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# Proactive and Retroactive Interference in Implicit Odor Memory

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## Abstract

To test the hypothesis that longevity of odor memory is due to strong proactive interference (reduction of new learning by prior learning) and to absence of retroactive interference (reduction of prior memory by new learning), subjects, matched in age and gender with those of a previous experiment, were unknowingly exposed in two sessions to the weak concentrations of lavender or orange used before. Implicit odor memory was later tested in a separate experiment. Comparison of the results with those of the previous experiment showed that both proactive and retroactive interference occurred. These results have implications for the general theory about implicit memory for new associations, which may have to be amended when non-verbal material is used. The longevity of odor memory should be explained by the improbability of occurrence of incidences that provoke retroactive interference rather than by the absence of the retroactive interference itself.

## Introduction

The very flat forgetting curve, which, according to many authors (Engen and Ross, 1973; Lawless and Cain, 1975; Murphy *et al*., 1991), characterizes recognition memory for odors and is one of the arguments for the existence of a separate odor memory, is often explained (Lawless and Engen, 1977; Engen, 1991; Herz and Engen, 1996) by the fact that proactive interference, the phenomenon that earlier experiences with stimuli reduce the memory for later ones, is strong and that retroactive interference, the phenomenon that later experiences with stimuli reduce the memory for earlier ones, is absent in long-term odor memory. Thus, first encounters with an odor would be remembered over very long times. Strangely enough, there exists only one experiment by Lawless and Engen (Lawless and Engen, 1977), to support this hypothesis. In this experiment, the subjects were exposed to combinations of odors and pictures. The retroactive experimental group first learned one set of pairings of these. In a subsequent test they had to reproduce the pairings and were given feedback when they failed to give the correct answers. In a second session, 48 h later, they had to learn a second and different set of pairings of the same odors and pictures on which they were subsequently tested in the same way. Finally, after a retention time of 2 weeks, they were requested to reproduce the pairings of the first set by means of a forced choice (one of 12) test procedure. Their results on the final test were compared with those of a control group who only learned during the first session and then, during the second session, were just asked to familiarize themselves with the odors by smelling each odorant during 15 s and paying careful attention to the odor quality. The retroactive experimental group and this control group performed equally well and it was concluded that there was no retroactive interference. The proactive experimental group also received the two sets of pairings during the two sessions, but they were asked to reproduce the pairings of the second session and their results were compared with a control group who familiarized themselves with the odors in the first session and then performed the learning task during the second session. In this case the proactive experimental group performed significantly less well than their control group, indicating that proactive interference had taken place. Nevertheless, this experiment had some flaws. First, the odorants varied strongly in identifiability (e.g. vanillin, heptanal and cyclotene). The influence of verbal memory on the results of some of the odors could therefore not be excluded. Secondly, in the retroactive control group, the instruction in the second session to familiarize themselves with the odors by paying careful attention to the quality of the odors in the absence of the pictures, may have led to extinction of the original association of odors and pictures formed in the first session. This would reduce the difference between the experimental and the control group. In the proactive control group, where the familiarization took place in the first session and the association was formed afterwards in the second session, such extinction could not have taken place. This may explain why the proactive control group performed indeed somewhat better than the retroactive control group. Thirdly, the subjects were explicitly instructed to remember pairings of odors with pictures and later reported having constructed mediational schemes for relating these arbitrary combinations of odors and pictures. This raises the question, whether the subjects really remembered the association by the odors and not by the constructs they used. In itself this would not be fatal, because in the final tests odors served as the only clues, but it may have influenced the data on interference, because of similarities between the constructs in the two sessions or because it was more difficult to find new and equally effective constructs in the second session. It seems even likely that constructs used in the first session or analogies of such constructs, were also used in the second session. As Lawless and Engen (Lawless and Engen, 1977) reported, the use of such mediators by the subjects, had indeed a very significant and positive effect on the correctness of the responses. Therefore, when Lawless and Engen (Lawless and Engen, 1977) showed that proactive interference was strong and that retroactive interference was absent in their experiment, it is somewhat questionable whether they were really discussing odor memory.

Since then, only one other direct attempt has been made to prove the correctness of the hypothesis that proactive interference is strong and retroactive interference is weak or even absent in odor memory. In 1984, Walk and Johns (Walk and Johns, 1984) reported an experiment in which they presented the subjects two odors during the acquisition phase, one of which had to be recognized in a forced choice (one of four) procedure after a retention interval of 26 s. During this interval different distractor items were presented to different groups. Memory interference occurred if the distractor item was an odor from the same category as the to-be-remembered odors and when the subjects were asked to make a free association to this distractor odor. Thus, they concluded that interference did take place and that this interference was mainly olfactory and not semantical in nature, because in another of their distractor conditions, the subjects had been asked to associate freely to an odor name and in this condition interference was considerably lower. According to Walk and Johns (Walk and Johns, 1984) their results demonstrated that odor memory is a separate, but not a qualitatively different memory system. The first part of this conclusion was based on the fact that in their experiments, as in those of Engen and Ross (Engen and Ross, 1973) and Lawless and Cain (Lawless and Cain, 1975), it could be shown that odors are encoded largely as perceptual entities. Although the experiments of Walk and Johns and of Lawless and Engen have much in common (explicit learning task, incentives for using verbal mediation, forced choice recognition), it should be remembered that Walk and Johns studied pure odor memory (i.e. remembrance of the odor itself) instead of memory for an arbitrary association between odors and pictures and that both the acquisition period and the retention time in Walk and Johns' study were extremely short compared with those in the Lawless and Engen (Lawless and Engen, 1977) experiments. In fact, Walk and Johns (Walk and Johns, 1984) studied short-term memory and Lawless and Engen (Lawless and Engen, 1977), studied long term memory. According to White (White, 1998) there are good reasons to assume the presence of at least a form of short-term memory in olfaction, although many authors, including Engen (Engen, 1982, 1987, 1991) have denied its existence for a long time. This may be the reason why, in their discussion of retroactive interference, Engen (Engen, 1991) and Herz and Engen (Herz and Engen, 1996) do not mention the results of Walk and Johns (Walk and Johns, 1984), although they cite the work in another connection. To the knowledge of the present authors, since 1984 no other studies on retroactive or proactive interference of pure odor memory have been reported.

There is, however, another line of research which suggests that memory for odors has a special resistance to extinction. Baeyens *et al.* (Baeyens *et al.*, 1990), showed that when odors are paired with taste stimuli, the liking for them may be changed in the direction of the liking of the taste stimulus and that such a change is markedly resistant to extinction (Baeyens *et al.*, 1995). In similar experiments, Stevenson and his colleagues (Stevenson *et al.*, 1995, 1998, 2000a,b) showed that the perceptual properties ascribed to the odor ('sweetness', 'sourness') were affected even more strongly than liking and that these properties were also resistant to extinction and to counter-conditioning. A sweetness enhancement effect, showing that when a sweet smelling, but tasteless odor, is added to a sucrose solution, the mixture will be judged to be sweeter than the sucrose solution alone, was also found (Stevenson *et al.*, 1999). Stevenson *et al.* (Stevenson *et al.*, 2000a) suggested that the reason for the resistance to extinction lies in the special way in which odor–taste associations are encoded. The fact that the odors of flavors, although perceived by nasal receptors, are perceived as stemming from the mouth, might lead to the encoding of a configural (unitary) odor–taste memory (Stevenson *et al.*, 1998). In combining tastes and colors no such configural encoding takes place and in this case no resistance to extinction or counter-conditioning is found. Thus, the findings seem to lend support to the idea that resistance to extinction and counter-conditioning is a unique feature of odor–taste memory. It should be remembered, however, that these experiments deal with odor–taste associations and not with pure episodic odor memory.

All earlier mentioned findings about the slow forgetting curve for odors are based on explicit recognition experiments. Moreover, in the majority of them explicit learning instructions were given in the acquisition phase and, although Sulmont (Sulmont, 2000) has demonstrated that intentional learning produces the same results in a recognition test as exposure without learning intention, all of these studies differ considerably from normal everyday learning about odors, as attention was drawn to the odors, because the subjects knew that they were supposed to learn something and because familiar odors were used out of context in a laboratory situation. Often, such research was carried out to study the influence of odor identifiability on retention and recognition, a problem that, although interesting in itself, provides more insight in verbal memory than in odor memory.

