	.00	.55	.48	.79								
7. Pre-Intentions to Fake	2.71	.73	-.02	.04	.01	.72	.57	.54	.89							
HONEST CONDITION																
8. Neuroticism	20.16	8.39	-.06	-.03	-.05	.13	.08	.02	.17	.88						
9. Extroversion	32.56	6.77	-.09	.00	.01	-.07	-.10	-.05	-.11	-.38	.84					
10. Openness	30.24	6.53	.05	.03	.10	.04	.08	.04	.06	-.20	.15	.76				
11. Agreeableness	32.07	6.11	.00	.02	.09	-.25	-.19	-.23	-.30	-.33	.34	.09	.77			
12. Conscientiousness	31.75	7.08	.07	.15	.04	-.25	-.22	-.17	-.27	-.41	.30	.00	.30	.86		
13. Self-deception	1.71	2.27	-.02	-.01	.06	-.11	-.08	-.01	-.15	-.36	.24	.21	.06	.26	.74	
14. Impression Management	1.50	2.19	-.03	.02	.12	-.25	-.25	-.23	-.31	-.29	.16	.13	.31	.27	.56	.76

Table 3. Correlations Among Demographic Variables and Measures (cont'd).

	Mean	S.D.	1	2	3	4	5	6	7	8	9	10	11	12	13	14
APPLICANT CONDITION																
15. Neuroticism	15.04	7.66	-.05	.01	.03	.06	.02	-.03	.11	.60	-.28	-.27	-.22	-.23	-.33	-.24
16. Extroversion	34.73	6.56	-.04	.01	.00	.01	-.01	.02	-.02	-.25	.72	.20	.24	.21	.21	.11
17. Openness	30.41	6.10	.04	.01	.11	.04	.05	.04	.04	-.18	.18	.79	.09	.00	.20	.15
18. Agreeableness	34.74	5.92	.00	.03	.02	-.11	-.08	-.07	-.18	-.25	.27	.15	.70	.22	.13	.23
19. Conscientiousness	36.75	6.75	.00	.10	-.09	-.01	.00	.00	-.05	-.21	.20	.12	.15	.46	.17	.14
20. Self-deception	2.95	3.66	-.03	-.02	-.00	-.04	-.03	.05	-.11	-.25	.19	.24	.07	.20	.61	.37
21. Impression Management	3.02	4.10	-.01	-.01	-.01	-.12	-.08	-.10	-.16	-.20	.12	.18	.20	.27	.37	.46
DIFFERENCE SCORES																
22. Neuroticism	-5.01	7.20	.01	.05	.08	-.08	-.08	-.05	-.09	-.52	.13	-.06	.15	.23	.07	.08
23. Extroversion	2.22	4.94	.07	.00	-.01	.09	.12	.08	.11	.19	-.43	.05	-.15	-.14	-.06	-.08
24. Openness	.22	4.12	-.03	-.02	.02	-.02	-.06	-.01	-.03	.06	.02	-.44	-.02	.01	-.05	-.02
25. Agreeableness	2.71	4.64	-.01	.00	-.08	.16	.13	.19	.14	.12	-.11	.05	-.45	-.13	.09	-.11
26. Conscientiousness	5.05	7.17	-.07	-.06	-.12	.22	.19	.15	.20	.22	-.12	.11	-.16	-.57	-.10	-.13
27. Self-deception	1.20	2.88	-.03	.00	-.05	.03	.03	.08	-.01	-.03	.05	.13	.05	.05	-.02	.02
28. Impression Management	1.50	3.62	.00	-.02	-.09	.00	.06	.02	.00	-.05	.03	.11	.04	.15	.08	-.08

61

... Management Measures (cont'd)

Table 3. Correlations Among Demographic Variables and Measures (cont'd).

	15	16	17	18	19	20	21	22	23	24	25	26	27	28
APPLICANT CONDITION														
15. Neuroticism	.88													
16. Extroversion	-.46	.84												
17. Openness	-.27	.32	.72											
18. Agreeableness	-.45	.47	.26	.78										
19. Conscientiousness	-.57	.53	.24	.52	.88									
20. Self-deception	-.55	.44	.29	.36	.51	.86								
21. Impression Management	-.51	.37	.22	.48	.55	.71	.90							
DIFFERENCE SCORES														
22. Neuroticism	.37	-.24	-.11	-.22	-.40	-.29	-.32	.61						
23. Extroversion	-.25	.32	.13	.21	.39	.33	.32	-.49	.42					
24. Openness	.00	.10	.20	.09	.12	.05	.03	-.06	.10	.21				
25. Agreeableness	-.31	.24	.16	.33	.41	.36	.33	-.47	.46	.14	.24			
26. Conscientiousness	-.33	.26	.19	.24	.47	.29	.23	-.60	.51	.10	.50	.75		
27. Self-deception	-.43	.40	.21	.36	.52	.78	.60	-.42	.46	.10	.39	.44	.31	
28. Impression Management	-.42	.35	.15	.39	.53	.58	.85	-.41	.40	.04	.43	.34	.67	.79

Table 3. Correlations Among Demographic Variables and Measures (cont'd).

POST QUESTIONNAIRE

	Mean	S.D.	1	2	3	4	5	6	7	8	9	10	11	12	13
29. Post-Intentions to Fake	2.53	.92	-.06	.03	**-.10**	**.24**	**.17**	**.22**	**.31**	**.23**	-.07	**.09**	**-.22**	**-.17**	-.07
30. Valence Toward Doing Well on Test	3.50	.81	-.03	.02	-.03	.02	.01	.06	.01	**-.09**	**.11**	.08	**.11**	**.12**	**.09**
31. Knowledge of Constructs Assessed	4.02	.46	-.03	.03	**.14**	**-.10**	**-.09**	-.06	**-.12**	**-.16**	**.20**	**.20**	**.20**	**.21**	**.29**
32. Concern With Being Detected By Lie Scale	2.95	.60	-.02	.00	-.03	.01	-.01	.04	.07	**.25**	**-.10**	**-.14**	**-.11**	**-.15**	**-.10**

POST QUESTIONNAIRE

	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32
29. Post-Intentions to Fake	**-.21**	**-.27**	**.23**	**.10**	**.12**	**.36**	**.23**	**.21**	**-.54**	**.38**	.00	**.43**	**.51**	**.33**	**.36**	**.91**			
30. Valence Toward Doing Well on Test	.02	**-.23**	**.21**	**.09**	**.22**	**.25**	**.20**	**.22**	**-.14**	**.13**	.01	**.12**	**.10**	**.17**	**.22**	**.29**	**.90**		
31. Knowledge of Constructs Assessed	**.29**	**-.28**	**.26**	**.20**	**.25**	**.26**	**.31**	**.34**	**-.11**	.06	-.03	.05	.04	**.16**	**.21**	.04	**.21**	**.80**	
32. Concern With Being Detected By Lie Scale	**-.18**	**.26**	**-.16**	**-.17**	**-.16**	**-.21**	**-.16**	**-.23**	-.01	-.07	-.02	-.07	-.04	**-.12**	**-.14**	.04	**.19**	-.05	.77

Note. Correlations in bold have p ≤ .05.

<u>Manipulation Checks</u>

The post-questionnaire administered after the participants took the personality test under the applicant condition contained items to measure valence toward doing well on *the test*, concern with being detected by a lie scale, and knowledge of what the test was measuring. These scales were included to serve as manipulation checks. To determine if the manipulations were effective, the differences across the two groups (e.g., those who were warned of the lie scale versus those who were not) on each of these measures was assessed. Three independent samples t-tests were conducted. In the first t-test, whether or not the participant was offered the $20 incentive served as the independent variable (those not offered the incentive; "0;" versus those offered the incentive, "1") and valence toward doing well on the test served as the dependent variable. Significant differences between these two groups was found, with those who were offered the incentive having significantly greater valence toward doing well (t (542) = 4.99, p < .001). Therefore, the valence manipulation was successful.

Second, the lie scale manipulation was examined. Whether or not the participant was told a lie scale was included on the test served as the independent variable and concern with being identified as faking by a lie scale was the dependent variable. Again, results indicate that those who were warned about the lie scale being included on the test were significantly more concerned that they would be identified as faking (t (542) = 9.15, p < .001). Thus, this manipulation was also successful.

Finally, the knowledge manipulation was examined by assessing if those who were provided information about what constructs the test measured felt they had greater knowledge of what the items on the test were measuring. Results indicate that this

manipulation was not successful. There was no significant difference in knowledge between those who were given the information versus those who were not (t (542) = .52, N.S.).

As will be discussed shortly, there were significant mean differences across the eight manipulations. In fact, those who were given knowledge of what constructs the test assessed tended to have the highest scores on the personality scales in the applicant condition. Therefore, exploratory analyses were conducted to determine why the knowledge manipulation may have failed. First, participants' experience with personality tests was examined and controlled for in a factorial ANOVA. Although past experience with personality tests did predict scores on the knowledge scale, the manipulation did not become significant when experience was controlled for. Second, the extent to which knowledge varied across the other manipulations was examined. It is possible that knowledge did not work because it interacted in some way with the other manipulations. An ANOVA was conducted so that knowledge scale scores across the eight between subject conditions could be examined. No differences in knowledge of the constructs were found across these eight conditions.

As can be seen in Appendix B, eight knowledge items composed the knowledge scale that was used to assess the knowledge manipulation. Five of the items ask respondents whether the test they just took measured specific constructs, while the other three asked whether it was clear what the test was measuring in general (e.g., "it was usually clear to me what personality trait an item was measuring). Therefore, it was thought that these two different types of items may have yielded different results. For instance, we should certainly expect differences in knowledge of the specific constructs

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being measured between those given knowledge of the constructs and those not given this information. However, we may not find this difference in the perception of the general transparency of the measure. Therefore, two different knowledge scales were created, with the five specific items making up the first scale and the three general knowledge items composing the other scale. The mean differences on these two scales for those who were given knowledge of what the test measures versus those who were not, was tested with an independent samples t-test. Neither test was significant. Therefore, even when one simply considers specific knowledge of the constructs measured there are no differences between those given knowledge and those who were not. Overall, it seems that the knowledge manipulation was not successful in manipulating knowledge. However, the knowledge manipulation did alter something as those given knowledge of the constructs measured did score significantly different from the other groups. Perhaps the manipulation check is not capturing the differences between these two groups because something different was altered. That is, instead of manipulating knowledge, something else was manipulated. Another explanation is that the manipulation did work, but the manipulation check was flawed (i.e., did not adequately measure knowledge of what the test was measuring). Unfortunately, there is no way to determine if the manipulation was flawed or if the manipulation check was inadequate.

Table 4. Analysis of Variance Across the Eight Experimental Conditions

Scale	$\underline{F}$	$\underline{n}^2$
HONEST CONDITION		
Neuroticism	1.68	N.S.
Extroversion	1.33	N.S.
Openness	3.05*	.04
Agreeableness	1.05	N.S.
Conscientiousness	3.31*	.04
Self-Deception	1.15	N.S.
Impression Management	1.07	N.S.
APPLICANT CONDITION		
Neuroticism	7.61*	.09
Extroversion	4.82*	.06
Openness	3.41*	.04
Agreeableness	4.05*	.05
Conscientiousness	13.54*	.15
Self-Deception	6.52*	.08
Impression Management	4.55*	.06

Table 4. Analysis of Variance Across the Eight Experimental Conditions (cont'd)

Scale	$\underline{F}$	$\underline{\eta}^2$
DIFFERENCE SCORES		
Neuroticism	9.88*	.11
Extroversion	3.72*	.05
Openness	2.33*	.03
Agreeableness	3.44*	.04
Conscientiousness	9.12*	.11
Self-Deception	8.12*	.10
Impression Management	6.21*	.08
POST-QUESTIONNAIRE		
Post Intentions	7.34*	.09
Valence	4.14*	.05
Knowledge	2.33*	.03
Concern with Lie Scale	12.95*	.14

*$\underline{p} \leq .05$

Note. The df = 7 for all analyses.

Table 5. Means and Standard Deviations Across the Eight Between-Subject Conditions.

