“But I thought …”

Literature review (Vers. 2) of some well known facts in dog training

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Table of Contents

Foreword

Who am I? Well, I am a person who has done some individual study, some structured study and some more individual study. I work mostly with fearful and fear aggressive dogs. When I get the chance.

I attend seminars, workshops, webinars, all to increase my knowledge. The more I learned, the more I found I needed to learn. And this need to learn has "driven" me to investigate, as well as I can, some topics which have disturbed/interested me. So what I'm going to do is first relate "stories" of incidents which, with time, brought me to a stop, saying to myself: "something's not right here".

1) I was taught, that you handle Behavioral Modification (BM) cases centered around emotions with respondent methods. You change the emotion and the need to behave in an undesired manner disappears. If you however have unwanted behavior based upon learning history of having learned undesired behaviors with little or no effective training, you work with operant methods.
Then I got an email message from a well known behaviorist with instructions correcting the class notes. Desensitization was to be eliminated from a course on aggressive dog behavior. Counterconditioning would be handled as a by-product of operant conditioning. This was puzzling. I asked for clarification and the answer was, that counterconditioning was achieved in an **operant procedure** as long as exposure was graded (desensitization?) and only positive reinforcement was used. This seemed to be a rather radical departure from that which I'd heard (see above). Where did this come from and how did it change what I was doing with my clients' dogs? And .. I'm now to work with emotionally elicited behaviors using operant procedures? Confusing.

2) Related to Point 1, there is no understanding as to why desensitization works (http://www.associationofanimalbehaviorprofessionals.com/glossary.html), however we do know it works, but as with counterconditioning, there is most probably operant learning going on during D&CC "Pavlov is sitting on your shoulder" (attrib. Bob Bailey) "...and Skinner is sitting on the other." (attrib. Unknown).

3) Respondent conditioning for fear is resilient; once treated, it doesn't come back. However both Paul Chance[1] and Burch & Bailey[2] do say that **Spontaneous Recovery** is possible, but they don't go into when or how to avoid it. On the other hand, when asked directly what to do about fear returning after having done a D&CC, a well known expert replied "*If DS/CC is done competently, there *is* no resurgence. Kind of like asking, "What do you do about the dog turning into a kiwi?*" That would mean, that if you have spontaneous recovery, you did your D&CC wrong. But that wasn't what Chance or Burch & Bailey wrote. And it certainly wasn't what I'd just seen/heard in a lecture. (see point 5 below)

4) As above – aggressive behavior, when properly treated doesn't come back. And unwanted behavior need only be properly trained and substituted with wanted behavior. But then Ken Ramirez said that once the animal has learned aggression, aggression is in the animal's repertoire. It just needs the right circumstances to reappear. This flies in the face of the entire concept of behavior modification. Don't we replace unwanted behavior with desired behavior by changing the associated emotions?

5) I re-watched a filmed lecture by neuroscientist Joseph LeDoux, PhD, at the Karolinska Institutet in Sweden[3]. I'd thought originally it was a interesting lecture about brain structures and behavior and how they could effect changes in behavior by cutting bits of the brain at specific time intervals during or after training. Something I couldn't use, but was interesting.

1 "Learning and Behavior: Active Learning Edition", 6th edition 2008
2 "How Dogs Learn", 1999
3 https://www.youtube.com/watch?v=9_IIgXWdF-w

Then towards the end he went into the pros and cons of respondent methods as opposed to operant ones and stated that in his fear reduction experiments, around 50% of the respondently treated mice experienced "***Return of Fear***" (did they just do their respondent trials incorrectly?) while those trained operantly did not experience ANY "*Return of Fear*" (*ROF*). Why? I didn't know, but maybe because of some respondent conditioning going during the operant condition as above stated? Fear is however an emotion – you don't deal with emotions via operant training.

6) Everyone has a different definition of "threshold".
"You can tell if your dog is over threshold, if she stops eating",
"Your dog is over threshold if she reacts to the trigger.",
"Your dog is over threshold if she looks stressed.",
"Your dog is over threshold if she looks away from the trigger.",
"Your dog is over threshold if she looks towards the trigger.",
"Your dog is over threshold if she starts giving undesirable behaviors."
"Your dog is over threshold if she shows avoidance behaviors."
"Your dog is over threshold, if …"
There was no agreement as to what threshold was. There must be some "official" definition of threshold we can agree on, something anchored in science.

So now I had something to look up. So I went to Google Scholar and I would like to share what I found there – this is not my hypothesis, but rather what others have researched and not just one-off, but rather in some cases for some ideas spanning decades, that might also cause others to pause and think, as it has me. ....

Return of Fear (ROF)

I'm going to start with ROF, because this one interested me the most, but also lead me to some conclusions with some of the other points above. One of the first documents using a completely different search criteria was from 1979 and written by Dr. Stanley Rachman. **Return of Fear**'s (ROF) meaning has slightly changed over the years but he coined the term and he defined it (Rachman 1979):

> The return of fear is defined here as the reappearance of fear that was present earlier but had undergone a decline. Insofar as fears are best regarded as having quantitative properties. strictly one should speak of a partial reduction or a partial return of fear.
>
> Following this definition, it is plain that fear-reducing forms of therapy, such as desensitization or flooding, provide an excellent basis for studying the return of fear. To begin with, a fear is present and is then reduced by say, desensitization. When at a later time the appropriate fear-evoking stimulus (e.g. a snake) is represented the person may or may not display a (partial) return of the original fear (p. 165).

And he wrote further:

1) Commonly, fear seems to return after a prolonged rest is taken following a period of exposure to stress (e.g. Janis, 1951: Rachman. 1978: Goorney and O'Connor, 1971).
2) Paradoxically, the return of fear seems more likely to occur after a long rest period than a brief one (e.g. Goorney and O'Connor, 1971; Rachman, 1966).
3) There is no necessary relationship between either (a) the original level of fear or (b) the speed of fear-reduction or (c) the extent of the fear-reduction-and the return of fear (Grey et al.. 1979).
4) There may be a relationship between the level of demand operating in a fear-reducing situation and a return of fear; in particular, high-demand experiences are more likely to be followed by a return of fear than low-demand experiences (Grey et al.. 1979).
5) The probability of a return of fear may be increased by the occurrence of an aversive event prior to the rest period (p. 165).

So now I have:

1) A term to search for in Google Scholar. **Return of Fear** (ROF)
2) and an indication, that maybe there IS something here, not just bad D&CC technique. And it's not something Rachman just found in 1979. According to his sources, it's been documented going back to, in one context, 1951, but even Pavlov in 1927 wrote of it, see Jayson (2006)!

And I found stacks of documents concerning this phenomenon, the next one by Dr. Rachman (1989) himself. In which he writes (several different quotes):

> The "return of fear" is the reappearance of fear that has undergone partial or complete extinction, the *phenomenon* is illustrated by clinical and other examples. The information on return of fear in humans, drawn from a review of experimental findings and clinical reports, is summarized. **It is a robust and common phenomenon.** (emphasis L.C.) The presence of an elevated heart rate response at the start of a fear reduction program is a strong predictor of the return of fear. Highly demanding treatment or training programs promote the return of fear. There is no relation between the return of fear and the amount of time taken to reduce the fear initially. The return of fear can be blocked by the repeated practice of fear-reducing tasks, or by ensuring a low level of arousal prior to and during fear-reduction exercises. The return of fear can be influenced by events that take place before, during and after the fear-reduction sessions. Attention is drawn to the post training enhancement of fear, and to the likelihood of fear inflation occurring after aversive events, even if they are not directly relevant to the fear itself The similarities and differences between the return of fear, spontaneous recovery, and dishabituation are set out, and various explanations of the phenomenon are considered. The clinical implications of the return of fear are discussed, and research priorities are put forward (p. 147).

> A simple sequence of events is involved in the return of fear: a decline in fear (whether it occurs spontaneously or by *design*) is followed by an interval without relearning or of re-exposure to the relevant fear stimulus, and at the end of that interval a revival in the strength of the fear is evident (p. 147-148).
>
> An earlier example of the return of fear, although not recognized as such at the time, was described by Rachman in *1966*. An investigation into the speed of generalization from desensitization treatment sessions to extra-clinic situations unexpectedly produced substantial evidence of what was then called spontaneous recovery, or relapse. On 50% of the occasions tested, at least a partial return of fear was observed (p. 148).

Wait, let me repeat this: "**On 50% of the occasions tested, at least a partial return of fear was observed.**" This sounds a lot different than it **never happening if you do it correctly,** or being only worth a half page in two different books. This sounds like a significant statistic and repeats what LeDoux described in his lecture (see above).

> Although the return of fear and clinical relapse are sometimes related, the two concepts are not synonymous. Clinical relapses can take various forms and have multiple causes. The return of fear is a psychological phenomenon in its own right, and does not always have clinical connotations (see below). An example in which the psychological concept and clinical relapse coincide is provided in the work reported by Barlow, Mavissakalian, and Schofield (1980). One of the patients who participated in their study of the treatment of agoraphobia showed consistent improvements in her self-reported fear ratings. In fact, she progressed more quickly than the other patients in the group of which she was a member. By the seventh session, she reported herself to be free of anxiety in the formerly fear- provoking situations. This was confirmed by reports from her husband and by direct observations of her behavior. However, her heart rate during behavioral testing was exceedingly high before, during, and after the completion of treatment. It did not improve concordantly with her subjective fear or her behavior. Four weeks after completing the post-treatment assessment, the patient "tearfully reported that she had relapsed completely and was once again unable to leave her house," (Barlow et al., p. 446). As we shall see, elevated heart rate response has turned out to be a highly significant predictor of the return of fear (p. 148).
>
> In the following year, Grey, Rachman, and Sartory (1981) reported that only those four of their 28 fearful subjects who showed desynchronously high heart rate (i.e., high heart rate with low fear subjective ratings) showed a return of fear. This small group of subjects with conspicuously high heart rates, with a mean level of 120 bpm, showed a significant increase in fear after an interval of one week, even though they had shown improvement similar to the other subjects during the treatment session (p. 148).
>
> Despite the absence of satisfactory clues at that time, it was suggested that the probability of a return of fear occurring could be reduced by frequent repetition of the desensitization items and by over-learning. The possibility that frequent repetitions of trials that lead to fear reduction might, in the longer term, prevent the return of fear was also raised by Agras (1965). He found that 50 % of the anxiety hierarchy items which he presented to his subjects showed "relapse" when tested at the next session. When these items were re-treated in the next session, the fear was reduced more quickly. After this second treatment session, fewer of the items showed a return of fear. After a third attempt at desensitization, even less return of fear was observed. The trend was clear; the frequency of "relapses" diminished with repeated fear-reducing trials (p.150).

This was not just a problem with Dr. Rachman. Dr. Agras also reported also a 50% relapse of fear from the first session to the next, BUT, if you repeat the process, the next time, there was less relapse amongst those who'd relapsed the first time. I think there is probable cause for assuming, that the reason for relapses of this magnitude are not due to faulty implementation of the desensitization. I think we can assume, we're dealing with something very real here.

> We also have recent evidence that points to the importance of practicing the fear-reducing skills in the interval between completion of training and the retest (Craske & Rachman, 1987). The use of distraction during this same period can, on the other hand, inflate the return of fear. (It should be borne in mind, here, that indications from animal research carry the strong suggestion that the return of fear will be found to be subject to large inflation effects if the subject is exposed to a disturbing event in the post training period. For example, Henderson and Blaccioniere (1984) showed that the fears displayed by rats were greatly increased after they had been subjected to a disturbing experience (p. 159).
>
> It is highly probable that the return of fear is subject to the inflation effect, an enhancement of fear by exposure to an aversive event after the original learning has been established. Following the conditioning of a CS to a UCS of moderate intensity, exposure to a UCS of greater intensity can give rise to an inflated CR (Mineka, 1985). Furthermore, such inflation can occur after "chance encounters with a traumatic event, even of a different sort than that involved in the original conditioning" (Mineka, 1985). A conditioned response that has been subject to extinction may reappear after the subject is exposed to highly intense stimulation, and presumably, human fears can return (be reinstated or strengthened) in a similar manner. Recent evidence from animal experiments suggests that the inflation effect can be exaggerated if a long interval intervenes between the original conditioning and the inflation experience (Mineka, 1985), and this may help to account for fears that return after a long period of non-practice or non-exposure (p. 159-160).

I didn't download and read EVERYTHING concerning ROF, but one thing is clear. It hasn't gone away and it hasn't been discounted, that I can find.

Rowe and Craske (1997) found the following when experimenting with spider phobia:

> With only intermittent encounters with targeted feared situations following successful treatment, non-fearful and old, fearful responses to the stimulus (cue) fall into relative disuse. When the stimulus arises again, the relative retrieval strength of the new and old responses will have altered since the end of treatment to be much closer in strength. This regression of memory is attributed to the relative storage strengths of the old and new information. The older memories will not have lost as much retrieval strength over time as the newer memories because the former will likely have much higher storage strength. This would result in an increased probability that the older information in the fear structure would be retrieved when faced with the phobic stimulus after lengthy intervals without stimulus exposure. A period of disuse of both old and new learning followed by recovery of old learning directly parallels Rachman's description of ROF (p. 720).
>
> At follow up, both groups evidenced significant ROF in maximum fear and heart rate in response to the novel spider. This indicates that both groups responded more fearfully to the novel spider than the control spider. These results at follow-up demonstrate the powerful effects of time and context as predicted by Bjork and Bjork (1990) and Bouton and Swartzentruber (1991). Shifting context cues, including stimulus cues, can abruptly trigger older, fearful learning. In Bjork and Bjork's (1990) model, this would be explained as less retrieval strength in the face of competing retrieval cues with greater storage and retrieval strength. According to Bouton (1988), this would be explained by the context specificity of extinction (p. 732).
>
> If tests of generalization had not been included in this investigation, the degree of ineffectiveness of the exposure protocol would have been undetected. These results reinforce Schmidt and Bjork's (1992) advocacy of various kinds of transfer and retention tests to properly evaluate training effectiveness. Although some research has investigated the transfer of fear reduction from one type of phobia to another (e.g. Cotler and Garlington, 1969; Williams et al., 1989), tests of generalization have not been systematically included in investigations of ROF (p. 733).

Bouton and Peck (1992) wrote about the ROF he encountered after counterconditioning, which he called “cross-motivational transfer”:

> Research on these "cross-motivational transfer" paradigms has been guided by the view that the aversive and appetitive motivational systems inhibit one another reciprocally (see, e.g., Dickinson & Dearing, 1979; Konorski, 1967; Rescorla & Solomon, 1967). In associative terms, a CS associated with a US of one type is seen as equivalent to a conditioned inhibitor within the other motivational system (see, e.g., Dickinson & Dearing, 1979; Krank, 1985; see also Daly & Daly, 1982, 1987, for an extension of this analysis). In principle, this view allows one to put cross-motivational transfer into the terms of conditioning models that otherwise cannot deal with conditioning phenomena involving more than one US. The best-known of these models is probably the Rescorla-Wagner model (Rescorla & Wagner, 1972; see Krank, 1985). This model assumes that the result of conditioning is a change in a single variable called associative strength. Conditioned excitors and inhibitors are assumed to have positive and negative values of associative strength, respectively. In aversive-to-appetitive transfer, for example, the CS would initially acquire "negative" appetitive strength, but in the second phase it would eventually acquire "positive" appetitive strength (p. 313).

