

The weapon focus effect revisited: The role of novelty

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Purpose. The relationship between novelty, self-reported affect, and the *weapon focus effect* was investigated.

Methods. In two experiments, college students watched a videotaped scene in which either: no item, a novel item (celery) or a gun was brandished. The context of the gun presentation (friendly vs. crime) was also manipulated. A forced-choice questionnaire assessed memory for details of the scene, including the 'perpetrator'. A self-report of affect was also included.

Results. In both experiments, reliably poorer memory for details of the perpetrator's appearance was demonstrated by participants who viewed the scene with the novel item (i.e. the 'celery' group). A traditional *weapon focus effect* was obtained in Expt 2 only.

Conclusions. These data suggest that novelty/salience may be sufficient to produce reliable deleterious memorial consequences in an eyewitness situation, while arguing against the idea that any unique features of a weapon are necessary for the effect. Although self-reports of affect varied between groups in both experiments, there was little relationship between self-reports of affect and memory. It is suggested that the term *weapon focus effect* may be a misnomer for a more general attentional effect.

Nothing sways the hearts and minds of jurors as do the words of an eyewitness (Loftus, 1979; Wells, Lindsay & Ferguson, 1979; Woocher, 1977). Every day eyewitnesses are called upon to testify as to the identity of the individuals involved in the incidents they have witnessed. In many cases these eyewitnesses claim to remember, sometimes over the course of years, the exact identity of the person they

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saw. Human experience and intuition has led the courts to question the veracity of these claims. For example, in 1972, the United States Supreme Court in *Neil v. Biggers* identified five criteria for judging the validity of eyewitness identification (see Wells & Murray, 1983, for a review). As a result of the court's concern, experimental psychologists are being asked to serve as expert witnesses in an ever-increasing number of cases in which eyewitness testimony is a factor.

However, the role of psychologists in these cases has itself become a contentious issue as psychologists openly debate the question of whether or not the state of our current understanding allows for expert testimony regarding the accuracy of eyewitness memory in many cases (e.g. see Egeth, 1994; McCloskey & Egeth, 1983, for arguments against psychologists testifying; see Goodman & Loftus, 1988; Loftus & Doyle, 1987, for arguments in favour of expert testimony; see also, Christianson, Goodman & Loftus, 1992, for a recent spirited review of some aspects of the debate). The crux of this debate is the premise that psychologists should testify only about evidence concerning theories and concepts that are generally well accepted within the field of psychology. The topic of this paper is one of those concepts that remains questionable in terms of its acceptance—the oft-cited *weapon focus effect*.

The weapon focus effect, defined as '... the concentration of a crime witness's attention on a weapon, and the resultant reduction in ability to remember details of the crime' (Loftus, Loftus & Messo, 1987, p. 55), is often cited in reviews of eyewitness memory literature as a factor that reduces the accuracy of eyewitness memory (e.g. Christianson, 1992; Deffenbacher & Loftus, 1982). Discussion of this phenomenon has even made its way into psychology textbooks, which tend to characterize it as a distinct and fairly reliable psychological phenomenon (e.g. Reisberg, 1997; Searleman & Herrmann, 1994). Practising lawyers and judges assume its validity. The concept has crept into formal training programmes for security guards and store clerks. However, its existence is not unequivocally accepted by psychologists in the field of eyewitness testimony. For example, Kassin, Ellsworth & Smith (1989) found that only 56.6 per cent of experts surveyed reported that the weapon focus effect was reliable enough for psychologists to present in a court of law, while only 53.3 per cent of the experts said they would actually testify to the effect. Thus, it would appear that there continues to be some question as to its reliability.

In an attempt to clarify the issue, Steblay (1992) surveyed results from 12 papers, published and unpublished, representing 19 experiments in which exposure to a weapon was a variable of interest. Of the 19 sets of data available, 6 showed reliable support for the weapon focus effect, 13 provided null results, while none presented opposite results. Despite the disparaging appearance of these results, the meta-analysis of these data provided evidence that, overall, the presence versus the absence of a weapon does have a reliable detrimental effect on participants' line-up identification and recall accuracy (Steblay, 1992). She suggested that methodological differences, such as variance in stimuli presentation (e.g. slides vs. videotape), arousal levels and manipulations, degree to which a weapon was absent/present, and type of dependent measure (e.g. line-up identification vs. questionnaire accuracy) might explain the contrasts in findings. However, in spite of this

meta-analysis, based on a review of the literature, Egeth (1994) recently made a strong argument for the position '... that it is still too early for psychologists to testify about weapon focus in front of juries' (p. 264).

Moreover, despite a decade of empirical investigation into the existence of the weapon focus effect and variables that mediate it (e.g. Cutler, Penrod & Martens, 1987*a, b*; Cutler, Penrod, O'Rourke & Martens, 1986; Kramer, Buckhout & Eugenio, 1990; Kuehn, 1974; Loftus *et al.*, 1987; Maass & Kohnken, 1989; O'Rourke, Penrod, Cutler & Stuve, 1989; Shaw & Skolnick, 1994; Tooley, Brigham, Maass & Bothwell, 1987), a solid theoretical explanation for the effect remains lacking. Cited most often as possible causes are arousal and focus of attention. In fact, interaction of these two factors has been offered as a conceivable explanation for the effect by Loftus *et al.* (1987), and several studies (e.g. Kramer *et al.*, 1990; Maass & Kohnken, 1989; Tooley *et al.*, 1987) have explicitly set out to test this possibility.