Scale	Control Group		Knowledge Only		Warning Only		Incentive Only		Knowledge and Warning		Knowledge and Incentive		Warning and Incentive		Knowledge Warning and Incentive	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
PRE-QUES																
Attitude Toward																
Faking	2.12	.69	2.18	.72	2.33	.69	2.19	.67	2.08	.61	2.27	.65	2.25	.65	2.08	.74
Subjective Norms	2.23	.83	2.27	.76	2.30	.78	2.31	.82	2.13	.72	2.39	.73	2.40	.94	2.38	.85
PBC	2.73	.87	2.90	.98	2.96	.95	2.96	.93	2.71	.88	2.79	.85	2.81	1.01	2.84	.94
Pre-Intentions	2.67	.76	2.73	.73	2.83	.73	2.75	.79	2.55	.67	2.76	.62	2.74	.76	2.70	.73
HONEST																
Neuroticism	21.64	8.84	18.76	8.31	21.15	7.54	21.59	8.80	18.77	7.20	18.40	9.99	20.48	8.46	20.10	7.94
Extroversion	32.84	5.97	32.31	7.33	30.92	6.80	33.05	5.78	32.58	6.96	33.85	6.84	31.63	7.47	33.34	6.79
Openness	31.26	6.29	31.03	6.16	29.42	6.84	29.90	6.21	27.96	7.21	32.36	5.69	31.23	5.57	29.57	1.01
Agreeableness	31.33	6.35	32.22	6.10	31.05	6.79	32.78	4.77	32.62	5.58	33.29	6.19	32.11	5.32	31.62	7.05
Conscientious-ness	30.40	8.36	33.29	6.72	30.72	6.88	31.09	7.09	32.06	6.32	34.67	6.89	29.73	6.89	32.16	6.54
Self-Deception	1.60	1.85	1.80	2.81	1.54	1.92	1.71	2.43	1.34	1.82	2.29	2.85	1.48	1.71	2.00	2.56
Impression Management	1.45	2.09	1.48	2.18	1.46	2.10	1.51	2.37	1.10	1.59	1.51	2.25	1.43	2.01	2.05	2.75
APPLICANT																
Neuroticism	13.14	6.54	13.00	6.63	17.79	6.64	12.53	7.97	16.30	6.41	12.00	8.11	17.30	8.20	17.44	8.41
Extroversion	35.48	5.18	36.04	7.32	32.82	5.80	36.95	6.17	33.77	5.60	36.76	6.83	32.43	6.75	34.43	6.81
Openness	31.51	5.32	31.29	5.52	29.67	6.23	31.82	4.77	28.36	5.87	32.05	6.64	30.36	6.15	29.43	6.35
Agreeableness	34.95	5.12	36.19	5.45	33.42	6.01	36.48	4.59	34.31	5.16	36.58	6.36	33.73	5.66	33.22	6.58
Conscientious-ness	39.00	6.32	39.29	5.58	34.06	5.81	39.95	6.55	34.81	5.27	39.38	6.07	33.03	6.99	35.30	6.49
Self-Deception	3.48	3.82	3.50	4.12	1.99	2.19	4.34	4.71	1.81	2.35	4.54	4.78	1.88	2.33	2.25	3.16
Impression Management	3.81	4.54	3.50	4.49	2.00	2.55	4.19	5.24	1.77	2.89	4.33	4.91	2.16	3.20	2.40	3.43

69

Table 5. Means and Standard Deviations Across the Eight Between-Subject Conditions (Continued).

Scale	Control Group		Knowledge Only		Warning Only		Incentive Only		Knowledge and Warning		Knowledge and Incentive		Warning and Incentive		Knowledge Warning and Incentive	
DIFFERENCE SCORES																
Neuroticism	-8.51	8.87	-5.29	6.09	-3.36	5.15	-8.97	8.78	-2.59	5.66	-6.40	8.53	-3.18	4.78	-2.66	5.72
Extroversion	2.64	5.26	3.69	5.53	1.88	3.60	3.77	5.19	1.33	4.42	2.91	5.99	.80	2.92	1.09	5.22
Openness	.25	4.26	.01	4.44	.23	3.57	1.87	4.77	.54	2.51	-.31	5.20	-.88	3.31	-.14	4.24
Agreeableness	3.62	5.08	3.93	4.89	2.38	4.31	3.84	4.47	1.68	2.86	3.29	5.28	1.63	3.79	1.59	5.29
Conscientious-ness	8.60	8.71	6.09	6.82	3.38	5.01	8.93	7.94	2.70	4.76	4.71	7.77	3.36	5.97	3.14	6.84
POST-QUES																
Valence	3.36	.84	3.25	.87	3.33	.71	3.75	.88	3.40	.76	3.73	.78	3.53	.71	3.69	.76
Knowledge	4.02	.45	4.14	.38	3.95	.43	4.12	.40	3.93	.48	4.12	.56	3.97	.43	3.98	.48
Concern	2.75	.56	2.61	.47	3.13	.51	2.81	.64	3.14	.54	2.74	.66	3.13	.58	3.24	.52
Post-Intentions	2.84	1.02	2.63	.86	2.25	.73	3.00	1.04	2.27	.79	2.75	1.00	2.35	.75	2.32	.88

70

Table 6. Paired Samples t-tests Comparing Means Across the Honest and Applicant Condition

Measure	HONEST CONDITION Mean	CONDITION S.D.	APPLICANT Mean	CONDITION SD	t-value	p	d
Neuroticism	20.11	8.37	15.09	7.69	16.24	.001	.63
Extroversion	32.56	6.78	34.79	6.46	-10.47	.001	.34
Openness	30.27	6.53	30.48	5.98	-1.23	N.S.	NA
Agreeableness	32.05	6.10	34.77	5.77	-13.61	.001	.46
Conscientiousness	31.72	7.11	36.77	6.62	-16.42	.001	.74
Self-Deception	1.71	2.28	2.92	3.64	-9.74	.001	.41
Impression Management	1.49	2.17	2.99	4.07	-9.64	.001	.48

Note. For all analyses n = 543. The personality measures were all assessed with a five point likert scale with high values indicating higher scores on that trait. Self-Deception and Impression Management were both measured on a 20 point scale. Effect sizes were calculated by subtracting the mean in the honest condition from the mean in the applicant condition and dividing by the pooled standard deviation.

71

Table 7. Regression Analyses Regressing Pre- and Post-Intentions on Attitudes, Subjective Norms,
and Perceived Behavioral Control.

Model	b	β	$\underline{R}$	$\underline{R^2}$	$\underline{F}$
<u>Pre-Intentions</u>					
Attitude Toward Faking	.55*	.52	.76	.57	215.88*
Subjective Norms Toward Faking	.17*	.19			
Perceived Behavioral Control Toward Faking	.13*	.17			
<u>Post-Intentions</u>					
Attitude Toward Faking	.22*	.17	.26	.07	11.99*
Subjective Norms Toward Faking	.02	.02			
Perceived Behavioral Control Toward Faking	.12*	.12			

* $p < .05$

72

Table 8. Post Intentions Regressed onto Attitude Toward Faking, Subjective Norms,

Perceived Behavioral Control, Warning of a lie scale, and Valence.

Model	b at Entry	β at Entry	R^2	F	ΔR^2	$\Delta R^2 F$
STEP 1			.140	15.27*		
Attitude Toward Faking	.23*	.17				
Subjective Norm	.02	.02				
Percieved Behavioral Control	.12*	.12				
Valence	.09	.28				
Warning of A Lie Scale	-.47*	-.26				
STEP 2			.145	7.11*	.005	.414
Attitude*Valence	.02	.03				
Subjective Norm*Valence	.06	-.08				
PBC*Valence	.03	.05				
Attiude*Warning	.01	.01				
Subjective Norm*Warning	.07	.09				
PBC*Warning	-.15	-.25				

ANOVAs were also conducted to determine if there were mean differences across

the eight conditions for each of the personality measures, the difference scores, and the

post-questionnaire measures. Results are presented in Table 4 . As this Table shows, the

manipulations had little effect on honest responses. However, there were significant

differences across the manipulations for all the personality scales in the applicant

condition, for the difference scores, and for all post-questionnaire measures. The effect

sizes associated with these differences are also presented in Table 4. The effects were

small to moderate. These results provide support that when asked to respond honestly,

the respondents did follow these instructions. That is, if participants were really

responding honestly, then it should not matter what condition they were in (there should

be no mean differences across the eight groups when responding honesty). We found

that the manipulations only affected two of the seven scales (openness and

contentiousness) within the honest condition, and these effects were small (effect sizes

were .04) and smaller than effects found in the applicant condition. However, within the

applicant condition, the manipulations had effects on all scores and these effects were

typically stronger. The means and standard deviations across the eight conditions for

both the honest and applicant condition are presented in Table 5.

Past research has found that applicants score about a half a standard deviation

greater than participants who respond to personality measures honestly (Hough et al.,

1990). Therefore, to determine if the applicant condition was an effective manipulation

(i.e., really made participants behave like applicants), paired samples t-tests were

conducted for each of the personality scales comparing the honest responses to the

applicant responses. The results of this analysis are presented in Table 6. There were

significant differences between the honest and applicant personality test scores for all but the Openness scale. For the remaining six scales (the four other personality scales and the two social desirability scales), participants scored significantly higher in the applicant condition. Additionally, the effect sizes in the last column on the right of Table 6 show that participants scored, on average, a half of a standard deviation greater than in the honest condition. Therefore, the mean differences between the honest and applicant responses that were observed in this study mimic those found using real applicants. This lends support that the applicant manipulation was effective.

It should be pointed out that the self-deception mean increased significantly in the applicant condition. This is surprising because Paulhus (1986) suggests that self-deception is a measure of how a person sees oneself and therefore should not change across experimental manipulations. However, as Table 6 shows, self-deception increased in the applicant condition as much as impression management did (which is a measure of conscious distortion). Therefore, these results cast doubt on the construct validity of this social desirability measure.

Preliminary Analyses

In the applicant condition, participants were told to imagine that they were taking the personality test as an applicant applying for a job they really wanted. Therefore, they were not given any information about a specific job for which they were applying. However, it is possible that some participants had a specific job in mind while they took the personality test in the applicant condition and this may have influenced results. To test this, on the demographic form, participants were asked to indicate if they were thinking of any particular job as they took the personality test in the applicant condition.

If they indicated they were thinking of a job, they were asked to indicate which job they were imagining applying for. Results indicated that 135 of the participants were thinking of a specific job when they took the personality test as an applicant. However, the type of job they thought of varied widely. Salesperson was the job most frequently mentioned ($\underline{n} = 25$). The next most frequent was a "business" job ($\underline{n} = 18$; no mention was made about what kind of business). This was followed by people indicating they imagined they were a manager ($\underline{n} = 13$). The rest of the jobs imagined were indicated by less than 5 participants. Clearly, it would be very difficult to conduct analyses within these job categories considering the small sample sizes. Therefore, several independent samples t-tests were conducted to determine if those who thought of a specific job scored differently on the personality measures (or on any of the post-questionnaire measure) than those who did not. After correcting for Type I error using a Bonferroni correction, no significant differences were found between those who were thinking of a specific job and those who did not on any of the measures.

Some studies have found that order effects exist in faking research such that those who are asked to take the test honestly before the applicant or fake administration will be able to increase their scores more through faking (Klein & Owens, 1965). To test for such order effects, 18 independent samples t-tests were performed (one for each of the personality and social desirability scales across both the honest and applicant conditions, and for each of the four post-questionnaire measures) with a Bonferroni correction. The scale score for the test in question served as the dependent variable and the order of administration served as the independent variable. Results indicate that only two scales, conscientiousness from the applicant condition and valence from the post-questionnaire,

had order effects. The effects were such that those who were given the tests under the honest condition first had higher conscientiousness scores and greater valence for doing well on the test. This is consistent with past research. Further analyses demonstrate that the effect size for order was small (less than a quarter of a standard deviation). However, to be conservative, tests that include these two scales will be conducted both with and without order controlled. The effect of controlling for order will only be reported when it significantly alters results.

<u>Hypothesis Tests</u>

Hypothesis 1 predicted that attitude toward faking would be positively correlated with the intention to fake such that those who had more positive attitudes toward faking would have a greater intention to fake. As Table 3 shows, the correlation between attitude toward faking and the pre-intention to fake (the measure administered via Internet one week prior to the experiment) was .72 ($p < .001$), while the correlation with the post-intention measure (the one administered after the test) was .24 ($p < .001$). Therefore, support was found for Hypothesis 1.

