Malingering on the RAVLT  
Part I. Deterrence strategies

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Abstract

The effect of warning regarding detection of malingering on the Rey Auditory Verbal Learning Test (RAVLT) was examined in this study. Sixty undergraduate students were randomly assigned to one of four conditions: malingerers, malingerers-with-warnings, warning-only, and control. An incentive that appeared differential, but was an actual constant reward, was offered to participants who could fake in a believable manner (for those in malingering conditions), or to those who performed to the best of their ability (non-malingering conditions). It was predicted that warning participants about the possibility that faking could be detected would modify the behaviour of malingerers, but not those instructed to perform to the best of their ability. Warning had no effect on behaviour in either condition, which was consistent with expectations for the warning-only group, but not for the malingering group. Results are discussed in terms of the ethical and legal issues associated with malingering in neuropsychological practice. © 2001 National Academy of Neuropsychology. Published by Elsevier Science Ltd.

Keywords: Malingering; Neuropsychology; Warning; RAVLT

1. Introduction

The American Psychiatric Association (1994) defines malingering as intentional production of negative physical or psychological symptoms. Malingering is differentiated from factitious, conversion, and somatoform disorders by the presence of an external incentive.
The American Psychiatric Association lists the following examples of external incentives, avoidance of work or military duty, evading criminal prosecution, or the attainment of financial compensation. This definition suggests individuals seeking to avoid undesirable outcomes or gain beneficial outcomes may be motivated to exaggerate or fabricate deficits, including cognitive impairment.

Much of the research on malingering in neuropsychology has focussed on methods of detecting malingering (see Haines & Norris, 1995; Nies & Sweet, 1994; Rogers, Harrell, & Liff, 1993, for reviews). For example, there have been numerous investigations attempting to determine effective ways of detecting malingering, or seeking to identify the strategies that malingerers use when attempting to avoid detection (e.g., Haines & Norris, 1995). However, there has been much less research on factors that might mediate or reduce malingering behaviour. Of those studies that have explored factors that may reduce malingering behaviour, the main variable of interest has been the effect of warnings on malingering (e.g., Johnson & Lesniak-Karpiak, 1997). The importance of furthering our understanding of this issue is clear, given that in clinical settings neuropsychologists may be ethically obliged to obtain full and informed consent from clients, including acknowledgment that methods of detecting malingering may be employed during assessment (Johnson & Lesniak-Karpiak, 1997).

Research in the area of malingering has typically involved the use of volunteers asked to simulate abnormal performance on psychological tests (Nies & Sweet, 1994). However, a number of criticisms have been made of studies using analogue designs that need to be understood in order to evaluate research in this area (e.g., Haines & Norris, 1995). Perhaps most importantly, simulation studies have been criticised for their lack of generalisability (Haines & Norris, 1995; Rogers & Cruise, 1998). This criticism has been attributed to motivational differences between study participants and clients seeking financial reward through litigation, and also because the strategies used in studies investigating malingering may not parallel those used in clinical practice (Bourg, Connor, & Landis, 1995). The American Psychiatric Association’s definition of malingering highlights the need to incorporate a motivational element in malingering-simulation research, to replicate external incentives perceived by the clinical population (Binder & Pankratz, 1987; Nies & Sweet, 1994). In recognition of this, Nies and Sweet (1994) have recommended the inclusion of incentives in malingering research. They also recommend informing malingerers of a reward for faking credibly to provide an appropriate model of the clinical situation in which malingering is most likely to occur.

Second, malingering-simulation research has been criticised on the grounds that the methods used to induce simulation may not have provided adequate information about symptoms of the group being simulated to ensure realistic faking (Nies & Sweet, 1994). That is, simulation studies require that participants know how to “fake-bad”. However, it should be noted that the extent to which knowledge of symptoms needs to be induced might depend on the type of symptoms being simulated. For example, responses from almost 100 untrained examinees asked to endorse symptoms associated with depression using self-report questionnaires satisfied diagnostic criteria for this illness, compared to 63.3% of the sample endorsing symptoms of mild brain injury and meeting relevant diagnostic criteria (Lees-Haley & Dunn, 1994). This suggests that affective disorders might be easier for naïve subjects to simulate than cognitive deficits. Inducement in malingering studies is usually achieved by
coaching participants (showing them how to fake-bad; e.g., Johnson & Lesniak-Karpiak, 1997) or using people with direct knowledge of specific syndromes, such as nurses or psychologists (e.g., Franzen & Martin, 1996; Hayward, Hall, Hunt, & Zubrick, 1987). When non-experts are used, some form of coaching of possible cognitive deficits is important as indicated previously, given that lay people have been shown to have little understanding of memory and other cognitive symptoms associated with traumas such as minor head (Aubrey, Dobbs, & Rule, 1989).

