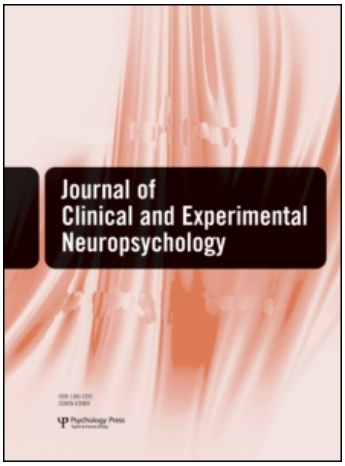

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The residual effect of feigning: How intentional faking may evolve into a less conscious form of symptom reporting

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The residual effect of feigning: How intentional faking may evolve into a less conscious form of symptom reporting

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We conducted three studies that address the residual effects of instructed feigning of symptoms. In Experiment 1 ($N = 31$), undergraduates instructed to exaggerate symptoms on a malingering test continued to report more neurocognitive and psychiatric symptoms than did nonmalingering controls, when later asked to respond honestly to the same test. In Experiment 2 ($N = 28$), students completed a symptom list of psychiatric complaints and then were asked to explain why they had endorsed two target symptoms that they did not, in actuality, endorse. A total of 57% of participants did not detect this mismatch between actual and manipulated symptom endorsement and even tended to adopt the manipulated symptoms when provided with an opportunity to do so. In Experiment 3 ($N = 28$), we found that self-deceptive enhancement is related to the tendency to continue to report neurocognitive and psychiatric symptoms that initially had been produced intentionally. "Blindness" for the intentional aspect of symptom endorsement may explain the intrinsic overlap between feigning and somatoform complaints.

Keywords: Neuropsychological assessment; Malingering; Choice blindness; Self-deception.

INTRODUCTION

The past decade has witnessed a rapid expansion of studies attempting to optimize symptom validity assessments in neuropsychology. The vast majority of such studies seem to take the categorical thinking of the *DSM–IV–TR* (*Diagnostic and Statistical Manual of Mental Disorders–Fourth Edition, Text Revision*; American Psychiatric Association, 2000) for granted by assuming that there exists a clear line of demarcation between the intentional fabrication of symptoms and the nonconscious production of symptoms (e.g., LoPiccolo, Goodkin, & Baldewicz, 1999). The *DSM–IV–TR* uses the label *malingering* when feigning symptoms is motivated by external benefits (e.g., financial compensation) and the label *factitious disorder* when feigning is motivated by internal reasons (i.e., acting out the patient role). The basic assumption that underlies this distinction is that patients with somatoform complaints create their symptoms nonconsciously and truly believe that they experience often-distressing symptoms, whereas malingerers or factitious-disordered patients feign illness.

Several authors, however, have questioned whether there exists such a clear demarcation between feigning and somatoform symptoms (e.g. Delis & Wetter, 2007; Hamilton, Feldman, & Cunnien, 2008). They point to evidence suggesting that malingered conditions, factitious disorders, and somatoform symptoms share many characteristics. Surveying the literature, Jonas and Pope (1985) observed that patients who feign symptoms and patients with somatoform symptoms are very similar in terms of age of symptom onset, course of symptoms, and lack of treatment responsiveness. More recently, workers in the field have similarly argued that patients with factitious disorder cannot often be distinguished from patients with somatoform disorder (e.g., Krahn, Bostwick, & Stonnington, 2008).

According to the *DSM–IV–TR*, intentional control over symptoms is a hallmark feature of feigning. Jureidini and Taylor (2002), however, speculated that some individuals become so deeply involved in their role of pretending to be sick that they lose sight of the conscious origins of their role-playing behavior. Relatedly,

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experimental evidence also buttresses the idea that people may come to believe in the reality of their intentional fabrications. For example, in two studies, Polage (2004) provided participants with a list of childhood events (e.g., having experienced a hospitalization overnight). Participants first rated how certain they were that each event had happened to them before the age of 10 and then were instructed to fabricate stories about some of the events they previously evaluated as unlikely. During a second test session, one week later, participants were again asked to evaluate the likelihood of all the childhood events. Polage found that 10–16% of the participants in the two studies eventually came to believe that the stories they had fabricated reflected what really happened to them.