> In both aversive-to-appetitive and appetitive-to-aversive transfer, Phase 1 performance recovered and Phase 2 performance became resuppressed when groups were tested 28 days following the end of Phase 2. These findings are consistent with what is known about the effects of retention interval in other interference paradigms. Retention intervals introduced following Phase 2 cause spontaneous recovery of Phase 1 performance after extinction; they can also cause recovery of Phase 1 performance in human verbal interference (see, e.g., Postman, Stark, & Fraser, 1968; Underwood, 1948a, 1948b) and in animal discrimination reversal learning (see, e.g., Chiszar & Spear, 1969; Gleitman, 1971; Gordon & Spear, 1973; Spear et al., 1980). The latter two paradigms may be especially consistent with the present data, because they also suggest that performance from Phase 2 is also resuppressed-that is, that proactive interference recovers over time, in similar settings. Put together, the results suggest that both proactive interference and retroactive interference may come about because interference occurs at retrieval or performance output rather than during learning.
>
> This conclusion contrasts with a tradition in animal conditioning theory which often assumes that proactive and retroactive interference effects result from interference with what is learned during, or stored from, the target phase. As we noted in the introduction, the Rescorla–Wagner model and related models (see, e.g., Daly & Daly, 1982) assume that Phase 2 damages, or causes the unlearning of, information stored in Phase 1. The same assumption can be found in many connectionist models of human learning and memory (see McCloskey & Cohen, 1989). Current treatments of proactive interference make the related assumption that Phase I interferes with the subject's storage of information from the second phase. This view is clearly implied by the Rescorla-Wagner model's representation of reciprocal inhibition theory; it also dominates current thinking about other proactive interference paradigms such as latent inhibition, which is usually assumed to occur because the animal fails to attend to or process the CS during Phase 2 (see, e.g., Mackintosh, 1975; Pearce & Hall, 1980; Wagner, 1978, 1981). We cannot rule out the possibility that there was some damage to Phase 1 storage or to Phase 2 learning in the present experiments. However, nothing in the data would require us to assume such damage. The present results clearly indicate that an emphasis on learning and storage deficits would be an oversimplification for the case of cross-motivational transfer (p 319).

In another spider study, Rodriguez, Craske, Mineka & Hladek (1998) looked at different contexts to see if these, after the therapy rather than during the therapy, could be responsible for ROF:

> Context-specificity of fear extinction was tested among 65 participants who were fearful of spiders by manipulating the contexts used for exposure treatment and two-week follow-up assessment. Context was defined by both meaningful (presence of a particular therapist) and incidental (room location and furnishings) environmental cues. Distinct phobic stimuli were used to examine interactions of context with stimulus. Physiological, behavioral and verbal indices of fear were measured. Results provided modest support for context-specific return of fear. With

one stimulus, participants assessed in a non-treatment context at follow-up exhibited greater returns in heart rate levels. In addition, three of four participants who could not touch the stimulus at follow-up had been tested in a non-treatment context (p 845).

Results indicated the predicted changes in fear levels across the three experimental sessions. Because of the absence of experimental intervention, fear reduction from initial to pre-treatment assessment must necessarily be interpreted as naturally-occurring habituation to both the stimulus and nonspecific factors that characterized the experimental situation. This nonspecific habituation was marked, however, by decreases in self-report and behavioral fear measures with no coincident change in HR, a desynchrony possibly due to demand effects. In contrast, fear reduction during treatment was synchronous, of greater magnitude and supplemental to that achieved prior (e.g., between the first and second sessions). Thus, the requirement from Rachman (1989) that significant fear reduction precedes returns in fear was met. Finally, the majority of self-report measures indicated significant return of fear, with concordant increases in HR, over the two-week follow-up interval. Fear recovery was incomplete in that fear ratings did not return fully to their initial levels. Also, there were no signs of return of fear using state anxiety or phobic cognitions as dependent variables (p. 856).

Mystkowski, Jayson Craske, Echiverri & Labus presented this paper (2006) also – they reference lots of others who have done work with the similar or same findings. Feel free to check them out:

Extant findings in the animal and human conditioning literature demonstrate that renewal, termed return of fear in studies with humans, occurs when re-exposure to a previously feared phobic stimulus occurs in a context different than the one present during extinction. The present study investigated whether mental reinstatement of the treatment context at follow-up could attenuate context based return of fear. Forty-eight spider-fearful individuals received exposure therapy in one of two contexts, and were followed-up 1 week later in the treatment or a new context. Half of the participants received instructions to mentally reinstate the treatment context before the follow-up test. Self-report data replicated previous research on contextually driven return of fear. Furthermore, participants who mentally reinstated the treatment context, before encountering the phobic stimulus in a new context at follow-up, had less return of fear than those who did not.

Despite clear evidence of being an empirically supported treatment, some of the effects of graded exposure-based therapy for specific fears and phobias may only be temporary in that fear may return with the passage of time (Mineka et al., 1999, Mystkowski et al., 2002 and Rodriguez et al., 1999). That this occurs is not at all surprising given the phenomenon, first reported by Pavlov (1927), of spontaneous recovery following extinction of classically conditioned responses. According to Rachman, 1989 and Rachman, 1990, “return of fear” is the reappearance of fear (not to be confused with full clinical relapse) that has undergone full or partial extinction. Mechanisms underlying fear recovery, unfortunately, have proven difficult to isolate. Researchers have explored individual difference variables that predispose certain individuals to return of fear (e.g., baseline fear levels), treatment variables (e.g., exposure duration), and post-treatment variables, or events (e.g., post-treatment cognitions) that occur after treatment and before retesting at follow-up, without demonstrating consistent effects on return of fear (please see Rodriguez et al., 1999, for a review) (p. 49).

Return of Fear (ROF) was first reported by Pavlov in 1927[4]

Within the last two decades, new research has examined the effects of context on return of fear. Inspired by significant work in animals and humans on contextual control of learning and memory, Bouton and colleagues (Bouton, 1988, 1991, 1994; Bouton & Bolles, 1979; Bouton & Nelson, 1998) have provided strong evidence that partial fear recovery occurs in rats when a conditioned stimulus (CS) extinguished or counterconditioned in one context (other than the one where acquisition occurred) is later presented in a different recovery context. Having similarity across extinction and recovery contexts, on the other hand, prevents or minimizes a fear recovery effect (Bouton & King, 1983). In general, Bouton’s “memory model” states that the

4 “CONDITIONED REFLEXES: AN INVESTIGATION OF THE PHYSIOLOGICAL ACTIVITY OF THE CEREBRAL CORTEX”, Oxford University Press, 1927

> learning that takes place during extinction does not replace the learning that took place during acquisition, but rather both types of associations are represented in an organism's memory network and that subsequent fear performance depends on the competition between both types of learning. When confronted with a context that is novel or different than the one encountered during fear extinction, changes in context cause an organism to rely on associations of fear acquisition, rather than fear extinction, leading to fear renewal (Bouton, 1993) (p. 49-50).
>
> All in all, Bouton's work with rats suggests the importance of studying contextual cues in human fear extinction because it has potentially important implications for behavior therapy. In particular, it may suggest the importance of exposure therapy in multiple contexts in order to prevent or minimize return of fear because of the variety of conditions in which a treated individual may encounter phobic stimuli in the real world (e.g., being treated only in a therapist's office might not be helpful when later confronting a spider in an attic or garden) (p. 50).
>
> In summary, this study's self-reported fear data supports extant findings that some fear returns when individuals are confronted with their previously feared phobic stimulus in a context different than the one present during fear extinction (Mineka et al., 1999, Mystkowski et al., 2002 and Rodriguez et al., 1999). Moreover, the contextually driven return of fear observed in this experiment further substantiates the relevance of the animal conditioning literature to the practice of behavior therapy for phobias, and perhaps for other anxiety disorders as well (Bouton & Nelson, 1998) (p. 59).

In any case, **Return of Fear** after respondent therapies is something very real and well known. It's certainly not just a problem of D&CC not having been done properly.

Extinction and Return of Fear (ROF)

So now I will quote from the some of the studies dealing with extinction. Operant extinction as we know it has to do with ignoring behavior or simply not reacting in any way to it. Do that long enough, the behavior will die out, perhaps with the well known extinction burst first. When we do this in our training, we fully anticipate, that the unwanted behavior won't come back. Extinction is also a part of shaping and there can be an extinction burst there. If you've ever played Karen Pryor's shaping game for people, you'll know what that feels like. You get clicked 5 times for doing an approximation properly, you repeat it a sixth time, and nothing happens – no click. Why not? I did the same as the other times. So you try this, and that and something else. You try the behavior again – maybe the trainer just didn't SEE you doing it properly. There were times I was ready to throw the trainer out the window. Extinction burst with a healthy dose of frustration. But we're NOT talking about that in the context of shaping.

We're talking about it in terms of a behavior that has been respondently extinguished, the fear which elicited that unwanted behavior having been treated.

Extinction is also one of those things that is not very well understood in terms of the mechanics of how it works, why it works but it has been found, that it too is subject to ROF.

Bouton and Swartzentruber (1991) did a study in which they looked at ROF happening after extinction in both respondent and operant learning, but for the moment we'll only look at the respondent extinction:

> Recent research on Pavlovian conditioning and extinction has had interesting implications for both therapy and relapse (Bouton, 1988, 1991b). In fear conditioning, an animal (e.g., a rat) learns to associate a conditioned stimulus (CS; e.g., a tone) with an aversive unconditioned stimulus (US; e.g., a mild foot shock). As a result, the CS comes to evoke fear, a motivational state that organizes a constellation of physiological and behavioral defensive responses (e.g., Bolles & Fanselow, 1980). Conditioned fear can be removed by repeated exposure to the CS alone; this is the basic extinction procedure. Because extinction erases the signs of fear, it is tempting to assume that it erases the original learning. However, even after many extinction trials, the animal retains a memory of conditioning that can provide a powerful basis for relapse. Instead of erasing the original learning, extinction gives the CS a second, and therefore ambiguous, meaning (Bouton, 1988, 1991b; Vouton & Bolles, 1985) (p. 124).

> (Pavlovian extinction) In the laboratory, extinguished fear has a surprisingly unstable character. Fear of an extinguished CS can return even after quite prolonged extinction training. For example, Reberg (1972) and Hendry (1982) both tested fear of an extinguished CS together with another excitor after a very large number of extinction trials. Reberg extinguished a CS for 54 trials beyond a point at which the CS evoked no fear, and then tested it together with a CS that evoked moderate fear on its own. Hendry ran 96 extinction trials and then tested the CS in compound with another CS that had received the same extinction treatment. In either case, when the extinguished CS was tested together with the other fear-related stimulus, it again evoked substantial fear. The potential for fear thus persists long after extinction has removed signs of fear from behavior. Clearly, an extinguished CS is not merely neutral; it will evoke the response again when it is tested in a fear-associated background. Even after extensive extinction or exposure therapy, the response to an extinguished stimulus depends crucially on the stimuli with which it is presented (p. 125-126).

> To summarize this section on the extinction of Pavlovian learning, converging lines of evidence indicate that even prolonged extinction procedures do not abolish the original learning… Although extinction does eliminate behavior, it does not eliminate a memory corresponding to conditioning; under the right circumstances, the extinguished response is easily recovered or restored. The ambiguous word analogy is a convenient rule of thumb: An extinguished CS behaves as if it has two separate meanings, ready to be retrieved or selected by the appropriate context. Performance after extinction is inherently unstable. From this perspective, relapse, or a

> recovery of conditioned performance, is no anomaly; it is to be expected after extinction or exposure therapy. The response elicited by an extinguished evoking stimulus, and thus the success or failure of a therapy, will depend importantly on the context in which the stimulus is encountered (p. 128).

So the core of the study, also with cites is, that extinguished behavior can return at any time. This having to do with the respondent type of extinction and the context in which the "cue" takes place.. Gunther, Denniston and Miller (1998) besides confirming this, give clues as to what one can do to avoid ROF, basically, that one can avoid ROF -IF- one repeats the process in different contexts, different places, etc.:

> Most relevant to the present issue, Bouton (1988; see also Bouton and Swartzentruber, 1991) has also applied his findings concerning the importance of contextual information to the clinical setting (also see Kehoe and Macrae, 1997, for a review concerning the relevance of `savings' in animal learning to the clinical setting). He contends that those therapists using extinction (i.e. exposure) techniques as a treatment for various psychological disorders (e.g. anxiety disorders and phobias) should be aware of the renewal effect and related contemporary findings in Pavlovian conditioning. For example, the renewal effect is known to arise from extinction being more context-specific than is excitatory conditioning which generalizes relatively easily to contexts other than the training context (p. 77).

> Beyond suggesting that therapeutic extinction manipulations are constrained to the context in which they are conducted, Bouton (1988; also see Bouton and Swartzentruber, 1991) made several suggestions concerning potential means of preventing relapse following extinction treatment. One such suggestion was that in order to minimize renewal of conditioned fear, therapists should conduct exposure therapy sessions in a context as similar as possible to the context in which the fear was acquired (see Bouton and King, 1983, for experimental evidence of this effect with rats as Ss). However, this invites the renewal of fear seen with the AAB design, as reported by Bouton and Ricker (1994). Thus, rather than give extinction treatment in a context similar to that in which the dysfunctional association was acquired, there may be greater efficacy in minimizing relapse by conducting exposure therapy sessions in multiple settings. This treatment might provide the client with the view that the fear-inducing stimulus is now safe `everywhere' (p. 77).

> The results of the present studies, combined with those of Bouton and Bolles (1979; also see Bouton and King, 1983; Bouton and Ricker, 1994; Smith, 1982) have important implications for clinicians who conduct exposure therapy, and who hope to prevent relapse of conditioned fears. First, from the results of Experiment 1, we conclude that therapists should attempt to conduct exposure sessions in a variety of settings in order to prevent relapse in novel settings (Bouton and Bolles, 1979). Second, if this proves unsuccessful, we may suspect that the phobia was acquired in multiple settings, and that it would be wise to further increase the number of settings in which the exposure treatments are to be conducted (Experiment 2). Third, although reaching beyond the present data, we suggest that the therapist might consider incorporating some type of reminder technique that the client could use in novel (or fear-originating) contexts (Brooks and Bouton, 1994). For example, if the clients are able to carry with them a component of the extinction context (e.g. a pendant the S had been instructed to grasp during exposure sessions), relapse might be prevented by a `return to the extinction context', using that component as a reminder cue for extinction treatment (p. 89-90).

Boulton (2004) once again examines ROF in terms of extinction in respondent conditioning, ie desensitization and counterconditioning:

> This article provides a selective review and integration of the behavioral literature on Pavlovian extinction. The first part reviews evidence that extinction does not destroy the original learning, but instead generates new learning that is especially context-dependent. The second part examines insights provided by research on several related behavioral phenomena (the

interference paradigms, conditioned inhibition, and inhibition despite reinforcement). The final part examines four potential causes of extinction: the discrimination of a new reinforcement rate, generalization decrement, response inhibition, and violation of a reinforcer expectation. The data are consistent with behavioral models that emphasize the role of generalization decrement and expectation violation, but would be more so if those models were expanded to better accommodate the finding that extinction involves a context-modulated form of inhibitory learning (p. 485).

The literature on counterconditioning is not as large as the literature on extinction. But there is evidence that similar principles may apply. For example, experiments in my own laboratory have demonstrated a renewal effect (Peck and Bouton 1990): If rats receive CS–shock pairings in one context and then CS– food pairings in another, the original fear performance returns (and replaces food performance) when the animals are returned to the original context. Complementary results were obtained when CS–food preceded CS–shock. Other experiments have demonstrated spontaneous recovery (Bouton and Peck 1992): In this case, after CS–shock and then CS–food, animals tested at a 1-d retention interval showed primarily appetitive performance, whereas animals tested 28 d later showed a recovery of fear performance (and a suppression of appetitive). A complementary pattern was observed when CS–food preceded CS–shock. Finally, we have observed reinstatement (Brooks et al. 1995): When CS– food follows CS–shock, a number of non-contingent shocks delivered in the same context (but not in a different context) can reinstate the original fear performance. Counterconditioning thus supports at least three of the effects suggesting that extinction involves context-dependent new learning (p. 487).

Extinction is a highly complex phenomenon, even when analyzed at a purely behavioral level. It is worth noting that it is probably multiply determined. But according to the results reviewed here, it does not involve destruction of the original learning. Instead, the main behavioral factors that cause the loss of responding appear to be generalization decrement, and new learning that may be initiated by the violation of an expectation of the US. In SOP, perhaps the most powerful and comprehensive model of associative learning that is currently available, that expectation violation takes the form of the CS activating the US node into a secondarily-active (A2) state that potentially enables new inhibitory learning as long as the CS remains on and no US is presented. Importantly, this new inhibitory learning leaves the original CS–US association intact.