Use of arousal as an explanatory factor is tied to findings in human perceptual research, including work on arousal and focus of attention centring on Easterbrook's (1959) *cue utilization theory* (e.g. Baddeley, 1972; Brigham, Maass, Martinez & Whittenberger, 1983). This theory, in short, says that as arousal increases, the number of perceptual cues utilized decreases. This reduction will begin with peripheral cues at low levels of arousal and later, if arousal continues to mount in intensity, it will affect central cues. At an optimal arousal level, when utilization of peripheral cues is minimized, allowing total attention to be paid to central cues, performance on a central task could actually improve. Further heightening of arousal level, however, could cause even central cues to be underutilized, thus leading to a decrement in performance. This leads to an inverted-U function similar to the Yerkes-Dodson performance curve (Brigham *et al.*, 1983; Christianson, 1992; Deffenbacher, 1983; Easterbrook, 1959).

In accord with this theory, it follows that the weapon focus effect occurs because in a crime situation the weapon becomes a central cue, the criminal's characteristics become peripheral cues, and as arousal increases, encoding of these characteristics (i.e. peripheral cues) decreases. Such an explanation may even account for the absence of a reliable weapon focus effect in the laboratory, since memory performance would depend on where participants were on the arousal curve when encoding took place (Deffenbacher, 1983). However, many of the weapon focus studies that seek to use Easterbrook's (1959) theory in an explanatory fashion either fail to use a measure of arousal (e.g. Loftus *et al.*, 1987) or fail to find support for an interaction between arousal and attentional focus (e.g. Cutler *et al.*, 1987*a*; Kramer *et al.*, 1990; Maass & Kohnken, 1989; Tooley *et al.*, 1987).

Of course, information regarding witnesses' focus of attention (i.e. gaze direction) at the time of encoding should inform the issue. The seminal paper by Loftus *et al.* (1987), the only weapon focus study to actually utilize eye fixation equipment to track movements of participants' eyes as they viewed a slide presentation, showed that participants did, in fact, focus more often and for longer durations on a weapon (i.e. gun) than on a neutral item (i.e. check). In Expt 1, the eye fixation data showed significant differences between the two groups that mapped on to differences in memory performance (i.e. the weapon focus effect).

Although the second experiment also showed a clear weapon focus effect, no eye fixation data were collected.

In discussing their findings, Loftus *et al.* (1987) argue that an increase in arousal level caused by exposure to a crime scene and exacerbated by the presence of a weapon could lead to perceptual narrowing in accord with Easterbrook's (1959) theory. If this perceptual narrowing did occur, focus on the gun to the exclusion of peripheral details (e.g. perpetrator's features) could lead to a decrement in participants' recall for details of the perpetrator's characteristics. Lack of an arousal measure in this study makes these conclusions speculative, and it should be noted that results of studies investigating the interaction of arousal and focus of attention (Kramer *et al.*, 1990; Maass & Kohnken, 1989; Tooley *et al.*, 1987) have not supported this hypothesized interaction. (While support was found for an effect of attentional focus in these experiments, this effect was not enhanced by changes in arousal level.)

Moreover, a study by Christianson and colleagues (Christianson, Loftus, Hoffman & Loftus, 1991) suggests that focus of attention—defined as eye fixations—may not be the complete explanation for the deleterious memorial effects of exposure to emotional events/stimuli. Across three studies, Christianson *et al.* exposed participants to either an emotional or a neutral event (i.e. via slides). Number of eye fixations on the central stimuli were either controlled (i.e. via exposure time, Expts 1 & 2) or measured (i.e. via eye fixation apparatus, Expt 3). Although participants in the emotional condition did, in fact, fixate more on the central details, this alone could not account for the memorial advantage this group exhibited for central details. Even when fixations were equated the emotional group showed an advantage for central details. Thus, these authors concluded that some type of post-stimulus elaboration may underlie the memorial effects of exposure to emotional events (i.e. better memory for central stimuli with concomitant decrements in memory for peripheral details; see also, for example, Hashtroudi, Johnson, Vnek & Ferguson, 1994; Johnson, Nolde & DeLeonardis, 1996, for similar arguments and empirical evidence).

Interestingly, Loftus *et al.* (1987) did not rule out the possibility that participants' tendency to focus on the gun may represent nothing more than the tendency to focus on any novel (i.e. unusual) item. Human eye fixation research provides evidence that people will fixate sooner, more often and for longer durations on unusual or highly informative objects (Loftus & Mackworth, 1978; Mackworth & Morandi, 1967; Noton & Stark, 1971), and that memory for these objects will benefit (Berlyne, 1960; Loftus, 1972). This increased fixation on the novel item could be expected to have a detrimental effect on encoding of other details of the scene. Thus, it might be hypothesized that people viewing a crime will focus on a weapon merely because it is the most informative item in the 'picture'. By the same logic, however, it could be hypothesized that witnesses would focus on *any* novel or unusual item. If this were the case, a weapon would not be necessary to produce the deleterious memorial effects that are the defining feature of the weapon focus effect. That is to say, the weapon focus effect may not be dependent on the inclusion of a weapon *per se*. One might hypothesize, as did Loftus *et al.* (1987), that any novel item could produce analogous effects.