The current studies examine whether a similar effect, whereby fabricated symptoms come to be mistaken for "real symptoms," occurs when people are instructed to feign symptoms. We also explored whether self-deception—a trait that can be conceptualized as prerequisite to lack of introspective ability (see Boone, 2007a)—may underlie the hypothesized effect.

EXPERIMENT 1

The first study used a test–retest design with an instructed feigning group and a nonfeigning control group. Both groups were provided with a brief vignette of a criminal case. Participants in both groups were asked to identify with the defendant described in the case. The feigning group was initially instructed to feign symptoms in a credible way, whereas controls were asked to respond honestly. Next, all participants completed a symptom scale that is widely used in neuropsychological assessments. After about 60–70 min, the symptom list was administered again, but this time both groups were instructed to respond honestly. We examined whether at the retest former malingerers (feigning–honest condition) would report more symptoms than control participants who were never instructed to malinger (honest–honest condition). This pattern of findings would support the hypothesis that former malingerers become less aware of the intentional nature of symptom report following instructions to feign symptoms.

Method

Participants

A total of 31 (9 men, 22 women) undergraduate students participated in the study. Their mean age was 22.3 years (SD = 3.68; range 19–38). Participants were randomly assigned to either the feigning–honest (n = 17) or the honest–honest group (n = 14). Students received course credits for their participation. The standing ethical committee of the Faculty of Psychology and Neuroscience of Maastricht University approved the study.

Measures

Participants completed a Dutch research version of the Structured Inventory of Malingered Symptomatology (SIMS; Smith & Burger, 1997; Merckelbach & Smith, 2003) twice (Test 1, Cronbach's alpha = .97; Test 2, Cronbach's alpha = .94). The SIMS is a 75 true–false self-report instrument designed to screen for the exaggeration of neurocognitive and psychiatric complaints (e.g., Widows & Smith, 2005) and is composed of items that describe atypical and rare symptoms and experiences. The SIMS contains five subscales, each with 15 items, which address commonly feigned conditions: amnesia, neurologic impairment, psychosis, affective disorders, and low intelligence. After recoding some items, yes-answers are summed to obtain a total score, with higher scores indicating more symptom overendorsement. Previous studies have recommended a cutoff of 16 to identify possible feigning (Merckelbach & Smith, 2003).

Design and procedure

The study was conducted in a quiet teaching room, and participants were tested in small groups (4 to 6 participants). Participants in both conditions were provided with a one-page case vignette describing a real criminal case in which the defendant had illegally entered a medieval building, thereby causing stones to fall down. One of the stones hit a girl, who died instantly. The defendant was charged with manslaughter (see Appendix A for the scenario and instructions). We have used this scenario in previous experimental simulation studies (e.g., Merckelbach, Smeets, & Jelicic, 2009). We instructed all participants to imagine that they were the defendant and that they were required to undergo a forensic evaluation. Next, we told participants in the feigning–honest group to fabricate a serious psychological condition in a credible way, so as to minimize criminal responsibility. In contrast, we instructed the honest–honest group to respond honestly. Both groups were then given the SIMS. After they completed the SIMS, participants carried out a series of filler tasks (i.e., Sudoku puzzles). After about one hour, participants in the feigning–honest group were told that they had been identified as malingerers and that they had to complete the SIMS again, this time in an honest way. Participants in the honest–honest group were told that people sometimes change their minds about the degree to which they have complaints and that they were therefore asked to complete the SIMS a second time.

Results and discussion

Figure 1 shows total SIMS scores of both groups at the first and the second test. A 2 (groups) × 2 (tests) analysis of variance (ANOVA) with repeated measures on the second factor yielded the expected main effect of test and the expected test group interaction, both Fs(1, 29) > 14.60, both ps < .01. Both effects reflect the different instructions given on the first test (i.e., feigning vs. honest responding) and indicate that, overall, participants did what they were asked to do.