A variety of methods have been used to coach naïve participants ranging from providing instructions on how to simulate, to the use of vignettes as a means of inducing simulation. As noted previously, when instructions to simulate have been used, these have been criticised as too vague and not providing specific information regarding symptoms (Nies & Sweet, 1994). Similar criticisms have been levelled at the use of simple vignettes (Nies & Sweet, 1994). Failure to provide adequate coaching is a serious issue for malingering-simulation research, as it reduces the likelihood that participants have sufficient knowledge to simulate malingering credibly. In addition, studies that fail to induce credible simulation are at risk of producing results that do not readily generalise to target populations. Clearly, there is a need to devise coaching strategies that induce credible simulation, given that this type of faking is likely to be more difficult to detect, and may more closely reflect the behaviour of malingers who probably also attempt to educate themselves regarding how to feign deficits or elude detection (Berry, Lamb, Wetter, Baer, & Widiger, 1994; Cochrane, Baker, & Meudell, 1998).

Despite the limitations of malingering-simulation research, analogue studies are important for two reasons. First, studies using samples of actual malingerers are extremely difficult to conduct, necessitating the use of analogue designs (Franzen & Iverson, 1997). Second, simulation studies have been important in furthering our understanding of malingering, particularly methods of detecting malingering, although there is clearly a need to expand the literature on the effect of warnings on malingering.

There has been one published paper on the effect of warnings on malingering by Johnson and Lesniak-Karpiak (1997), which suggested that warnings may be effective in reducing malingering behaviour. This result was based on a simulation design however, and it is important to consider whether common limitations associated with malingering-simulation research were adequately addressed in this study, especially since this result has yet to be replicated.

Johnson and Lesniak-Karpiak (1997) examined the effect of warnings regarding detection on malingering behaviour in a sample of 87 undergraduate students. Subjects were randomly assigned to one of three groups (simulators-without-warning, simulators-with-warning, and control), and vignettes were used to induce simulation behaviour. Students in simulation conditions were asked to simulate the performance of a head-injured patient, encouraged to “fake-bad” in a realistic manner, and instructed to imagine they were motivated by the possibility of increased compensation although no actual incentive was offered. Results showed that the performance of simulators-with-warning frequently approximated that of controls, and was significantly better than the performance of simulators-without-warning on some measures. That is, the pattern of results was generally consistent with expectations that warnings would modify the behaviour of simulators.
There were some exceptions to the pattern of results found by Johnson and Lesniak-Karpiak (1997) that were not consistent with expectations however. For example, Johnson and Lesniak-Karpiak examined the effect of warnings on performance on memory and motor tasks, using the Wechsler Memory Scale-Revised (WMS-R) and Grooved Pegboard, respectively. Differences between simulators with- and without-warning were found on selected WMS-R index scores (verbal, general, and delayed memory indices), but not visual memory or attention/concentration index scores. On motor tasks, Johnson and Lesniak-Karpiak found that although malingerers-without-warning were significantly different from controls, there was no significant difference between malingerers-with- or -without-warning, or malingers-with-warning, and controls.

The results of Johnson and Lesniak-Karpiak (1997) are interesting, and suggest that warnings may have a deterrent effect against malingering, at least on some tasks. However, the pattern of results does not provide unequivocal support for the notion that warnings reduce malingering behaviour.

In addition to findings from research, it is important to point out that there may be theoretical reasons for expecting that warnings may have a deterrent effect on malingering. For example, operant conditioning and deterrence theories may help to explain the mechanism by which warnings could have a deterrent effect. According to these theories attempts to avoid detection, or other negative consequences associated with such behavior, might result in behaviour change. These theories are the basis of current random-breath-testing models in Australia, which utilise the threat of detection as a means of modifying drunk-driving behaviour (Homel, 1993a, 1993b). While there are obviously differences between the potential costs and benefits of malingering and drunk-driving, these theories may provide an account of the possible mechanisms by which warnings could have a deterrent effect on malingering behaviour.