A recent study designed to investigate the memorial consequences of viewing a scene containing an unusual item failed, for the most part, to find a deleterious effect of novel items on memory in a non-arousing witnessing situation (Shaw & Skolnick, 1994; see also Christianson & Loftus, 1991, for results involving memory for unusual *events*). In this study, participants viewed a six-slide scenario of a non-arousing situation (i.e. a student leaving a phone booth and walking down a hall). Different groups of participants viewed the scene with the character carrying either a weapon, a magazine (control item), or one of four 'unusual' items (i.e. space cones, conch shell, stethoscope, wooden snake). Memory was tested using both photo-spread identification and a recall measure. Interestingly, although there was an interaction of object by gender, demonstrating that males and females reacted differently to the items, there was no effect of item type on the recall measure. Even more surprisingly, identification via the photo-spread was worse in the control condition (i.e. magazine) than the weapon condition and all but one of the novel item conditions in this study. It is difficult to know what to make of these counter-intuitive results. But, in spite of this one set of anomalous data, it seems reasonable based on the human eye fixation and attentional literature to hypothesize that eyewitnesses would attend to a highly salient or novel item to the detriment of their memory for the details of the situation.

In fact, this hypothesis found support in the results of a small pilot study conducted to evaluate the possibility that an effect analogous to the classic weapon focus effect could be obtained using a novel (i.e. unusual) non-weapon item (i.e. celery), as suggested by Loftus *et al.* (1987). Participants ($N = 28$) were shown one of three videotaped versions of a scenario in which a 'perpetrator' approached a couple and pointed either his bare hand, a gun or a stalk of celery at them (the versions were identical except for the item).¹ Extending the findings of previous research (e.g. Loftus & Mackworth, 1978; Mackworth & Morandi, 1967) to a videotaped scene, it was hypothesized that participants would, in fact, focus on the novel item (i.e. celery), and that this focus would lead to a decrement in participants' memory for details of the situation.

Support for the notion that exposure to a novel item could lead to a decrement in memory for details seen during presentation of the novel item was obtained (M_s (proportion correct) = .86, .76 and .86, for the control, celery and weapon conditions, respectively; $F(2,27) = 5.91$, $MSE = .54$, $p < .01$). *Post hoc* testing revealed that the locus of this effect was a reliable decrement in the memory performance of the participants who viewed the novel item (i.e. celery) scenario as compared to both the control and weapon groups, whereas the latter did not differ from each other.² The results of this study also supported the hypothesis that memory for details seen before introduction of the experimental items (i.e. hand, celery, gun) would be unaffected, thus showing the effect to be an encoding phenomenon affecting only details seen at the same time as the experimental item.

¹Albeit the criticism against the ecological validity of using videotaped scenarios as stimuli in weapon focus studies is acknowledged and well taken (e.g. Egeth, 1994), we chose to use such stimuli so that the present results could be compared more easily to previous studies.

²Details of the pilot study are available from the first author. The basic procedure was the same as that used for both subsequent experiments, although a different (improved) videotape and questionnaire were utilized for Expts 1 and 2.

Although the results of this pilot study are suggestive, considering the small sample size and lack of a reliable weapon focus effect, they are far from conclusive. Thus, the present study was designed to further investigate the reliability and uniqueness of the classic weapon focus effect. Participants viewed one of four versions of a videotaped scenario in which a male 'perpetrator' confronts another male with either: his hand (i.e. no item, control group), a gun wielded in a threatening manner (i.e. an obvious 'crime scene' with the gun used as a weapon, weapon group), a gun held in an innocuous, non-threatening manner (i.e. obviously not a crime; weapon obviously not used as a weapon *per se*, gun group), or a stalk of celery (i.e. novel item, celery group). The main measure of interest was the extent to which participants' memory for the details of the situation, including the perpetrator's identity, would be negatively affected by the manipulation.³ Note that the inclusion of the condition in which there is a gun presented in an innocuous fashion allows examination of the effects of a gun as an object *per se*, apart from the attendant confound of it being used as a weapon *per se*. That is to say, regardless of how it is used, a gun may induce a decrement in performance (perhaps by serving as a novel item at a functional level). Evaluation of this possibility is important to an interpretation of the weapon focus effect as a 'novel item effect'.

A measure of participants' subjective report of affect was also included. While Kramer *et al.* (1990) found no difference in arousal between participants viewing scenes containing a weapon (meat cleaver with blood) as compared to a neutral item (news magazine), Maass & Kohnken (1989) found that simple exposure to a syringe in an experiment using a real-life paradigm was enough to induce a mood change (e.g. tension, uneasiness) in participants. Although determining whether a physiological arousal mechanism may underlie the weapon focus effect is beyond the immediate concern of this paper, determining whether participants' self-reports of affect might vary as a function of the manipulation and how such reports might relate to memory performance could prove informative on a practical level. After all, subjective report of affect would most likely be the only report that police/lawyers would be privy to after the fact. Thus, gathering information concerning the possible relationship between eyewitnesses' self-descriptions of their affect and their memory for the situation was a second goal of this study.