More interestingly, we found a main effect for group, $F(1, 29) = 23.4$, $p < .01$, $\eta^2_p = .45$, due to the fact that the feigning–honest group reported more symptoms at both test occasions. Follow-up t tests indicated that at the first

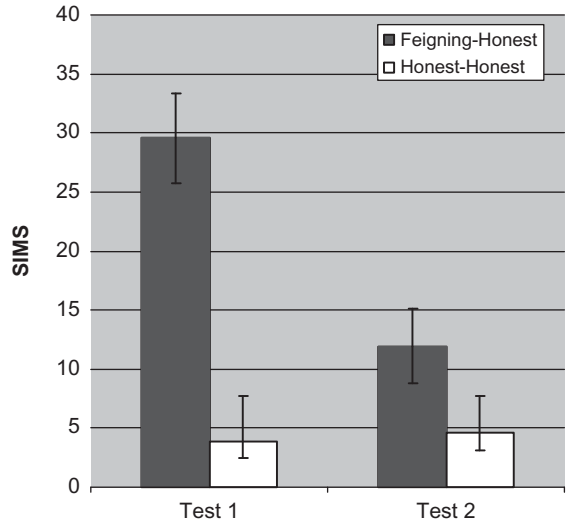

Figure 1. Mean SIMS (Structured Inventory of Malingered Symptomatology) scores of the feigning–honest (n = 17) and the honest–honest (n = 14) groups.

test, the feigning–honest group had higher SIMS levels than the honest–honest group, which, again, is not surprising as they were instructed to feign symptoms: $t(20.2) = 6.40$, $p < .01$. More importantly, on the second test, the feigning–honest group continued to score higher on the SIMS than did the honest–honest group: $t(22.9) = 2.10$, $p < .05$. This effect had a medium effect size in term of Cohen's d (i.e., $d = 0.73$).

We also examined the proportion of participants who at the second test occasion scored above the SIMS cutoff. In the feigning–honest group, this proportion was 29% (i.e. 5 out of 17 participants), whereas in the honest–honest group it was 7% (i.e., 1 out 14). This difference did not attain significance (Fisher's exact two-tailed p = .18), perhaps because our control condition was not sufficiently neutral. That is, we also provided the control group with the case vignette and instructed participants to identify with the defendant. Although we asked control participants to respond honestly to the SIMS items, the figure of 7% exceeding the cutoff at the second test suggests that at least 1 control participant filled out the questionnaire as if he or she were a defendant in a stressful situation. Accordingly, if we had used a more neutral control condition (i.e., no case vignettes and no identify-with-defendant instructions), experimental effects might have been more impressive.

At a minimum, our data indicate that feigning symptoms produces residual effects: Individuals who first feign symptoms, but later are asked to report honestly, endorse more symptoms at retest than do honest controls. Intentional overendorsement occurs at the first test, as a function of instructional set. However, at the retest the mechanism to account for the increased symptom reports in former malingerers, compared with controls, is less obvious.

The scenario in the current study pertained to a very specific and distinct theme: criminal responsibility. Because criminal defendants appear to feign in a much more exaggerated fashion than civil litigants (Boone, Lu, & Herzberg, 2002; see also Merckelbach et al., 2009), it remains to be seen whether residual effects of feigning depend on extreme initial levels of feigning, or whether they also emerge in situations where initial feigning is far subtler.

How can we explain the residual effects of feigning? In line with the notion that mental processes involved in intentions and choices are often poorly accessible for conscious introspection (Nisbett & Wilson, 1977), one could argue that some people may "forget" the fabricated origins of their symptom reporting. Indirect evidence for this hypothesis comes from studies on choice blindness. For example, Johansson, Hall, Sikström, and Olsson (2005) showed participants pairs of photographs of female faces. On each trial, participants were asked which face they found most attractive. Next, participants were provided with the opportunity to look more closely at the photograph they chose and to explain their choice. On some trials, however, participants were asked to scrutinize the wrong photograph (i.e., the one they did not choose) and were asked to explain why they had chosen it. On average, participants detected only 30% of these mismatch trials. In the words of the authors (Johansson et al., 2005, p. 116): "Participants failed to notice the conspicuous mismatches between their intended choice and the outcome they were presented with. . . . We call this effect choice blindness."

EXPERIMENT 2

In the second experiment, we explored whether something akin to choice blindness occurs when people are misinformed about symptoms they previously endorsed. Thus, in Experiment 2, we first asked participants to complete a list of psychiatric symptoms they experienced and then asked them to explain why they had endorsed certain target symptoms that they had not, in actuality, endorsed.