There are in fact only five truly implicit odor memory studies in which the subjects were completely left unaware of the purpose of the experiment and in which, during the acquisition phase, they did not even know that they took part in an odor experiment. Baeyens *et al*. (Baeyens *et al*., 1996) scented lavatories or massage oils differently for a few weeks and then asked those who used these lavatories or had been massaged to evaluate the hedonic properties of these odors in an implicit memory test, showing that they had developed affective associations to them. Aggleton and Waskett (Aggleton and Waskett, 1999) invited people who, some years ago, had visited an exhibition in the York Viking museum (UK) that was accompanied by 'Viking' odors, and demonstrated that their memory for the exhibition contents was raised under renewed exposition to these 'Viking' odors. Haller *et al*. (Haller *et al*., 1999) showed that grown-up people (mean age 28.8 years), who as a newborn had been bottle-fed with vanilla-flavored milk, later in life showed a preference for vanillin in a product such as ketchup, whereas people, who were breast-fed when newborn, clearly preferred ketchup without it, thus showing that early exposure to an odor has indeed very long-lasting effects. A similar experiment with children, showing the influence of early exposure to different infant formulas on later food preference, has since then be carried out by Garcia *et al*. (Garcia *et al*., 2001).

Two recent papers (Degel and Köster, 1999; Degel *et al*., 2001) demonstrated the existence of implicit memory for odors with a paradigm in which implicit learning and implicit memory were combined. They exposed their subjects to odor concentrations that were weak enough to remain unnoticed. Later they asked them, in an unrelated experiment, to rate how well odors, among which was the odor that they had been exposed to, would fit to environments, among which was the room in where they had been exposed. As a consequence of the design of the last experiment, in which the subjects were exposed both to an odorous and to an odorless environment, the idea was born to supplement it with conditions, in which the subjects (new ones of course) would be exposed twice to an odor–room combination. Thus, it would be possible to measure the proactive and retroactive effects. The results of this new experiment carried out under rigorously the same conditions as the previous one (double-blind procedure, odor presentation, procedure, debriefing, etc.) are to be presented here, together with those of the groups in the previous experiment (Degel *et al*., 2001), which now serve as controls for the proactive and retroactive effects.

Before describing the experiment in detail it should be noted that the term implicit memory as it is used here, deviates somewhat from the strict sense in which it was defined by Schacter (Schacter, 1987). To Schacter, implicit memory is a facilitation or change of test performance without conscious or deliberate recollection. Here, this definition is only stretched in as far as an increase of the rating of fit is considered to be such a facilitation or change of test performance. The arguments that this occurred without conscious or deliberate recollection have been given in a previous paper (Degel *et al*., 2001).

## Methods

### Participants

A total of 155 subjects, citizens of Dijon, France, 81 men (mean age = 22.6 years, SD = 2.07 years) and 74 women (mean age = 22.4 years, SD = 1.96 years), matched in age and gender to the participants of the previous experiment (Degel *et al*., 2001) ($n$ = 77 men, mean age = 23.0 years, SD = 2.64 years; $n$ = 75 women, mean age = 22.9 years, SD = 2.49 years), who had not yet participated in any of the previous experiments, were recruited by an independent agency per telephone. Chi-square and $t$-tests showed no significant differences in gender distribution or age between the present and the previous group.

The subjects were invited to participate in psychological tests and to take part in an experiment on basic taste appreciation. The agency was also asked to provide new interviewers with experience in psychological testing. As in the previous experiment, these interviewers were left unaware of the presence of odors. The subjects were randomly split into four groups of ~40 (groups 1, 4, 7 and 10) according to the test design displayed in Table 1. The other groups in Table 1 (groups 2, 3, 5, 6, 8, 9, 11 and 12) represent the old groups of the previous experiment, which comprised a maximum of 20 subjects each. Some of the groups were not filled, because a few people did not appear (see Table 1). At the end of the experiment the subjects were paid FF150 (for their participation by the external agency). After the experiments, 14 subjects (four from the previous experiment and 10 of the new subjects) were omitted from the analysis of the main data set, because an extensive debriefing at the end of the experiment revealed that they might have been aware of the fact that there was an odor in one of the test rooms. All others remained unaware of the presence of an odor even after extensive questioning. The distribution of this 'remaining group' is also given in Table 1. Again there were no significant differences in age and gender distribution between the present and the previous groups (present group men: $n$ = 75, mean age = 22.7, SD = 2.08, women: $n$ = 70, mean age = 22.4, SD = 1.96; previous group men $n$ = 76, mean age = 23.0, SD 2.66, women: $n$ = 72, mean age = 22.9, SD = 2.47).

### Test rooms

The same three rooms as in the previous experiment were used as test rooms. Rooms A and B were used for the odor

**Table 1** Experimental groups and test sessions with mean age and number of subjects per group, and the remaining subjects in each group after the 14 subjects who had been aware of the presence of an odor in the building were taken out

| Factor | Group | Test session 1 | Test session 2 | Total group | | Remaining subjects | |
|---|---|---|---|---|---|---|---|
| | | | | Mean age *n* male | Mean age *n* female | Mean age *n* male | Mean age *n* female |
| Different room—same odor | 1 | RaLa | RbLa | 23.1 *n* = 19 | 22.9 *n* = 20 | 23.1 *n* = 19 | 22.9 *n* = 19 |
| | 2 | RaLa | RcCo | 24.2 *n* = 10 | 24.3 *n* = 10 | 24.2 *n* = 10 | 24.3 *n* = 10 |
| | 3 | RcCo | RbLa | 23.8 *n* = 8 | 23.9 *n* = 10 | 23.8 *n* = 8 | 23.9 *n* = 10 |
| | 4 | RbOr | RaOr | 22.6 *n* = 20 | 21.8 *n* = 20 | 22.7 *n* = 19 | 22.7 *n* = 19 |
| | 5 | RbOr | RcCo | 23.4 *n* = 8 | 23.1 *n* = 9 | 23.4 *n* = 8 | 23.1 *n* = 9 |
| | 6 | RcCo | RaOr | 21.7 *n* = 9 | 22.0 *n* = 10 | 21.7 *n* = 9 | 22.0 *n* = 10 |
| Different odor—same room | 7 | RaOr | RaLa | 21.8 *n* = 21 | 21.6 *n* = 18 | 21.9 *n* = 19 | 21.6 *n* = 16 |
| | 8 | RaOr | RcCo | 24.1 *n* = 10 | 24.1 *n* = 10 | 24.1 *n* = 10 | 24.1 *n* = 10 |
| | 9 | RcCo | RaLa | 22.4 *n* = 12 | 22.1 *n* = 8 | 22.4 *n* = 12 | 21.3 *n* = 6 |
| | 10 | RbLa | RbOr | 23.1 *n* = 21 | 22.3 *n* = 16 | 22.9 *n* = 18 | 22.3 *n* = 16 |
| | 11 | RbLa | RcCo | 21.8 *n* = 9 | 21.7 *n* = 10 | 21.8 *n* = 9 | 21.9 *n* = 9 |
| | 12 | RcCo | RbOr | 22.9 *n* = 11 | 22.0 *n* = 8 | 22.9 *n* = 10 | 22.0 *n* = 8 |
| | All | | | 22.8 *n* = 158 | 22.5 *n* = 149 | 22.8 *n* = 151 | 22.7 *n* = 142 |
| | | Total | | *n* = 307 | | *n* = 293 | |

R = room, Or = orange, La = lavender, Co = control (no odor).

exposures and were laboratory rooms different in form, appearance and furniture. Room C, a circular conference room with large windows, was used in the odor-free control condition. The differences between the rooms were necessary to make them recognizable in the third phase of the experiment, where the implicit memory of the odor–room combinations was tested.