I have argued that the fact that extinction might leave the original learning intact means that the CS emerges from extinction with two available associations with the US. It therefore has properties analogous to those of an ambiguous word, and the current performance depends on which of two associations is retrieved. Consistent with this idea, another fact that emerges from behavioral research on extinction is that it is relatively context-dependent. I have therefore suggested that the second (inhibitory) association of CS is especially dependent on the context for its activation or retrieval. The role of the context is modulatory; its activates or retrieves the CS's own second (inhibitory) association, much as a negative occasion setter might (see Holland, 1992). This hypothesis begins to integrate several facts about extinction and brings relapse effects such as the renewal effect, spontaneous recovery, rapid reacquisition, and reinstatement to center stage. Extinction is not the same as unlearning, and the context-dependence of extinction performance is a central part of that insight (p. 492).

So the take-away message is, that even if you take precautions of generalizing the process several times for different contexts, you still risk the original behavior coming back, since it wasn't "unlearned" in the first place.

Vervliet, Baeyens, Van den Bergh & Hermans (2011) also looked at causes and mechanics of ROF in extinction, also covering generalization. They've designed a series of tests to help researchers further examine ROF and how it can be better countered. There's no point in quoting the tests they devised – much too technical, more interesting are their reasons for having done so.:

The main behavioral signature of fear extinction is its fragility. This is exemplified by the renewal

effect, where a change in the background context produces recovery of fear to a conditioned-and-extinguished stimulus. Renewal is the backbone of a widely accepted theory of extinction in animal research, as well as an important experimental model to screen novel treatment techniques. This has led to an explosion of fear renewal research in humans. However, the mere observation of return of fear in a renewal procedure is not sufficient to validate this particular theory of extinction in the tested sample/procedure. Here, we systematically outline a set of experimental tests that aid in evaluating alternative extinction/renewal mechanisms (p. 51).

In summary, contextual renewal represents a major pathway to the study of extinction and fear recovery, and has been established as a robust phenomenon in human fear conditioning. The prevailing theory in animal conditioning research (Bouton, 1994) is often taken for granted in this human literature, but critical tests are currently lacking. We have summarized these tests and their rationales, thereby indicating the wealth of alternative mechanisms that may be involved in fear extinction. Routinely including these tests in renewal studies is expected to greatly expand our understand- ing of the fear extinction process in humans. Renewal is likely to be governed by a multitude of factors, the relative weight of which depends on the experimental paradigm, the tested sample, and the renewal design employed. A clear view on the extinction process under different circumstances has important implications for human conditioning theories, brain imaging studies, and clinical innovations on fear extinction (p. 58).

Bouton (2012) once again has looked into extinction and ROF, but adds also operant extinction, ie ignoring of the unwanted behavior. He first starts reviewing the literature to that point, mostly concerning respondent extinction and ROF before moving into the operant phenomena of ROF:

It is widely recognized that extinction (the procedure in which a Pavlovian conditioned stimulus or an instrumental action is repeatedly presented without its reinforcer) weakens behavior without erasing the original learning. Most of the experiments that support this claim have focused on several “relapse” effects that occur after Pavlovian extinction, which collectively suggest that the original learning is saved through extinction. However, although such effects do occur after instrumental extinction, they have not been explored there in as much detail. This article reviews recent research in our laboratory that has investigated three relapse effects that occur after the extinction of instrumental (operant) learning. In renewal, responding returns after extinction when the behavior is tested in a different context; in resurgence, responding recovers when a second response that has been reinforced during extinction of the first is itself put on extinction; and in rapid reacquisition, extinguished responding returns rapidly when the response is reinforced again. The results provide new insights into extinction and relapse, and are consistent with principles that have been developed to explain extinction and relapse as they occur after Pavlovian conditioning. Extinction of instrumental learning, like Pavlovian learning, involves new learning that is relatively dependent on the context for expression.

There is now a broad consensus that extinction, the procedure in which a Pavlovian conditioned stimulus (CS) or an instrumental action or response occurs repeatedly without its reinforcer, generally decreases learned behavior without erasing the original learning. The consensus is based largely on a growing body of evidence indicating that an extinguished response can readily return with certain treatments or manipulations—and has thus been “saved” through extinction (e.g., Bouton, 2004). We have suggested that, instead of creating unlearning, extinction gives the Pavlovian CS or the instrumental action a new second “meaning” that is stored in memory along with the first, thus creating a CS or action whose current meaning is ambiguous (e.g., Bouton, 1988, 2002, 2004). And like the response evoked by an ambiguous verbal stimulus (i.e., a word with more than one meaning), the response observed after extinction pivots on what the current context retrieves (p. 130).

Renewal, reinstatement, spontaneous recovery, and rapid reacquisition have increasingly been described as “lapse” and “relapse” effects (e.g., Bouton et al., 2006b; Laborda et al., 2011). This is because, in addition to being a naturally-occurring behavior- change process, extinction is thought to be involved in cognitive behavior therapies that are designed to eliminate learned but

unwanted thoughts, behaviors, and emotions such as fear and anxiety disorders (e.g., Craske et al., 2008). The implication of these effects is that the elimination of classically-conditioned behaviors by extinction is not permanent, and that the eliminated behavior can readily return. Thus, studying renewal, reinstatement, spontaneous recovery, and rapid reacquisition may tell us about some of the mechanisms of clinical relapse (and methods that can be used to prevent it) in addition to the basic extinction process (p. 131).

Renewal is a particularly important recovery effect following extinction. Three different forms of renewal have been established in studies of Pavlovian extinction (for an overview see Bouton, 2004). In the simplest, ABA renewal, the animal receives a conditioning experience in one context (Context A), extinction in a different context (Context B), and then testing in the original Con- text A. (The contexts are usually provided by different sets of conditioning chambers that differ in their location in the lab and in their olfactory, visual, and tactile respects.) The conditioned response reliably returns upon presentation of the CS in Context A (e.g., Bouton and Bolles, 1979a; Bouton and King, 1983). In ABC renewal, acquisition occurs in Context A, extinction in Context B, and then testing in Context C. Here again, the conditioned response renews even when every phase of the experiment occurs in a new context (e.g., Bouton and Bolles, 1979a). In AAB renewal, acquisition and extinction occur in the same Context A, and the animal is tested in a second context (Context B), which again causes a return to responding to the CS (e.g., Bouton and Ricker, 1994). ABC and AAB renewal both suggest that some sort of inhibition in the extinction context plays a role in producing renewal — mere removal of that inhibition by testing outside the extinction context is sufficient to cause it to happen. A variety of research suggests, though, that under common conditions, the phenomenon does not depend on direct context-US inhibitory associations (or excitatory associations in Context A) (e.g., Bouton and King, 1983; Bouton and Swartzentruber, 1986). We have suggested that renewal is mainly controlled by an occasion setting mechanism in which the extinction context retrieves or activates the CS's current association with no US (e.g., Bouton, 1993). Much like a verbal context that disambiguates the meaning of an ambiguous word, it signals the CS's current meaning, or whether or not it will be paired with a US (.131).

There has now been quite a bit or research investigating the renewal effect in Pavlovian extinction (e.g., Bouton and Woods, 2008). In contrast, as noted earlier, there has been less research on the phenomenon in instrumental learning. In the instrumental case, the reliability of the different forms of renewal has not been clear until recently. ABA renewal of lever pressing has been reported in both the learning literature with food reward (e.g., Nakajima et al., 2000) and in the drugs self-administration literature with a number of drugs as reinforcers (e.g., heroin–cocaine combination, Crombag and Shaham, 2002; heroin, Bossert et al., 2004; cocaine, Hamlin et al., 2008; Kearns and Weiss, 2007; alcohol, Hamlin et al., 2007). However, several of these reports failed to produce evidence of AAB renewal (Bossert et al., 2004; Crombag and Shaham, 2002; Nakajima et al., 2000). Zironi et al. (2006) found preliminary evidence that lever presses reinforced by sucrose may produce ABC renewal, but that ethanol reinforcers did not (unfortunately, the positive sucrose-pellet experiment did not counterbalance the test contexts). The comparative lack of evidence of ABC and AAB renewal was theoretically important, because these phenomena are essential in forcing the conclusion that extinction is more context-specific than conditioning. If conditioning and extinction were equally context-dependent, they would generalize equally to a new context, and there would be no basis for ABC or AAB renewal (p. 131).

These experiments together show that, just as in Pavlovian conditioning, removal from the extinction context after instrumental extinction is sufficient to cause a lapse in behavior. In renewal, con- text change evokes responding (or removes the inhibitory effects of the extinction context upon responding) without further exposure to any reinforcer. In clinical application, the critical observation is that actions or habits that have been inhibited in some context may return and lapse in a new one. A drug user or an overeater might lose access to the inhibitory "tools" he or she has learned to suppress drug use or overeating, and once again engage in these behaviors when returned to the original or a new context (p. 131-132).

> Our research on renewal, resurgence, and reacquisition after instrumental extinction suggests that the principles that have been developed to understand Pavlovian extinction and relapse transfer easily to the instrumental situation. In either paradigm, extinction is not erasure, and appears to result at least partly from new learning that crucially involves the context. We would note that our conceptualization of "context" includes other stimuli besides the ones provided by the Skinner boxes that have typically been used (e.g., see Bouton, 2002) (p. 139).

> We end by noting that although the field's current interest in recovery effects like renewal, resurgence, and reacquisition may be relatively new, these effects and the contextual analysis of them and of extinction itself are deeply connected with an older perspective on extinction provided by discrimination and generalization decrement theories (e.g., Kimble, 1961; Mackintosh, 1974). As we emphasized above, research on Capaldi's sequential theory (e.g., 1967, 1994) demonstrated the value of thinking that memories of recent trials can provide stimulus support, or a context, for behavior in extinction (p. 139).

This study above raises some important questions about how to avoid ROF in extinction in "real life". If I understand it correctly, one needs to insert a competing more reinforcing behavior after the extinction process, so that the original context now evokes the newly learned behavior. The old behavior MAY still reappear, but generally not, IF the new behavior is adequately reinforced and the old is once again extinguished if it returns. This may point to one typical problem some have with the "ignore bad behavior" paradigm. If you just ignore the unwanted behavior, it will come back, similar to return of fear (ROF) – so one could at this point say that in operant conditions, it may be more correct to call this "return of unwanted behavior" (ROUB). So to avoid this, we would need to

1) train a subsequent "wanted" behavior
2) generalize the setting, taking into consideration setting, trigger, etc.

Mario A. Laborda, Ralph R. Miller (2013) also looked into preventing ROF while using extinction. As we can see, this is still a subject of interest, the study having been written up in 2013. To summarize this study - Return of Fear, something known since Pavlov wrote about it in 1927 without calling it that, is not new. Stanley Rachman put the name to it in 1979. Rachman and other suggested, that relapse and renewal of fear could be treated and "solved" by generalizing the locations and with repetition, inasmuch as ROF was subject to time considerations, ie when after a treatment the subject was once again exposed to the stimulus, as well as different contexts, meaning environments. This study examines this FOR in conjunction with extinction and comes to the conclusion, that these hypothesis were indeed true, when concerning these repeated trials in different contexts.

Operant fear reduction strategies

Before we go any further, and since the above study did NOT deal with fear, although there may very well have been other emotions in play, I'd like to look at a few fear reduction studies of operant conditioning, usually positive reinforcement, usually some kind of shaping, done with humans. The first one below was done by Miklich (1973) and was, according to Miklich, a combination of systematic desensitization and shaping:

> Panic during asthma attacks interfered with medical treatment of an asthmatic boy. His hyperkinesis and youth (6 yr) prevented the use of ordinary systematic desensitization procedures in treating his panic, therefore an operant conditioning procedure was used. First, it was used to shape relaxed sitting. Then the boy was able to earn tokens by remaining relaxed while the therapist described to him his having progressively worsening asthma. After working through descriptions ranging from mild asthma to death from status asthmaticus, the nurses and physicians attending the boy reported no further difficulties in treating his asthma. Moreover, the hypoerkinesis also showed considerable improvement in the first few months following therapy (p. 177)

(But, despite the title, no SD was used!)

> Two factors made systematic desensitization with the usual procedures impossible with this child. The first. of course, was his hyperkinesis. He could not even sit still, let alone attempt to relax. Secondly, he denied any feelings of fear or anxiety while having asthma, thus no hierarchy could be constructed. To overcome these problems, a two part therapy plan was formulated. The first part used operant conditioning to shape a quiet, relaxed chair sitting. in the second part, I reinforced the child for maintaining his relaxed posture while I attempted to arouse him from it by vividly describing his progressively worse asthma (p. 178).

> Some physicians have expressed reservations to me over the use of desensitization of asthma panic lest by removal of the fear, the patient not seek appropriate medical attention. There was no hint of this kind of response in any of the data nor in the reports of the boy's physicians, nurses and dormitory counselors.
>
> Somewhat unexpectedly, there was a considerable improvement in the boy's general behavior (p. 180).

> Follow up data obtained 8 months after the end of therapy and immediately before the boy's discharge from CARIH showed that the asthma panic and the behaviors relating to asthma treatment were not only maintaining their improvement, but continued to show slight improvement. Hyperkinetic misbehaviors, however, had regressed to their pre-therapy rate. At least part of this can be attributed to the anxieties and excitement of his impending return home (p. 181).

Mansdorf (1976) wrote another case study of the use of operant conditioning to help a young woman be able to ride in cars, despite fear of them – it would seem to be a shaping for intensity of exposure to the fearsome object:

> A hierarchy of 19 items was constructed which dealt with H.D.‘s fear of riding in cars to a workshop. The procedure was a modified version of Wolpe’s (1969) systematic desensitization technique. Instead of pairing hierarchy items with relaxation, the items were seen as elements of an operant shaping paradigm, where each item completed was reinforced with three tokens (Ayllon and Azrin, 1968). Therapy sessions were held once weekly. Sessions l-4, during which no attempt was made to apply any systematic behavior therapy, were general counseling sessions aimed at developing a working relationship between the therapist and H.D. In session 5, H.D. was explained the proposed general plan and told that she would receive three tokens after each successful session. A successful session was defined as one in which progress to a higher hierarchy item was made. The hierarchy was constructed without H.D.‘s participation and

> was not told to her in any detail. Rather, she was told that she would be rewarded every time she completed a task “having to do with cars”. The hierarchy of 19 total items is presented in Table 1. (p. 189) (not shown here but looks like such a shaping plan)
>
> Results of the treatment are shown in Fig. 1. While the entire hierarchy consisted of 19 items, only 9 of those items had to be completed before the attainment of the target behavior (step 2). Upon completion of step 11 (session 10), H.D. expressed a desire to enter a workshop and travel there by car. A test of this was done on the following session (session 11), with the therapist accompanying H.D. in a car to her workshop. During this ride, no overt fear reaction of any sort was noted, with H.D. speaking of her eventual outside placement. As a result of reaching target behavior, H.D. was placed immediately in a temporary workshop at her institution and on a waiting list for outside placement. The outside placement actually began some weeks later (session 12- week 17). A follow-up of 58 weeks showed H.D. still attending outside program by car and free of any apprehension which initially was manifested (p. 189-190).

The description of a successful combination of respondent and operant techniques as well as generalization of the environment (antecedent stimuli) is shown by Waranch et al (1981):

> Figure 2 presents the parent collected data, expressed as H.A.‘s cumulative compliance with requests to go to the shopping mall. He consistently refused to accompany his parents to the mall both before and immediately following treatment conducted in the clinic. However, his parents reported that, after conducting the in vivo session at the mall, H.A. rarely refused to make the trip. During the patient’s follow-up visits, his parents stated that he had made approximately 40 trips during the 6-month period following the completion of treatments (p. 361).
>
> The results of this case study provide additional evidence that a combination of in viva desensitization and shaping approach responses is an effective means of modifying longstanding phobias in mentally retarded individuals. These results support the few previous studies conducted with this behavior class and population despite the early recognition by Bijou (1966) and Zigler (1966) that avoidance behavior is especially characteristic of the mentally retarded person. The use of multiple-baseline clinic and community probes throughout treatment showed that while generalization from one training situation to another (i.e. from clinic to shopping mall) did not occur automatically, the training procedure used in the clinic was easily repeated in a community setting (p. 361).