Method

Participants

A total of 28 (23 women, 5 men) undergraduate students, who had not participated in Experiment 1, volunteered to participate in return for course credits. Participants' mean age was 21.7 years (SD = 2.67; range: 18–29). The study was approved by the standing ethical committee of the Faculty of Psychology and Neuroscience of Maastricht University.

Measures and procedure

Participants were tested individually. Students filled out two symptom lists with a time interval of approximately 1 hour between the administrations of each list. The first list was a Dutch version of the Symptom Checklist-90 (SCL-90; Derogatis, Lipman, & Covi, 1973), a 90-item self-report checklist developed as a

screening instrument for general psychiatric distress. The SCL-90 items refer to symptoms that can be grouped into nine subscales (e.g., somatization, obsessive-compulsive symptoms, depression, anxiety). Participants evaluate each item on a 5-point scale ranging from "not at all" (0) to "all the time" (4) to indicate to what extent they experienced the symptoms in the past week. In the current study, we were not interested in participants' total SCL-90 scores or their scores on SCL-90 subscales. Rather, we focused on how they had rated selected target items that were embedded in a series of control items.

In the second phase of the experiment, we asked participants to solve two Sudoku puzzles. The puzzles differed in difficulty, with one being moderately difficult and the other extremely difficult. While the participant was busy solving the Sudoku puzzles, the experimenter manipulated Items 55 (i.e., experiencing concentration difficulties) and 61 (i.e., feeling uncomfortable when people are looking at you) of the already completed SCL-90. More specifically, we increased the scores for these target items by 2 full scale points. For example, when the participant rated the item about concentration difficulties a 0 (i.e., "not at all"), this score was erased and was replaced by a 2 (i.e., "occasionally"). In those rare instances[1] in which participants had scored the critical items with a 3 (i.e., "a lot") or 4 (i.e., "all the time"), the manipulation consisted of decreasing the items by 2 full scale points (i.e., they were recoded as 1 or 2, respectively). During subsequent statistical analyses, these scores were reverse coded.

Following the manipulation, the experimenter showed participants their SCL-90 answer sheets. Participants were asked to explain why they had rated 10 items—8 control items and the 2 manipulated targets—the way they did. For example, in the case of a target item, the experimenter might ask: "Could you please tell me why you responded with *occasionally* to this item?" when in fact the participants had answered "not at all." We tested whether participants would detect such mismatches. The items were evenly distributed over the SCL-90, and the target items were positioned half way in the series, with 1 control item in between the target items. Participants were given approximately 5 minutes to explain their ratings.

After the interview, in the final postmanipulation phase, we provided participants with a short 30-item version of the SCL-90 containing the 8 control and the 2 manipulated target items. Again, participants rated each item on a 5-point scale (anchors 0 = "not at all"; 4 = "all the time"). In this way, we were able to examine whether participants, who were unaware that we manipulated the target items, would revise their scoring in the direction of the manipulation.

Results and discussion

During the interview, 21 participants (75%) accepted the manipulation of the first target item. For the second target, 19 participants (68%) accepted the manipulation—that is, explained why they had scored X when in fact they had scored $X \pm 2$. For example, participants would say that they occasionally or rather often experienced concentration difficulties because they had been drinking a lot of coffee lately or because they were going through a difficult time in life with a lot of exams. When participants did not accept the manipulated score, they would say things like "apparently, I made an error on this one because I rarely have concentration difficulties" or "you must have mixed up my data with those of another participant."

In total, 16 participants (57%) were "blind" to both target manipulations. In our follow-up analyses, we focused on this subsample.[2] After recoding answers to be in the same direction, as described above, scores (range 0–4) were averaged across the 8 control items and the 2 target items of the SCL-90 for the premanipulation and postmanipulation test, separately. Pre- to postmanipulation changes are shown in Figure 2. At the pretest, control and target items did not differ, $t(15) < 1.0$. Furthermore, the pre-to-post change for the control items did not attain significance, $t(15) < 1.0$, whereas the increase for the target items reached borderline significance, $t(15) = 1.41$, $p < .09$; Cohen's $d = 0.34$. Most importantly, at the posttest, target items received higher scores than control items: target mean = 1.56, $SD = 0.66$; control mean = 1.14, $SD = 0.67$; $t(15) = 2.78$, $p < .02$; Cohen's $d = 0.65$.