Finally, if warnings are to become part of routine clinical practice, at least in medico-legal settings (Johnson & Lesniak-Karpiak, 1997), despite recent opposition to this proposition (Youngjohn, Lees-Haley, & Binder, 1999), it is important to demonstrate that warnings do not alter the behaviour of clients attempting to perform at their best. This has yet to be investigated.

1.1. The present study

It is evident from this review of the literature that the effect of warnings on the behaviour of malingerers is not well understood, nor is the extent to which such warnings might influence the behaviour of non-malingerers. In addition, this review of the literature has highlighted several important limitations of previous research in this area. Most obviously, there are important methodological issues in relation to coaching of participants, motivation, and the generalisability of results. Taking these issues into consideration the aim of the present study was to expand our understanding of the effect of warnings about detection on the behaviour of malingerers and non-malingers, by replicating and extending the work of Johnson and Lesniak-Karpiak (1997).

Specifically, the present study aimed to expand on the work of Johnson and Lesniak-Karpiak (1997) in two ways. First, to determine whether non-malingerers are affected by
warnings, a warning-only group was added to the experimental design. Second, a more powerful research design was used in this study (repeated measures), and attempts were made to overcome some of the other well-documented limitations of simulation research, such as the use of a more comprehensive coaching strategy to induce credible malingering and the inclusion of an actual incentive in the research design.

Several hypotheses were proposed for this study. First, based on the results of Johnson and Lesniak-Karpiak (1997), it was expected that the malingering-with-warning group would perform better on the Rey Auditory Verbal Learning Test (RAVLT) than the malingering group. Second, it was expected that the warning-only group would perform similar to the control group. This result would suggest that warnings about detection have no effect when subjects are not attempting to malinger.

2. Method

2.1. Participants

Participants were recruited from the School of Psychology and Counselling undergraduate subject pool and the Law Faculty, at Queensland University of Technology. Sixty-one volunteers participated in this study. An a priori power analysis was conducted to determine sample size. Effect size estimates were based on results reported by Johnson and Lesniak-Karpiak (1997). The results of this analysis showed that with an effect size of .50, a sample size of 56 was needed to obtain power of 0.87 \( F(3,52) = 2.78 \).

Sample characteristics are outlined in Table 1. Ages ranged from 17 to 54. To determine group equivalence on variables that may affect performance on memory tasks, a one-way ANOVA was conducted on age by group, however no significant age differences were found between groups \( F(3,56) = 1.006, P > .05 \).

2.2. Design

To examine the effect of warnings on malingering a mixed two-way repeated measures ANOVA with one between-groups variable (group) and one within-groups variable (occasion) was used. There were four levels of the between-groups independent variable (malingers, malingers-with-warning, warning-only, and control), and two levels of the

<table>
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<tr>
<th>Table 1</th>
<th>Demographic characteristics of sample</th>
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<tr>
<td></td>
<td>Males (30%)</td>
</tr>
<tr>
<td>Gender</td>
<td>18 (30%)</td>
</tr>
<tr>
<td>Age</td>
<td>( M = 24.94 ), S.D. = 7.85</td>
</tr>
<tr>
<td>Faculty</td>
<td>Law (61.1%)</td>
</tr>
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<td></td>
<td>Psychology</td>
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within-groups independent variable (Occasions 1 and 2). The dependent variable for this analysis was the total number of words correctly recalled across immediate recall trials on the RAVLT.

2.3. Materials

The RAVLT is a commonly used clinical measure of verbal learning and memory (Spreen & Strauss, 1998; Sullivan & Bowden, 1997). The RAVLT provides a measure of verbal memory function, including immediate memory span, new learning, susceptibility to interference, and recognition memory, and is widely recognised as a valid measure of memory (Rosenberg, Ryan, & Prifitera, 1984). Further, the RAVLT has been used to investigate simulated memory impairment in a number of studies previously (e.g., Bernard, 1991; Suhr, Tranel, Wefel, & Barrash, 1997), and has been described as having “some utility for detecting feigned memory impairment” (King, Gfeller, & Davis, 1998, p. 611).