After having seen clinical case studies of operant conditioning used to deal with fears and reading WHY these were used, one could then project, that this should also work with dogs, generally because to a certain extent, we may be dealing with some similar problems in arranging the necessary hierarchies for the graduation of exposure to the fearful thing and difficulties in arranging means and achieving the necessary relaxation between trials needed for effective systematic desensitization. The usual explanation from dog trainers is, we don't do that. It's not clear if they know relaxation's part of the process or have simply chosen not to do it. This relaxation does enter into some operant dog training therapies for fear, the idea being going from one relaxed state to another – or at least not “over threshold” (more later).

What's also interesting is the report from all of them that there were no relapses, spontaneous recovery, in other words, no “Return of Fear” after having reach a certain level of performance, at least judging by the behaviors continued after therapy. What these clinical studies also show is, that there ARE **operant** ways of dealing with emotion of fear and the associated unwanted behaviors by dealing with these unwanted behaviors – these are to be considered “proofs of concept” for this review! While operant strategies are still used in such circumstances, there are also more sophisticated methods used, which have been conceived since these cited here.

Comparison of respondent methods with operant ones for fear reduction

Next I'd like to present some studies which have examined the efficiency of either respondent or operant procedures or even compared the efficiency between the two. One of the first was prepared by William T. McReynolds, Robert H. Grizzard (1971, who compared systematic desensitization, operant treatment and attitude change treatment (cognition) in college coeds:

> The results support the superiority of behavior shaping and cognitive approaches to the reduction of sub-clinical fears. Compared to systematic desensitization and attention-placebo treatments, the operant treatment effected greater changes in fear-related motor behavior, reported attitudes toward snakes, and reported fearfulness. Attitude-change treatment proved significantly more effective in reducing unfavorable attitudes toward snakes and fear reports. Systematic desensitization, conceptualized herein as an affect-change treatment, was the least effective of the therapy approaches in reducing fear-related behaviors. Although Ss receiving desensitization evidenced reductions in subjective fear associated with imagined snake scenes (anxiety hierarchy), these affective changes did not generalize to other affective, attitudinal, or motor reactive systems (p. 266-267).

> **Systematic desensitization** (Group D) followed closely the treatment guidelines provided by Wolpe (1958, 1969), with the introduction and relaxation instructions taken from Wolpe and Lazarus (1966). The anxiety hierarchy was constructed by having 5s choose five increasingly disturbing scenes from a series of 20 snake-encounter situations similar to the items in the approach test. "Filler" scenes were provided where appropriate to give a more gradual approach to difficult scenes. Thus the anxiety hierarchies ranged from 5 to 9 scenes in length. A scene was considered to have been successfully completed if a 5 could imagine it twice for 10-12 seconds without signaling anxiety. Three 5s completed their entire hierarchy with all six 5s being able to imagine successfully their touching or picking up the snake.
>
> **Operant treatment** (Group 0) involved a snake-approach behavior-shaping procedure introduced to 5s as an attempt to eliminate fear behavior by providing the necessary encouragement and reinforcement to enable 5s to have more contact with the snake than they had thought possible. 5s were instructed to perform, one at a time, gradual snake-approach behaviors beginning with looking at the live snake from a distance, touching the cage, cracking and opening the cover, putting fingertips, hands and then wrist into the cage, touching the inside wall and floor, and ending in touching, stroking, moving, and lifting the snake. The performance of each behavior step was reinforced with such verbal praises as "good, you're doing fine" or "great, you can see that you're really making progress." Reluctant 5s were encouraged to "Just try, I think you'll find you can behave at least a little fearless," but all Ss were free to refuse to perform a behavior and terminate treatment at any time. Brief "rest breaks" were given after periods of substantial progress as further reinforcement of snake-approach behavior. All but two 5s were able to touch the snake, the other four 5s reducing their fearful avoidance to such an extent that they were able to pick up the snake (p. 265).

> The brevity of the desensitization treatment in light of more extended and favorable trials (e.g., Davison, 1965) makes conclusions hazardous. Our finding is in most open conflict with the work of Paul (1966). However, in our study the cognitive-"insight" treatment was highly specific to snake fears and highly focal in its treatment goal to reduce snake fears. Paul's insight-oriented treatment tended to be very general in nature, embracing "in- sight" as a diffuse treatment goal and apparently entailing efforts at large scale cognitive reorientation. Although the brevity of treatment is a main weakness in the current study, the present comparison of desensitization and cognitive-attitudinal approaches to fear reduction is more meaningful than that by Paul since both forms of treatment had equally focalized treatment goals, giving no advantage to a more focal treatment when very specific treatment related outcome measures are employed (p. 267).

Hamilton and Schoeder (1973) also compared desensitization with, as they called them "reinforced procedures" for fear reduction:

> In this study, reinforced practice of feared behavior proved as effective in reducing avoidance behavior as systematic desensitization when time in treatment was controlled. In addition, there is suggestive evidence from the FT data that subjective anxiety was reduced by reinforced practice procedures but not by systematic desensitization treatment. Systematic desensitization proved more effective, however, than treatments which ignored anxiety in reducing the expectation of fear as measured by the SNAQ. Thus, in the present study, reinforced practice had a greater effect on actual subjective fear in the aversive situation than systematic desensitization but it influenced anticipation of fear less.
>
> This study confirmed the clinical findings of Agras and his colleagues (Agras et al., 1968; Agras et al., 1969) that operant conditioning techniques can modify phobic behavior. It further demonstrated that non-social reinforcement is effective in doing so. The present data also suggest that the superiority of "shaping" procedures over desensitization reported by Barlow et al. (1970) may have been an artifact of the greater amount of experience "shaping" Ss had with the feared object.
>
> In general, both the present results and the evidence reported by the Agras group raise questions about the necessity of treating anxiety directly to modify phobic behavior. It appears that this symptom can be ignored without a reduction in behaviorally measured effectiveness. In addition, the finding that subjective anxiety itself can be reduced with reinforced practice only , brings into question the need for relaxation training to countercondition an incompatible response to the feared stimulus. In systematic desensitization the role of relaxation may be merely facilitative (p. 651-652).

The following study by Capaldi et al (1983) looked at and compared counter-conditioning and extinction, and ends up asking a very provocative question: "Is there a counterconditioning process?"

> In three experiments, counterconditioning was found to reduce fear less effectively than extinction. In Experiments 1 and 2, the resistance to extinction of avoidance was greater if food was given during extinction of fear to the CS than if no food was given, even when exposure to the CS and numbers of food and no food confinement trials were equated. It is suggested that these results could be attributed to contextual control of fear extinction by the food cue and/or to frustration produced by removing food for the counterconditioning group. Experiment 3 also found counterconditioning to be less effective than extinction and provided evidence that this difference occurs because of contextual control of fear extinction by the food cue. Measuring conditioned suppression of licking, in a test with no food present, less fear was shown if no food had been present during fear extinction, and greater fear was shown if no food had been present during fear conditioning. These results indicate that food is an important part of the context controlling fear and fear extinction. It is suggested that there may be no unique counterconditioning process. Rather, when counterconditioning procedures are employed, rules governing interference paradigms in general may apply. Thus, in a test for fear following counterconditioning, fear will be shown to the extent the test situation is similar to that in which fear conditioning occurred rather than that in which fear reduction occurred (p. 213).
>
> In all three experiments, groups given food during fear extinction subsequently showed greater residual fear than those not given food. This is the reverse of what would be expected on the basis of a counter- conditioning process. We have suggested that the results of Experiments 1 and 2 can be attributed in part to the aversive effects of removing food (frustration), which motivates escape responses in the test phase of the avoidance situation, and/or to the stimulus effects of food cues. For a counterconditioning group, fear acquisition is conducted with- out food, extinction is given with food, and then testing is given without food. This stimulus difference provides the basis for a discrimination when food is present, the situation and the CS are safe. When food is not present, shock is given. Experiment 3 showed that food can, indeed, function in this manner. In Experiment 3, in a test with no food, having no food present during conditioning increased the amount of fear shown, whereas having no food present during fear

> extinction decreased fear shown. Although we have suggested that the operation of frustration and stimulus change can interfere with measuring a counterconditioning process, there was no evidence at all for a counterconditioning process in these experiments. In all three experiments, extinction was more effective in reducing fear- motivated behavior than was counterconditioning. While these results can be explained in terms of stimulus change, the fact that the results were the opposite of those expected on the basis of counter- conditioning suggests that if there is a counterconditioning process, it is weak, weaker than stimulus change effects. Because the empirical evidence for a counterconditioning process is weak, it seems worthwhile to consider the theoretical basis for expecting a counterconditioning effect (p. 220).
>
> Therefore, recently a common view has been that the interference produced by counterconditioning is not between peripheral responses but between central motivational states. Counterconditioning has been viewed as just one example of a more general reciprocal inhibition of motivational states. A common assumption is that there are two central motivational states, appetitive and aversive, with a reciprocally inhibiting influence on one another (e.g., Bindra, 1974; Estes, 1969; Konorski, 1967; Millenson, 1971; Rescorla & Solomon, 1967). In a counterconditioning procedure, providing food excites the appetitive motivational system, thereby weakening the aversive motivational system. The evidence for this idea is weak at best. Lovibond and Dickinson (1982), for example, conditioned a nictitating membrane response by CS-shock pairings and then associated that CS with both shock and sucrose on different trials. The development of a jaw-movement CR did not reduce the frequency of the eye-blink response as would be expected on the basis of counterconditioning, but actually enhanced the eye-blink somewhat (p. 221).
>
> Food reduces appetitive motivation, which should thereby enhance the action of the aversive motivational system; this means there is no counterconditioning process whereby fear is weakened through providing food. This hypothesis, unsupplemented by other processes such as stimulus change, has difficulty explaining cases in which counterconditioning procedures appear to be more effective than extinction procedures in reducing fear (e.g., Wilson & Dinsmoor, 1970). The variety of results that can occur when an aversive CS is associated with an appetitive US suggests that an alternative conceptualization of counterconditioning may be desirable (p. 221).

I can't make any kind of educated guess as to how this last paragraph above is to be accepted or rejected. I don't have the background for that. I'm not sure that I could imagine, if that is a valid point, how one could respondently countercondition something if not by food. Play? I also have to admit, that I was not able to find a further similar statement. Perhaps someone can find either a confirmation of this or a rebuttal.

In any case, De Silva & Rachman (1984) did a preliminary study in which they compared what seems to be a exposure/habituation situation with an escape/avoidance situation with people with agoraphobic – unfortunately without long-term follow-up:

> Two groups of agoraphobic patients were treated with one of two forms of exposure-based therapy. In one group, the patients stayed in the target situation until their self-rated anxiety dropped by half (Endurance condition). In the other, the patients were required to leave the fear situation when their anxiety reached a high pre-set level, without waiting for anxiety to drop (Escape condition). The patients in both groups improved, in contrast to those in a Waiting-list control group. There were no significant differences between the two treated groups. The results are discussed in terms of Mowrer's theory of fear and avoidance from which it can be predicted that patients in an 'escape condition' should not improve. Contrary to a Mowrerian prediction, patients who were taught to escape when still fearful, did not show increased avoidance behaviour (p. 87).

What this indicates is, that if a subject is given the opportunity to “escape” an aversive stimulus during therapy, there is no more avoidance going on, than if this opportunity is not present in therapy. In a straight desensitization or a straight counterconditiioning, the subject has no control over her environment. She cannot leave if she feels the aversive stimulus is too high or if she wants a break. In an operant fear reduction protocol, where the subject IS allowed to leave (escape) if she has had “enough”, this study would mean, that there is no more indication that this subject dog would after therapy avoid the formerly fear eliciting stimulus than if she had not had this opportunity during a therapy. In other words escape possibilities during therapy or none - make no difference on the outcome after therapy.

They go on to quote a previous golden rule, which would then be not accurate:

> The golden rule is to try never to leave a situation until the fear is going down,” (original emphasis, Mathews, Gelder and Johnston, 198 I, p. 182) (p. 87)

and further:

> After initial assessment, the patients were randomly assigned to three groups. A. B and C (see below). Each S in Groups A and B had 8 in vivo therapy sessions, averaging I per week. They were assessed once again at the end of the therapy: due to practical difficulties, no follow-up was undertaken.
>
> Group A.. Patients were exposed to a selected fear-provoking situation, and were instructed not to leave until the anxiety dropped at least to half of peak anxiety felt. (The patients were taught to use a rating scale from 0-100 for subjectively-felt anxiety. a ‘fear thermometer’. during the first interview; they found it quite easy to use.) This was the ‘Endurance’ group.
>
> Group B. Patients were asked to approach the fear-provoking situation, but were instructed to leave when subjectively-felt anxiety reached a preset level; this was set individually, being 75”,, of the maximum anxiety the S estimated he would feel in the target situation. This was the ‘Escape/Avoidance’ group.
>
> Subjects in both groups were accompanied by the experimenter. No homework instructions were given.
>
> Group C. These patients were put on a waiting-list and were taken on for therapy later. They had assessments identical to those of Groups A and B (p. 88).
>
> The study provides preliminary data that escaping and/or avoiding while still fearful does not necessarily lead to increased fear and to increased avoidance. On the contrary, an exposure strategy incorporating systematic escaping proved to be as effective as the widely-used therapeutic strategy of ensuring exposure to fear situations until anxiety drops by a significant amount.
>
> The present, preliminary, study justifies a critical re-examination of the key component of Mowrer’s two-stage theory regarding the reinforcement of avoidance behavior by repeated escapes from a fear-evoking situation. If these findings can be confirmed in a full Investigation, the need for a more satisfactory explanation of persistent (agoraphobic) avoidance behavior will be strengthened (p. 91).

And so Rachman, Craske, Tallman & Solyom did do another study in 1986 , again with people with agoraphobic:

> In an attempt to find out whether escape behavior strengthens agoraphobic avoidance, two groups of agoraphobic patients were given 8 sessions of individually administered exposure treatment. The patients in the no-escape group were exposed progressively to selected fear-evoking situations in the standard manner. The patients in the escape group were also exposed progressively but were instructed to escape when their fear reached a preset level of 70 on a

> scale of 0–100. Both groups of patients showed significant and equivalent improvements on all measures of agoraphobia, and these changes were still evident 3 months later. The patients in the escape condition reported greater control and less fear than those in the no-escape condition. Escapes were not followed by increases in fear, or in estimates of danger, nor by decreases in estimates of control or safety. The pre-session estimates of control and safety were reduced on treatment sessions that followed a panic. It is concluded that escape behavior does not necessarily strengthen agoraphobic avoidance (p. 366).
>
> The main hypothesis--that escape behavior would not be followed by increases in agoraphobic avoidance--was confirmed. Over the full treatment program, patients in both groups showed comparable improvements. The improvements of those patients who exercised the option to escape were comparable to those of the no-escape patients. Furthermore, detailed analyses revealed that on the trial after an escape, patients re-ported less fear and weaker urges to leave. The main results replicate the findings of de Silva & Rachman (1984) (p. 380).
>
> The confirmation of the main hypothesis is consistent with the study reported by de Silva and Rachman (1984), and it begins to seem likely that instructions to escape at high, prearranged levels of anxiety do not result in increased fear or avoidance and do not impede therapeutic progress. ... Escape behavior is not necessarily followed by strengthened avoidance (p. 380-381).
>
> A striking feature of the present results, and those of the earlier experiment by de Silva and Rachman (1984), is that fear and avoidance declined significantly in both groups. These declines took place regardless of the occurrence or nonoccurrence of escape behavior. Presumably, these reductions of fear and avoidance can be accounted for by the same explanation that applies to other occurrences of fear-reduction in which exposure plays a prominent role. There still is no generally accepted explanation for such fear-reduction, but most theories are couched in terms of extinction/habituation/inhibition processes (Rachman, 1978). Whatever process of fear-reduction is involved, it certainly is strong enough to produce significant reductions in fear and avoidance--regardless of the occurrence of escaping or of endurance (p. 382).
>
> To sum up the response to the original question, the results of this clinical experiment, and the study by de Silva and Rachman (1984), show that escaping does not increase the strength of avoidance behavior; over-all, escaping was followed by reductions in avoidance and fear. The present and past results do not exclude the possibility that escaping helps to initiate avoidance behavior, perhaps in the early stages of a developing agoraphobia. However, as far as established patterns of avoidance behavior are concerned--contrary to Mowrer's two-stage theory--escaping does not lead to increments in avoidance. The most likely alternative explanation is that avoidance behavior is maintained by the achievement of safety signals (Rachman, 1984) (p. 383).