EXPERIMENT 1

Method

Participants

Eighty-three undergraduates were recruited from lower level summer courses at a small college in the United States. Thirty-seven (44.6 per cent) of the participants received no compensation for taking part, while 46 (55.4 per cent) of the participants received extra credit points in their course.

³Note that no line-up identification procedure was used in the present study. We acknowledge that such a measure could potentially supply important data regarding these effects and increase the ecological validity of the study. However, we chose to avoid potential confounding of results, since verbal description can sometimes affect line-up identification (Leippe, 1980) or vice versa, perhaps via the mechanisms of unconscious transference (Loftus, 1979) or verbal overshadowing (e.g. Schooler & Engstler-Schooler, 1990).

Preliminary analyses revealed that level of compensation had no effect on performance, thus the data are collapsed across this variable for all reported analyses.

Materials and procedure

Videotape. A videotape was made by a professional videographer. Three amateur white males acted in the video, that contained four versions of a vignette (described below) varying only in the item presented by the 'perpetrator'. To decrease extraneous distraction, there was no audio, although to keep the actions as realistic as possible the actors did visibly engage in dialogue. Total time of each vignette version was approximately 33.5 seconds.

Pilot work, in which informed advanced psychology students ($N = 15$) answered a questionnaire *while watching* all four versions of the scenario, confirmed that there were no noticeable differences between condition versions except the item presented and the demeanor of the actors. These participants reported that the difference between the two gun conditions was clear to them. That is, they recognized that one situation (weapon group) depicted a crime while the other (gun group) did not. Likewise, they all recognized that the demeanor of the perpetrator in the celery group was non-threatening and, in fact, both men appeared pleasant. This piloting also assured that all details queried on the memory questionnaire were in fact visible and recognizable in the film. None of these students participated in the actual experiments.

Experimental participants were randomly assigned to one of four conditions, each of which viewed a different version of the videotaped scenario (described below). They were tested in small groups (≤ 5), seated approximately 45 in, and at no more than a 45 degree angle, from the centre of the screen of a Mitsubishi colour television (20 in diagonal screen) linked to a Mitsubishi U32 DJ 4-head VCR. They were accurately informed that they would view a brief videotape and later complete a questionnaire pertaining to it. The videotape was shown in a darkened room. Prior to the beginning of the vignette, a black screen with white letters reading 'CONDITION—' was shown for 15 seconds, giving participants a chance to focus on the screen. One of four scenes (described below) then began, and its end was signalled by a black screen.

Each vignette opened in an identical manner with a brief friendly exchange between two white males in a business office. After the exchange, one of the men left, while the other walked across the room to a second desk (not visible during the opening of the scene) to pick up and read a piece of paper. This beginning scene (approx. 15.0 seconds—i.e. 'before presentation' portion) was filmed only once and then edited into all four versions.

The ending (i.e. 'perpetrator specific' portion) was filmed individually for each condition. Considerable care was taken during filming and editing to ensure that everything was identical in each version except the item presented by the perpetrator and his demeanor in the weapon (i.e. crime) condition. In this part of the scene a third white male entered the office carrying a briefcase in his left hand, with his right hand reaching into the briefcase. As he approached the man standing at the desk, he pulled out either: (a) his bare hand (the two men appeared cordial and a handshake followed—control condition), (b) a stalk of celery which he appeared to be 'showing' the other man (novel item—i.e. celery condition; again, both men appeared pleasant), (c) a handgun handled in an obviously non-threatening manner, as if the third person were merely showing it to the other man (gun group), or (d) a handgun (pointed at the 'victim' in an obviously threatening manner, that is, it was clear this was a hold-up and the gun was used as a weapon—weapon group). The perpetrator was visible for a total of approximately 6.9 seconds; 1.7 of these occurred before presentation of the item.

Albeit initial questioning of actual crime witnesses may sometimes take place in the same general vicinity as the crime, it would probably seldom occur in exactly the same context. Therefore, to obtain some small degree of ecological validity, participants were asked to relocate to another room in the lab to complete the memory questionnaire. The experimenter accompanied them during the relocation, and they were not allowed to converse with each other during this time. Total time from the end of the videotape until participants began the questionnaire was approximately 1.5 minutes (i.e. movement and instructions). This time, combined with the 1.5 minutes most participants took to complete a page of demographic information, served as a brief distraction period.

Questionnaire. On the first page of the questionnaire participants were asked to rate the feelings they experienced while watching the videotape using a seven-item Likert-type scale that included five words (i.e. afraid, frightened, interested, happy, amused) from the Anxiety and Positive Affect portions of the Multiple Affect Adjective Checklist Revised (Zuckerman & Lubin, 1985), along with two others (i.e. surprised and aroused). Participants were told to rate how well each word described their feelings during the videotaped scenario using four modifiers that ranged from *very* (3) to *not at all* (0).