### Odors

The same odors and concentrations as in the previous experiment were used. Orange (Sweet orange, brasilia, citrus aurantium dulcic) and Lavender (Lavender, Mont Blanc, France, Lavandula angustifolia) were chosen as the odors in the experimental rooms. According to Sulmont *et al.* (Sulmont *et al.*, 2002) and Degel and Köster (Degel and Köster, 1999) both odors can be identified by just over half of the French population. The concentrations were chosen just above detection threshold to make sure that only very few of the subjects would consciously notice them. In the previous experiment this was checked by extensively debriefing eight persons immediately after they had been in the odorized rooms during at least 10 min for normal business. None of them had noticed the odor. The odors were injected into the ventilation system of the room with short pulses at regular 5-min intervals in order to prevent complete adaptation to them. After each session the rooms were aired and when necessary the odor was changed according to the schedule. This took 5 min and when new groups entered after 30 min odor equilibrium was supposed to be re-established.

For the rating of fit, pleasantness and familiarity, 12 jars, equal in size and color marked by a random three-digit code, were presented at the end of the experiment. As in the previous experiment, 11 of these jars contained each a different odor in a weak, but supra-threshold concentration and one jar (control) contained no odor at all (see Table 2). Subjects were told that each jar contained an odor, although sometimes in such a weak concentration that they might not

**Table 2** Comparison of the data on pleasantness, familiarity and correct identification of the 152 subjects in the previous experiment (Degel *et al.*, 2001) and the 155 new subjects in the present experiment

| Odor name | Pleasantness[a] | | | | Familiarity[a] | | | | Correct identification[b] | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Previous | | Present | | Previous | | Present | | Previous (%) | Present (%) |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD | | |
| Lavender | 57.0 | 30.1 | 58.9 | 28.6 | 69.1 | 29.9 | 70.1 | 28.1 | 57.2 | 59.4 |
| Aftershave | 55.4 | 22.3 | 56.2 | 23.7 | 64.9 | 25.6 | 59.9 | 27.0 | 9.9 | 9.0 |
| Cedarwood | 34.2 | 25.7 | 35.9 | 25.4 | 45.7 | 32.7 | 45.0 | 29.9 | 2.6 | 6.5 |
| Jasmine | 30.5 | 27.5 | 35.7 | 26.7 | 34.5 | 30.8 | 35.4 | 29.5 | 2.6 | 1.9 |
| Coffee | 38.7 | 33.8 | 40.4 | 33.9 | 66.9 | 36.1 | 66.0 | 36.1 | 63.2 | 67.1 |
| Laundry | 52.9 | 25.6 | 53.1 | 26.3 | 50.1 | 31.9 | 49.3 | 31.1 | 21.7 | 18.7 |
| Leather | 26.0 | 25.0 | 29.2 | 25.0 | 37.1 | 32.6 | 35.3 | 31.2 | 17.1 | 16.8 |
| Sandalwood | 37.3 | 26.7 | 44.1 | 25.3 | 32.5 | 28.7 | 35.6 | 27.7 | 2.6 | 1.9 |
| Thyme | 25.2 | 27.1 | 27.7 | 26.0 | 34.2 | 31.5 | 34.6 | 30.6 | 5.9 | 6.5 |
| Orange | 72.1 | 22.6 | 71.7 | 24.5 | 71.1 | 26.3 | 73.6 | 25.6 | 38.2 | 47.4 |
| Peach | 73.0 | 24.6 | 75.8 | 21.5 | 63.6 | 28.6 | 61.4 | 30.7 | 8.6 | 9.7 |
| Control | 62.4 | 23.2 | 59.2 | 19.8 | 67.7 | 30.1 | 65.4 | 30.9 | 75.7 | 75.5 |

smell anything. The positions of the jars in the presentation series for the rating were systematically varied over all subjects.

None of the data for the subjects in the previous experiment and the subjects in the present experiment were significantly different, with the exception of the pleasantness judgements for sandalwood [$T(303) = 2.258$, $P = 0.025$]. For the difference in the numbers of identifiers of the experimental odor orange, a Chi square value of $P = 0.10$ was found. As in the previous experiment, there was no significant difference in the percentage of men and women that could identify either of the two experimental odors.

### Visual materials

In the rating of fit phase, the same 12 pictures as in the previous experiment were used, showing the three test rooms and different surroundings from everyday life (the counter hall of a bank, an office with an empty desk, the women's department of a clothing store, experimental room A, a canteen room, a train lavatory, a kitchen. experimental room B, an office with a crowded desk, a large train compartment, a bank advisory room and the control room C). None of them contained a visual cue for an odor. As in the previous experiment, the above sequence of pictures was kept constant for all subjects and was chosen at random with the restriction that the pictures of the test rooms were at the same distance from each other (positions 4, 8 and 12).

### Test material

During the odor exposure a letter counting concentration test and then a mathematical test were administered. In the two sessions in which the subjects participated, different versions of these tests were used. As the results of these tests have been combined with those of the subjects of the previous experiment and will be submitted for publication separately, they will not be described here.

### Procedure

The procedure was exactly the same as in the previous experiment, except for the fact that the new subjects were exposed to odor twice (see Table 1, groups 1, 4, 7 and 10), instead of once to an odor and once to the non-odorous control condition. For a full description of the procedure see Degel *et al.* (Degel *et al.*, 2001), but here only the main points of the procedure are highlighted. Again a complete double-blind procedure was used, leaving both the subjects and the experimenters unaware of the true purpose of the experiment and about the fact that odors were present. When extensively debriefed at the end of the experiment, all, except 10 of the 155 new subjects (four of the 152 in the previous experiment), did not remember having smelled the experimental odors anywhere in the building, including the room they had been exposed in. The same was true for the recruited experimenters, who also were convinced that psychological testing was the only purpose of their part of the experiments.

When they arrived, the subjects of a group were assigned for performance testing to a room that was scented with either an ambient odor [Lavender (La) or Orange (Or)] or no odor [Control (Co)] respectively (test session 1; see Table 1). The subjects were not told that odors played a part in the study or that odors were present in the rooms. They were told that the psychological tests for which they had been invited, were divided over two equivalent sessions to check their reliability and that they would participate in the experiment on basic taste appreciation between these two sessions and after the second session.

The subjects participated in groups with a maximal size of 10. After meeting the interviewer, the group was taken to the test room and had to wait for 5 min before the interviewer gave an instruction and started the test. The next test started exactly 5 min. after completion of the first one. The total duration of the tests, including the initial 5 min waiting time and the two instructions (2 min each) as well as the 5 min break between the two tests, was 30 min.

After the first part of the psychological tests, the subjects were collected in the hall by another experimenter to take part in a yogurt tasting experiment. Here they had to indicate their liking for the sweetness of a series of the same yogurts that were just noticeably different in sweetness. In this procedure, that served as a filled interval equal for all subjects, there was nothing (neither in the food nor in the environment) that drew the attention to odor or flavor. After this, they performed the second part of the psychological test (test session 2, Table 1). In order to control for interviewer effects, the three interviewers were rotated systematically over the subjects in the four experimental groups. Thus, each interviewer in the exposure conditions saw an almost equal number of men and women in each group and in each test room.

After the second part of the psychological tests, the subjects were again collected by the experimenter of the taste experiment, but when the subjects then returned after the second taste experiment, they were collected by still another experimenter, who took them to another floor of the building. There, they were told that there was a trend in the market for the use of odors in different environments and they were asked to help in 'finding odors that would fit well to different environments' (12 pictures among which were those of the two rooms they had been in during the two phases of the psychological testing experiment). For this rating the subjects were seated in groups of 10 maximum, separated by side walls in front of a screen on which the images of different contexts were projected. They each had a set of 12 jars in front of them. The subjects were instructed to rate how well each odor did fit in each of the contexts shown. The rating was made on a 100 mm visual analog scale with the end labels 'does not fit' and 'fits'. After rating the fit of all odors to a given context, a new context was shown on the screen. In order to reduce olfactory adaptation a pause of 45 s was made before a new visual context was shown.

At the end of this session and after a pause of 5 min, the subjects were asked to rate the 12 odors for pleasantness and familiarity on a 100 mm visual analog scale with the end labels 'very unpleasant' and 'very pleasant' or 'very unfamiliar' and 'very familiar' respectively.