The importance then of both of these studies is that the patients allowed to escape from their fears not only were able to reduce the level of their fears to the same degree as those not allowed to, but also in being allowed to escape, reported having had more control over their environment which in itself reduced the level of fear they'd experienced. These results also did not result in any Return of Fear after a period of 3 months.

Two-factor or Two-process theory

I was unable to find any further newer studies addressing the above issue, but there is something else to consider and this is alternately called the two-factor or two-process theory. This is basically that to some extent respondent methods are influenced by operant processes, but also operant methods are influenced by respondent processes. In a study by Wagner and Cauthen (1968) having to do with treatment of fear of snakes and looking at respondent vs operant methods, they write:

> If one superimposes the operant conditioning paradigm with positive social reinforcement on the various systematic desensitization techniques, the results become more explicable. Reinforcement for successful progress through the hierarchy may be the necessary and sufficient condition for what may be an example of relatively straightforward operant conditioning. This may be no different than the shaping of any behavior within the typical Skinnerian framework (p. 226).

Now what they're describing from the method is more of a combination of the two, in which operant methods were used to approach and touch the snakes, but then switched to desensitization in handling. On the other hand, Rescorla & Solomon (1967) did write about the "two-process theory":

> The history of 2-process learning theory is described, and the logical and empirical validity of its major postulates is examined. The assumption of 2 acquisition processes requires the demonstration of an empirical interaction between 2 types of reinforcement contingencies and (a) response classes, (b) reinforcing stimulus classes, or (c) characteristics of the learned behavior itself. The mediation postulates of 2-process theory which argue that CRs are intimately involved in the control of instrumental responding are emphasized, and 2 major lines of evidence that stem uniquely from these postulates are examined : (a) the concurrent development and maintenance of instrumental responses and conditioned reflexes, and (b) the interaction be- tween separately conducted Pavlovian conditioning contingencies and instrumental training contingencies in the control of instrumental behavior. The evidence from concurrent measurement studies pro- vides, at the very best, only weak support for the mediational hypotheses of 2-process theory. In contrast, the evidence from interaction studies shows the strong mediating control of instrumental responses by Pavlovian conditioning procedures, and demonstrates the surprising power of Pavlovian concepts in predicting the outcomes of many kinds of interaction experiments (p. 151).

> We have argued that the basic operational distinction between response- and stimulus-contingent reinforcement may interact with various other variables in such a way as to justify the claim that two independent processes are acting (p. 163).

> We can conclude that, following one strategy suggested by two-process theory, Pavlovian conditioning procedures can readily be used to control instrumental responding. Furthermore, it might very well turn out that instrumental responding is as sensitive, or perhaps even more sensitive, a measure of the effects of Pavlovian conditioning procedures than are the traditionally measured conditioned visceral or motor reflexes themselves. If this should turn out to be true, it would constitute a major heuristic, albeit somewhat ironic, contribution of two-process learning theory.
>
> Finally, we point to the success achieved in controlling instrumental responding by means of a wide variety of Pavlovian procedures, contrasted with the failure to establish definitive relationships between CRs (as mediators) and concurrent instrumental responses. Such success gives support to the version of two- process theory postulating that the concomitance we do observe between CRs and instrumental responding is mediated by a common central state, and the changes in that state are subject to the laws of Pavlovian conditioning (p. 178).

Graziano, DeGiovanni and Garcia (1979) did a review of treatments of childrens' fears and came to some interesting findings. They covered many different methods of treatment, but I will only quote

the important parts of interest to us including two-factor theory, leaving out cognitive treatments, for example.:

> Behavioral literature on childhood fears, including conceptual models, normative research, and fear-reduction studies is reviewed. The main conclusions are as follows: (a) The information value of nearly 60 years of normative studies is meager, and their continuation is of doubtful value; (b) most research has been limited to laboratory studies of mildly to moderately fearful children, and few data exist on severe fears studied in the child's natural environment or on the clinical prevalence of fear; (c) cognitive and developmental factors have been largely ignored; (d) modeling is the most frequently used and reliably effective fear-reduction strategy; (e) a cognitive, verbal-mediation approach is promising, but is not yet sufficiently researched; (f) there is little evidence that systematic desensitization or contingency management strategies are effective. Implications for large-scale fear reduction and prevention are discussed. The need for research that recognizes the complex paradigms of children's fears is suggested)p. 804).

> Behavioral approaches consist of three paradigms, each of which assumes that fears and phobias are learned: respondent conditioning, operant conditioning, and the two-factor theory of learning. The respondent model postulates that anxiety is the central aspect of phobic behavior and that any neutral stimulus present at the time a fear response occurs may become a conditioned stimulus. In future presentations it will elicit the associated fear response. The conditioning occurs if the original conditioning situation was of high intensity or was repeated a number of times. The conditioned fear is presumably maintained in the absence of reinforcement because the avoidance behavior precludes the requirements for extinction, that is, the repeated confrontation of the actually benign conditioned stimulus without its pairing with the unconditioned stimulus. A decrease in avoidance behavior can be achieved by re- placing the conditioned anxiety response with a response antagonistic to anxiety (e.g., re- laxation) that inhibits or weakens the anxiety. Wolpe's (1974) systematic desensitization is best known of the therapy procedures based on this reciprocal inhibition paradigm. The major problems with the respondent model are that it does not explain why some neutral stimuli seem more likely than others to become conditioned fear stimuli, and, although it attempts to, it does not adequately explain the maintenance of phobic behavior in the absence of reinforcement.
>
> Operant models hold that reinforcement rather than anxiety, primarily social reinforcement such as parental attention, is the central aspect of phobic behavior. Children are presumably taught to be afraid by parents and other significant persons who selectively, albeit unintentionally, attend to and reward fearful behavior. Therapy procedures based on operant models attempt to reduce children's phobic behavior by changing the reward structure in their immediate environments, that is, by teaching parents to ignore phobic behavior and to differentially reward alternative non-fearful approach behaviors. The major problem with the operant paradigm is its failure to adequately account for and therapeutically treat the subjective feelings of intense fear or anxiety and their accompanying thoughts that are often experienced by phobic children (p. 806).

> The two-factor theory incorporates both respondent and operant learning concepts. First proposed by Mowrer (1939), it postulates that phobias originate according to the respondent conditioning paradigm and are maintained according to the operant conditioning model. After initial respondent conditioning of fear or anxiety with the conditioned stimulus, anxiety reduction associated with avoidance of the noxious stimulus becomes a positive reward or reinforcer, since anxiety or fear is unpleasant. Thus, anxiety reduction becomes the reinforcement for avoiding the noxious stimulus created by respondent conditioning. Problems with the two-factor theory have been extensively discussed over the last several years (Bandura, 1969; Herrnstein, 1969; Rachman, 1976, 1977, 1978). In brief, (a) the theory assumes that phobias are mediated through the autonomic nervous system, although studies by Solomon and Turner (1962) and Bandura (1969) suggest that behavior is regulated in large part by the central nervous system; (b) it does not explain why people fail to acquire fears in what are theoretically fear-evoking situations (e.g., air raids); (c) it fails to explain the "choice of symptom" issue of why some stimuli are more likely than others to become fear signals, that is, the distribution of human fears

> is not consistent with the equipotentiality premise of the theory; (d) it accepts the faulty assumption that all fears are acquired directly through classical conditioning, although it has been shown that operant conditioning and observational learning can also indirectly produce fearful behavior; (e) it fails to explain why active avoidance behavior is so persistent and does not extinguish in the absence of further reconditioning or trauma experiences to maintain the anxiety associated with the conditioned fear stimulus. In response to some of these problems, Herrnstein (1969) has reported recent experiments showing that the conditioned stimulus may function as a discriminative stimulus for the avoidance response rather than as a noxious stimulus whose removal is inherently reinforcing, as two-factor theory requires (p. 807).
>
> Previous reviews suggest that the prognosis for improvement is good with most types of treatment, provided that school refusal has not yet become a chronic pattern. Behavior therapy procedures for school phobia generally are based on both classical and operant conditioning models. Thus, school phobia is seen as avoidance behavior motivated by high anxiety and maintained by reinforcement for not attending school. Four major respondent based procedures have been reported: (a) systematic desensitization (Chapel, 1967; Lazarus, 1960; Lazarus & Abramovitz, 1962; P. M. Miller, 1972); (b) in vivo desensitization (Garvey & Hegrenes, 1966; P. M. Miller, 1972; Olsen & Coleman, 1967; Tahmisian & McReynolds, 1971); (c) flooding (Kennedy, 1965); and (d) implosion (Smith & Sharpe, 1970). Three major operant conditioning procedures were reported: (a) home-based contingency management (Ayllon, Smith, & Rogers, 1970; Cooper, 1973; Edlund, 1971; Hersen, 1970; Kennedy, 1965; Vaal, 1973); (b) school-based contingency management (Brown, Copeland, & Hall, 1974; Hersen, 1970; Rhines, 1973; Weinberger, Leventhal, & Beckman, 1973), and (c) behavioral shaping in the clinic (Hersen, 1970; Patterson, 1965). Most case studies report procedures based on combinations of the classical and operant models, with emphasis on one. The choice of which theoretical model and therapeutic procedure to use seems determined by whether the most pressing therapeutic need is to reduce the child's anxiety with a desensitizing procedure or to avoid reinforcing his or her escape behavior. Follow-up data (ranging from 4 weeks to 2 years) were reported for the majority of cases, although the data were limited either to assessment of continued school attendance or to attendance data plus a verbal report from parents and teachers on the child's social or emotional functioning. Actual behavioral assessment of improvement after return to school was not reported for any case. Whether such follow-up data should be collected is apparently still debatable. Hersen (1971) has criticized behaviorally oriented therapists for not being more rigorous in obtaining data regarding academic, social, and emotional adjustment. However, Ayllon et al. (1970) stated that "school attendance is a legitimate if not the only relevant treatment objective for school phobia" (p. 135) (p. 815).

It would seem that this particular study shows that there were major problems in most therapies being offered at the time. This is the only paper I've found that states that **desensitization is ineffective.** (emphasis L.C.) All others have said how effective it is, but many have pointed to the ROF issue. I'm not sure if the authors meant initially and then subject to ROF, or if it's initially ineffective. What is also of interest is the time frame for the inclusion of two-factor theory, even if they are not convinced that it works as described. We see however, that 20 years later, others have another opinion. This I suppose is not uncommon, as more research is done, more questions answered and even more posed.

Rehfeldt and Hayes (1998) wrote a review in which they not only re-outline what the differences are between respondent and operant processes, but also show how they work together. They are not only saying that they work together, but rather that they are dependent upon each other:

> The distinction between operant and respondent behavior classes has received considerable attention throughout the history of behavior analysis. Some have contended that because operant and respondent processes share a number of similarities, the distinction should be dropped. Others, for lack of a better theoretical alternative, have supported the continued distinction. It is suggested that the failure of behavior analysts to recognize the ever-present role of respondent relations in operant conditioning experiments may be impeding the formulation of

an effective explanation for stimulus equivalence, which has been investigated primarily as an operant phenomenon. Conceptual issues historically relevant to the operant-respondent distinction are discussed, and equivalence researchers are urged to consider the involvement of both classes of behavior in their analyses (p. 187).

In the present paper, we suggest that our reluctance to do away with the distinction between operant and respondent learning may be impeding our ability to arrive at an explanation for stimulus equivalence. Equivalence has been investigated primarily as an operant problem, with few exceptions (L. Hayes, 1992; Leader et al., 1996). Given that an agreed-upon explanation for equivalence has not been forthcoming, we argue for a close examination of how respondent processes might be involved in the phenomenon (p. 188).

Conceptualizing Operant-Respondent Interactions

Although the mediation of the operant by the respondent has not been unequivocally demonstrated, it is nonetheless difficult to argue that instances of operant behavior do not also include instances of respondent behavior. This has been concealed, in part, by Skinner's elimination of the discriminative stimulus as a necessary condition for response emission. Just when operant behavior occurs in the absence of stimuli remains to be addressed in Skinner's analysis, however. Some have contended that it is impossible for behavior to occur separate from the stimulating environment; the organism is always behaving and the environment is always present (Donahoe, 1991; L. Hayes, 1994). The organism may thus interact with both eliciting and occasioning stimuli in a single setting, as well as conditional stimuli which actualize the discriminative functions of other stimuli, and contextual stimuli which actualize relations between conditional and discriminative stimuli. Thus, the environment is never absent, nor is it ever static; an organism's interaction with the environment may consist of a multitude of stimulus functions which continually stimulate responding, which in turn stimulates the functions of environmental stimuli. Given this, one might ask whether the distinction between operant and respondent behavior on the basis of the role of prior stimuli-given that behavior never occurs in the absence of stimuli-is worthwhile (p. 192-193).

Recent evidence suggests that operant contingencies may not be all that are in effect in stimulus equivalence performance. The "respondent-type" training procedure employed by Leader et al. (1996) convincingly demonstrates that a history of differential reinforcement is not necessary for equivalence classes to form. In lieu of matching-to-sample training, subjects were presented with a series of nonsense syllable pairs. No response was required during this training; subjects were simply required to attend to the presentation of the stimuli. Then, subjects were tested for symmetry and equivalence relations in the typical matching-to- sample test format. All subjects demonstrated the establishment of both relations, accuracy being highest when training had consisted of longer between-pair delays relative to within-pair delays. The authors, comparing their high success rate to that of studies using matching-to sample training, claim that the respondent-type training procedure may actually be a more efficient means of establishing equivalence relations (Leader et al., 1996). From these results, it can be concluded that a history of explicitly reinforced conditional discriminations is not necessary for successful equivalence testing; the temporal contiguity between stimuli may be sufficient (p. 199).

To summarize, an organism's interaction with the environment does not occur on a trial-by-trial basis; rather, behavior is a continually evolving stream involving relations between stimuli and stimuli and stimuli and responses, which, from the perspective of the organism that is always interacting with its stimulating environment, may include more than that which is explicitly arranged by the experimenter. In addition, measurements of behavioral events are only measurements, they are not the events themselves. Consistent with these ideas, it seems that operant and respondent contingencies are simultaneously in effect in the establishment of equivalence classes. Performances that have been characterized as derived, emergent, and untrained may simply be the outcomes of interactions between operant and respondent

processes. Moreover, respondent conditioning may be a special case of stimulus equivalence, one in which the transfer of eliciting functions can be observed, in one direction only. We do not mean to imply that stimulus equivalence should no longer be examined as an operant phenomenon, but we are suggesting that an operant analysis alone may not be able to account for stimulus equivalence, and for possibly a variety of other forms of complex behavior. Rescorla (1988) elucidates the important and often overlooked role of respondent conditioning in behavior that is typically considered by behavior analysts to be under operant control; stimulus equivalence is just one example of such an area of research (p. 203).