The RAVLT consists of a 15-item free-recall test, an interference list recall task, a delayed recall test, and a recognition task. The free-recall test required participants to recall the 15-items in any order immediately after they were presented verbally. Five trials of the recall task were conducted in which the order of words remains fixed and the instructions were repeated for each trial.

This recall task was followed by the presentation of a 15-item interference list to be immediately recalled by the subject. Subjects were then required to recall the original list without further presentation of the original list words. Finally, subjects were required to read a list of words and distracters and identify original list words; however, recognition trial data will be discussed elsewhere.

Scores on the RAVLT were calculated using standard criteria. That is, the number of words correctly recalled was summed across five immediate recall trials. RAVLT total score data was used in all analyses as this information is considered to be the most reliable index of memory that can be derived from this test (Spreen & Strauss, 1998).

2.4. Procedure

The experimental design consisted of three stages, pre-intervention, intervention, and post-intervention. Two examiners were involved in the testing of each subject. One examiner facilitated the pre-intervention and intervention stages and the other examiner administered the RAVLT during the post-intervention stage. The use of a second examiner during the post-intervention stage of this study was important for two reasons. First, to minimise experimenter bias given that group assignment was unknown to the examiner conducting the repeat administration of the RAVLT; and second, to facilitate malingering in faking conditions, given that subjects may have been less likely to implement faking strategies with an experimenter who had already witnessed their best effort on the RAVLT.

Two equivalent forms of the RAVLT were administered during pre- and post-intervention stages, respectively (Spreen & Strauss, 1998). To ensure equivalence of forms in this sample,
scores on alternate forms at time one (pre-intervention) were compared. No significant differences were found between forms ($F(1,58) = 0.895, P > .05$), therefore data was collapsed across forms for all subsequent analyses.

The pre-intervention stage consisted of obtaining informed consent, followed by the administration of the RAVLT, and completion of a demographics questionnaire. At the intervention stage, participants were randomly allocated to groups by handing participants a sealed envelope containing instructions specific to one of the four conditions (malingers, malingers-with-warning, warning-only, and control). Each group received a standardised set of step-by-step instructions. All four groups watched a video of memory deficits experienced by some people as a consequence of brain injury. That is, participants viewed a 15-min segment of a public education video obtained from the Brain Injury Association of Queensland. The use of video in this study was intended to facilitate coaching of participants in malingering conditions when combined with instructions emphasising the purpose of showing the video, but not have an effect in non-malingering conditions where no such instructions were provided. To further illustrate this point, Table 2 shows excerpts from the instructions given to groups in relation to the video.

It is important to reiterate that, the only difference between the groups was the instructions they received regarding the purpose of the video and how to perform on the repeat administration of the RAVLT that, in the case of malingers, included possible clues from a brief vignette. All groups were informed of a reward for performing to the best of their ability, or in a credible (undetectable) manner, depending on group allocation (see Table 2).

Finally, at the post-intervention stage participants were required to complete an alternate form of the RAVLT, followed by the Intervention Effectiveness Appraisal (IEA) Questionnaire, and were then debriefed. The IEA was based on the questionnaire used by Johnson and Lesniak-Karpiak (1997). The purpose of the IEA was to assess

<table>
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<tr>
<th>Table 2</th>
<th>Excerpts from instructions given to controls and malingers-with-warning demonstrating how the video was introduced, the way incentives were used, and how warnings were issued</th>
</tr>
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<tbody>
<tr>
<td>Group</td>
<td>Video</td>
</tr>
<tr>
<td>Control</td>
<td>You will be asked to watch a short video that will be shown to you once you have read these instructions.</td>
</tr>
<tr>
<td>Malingering-with-warning</td>
<td>As above plus: While viewing the video you should note the effects brain damage has on memory. You will be required to attempt to simulate these difficulties when you are given the memory test.</td>
</tr>
<tr>
<td></td>
<td>We are offering a reward for those who can perform to the best of their ability.</td>
</tr>
<tr>
<td></td>
<td>During the administration of the memory test, you are required to respond in a manner that reflects the difficulties you saw on the video and have read about. However, you should attempt to do this in a way that appears believable and realistic. Plus above.</td>
</tr>
<tr>
<td></td>
<td>Efforts to mangle (fake or exaggerate difficulties) will be detected through in-built methods within the RAVLT.</td>
</tr>
<tr>
<td></td>
<td>N/A</td>
</tr>
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participants’ understanding of the instructions and perceived ability to follow them. In addition, the IEA was included in this study to comply with recommendations for malingering-simulation research made by reviewers of the literature in this area (e.g., Nies & Sweet, 1994).