Then they were asked to identify the odors and they were debriefed extensively. For odor identification only an exact definition of an odor name (lavender, orange or no odor) was counted as a correct answer. Near veridical labels (tangerine or citrus for orange, bed linen for lavender) were not accepted. This did not pose a real problem, because such near veridical labels were very rare (<2% as in the previous experiment). In the debriefing the subjects were explicitly asked 'When and where did you smell this odor last' and 'Did you not smell it today elsewhere in this building?'. After this debriefing, the subjects filled out a questionnaire about the vividness of their odor imagination [extended version on odor of Sheehan (Sheehan, 1967)].

### Statistical analysis

The analysis was performed with SPSS for Windows, Version 6.1.3 (SPSS, Inc., 1994). For the rating of fit, as in the previous experiment, normalized means were calculated for the new subjects. Normalization was performed by dividing the rating of fit for each individual odorant–context combination by the mean of the 12 contextual ratings for that same odor by the same subject. Thus, values < 1.00 express a rating below the average, and values > 1.00 a rating above the average rating for that odor. The normalization served two purposes. In the first place, it reduced the variance in the data that was due to the different scaling behavior of the subjects, some of whom used the high end of the scale, whereas others used only low values. Secondly, and related to the first point, it gave an equal weight to each of the participants irrespective of their use of high or low numbers. By expressing the individual ratings in units based on their mean, the relative position of the judgements remains unaffected. Nevertheless, as this type of normalization has the disadvantage that people, who on average give high ratings, are less likely to get normalized ratings that deviate strongly from 1.00 than people who frequently give low ratings, the average rating levels of the identifier group and the non-identifier group were compared as a control measurement. No significant differences between the mean rating levels or the SDs of these two groups were found. For the ratings of pleasantness and familiarity the means per odor were calculated over the new subjects and are given along with those from the previous experiment in Table 2. As indicated above, for identification the number of veridical labels was counted and a percentage of correct identifications was calculated (Table 2). An additional analysis in which the few persons who produced 'near veridical' labels were moved to the identifier group showed no difference in the results.

In order to estimate the influence of interference on the implicit memory as shown by the ratings of fit of the odors to the rooms, several comparisons were made.

In the first place the results of retroactive experimental groups (RAEG) in which exposure to a given odor–room combination was followed by exposure to another partially overlapping odor–room combination (either the same odor in another room or another odor in the same room) were compared with the results of groups in which the same given odor–room combination was followed by exposure to the non-odorous control room [retroactive control group (RACG)]. In the second place, the results of the RAEG and RACG were compared with the ratings made by people who had never been exposed to the given odor–room combination that was shared by the RAEG and RACG, a so-called non-exposed control group (NECG). In the left-hand columns in Table 3, the data obtained by non-identifiers and identifiers in four RAEGs (groups 1, 4, 7 and 10) are presented for direct comparison with the results in four RACGs (groups 2, 5, 8 and 11) and four NECGs (groups 2, 5, 8 and 11). It should be noted that in some comparisons the ratings of fit to different odor–room combinations of the same groups have been used. Thus, the result of group 5 for the rating of fit in the combination of room A and the Odor of Lavender (RaLa) has been used as a NECG in the comparison with RAEG 1, whereas the result of the same group 5 obtained for the combination RbOr serves as a RACG in the comparison with the RAEG 4.

For the measurement of the proactive interference similar comparisons were made. The results of proactive experimental groups (PAEG), in which the exposure condition was preceded by another exposure condition, were compared with the results of proactive control groups (PACG), in which exposure was preceded by the non-odorous control condition, and with the results of NECGs who had never been exposed to the same odor–room combination as the PAEG and the PACG. In the left-hand columns in Table 4, the data obtained by non-identifiers and identifiers in four PAEGs (groups 1, 4, 7 and 10) are presented for direct comparison with the results in four PAEGs (groups 3, 6, 9 and 12) and four NECGs (groups 3, 6, 9 and 12) It should be noted again that the same groups served as RAEG and as PAEG, but that in the retroactive comparisons their results from the first session were used and that in the proactive comparisons their results of the second session were used.

## Results

For the sake of clarity, in this section first the detailed results of the experiment are described both for retroactive and for proactive interference. Then the analysis of the results in terms of the statistical significance of the described effects is given.

### Retroactive interference: description of the results

The detailed results obtained for the retroactive effects by the non-identifiers and the identifiers are given in Table 3. This table is divided into two types of exposure condition, one in which the experimental group received the same odor in different rooms and one in which the experimental groups

**Table 3** Retroactive interference comparisons of the mean ratings of fit for the non-identifiers and the identifiers in the retroactive experimental groups (RAEG), the retroactive control groups (RACG) and the non-exposed control groups (NECG) for the odor–room combination to which the RAEG was exposed in the first session

| Group | Exposure condition | Type of group | Combination rated for fit | Non-identifiers | | Identifiers | |
|---|---|---|---|---|---|---|---|
| | | | | Mean | SD | Mean | SD |
| Same odor–different room | | | | | | | |
| Group 1 | RaLa–RbLa | RAEG | RaLa | 0.67 | 0.50 | 0.62 | 0.57 |
| Group 2 | RaLa–RcCo | RACG | RaLa | 1.71 | 1.91 | 0.45 | 0.31 |
| Group 5 | RbOr–RcCo | NECG | RaLa | 0.46 | 0.43 | 0.36 | 0.24 |
| RACG(2)–RAEG(1) | | | | 1.04 | | –0.17 | |
| NECG(5)–RAEG(1) | | | | –0.21 | | –0.26 | |
| Group 4 | RbOr–RaOr | RAEG | RbOr | 0.77 | 0.69 | 0.86 | 0.65 |
| Group 5 | RbOr–RcCo | RACG | RbOr | 1.26 | 0.85 | 0.61 | 1.06 |
| Group 2 | RaLa–RcCo | NECG | RbOr | 0.80 | 0.65 | 0.70 | 0.90 |
| RACG(5)–RAEG(4) | | | | 0.49 | | –0.25 | |
| NECG(2)–RAEG(4) | | | | 0.03 | | –0.17 | |
| Same room–different odor | | | | | | | |
| Group 7 | RaOr–RaLa | RAEG | RaOr | 0.63 | 0.50 | 0.72 | 0.86 |
| Group 8 | RaOr–RcCo | RACG | RaOr | 0.75 | 0.48 | 0.10 | 0.05 |
| Group 11 | RbLa–RcCo | NECG | RaOr | 0.36 | 0.47 | 0.66 | 0.43 |
| RACG(8)–RAEG(7) | | | | 0.12 | | –0.62 | |
| NECG(11)–RAEG(7) | | | | –0.27 | | –0.06 | |
| Group 10 | RbLa–RbOr | RAEG | RbLa | 0.86 | 0.65 | 1.03 | 0.88 |
| Group 11 | RbLa–RcCo | RACG | RbLa | 1.15 | 0.27 | 0.60 | 0.70 |
| Group 8 | RaOr–RcCo | NECG | RbLa | 0.86 | 1.18 | 0.46 | 0.51 |
| RACG(11)–RAEG(10) | | | | 0.29 | | –0.43 | |
| NECG(8)–RAEG(10) | | | | –0.01 | | –0.57 | |

received different odors in the same room. The retroactive effects are shown by the differences between the RAEGs and the RACG on the one hand and the NECGs on the other. When the difference between RACG and RAEG is positive, this may indicate that there is a retroactive effect. When the difference between NECG and RAEG is positive, this may indicate that the retroactive effect does not completely annihilate the implicit memory of the first session. When the latter difference is negative, this may indicate that the retroactive effect completely annihilates the implicit memory and that even retroactive enhancement takes place.

#### Proactive interference: description of the results

With regard to the measurement of possible proactive interference, similar comparisons have been made, but this time the PAEG were those in which exposure to a given odor–room combination was preceded by exposure to another combination in which either the same odor was given in another room or another odor was given in the same room. The PACGs were those in which the same given odor–room combination was preceded by the odorless control condition and the NECG were those who had not been exposed to either the odor or the room of the given combination. The results of these groups are given in the left-hand columns in Table 4.

The proactive effects are shown by the differences between the PAEGs and the PACGs on the one hand and the NECGs on the other. When the difference between PACG and PAEG is positive, this may indicate that there is a proactive effect. When the difference between NECG and PAEG is positive, this may indicate that the proactive effect does not completely annihilate the implicit memory of the second session. When this latter difference is negative, this may indicate that the proactive effect completely annihilates this implicit memory and even that proactive enhancement took place.