Alan Baron & Michael Perone (2000) also wrote a paper describing how they see two-factor theory, which is the same as above:

> Two-factor theory remains a viable account of avoidance behavior. By emphasizing the interplay of respondent and operant contingencies, two-factor theory encourages the analysis of stimuli that mediate molar consequences and incorporates control by local events as well as events that are temporally remote, improbable, or cumulative (p. 357).
>
> In his article, Dinsmoor reviews and expands the version of the two-factor theory of avoidance that he has espoused over the years (cf. Dinsmoor, 1954, 1977). He presents a convincing case for the value of including Pavlovian as well as operant mechanisms in the account and disposes of what appears to be a common misunderstanding: Acknowledgment of the role of Pavlovian contingencies should not be taken to imply that two-factor theory requires the conditioning of a fear drive or the reinforcement of avoidance behavior through the reduction of fear (p. 357-358).
>
> An appealing feature of two-factor theory is the integration of operant and respondent processes. It is easy to forget in the search for response-contingent forms of reinforcement that avoidance procedures—confining the animal in an environment in which painful stimuli are delivered—provide fertile grounds for emotional conditioning. As Hineline (1986) has noted, ‘‘one cannot arrange a procedure for operant conditioning without stimulus-stimulus relationships being embedded therein, and thus the possibility of concomitant Pavlovian conditioning’’ (p. 63). Perhaps that is one reason why clinicians have been so accepting of Mowrer’s version of two-factor theory (Stampfl, 1987). Their phobic clients come to them with two sorts of complaints: on the one hand, descriptions of the problems that their avoidance behavior has created for them (interference with their job and social life), but on the other, reports of disturbing symptoms that accompany avoidance variously reported as fear, anxiety, and panic) (p. 360) .
>
> The empirical neglect of avoidance is unfortunate because the research seems especially well suited to advancing our understanding of intractable problems of societal importance. Human behavior is not well controlled by outcomes that are temporally remote, improbable, or cumulative in nature, and this is the source of much of the problematic, self-defeating behavior that can be observed outside the laboratory. Such behavior is endemic, not only on the level of the individual (familiar examples are overeating, alcohol abuse, and compulsive gambling), but also on the broad societal level (as may be seen in environmental pollution, overpopulation, and depletion of natural resources).
>
> In avoidance we have a laboratory model for the study of behavior that appears to have adapted to its long-term consequences. The fact is, animals do avoid shock under a wide range of conditions. The question is, how do they do it? Two-factor theory, with its integration of respondent and operant contingencies, prompts a search for stimuli that mediate control by molar outcomes. In addition to clarifying important theoretical issues, such research will help us to understand the operation of mediational stimuli and exploit them in the solution of human ills (p. 360-361).

Dr. Bruce Abbott (see Bibliography) prepared some class notes on “two-factor” or “two-process” theory that I'd like to quote here:

> **Mowrer's Solution to the Problem of Avoidance**
>
> Soon after the problem of avoidance surfaced, Hobart Mowrer proposed a solution that identified an immediate reinforcer for the avoidance response. According to Mowrer, during the early conditioning trials in the shuttlebox, subjects received many pairings between the tone and shock. These pairings constitute a typical classical conditioning procedure, with the tone serving as the initially neutral stimulus and the shock as an unconditioned stimulus arousing the emotional response of fear. After a few pairings of tone and shock, the previously neutral stimulus became a conditioned stimulus (CS), capable of producing a strong conditioned fear response.
>
> Now that the tone produced strong fear, any response that terminated this conditioned stimulus would be strengthened through the process of negative reinforcement. What response terminates the CS? Shuttling to the other compartment! Thus, according to Mowrer's account, the subjects shuttle during the tone and before the shock begins, not to avoid the shock, but rather to escape the fear-inducing tone CS.
>
> Note that Mowrer's account appeals to two processes or "factors." First, there is classical conditioning of fear to the tone, transforming it into an elicitor of strong fear. Second, there is instrumental conditioning of shuttling through negative reinforcement, the termination of the aversive and fear-evoking CS. Because it appeals to both classical and instrumental conditioning, the account is today known as Mowrer's two- factor theory of avoidance.

While looking for something else, I ran into the following study by Brodigan & Trapold (1974), In it they wrote in the discussion:

> First, why did conditioned suppression fail to be reacquired by rats that recovered from suppression to CSI /CSI CS2 and then experienced CSI alone paired with the US for several sessions? Although this question cannot be answered satisfactorily at the present time, **a version of the operant counterconditioning hypothesis might be considered**; during trials in which fear robbery develops, food-reinforced barpress responses gradually come to supplant fear responses elicited by CSI . That is, rats learn to barpress to obtain food in the presence of gradually decreasing amounts of fear elicited by CSI . Following this experience, rats might be capable of continued barpressing in more fearful situations. The animals might, for example, continue to barpress even while CSI reacquired its former levels of fear following the removal of CS2 (p. 91).

So why is this interesting? Because operant conditioning has to do with reinforcing desired behavior while counterconditioning has to do with building new associations to the older stimuli but without reinforcers or punishers. Moreover, in the AABP Glossary (see above), there is no mention of “operant counterconditioning”.

Besides the ethical questions involved with two-factor/two-process methods, **I believe here the important thing to note, is the overall portrayal that respondent and operant conditioning do actually work together in this two-factor or two-process theory. A kind of proof of concept.** But, we're not there yet, as the above has to do with -R and avoidance/escape and we all know how awful that is. We really need to find a positive reinforcement method. Shaping as an operant strategy did very well as shown above (see page 13). Shaping has also been called **Differential reinforcement of successive approximations to a terminal behavior** and had no ROF.

I was however intrigued by

1) the seemingly paradoxical term “operant counterconditioning” and

2) a statement on the email I'd received about counterconditioning being better done from within an operant protocol. And that DRO and systematic desensitization procedure are almost indistinguishable.

Differential Reinforcement: implications and use

So I decided to look up DRO and DRA in Google Scholar and came across more studies which DID actually mention "**operant counterconditioning**". The first piece I came across was very promising but disappointing. It was a Master's thesis in which Savage (2010) compared the treatment and results of 2 aggressive dogs. She used counterconditioning (?? - with clicker/reinforcers as well as punishers such as "uh-uh", body blocks etc.) and then Differential Reinforcement of Alternative Behaviors. Both dogs' behaviors improved during the experiment, but followups showed both had regressed to baseline behaviors. The next studies I found were in no particular order, so I'll just present them chronologically.

Luiselli, & Slocum (1983) wrote about the treatment of a child using a DRO procedure:

> The present study examined the use of a DRO procedure (differential reinforcement of other behavior) to manage multiple forms of aggression exhibited by a develop- mentally handicapped child. The effects of treatment were contrasted to baseline conditions in an ABAB reversal design. Treatment consisted of reinforcing the child with social and consumable rewards for not responding aggressively during timed intervals. Results indicated that DRO was successful in reducing the frequencies of targeted aggressions. The positive effects of intervention were maintained at a 34-week follow-up assessment at which time a substantial increase in the interval for reinforcement had been achieved (p. 343).
>
> Despite the advantages of a reinforcement approach to aggression, there are few clinical examples of its application to developmentally disabled persons (cf. Carr, Newsom and Binkoff, 1980; Luiselli and Reisman, 1980) and more empirical demonstrations are warranted. In the present study, a reinforcement technique in the form of a DRO procedure (differential reinforcement of other behavior) was applied to manage the aggression of a young handicapped child. Procedures in this case were implemented under naturalistic conditions, were directed at multiple aggressive responses, and were evaluated using a controlled single case design including long-range follow-up assessment (p. 343-344).
>
> To implement the DRO procedure, the teaching staff carried a small interval timer with them. The timer was set at a prescribed duration as indicated on the daily data collection sheet. When the pre-set duration had expired (indicated by the bell on the timer sounding), one staff member praised Mary lavishly, stating, "Good girl, you kept hands to yourself and mouth dry." Then a small piece of favorite edible was presented to her. After Mary consumed the edible, the timer was re-set and instructional activities continued. Any occurrence of target behaviors resulted in the timer being re-set to the specified duration, thus postponing delivery of reinforcement. For an aggressive act and spitting, the timer was re-set approximately 15 set following the termination of either response. For an aggressive tantrum, the timer was re-set approximately 5 min following termination of the immobilization period. This extended time was utilized to insure that Mary was sufficiently non-agitated to be integrated into ongoing activities.
>
> The interval for reinforcement was set at 7 min. This duration remained constant throughout the course of the study and was increased gradually by the teaching staff when the formal evaluation had concluded (described below) (p. 345).
>
> Results are presented in Fig. 1 which shows the frequency of target behaviors recorded daily. For Aggressive Act, an average of 10.0 responses occurred each day during baseline. With the DRO treatment program in effect, responding was reduced to an average of 4 occurrences daily. During the reversal to baseline condition, the behavior increased to an average rate of 10.5 responses. Finally, with the reinstatement of DRO, responding decreased to a daily average of 1 (p. 345).
>
> Data collected at the 34-week follow-up indicated that target behaviors were occurring at low levels. Average occurrences per day were 0.80 for Aggressive Act, 0 for Spitting, and 0.20 for

> Aggressive Tantrum. The interval for reinforcement at this time had also been increased to 24 min. Thus, at follow-up, the combined daily frequency for all three aggressive behaviors was 1 response in contrast to a pre-intervention rate of 17.8 responses per day (p. 346)

I'm including a study by Stephen E. Glasscock & William E. MacLean Jr. (1990) here although the study itself is about desensitization as well as shaping. I'll go into the "why" in a minute. The subject is a 6 year old girl afraid of dogs. She was subject to "contact desensitization, shaping and family counseling". She was afraid of going outside by herself to play and of familiar and unfamiliar dogs when they approached her - even the family Labrador. This after she'd been attakced and injured by a dog.

Desensitization consisted of a 10 step graduated approach plan. Shaping was used with praise as reinforcer, whereby the next higher intensity (criteria) was installed.

The results did not spontaneously generalize to the front and side yards, when repeated there similar results were reached. After 9 months the family reported she could play unsupervised in untrained areas.

> The shaping program was employed to reteach this child to play outside for periods comparable to her indoor play. This component of the intervention required a gradual increase in the amount of time this child remained outside without avoiding or es- caping to a safe environment and thus obviated the suspected reinforcement of avoidance behaviors. The shaping program was an integral part of maintaining continued exposure and contact to the feared stimulus within the child's home ecology. Marks (1975) argued that the most beneficial factor involved in the use of operant procedures may in fact be the continued exposure to the phobic stimulus, which aids in the extinction of fear responses. Functionally, the child learns that no aversive consequences occur, and that fear responses are not reinforced.

So again, operant protocols are effective in fear reduction. Something to note as opposed to the previous studies:

> Kim's program was continued for 9 months after the treatment phase in order to provide for the maintenance of her outdoor play behavior. Kinn's maintenance program involved daily independent play outside for a minimum of 20 min. She was expected to play away from the house and to tolerate either her dog or a neighborhood dog that was introduced into her immediate play environment. Any avoidance resulted in a family member returning her to the environment to complete her play time. Successful completion of each play time outside was socially reinforced by Kim's family. Kim was quite successful with her treatment maintenance program. This program was discontinued after 9 months due to Kim's success.

➔ She was not allowed to stop playing if under 20 minutes exposure time. If she cut this time short, she was brought back to the aversive situation and only to stop when this time was completed. This constitutes clearly a negative reinforcement contingency.

➔ She was however "socially reinforced by her family" for completing the minimum play time outside.

It also must be noted that the initial contact desensitization was described thusly:

> The desensitization was carried out according to the procedures described by Ritter (1969). In this instance, the therapist served as a live social model who demonstrated the relevant behaviors and used physical contact with the child to shape her performance. Given the therapist's presence, encouragement, prompting, and praise for the child's progress, Kim progressed readily through each step of the desensitization hierarchy in each of her outside play environments.

I think that we all can agree, that this is not strictly respondent desensitization, but rather actually a graduated operant procedure of increasing intensities of the aversive stimulus, reinforced by positive social interaction. This itself was a shaping for distance and with a DRO contingency, the "other" being first playing outside and later petting the dog. Then the shaping was continued with the added raised criteria of time.

Vollmer and Iwata (1992) also take a look at differential reinforcement. They make, for me, a rather remarkable statement in the Abstract, by implying, that **we should concern ourselves more with the behavior than with the underlying emotions.** They also say that this is a "**function-based approach**", which sounds rather familiar, with some today claiming that such an approach necessitates negative reinforcement by definition. This may however not be the case.

> For many years, differential reinforcement has been a prevalent and preferred treatment procedure for the reduction of behavior disorders. This paper reviews the procedural variations of differential reinforcement and discusses their functional properties. It is proposed that such procedures are more likely to be successful if behavioral function is a primary consideration in prescribing treatments; furthermore, limited success noted in previous research may be due to the arbitrary relationship that often exists between reinforcers and target behaviors when behavioral function is unknown. Despite the promise of a function-based approach to differential reinforcement, several current limitations exist in the identification and manipulation of relevant variables. Thus, further research is required to elucidate the relationship between aberrant behaviors and the variables responsible for maintaining them; otherwise, successful treatment cannot be expected. Several areas for future research are discussed conceptually as extensions of current and past experimentation (p. 393).

> By isolating specific reinforcers during an assessment condition, it becomes possible to remove or withhold those reinforcers during treatment while using them to develop alternative behaviors. However, despite the promise of a functional analysis approach, other technological limitations exist because it is sometimes difficult to identify and/or control the variables responsible for maintaining behavior. Furthermore, the development of effective reinforcement procedures can be highly complex when behavior is multiply controlled. It is likely that, as analyses of behavioral function become more common and more precise, the effectiveness of reinforcement-based procedures will increase (p. 394).

> Differential reinforcement of other behavior (DRO) involves the delivery of reinforcement contingent on the nonoccurrence of the target response for a pre-specified interval of time. DRO schedules were first examined extensively by Reynolds (1961); the experimental component relevant to this discussion was a condition in which pigeons received food for not engaging in a key-peck for a fixed period of time. Key-pecking eventually fell to low levels in that DRO condition (p. 395).
>
> Reynolds' terminology (DRO) has not been adopted consistently because it implies non-behavior as a response class, which is inconsistent with Skinner's (1953) definition of reinforcement based on increased probability of future responses (p. 395).

> In addition to fixed and variable schedules, there is a third variation of interval length known as the "escalating schedule." In escalating DRO schedules, a successfully completed interval not only results in the delivery of a reinforcer, but also increases the length of the ensuing interval (Pickering & Topping, 1974). Such a schedule was successfully applied by Repp and Slack (1977) who reduced various undesirable responses in developmentally disabled students by increasing the time requirements after each successfully completed interval. A similar schedule was used by Topping, Graves, and Moss (1975) to eliminate problematic responding in school children. Presumably, the function of this type of schedule is to arrange reinforcement on a more intermittent basis, making the schedule more naturalistic (p. 396).

Differential Reinforcement of Alternative Behavior

The differential reinforcement of alternative behavior (DRA) is a procedure that involves systematically reinforcing behavior that is topographically dissimilar to, but not necessarily physically incompatible with, the behavior targeted for reduction. Because of its potential utility in developing functional responses while simultaneously eliminating undesirable behavior, several authors have favored the application of DRA procedures over DRO (e.g., Lavigna & Donnellan, 1986). This is probably an important concern because many people with developmental disabilities have limited means of obtaining positive reinforcement due to physiological and environmental variables (Bijou, 1966; Ferster, 1961), and because DRO schedules do not explicitly arrange for reinforcement of any specific new responses. In fact, Poling and Ryan (1982) found that only 2 of 19 reviewed DRO studies reported data on behavior other than the response targeted for reduction (398-399).

One variation of DRA procedures has been termed, “differential reinforcement of incompatible behavior” (DRI), which involves reinforcing behaviors that compete with the target response due to topographical dissimilarity (Young & Wincze, 1974). For example, if a target response is head slapping, DRI might involve reinforcing occurrences of sitting on the hands. Although some research on DRI has been encouraging (e.g., Tarpley & Schroeder, 1979), it is unclear whether the distinction between DRA and DRI is a useful one. The reasons for this possibility will be discussed later when the relevance of behavioral and treatment function to DRA procedures is considered (p. 400).

Differential reinforcement, one of the most commonly used procedures for reducing undesirable behavior, has met with moderate success. It appears that the effectiveness of differential reinforcement depends on either identifying reinforcers that are more potent than those maintaining the target behavior or identifying the maintaining variables so that procedures involving extinction and reinforcement of alternative behavior can be utilized. Furthermore, even if such maintaining variables are identified, technological difficulties remain in arranging procedures that allow them to be systematically manipulated - such as in automatically produced stimulation (p.411).