3. Results

The data file was screened prior to running statistical tests to check for violations of assumptions and outliers following procedures recommended by Tabachnick and Fidell (1996). There was evidence of slight skewness and kurtosis in time two (post-intervention) RAVLT data. However, the decision to transform the data was rejected on the basis that time one RAVLT scores were comparable to normative data (see Spreen & Strauss, 1998), therefore, the change in the distribution was interpreted as an effect of the intervention.

To determine whether participants in this study were representative of the general population, mean RAVLT scores for each age group in this study were compared to current normative standards (Spreen & Strauss, 1998). Except for one age group (males aged between 30 and 39), means scores for participants in this study were within one standard deviation (S.D.) of current normative standards.

3.1. Exclusion criteria

Subjects were excluded from this study if they had experienced brain injury, concussion, and amnesia or memory loss, or if they were unable to understand the instructions provided, as measured by the IEA. Using these criteria, data from one subject was excluded from analysis on the basis of past medical history, leaving a total sample size of 60 and 15 participants per group.

Descriptive statistics for total scores on both testing Occasion 1 (pre-intervention) and testing Occasion 2 (post-intervention) are presented in Table 3. Table 3 shows that mean scores did not dramatically change for non-malingering groups across testing occasions, whereas malingerers performed worse post-intervention than at initial testing.

<table>
<thead>
<tr>
<th>Group</th>
<th>Total score on Occasion 1</th>
<th>Total score on Occasion 2</th>
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<tbody>
<tr>
<td></td>
<td>$M$</td>
<td>S.D.</td>
</tr>
<tr>
<td>Control</td>
<td>57.45</td>
<td>7.62</td>
</tr>
<tr>
<td>Warning-only</td>
<td>52.00</td>
<td>8.43</td>
</tr>
<tr>
<td>Malingerers</td>
<td>53.67</td>
<td>7.51</td>
</tr>
<tr>
<td>Malingerers-with-warning</td>
<td>53.27</td>
<td>8.24</td>
</tr>
</tbody>
</table>
3.2. Analysis of the effect of warnings

A two-way mixed repeated measure ANOVA was conducted to analyse the effect of warnings on malingering. The observed power for this test was 1.00. There was a significant main effect for occasion \([F(1,56) = 106.53, P < .05, \text{effect size .65}]\), group \([F(3,56) = 12.90, P < .05, \text{effect size .41}]\), and the interaction between occasion and group \([F(3,56) = 27.18, P \leq .05, \text{effect size .59}]\) (see Fig. 1).

The significant interaction effect was further explored by calculating the difference between testing Occasion 1 and testing Occasion 2 and conducting a one-way ANOVA on difference scores. For this analysis, the independent variable was group with four levels, and the dependent variable was the change in total RAVLT recall scores between Occasions 1 and 2. Results indicated the change in total score on testing Occasions 1 and 2 was significantly different \([F(3,56) = 27.69, P \leq .05]\). Given that there were equal numbers of participants in each group, Tukey HSD post-hoc comparisons were used to adjust for experiment-wise error. There were no significant differences between malingerers-with-warning and malingerers, or controls and those in the warning-only group. However, differences across these pairs were significant. That is, on average malingerers-with-warning and malingerers recalled 22 fewer words at time two compared to time one, whereas subjects in control- and warning-only conditions either lost far fewer words or improved their recall at time two (mean change scores and S.D. for control group and warning-only group, respectively: \(M = 3.00, \text{S.D.} = 7.46\) and \(M = -6.67, \text{S.D.} = 7.17\)).

![Mean RAVLT immediate recall scores on testing Occasions 1 and 2 across groups. Note that higher scores indicate better performance.](image)
4. Discussion

The aim of the current study was to further investigate the deterrent effect of warnings on behaviour in malingering and non-malingering conditions. In a more general sense, this study was intended to expand the work of Johnson and Lesniak-Karpiak’s (1997), and address some of limitations identified in previous malingering-simulation studies by including an actual incentive, using a more comprehensive approach to coaching malingers, and using a stronger research design.