The next two pages contained 25 multiple-choice questions concerning participants' memory for details of the videotape. The first 10 questions referred to details in the opening of the scene, including items on the desk, details about the first man, the office furnishing, etc. (e.g. 'Which of the following was visible on the *first* desk in the scene?'). The next 15 questions referred to perpetrator-specific details, including questions about facial hair, glasses, his clothing, what he held in his extended hand, etc. (e.g. 'The *third* person in the film (the *last* person to enter) had what color hair?'). One question had a yes/no response choice, but also included 'don't know' as an option. All others had five response choices (i.e. don't know, other: —, correct response, two foils). Order of the response options was randomized for each question but order was identical for all participants. Completion of the instrument was self-paced, and most participants finished in about 5.5 minutes.

Results and discussion

For all analyses reported in this paper significance was tested at an alpha level of .05 unless otherwise noted.

Number of: (a) hits (circling the correct option), (b) false alarms (circling a wrong option), (c) 'other' (filling in an incorrect answer), and (d) 'don't know' responses were recorded separately for questions pertaining to the items seen *before presentation* (i.e. beginning of the scene) and *perpetrator-specific* details (i.e. details observable at the time the independent variable was exposed). Use of the 'other' or 'don't know' responses was quite low and there were no significant differences between groups on their use, indicating no apparent bias for any group to utilize those options. All remaining analyses are reported on proportion correct.

Contrary to evidence in the literature concerning differential effects of eyewitness paradigm manipulations, including inclusion of a weapon, on males and females (e.g. Brigham *et al.*, 1983; Johnson & Scott, cited in Brigham *et al.*, 1983; Shaw & Skolnick, 1994), when gender was entered as a factor in affect and memory performance ANOVAs, no significant main effects, nor reliable interactions, were found. This factor will, therefore, not be discussed further.

Of primary importance, there was a reliable difference in performance between groups on perpetrator-specific questions (see the top half of Table 1) ($F(3,79) = 2.77$, $MSE = .03$). *Post hoc* testing (Fisher's LSD) confirmed that, reminiscent of the results of the pilot experiment discussed in the introduction, recognition in the celery group was significantly worse than that in the control group. Interestingly, it was also reliably lower than that of the gun (no crime) group, whose performance did not vary from that of controls. So, although there does appear to be a detrimental effect of presenting a novel item (i.e. celery), a gun *per se* does not appear to have been novel enough to induce such an effect in this experiment. While a classic weapon focus effect (i.e. performance for the weapon group significantly lower than that of controls) did not obtain ($p = .08$), the scores were in the right direction (i.e. weapon lower than control).

Table 1. Mean proportion correct (and standard error) for the 'perpetrator-specific' questions of the memory questionnaire for Expts 1 and 2

Condition	N	Mean (standard error)
Experiment 1		
Control	20	0.69 (0.04) _a
Celery	22	0.58 (0.04) _{ab}
Gun (no crime)	20	0.70 (0.04) _b
Weapon (crime)	21	0.60 (0.04)
Experiment 2		
Control	40	0.79 (0.02) _{abc}
Celery	40	0.74 (0.02) _a
Gun (no crime)	40	0.75 (0.02) _b
Weapon (crime)	40	0.72 (0.02) _c

Notes. Means with the same subscript differ significantly at the .05 level using Fisher's least significant difference test; 95% confidence intervals for Expt 1 = $\pm .08$, for Expt 2 = $\pm .04$.

It is important to note that, as in our pilot study, the celery group evidenced the poorest performance (see Table 1). Thus, clearly it is possible to induce an effect analogous to the weapon focus effect with another item—one that is quite novel. This finding would seem to argue against the weapon focus effect as a unique memory phenomenon. Furthermore, the finding of equally accurate performance in the gun (no crime) group as in the control group, coupled with the fact the gun group performed marginally better than the weapon (crime) group ($p = .06$), argues against the supposition that something inherent in the gun *per se* (i.e. attention-holding or emotionally arousing qualities) underlies the reduction in memory that is the hallmark of the weapon focus effect.

It should be briefly noted that once again, as in the pilot study, participants' performance on questions asking about details seen before presentation of the independent variable was fairly low and did not vary between groups ($F(3,77)^4 = .51$, $MSE = .03$, $p > .10$; $M \pm 95$ per cent confidence intervals $CI's = .38 \pm .10$, $.41 \pm .08$, $.42 \pm .06$, $.36 \pm .06$ for the control, celery, gun (no crime) and weapon (crime) groups, respectively). This finding supports the contention that the obtained decrement is in fact an encoding phenomenon occurring as a function of exposure to, and affecting only details seen at the same time as, the experimental item.

Each participants' affect ratings were first summed across descriptor to give a total affect score as a measure of subjective report of degree of affect, regardless of valence. While total affect ratings were low in general⁵ (i.e. actual range of individual's scores = 1–12; possible range = 0–21), there were reliable differences

⁴We were unable to score the first portion of the questionnaire for two participants due to ambiguity; therefore, for this analysis, $N = 81$.

⁵Since the measure used adjectives of both positive and negative valence it seems logical that scores would be low. That is, participants were expected to score high on only one or the other valence in describing their feelings.