#### Retroactive interference: analysis of the effects.

In order to check for retroactive interference effects a $3 \times 2 \times 2 \times 2$ ANOVA (group: experimental, exposed control, non-exposed control × identification: non-identifier, identifier × room Ra, Rb × odor La, Or) was carried out.

Main effects were found for the factors identification [$F(1,271) = 7.20$, $P < 0.01$], the non-identifiers having a higher rating of fit than the identifiers (non-identifiers: mean = 0.81, SD = 0.80; identifiers: mean = 0.67, SD = 0.70) and room [$F(1,271) = 4.63$, $P < 0.05$], the rating of fit for room A being lower than the rating of fit for room B (A: mean = 0.65, SD = 0.80; B: mean = 0.84, SD = 0.77).

More important in the context of this paper, was the finding of a two-way interaction for the factors, group and identification [$F(2,271) = 7.21$, $P < 0.01$]. The effects of

**Table 4** Proactive interference comparisons of the mean ratings of fit for the non-identifiers and the identifiers in the proactive experimental groups (PAEG), the proactive control groups (PACG) and the non-exposed control groups (NECG) for the odor–room combination to which the PAEG was exposed in the second session

| Group | Exposure condition | Type of group | Combination rated for fit | Non-identifiers | | Identifiers | |
|---|---|---|---|---|---|---|---|
| | | | | Mean | SD | Mean | SD |
| Same odor–different room | | | | | | | |
| Group 1 | RaLa–RbLa | PAEG | RbLa | 1.04 | 0.76 | 0.76 | 0.68 |
| Group 3 | RcCo–RbLa | PACG | RbLa | 2.03 | 1.11 | 1.28 | 1.46 |
| Group 6 | RcCo–RaOr | NECG | RbLa | 0.80 | 0.76 | 0.73 | 0.47 |
| PACG(3)–PAEG(1) | | | | 0.99 | | 0.52 | |
| NECG(6)–PAEG(1) | | | | –0.24 | | –0.03 | |
| Group 4 | RbOr–RaOr | PAEG | RaOr | 0.71 | 0.80 | 0.95 | 1.41 |
| Group 6 | RcCo–RaOr | PACG | RaOr | 1.00 | 0.59 | 0.54 | 0.43 |
| Group 3 | RcCo–RbLa | NECG | RaOr | 0.50 | 0.48 | 0.45 | 0.63 |
| PACG(6)–PAEG(4) | | | | 0.23 | | –0.33 | |
| NECG(3)–PAEG(4) | | | | –0.27 | | –0.42 | |
| Same room–different odor | | | | | | | |
| Group 7 | RaOr–RaLa | PAEG | RaLa | 0.94 | 0.64 | 0.61 | 0.61 |
| Group 9 | RcCo–RaLa | PACG | RaLa | 1.19 | 0.42 | 0.77 | 0.84 |
| Group 12 | RcCo–RbOr | NECG | RaLa | 0.73 | 0.42 | 0.95 | 0.78 |
| PACG(9)–PAEG(7) | | | | 0.25 | | 0.16 | |
| NECG(12)–PAEG(7) | | | | –0.21 | | –0.34 | |
| Group 10 | RbLa–RbOr | PAEG | RbOr | 0.84 | 0.76 | 1.30 | 1.10 |
| Group 12 | RcCo–RbOr | PACG | RbOr | 1.36 | 0.85 | 0.94 | 0.66 |
| Group 9 | RcCo–RaLa | NECG | RbOr | 0.98 | 0.65 | 1.26 | 1.10 |
| PACG(12)–PAEG(10) | | | | 0.52 | | –0.36 | |
| NECG(9)–PAEG(10) | | | | 0.14 | | –0.04 | |

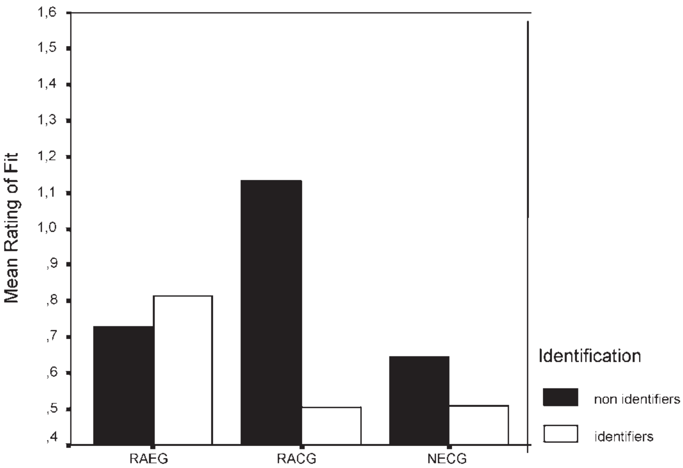


**Figure 1** Analysis of the two-way interaction between experimental condition and odor identification for retroactive interference. RAEG = retroactive experimental group; RACG = retroactive control group; NECG = non-exposed control group.

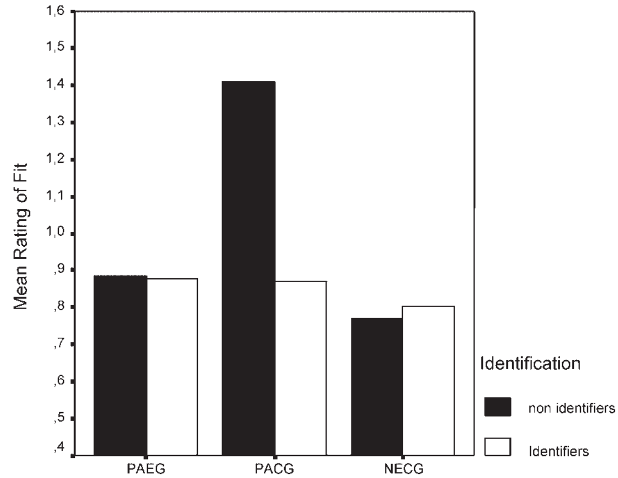


**Figure 2** Analysis of the two-way interaction between experimental condition and odor identification for proactive interference. PAEG = proactive experimental group; PACG = Proactive control group; NECG = non-exposed control group.

group on the results of the non-identifiers and the identifiers are shown in Figure 1.

*Non-identifiers*

Inspection of the two-way interaction (see Figure 1) showed that for the non-identifiers, the exposed control groups RACG (mean = 1.13, SD = 1.05) had a higher rate of fit (Bonferroni, $P = 0.014$) than the NECGs (mean = 0.64, SD = 0.77), which indicated that implicit memory was found. Furthermore, the RACG group had also a higher rating of fit (Bonferroni, $P = 0.025$) than the experimental RAEG group (mean = 0.73, SD = 0.58), which indicated that the retroactive effect was significant. There was a small difference between RAEG and NECG, but this difference was not significant at all (Bonferroni, $P = 1.0$), indicating that the retroactive interference was probably complete.

*Identifiers*

For the identifiers there was no difference between RACG (mean = 0.50, SD = 0.66 and NECG (mean = 0.51, SD = 0.52) and hence no implicit memory could be demonstrated, whereas the RACG was even lower, although not significantly (Bonferroni, $P = 0.098$) than the RAEG (mean = 0.81, SD = 0.76), indicating a tendency towards retroactive enhancement rather than retroactive interference. There was a difference between RAEG and NECG in favor of RAEG, indicating retroactive enhancement rather than interference, but this difference failed to reach significance (Bonferroni, $P = 0.111$).

**Proactive interference: analysis of the effects**

For the analysis of the retroactive effects, a 3 × 2 × 2 × 2 ANOVA (group experimental, control exposed, control non-exposed × non-identifier, right × room A, b × odor La, Or) was conducted to analyze effects of proactive interference.

A main effect was found for the factor group [$F(2,267) = 3.24$, $P < 0.05$]. Analysis of the means showed that the exposed control group PACG had a higher mean than the two others (experimental PAEG: mean = 0.88 SD = 0.89; exposed control PACG: mean = 1.11, SD = 0.93; NECG: mean = 0.79, SD = 0.66), but Bonferroni testing showed only a significant difference between PACG and NECG ($P = 0.05$). Another main effect was found for the factor room [$F(1,267) = 10.15$, $P < 0.01$], the rating of fit for room B being higher than for room A (A: mean = 0.77, SD = 0.79; B: mean = 1.07, SD = 0.89) as in the results for retroactive interference.