Experiment 2 showed that a substantial proportion of participants are "blind" with respect to changes in their symptom scores. These participants accepted manipulated

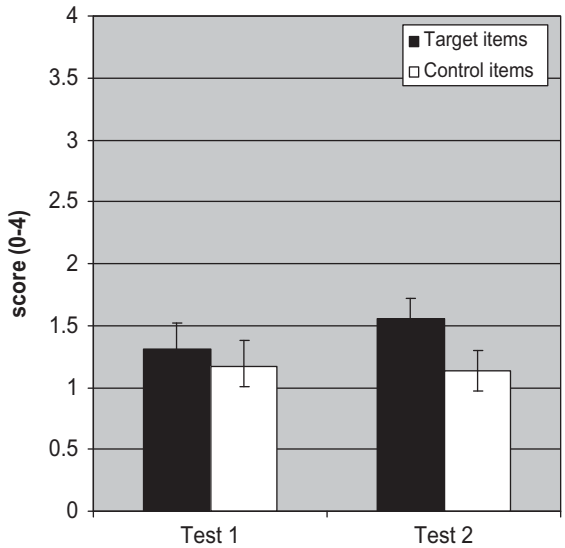

Figure 2. Pre- and postmanipulation scores for control items and target items of the SCL-90 (Symptom Checklist-90) in the "blind" group ($n = 16$).

[1] In total, $28 \times 2 = 56$ target items were manipulated. Of these, 8 (14%) concerned downgrading.

[2] In the "nonblind" subsample, there were no significant pre- to postmanipulation changes, across the control and target items, both $t(11)$s < 1.0.

symptom intensity ratings, not recognizing that they deviated from their original ratings. Additionally, "blind" participants tended to change their symptom intensity ratings in the direction of the manipulation, although this effect was not particularly strong and needs independent replication. In future studies, it would be interesting to examine whether "blind" participants score higher on acquiescence and/or compliance than do "nonblind" participants, although the literature on laboratory-induced false confessions contradicts the view that these traits are straightforward predictors of people's readiness to accept misinformation (e.g., Horselenberg et al., 2006).

The phenomenon of participants being "blind" to symptom manipulations is reminiscent of Loftus's extensive work on creating false memories. Typically, false-memory studies rely on suggestive interview methods, such as providing participants with false information or guiding them through imagination exercises. Recent experiments by Loftus and coworkers demonstrate that researchers can easily make people believe that, as children, they became sick after eating a certain type of food. After this suggestion, participants are less willing to eat that particular food than are controls who did not receive misinformation (Bernstein & Loftus, 2009). Likewise, this research group showed that after implanting a false childhood memory of being mistreated by the character Pluto at an amusement park, participants were less willing to pay for a Pluto souvenir (Berkowitz, Laney, Morris, Garry, & Loftus, 2008).

Our findings indicate that participants' introspective monitoring of symptom intensity is not always accurate. However, people differ in their introspective abilities. A trait that is relevant in this context is self-deceptive enhancement, which refers to an egoistic bias—that is, a tendency to see oneself as a powerful agency (Paulhus, 2002). Self-deceptive enhancement is related to poor insight into symptoms. For example, in a sample of schizophrenic patients, Moore, Cassidy, Carr, and O'Callaghan (1999) found that high scores on self-deceptive enhancement were associated with poor insight into schizophrenic symptoms (r = .40) and their social consequences (r = .40). Moreover, researchers have demonstrated that self-deceptive enhancement is associated with self-enhancing distortions of memory (e.g., Djikic, Peterson, & Zelazo, 2005) and other types of cognitive biases (e.g., hindsight bias; Paulhus, 2002).

EXPERIMENT 3

In Experiment 3, we explored whether one aspect of poor introspective ability—namely, self-deceptive enhancement—is correlated with the residual effects of feigning. We hypothesized that high scores on self-deceptive enhancement—which reflect poor insight into the causes of one's own behavior—will predict greater residual effects of feigning in a test–retest procedure.

Method

Participants

A new sample of 28 female undergraduate psychology students, who received course credit for their participation, participated in Experiment 3. Participants ranged in age from 18 to 43 years with a mean age of 20.7 years (SD = 4.76). The study was approved by the standing ethical committee of the Faculty of Psychology and Neuroscience of Maastricht University.