Hypothesis 2 was also supported, as it was found that those who had more positive subjective norms toward faking were more likely to intend to fake. As Table 3 shows, the correlation between subjective norms and the pre-intention to fake is .57 ($p < .001$), and .17 ($p < .001$) with post-intentions. Thus, again we see that the correlation is stronger with pre-intentions.

Support was also found for Hypothesis 3, which predicted that those who have greater perceived behavioral control over faking would also be more likely to intend to fake. Again, as Table 3 shows, the correlation between pre-intentions to fake and post-intentions to fake was .54 ($p < .001$) and .22 ($p < .001$) respectively.

It is not surprising that attitude toward faking, subjective norms, and perceived behavioral control correlated more highly with the pre-intention measure. It has been suggested that the closer the attitude is measured to the intention measure, the stronger the relationship will be (Fishbein & Jaccard, 1973; Ajzen & Madden, 1986). It may not simply be the passage of time itself that attenuates the relationship between intentions and these variables, but exposure to new information or other events that may alter attitudes, subjective norms, perceived behavioral control, or intentions (Ajzen, 1991; Marcey & Brown, 1983). In fact, two manipulations were used to alter the relationship between intentions to fake and these three variables (valence and warning of lie scale). It should also be noted that the high correlation between these three variables and pre-intentions to fake is most likely not a result of method bias, as these three variables do not correlate as strongly with each other as they do with pre-intentions.

However, it should also be pointed out that the pre- and post-intention measures also differ in referent. That is, the pre-intention measure refers to tests in general, whereas the post-intention measure asks about faking on a specific test. While the pre-intention measure asked participants if they intended to fake on a selection test some time in the future, the post-intention measure asked if participants intended to fake the personality test they just took. This might explain some of the differences between results for the two measures. The correlations with the post-intention measures may be

weaker, not only because it was measured at a different time from the independent variables (i.e., attitudes, subjective norms, and perceived behavioral control), but also because it is of a more specific nature.

To gain a deeper understanding of the way in which attitudes toward faking, subjective norms, and perceived behavioral control predict the intention to fake, these variables were examined using regression analyses. This type of analysis can allow for a deeper understanding of the relationships between these variables and intentions to fake. One regression analysis was run with pre-intentions as the dependent variable and another with post-intentions to fake as the dependent variable. Attitudes toward faking, subjective norms toward faking, and perceived behavioral control over faking served as the independent variables in both analyses. Results of these analyses are presented in Table 7. As the table shows, all variables explained a significant amount of the variance in pre-intentions beyond that explained by the other two variables. Taken together, attitudes, subjective norms, and perceive behavioral control explained 57% of the variance in pre-intentions to fake. When post-intentions serve as the dependent variable the explained variance is only 7%. Additionally, subjective norms does not significantly predict post-intentions to fake beyond that explained by attitudes.

The results of the regression analyses obtained when pre-intentions was the dependent variable are very similar to estimates that have been found in other studies testing the theory of planned behavior (see Ajzen, 1991 for a review). For example, on average, research has found that attitudes, subjective norms, and perceived behavioral control explain 55% of the variance in intentions to fake. However, when post-intentions were used as the dependent variable, the relationships decrease substantially and are

much lower than has been found with other tests of the theory. Again, this is likely because post-intentions were measured well after attitudes, subjective norms, and perceived behavioral control were assessed. In other tests of the theory of planned behavior, these variables are assessed at the same time as intentions. Therefore, it is expected that relationships observed with the pre-intention measure would be more consistent with past research.

Hypothesis 4 predicted that the intention to fake would be positively correlated with faking behavior. This hypothesis was tested in two ways. First, both pre and post-intentions to fake were correlated with the difference scores for the five personality scales. Keep in mind that the difference scores serve as an operationalization of faking because any change in scores from the honest to the applicant condition may be attributable to faking. It was found that both pre- and post-intentions were significantly correlated with the difference score for four of the five scales (see Table 3). Neither of the intention measures was significantly related to the openness difference score (which is not unexpected given the extremely low alpha for this difference score). Therefore, we find that the intention to fake does explain a significant amount of variance in the difference scores. However, the pre-intention measure explains only one to four percent of the variance in the difference scores, while post-intentions explain fourteen to twenty-six percent. Again, this may be explained by the closer proximity of the post-intention measure with the administration of the tests.

Hypothesis 4 was also tested by correlating pre- and post-intentions with social desirability scores in the applicant condition. The results, which are presented in Table 3, indicate that both social desirability scale scores (self-deception and impression management scores from the applicant condition) significantly related to pre and post-intentions to fake the test. However, unexpectedly, pre-intentions to fake correlate negatively with both social desirability scales, while post-intentions correlate positively. This means that those who had high pre-intentions to fake a selection test got lower scores on the social desirability scale in the applicant condition, but those who indicated that they did try to fake the test received high social desirability scores. Another interesting finding is that both self-deception and impression management correlated similarly with both intention measures. Paulhus (1986) suggests that self-deception assesses unconscious distortion, while impression management measures intentional faking. Therefore, one would expect to find no relationship between self-deception and intention to fake, because self-deception is unconscious and unintentional. Again, these results do not support the construct validity of the two components of Paulhus' (1986) social desirability scale.

As shown in Table 9, support was not found for Hypotheses 5a, 5b, and 5c. It was expected that valance toward performing well on the test would moderate the relationship between the three main effects on the intention to fake, such that those who had high valence toward performing well (those offered the $20 incentive) would be more likely to intend to fake, regardless of their attitudes, subjective norms, or perceived behavioral control over faking. First, a moderated regression was conducted with post-intentions serving as the dependent variable and attitude toward faking, the dichotomous

measure of valence (0 versus 1), and their interaction as the independent variables. Keep in mind that post-intentions had to serve as the dependent variable (as opposed to pre-intentions) because the pre-intention measure was administered *before* the manipulations. Results indicate that only the main effect of attitude was significant. Thus, valence toward performing well on the test did not significantly predict the intention to fake beyond that predicted by attitude toward faking. Second, the interaction between subjective norms and valence toward doing well on the test was examined in a similar manner. Once again, only the main effect for subjective norms was significant. Finally, the interaction between perceived behavioral control and valence was examined and again only the main effect of perceived behavioral control was significant.

Next, Hypotheses 6a, 6b, and 6c were examined. These hypotheses predicted an interaction between the three main effects (attitudes, subjective norms, and perceived behavioral control) and concern with being detected as a liar by the lie scale on the intention to fake. Three different moderated regressions were conducted with post-intentions to fake serving as the dependent variable and the main effect of interest, whether or not the participant was told about the lie scale being included on the measure, and their interaction as the independent variables. As Table 10 shows, none of the three interactions were significant. However, for all three analyses, the two main effects were significant. Results were such that those with more positive attitudes toward faking, positive subjective norms toward faking, and high perceived behavioral control, were more likely to intend to fake. However, those who were warned about the lie scale being included on the test were less likely to fake.

The same analyses described above with valence toward doing well on the test and concern with being identified as lying by a lie scale, were run again with the manipulation checks (the scales which measured valence and concern on the post-questionnaire) in place of the dichotomous variable for valence and concern (0 or 1). Although the results were slightly larger (more variance was explained) when these continuous scales were used as independent variables, these analyses did not result in significant interactions.

A regression was also run with post-intentions regressed onto attitude toward faking, subjective norms, perceived behavioral control, warning of a lie scale, and valence. Table ?? shows the results of this analysis. As can be seen, warning was the strongest predictor of post-intentions to fake. Those who were warned that a lie scale was included on the test were much less likely to have tried to fake the test. Attitude toward faking, and perceived behavioral control were also predictors of post-intentions. Additionally, subjective norm did not predict post-intentions. None of the interactions were significant.

It should be noted that a paired samples t-test was conducting comparing pre-intentions to post-intentions. It was found that pre-intentions to fake were significantly higher than post-intentions. This is not surprising given that those who were warned that there was a lie scale included on the test were much less likely to intend to fake. Thus, the warning contributed to lowering post-intentions to fake.

Table 9. Regression Analyses Regressing Post-Intentions on Attitudes, Subjective
Norms, Perceived Behavioral Control, and Valence.

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
Attitude and Valence						
STEP 1			.063	15.73*		
Attitude Toward Faking	.33*	.25				
Valence	.07	.04				
STEP 2			.063	10.50*	.000	.11
Attitude * Valence	.04	.05				
Subjective Norms and Valence						
STEP 1			.032	7.75*		
Subjective Norms Toward Faking	.20*	.17				
Valence	.05	.03				
STEP 2			.032	5.18*	.000	.08
Subjective Norm * Valence	.03	-.04				
Perceived Behavioral Control and Valence						
STEP 1			.051	12.64*		
Perceived Behavioral Control	.22*	.22				
Valence	.08	.04				
STEP 2			.051	8.46*	.000	.128
Perceived Behavioral Control * Valence	.03	.05				

* $p < .05$

Note. The "Valence" variable was coded 0/1 ("0" being no incentive was offered; "1"

being a $20 incentive was offered to those who scored in the top 15% on the test).

Table 10. Regression Analyses with Post Intentions Regressed on Attitudes, Subjective Norms, Perceived Behavioral Control, and Warning of a Lie Scale.

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
Attitude and Warning						
STEP 1			.126	34.02*		
Attitude Toward Faking	.33*	.24				
Warning	-.47*	-.26				
STEP 2			.126	22.68*	.000	.12
Attitude * Warning	-.04	-.05				
Subjective Norms and Valence						
STEP 1			.099	25.93*		
Subjective Norms Toward Faking	.20*	.18				
Warning	-.48*	-.26				
STEP 2			.099	17.26*	.000	.04
Subjective Norm * Warning	-.02	-.03				
Perceived Behavioral Control and Valence						
STEP 1			.116	30.85*		
Perceived Behavioral Control	.22*	.22				
Warning	-.48*	-.26				
STEP 2			.118	21.09*	.003	1.50
Perceived Behavioral Control * Warning	-.11	-.18				

* $p < .05$

Note. The "Warning" variable was coded 0/1 ("0" indicates the participant was not warned that a lie scale was included on the test; "1" indicates the participant was told a lie scale was included on the measure).

Table 11. Regression Analyses Regressing Pre-Intentions and Knowledge on Faking

Behavior

Model	B	β	R^2	F	ΔR^2	$\Delta R^2 F$
Impression Management						
STEP 1			.025	6.14*		
Pre-Intentions	-.89*	-.16				
Knowledge	-.30	-.04				
STEP 2			.029	4.58*	.003	1.47
Pre-Intentions*Knowledge	.63	.22				
Self-Deception						
STEP 1			.010	2.31		
Pre-Intentions	-.50*	-.09				
Knowledge	.04	-.00				
STEP 2			.010	1.54	.000	.003
Pre-Intentions*Knowledge	.02	.05				
Neuroticism Difference Score						
STEP 1			.029	6.94*		
Pre-Intentions	-.83	-.08				
Knowledge	2.08*	.14				
STEP 2			.029	4.63*	.000	.038
Pre-Intentions*Knowledge	.18	.03				
Extraversion Difference Score						
STEP 1			.013	3.11*		
Pre-Intentions	.74*	.11				
Knowledge	-.30	-.03				
STEP 2			.014	2.24	.001	.495
Pre-Intentions*Knowledge	.44	.13				
Openness Difference Score						
STEP 1			.005	1.17		
Pre-Intentions	-.17	-.03				
Knowledge	-.55	-.07				
STEP 2			.015	2.35*	.010	4.69*
Pre-Intentions*Knowledge	1.16*	.40				
Agreeableness Difference Score						
STEP 1			.023	5.64*		
Pre-Intentions	.94*	.15				
Knowledge	-.38	-.04				
Step 2			.023	3.75*	.000	.006
Pre-intentions*Knowledge	-.04	-.01				

Table 11. Regression Analyses Regressing Pre-Intentions and Knowledge on Faking

Behavior (cont'd).