Here also the most important finding in the context of this paper was the two-way interaction for the factors group and identification [$F(2,271) = 7.21$, $P < 0.01$]. Again, the effects of group on the results of the non-identifiers and the identifiers, respectively, are of special interest in this connection.

*Non-identifiers*

Inspection of the two-way interaction (see Figure 2) showed that for the non-identifiers, the exposed control groups PACG (mean = 1.41, SD = 0.86) had a higher rate of fit (Bonferroni, $P = 0.001$) than the NECG (mean = 0.77, SD = 0.60), which indicated that implicit memory was found. Furthermore, the PACG groups had also a higher rating of fit (Bonferroni, $P = 0.003$) than the experimental PAEG (mean = 0.88, SD = 0.75) groups, which indicated that the proactive interference effect was significant. There was no difference between PAEG and NECG, indicating that the proactive interference was probably complete (Bonferroni, $P > 0.999$).

*Identifiers*

For the identifiers there was no difference (Bonferroni, $P > 0.999$) between PACG (mean = 0.87, SD = 0.91) and NECG (mean = 0.80, SD = 0.73) and hence no implicit memory could be demonstrated, while there was no difference between PACG and RAEG (mean = 0.88, SD = 1.00), indicating that no proactive interference took place. The small difference between PAEG and NECG, in favor of PAEG was not significant at all (Bonferroni, $P > 0.999$), indicating that previous exposure in no way influenced the results.

## Discussion

Both proactive and retroactive interference are found in implicit odor memory. That such interference is only present in the non-identifiers and not in the identifiers is no surprise, as it was already shown in the previous experiments (Degel and Köster, 1999; Degel *et al*., 2001) that only the non-identifiers show implicit memory for the combination of odor and room to which they have been exposed. Although these results were based on post-hoc analyses, the convergence of the data from three independent experiments in which odors were used that differed considerably in the degree of identifiability, strongly supports the finding that knowing the name of an odor disturbs the formation of a new implicit association with it, or may lead to a more rapid extinction of such a memory (Degel *et al*., 2001). Although the retroactive interference effects seem to be somewhat weaker than the proactive effects, as they do not completely annihilate the memory effects, whereas the proactive effects seem to prevent the formation of any new link, the longevity of olfactory memory in the form studied here, cannot be explained by their complete absence as was supposed by Lawless and Engen (Lawless and Engen, 1975), Engen (Engen, 1991) and Herz and Engen (Herz and Engen, 1996).

The results are in good agreement with those of Walk and Johns (Walk and Johns, 1984), who also found evidence for retroactive interference in episodic short-term memory for odors using a recognition method. On the other hand, they are in disagreement with those of Lawless and Engen (Lawless and Engen, 1977), who found no evidence for retroactive interference and with those of Baeyens *et al*. (Baeyens *et al*., 1995) and Stevenson *et al*. (Stevenson *et al*., 2000a) who found resistance to extinction and to counter-conditioning (Stevenson *et al*., 2000b). It should be remembered, however, that these experiments did not test episodic memory for the odor itself, but either tested the memory for arbitrary associations between odors and pictures that were explicitly learned (Lawless and Cain, 1975) or tested the effects of intensive odor–taste conditioning on the verbal description of odor (Stevenson *et al*. 2000a,b) or taste perception (Stevenson *et al*., 1999). In the study of Lawless and Engen (Lawless and Engen, 1977), the use of the mediator schemes raised the percentage correct choices to 53.3%, whereas without such a mediator the percentage of correct choices (13.8%) was probably not even significantly different from chance guessing in a 1 of 12 alternative forced choice (AFC) task (8.3%). This means that the odors were not remembered as such, but just functioned as signals in the memory for cognitive constructs. Therefore, the difference between proactive and retroactive interference may have been due to the difficulty in constructing these successive constructs. In the experiments of Baeyens *et al*. (Baeyens *et al*., 1995) and Stevenson *et al*. (Stevenson *et al*., 2000a), which showed that conditioned odor–flavor associations were resistant to extinction, whereas color–flavor associations were not, the measurements of the effects were based on pre-test and post-test differences in judgements of odor liking and sensory properties, respectively, or on differences in post-conditioning sweetness or sourness expectation scores. Episodic memory of the odor itself was never involved in these judgements. Only in one experiment by Stevenson *et al*. (Stevenson *et al*., 2000a) episodic memory for the odor was tested by an additional frequency test. In this test, at the end of the experiment, the subjects tasted all the combinations again and were asked to rate how frequently they thought each given combination of odor or color with sucrose, citric acid or water had occurred during the experiment. This test of episodic memory showed very different results from the other two response methods. Here, the subjects estimated the frequency of the odor–taste combinations very well and also showed clearly the effects of the extinction trials in their estimation. It seems, therefore, that where episodic memory is concerned configural encoding does not necessarily promote insensitivity to extinction or retroactive interference as Stevenson *et al*. (Stevenson *et al*., 2000a,b) and other authors (Shanks *et al*., 1998) have proposed. The fact, that no retroactive interference was found in the experiment of Lawless and Engen (Lawless and Engen, 1977), where there was certainly no question of configural encoding, may also cast some doubt on the value of configural encoding as the sole explanation of the absence of interference.

Moerover, some aspects of the odor–taste experiments, such as the use of taste terms in the characterization of odors and the limitation of response possibilities have been criticized. It has been shown (Frank *et al*., 1993; Clark and Lawless, 1994; Van der Klaauw and Frank, 1996) that giving more (or more adequate) response possibilities than just sour or sweet may reduce odor–taste enhancement considerably and may even turn it into sweetness or sourness suppression. Furthermore, Bingham *et al*. (Bingham *et al*., 1990) demonstrated that odor–taste enhancement was not found in trained panels and more recently Prescott (Prescott, 1999) showed that the perceptual approach (analytical or synthetic) of the subjects played an important part in the enhancement phenomena. These and other arguments, such as the verbal non-equivalence of the relationships between odor and taste on the one hand and color and taste (sweet, sour or bitter reds, greens and blues?) on the other, point at a strong influence of verbal mediation in these experiments, which was absent in the Walk and Johns (Walk and Johns, 1984) study and in the present experiment. Thus, the non-episodic nature of the material to be remembered and the influence of semantic factors in the encoding seem to be better candidates for an explanation of the resistance to extinction and interference than perceptual configural encoding.

Nevertheless, other factors, such as the stress laid on learning and the time allowed for consolidation of the memory, might be invoked to explain the differences between the experiments. The experiments that find resistance to interference have given much more learning exposure (and in one case even feedback) than the two experiments that find retroactive interference. Therefore, it might be argued that the exposition to the odors in the latter two was either too short (Walk and Johns, 1984) or too weak (present experiment) to build up an effective resistance to retroactive interference. However, as in both these experiments the existence of an episodic memory for the odors was clearly established and, at least in the case of Degel *et al*. (Degel *et al*., 2001), it persisted almost without any change over the last 60 min of a 2 h retention period, such an explanation must be rejected.

Could differences in the retention phase of the experiments explain the differences in the results? The experiments differed considerably both in the times between learning and interference and in the times between interference and retrieval. Especially in the experiment of Walk and Johns (Walk and Johns, 1984), the time between learning and interference was very short (12 s) compared with that in the other experiments with the exception of the experiment of Baeyens *et al*. (Baeyens *et al.*, 1995) (20 s). Walk and Johns (Walk and Johns, 1984) based their choice of the interval on the results of Engen *et al*. (Engen *et al*., 1973), who had shown that the accuracy of short-term memory increased when the interval was increased from 3 to 12 s, but remained stable at longer intervals up to 30 s. Thus, it seems that any objections to the use of such a short interval might also partly apply to the results of Baeyens *et al*. (Baeyens *et al*., 1995), who found resistance to extinction. In the present experiment, the interval (1 h) was certainly not too short to permit the full development of the memory. In three of the other experiments, much longer intervals [48 h in Lawless and Engen (Lawless and Engen, 1977) and 24+ h in Stevenson *et al*. (Stevenson *et al*., 2000a,b)] between learning and interference were used, allowing time for consolidation of the memory during sleep (Stickgold, 1998), but as Baeyens *et al*. (Baeyens *et al*., 1995) used a short interval and also found resistance to extinction, it seems unlikely that this has been a major cause for the difference between the experiments in which the resistance was found and the two experiments that showed retroactive interference. The same can be said about the differences in interval between the interference and retrieval. In most studies this interval was extremely short. Only in two cases, Lawless and Engen (Lawless and Engen, 1977) (14 days) and the present experiment (1 or 2 h) was the interval longer than 20 s. In one of these two cases, retroactive interference was found and in the other it was not. The same was true in the other group of experiments, where Walk and Johns (Walk and Johns, 1984), who, like Baeyens *et al*. and Stevenson *et al*., used immediate retrieval and showed retroactive interference, while the others showed resistance to extinction and interference.