Measures

Participants were tested individually. They completed Dutch research versions of the SIMS (Merckelbach & Smith, 2003) and the self-deceptive enhancement subscale of the Balanced Inventory of Desirable Responding (SDE-BIDR; Paulhus, 2002).[3]

The SIMS (Test 1, Cronbach's alpha = .92; Test 2, Cronbach's alpha = .72) was administered twice in a test–retest design. We calculated a difference score (SIMS1 – SIMS2) reflecting decreases in SIMS scores when participants had been instructed to retake the inventory responding in an honest fashion (see below). Thus, the lower the value of this difference score, the stronger the residual effects of feigning. The SDE-BIDR (Cronbach's alpha = .80) was used as an index of poor introspective ability. Its items tap narcissistic overinterpretation of the causal role that a person attributes to him- or herself and therefore reflect poor insight into the real causes of one's own behavior (e.g., Djikic et al., 2005). The scale consists of 20 items such as, "I never regret my decisions" and "The reason I vote is because my vote can make a difference." Participants are instructed to indicate to what extent the items are true for them using 7-point scales (anchors: 1 = "not true"; 7 = "very true"). Half the items are keyed positively and half negatively. After recoding the negatively keyed items, responses are summed. Thus, the total score ranges between 20 and 140, with higher scores indicating more self-deceptive tendencies.

Procedure

We first provided participants with a case vignette (see Appendix B) that they were asked to read carefully. The case—a real civil case that we used in previous experimental simulation studies (Merckelbach et al., 2009)—was about a worker who had been a loyal employee for more than 25 years in a factory producing paint coatings. The worker had a conflict with his manager, who wanted to fire him. Participants were told that the worker had decided to phone in sick, complaining about cognitive and emotional problems due to toxic exposure to paint. The participants' task was to identify with the worker and to feign complaints in a credible way.

[3]Participants also completed the SCL-90 (see Experiment 2) before feigning instructions were given, but these data are not discussed here.

Participants were then given the SIMS 1. In the next phase, we asked participants to solve a series of Sudoku puzzles and, after about an hour, told them to disregard both the story that they had read earlier and the instructions we provided. We specifically asked them to relinquish their role as malingerers. We then administered the SDE-BIDR and finally the SIMS 2, this time with the instruction to respond honestly.

Results and discussion

Mean SIMS 1, SIMS 2, and SDE-BIDR scores were 23.4 (SD = 10.2), 4.9 (SD = 3.5), and 80.1 (SD = 13.2), respectively. A paired t test showed that the decrease in SIMS scores was significant, $t(27) = 11.26$, $p < .01$. At Test 2, none of the participants scored above the SIMS cutoff, while at Test 1, 23 participants (75%) had a score exceeding the cutoff. In Experiment 3, SIMS scores at Test 1 were somewhat lower than scores of the feigning–honest group at Test 1 in Experiment 1, $t(43) = 1.61$, $p = .11$. We suspect that this discrepancy is related to civil case vignettes eliciting less intensive feigning behavior than criminal case vignettes, such as the one used in Experiment 1 (see, for similar effects, Boone et al., 2002; Merckelbach et al., 2009).

The Pearson product moment correlation between SDE-BIDR and decrease in SIMS scores was –.38 ($p < .05$), indicating that the higher participants scored on the SDE-BIDR, the less they revised their responses on the SIMS. Thus, we found that the residual effects of feigning were greater in individuals scoring high on self-deceptive enhancement—that is, individuals who have poor introspective abilities.

GENERAL DISCUSSION

The *DSM–IV–TR* assumes that feigned and somatoform complaints are mutually exclusive categories. Although our findings are based on laboratory studies involving nonclinical samples, they do suggest that such definitive categorical demarcation needs to be reconsidered. More specifically, our data show that feigning produces a residual effect, such that former malingerers continue to endorse more symptoms when asked to respond honestly than do nonmalingering control participants. Furthermore, we obtained tentative evidence for a role of self-deceptive enhancement in producing this effect: Participants who tended to overvalue their own causal role exhibited more pronounced residual effects of feigning. We also determined that people could be easily misled about the intensity of their symptoms. Our findings suggest that the phenomenon of choice blindness, described by Johansson et al. (2005) in the domain of preferences (e.g., for faces or products), may also extend to symptom endorsement. Thus, people with poor introspective knowledge about the origins of their symptoms may gradually forget that their symptom reports were initially dependent on a deliberate choice to exaggerate.