To achieve this aim, two hypotheses were proposed. First, the hypothesis that the malingerers-with-warning group would perform better on the RAVLT than malingerers (without warning) was not supported. That is, it was hypothesised that warning regarding the possibility of detection would reduce malingering behaviour. The results of this study showed that malingerers-with-warning did not perform significantly better than malingerers, suggesting that warnings about detection do not reduce malingering behaviour. This result is unlikely to be due to the failure to induce simulation adequately, given that both malingering groups performed worse than the control group. In addition, responses to the IEA showed that malingering participants generally felt confident of their ability to mangle, suggesting that coaching strategies were perceived as effective.

The finding that warning regarding detection did not reduce malingering behaviour is inconsistent with Johnson and Lesniak-Karpiak’s (1997) results for verbal memory tasks. For example, Johnson and Lesniak-Karpiak showed that malingerers-with-warning performed at a level that was significantly different to controls, as well as malingerers-without-warning on the verbal memory index of the WMS-R. To illustrate this point more clearly, Fig. 2 depicts the pattern of results from this study, and findings for Johnson and Lesniak-Karpiak on motor and memory tasks. Fig. 2 shows the results for memory test data (unfilled circles), as well as the data from motor tasks (filled circles). Looking at the pattern of relationships shown in Fig. 2 for this study (above group names), it is clear that although there were no significant differences between malingerers and malingerers-with-warning, or those in warning-only and control groups, the difference between those in malingering and non-malingering conditions was significant. However, this pattern of results is different to that found by Johnson and Lesniak-Karpiak on both memory and motor tasks.

There are several reasons that could account for the discrepancy between the results from this study and the study by Johnson and Lesniak-Karpiak (1997). First, the discrepancy in results could be attributed to different methods of coaching used in these studies. For example, although Johnson and Lesniak-Karpiak used vignettes to induce simulation, the present study used a more comprehensive approach to induce malingering (adding a video to the vignette). If subjects in this sample were encouraged to malinger more effectively and as a consequence had more faking strategies at their disposal, when warned of the possibility of being detected they may have modified only some aspects of their faking behaviour, with the net effect of an overall reduction in the impact of warning.

Second, differences in the results of this study and that of Johnson and Lesniak-Karpiak (1997) may be due to the performance measures that were used. For example, Johnson and Lesniak-Karpiak used the WMS-R and found differences between simulators and simulators-with-warning on selected WMS-R index scores (verbal, general, and delayed memory
indices), whereas this study did not find significant differences between these groups using RAVLT immediate recall scores. The failure to detect differences between these groups on the RAVLT may be due to the differences in the psychometric properties of these tests, or the perceived sophistication of the WMS-R relative to the RAVLT. For example, differences

Fig. 2. Schematic representation of results from malingering-simulation studies investigating the effect of warnings on behaviour. The set of relationships above experimental groups listed in the figure (e.g., malingerers-without-warning) represents the results from the present study. The set of relationships below the groups represents results from Johnson and Lesniak-Karpiak (1997). Two sets of results are shown from Johnson and Lesniak-Karpiak: lines connected by filled circles represent results for motor tasks, lines connected by unfilled circles represent results found on selected WMS-R index scores. Circles including equals signs indicate that no significant differences were found between groups connected by lines, whereas circles including the not-equal symbol indicate that groups were significantly different.
between malingerers-with- and -without-warning apparent on WMS-R composite scores but not the RAVLT might be due to increased opportunity for malingerers to modify their behaviour following warning on a multi-subtest measure. Alternately, if the WMS-R was perceived as a more complex test, the effect of warning malingerers may be more potent. That is, subjects may feel the parameters of a complex task are less obvious; therefore the probability of being detected may be perceived as greater if they fail to heed the warning and do not modify their behaviour.

Finally, and perhaps most importantly, the discrepancy in results could be due to the nature of the warnings issued, and the inclusion of an incentive in this study but not in that of Johnson and Lesniak-Karpiak (1997). However, based on operant and deterrence theories, including an incentive should have provided a stronger test of the hypothesis that warnings would reduce malingering (Homel, 1993a, 1993b). For example, those in the malingerer-with-warning condition, not only received a warning that they may be detected, but were also informed that they would be rewarded for malingering credibly. The failure to find significant effects, under optimal conditions, underscores the need for further research in this area.