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
Conscientiousness Difference Score						
STEP 1		.060	15.04*			
Pre-Intentions	1.96*	.20				
Knowledge	-1.92*	-.14				
STEP 2			.060	10.01*	.000	.010
Pre-Intentions*Knowledge	-.08	-.02				

p < .05

Note. The "Knowledge" variable was coded 0/1 ("0" indicates the participant was not given information about the constructs assessed by the test; "1" indicates the participant was given information about the constructs the test assesses). Impression management and self-deception scores are from the applicant condition.

Table 12. Regression Analyses Regressing Post-Intentions and Knowledge on Faking

Behavior

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
Impression Management Score from Applicant Condition						
STEP 1			.049	13.79*		
Post-Intentions	.97*	.22				
Knowledge	-.02	-.00				
STEP 2			.049	9.24*	.000	.17
Post-Intentions* Knowledge	-.15	-.05				
Self-Deception						
STEP 1			.051	14.17*		
Post-Intentions	.89*	.22				
Knowledge	.10	.01				
STEP 2			.051	9.56*	.000	.18
Post-Intentions*Knowledge	-.14	-.05				
Neuroticism Difference Score						
STEP 1			.303	116.49*		
Post-Intentions	-4.18*	-.53				
Knowledge	1.41*	.10				
STEP 2			.312	80.92*	.009	7.10*
Post-Intentions*Knowledge	1.50*	.29				
Extraversion Difference Score						
STEP 1			.143	44.63*		
Post-Intentions	2.03*	.38				
Knowledge	.12	.01				
STEP 2			.147	30.72*	.004	2.63
Post-Intentions*Knowledge	.70	.19				
Openness Difference Score						
STEP 1			.002	.439		
Post-Intentions	.02	-.01				
Knowledge	-.33	-.04				
STEP 2			.010	1.87	.009	4.72*
Post-Intentions*Knowledge	-.84*	-.28				
Agreeableness Difference Score						
STEP 1			.181	59.17		
Post-Intentions	2.14*	.424				
Knowledge	-.09	-.01				
STEP 2			.182	39.73*	.001	.881
Post-intentions*Knowledge	-.37	-.11				

<u>Table 12. Regression Analyses Regressing Post-Intentions and Knowledge on Faking</u>

<u>Behavior</u> (cont'd).

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
<u>Conscientiousness Difference</u>						
<u>Score</u>						
STEP 1			.270	98.97		
Post-Intentions	3.90*	.50				
Knowledge	-1.48*	-.10				
STEP 2			.277	68.31*	.007	5.36*
Post-Intentions*Knowledge	-1.33*	-.26				

p < .05

<u>Note.</u> The "Knowledge" variable was coded 0/1 ("0" indicates the participant was not given information about the constructs assessed by the test; "1" indicates the participant was given information about the constructs the test assesses). Impression management and self-deception scores are from the applicant condition.

89

Table 13. Regression Analyses Regressing Faking Behavior on Pre-Intentions to Fake and the Knowledge Scale

Model	$\underline{b}$	β	$\underline{R^2}$	F	ΔR^2	$\frac{\Delta R^2}{F}$
Impression Management						
STEP 1			.134	36.61		
Pre-Intentions	-.71*	-.12				
Knowledge	3.02*	.33				
STEP 2			.135	24.48*	.001	.322
Pre-Intentions* Knowledge	-.33	-.24				
Self-Deception						
STEP 1			.10	26.21*		
Pre-Intentions	.37	.22				
Knowledge	2.43*	.30				
STEP 2			.102	17.92*	.002	1.28
Pre-Intentions*Knowledge	-.60	-.50				
Neuroticism Difference Score						
STEP 1			.022	5.34*		
Pre-Intentions	-1.04*	-.10				
Knowledge	-1.89*	-.12				
STEP 2			.026	4.11*	.003	1.65
Pre-Intentions*Knowledge	-1.39	-.59				
Extraversion Difference Score						
STEP 1			.016	3.94*		
Pre-Intentions	.79*	.12				
Knowledge	.72	.07				
STEP 2			.017	2.69*	.000	.225
Pre-Intentions*Knowledge	.35	.22				
Openness Difference Score						
STEP 1			.002	.45		
Pre-Intentions	-.19	-.03				
Knowledge	-.30	-.03				
STEP 2			.004	.56	.002	.80
Pre-Intentions*Knowledge	-.57	-.42				
Agreeableness Difference Score						
STEP 1			.025	5.99*		
Pre-Intentions	.97*	.15				
Knowledge	.70	.07				
STEP 2			.026	4.12*	.001	.405
Pre-intentions*Knowledge	.45	.30				

Table 13. Regression Analyses Regressing Faking Behavior on Pre-Intentions to Fake and the Knowledge Scale (cont'd).

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
Conscientiousness Difference Score						
STEP 1			.046	11.44*		
Pre-Intentions	2.08*	.21				
Knowledge	1.09	.07				
STEP 2			.049	8.02*	.002	1.17
Pre-Intentions*Knowledge	1.14	.49				

* $p < .05$

Note. The "Knowledge" variable was the continuous scale score from the post-questionnaire. Impression management and self-deception scores are from the applicant condition.

Table 14. Regression Analyses Regressing Faking Behavior on Post-Intentions to Fake

and Knowledge

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
Impression Management						
STEP 1			.15	49.19*		
Post-Intentions	.88*	.20				
Knowledge	2.95*	.33				
STEP 2			.176	38.68*	.023	15.09*
Post-Intentions* Knowledge	1.25*	1.24				
Self-Deception						
STEP 1			.14	45.51*		
Post-Intentions	.85*	.21				
Knowledge	2.45*	.31				
STEP 2			.155	33.19*	.012	7.46*
Post-Intentions*Knowledge	.80*	.88				
Neuroticism Difference Score						
STEP 1			.301	115.73*		
Post-Intentions	-4.21*	-.54				
Knowledge	-1.33*	-.085				
STEP 2			.301	77.15*	.000	.294
Post-Intentions*Knowledge	-.28	-.16				
Extraversion Difference Score						
STEP 1			.145	45.80*		
Post-Intentions	2.02*	.38				
Knowledge	.43	.04				
STEP 2			.149	31.36*	.004	2.267
Post-Intentions*Knowledge	.59	.49				
Openness Difference Score						
STEP 1			.001	.296		
Post-Intentions	-.01	-.00				
Knowledge	-.29	-.03				
STEP 2			.004	.70	.003	1.51
Post-Intentions*Knowledge	-.44	-.43				
Agreeableness Difference Score						
STEP 1			.182	59.98*		
Post-Intentions	2.14*	.42				
Knowledge	.38	.04				
STEP 2			.185	40.68*	.003	1.89
Post-intentions*Knowledge	.50	.44				

Table 14. Regression Analyses Regressing Faking Behavior on Post-Intentions to Fake and Knowledge (cont'd).

Model	b	β	R^2	F	ΔR^2	$\Delta R^2 F$
Conscientiousness Difference Score						
STEP 1			.261	94.78*		
Post-Intentions	3.96*	.51				
Knowledge	.31	.02				
STEP 2			.263	63.84*	.002	1.715
Post-Intentions*Knowledge	.70	.40				

* p < .05

Note. The "Knowledge" variable was the continuous scale score from the post-questionnaire. Impression management and self-deception scores are from the applicant condition.

Table 15. ANOVA Predicting Faking Behavior.

Source	SS	df	MS	F	p	η^2
Impression Management	499.41	7	71.34	4.55	.001	.056
Warning	465.87	1	465.87	29.69	.001	.053
Valence	33.58	1	33.58	2.14	n.s.	.004
Knowledge	.19	1	.19	.01	n.s.	.000
Warning x Valence	1.49	1	1.49	.10	n.s.	.000
Warning x Knowledge	.28	1	.28	.02	n.s.	.000
Valence x Knowledge	7.03	1	7.03	.45	n.s.	.001
Warning x Valence x Knowledge	.00	1	.00	.00	n.s.	.000
Error	8346.93	532	15.69			
Total	13593.00	540				
Neuroticism Difference Score	3224.16	7	460.60	9.88	.001	.115
Warning	2512.24	1	2512.24	53.88	.001	.092
Valence	17.69	1	17.69	.38	n.s.	.001
Knowledge	415.85	1	415.85	8.92	.01	.016
Warning x Valence	23.31	1	23.31	0.50	n.s.	.001
Warning x Knowledge	167.30	1	167.30	3.59	n.s.	.007
Valence x Knowledge	6.70	1	6.70	.14	n.s.	.000
Warning x Valence x Knowledge	1.28	1	1.28	.03	n.s.	.000
Error	24852.42	533	46.63			
Total	41752.00	541				
Extroversion Difference Score	613.93	7	87.70	3.72	.001	.047
Warning	520.33	1	520.33	22.06	.001	.040
Valence	7.86	1	7.86	.33	n.s.	.001
Knowledge	.07	1	.07	.00	n.s.	.000
Warning x Valence	23.16	1	23.16	.98	n.s.	.002
Warning x Knowledge	1.71	1	1.71	.07	n.s.	.000
Valence x Knowledge	9.54	1	9.54	.40	n.s.	.001
Warning x Valence x Knowledge	63.01	1	63.01	2.67	n.s.	.005
Error	12569.26	533	23.58			
Total	15885.00	541				
Openness Difference Score	271.91	7	38.84	3.23	.05	.030
Warning	35.39	1	35.39	2.12	n.s.	.004
Valence	1.97	1	1.97	.12	n.s.	.000

94

Table 15. ANOVA Predicting Faking Behavior (Continued).

| Source	SS	df	MS	F	p	η^2
Knowledge	15.46	1	15.46	.93	n.s.	.002
Warning x Valence	79.27	1	79.27	4.75	.05	.009
Warning x Knowledge	99.47	1	99.47	5.96	.05	.011
Valence x Knowledge	19.31	1	19.31	1.16	n.s.	.002
Warning x Valence x Knowledge	46.75	1	46.75	2.80	n.s.	.005
Error	8896.07	533	16.69			
Total	9192.00	541				
Agreeableness Difference Score	503.96	7	71.99	3.44	.001	.043
Warning	452.88	1	452.88	21.65	.001	.039
Valence	13.27	1	13.27	.63	n.s.	.001
Knowledge	7.77	1	7.77	.37	n.s.	.001
Warning x Valence	1.59	1	1.59	.08	n.s.	.000
Warning x Knowledge	2.01	1	2.01	.10	n.s.	.000
Valence x Knowledge	.30	1	.30	.01	n.s.	.000
Warning x Valence x Knowledge	19.37	1	19.37	.93	n.s.	.002
Error	11149.34	533	20.92			
Total	15653.00	541				
Conscientious. Difference Score	2988.66	7	426.95	9.18	.001	.108
Warning	2062.59	1	2062.59	44.34	.001	.077
Valence	3.35	1	3.35	.07	n.s.	.000
Knowledge	485.49	1	485.49	10.44	.001	.019
Warning x Valence	17.67	1	17.67	.38	n.s.	.001
Warning x Knowledge	283.26	1	283.26	6.09	.01	.011
Valence x Knowledge	12.88	1	12.88	.28	n.s.	.001
Warning x Valence x Knowledge	39.44	1	39.44	.85	n.s.	.002
Error	24793.52	533	46.52			
Total	41700.00	541				
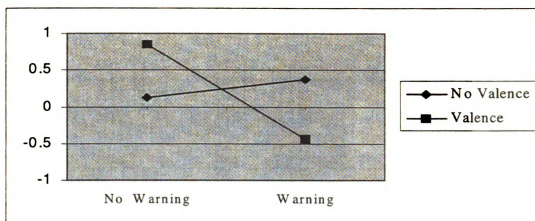
Figure 2. Interaction Between Warning and Valence When Predicting Faking as Measured by the Openness Difference Score.
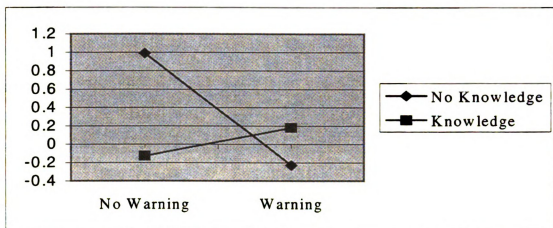
Figure 3. Interaction Between Warning and Knowledge on Faking as Operationalized as the Openness Difference Score.
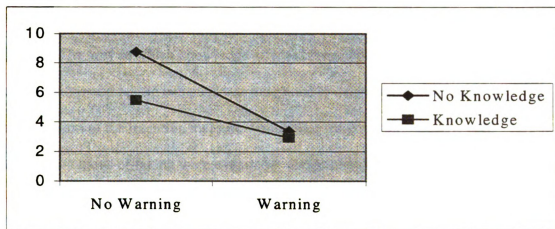
Figure 4. Interaction Between Warning and Knowledge on Faking as Operationalized by the Conscientiousness Difference Score.