The nature of the interfering condition or task might also be an important factor in the explanation of the differences between the experiments. In the two experiments that found retroactive interference, the same condition as during the learning phase was repeated with another odor or other odor–room combination, which directly interfered with the episodic aspects of the situation, whereas in the other experiments the odors remained the same and the picture or taste with which they were paired, changed. This, combined with the fact that the learning in these latter experiments almost certainly was partly based on semantic aspects of the stimuli, may have led to a mental separation of the learning and the interference phases in the subjects. For the experiment of Lawless and Engen (Lawless and Engen, 1977) with its strong influence of mediational schemes this is evident, but it may also have played a major role in the Baeyens *et al*. (Baeyens *et al.*, 1990, 1995) and Stevenson *et al*. (Stevenson *et al*., 1995, 1998, 1999, 2000a,b) experiments. Once the subjects had consciously learned to connect the odor with liking or sweetness or sourness—and by having to rate these properties they did so consciously—they may have perceived the water or the counter-conditioning taste of the interfering stimuli as additions that spoiled the originally learned taste of the odor, but did not change it. This seems quite a normal coping mechanism to keep the sensory world intact. That the same mechanism does not work for color–taste combinations is no objection to this idea. As was pointed out above, colors and tastes cannot be semantically integrated in the same way as odors and tastes. There are no sour or sweet colors in normal life. Red strawberries or orange oranges may be sour or sweet and bananas and lemon or grapefruit may have the same yellow color but very different sweetness, sourness and bitterness. Furthermore, the link between colors and tastes is certainly not formed by configural encoding, as Stevenson *et al*. propose for the link between odor and taste. It might well be that it is precisely because of this configural aspect that the subjects are invited to use a conscious coping mechanism that preserves this originally learned relationship between odor and taste. Such coping mechanisms are quite common, because it should not be forgotten that odors, through their strong links with specific situations, make us feel at home in this world. This is a basic feeling, that people who become anosmic miss and which in their case often leads to a certain bleakness of memory, to feelings of uneasiness and even to depression (Tennen *et al*., 1991; van Toller, 1999). Others (De Boer *et al*., 1987) have described the use of conscious coping mechanisms with regard to odors and odor pollution and Herz (Herz, 1997) in her discussion of the role of odor cue distinctiveness in context-dependent memory also describes such mechanisms without labeling them as coping. A similar coping mechanism could not be used by the subjects of Stevenson *et al*. (Stevenson *et al*., 2000a) when, in the frequency test, the explicit episodic question was asked how often they had encountered a given odor–taste combination in the experiment. In the experiments of Walk and Johns (Walk and Johns, 1984) and the present experiment, there was also no room for such conscious coping mechanisms. In the first one, it was just the episodic memory of the odor itself that had to be remembered and in the second one, a configural relationship between the odor and the room was to be remembered, but this relationship did not reach the conscious level in the subjects and thus could not give rise to conscious coping mechanisms.

In conclusion, it can be said that there are clear indications that where explicit (conscious) semantic processing is involved in the memory for relationships between odor and other stimuli-like pictures (Lawless and Engen, 1977) or tastes, resistance to extinction and retroactive interference occurs, even when, but not because configural encoding has taken place (Baeyens *et al*., 1995; Stevenson *et al*., 2000a,b). Whether this resistance is due to the rather high degree of verbal and conceptual processing at encoding, to conscious coping mechanisms or to the limitation and artificiality of the retrieval questions cannot be decided yet. Furthermore, it seems reasonable to conclude that the outcomes of the present experiment, the experiment of Walk and Johns (Walk and Johns, 1984) and of the frequency test of Stevenson *et al*. (Stevenson *et al.*, 2000a) show that normal extinction and retroactive interference are found when non-semantic episodic memory for a unitary perceptual experience (an odor–environment combination, an odor alone or an odor–taste combination) is tested.

### Retroactive interference and implicit memory theory

In connection with this last conclusion, the ideas of Graf and Schacter (Graf and Schacter, 1989) on unitization as a necessary prerequisite for implicit memory, that were already discussed in a previous paper (Degel *et al*., 2001), and their ideas on the occurrence of retroactive interference in implicit memory (Graf and Schacter, 1987), should also be mentioned. For a long time these two authors have been interested in the dissociation between implicit and explicit verbal memory for new associations between normatively different words (Graf and Schacter, 1985, 1987; Schacter and Graf, 1986, 1989). In one of their last papers on this topic Graf and Schacter (Graf and Schacter, 1989) came to the conclusion that active 'unitization' (finding a conceptual link between the two words and representing them as a single unit) is a necessary prerequisite for the formation of implicit memory for such associations (Graf and Schacter, 1985; Schacter and Graf, 1986). As it was argued in a previous paper (Degel *et al*., 2001), the results on implicit memory for odor–context combinations could be brought in line with this conclusion if one assumed that such combinations were indeed unitary experiences that needed no further unitization by conceptual processing. As Graf and Schacter (Graf and Schacter, 1989) point out in the same article, 'unitization is thought to occur in two ways, either as a result of perceiving structure or coherence among separate stimulus components or as a result of conceiving a structure for connecting materials that are processed concurrently'. As the relationship between an odor and its source or the context in which it is encountered, is a very special one (see above), the case of 'coherence' mentioned by Graf and Schacter seems applicable. However, in the same paper Graf and Schacter (Graf and Schacter, 1987) cite their earlier work on retroactive interference in which they found retroactive interference in explicit (cued recall), but not in implicit (word completion) memory for word pairs that had been unitized by a procedure in which the subjects had to form a meaningful sentence with the words in a pair. As it was argued in a previous paper (Degel *et al*., 2001) there are considerable differences between the experiments of Graf and Schacter (Graf and Schacter, 1987) and the odor experiments described here. In the first place, they used explicit exposure to the to-be-remembered associations, whereas the odor–room combinations in the experiments described here were presented without the subjects' explicit awareness. In the second place, Graf and Schacter unitized the words by conceptual means, whereas the link between odor and context seems to be more of a perceptual nature. In Degel *et al*. (Degel *et al*., 2001) it was even argued that this link had in principle all the characteristics of the perceptual representation systems (PRS), which were described for visual word form, structural description and auditory word form by Schacter more recently (Schacter, 1990, 1994). According to Schacter, a PRS operates at a pre-semantic level, i.e. at a level of processing that does not involve access to the meanings of words or objects. Thirdly, Graf and Schacter used word completion as their implicit memory test, which has the disadvantage that only finding the associated word is counted as a correct answer, whereas in the present and the previous odor experiments a graded response to a seemingly unrelated question, that did not necessitate any verbal mediation of the memory involved, was measured. As words are their own names and as it has been shown in the present odor experiments that knowing the name of an odor disturbs the implicit memory for it, there might be no essential difference between Graf and Schacter's results and those of the identifiers in the present experiment, who too, showed no retroactive interference, but, admittedly, also showed no strong implicit memory. In this explanation, the fact that the non-identifiers show both implicit memory and retroactive interference also does not pose a problem. In Graf and Schacter's experiments there simply were no non-identifiers. In fact, Graf and Schacters' results seem to fit well in the conclusions drawn on the basis of the analysis of the experiments of Lawless and Engen, Baeyens *et al*. and Stevenson *et al*., because they also relied on explicit semantic processing (constructing a sentence) for unitization and did not test episodic memory. Moreover, their subjects had to find a word in the retrieval test.