Table 2. Group means (and standard errors) for total self-reported affect and for each descriptor in Expts 1 and 2

Descriptor	Condition			
	Control	Celery	Gun	Weapon
Experiment 1				
Total	4.75 (0.50) _a	7.00 (0.50) _{ab}	5.75 (0.65)	5.11 (0.46) _b
Nervous	0.40 (0.11)	0.27 (0.13)	0.35 (0.11)	0.14 (0.08)
Afraid	0.05 (0.05)	0.09 (0.09)	0.20 (0.09)	0.00 (0.00)
Aroused	0.00 (0.00)	0.50 (0.17)	0.45 (0.11)	0.29 (0.12)
Interested	1.80 (0.21)	1.90 (0.17)	1.75 (0.18)	1.48 (0.15)
Amused	0.35 (0.13)	1.36 (0.17)	1.05 (0.20)	0.67 (0.16)
Happy	1.45 (0.23)	1.22 (0.19)	1.10 (0.20)	0.95 (0.18)
Surprised	0.70 (0.16)	1.64 (0.18)	1.30 (0.18)	1.57 (0.22)
Experiment 2				
Total	2.65 (0.24) _{ab}	3.85 (0.30) _{ac}	3.00 (0.23) _{cd}	4.00 (0.28) _{bd}
Nervous	0.30 (0.08)	0.44 (0.09)	0.33 (0.09)	0.40 (0.10)
Aroused	0.20 (0.07)	0.36 (0.10)	0.18 (0.08)	0.35 (0.09)
Interested	1.43 (0.12)	1.64 (0.13)	1.35 (0.09)	1.60 (0.12)
Surprised	0.73 (0.12)	1.41 (0.14)	1.15 (0.15)	1.65 (0.14)

Notes. Total self-reported affect scores with the same subscript differ at the .05 level using Fisher's least significant difference test. For Expt 1, the actual range of individuals' total scores = 1–12; possible range = 0–21. For Expt 2, the actual range of individuals' total scores = 0–8; possible range 0–12. The possible range for each individual descriptor was 0 (not at all) to 3 (very).

between groups on mean total affect rating ($F(3,79) = 3.64$, $MSE = 5.76$) (see the top half of Table 2). However, *post hoc* testing (Fisher's LSD) showed that the pattern of group differences in affect did not match the pattern of differences on the recognition measure. Specifically, for total affect score, reliable differences exist between the celery group and both the control and weapon (crime) groups, with celery group participants reporting higher overall affect than either of the latter participants. However, the difference in affect between the celery and gun (no crime) groups was not reliable ($p = .10$) albeit on memory performance the difference was reliable.

Furthermore, when the total affect score was entered as a covariate in an ANCOVA of the perpetrator-specific memory scores the pattern of results discussed above did not change. Specifically, even when total affect scores are taken into account there is still a reliable effect of group ($F(3,78) = 3.06$, $MSE = .03$). The covariate factor was not significant. Moreover, *post hoc* tests (LSD) done on the adjusted memory performance means showed the same pattern of differences between the groups—that is, the celery group was significantly worse than the control and gun (no crime) groups. The gun (no crime) group still did not differ from controls, while the weapon group was marginally lower than both the controls ($p = .08$) and the gun (no crime) group ($p = .06$).

One way to get a better picture of participants' subjective affect reports is to look at what types of affect descriptors participants might differentially (i.e. by group) use in describing their feelings. Looking at the mean affect ratings by affect descriptor for each group (see Table 2), it would appear that the difference in affect reported by the celery group was probably a function of the differentially higher ratings given to the descriptors 'amused' and 'surprised' by this group as compared to controls. However, albeit one intuitively appealing explanation for the celery group's poor recognition memory would be to attribute it to their great surprise, this notion does not seem to be supported by the patterns in the data. The mean ratings of the celery, gun and weapon groups look remarkably similar and different than those of the control group for the 'surprised' descriptor, although only the celery group evidenced poorer memory performance. Moreover, the results of an ANCOVA in which the 'surprise' score was used as the covariate are similar to the initial ANOVA and the total affect score ANCOVA ($F(3,78) = 2.64$, $MSE = .03$, $p = .055$). The covariate was not significant. All *post hoc* differences, which were performed on the adjusted memory performance means, are as previously reported.

Finally, a regression analysis with all of the affect scores entered as factors showed that none of the scores significantly predicted memory performance ($F = 1.42$, $p > .10$; $R^2 = .12$; all betas n.s.). Thus there appears to be no clear one-to-one correspondence between the magnitude of participants' retrospective subjective reports of affect (including surprise)—at least using this measure—and their memory performance.

EXPERIMENT 2

Experiment 2 was intended as a replication of Expt 1 to see if we could once again obtain a reliable effect of exposure to the novel item. In addition, the pattern of means in Expt 1 suggested that a reliable weapon focus effect might obtain with more participants. Finally, we were interested in altering the memory measure. Although there were no differences between the groups in Expt 1 in their use of the 'other' and 'don't know' options, these responses were used by some participants. We reasoned that forcing participants to select only from among the correct answer and two lures might improve the chances of seeing group effects. Our logic was that for participants who had absolutely no memory for a detail, guesses on a forced-choice test should be randomly distributed across the options regardless of condition. However, for participants who had any memory of the correct answer, guesses on a forced-choice test should be biased towards the correct answer. Thus, to the extent that the manipulations differentially affected memory for the details, forcing participants to choose an answer by removing the 'don't know' and 'other' options might increase the sensitivity of the measure.

The affect measure was once again utilized, although in Expt 2 the number of descriptors was reduced.