So here we have descriptions of how DRO, DRA and DRI differ from each other and how they are applied and described as moderately successful. I'm not sure that DRO in “our” world is applied according the the schedules outlined, though. There is a lot more information on other applications of differential reinforcement, but for the scope of this paper, I've limited my discussion to these 3. Continuing...

Vollmer, Roane and Ringdahl (1999) show here, that if not quite perfectly done, the implementation of treatment using differential reinforcement behaviors (DRA) must not necessarily have a totally adverse effect upon the outcome of these treatments.

In prior research, differential reinforcement of alternative behavior (DRA) has been implemented at optimal treatment values: Problem behavior is never reinforced, and alternative behavior is always reinforced. However, in application, DRA is unlikely to be conducted optimally. In this study, following a functional analysis phase and a differential reinforcement at full implementation phase, we challenged initially positive treatment effects for 3 participants by implementing DRA at less than optimal parameters. For example, some occurrences of problem behavior were reinforced, and some occurrences of alternative behavior were not reinforced. Results suggested that when exposed to DRA at full implementation, participants showed a bias toward appropriate behavior in subsequent conditions during which “mistakes” (treatment challenges) were intentionally introduced. In addition, the negative effects of treatment challenges were quickly reversible, in comparison to the positive effects of DRA, which were not quickly reversible in the face of treatment challenges (p. 9).

Since the emergence of functional analysis methods (e.g., Iwata, Dorsey, Slifer, Bauman, &

> Richman, 1982/1994), studies on differential reinforcement based on functional analyses have proliferated (e.g., Mazaleski, Iwata, Vollmer, Zarcone, & Smith, 1993; Steege, Wacker, Berg, Cigrand, & Cooper, 1990). In a DRA arrangement, appropriate and inappropriate behavior can be conceptualized as concurrent operants (Fisher & Mazur, 1997). If the reinforcement schedule favors the alternative behavior, as it should in a well-designed DRA, responding should be allocated toward appropriate behavior and away from problem behavior. Typically, in evaluations of differential reinforcement, problem behavior is never rein- forced (i.e., is placed on extinction), and de- fined instances of the alternative behavior are reinforced to maximize the probability of response allocation in favor of the alternative behavior (p. 10).

> Differential Reinforcement: Partial Implementation
>
> During subsequent differential reinforcement phases, treatments were intentionally eroded to mimic various extremes of treatment implementation integrity failures. In other words, not all appropriate behaviors were reinforced and some aberrant behaviors were reinforced. Various partial implementation schedules were evaluated, although no attempt was made to evaluate exhaustively all possible schedule arrangements or to control for all possible order effects. The purpose of this analysis was to evaluate the effects of treatment challenges after an initially effective treatment had been implemented (p. 14-15).

> Taken as a whole, results suggested the following. At full implementation of differential reinforcement, inappropriate behavior was virtually replaced by appropriate behavior; lower levels of implementation eventually reduced treatment efficacy if the schedule of reinforcement favored inappropriate behavior, but there was a general bias toward appropriate behavior (p. 20).

> From a clinical standpoint, the finding that partial treatment implementation can be effective following exposure to full implementation suggests the possibility of intentionally thinning implementation levels prior to generalizing a treatment plan into environments in which treatment fidelity will be difficult to maintain. Given that treatment effects may eventually erode, booster sessions might be conducted periodically to reestablish 100% implementation (p. 21).

> Thus, the methods from the current study could be adapted and expanded upon to evaluate numerous differential reinforcement parameters. The evaluation in the current study suggests that differential reinforcement, at least when based on a prior functional analysis, can be quite resistant to treatment failures. However, the effects of recent and long-term reinforcement histories were not controlled in this study. In addition, other factors that influence response allocation (e.g., reinforcer delay, quality, duration, and magnitude) remain untested within a similar procedural format (p. 21-22).

This leaves a question open for me. It doesn't seem to be that horrible, if we miss an opportunity to reinforce the offered good behavior. Since they only described the acquisition phase of the new behavior and the extinction of the old AND showed that after extinction it's important to train that new behavior, what about later since no long-term study was done for resiliency? So if this were a fear based behavior, when can one begin to thin the reinforcement schedule and to what degree without risking the “Return of Fear” with it's previously associated behavior?

Shabani and Fisher (2006) write about a proximity experiment they used, using DRO. Their method, which is basically 90% of the the text of the study was to define a place where the subject needed to stay. If he stayed there, he was reinforced, if he moved away, they stopped the test for a kind of time out. This proved effective.

> Stimulus fading in the form of gradually increased exposure to a fear-evoking stimulus, often combined with differential reinforcement, has been used to treat phobias in children who are otherwise normal and in children with autism. In this investigation, we applied stimulus fading plus differential reinforcement with an adolescent with autism and diabetes whose needle

phobia had prevented medical monitoring of his blood glucose levels for over 2 years. Results showed that the treatment was successful in obtaining daily blood samples for measuring glucose levels (p. 449).

During the initial baseline, Oliver pulled his hand and arm away every time the experimenter attempted to draw blood with the lancet. During F1 (M 5 97%) and F2 (M 5 100%), Oliver consistently kept his hand and arm within the outline drawn on the poster- board. During a return to baseline, he continued to pull his hand and arm back when the experimenter attempted to draw blood. Fading was reintroduced, and Oliver continued to keep his hand and arm within the posterboard outline. During the probe session (i.e., Session 21), Oliver consistently pulled his hand back. During the remainder of Step F5 and Steps F6 and F7, he continued to keep his hand still. In Step F8, a blood draw attempt was initiated following Session 30 (the first session in Step 8 in which Oliver was successful for 100% of the trials). In Step F9, his hand remained still during 100% of trials for all sessions except the first one (Session 32, 80%). In addition, all attempts to draw blood in Step F9 were successful, and glucose levels were obtained. In addition, one of the blood draws took place in another setting (the nurse's station), and the trials and blood draws conducted at a 2-month follow-up visit were all successful. Oliver's mother also reported that she was able to draw blood and measure glucose levels on a daily basis with no problems (p 451-452).

Results of this investigation suggest that procedures used to treat phobias in individuals with less severe disabilities may also be effective with individuals diagnosed with autism and mental retardation. In addition, the results are important in addressing the challenges of assessing and treating phobias in individuals who do not speak. One challenge consists of developing a fear hierarchy, which is generally developed using a self-report measure. The current results suggest that fear hierarchies for individuals who do not speak can be based on specific overt escape behaviors (p. 452).

Anecdotally, he showed clear signs of distress (whimpering, crying, and other negative vocalizations) during the baseline phases and at the start of treatment, and these responses were absent at the end of the treatment and during follow-up.

The last paper I'll mention is by Petscher, Rey, Bailey (2009). This paper, along with the paper above by Vollmer and Iwata (1992) should be obtained and read by anyone doing fear reduction processes with clients' dogs. It would be counterproductive to copy the most important quotes into this paper, inasmuch as it would include most of this review itself. I will however quote the abstract and parts of the conclusion.

Differential reinforcement of alternative behavior (DRA) is one of the most common behavior analytic interventions used to decrease unwanted behavior. We reviewed the DRA literature from the past 30 years to identify the aspects that are thoroughly researched and those that would benefit from further emphasis. We found and coded 116 empirical studies that used DRA, later grouping them into categories that met APA Division 12 Task Force criteria. We found that DRA has been successful at reducing behaviors on a continuum from relatively minor problems like prelinguistic communication to life-threatening failure to thrive. DRA with and without extinction is well established for treating destructive behavior of those with developmental disabilities, and to combat food refusal (p. 409).

3. Conclusion

DRA has been successful at reducing severe behaviors for many participants, while replacing the unwanted response with appropriate behaviors that can enhance participants' quality of life. It rarely produced unwanted side effects but instead commonly resulted in positive collateral changes. However, in those cases where the alternative behavior rate would be difficult to maintain, schedule thinning has been shown to successfully reduce the rate to acceptable levels.

DRA has been performed in many studies and is a well-established treatment for food refusal and participants with developmental disabilities exhibiting destructive behavior. The published studies with participants otherwise diagnosed, such as Schizophrenia (Wilder, Masuda,

O’Connor, & Baham, 2001) were generally positive and warrant further study.

Many studies were identified whose data were promising but the designs did not meet Task Force criteria. Most often the design did not replicate findings appropriately, such as the ABCD design in Derby et al. (1997), and multiple baselines whose lengths were not varied enough in Durand (1993). While they were commonly successful at answering their research questions, the data could not be used for the current review. Over time this can cause problems because the interventions cannot demonstrate empirical validity accepted by mainstream psychology. If behavior analysts desire to increase such awareness, we suggest that designs are tweaked when possible to meet the Task Force criteria.

Future research should focus on DRA with NCR for the treatment of destructive behavior of persons with developmental disabilities in order for it to be considered a well-established treatment. Additional directions of interest would include participants without diagnoses or with those other than developmental disabilities, and with behaviors other than destruction and food refusal. Other promising areas include the studies manipulating response effort and reinforcer magnitude (p. 420).

My comments about these studies

1) Neither emotions nor behaviors based upon emotions need to be ONLY handled with respondent methods. Operant methods work very well. There are reasons why respondent methods, as explained, may fail – with a rate of up to 50% according to the studies, especially if one does the successful D&CC to the end goal. And operant methods such as shaping seem to be in many cases more effective according to the studies presented. These operant methods do not need to be based in negative reinforcement, although these were shown also to be effective and not to have increased fear, neither short nor long term, neither with the treated fear nor did they cause other fears. This is not to say that I am endorsing this. Most of the described operant methods were using positive reinforcement, especially differential reinforcement of one type or another or in combinations.

2) Yes, working with emotional behavioral problems with operant methods without specific respondent methods is possible. **The so-called two-factor or two-process theory serve as "proof of concept".** Granted, this theory has fallen in and out of favor, but as I wrote above we all know of it thanks to Bob Bailey and his "Pavlov is always on your shoulder" as well as "...and Skinner is sitting on the other." (attrib. Unknown). At the same time however, while the two-factor/two process based methods rely on negative reinforcement, these **DRx models, which are "function-based" approaches, use positive reinforcement and do support the assertion, that if we address the behavior, the emotions do get addressed as a by-product**. See also James O'Heare[5]

3) The statement, that there is no resurgence of fear having to do with D&CC, if you do it correctly is, based upon the information given here, simply not true. In spades. And ROF is in one form or another a continuous red flag to be aware of. There is a history of research having to do with this going back to 1951 and continuing to the present time. And even Pavlov wrote of it in 1927. This research has looked into it for desensitization, for counterconditioning, for operant conditioning and others that don't necessarily concern us. The findings have been pretty conclusive:

1) desensitization has a high rate of "Return of Fear" (ROF) up to 50% depending upon time span between sessions or after therapy, environmental changes (training facility vs street), intensity or even type of trigger (breed, size as a possible example)

2) extinction also has the same high rate, but for another reason. The behavior has stopped, but has been neither forgotten, nor unlearned. It's still there. This is never explained in "our" terms, but I'd theorize, that unless that unwanted behavior that was extinguished was replaced with a stronger competing behavior, the chances are higher, that the unwanted extinguished behavior will come back. That applies to respondent as well as operant extinction. Operant methods did not suffer from this ROF.

4) Behavior cannot be unlearned – this was demonstrated with the subject of extinction and the problems that arise when used. And the literature is very clear. It does not get unlearned, nor forgotten. Paired with the right stimulus at the right intensity in the right environment, it can very well come back. This is not only true for extinction, but also for D&CC. We have no way of knowing, that the emotion has actually been changed. The literature is not conclusive. The behavior elicited by the emotion might change, but the previous behavior was not unlearned. So even what we say we do, behavior modification, is a misnomer. We substitute behaviors, we train alternative behaviors, we train other responses to existing triggers. But we do not necessarily modify existing behavior.

5 "Dog Aggression Workbook...", James O'Heare, DogPsych Publishing, Ottawa, Canada, © 2007, specifically pages 68-72

Threshold: a search on Google Scholar for "fear threshold" or "aversion threshold" brought almost nothing. Several studies having to do with robotics and otherwise studies in which threshold is mentioned, but not defined.

I did however find these:

http://www.alleydog.com/glossary/definition.php?term=Threshold :

> "Threshold: The term "threshold" has a couple of different meanings, but let's start with the simple one. A threshold is the minimum amount of stimulation needed to start a neural impulse (you know, the electrical impulses that travel throughout your body carrying important information)."

http://psychologydictionary.org/threshold/

> noun. 1. with regard to psychophysics, the strength or greatness of a stimulant which will generate its detection half of the time. 2. the minimum severity of a stimulant which is required to elicit a reaction.

Lewis O. Harvey (2012):

> One goal of classical psychophysical methods was the determination of a sensory threshold. Types of sensory thresholds include detection, discrimination, recognition, and identification. What is a threshold? The concept of threshold actually has two meanings: One empirical and one theoretical. Empirically speaking, a sensory threshold is the stimulus level that will allow the observer to perform a task (detection, discrimination, recognition, or identification) at some criterion level of performance (75% or 84% correct, for example). Theoretically speaking, a sensory threshold is a property of the detection model's sensory process (p. 3).

> Finally, it must be recognized that no psychophysical method is perfect. Observers may make decisions in irrational ways or may try to fake a loss of sensory capacity. Care must be taken, regardless of the psychophysical method used to measure capacity, to detect such malingering (Linschoten & Harvey, 2004). But a properly administered, conceptually rigorous psychophysical procedure will insure the maximum predictive validity of the measured sensory capacity (p. 14).

(http://en.wikipedia.org/wiki/Sensory_threshold) chosen because the dog will become aware of a stimulus through sensory organs.

> **Absolute threshold**: the lowest level at which a stimulus can be detected.
>
> **Recognition threshold**: the level at which a stimulus can not only be detected but also recognized.
>
> **Differential threshold**: the level at which an increase in a detected stimulus can be perceived.
>
> **Terminal threshold**: the level beyond which a stimulus is no longer detected.

As well as the following, which I find especially appropriate for our work with dogs:

> "Exploring The Psychology of Interest" P.J. Silva:
>
> "In sum, Tomkins (1962, 1991) answered the first of our questions – What makes something interesting? By asserting that an increase in information increases interest, provided that the rate isn't high enough to induce fear or startle. **He posited a stimulation threshold between interest and fear**: increase in the rate of informational input arouse interest, until the rate exceeds the **interest-fear threshold**[6]." (**emphasis** mine)

More to Tomkins here: http://en.wikipedia.org/wiki/Silvan_Tomkins

> "Tomkins' Affect theory[edit]
>
> Main article: Affect theory
>
> Disagreements among theorists persist today over Tomkins' firm insistence in his Affect theory that there were nine and only nine affects, biologically based. The basic six are: interest-

6 Actually, Tomkins described an interest-excitement and fear-terror relationship. Silva seems to have changed this to **interest-fear** http://en.wikipedia.org/wiki/Affect_theory

> excitement, enjoyment-joy, surprise-startle, distress-anguish, anger-rage, and fear-terror. Tomkins always described the first six, and one that "evolved later" (shame-humiliation) in pairs. In these pairs, the first pair part names the mild manifestation and the second the more intense. [2] The final two affects described by Tomkins are "dissmell" and disgust. Tomkins argued that these nine affects are quite discrete (whereas emotions are complex and muddled), that they manifest a shared biological heritage with what is called emotion in animals, and that they differ from Freudian drives in lacking an object."

So, although **Affect Theory** is an aspect of human psychoanalysis, after speaking to several trainers, Ethologists, Biologists, etc. I was steered away from Tomkins' Affect Theory and toward Jaak Panksepp and his **Affect Systems** in Neuroscience as being more useful in considering the application of SEEKING-FEAR paradigm as a help in determining the overall "emotional" state of dogs during fear reduction trials, inasmuch as Panksepp himself makes the concrete connection to animals and how they might perceive this.[7] More about Panksepp and Affective Neuroscience in here:

http://www.auf-den-hund-gekommen.net/-/paper2.html

What I couldn't find was anything approaching what we trainers have made up. There was no threshold of "when the dog displays unwanted behavior" or "when the dog doesn't take food" or "when the dog is stressed"

There is no mention of the word "aversive". And if one can look at a trigger in desensitization and be relaxed, then it is not aversive. That, after all, is the point of systematic desensitization – that the distance to this trigger becomes smaller and the subject goes from one relaxed state to another, not from one "under threshold state to another"..