Admittedly, there are other ways to conceptualize our findings. For example, the effects obtained in Experiment 2 might be interpreted in terms of misinformation and its potential to distort memory (e.g., Bernstein & Loftus, 2009). Also, the residual effects of feigning described in Experiments 1 and 3 may reflect so-called anchoring—that is, the phenomenon that people's estimates on a first task affect their estimates on a second, unrelated task (e.g., Jacowitz & Kahneman, 1995).

In more general terms, the studies described above support the critical analysis of authors such as Boone (2007a, 2007b) and Hamilton et al. (2008), who commented on *DSM*'s use of the intentionality criterion to define feigning. Our findings hint at why this criterion might be problematic: Feigning may evolve into a less conscious form of symptom reporting. Our studies underline Boone's (2007a; p. 676) conclusion that "it is now appreciated that conscious and non-conscious creation of symptoms may lie on one or more continua, rather than falling into discrete categories." Kopelman (2000) has advanced a similar argument in the case of retrograde amnesia. He argues that concepts like organic amnesia, hysterical amnesia, and feigned amnesia define the extreme endpoints of a continuum, and over time patients may move along this continuum.

Several limitations of the current studies merit attention. To begin with, our studies relied on nonclinical samples that were relatively small. Also, women were overrepresented, and this preponderance of one gender over another varied across studies. Moreover, we did not conduct exit interviews to probe whether participants had intentionally endorsed symptoms at retest. Accordingly, replication studies preferably should include exit interviews and rely on more heterogeneous samples to test the robustness of residual effects of feigning. For example, it would be interesting to examine whether individuals who are trained to role-play patient scenarios with medical students (i.e., simulated patients) exhibit residual effects of feigning. Consistent with this possibility, simulated patients experience an increase in symptoms after they have been required to play a difficult role, a phenomenon interpreted in terms of the stress associated with enacting the patient role with students (e.g., Bokken, van Dalen, & Rethans, 2004). However, another possibility is that "blindness" for the intentional aspects of symptom endorsement contributes to the increase in symptom levels (see also Wallace, Rao, & Haslam, 2002).

Various authors have pointed out that feigning can occur differentially across the domains of psychopathology, neurocognitive deficits, and medical complaints (e.g., Heilbronner et al., 2009). Studies attempting to replicate the residual effects of feigning in each of these domains would be informative, as would replication attempts in clinical groups characterized by a lack of self-knowledge or insight (e.g., personality disordered patients; Wilson, 2009). However, there probably are lower bound limits to the extent to which lack of self-knowledge fuels residual effects of feigning. One such lower bound limit might be poor awareness of deficits inherent to anosognosia. Patients with anosognosia (e.g., as found in Alzheimer disease; see. e.g., Starkstein, Jorge, Mizrahi, & Robinson, 2006) underreport their

symptoms, and residual effect of feigning would be very unlikely in this group.

Another limitation of our studies was that we did not examine the time course of the residual effects of feigning. That is, we administered pre- and posttests during one session. Accordingly, the effects that we describe are potentially brief, short-term phenomena. Future studies should assess the stability of these effects to determine whether feigning has long-term as well as short-term consequences. Although they are in the domain of autobiographical memory, the Polage (2004) study and the simulation studies conducted by Christianson and Bylin (1999) and Van Oorsouw and Merckelbach (2004) suggest that fabrication may produce a long-term self-fulfilling prophecy.

Our research identified clear-cut residual effects of malingering on subjective symptom reporting. Whether performance-based measures of response bias, such as forced-choice memory tasks, are also sensitive to residual effects of deliberate faking is unknown and requires investigation. If such effects do exist, it is a priori unlikely that they will take the form of below-chance performance because, in the majority of cases, this type of performance reflects intentional avoidance of the correct alternative in a forced-choice task.