Method

Participants

Participants were 160 undergraduates recruited from the psychology subject pools of two universities in the United States. They were randomly assigned to one of the four experimental conditions

($N_s = 40$), as in Expt 1. Participants received credit towards meeting a class requirement as compensation.

Materials and procedure

The materials and procedure were identical to Expt 1 with the following exceptions. Rather than five response options on the memory portion of the post-event questionnaire as in Expt 1, the questionnaire in the present experiment had only three response options for each question (i.e. the correct answer and two lures). Otherwise, the memory portion of the questionnaire was the same as that used in Expt 1. The subjective affect rating portion of the questionnaire was the same as in Expt 1, except that it contained only 'nervous', 'aroused', 'interested' and 'surprised' as descriptors. As in Expt 1, participants rated how well each word described their feelings during the videotaped scenario using four modifiers that ranged from *very* (3) to *not at all* (0).

Results and discussion

The result of primary interest was participants' memory for the details queried in the perpetrator-specific questions (see the bottom half of Table 1). Once again, there was a reliable group effect ($F(3,156) = 3.11$, $MSE = .01$). *Post hoc* testing (Fisher's LSD) confirmed that the results of the pilot study and Expt 1 were replicated in that the memory of the celery group was once again significantly worse than that of the control group. However, unlike Expt 1, a reliable weapon focus effect did obtain (i.e. weapon (crime) group performance was significantly worse than that of controls). The difference between the control and gun (no crime) group's performance just made significance ($p = .05$), while the celery, weapon and gun groups were not reliably different from each other in this experiment.

Thus, looking across experiments, the message from this study seems clear. Although it is possible to produce a weapon focus effect in the laboratory using a videotape as stimuli, the effect is somewhat tenuous. We discuss possible reasons for this below. In addition, a reliable decrement in memory for perpetrator-specific details has been repeatedly demonstrated in the current experiments with an arguably more novel item—celery. Looking at the performance of both the weapon (crime) and gun (no crime) groups overall, whether or not a gun, *per se*, is novel enough to produce a memory decrement appears from these data to be an open question.

Each participant's affect ratings were once again summed across descriptor to give a total affect score as a measure of subjective report of degree of affect, regardless of valence. Albeit there were reliable differences between groups on mean affect rating in this experiment also ($F(3,155)^6 = 6.17$, $MSE = 2.75$) (see the bottom half of Table 2), *post hoc* testing showed that the pattern of group differences in affect once again did not really match the pattern of differences on the recognition measure. Specifically, for total affect score, both the celery and the weapon (crime) participants (who did not vary from each other) reported higher overall affect than the control and gun (no crime) participants (who also did not vary from each other), albeit all groups varied from the control group on the memory measure.

⁶One participant failed to complete the affect rating scale.

The total affect score was entered as a covariate in an ANCOVA of the perpetrator-specific memory scores, and the pattern of results discussed above did not change. Specifically, even when total affect scores are taken into account there is still a reliable effect of group ($F(3,154) = 2.77$, $MSE = .01$). The covariate factor was not significant. Moreover, *post hoc* tests (LSD) done on the adjusted memory performance means showed the same pattern of differences between the groups—the celery group was once again significantly worse than the control group, as was the weapon (crime) group. The difference in the performance between the control and gun (no crime) groups was only marginally significant in this analysis ($p = .06$).

A look at the means in Table 2 (bottom) shows that some similarity between the pattern of group means for responses to the 'surprised' descriptor and memory performance might be noted. Specifically, all groups would appear to have been more surprised than the control group, which is a pattern similar to memory performance. And, in fact, when 'surprise' scores were entered as the covariate into an ANCOVA done on the memory scores, the effect of condition was only marginally significant ($F(3,154) = 2.58$, $MSE = .01$, $p = .06$), suggesting that at least some of the variability in performance was attributable to surprise. However, the covariate was not significant and *post hoc* tests (LSD) done on the adjusted memory means showed the same pattern of differences between the groups as when the ANCOVA included the total affect score. The possibility that surprise might be having some effect on memory performance deserves further study.

It might be briefly noted that a regression analysis with all of the affect scores entered as factors showed that none of the scores significantly predicted memory performance ($F < 1$; $R^2 = .02$; all betas n.s.).

GENERAL DISCUSSION

The data from this study send a fairly straightforward message. Namely, an effect completely analogous to the oft-cited weapon focus effect can be reliably produced in the laboratory without a weapon, while inclusion of a weapon does not guarantee a weapon focus effect will occur. Specifically, these experiments empirically demonstrate what has hitherto been merely suggested. That is, it is clearly possible to produce a reliable decrement in memory performance similar to the weapon focus effect using a highly novel item (i.e. celery).

Although further experimentation would be necessary to isolate the exact locus of the mechanism by which the celery produced the memory decrement in this situation, these data are certainly consistent with the idea that novelty/salience may be sufficient to produce deleterious memorial consequences in an eyewitness situation. At the same time, they argue against the idea that any unique features of a weapon are necessary for the effect. This is certainly consistent with several well-established theories of human attention and memory.