Next, Hypothesis 7 was tested by examining if knowledge of the constructs being measured would moderate the relationship between the intention to fake and faking behavior. The way in which knowledge affected the relationship between both pre- and post-intentions and faking behavior was examined to determine if the time of the administration of the intention measure would alter results. Additionally, both operationalizations of faking were examined. Remember, faking behavior was assessed in two ways: with scores on the lie scale, and with difference scores. Although self-deception is not a measure of conscious distortion, both lie scale measures were used in these analyses so that these relationships could be explored.

First, a moderated regression was run, with impression management scores (from the applicant condition) as the dependent variable. The dichotomous knowledge variable (coded as 0 or 1), pre-intentions, and their interaction served as independent variables. As Table 11 shows, only pre-intentions significantly predicted impression management scale scores such that individuals with greater pre-intentions to fake scored lower on the impression management scale.

Six more moderated regressions (using self-deception scores and each of the five difference scores) were run with self-deception or the difference score of interest as the dependent variable. The dichotomous knowledge variable (coded 0 or 1), pre-intentions to fake, and their interaction, served as the independent variables. Table 11 shows that a significant interaction between pre-intentions and knowledge was only found when predicting faking on the openness scale. This interaction supports the prediction that those who had high intentions to fake and were given knowledge of what the test

measured would be able to increase their scores more than those who had similar intentions but were not given this information.

The same seven analyses (five with the difference scores and with each of the two social desirability scales) were conducted with post-intentions as an independent variable and are shown in Table 12. These analyses yielded three significant interactions. Faking on neuroticism, openness, and conscientiousness (as indicated by the difference scores for each of these scales) was significantly predicted by the post-intention to fake x knowledge interaction in the predicted direction. This means that those who had a high intention to fake, and were given knowledge about what the personality test measured, were able to increase their scores more through faking these three measures than those who had high intentions to fake but were not given this information. Therefore, the interactions that were significant support the hypothesis that knowledge would increase the relationship between intentions to fake and faking behavior by making those with greater intentions to fake more able to do so.

As described earlier, the knowledge manipulation was not successful (those provided with knowledge of the constructs scored similar to those on the knowledge scale, which was the manipulation check, as those who were not given this information). Therefore, the same analyses just discussed that used the dichotomous knowledge variable (received information versus did not receive information) were tested again with the knowledge scale (the measure of how much participants knew about the personality measure). Therefore, fourteen more regressions were run with the knowledge measure from the post-questionnaire as one of the independent variables. Table 13 includes the result of these analyses when the pre-intention to fake was used, while Table 19 displays

the results for post-intentions. None of the analyses that included the pre-intention measure as an independent variable yielded significant interactions. However, the post-intentions by knowledge interaction did significantly predict faking when impression management scores and self-deception scores were used to assess faking behavior. As expected, those who had high intentions to fake and had greater knowledge of what the test assessed, were better able to fake compared to those who had high intentions to fake and were not given the information. Therefore, again we find that providing knowledge of the constructs assessed by the test increases the relationship between the intention to fake and faking behavior.

To assess the contribution of each of the manipulations (i.e., warning, knowledge, and valance) on faking behavior, six ANOVAs were conducted with each of the measures of faking as the dependent variable and warning, valence, and knowledge as the independent variables. Results are presented in Table 15. As the Table shows, warning had the largest influence on faking with those who were warned about the lie scale being included on the test faking a lesser extent. Knowledge also predicted faking as measured by the conscientiousness difference score. Across the six ANOVAs only three interactions were significant. Figures 2, 3, and 4 plot these interactions. As can be seen, those who were offered an incentive and warned that a lie scale was included on the measure faked much less than those who were warned, but were not offered the incentive. Perhaps this occurred because those who were offered the incentive were told that they would not receive the money if they were detected faking. Therefore, the individuals who were warned realized the consequences for faking and therefore, faked to a lesser extent. Figure 4 shows the interaction between warning and knowledge on the openness

difference score. As the Figure shows, those who were not given a warning and had no knowledge of what the test was measuring were more likely to fake. However, keep in mind that the openness difference score had a very low reliability. Also, there were no significant differences between the honest and applicant responses to the openness scale. So, it appears that this scale was not faked. Therefore, these results are difficult to interpret. Figure 5 shows the interaction between warning and knowledge when the conscientiousness difference score was used as the dependent variable. Those who were given more knowledge of the construct being assessed faked more when there was no warning. However the differences between those given knowledge and those that were not are much smaller when a warning about the lie scale was given. Overall, these results show that warning had the strongest effect on faking and valence generally had no effect.

Exploratory Analyses

It has been suggested that faking may reduce the criterion-related validity of personality measures (e.g., Dunnette et al., 1962; McDaniel et al., 1997; Paulhus, 1986). Therefore, the relationship between the personality scales and performance criteria were examined across the two experimental conditions (i.e., honest and applicant) to determine how faking may influence the criterion-related validity of the measure.

As discussed earlier, a number of studies have found that conscientiousness is a valid predictor of job performance (Barrick & Mount, 1991; Hough et al., 1990). Research has also demonstrated that conscientiousness is a useful predictor of academic achievement (Dollinger & Orf, 1991). Therefore, in the present study, participants' Grade Point Average (GPA) served as the performance criterion.

The relationship between these criteria and the Conscientiousness scale across the two administration conditions was explored. As Table 3 shows, conscientiousness significantly predicted GPA and this correlation was larger for the honest condition ($\underline{r}$ = .15) than the applicant condition ($\underline{r}$ = .10). However, further analyses revealed that this difference was not significantly different across the two manipulations. This results is consistent with the preponderance of evidence suggests that faking has a small, negligible effect on validity (e.g., Christiansen et al., 1994). However, just because the difference is not significant in the present study, a .05 difference in the validity estimate across the honest and applicant condition may have practical effects. That is, selection decisions may be altered such that the best candidate for a job (as indicated by the test under honest conditions), will not be selected.

<u>Summary</u>

The theory of planned behavior predicted pre- and post-intentions to fake and faking behavior. These findings are similar to other studies that have used the theory of planned behavior in other contexts. However, valence toward doing well on the test and warning of a lie scale did not act as moderators of the relationships between attitudes, subjective norms, and perceived behavioral control and the intention to fake. Despite this, concern with a lie scale being included on the test did have a main effect on the intention to fake, with those who were more concerned about the lie scale having lower intentions to fake the test. Valence toward doing well on the test had no such main effect.

As the theory of planned behavior predicts, the intention to fake related to faking behavior, however, there was little support for knowledge of the constructs the test

measured as a moderator of this relationship. Support for knowledge as a main effect on faking behavior was also inconsistent.

Finally, faking did not adversely affect the relationship between conscientiousness and GPA. Although the correlation between these two variables was higher in the honest condition, no significant differences were found.

Discussion

Several empirical studies have been conducted examining the antecedents of faking behavior and the effects of faking on tests results, however, there is still no clear consensus as to the causes of faking or what its effects are. This may be because no attempt has been made to integrate the various empirical findings into any kind of theoretical framework. Instead, each study stands in isolation and does not build upon other research or explore possible interactions between different variables. This is the first study that attempts to examine multiple antecedents and effects on faking behavior simultaneously. It is also the first to use a theoretical framework to do so.

The present study was conducted to integrate empirical research regarding faking on personality tests into a model of faking behavior. The theory of planned behavior (Ajzen & Madden, 1986) was used as the framework to integrate these empirical findings. Support was found for the theory of planned behavior in predicting faking behavior. However, evidence for the moderating effects of valence, warning of a lie scale, and knowledge was generally not found.
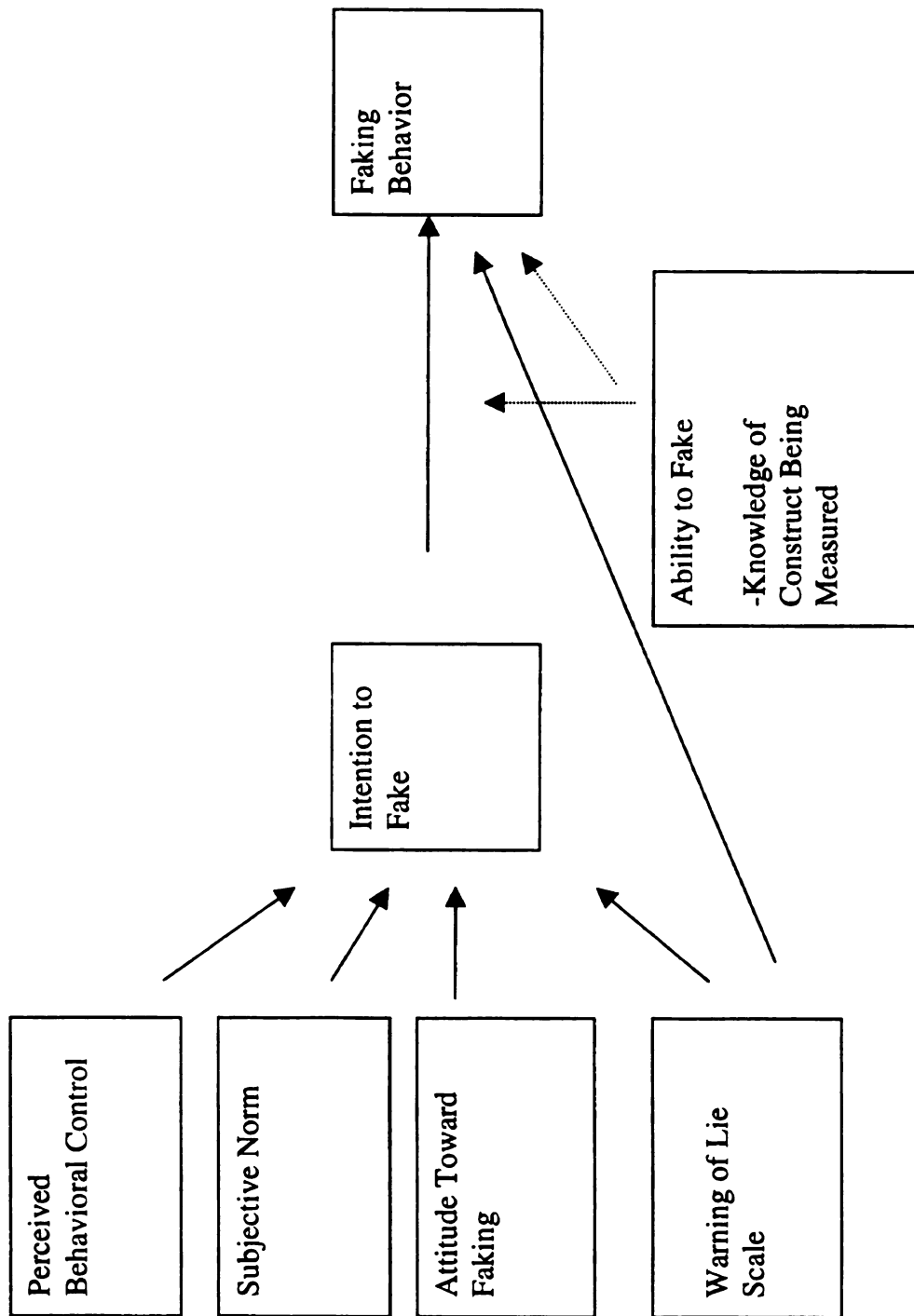
Figure 5. Supported Relationships.