Interestingly, the results of this study were consistent with some of Johnson and Lesniak-Karpiak’s (1997) findings for performances on motor tasks (see Fig. 2). That is, although Grooved Pegboard scores for malingerers-with-warning were not significantly different from controls, there was no significant difference between malingerers-with-warning and malingerers-without-warning. This suggests that the warning on motor task performance may have had a weak effect relative to memory tasks, and that it did not as clearly modify the behaviour of simulators in Johnson and Lesniak-Karpiak’s study. Based on the results of the present study, combined with those of Johnson and Lesniak-Karpiak (which indicated a null effect of warning on motor tasks in malingering conditions, and the absence of a consistent effect on memory tasks) it seems reasonable to suggest that the weight of evidence does not currently support the efficacy of warnings. However, further research is needed to clarify the effect of warnings on the RAVLT, the WMS-R (or WMS-III), and possibly other cognitive tasks. In addition, the effect of warnings on genuine malingerers (non-simulators), remains unknown, and future studies could look at exploring the deterrent effect of warnings in a sample of known malingerers or at least in high-base rate groups (King et al., 1998). Testing the limits of generalisation of the results of the present study is particularly important given concerns about the extent to which samples of student simulators can mimic malingering behaviour noted previously, as well as concerns about the extent to which simulators can mimic what may be a complex interaction between malingering behaviour and other post-injury variables that may impact on memory, such as medication (Suhr et al., 1997). Finally, to explore the possibility that warning malingerers about detection may produce more sophisticated malingering (Youngjohn et al., 1999), future studies need to incorporate a method of assessing the strategies that malingerers in warning and non-warning conditions use.

The second hypothesis proposed for this study, that the warning-only group would to perform similar to the control group was supported. This result indicates that warnings about detection have no effect when subjects are not attempting to malinger. At a practical level, this suggests that practitioners can include warnings without confounding assessment results. This is important, given that issuing warnings about the possible use of detection strategies
may be an important part of gaining informed consent for neuropsychological assessment (Johnson & Lesniak-Karpiak, 1997).

As mentioned previously, both operant conditioning and deterrence theories were cited as possible explanations for the anticipated deterrent effect of warnings. These theories emphasise changes in behaviour resulting from attempts to avoid detection. However, it should be noted that the warning incorporated in this study did not include clear information about possible consequences of detection, although the possibility of not receiving the reward was implied. Thus the lack of support for the deterrent effect of warnings on malingering in this study may be due to the failure to provide clear information about the negative consequences of being detected. Future studies may need to include information about the consequences of being detected to adhere more closely to the principle of deterrence. Further, the inclusion of information about the consequences of being detected may improve the generalisability of simulation studies by more closely approximating the cost–benefit analysis that malingerers are thought to undertake when deciding whether or not to fake-bad (Horry & Shores, 1999). Obviously, it will also be important to determine whether warnings with a clear statement of consequences modify the behaviour of non-malingerers.

In the meantime, the use of warnings as part of assessments in medico-legal settings may serve to highlight the consequences of being detected, at least indirectly. Although, Johnson and Lesniak-Karpiak (1997) suggest that providing warnings to clients can lead to opportunities to address the issue of simulation directly, the use of an explicit warning with a statement of consequences, would remove the uncertainty of this process, if this can be shown to have no adverse effects on non-malingerers.

Finally, the provision of warnings to clients as a routine part of forensic neuropsychological assessments may better serve clients and the profession, if used in conjunction with standard detection methods (Johnson & Lesniak-Karpiak, 1997). Potential benefits of providing a warning are apparent from an ethical standpoint, and as a means of returning the neuropsychologist “to the role of expert examiner and interpreter of test information, as opposed to the more ambiguous role of one who surreptitiously detects potentially fraudulent behaviour…” (Johnson & Lesniak-Karpiak, 1997, p. 236). However, as noted previously, views against this position have recently been expressed on the grounds that warning malingerers may produce more sophisticated malingering (Youngjohn et al., 1999). While practitioners need to weigh up this possibility, further empirical investigation of the specific relationship between warnings and malingering strategies is probably warranted. In addition, if warnings are to be provided in clinical practice, this need not occur instead of instructions to clients to do their best (Youngjohn et al., 1999), but could occur as part of the general introduction to assessment or during the consent process. Finally, even though the efficacy of warnings as a means of reducing malingering has yet to be unequivocally demonstrated, this practice may be warranted in some clinical settings.

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