Further research, studying implicit memory by comparing verbal and non-verbal stimuli using implicit learning and additional more non-semantic and more episodic memory oriented retrieval tasks than just word completion, would be needed to clarify this discussion. For the time being, it seems that the statements of Graf and Schacter about unitization as a necessary condition for implicit memory and about the absence of retroactive of interference in implicit memory have a more limited application than is sometimes assumed.

### Retroactive interference and the longevity of odor memory

Before concluding this discussion, it is necessary to turn around once more and to scrutinize the present experiment [and that of Walk and Johns (Walk and Johns, 1984)] trying to explain the occurrence of retroactive interference. After all, if retroactive interference does exist in episodic memory for implicitly learned odors, how do we explain the extraordinary longevity of such implicitly learned odor memories in everyday life (Haller *et al*., 1999) and in the explicit learning experiments of Engen and Ross (Engen and Ross, 1973), Lawless and Cain (Lawless and Cain, 1975) and Lawless (Lawless, 1978)? In other words, why do the Walk and Johns (Walk and Johns, 1984) experiment and the implicit learning in the present experiment show retroactive interference, while at the same time odors seem so unforgettable that other distinguished authors, who admittedly did not test retroactive interference, jump to the conclusion that such interference does not exist? The answer is simple and has already been mentioned in a somewhat different context by Herz (Herz, 2000) in a recent paper. Her argument was that it is precisely because odors attach themselves so strongly to the very specific situation in which they are perceived and because these odor–situation combinations are so unique, that there are very few occasions for retroactive interference to occur in normal life. Almost all places in our life (home, office, friends' houses, shops, traditional foods) have their own characteristic smell, which we normally fail to notice, and which hardly ever changes, unless something drastic has happened (we had others stay in our house, the flavor of our favorite brand of marmalade has changed, our office has been cleaned). When this occurs we usually notice it immediately and consciously set it aside as an external event, thus protecting the original link. The series of experiments by the present authors [(Degel and Köster, 1998, 1999; Degel *et al*., 2001) (Degel *et al*., submitted for publication; present paper)] has been set up precisely to demonstrate this intimate and implicit link between odors and the situation in which they are perceived. It quite accidentally showed that once odors were objectified in a semantic form they lost their capacity to build up or sustain such links. As it has been argued elsewhere (Köster, 2000, 2002) the sense of smell, as one of the 'lower' senses, differs in many respects from a 'higher' sense such as vision. Among other things, it is a 'hidden' sense, that often acts more strongly when the odor is not consciously perceived (Köster and Degel, 2001). Treating odors as objective stimuli, just like visual stimuli, may obscure their true meaning. Objectification of odors by training is certainly possible and is widely used by perfumers, flavorists and trained panels in the food industry, but it leads to a deviation from the meaning of odors in everyday life and to a loss of the normal emotional bonds between odors and the environment. Loosening these emotional bonds by forbidding the use of evaluative non-descriptive terms is usually the first step in training and it may well be that loss of emotional meaning is another principal reason why Rabin (Rabin, 1988) found training and profiling to be so effective in learning to discriminate between odors. In naïve subjects odor similitude is mainly determined by similitude in liking as Woskow (Woskow, 1968) showed and such a hedonic factor might lead to more false alarms in the untrained subjects, as indeed Rabin found when he used unfamiliar components in mixtures. Nevertheless, familiarity has certainly an influence on odor discrimination, as Rabin demonstrated. Could it be that familiarity also played a major part in the present series of experiments? Might the occurrence of retroactive interference be due to a weaker memory as a result of low familiarity of the odorants and is this especially true for the non-identifiers, who indeed show a lower familiarity with the odorants? It seems highly unlikely for three reasons: (i) it has been shown in three experiments that the memory is stronger in the non-identifiers than in the identifiers, who rate the odors as more familiar; (ii) both odorants in this and the previous experiment were the most familiar stimuli of the whole range and are well-known odors in normal life; and (iii) in an earlier experiment (Degel and Köster, 1999) the same results on the occurrence of implicit memory were obtained with jasmine, which was an unfamiliar odor, that was identified only by one person of a group of 108 subjects and that in the last two experiments also had a low familiarity and identification score (see Table 2).

Finally, it seems that there is only one explanation for the occurrence of retroactive interference in the present experiment. In fact, the experiment created an extraordinary situation. As was pointed out above, in everyday life the links between odors and the environment in which they are perceived remain very stable. Buildings, people, streets and even cities or countries have their own characteristic smells, and when they change their smells we usually notice it and ascribe the change to an external cause, which is an effective coping mechanism to leave the original relationship between odor and environment intact. In this experiment the relationship between odor and environment was changed, but the odors were so weak as to remain unnoticed consciously by the large majority of the subjects (and by the experimenters), thus preventing the use of the coping mechanism. The same may be true for the results of Walk and Johns (Walk and Johns, 1984), who presented a third odor in the same environment, and prevented the intervention of a coping mechanism by using immediate retrieval after a very short and partly filled (asking the subject to observe the stimulus attentively) retention time. Thus, in both experiments a somewhat exceptional situation was created by changing the bond between odor and environment while preventing the subject to notice it consciously. Although such situations undoubtedly also occur in normal life they may be quite exceptional. On the other hand it should be noted that other experiments that showed the extreme longevity of odor recognition memory (Engen and Ross, 1973; Lawless and Cain, 1975; Lawless, 1978) are even more exceptional and non-ecological. They either used pure odors, which are virtually never encountered in normal life, or they used familiar odors out of context in a very specific laboratory situation, which never recurred during the retention period. Thus, it is no wonder that, even without verbal mediation, the episodic relationship between odor and environment is retained over very long periods.

### The role of emotion in odor memory

That in such memories emotional links to exceptional situations also play a mediating role cannot be excluded (Kirk-Smith *et al.*, 1983; Herz, 1999). Herz (Herz, 2000) discusses the Proustian memory phenomenon and claims that such memories are not only special because of their uniqueness, but also that the vividness of these memories relies completely on their emotionality and not on a better memory for particular details of the remembered situation. Although Chu and Downes (Chu and Downes, 2000) and Aggleton and Waskett (Aggleton and Waskett, 1999) provide strong arguments against the latter part of her argumentation, they too admit that emotional value may play an important part in Proustian memory. In the present experiments stressful emotions, created by the fact that the subjects were asked to perform psychological tests, might also have played a part in the establishment of the memory. This is unlikely, however, because in the previous experiment (Degel *et al.*, 2001) no difference in the ratings of pleasantness for the experimental odors between exposed and non-exposed groups were found. Furthermore, there seems no reason to assume that in earlier experiments on episodic odor memory (Engen and Ross, 1973; Lawless and Cain, 1975; Lawless, 1978), other emotional factors than the exceptionality of the situation (conscious odor learning in a laboratory) have played a part. This means that, although memories elicited by odors appear to be more emotional and emotional encoding situations enhance the effectiveness of an odor as a memory cue (Herz and Engen, 1996), this does not mean that emotions are involved in odor memory in a direct way. Thus, in the previous experiment (Degel *et al.*, 2001) no correlation between the individual memory for an odor and its pleasantness was found. Perhaps emotions just prevent retroactive interference by providing the situation to which a specific odor is linked, with such a uniqueness, that its recurrence in the presence of another interfering odor is as highly improbable as the recurrence of that specific odor itself. After all, it was the extraordinary combination of a special tea with a special madeleine that to Proust brought back the very special and emotional situation of aunt Sophie's room on Sunday before mass with great precision. Herz (Herz, 2000) is right when she claims that the longevity of such odor memories depends on their uniqueness and that emotions may play a part in establishing this uniqueness, but she is wrong when she claims that such memories miss precision [see Aggleton and Waskett (Aggleton and Waskett, 1999)] and probably wrong when she supposes that retroactive interference is weaker for odors than for verbal or visual associations.

In conclusion it can be said, that both proactive and retroactive interference do occur in the pre-semantic episodic memory that links an odor to the environment in which it is perceived, and that the longevity of olfactory memory should rather be explained by the improbability of incidences that provoke retroactive interference than by the absence of the retroactive interference itself.

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