Figure 2 contains the relationships that were supported in the present study. The author is not suggesting that this is a revised model of faking behavior, but simply wants to summarize the findings in a clear way. The figure shows that, as predicted, attitude toward faking, subjective norms toward faking, and perceived behavioral control all related to the intention to fake. However, warning of a lie scale, which was predicted to moderate these relationships, actually was found to have a main effect on the intention to fake. Notice that valence toward doing well on the test is not included in the model because it was not found to be a moderator or have a significant main effect on the intention to fake. As expected, the intention to fake related to faking behavior. The arrows from knowledge of the constructs assessed to faking behavior and the line that designates knowledge as a moderator between the intention to fake and faking behavior are broken arrows because there is inconsistent evidence for these two relationships. The results changed across operationalizations of faking and pre- and post-intentions. Below, each of these findings will be discussed in more detail.

Influences on Intention to Fake

The theory of planned behavior predicts that if one's attitude toward faking is positive, subjective norms toward faking are positive (i.e., the individual perceives that significant others would approve of the behavior in question), and it is believed that one could fake the test, the intention to fake will be higher than when these conditions are not present. This study supports these predictions. Therefore, the theory of planned behavior was successful at predicting the intention to fake the personality measure.

The correlations observed between attitudes, subjective norms, and perceived behavioral control and the intention to fake are similar to those observed in other tests of

106

the theory. For example, Ajzen (1991) reviewed studies that had tested the theory of planned behavior to explain everything from quitting smoking to theft. The correlations observed between these three variables and the intention to fake in the present study are very similar to those reported in the studies reviewed by Ajzen. Additionally, across the studies that Ajzen reviewed, when attitudes, subjective norms, and perceived behavioral control were used to predict intentions, the average multiple correlation was .71. A regression was run with the current data with pre-intentions regressed onto attitudes, subjective norms, and perceived behavioral control (see Table 7). The multiple correlation was .76; very similar to the estimate provided by Ajzen (when post-intentions were used the multiple correlation dropped to .26). Therefore, it seems that the results of this study are consistent with other tests of the theory of planned behavior.

Another expected finding is that attitudes, subjective norms, and perceived behavioral control related more strongly to pre-intentions than to post-intentions to fake. This is to be expected because as time between the measurement of attitudes, subjective norms, and perceived behavioral control and intentions increases, the weaker the relationship will be (Ajzen, 1991). Events may occur over time to alter these relationships. In the present study pre-intentions were measured the same time as attitudes, subjective norms, and perceived behavioral control. However, generally one week separated the administration of these measures and the post-intention measure. These results are likely not a result of method bias because attitudes, subjective norms, and perceived behavioral control did not correlate as strongly with each other as they did with pre-intentions to fake.

Another reason pre- and post-intentions may have yielded different relationships with the independent variables is that they use different referents. That is, while the pre-intention measure refers to faking in general, the post-intention measure refers to faking on the specific personality test the participants were administered. Therefore, post-intentions were more specific than pre-intentions and this specificity may have altered results.

Bagozzi (1992) suggests that the theory of planned behavior will not be successful at explaining substantial variance in behavior until other variables are considered. He suggests examining moderators of the relationships proposed by the theory. Two moderators of the relationship between attitudes, subjective norms, and perceived behavioral control of faking and intentions to fake, were examined. It was expected that valence toward performing well on the test would moderate these relationships. Valence toward doing well on the test was manipulated by offering $20 to participants who scored in the top 15% on the test. It was found that those who were offered the incentive had higher valence toward doing well on the test. However, no support was found for the moderation effect of valence. In fact, valence did not have any effect on either pre- or post-intentions to fake (i.e., the main effects were not significant). This finding contradicts past research. Research suggests that the more desirable a job is the more likely an individual is to fake to increase the chances of being hired (Schlenker, 1980; Schlenker & Weigold, 1992).

The failure to find that valence moderated these relationships may have occurred because the manipulation was not successful at simulating the valence one would feel if one really desired a job. That is, even though the manipulation did result in greater

valence toward doing well on the test, it did not create the same valence that would be felt if an applicant really wanted the job for which he or she was applying. Given the research that has proposed links between job desirability and faking (Fletcher, 1990; Schlenker, 1980) and that this relationship makes rational sense, future research should attempt to replicate these results using real applicants or using stronger valence manipulations (e.g., more money).

It was also expected that warning applicants that a lie scale was included on the personality measure would moderate the relationships between attitudes toward faking, subjective norms, and perceived behavioral control, and the intention to fake. Although no support was found for this relationship, a main effect for warning was found. Those who were warned that a lie scale was included on the test were less likely to intend to fake than those who were not warned about the lie scale. This finding is consistent with past research that has found that when individuals are warned that a lie scale is included in a measure, they fake less on the test (Doll, 1971; Kluger & Colella, 1993; Schrader & Osburn, 1977; Wheeler et al., 1996).

So, overall it seems that the variables in the theory of planned behavior (attitudes toward faking, subjective norms, and perceived behavioral control toward faking) predicted the intention to fake. However, proposed moderators of these relationships were not supported. Although Bagozzi (1991) has suggested that moderators would be useful for increasing the explanatory power of the theory of planned behavior, Ajzen (1991) has suggested that such moderators are generally not successful and does not encourage their inclusion in tests of the theory (Ajzen & Madden, 1986). He cites evidence that tests of the theory that have included such moderators are generally not

supported and the inclusion of them creates a less parsimonious model. The present

study confirms this position as no moderating effects were found.

Predictors of Faking Behavior

Most faking research has used scores on a lie scale to operationalize faking.

Other studies have used difference scores. Differences in the operationalization and

measurement of faking across studies may account for some of the discrepant findings

regarding the relationship between faking and test validity. Therefore, the present study

operationalized faking in two common ways by using a lie scale and difference scores.

By using these two methods, it could be determined if different operationalizations of

faking converged (i.e., drew the same conclusions).

The theory of planned behavior predicts that the intention to perform a behavior

will relate to the performance of that behavior. Therefore, it was anticipated that those

with greater intentions to fake the personality measure would be more likely to fake the

test. Two measures were used to operationalize faking: the five difference scores and

scores on the impression management and self-deception scales. Therefore, the

relationship between the intention to fake and faking behavior was examined across all

seven measures. Overall, findings were mixed and some were unexpected. It was found

that the difference scores were related to both pre- and post-intentions to fake the

measure, such that those with higher intentions to fake, faked a greater extent. However,

as would be expected, given their closeness in proximity (in terms of when they were

administered), the post-intention to fake was more strongly related to faking behavior.

Unexpectedly, impression management scores (and self-deception scores) were

negatively related to pre-intentions to fake. This suggests that those who indicated they

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would fake selection tests (on the pre-questionnaire) faked less on the personality test. However, it was also found that post-intentions were strongly and positively related to impression management and self-deception scores. That is, according to the post-intention measure, those who indicated after they took the test that they had intended to fake it had higher scores on the impression management scale. Again it was found that post-intentions was more strongly related to impression management scores than were pre-intentions. This is likely because of the time of measurement. The post-intention measure was administered shortly after the participant took the test (with the impression management scale included), whereas the pre-intention measure was administered about one week before the impression management scale. Alternatively, it may be the case that post-intentions, because it was more specific in referent, was a better predictor of faking on the personality test. Keep in mind that the pre-intention measure was more general in that it asked questions pertaining to the likelihood of faking a selection test in the future, while the post-questionnaire asked about faking on the personality test that was just administered.

Knowledge of constructs measured by the test was examined as a moderator of the relationship between the intention to fake and faking behavior. Limited support for the moderating effect of knowledge was found. However, these findings should be interpreted in light of the fact that the knowledge manipulation was not successful. It was found that those who were given knowledge of what constructs the test assessed scored similarly on the knowledge scale as those who were not given this information. Exploratory analyses did not yield any reason for this lack of effect. For example, the data was searched for possible outliers, but none were found. The knowledge scale was

also split into specific and general knowledge scales and still no effects were found between the two groups. Additionally, past experience with personality tests was controlled for and this did not influence the manipulation. However, there were significant differences in personality scale scores between those given knowledge and those not given knowledge. Therefore, it seems that although the knowledge manipulation may have failed, providing this information to participants manipulated something.

There are at least two possible explanations for this finding. First, maybe the knowledge scale did not adequately tap knowledge of the constructs measured. That is, the manipulation check was flawed, rather than the manipulation itself. Therefore, the manipulation actually worked, but the manipulation check was not adequate to measure this. Perhaps knowledge should have been measured in a different way. For example, after taking the test participants could have been asked to read a list of the personality items measuring the same construct (grouped together one right after the other) and try to explain what the group of items measured. This could have been done for each construct. This may have provided us with a more precise measure of knowledge.

Another alternative is that the knowledge manipulation was not successful at manipulating knowledge of the constructs assessed, but instead influenced variables that were not directly examined in the present study but do have an impact on faking behavior. For example, research has shown that perceptions of face validity indirectly influences test performance, with those who have greater face validity being more motivated and therefore performing better on the test (Chan, Schmitt, DeShon, Clause, Delbridge, 1997). It may be that those who were given knowledge of what constructs the

test assessed felt the test was more face valid (because they had a better understanding of why the test may relate to performance) and therefore, these individuals scored higher because they were more motivated. Clearly, this is speculation. The author is using this as an illustration to demonstrate how the manipulation may have effected faking behavior (as it clearly did) without affecting the knowledge participants had about the test.

Although unexpected in the present study, the results pertaining to knowledge are consistent with the work of others who have examined ability as a moderator in the theory of planned behavior. In the present study it was anticipated that those who were given knowledge of the constructs assessed by the test would be better able to fake. It was expected that knowledge would increase the strength of the relationship between the intention to fake and faking behavior. Ajzen and Madden (1986) suggested that ability has rarely been found to operate as a moderator in the theory of planned behavior. Although the results of the present study confirm Ajzen and Madden's position on moderators, it would be inappropriate to assume that knowledge (or ability) does not moderate the relationship between the intention to fake and faking behavior because the manipulation may have been unsuccessful.

Also of interest is the relationship between the two social desirability scales: impression management and self-deception. Paulhus (1984; 1986) suggests that self-deception is the component of socially desirable responding that reflects the tendency of individuals to see themselves more positively than is actually the case. Psychologically healthy individuals tend to see themselves more positively than they really are, so self-deception taps just how positive our opinions are of ourselves. Therefore, self-deception is related to true personality variance. Paulhus suggests that self-deception should not

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change across experimental manipulations. Unlike impression management, self-deception should remain constant because different situations should not alter how we view ourselves. Impression management, on the other hand, refers to conscious attempts to distort the way one is perceived. Paulhus proposes that impression management should change across experimental manipulations because people will want to be perceived in different ways across situations and will manipulate the impressions they make to ensure this occurs.

The present results do not support the construct validity of the self-deception or impression management scales. Self-deception and impression management were significantly correlated in the honest condition (.56) and the applicant condition (.71). Additionally, the difference scores for these two variables were significantly correlated at .67. These are substantial correlations indicating that there is no discriminant validity evidence for these two scales.

Additionally, self-deception scores changed across experimental manipulations. Not only did they change across the eight conditions (as demonstrated by the ANOVAs), but the relationship between self-deception and other variables changed across the honest and applicant conditions. Additionally, although not always the same strength, self-deception always changed in a similar manner to impression management. For example, it was found that, like impression management, self-deception was negatively related to pre-intentions to fake, but positively related to post-intentions to fake (but the relationship between self-deception and intentions was slightly smaller than it was with impression management).

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These results also provide little construct validity evidence for the impression management scale. Although impression management scores were significantly correlated with the difference scores, they were not as high as one would expect. However, it is not clear if the impression management scale failed to tap faking, if the difference scores were not successful at isolating faking, or both. Either way, it seems these two indicators of faking (impression management and difference scores) were measuring different things as they often resulted in different conclusions (they did not correlate similarly with other variables).

It should be noted that this study is not the first to find that self-deception and impression management are highly related and relate in similar ways to other constructs. In their study with truck driver applicants, Barrick and Mount (1996) found that the correlation between self-deception and impression management was above .40 in both samples examined. Additionally, they found that the two scales had similar relationships with the personality scales.