Experiment 2 suggests that providing people with the opportunity to describe and attribute their symptoms might contribute to "disease conviction" (Delis & Wetter, 2007). Indeed, when people are required to explain hypothetical events, their confidence that these events actually happened increases (e.g., Sharman, Manning, & Garry, 2005). This so-called "explain-this" effect may have clinical relevance because it implies that inviting patients with noncredible symptoms (Boone, 2007b) or excessive illness behavior (Hamilton et al., 2008) to describe and redescribe their symptoms on different occasions might be counterproductive. Germane to this issue are also the well-documented expectancy effects that occur when people define themselves as belonging to a diagnostic group that is known to perform poorly (i.e., "diagnosis threat"; see Suhr & Gunstad, 2002).

In closing, our research provides a clear demonstration of the residual effects of feigning symptoms: Individuals who are first instructed to feign symptoms will continue to endorse symptoms, even when they are later provided with an opportunity to respond in an honest fashion. Notably, poor introspective ability may underlie such residual effects, a finding with obvious and important clinical ramifications.

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APPENDIX A

SCENARIO AND INSTRUCTIONS USED IN EXPERIMENT 1

Please read the following case vignette carefully and try to imagine that this story is about you.

Imagine that one day you bet with your friend on whether you dare to climb to the top of a medieval tower that is forbidden territory for the public. The two of you agree that when you reach the top floor, you will put your arm through one of the small windows and you will wave your arm. If you succeed, you win the bet. So, you enter the building and start climbing the many stairs as fast as you can. It doesn't take long for you to reach the top; but then you realize that all the window shutters have been closed. You try to open one of the old shutters. It takes a lot of effort, but it moves when you push with your shoulder. You put your arm through the top window. At that moment it happens: the old shutter falls down together with some big parts of the stony window ledge. One of the stones hits a girl who happens to pass by. You run downstairs as fast as you can. When you arrive at the scene, you fully realize what a tragedy this is. The girl is lying at the ground and is bleeding heavily from her head. Within minutes, the ambulance arrives. The ambulance workers try to reanimate the girl, but without success. The police arrive, arrest you, and bring you to the police station where you are interrogated. The police accuse you of reckless behavior and manslaughter. They tell you that a forensics psychiatrist will examine you. As part of this examination, you are required to undergo psychological testing.

Instruction feigning–honest group (Test 1)

You have decided to fake symptoms of a serious psychological problem so as to minimize your responsibility for what happened. You are about to take a test that would be used in such a situation. I would like you to simulate psychological problems, but in a believable way, such that your examiner cannot tell that you are attempting to fake.

Instructions feigning–honest group (Test 2)

An analysis of the questionnaire that you completed about an hour ago shows that you have been exaggerating your symptoms and experiences. Please give up your role as a malingerer. We want you to answer the items again, but this time in an honest way.

Instructions honest–honest group (Test 1)

You have decided to be fully cooperative and to respond honestly. You are about to take a test that would be used in such a situation. I would like you to respond honestly to the test items.

Instructions honest–honest group (Test 2)

Sometimes, people change their minds about whether or not they have certain symptoms and experiences. Please answer the items again. We want you to complete the test once more in an honest way.

APPENDIX B

SCENARIO AND INSTRUCTIONS USED IN EXPERIMENT 3

Imagine yourself in this situation. Assume that you are the person in this story and that the scenario has actually happened to you.

For 25 years, you have been working in a factory producing paint coatings. All these years, you worked hard, contributing as much as you could to the success of the factory. One day, you are called to the office of the boss. There is this new top manager in the factory and as soon as you are in his office, he begins to criticize you. You are in trouble because some days ago, you took a few older cans of paint for private use with you home without asking permission. You do not understand the whole argument: everyone who works in the factory takes the

older paint (that would not be sold anyway) when they need some. This happened for years and there never was any discussion about it. Nevertheless, the new manager accuses you of having violated integrity rules. The discussion between you and the manager escalates and you run furiously away from the office. The following day, you phone in sick because you have enough of this. Also: you are afraid that the new manager will fire you, but he can't do that as long as you are sick. When the company physician visits you to determine whether your health problems are serious enough for you to stay home, you claim that years of exposure to paint have produced numerous complaints.

Instruction (Test 1)

You have decided to fake symptoms so as to appear seriously ill. You are about to take a test that would be used in such a situation. I would like you to simulate problems, but in a believable way, such that your examiner cannot tell that you are attempting to fake.

Instruction (Test 2)

Disregard the story and the instructions that you have been given earlier. Give up your role as a malingerer. Please answer the items again. This time, we really want you to complete the test in an honest way.