Additionally, there would appear to be no obvious relationship between participants' retrospective self-reports of affect at the time of the exposure to the

incident of interest and their memory for the details of the incident except, perhaps, for reports of surprise. Examining the surprise factor is one avenue for further investigation. Moreover, a full understanding of the role of surprise (or any other affect) will require determining the extent to which the present results might be due to the retrospective nature of the affect measure.

It is important to note that the present study is itself of limited ecological validity, and so it may be ill-advised to try to map these results onto real-world crime situations too closely. However, these results do serve well for heuristic purposes. Specifically, they point to novelty (i.e. salience) as a possible key factor behind the weapon focus effect (see also, for example, Kramer *et al.*, 1990, for additional empirical evidence supporting this conclusion). This possibility deserves further investigation.

Before discussing possible avenues for further inquiry, however, it is interesting to note that given the probable role of novelty, it is perhaps not so surprising that laboratory studies using slides and videotapes sometimes do not produce a reliable weapon focus effect. After all, with all of the exposure to weapons provided by the media today it is likely that most college-aged students are becoming desensitized to exposure to weapons in a media-like format. It may be tempting then to leap to the general assumption that a weapon should be even more novel/salient in a real crime situation and therefore the weapon focus effect should be expected to be more robust in a real-world eyewitnessing situation. But, it is not clear that such an assumption is necessarily valid. Since the specific context might define the novelty of any given item (including a weapon), whether or not a deleterious memorial effect is observed may depend on the specific circumstances under which the item is exposed—what is novel in one case may not be in another (and what is novel to one person may not be to another). This might also help explain the variability in the effect of the gun (no crime) manipulation across the two experiments. In any event, additional empirical research (i.e. studies with higher ecological validity) is necessary to determine whether the weapon focus effect is more or less reliable under more realistic conditions.

In fact, the current results suggest some interesting questions about the nature of the effect *in vivo* that should be examined in future studies (see also, for example, Egeth, 1994, for some similar suggestions). For example, are there individual differences in vulnerability to the weapon focus effect? Might some individuals, namely those regularly exposed to weapons for extended periods of time (e.g. police, gang members, gun dealers), find a weapon less salient/novel/surprising than others? If so, would they necessarily also be less likely to experience detrimental memorial effects in a crime situation in which a weapon was exposed? On the other hand, one might ask whether extended exposure to a weapon in a single criminal episode would decrease its salience. If so, then exposure time might be a telling factor in determining the memorial consequences of exposure to a weapon in a witnessing situation (see, for example, Kramer *et al.*, 1990, for data consistent with this possibility).

The current findings also serve as a reminder that a traditional weapon is probably not the only type of item that could compromise the integrity of eyewitness memory. For example, we are reminded of a recent (real-life) store

robbery in Canada in which the thief entered the store with a Canadian goose under his arm. He threatened to harm the goose if he was not given money. After several patrons gave him their cash he left, leaving the goose behind, unharmed (CNN Headline News, 26 April 1997). One might ask, given the current results, if eyewitnesses' memory for the details of the thief's appearance might be negatively impacted by their exposure to the goose. Clearly, a more thorough investigation of the range of item types that can produce analogous detrimental memorial effects might be informative regarding the nature of this effect. Such information regarding generalizability could be important on both a practical and theoretical level.

Finally, albeit understanding the relationship between physiological arousal and the deleterious effects of exposure to a weapon may be important for theoretical reasons, it seems likely that understanding the relationship between people's subjective reports of affect and their eventual memory performance would be more helpful on a practical level. Given the lack of a robust relationship between self-reported affect and memory performance in the present study (see also, for example, Kramer *et al.*, 1990), one might question whether witnesses can offer any self-reports of subjective experience that might help differentiate when a weapon focus-like effect will or will not occur in a witnessing situation. Based on the present findings, reports of surprise would seem to be a good place to start further investigation of this issue.

The bottom line is that rather than asking *if* a weapon produces deleterious memorial consequences in eyewitnessing situations, future research should methodically ask *exactly when*, and *under what precise circumstances* a weapon might be reliably expected to do so. (We are certainly not the first researchers to suggest this; see also, for example, Christianson, 1992; Egeth, 1994; Kramer *et al.*, 1990, for similar discussions.)

In any event, the present results suggest that albeit *weapon focus effect* may be a convenient label, it is probably somewhat of a misnomer. Analogous effects can be reliably obtained with other quite novel (and clearly non-weapon) items (e.g. celery). Thus, pending further, finer-grained investigation of this effect it is suggested that perhaps broader, more well-established theories of human attention and memory—theories that clearly support the possibility of a weapon focus effect—would provide a more parsimonious explanation for the memory decrements sometimes seen when a weapon is exposed in an eyewitness situation. The practical advantage is that such theories may be closer to the sorts of 'generally accepted' theories expert psychological witnesses are called upon to use in providing their testimony (e.g. under the *Daubert* opinion; e.g. Goodman-Delahunty, 1997). The empirical advantage is that such theories are likely to generate more specific hypotheses that can be used to systematically examine the precise circumstances under which a weapon might produce memory decrements. This study thus serves to echo the call of other researchers for such an approach in further studies aimed at advancing our understanding of eyewitness memory in situations in which weapons (both more and less traditional) are likely to be present (see also, for example, Egeth, 1994; Kramer *et al.*, 1990, for similar discussions).

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