As discussed earlier, most of the research that has explored faking and the effects of faking on test results have used scores on social desirability scales as the operationalization of faking. These studies have rarely questioned the construct validity of these measures. However, the present study provides very little support that these measures are construct valid. This lack of construct validity may explain why findings within the faking literature are so discrepant. For example, different social desirability scales are used across these studies and these scales may tap self-deception and impression management to different extents. Clearly, more time and effort needs to be spent providing construct validity for these social desirability measures and developing

more distinct and valid measures of these two constructs (assuming these two constructs exist and are distinct). It is critical that a construct valid measure of faking behavior be developed. Only when we can precisely measure faking will we be able to determine when faking actually occurs and how it influences the results of non-cognitive selection tests.

Effect of Faking on Criterion-Related Validity

Much of the research in the faking literature has examined how applicant faking influences the validity of non-cognitive or personality measures (Dunnette et al., 1962; Hough et al., 1990; Ones et al., 1996). Generally, the way in which the relationship between the test scores and performance criteria changes as a result of faking is examined. In the present study, there is substantial evidence that faking was more prevalent in the applicant condition. As the paired samples t-tests in Table 6 demonstrate, participants did significantly increase their scores when asked to behave like applicants versus when they were asked to answer honestly. Additionally, impression management scores were also significantly higher in the applicant condition. Therefore, it was examined if this faking had an adverse effect on the criterion-related validity of the personality measure.

Results of this analysis demonstrated that the relationship between conscientiousness scores and GPA was not significantly different across the honest and applicant condition. Therefore, it seems that faking did not have an effect on the validity of conscientiousness in this study. However, there was still a difference of .05 between the validity of conscientiousness in the honest and in the applicant condition. Such a difference might still have a practical effect in selection contexts.

116

Implications and Directions for Future Research

The results of this study have several implications for the theory of planned behavior and the faking literature. First, this study extends the theory of planned behavior to a selection context. As far as the author is aware, there is no published study that has applied this theory to explain faking behavior in selection settings. Thus, the theory itself was supported. Attitudes, subjective norms, and perceived behavioral control all related to the intention to fake and the intention to fake related to faking behavior. However, the proposed moderators of these relationships were generally unsuccessful. As discussed, Ajzen and Madden (1986) suggest that such moderators should not be included because it destroys the parsimony of the theory and little support has been provided for moderators when they are included.

It is true that the inclusion of moderators and other variables will make the theory more complex. The fact remains, however, that although the theory is a good predictor of faking behavior there is still considerable variance that it does not account for. Therefore, other variables should be examined and integrated in the theory as both moderators and direct effects on the intention to fake and faking behavior. Researchers should explore the literature to find other variables that have been linked to faking behavior and determine if these variables can successfully be integrated into the theory to better predict faking.

Second, this study demonstrates that the theory of planned behavior can successfully predict faking behavior and may therefore be useful for altering faking behavior. For example, as the theory of planned behavior predicts, positive attitudes, subjective norms, and perceived behavioral control toward faking were all related to the

intention to fake. In turn, the intention to fake related to faking behavior. We should now use this information and determine how we can decrease applicant faking by altering attitudes, subjective norms, and perceived behavioral control over faking. For instance, we may be able to create negative attitudes toward faking by describing to applicants (prior to the test administration) why faking may be bad, unpleasant, or foolish. By doing this we may be able to create negative (or at least less positive) attitudes toward faking, which should decrease the intention to fake. The test administrator can explain, prior to administering the personality test (or non-cognitive measure), reasons why it is unwise to fake on such tests. For example, the administrator can explain to applicants that performance on such tests is linked to job performance and that individuals who score well on these tests do well on the job they were hired for. When individuals lie on these tests, they may be selected but may not perform well on the job and may then not stay with the organization. Such an explanation may alter an applicant's attitude toward faking by making the applicant recognize that it may be unwise and unnecessary to fake the test.

A similar procedure could be followed to alter subjective norms. This could be done by explaining to applicants that lying on such tests is typically not perceived well by others and is generally frowned upon. Additionally, perceptions of behavioral control may be altered by explaining to applicants that lying on the test is probably not as easy as one may think and that people are likely to decrease their scores by faking. Applicants may also be told that a lie scale is included in the measure. Although not examined in the present study, such warnings may result in decreased perceived behavioral control over faking, thereby resulting in lower intentions to fake.

However, it would probably be easier to alter attitudes toward faking and perceived behavioral control over faking than subjective norms. After all, the test administrator would have a difficult time convincing an applicant that the people who matter to him or her would frown upon faking if the applicant knows this is not the case. Future research should attempt to explore if such procedures would result in a significant decrease in faking. Such questions can easily be addressed in both the field and in lab studies.

Third, these findings may help explain some of the discrepancies in the literature with regards to how faking influences the results of personality tests and, more generally, non-cognitive measures. In this study, two operationalizations of faking were used. The personality difference scores served as one measure of faking and scores on the social desirability scales served as another. These two operationalizations are the most frequently used in the literature. It was found that these two measures of faking rarely converged. For example, it was found that impression management scores correlated negatively with pre-intentions to fake, while the difference scores had positive relationships.

If the results using these two operationalizations of faking differed so drastically within the same study, it is not surprising that the literature is plagued by such discrepancies when different procedures and measures of faking are used across these studies. Future research should attempt to determine why these differences exist. Earlier it was discussed how there is little construct validity evidence for the social desirability scale. Perhaps the reason the impression management scores and difference scores led to different conclusions is because the impression management scale is not isolating

conscious distortion. Future work should work towards isolating the reasons for these differences in results.

These results also demonstrate that Paulhus' self-deception scale and impression management scale overlap a great deal. Although the impression management scale was generally more predictive of faking behavior, the two scales were highly correlated and related to other variables in a similar manner (e.g., both were negatively correlated with pre-intentions to fake, but positively correlated with post-intentions). Therefore, it is not clear if these two constructs are distinct. Very little work has been done determining the construct validity of these scales. Future research should attempt to determine the construct validity of these measures.

An alternative measure of faking that may more precisely measure faking is response latencies. It has been found that when individuals fake their responses, it takes longer for them to respond (Dwight & Alliger, 1997). However, when they respond honestly it is found that response times decrease substantially. Future work should examine how response latencies can be used to assess faking behavior. Using this measure of faking we may be able to better assess the antecedents and consequences of faking behavior.

Finally, these results have implications for other non-cognitive selection measures. Generally, results that have been obtained in the faking literature, using personality tests, have been replicated with other non-cognitive measures (e.g., biodata forms, integrity tests) and vice versa. For instance, it has been found that when asked to fake, people can successfully increase their scores on these measures by about a standard deviation (Dunnette et al., 1962; McDaniel et al., 1996; McFarland & Ryan, in press).

Therefore, research should be directed toward applying the theory of planned behavior to explaining faking on these other tests and also determining if the procedures described above for deterring faking (e.g., altering attitudes) decrease faking on these other non-cognitive measures.

Limitations

Although this is the first study to integrate empirical findings within the faking literature into a theoretical framework, the are some limitations of this study that should be pointed out. First, some may question the generalizability of these results because this study was conducted in the lab rather than the field. However, a lab study was used to provide the necessary control needed to test these relationships. To increase the external validity of the findings, participants were asked to pretend that they were applicants for a job they really wanted and some were even offered an incentive that was based on their performance. However, it is suggested that these findings be replicated within an actual field setting.

Second, three of the difference scores had very low reliability. This likely occurred because, for some of the scales, there was little variance in the extent to which people faked or there was little change in scores from the honest to the applicant condition. For instance, scores on the openness scale were not significantly different across the two administrations. Therefore, it seems that people either could not or simply did not fake this measure. Since there was little change across the two administrations for this variable, the difference score was not reliable.

Third, this study examined a number of different variables that effect faking in order to gain a deeper understanding of the antecedents and effects of faking behavior.

Clearly, however, all the variables that effect faking could not be considered within the context of this study. Therefore, future research should explore how the theory of planned behavior may be combined with other variables that have been found to influence faking behavior. Attempts should be made to examine as many of these different effects on faking behavior simultaneously. Only then will we have a true understanding of what the antecedents of faking behavior are and the effect faking has on test results.

Finally, it is important to note that the model of faking behavior presented in this study does not address the *process* of faking. That is, the thought processes and strategies people use when faking was not examined. Future work should be done to identifying the process of faking behavior. Such work may utilize a verbal protocol to assess the thoughts that respondents have when they fake on a non-cognitive test. This assessment method may also give us some ideas about how to best measure faking behavior.

Conclusion

The present study used the theory of planned behavior to integrate and organize empirical findings from the faking literature. Although the hypothesized moderators were generally not supported, it was found that the theory itself did predict faking behavior. Therefore, it seems that the theory of planned behavior can be used to predict when faking is likely to occur and also suggests a number of ways to deal with faking (e.g., altering attitudes toward faking). Future attempts should use this theory to predict faking behavior while simultaneously considering other empirical findings from the faking literature that may have direct effects on the intention to fake, faking behavior, or

may moderate the relationships in the theory. Only when we consider the multiple influences on faking behavior will we be able to truly understand when faking is likely to occur, how it may influence the results of our tests, and how we may deal with faking on personality measures to ensure that these tests provide useful information from which selection decisions can be based.

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APPENDICES

APPENDIX A

Pre-Questionnaire

Please respond to the following questions as honestly as you can. Several of the items below ask about your feelings toward lying on selection tests. Please note that by the term "lying" it is meant any response that is not completely accurate. Therefore, providing a response on a selection test that you think is an exaggeration of the truth is considered a lie, as is a response that is completely false. Please use this definition of "lying" as you respond to the items below.

Please indicate the extent to which you agree or disagree with items 1 to 17 using the following 5 point scale:

> 1 = Strongly Disagree
> 2 = Disagree
> 3 = Neither Agree nor disagree
> 4 = Agree
> 5 = Strongly Agree

1. If I lied on a selection test, most of the people who are important to me would disapprove. [SN]

2. No one who is important to me thinks it is OK to lie on a selection test. [SN]

3. Most people who are important to me will look down on me if I lie on a selection test. [SN]

4. It would be easy for me to lie on a selection test. [PBC]

5. If I want to, I can lie on a selection test. [PBC]

6. I can imagine times when I might lie on a selection test, even if I had not planned to. [PI]

7. Even if I had a good reason, I could NOT bring myself to lie on a selection test. [PI]

8. If I had the opportunity, I would lie on a selection test. [PI]

9. I would NEVER lie on a selection test. [PI]

10. I would exaggerate the truth on a selection test. [PI]

134

11. I may lie on a selection test in the future. [PI]

12. I would make myself look as good as I possibly could on a selection test. [PI]

13. I would NOT exaggerate the truth on a selection test. [PI]

14. My parents would approve of me lying on a selection test. [SN]

15. Lying on a selection test would be easy for me. [PBC]

16. I would try to make myself look as good as possible on a selection test. [PI]

17. I would lie on a selection test. [PI]

18. I think that lying on a selection test is (Circle the response that best describes how you feel): [A]

| 1 | 2 | 3 | 4 | 5 |
| Bad | | | | Good |

| 1 | 2 | 3 | 4 | 5 |
| Unpleasant | | | | Peasant |

| 1 | 2 | 3 | 4 | 5 |
| Foolish | | | | Wise |

| 1 | 2 | 3 | 4 | 5 |
| Useless | | | | Useful |

| 1 | 2 | 3 | 4 | 5 |
| Unattractive | | | | Attractive |

A=Attitude toward faking, SN=Subjective norms toward faking, PBC=Perceived behavioral control, PI=Pre-intentions to fake.

APPENDIX B

Post-Questionnaire

Please respond to the following questions as honestly as you can. Several of the items below ask about your feelings toward lying (or faking) on the test. Please note that by the terms "lying" and "faking" it is meant any response that is not completely accurate. Therefore, providing a response on a selection test that you think is an exaggeration of the truth is considered a lie, as is a response that is completely false. Please use this definition of "lying" and "faking" as you respond to the items below.

Answer each of the following statements using the five point scale below.

1	2	3	4	5
Strongly Disagree	Disagree	Neither Agree or Disagree	Agree	Strongly Agree

1. It was usually clear to me which personality trait an item was measuring. [K]

2. I intended to give some inaccurate answers on the test. [I]

3. I would like to score in the top 15% on this test. [V]

4. It is very desirable to me to get a high score on the personality test. [V]

5. I was concerned that A lie scale would identify me as a liar. [C]

6. Some items measured how reliable and responsible a person is. [K]

7. I attempted to fake my responses on the test. [I]

8. I think some items were on the test to detect lying. [C]

9. I intended to respond to all items honestly. [I]

10. It was obvious which personality traits the test was measuring. [K]

11. I was NOT worried that I would be identified as lying on the test. [C]

12. I intended to make myself look as good as possible on the test. [I]

13. I think there is a way the experimenter can tell if I lied on the test. [C]

14. Some items measured how friendly and agreeable a person is. [K]

15. I tried to be honest so that I would not be identified as lying on the test. [C]

16. It was very important to me to do well on the personality test. [V]

17. I intended to make myself look very good on the selection test. [I]

18. I do NOT think there is any way the experimenter can tell if I lied or not.   C]

19. I really wanted to score high on the test.   [V]

20. I could NOT tell what the personality test was measuring.   [K]

18. While I took the test, I was concerned that my test scores may be thrown out because I lied.   [C]

22. I attempted to lie a lot on the test.   [I]

23. Some items measured how concerned people are with how others view them.   [K]

24. Doing well on the personality test was NOT desirable to me.   [V]

25. Some items measured whether a person prefers to be alone or enjoys being around others.   [K]

26. I intended to lie on some items to increase my test score.   [I]

27. Some items measured how open a person is to new experiences.   [K]

28. I have heard that tests like the one I just took often include items to detect when an applicant is lying.   [C]

29. As I took the test I was concerned that there may be a lie scale included that would identify those who lie on the test.   [C]

K=Knowledge of construct being measured, I=Intention to fake, C=Concern with being identified as a faker, V=Valence toward doing well on the test.

APPENDIX C

Demographic Questionnaire

Please circle or fill in the appropriate response:

PID _____

1. **Year:**

    a. Freshman
    b. Sophomore
    c. Junior
    d. Senior
    e. Other_____

2. **Age:** _____

3. **Sex:**   a. Male   b. Female

4. **GPA** (If you are a freshman, give the cumulative
    GPA you had when you graduated high school):   _____

5. **Race:**
    a. White
    b. African American
    c. Hispanic
    d. Asian
    e. Native American
    f. Other_____

6. How many times have you taken the type of personality test you just took:

    1   Never
    2   Once or twice
    3   Three or four times
    4   More than 4 times

7. Where you thinking of any particular job as you answered the personality test?

8. If the answer to question 7 was yes, what type of job were you thinking about?

_____

Consent Form

Selection Testing

Purpose of Study

The following study has two parts. You will complete a personality test that is typically used to hire job applicants, two times. You will also be asked to answer some survey questions. The purpose is to obtain a better understanding of how individuals respond to such tests. The entire experiment will take no longer than one and a half hours (1.5 hours). You will receive 3 credits for your participation

Informed Consent

The nature and general purpose of this study has been explained to me and the attached statement has been read to me by Lynn McFarland, from the Department of Psychology. I understand that my participation is voluntary and that all information is confidential and my identity will not be revealed. I am free to withdraw consent and discontinue participation in the project at any time. Any questions I have about the project will be answered by the researcher.

On the basis of the above statements, I agree to participate in this project.

_____   _____
Participant's Signature   Date

_____
Participant's Name (Printed)

APPENDIX E

Protocol

Hello, my name is Lynn McFarland and I will be your experimenter today. The purpose of this study is to see how individuals respond to a personality test that is frequently used to select applicants into a variety of different jobs.

In this experiment you will take a 60 item personality test twice, given different instructions each time. You will also be asked to fill out a survey that will ask you for some demographic information and another survey that will ask some other questions relating to the test. This entire experiment should last one and a half hours (1.5 hours). You will receive one credit for your class per half hour. However, even if this experiment takes less than one and a half hours you will receive the full three credits if you complete all parts of the experiment.

[hand out consent form]

The consent form I am handing out states that you are participating in this study voluntarily and that all of your responses will remain completely confidential and anonymous. You can leave this experiment any time you want. However, you will only receive credit for the amount of time you are here.

[wait until all participants have signed the consent form and answer any questions they might have. Then collect the forms.]

[hand out scantron answer sheets]

I am now handing out an answer sheet. In the section of your answer sheet that is marked PID write your mother's birth date as your identification number and place that

number in the section of your questionnaire that is marked PID. For example, if your mother's birth date is June 13, 1950, you should record "061350' in the section of your questionnaire that is marked PID. If your mother was born on September 6, 1955, you should record 090655 in the section of your questionnaire that is marked PID. If you do not know or want to use your mother's birth date, then use some other number that you will remember because you will have to use this PID number throughout the experiment.

For the honest condition

For the knowledge of construct condition

[hand out description of constructs measured on the test]

The sheet that I am handing out describes the five constructs (personality traits) the personality test you are about to take [or about to take again] measures. The personality test was created to measure five personality traits. It measures Conscientiousness, Neuroticism, Openness to New Experiences, Agreeableness, and Extraversion. [hand out sheet that describes each construct and have them read along as you read the following] Conscientiousness is how reliable a person is. Those high in conscientiousness are typically rule abiding and reliable while those low on this trait are typically late and fail to meet deadlines. Neuroticism measures how worried a person typically is. Those high in neuroticism generally are very concerned with how others perceive them and they are very concerned with detail. Those low on this trait are less concerned with how others perceive them and they tend to worry much less. Openness to new experiences is how open minded a person is. Those high on this trait are typically very willing to try new things and are open to others' opinions and points of views, while those low on this trait tend to be closed minded. Agreeableness is how agreeable a

141

person is. Those high on this trait generally avoid confrontations or arguments and are very friendly. Those low on this trait tend to enjoy conflict and often disagree with others. Finally, extraversion measures how outgoing and friendly an individual is. Those high in extraversion generally speak more quickly, have no trouble speaking to new people, and enjoy being around others. Those low on this trait generally prefer to be alone and are more likely to get anxious when they are around people they do not know.

For all honest conditions

Before I hand out the personality test I want to remind you that the test you are about to take is one that is frequently used by employers to select employees for all types of jobs (e.g., managerial jobs, salesperson, police officer, etc.). However, keep in mind that within this context the results of this test has no bearing on you. Therefore, please answer the following questions on the personality test as honestly as possible. Your answers will remain completely anonymous. I have no way of connecting the test back to you. Your answers will be used for research purposes only, and will not be used to evaluate you in any way. The nature of the project requires that you answer the following questions as honestly as possible, so please provide as accurate answers as you can.

For warning of lie scale condition

[half of the participants will also be given the following instructions]

Sometimes people try to make themselves out to be better than they really are. Therefore, this test includes a scale to check on this. Such scales are used to identify individuals who give inaccurate or false responses. Please keep this fact in mind as you take this selection test.

[hand out personality test]

There is no time limit on this test. You may begin.

[wait till everyone appears to be done]

Has everyone completed the test? [if everyone has finished, proceed to next test/measure (this will change depending on if the test is taken under the honest condition first or after the applicant condition)].

Applicant Conditions

[hand out scantron answer sheets]

I am now handing out an answer sheet. In the section of your answer sheet that is marked PID write the same PID that you wrote on the other answer sheet.

For Knowledge of constructs measured condition

[if the test was already administered under the honest condition, then participants will simply be reminded that the test measures five constructs and given the names of these five constructs again. If the participants are given the personality test under the applicant condition first, then they will be given the same instructions as described above]

For all applicant conditions

Again I would like to remind you that the test you are about to take is one that is frequently used by employers to select employees for all types of jobs (e.g., managerial jobs, salesperson, police officer, etc.). When answering the questions on the personality test imagine that you are a job applicant. Please answer as you would if you were really taking the test to get a job. Keep in mind that your answers will be kept completely anonymous.

For incentive condition

[in addition to the general applicant instructions given directly above, participants in this condition will also be told the following]

To make this situation more like an applicant situation, we are offering an incentive. Those of you who score in the top 15% on this test will receive $20.

For warning of lie scale condition

[those in this condition will also be given the general applicant instructions above. In addition half will also be in the incentive condition.]

Sometimes people try to make themselves out to be better than they really are. Therefore, this test includes a scale to check on this. Such scales are used to identify individuals who give inaccurate or false responses. Individuals who have a high score on this scale (i.e., those individuals that the lie scale identifies as faking responses) will be eliminated from the selection process (and from consideration for the incentive if they are in that condition). Please keep this fact in mind as you take this selection test.

For all applicant conditions

[hand out personality test]

There is no time limit on this test. You may begin.

[wait until it appears that everyone has completed the test]

Has everyone completed the personality test? [if everyone is done hand out another scantron form]

Once again, I am handing out an answer sheet. In the section of your answer sheet that is marked PID write the same PID that you wrote on the other answer sheets. Please check to ensure that the PID you put on this answer sheet is the same one you placed on the others.

[hand out post-questionnaire]

The survey I am handing out [the post-questionnaire] asks you some questions regarding your orientation and frame of mind while you were taking the test. [Read instructions out loud as they read along] Please answer honestly. Again, I have no way to connect your responses back to you. Responses will be kept anonymous, so please respond honestly.

[when it seems like everyone is done]

Has everyone completed the survey? [If everyone is done proceed to hand out the demographic form].

[Hand out demographic questionnaire]

The questionnaire I am handing out asks you some demographic information. Please completely fill out this questionnaire. Where it says PID please write the same ID you put on all scantrons and on the post-questionnaire.

[when everyone is done filling out the demographic form, hand out debriefing form]

I am now handing out the debriefing form which will describe the purpose of this study. [I will read the debriefing form to them as they read it themselves]. Are there any questions? If anyone has any questions regarding this study feel free to call me at the number listed on the debriefing form. Thank you very much for your participation.

APPENDIX F

Debriefing Form

Research has indicated that personality tests (like the one you just took) can be faked. For example, when asked to increase scores on these types of tests, individuals can increase their scores considerably. Research has also shown that applicant faking on personality tests may reduce the useful of these tests. For example, it has been shown that when applicants fake these tests, the tests are not as valid (i.e., the test does a poor job of identifying those who would do well on the job). Therefore, researchers and employers have tried to determine ways to reduce applicant faking on these tests within selection contexts.

Considerable research has shown that warning applicants that there is a lie scale on the test that identify fakers may reduce applicant faking. Additionally, it has been suggested that the more information applicants have about a test, the more likely they are to fake it (because they are better able to fake). Research also suggests that the more desirable the outcome of doing well is, the more applicants fake. Therefore, if an applicant finds a job very desirable, he or she is considerably more likely to fake responses in an effort to get as high a score as possible in the hopes that this will get him or her the job.

This study attempts to examine a number of the factors that may reduce applicant faking in selection contexts. For example, some participants were given information about the constructs (personality traits) that the test was designed to measure. We believe that we will find that those of you given such information will have higher scores (when you were asked to take the test as an applicant) as compared to those who were not given this information.

At this point I would like to urge you to **remember (write down) the ID number** you have used throughout this experiment. Some of the participants in this study were given an incentive of $20 when they took the personality test under instructions to behave like an applicant. These individuals were told that those who scored in the top 15% on the test would receive the money. Therefore, those who were in this condition should email or call me (see below) after December 15, 1999. When you contact me, be sure to have your ID number handy. I will use this ID number to look up your score and tell you if you were in the top 15% (note that a lie score will not be used to eliminate any individuals from consideration for the money). **If you do not remember your ID number, I will have no way of knowing how you scored on the test and there is no way you can receive the money.** If you do qualify for the money (i.e., you are in the top 15%) I will tell you how and when to pick up the $20.

To be fair, those of you who were offered an incentive and were told that a lie scale was included on the measure will be considered separately from those who were not informed that a lie scale was included in the measure (as those of you warned about the lie scale

would likely have not scored as high as those who were not). Also, those not offered an incentive will also be eligible for $20 as well. Of the approximately 160 individuals who take part in the experiment who were not given such an incentive, 15% will be randomly selected to receive the $20. Contact me by phone or email (see below) after December 15, 1999 to find out if you were selected as one of the 15% to receive the money. **Again, make sure you have your ID number. If you do not know your ID number I will have no way of knowing if you were one of the people selected to get the money.**

Thank you very much for your participation. If you have any questions regarding the study please feel free to contact me, Lynn McFarland, by phone or email *(MCFARL18@pilot.msu.edu or 355-2171)*.