In terms of either a systematic desensitization or a counterconditioning, there was no definition given or even usage of the word "threshold" in any of the literature (reviews, studies) I examined. It would appear, this too is a made up word by dog trainers and our definition of threshold is as moveable as our own position on why we do something or do not do something. If I personally use this term, I will use the term "**interest-fear**" threshold and is as good (or bad or inappropriate or … but not made up) as any other.

I would also like to note that what we call D&CC or desensitization and counterconditioning is NOT how it is applied either in labs or clinical trials, neither in the studies quoted from here, nor in any I've run into. This doesn't mean that D&CC in this form is not used outside of the dog-training world, it just means, that I have not seen it so.

In these quoted trials, desensitization meant the hierarchically graded exposure from one relaxed state to another towards the trigger. It means a reduction of the intensity of the emotions and therefore the associated behaviors.

Counterconditioning is only the re-association of one good emotion to a trigger, replacing the previous bad emotion, thus effecting the behaviors. There have been suggestions however, that counterconditioning itself is operantly influenced. The subject wants the food, notices that a certain demeanor assures him of getting it, so does not change it and does receive the food. Also see Capaldi (1983). We, as dog trainers, use a method that combines the two, which may leave out the relaxation by assuming, that as long as the dog is still eating (see threshold above) as we approach the trigger or the trigger approaches the dog, that the emotions associated and thus the behaviors will change for the better. In modern cultural terms, it's a "mash-up" of the two. I have been unable to find this combination in any quoted or not quoted studies as a clinically applied method.

7 Those knowing about Baroque music will also recognize, that composers and poets of the day did not write about emotion in music, the wrote about **affections**. So are we speaking here about the same thing, restored to consideration after several hundred years? See "Doctrine of the Affections" http://www2.nau.edu/tas3/baroqueideal.html

And finally my pet peeve concerning fear and fear reduction:

A fearful dog has a reason for being so. Such an animal - and we too are animals - has basically 4 choices when afraid:

1) fight 2) flight 3) freeze or 4) fiddle

Dogs which choose choice number 1 have done so, not because they want to fight, but rather because in most cases, this behavior has PREVENTED a fight, by scaring the object of their fear away so they DON'T have to fight. Fighting should be the solution of last resort. It consumes the most energy and brings the highest risk. Dogs with aggressive behavior generally do NOT want to fight, but they want to convince the other that they do. That works until it doesn't and then they risk getting seriously injured or worse.

We can ask ourselves the biological question: Is it really wise to want to, or even expect to eradicate the fear in the case of other dogs or people? What if they do come face to face with a dog who does want to harm them? Don't they need to recognize this and have viable options open? Same with people? Is the decision not to engage, but rather to retreat an undesirable action? Does the dog not retain the biological “right” to protect his/her own life if escape is not possible?

When we help a dog with fear issues, we have 2 objectives:

1) deal with the displayed unwanted behaviors and the fear evoking them

2) provide the animal with an alternate behavioral pattern (DRA) by showing that that behavior works just as well (if not better) when confronted with fear through a eliciting stimulus.

When a dog learns to first assess a potentially dangerous situation and then work out an alternative solution to charging and fighting, he/she can continue to live, which makes more biological sense. And science shows, that with the new strategy and the reinforcement through success, the fear is also further reduced. This, because one knows now how one does NOT have to interact,

IF ONE DOESN'T WANT TO.

Here is a film of dogs in such situations. We can see that in some cases, especially towards the end, their fear was justified. It's also a great study of dog body language. And the film also shows the last 3 of the four "F"s.

http://youtu.be/S7znI_Kpzbs

Where dogs were “forced” to ignore their initial decision not to interact and instead try to pass the cat at close quarters, some were swatted. What implications does this have for future interactions with the cat and for the emotional relationship to the human who did the forcing?

“Pavlov and Skinner are sitting on your shoulders in every psychotherapy/pharmacotherapy session you administer.”

—J.P.MCCULLOUGH,JR. (Peter Neudeck, Dieter Schoepf, 2010)

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Bibliography

Bruce Abbott, *Instrumental Conditioning Avoidance*, Indiana University-Purdue University Fort Wayne, 2012, http://users.ipfw.edu/abbott/314/Avoidance.html

David H. Barlow, W. Stewart Agras & Harold Leitenberg, John P. Wincze, *AN EXPERIMENTAL ANALYSIS OF THE EFFECTIVENESS OF "SHAPING" IN REDUCING MALADAPTIVE AVOIDANCE BEHAVIOR: AN ANALOGUE STUDY"*, (1969), Behaviour Research and Therapy, Volume 8, Issue 2, May 1970, Pages 165–173

Alan Baron, Michael Perone, *EXPLAINING AVOIDANCE: TWO FACTORS ARE STILL BETTER THAN ONE*, (2000), JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR. 2001 May; 75(3): 357–378.

Mark J. Boschen, David L. Neumann, Allison M. Waters, *Relapse of Successfully Treated Anxiety and Fear: Theoretical Issues and Recommendations for Clinical Practice*, (2008), Aust N Z J Psychiatry. 2009 Feb;43(2):89-100.

Mark E. Bouton, Dale Swartzentruber, *Sources of relapse after extinction in Pavlovian and instrumental learning*, (1991), Clinical Psychology Review, Volume 11, Issue 2, 1991, Pages 123–140

Mark E. Bouton, *Spontaneous recovery in cross-motivational transfer (counterconditioning)*, (1992), Animal Learning & Behavior, 1992, 20 (4), 313-321

Mark E. Bouton, *Context and behavioral processes in extinction*, (2004), Learn Mem. 2004 Sep-Oct;11(5):485-94.

Mark E. Bouton, Neil E. Winterbauer, Travis P. Todd, *Relapse processes after the extinction of instrumental learning: Renewal, resurgence, and reacquisition*, (2012), Behav Processes. 2012 May;90(1):130-41.

Elisabeth D. Capaldi, Donna M. Viveiros, David H. Cambell, *Food as a contextual cue in counterconditioning experiments: Is there a counterconditioning process?*, (1983), Animal Learning & Behavior, 1983,11 (2),213-222

Peter J de Jong, Ingrid Vorage, Marcel A. van den Hout, *Counterconditioning in the treatment of spider phobia, effects on disgust, fear and valence*, (2000), Behaviour Research and Therapy, Volume 38, Issue 11, 1 November 2000, Pages 1055–1069

P. de Silva, Stanley Rachman, *Does escape behaviour strengthen agoraphobic avoidance? A preliminary study*, (1984), Behav Res Ther. 1984;22(1):87-91.

Stephen E. Glasscock & William E. MacLean Jr., *Use of Contact Desensitization and Shaping in the Treatment of Dog Phobia and Generalized Fear of the Outdoors* (1990) Journal of Clinical Psychology, 1990, Vol 19, No. 2 Pages 169-172

Anthony M. Graziano, Ina Sue DeGiovanni, Kathleen A, Garcia, *Behavioral Treatment of Children's Fears: A Review*, (1979), Psychological Bulletin, Vol 86(4), Jul 1979, 804-830.

Lisa M. Gunther, James C. Denniston, Ralph R. Miller, *Conducting exposure treatment in multiple contexts can prevent relapse*, (1998), Behaviour Research and Therapy, Volume 36, Issue 1, 31 January 1998, Pages 75–9

Maryanne Hamilton, Harold E. Schoeder, *A comparison of systematic desensitization and reinforced practice procedures in fear reduction*, (1973), Behaviour Research and Therapy, Volume 11, Issue 4, November 1973, Pages 649–652

Lewis O. Harvey, *Detection Theory: Sensory and Decision Processes* (2102) Department of Psychology, University of Colorado at Boulder

Dirk Hermans, Michelle G. Craske, Susan Mineka, Peter F. Lovibond, *Extinction in human fear conditioning.*, (2006), Biol Psychiatry. 2006 Aug 15;60(4):361-8. Epub 2006 Feb 28.

Hackjin Kim, Shinsuke Shimojo, John P. Doherty, *Is Avoiding an Aversive Outcome Rewarding? Neural Substrates of Avoidance Learning in the Human Brain*, (2006),

Neville J. King, Thomas H. Ollendick, Eleonora Gullone, Robert A. Cummins, Angela josephs, *FEARS AND PHOBIAS IN CHILDREN AND ADOLESCENTS WITH INTELLECTUAL DISABILITIES: ASSESSMENT AND INTERVENTION STRATEGIES*, (1990), Journal of Intellectual and Developmental Disability 1990, Vol. 16, No. 2 , Pages 97-108

Neville J. King, Peter Muris, Thomas H. Ollendick, *Childhood Fears and Phobias: Assessment and Treatment*, (2005), Child and Adolescent Mental Health, Volume 10, Issue 2, pages 50–56, May 2005

Mario A. Laborda, Ralph R. Miller, *Preventing Return of Fear in an Animal Model of Anxiety: Additive Effects of Massive Extinction and Extinction in Multiple Contexts*, (2013). SciVerse ScienceDirect, Behavior therap 44 (2013) 249-261

James K. Luiselli, Paul R. Slocum, *Management of multiple aggressive behaviors by differential reinforcement*, (1983) Journal of Behavior Therapy and Experimental Psychiatry Journal of Behavior Therapy and Experimental Psychiatry, Vol.14, Issue 4, December 1983, Pages 343–347

Irwin J. Mansdorf, *Eliminating fear in a mentally retarded adult by behavioral hierarchies and operant techniques*, (1976), Journal of Behavior Therapy and Experimental Psychiatry, Volume 7, Issue 2, June 1976, Pages 189–190

Johnny L. Matson, *Behavioral learning theory and current applications to severe behavior problems in persons with mental retardation*, (1991), Clinical Psychology Review, Volume 11, Issue 2, 1991, Pages 175–183

Sue McKniley, Robert J. Young, *The efficacy of the model-rival method when compared with operant conditioning for training domestic dogs to perform a retrieval-selection task*, (2003),

William T. McReynolds, Robert H. Grizzard, *A comparison of three fear reduction procedure*s., (1971), Psychotherapy: Theory, Research & Practice, Vol 8(4), 1971, 264-268

Donald R. Miklich, *Operant conditioning procedures with systematic desensitization in a hyperkinetic asthmatic boy*, (1973), Journal of Behavior Therapy and Experimental Psychiatry, Volume 4, Issue 2, June 1973, Pages 177–182

KM Myers, M Davis, *Mechanisms of fear extinctio*n, (2007), Molecular Psychiatry (2007) 12, 120–150.

Jayson L. Mystkowski, Michelle G. Craske, Aileen M. Echiverri, *Treatment Context and Return of Fear in Spider Phobia*, (2002), Behavior Therapy 33, 399-416, 2002

Jayson L. Mystkowski, Michelle G. Craske, Aileen M. Eschiverri, Jennifer S. Labus, *Mental Reinstatement of Context and Return of Fear in Spider-Fearful Participants*, (2006), Behavior Therapy, Volume 37, Issue 1, March 2006, Pages 49–60

Peter Neudeck, Dieter Schoepf, Learning Theory Aspects of the Interpersonal Discrimination Exercise in Cognitive Behavioral Analysis System of Psychotherapy, The Behavior Therapist, March 2010, p. 58 - 62

Niwako Ogata, DVM, Takefumi Kikusui, DVM, PhD, Zukari Takeuchi, DVM, PhD, Yuji Mori, DVM, PhD, *Objective measurement of fear-associated learning in dogs*, (2006), Laboratory of Veterinary Ethology, The University of Tokyo, Tokyo, Japan.

James O'Heare, *Dog Aggression Workbook...*, DogPsych Publishing, Ottawa, Canada, © 2007, specifically pages 68-72

Jane S. Orihel, David Fraser, *A note on the effectiveness of behavioural rehabilitation for reducing inter-dog aggression in shelter dogs*, (2008),

Joseph J. Pear, Gloria D. Eldridge, *THE OPERANT-RESPONDENT DISTINCTION: FUTURE DIRECTIONS, (1984), JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR* 19841 429 453-467 NUMBER 3 (NOVEMBER)

Erin S. Petscher, Catalina Rey, Jon S. Bailey, A review of empirical support for differential reinforcement of alternative behavior, (2009), Research in Developmental Disabilities - Volume 30, Issue 3, May–June 2009, Pages 409–425

Stanley Rachman, *The Return of Fear*, (1979), Behav. Res. & therapy, Vol 17, pp 164-165

Stanley Rachman, Michelle Craske, K. Tallman, C. Solyom, *Does Escape Behavior Strengthen Agoraphobic Avoidance?* A Replication, (1986), Behavior Therapy, Volume 17, Issue 4, September 1986, Pages 366–384

Paul M. Salkovskis*, David M. Clark, Ann Hackmann, Adrian Wells, Michael G. Gelde, *An experimental investigation of the role of safety-seeking behaviours in the maintenance of panic disorder with agoraphobia*, (1999) Behavior research and Therapy 37, (1999) 559-574

Stanley Rachman, *The Return of Fear: Review and Prospect*, (1989), Clinical Psychology Review, Volume 9, Issue 2, 1989, Pages 147–168

Stanley Rachman, M. Whittal, *The Effect of an Aversive Event on the Return of Fear*, (1989), Behav Res Ther. 1989;27(5):513-20.

R A Reescorla, R L Solomon, *Two-process learning theory: relationships between Pavlovian conditioning and instrumental training*, (1967), PSYCHOLOGICAL REVIEW Vol. 74, No. 3 May1967

Ruth Anne Rehfeldt, Linda J. Hayes, *THE OPERANT-RESPONDENT DISTINCTION REVISITED: TOWARD AN UNDERSTANDING OF STIMULUS EQUIVALENCE*, (1998), The Psychological Record: Vol. 48: Iss. 2, Article 1.

Stefanie Riemer, Corsin Müller, Zsofia Viranyi, Ludwig Huber, Friederike Range, *Choice of conflict resolution strategy is linked to sociability in dog puppies*, (2013), Applied Animal Behaviour Science, Volume 149, Issues 1–4, December 2013, Pages 36–44

Beverly I. Rodriguez, Michelle G. Craske, Susan Mineka, Deanna Hladek, *Context-specificity of relapse: effects of therapist and environmental context on return of fear,* (1998), Behaviour Research and Therapy, Volume 37, Issue 9, September 1999, Pages 845–862

Melissa K Rowe, Michelle G. Craske, *Effects of varied-stimulus exposure training on fear reduction and return of fear*, (1997), Behavior Research & Therapy 36 (1998) 719-734

Paul M. Salkovskis*, David M. Clark, Ann Hackmann, Adrian Wells, Michael G. Gelde, *An experimental investigation of the role of safety-seeking behaviours in the maintenance of panic disorder with agoraphobia*, (1999) Behavior research and Therapy 37, (1999) 559-574

Kristyn E. Savage, *A Comparison of Classical Counterconditioning and Differential Reinforcement of Alternative Behavior on Aggressive Behavior in Dogs*, (2010), Master's thesis, Applied Behavioral SciencesGraduate Faculty of the University of Kansas

Debora Vansteenwegen, Bram Vervliet, Carlos Iberico, Frank Baeyens, Omer Van den Bergh, Dirk Hermans, *The repeated confrontation with videotapes of spiders in multiple contexts attenuates renewal of fear in spider-anxious student*s, (2007), Behav Res Ther. 2007 Jun;45(6):1169-79.

Bram Vervliet, Frank Baeyens, Omer Van den Bergh, Dirk Hermans, *Extinction, generalization, and return of fear: A critical review of renewal research in humans*, (2011), Biological Psychology, Volume 92, Issue 1, January 2013, Pages 51–58

Bram Vervliet, Michell G. Graske, Dirk Hermans, *Fear Extinction and Relapse: State of the Art*, (2013), Annu. Rev. Clin. Psychol. 2013. 9:215–48

Timothy R. Vollmer, Brian A. Iwata, *Differential reinforcement as treatment for behavior disorders: Procedural and functional variations*, (1992), Research in Developmental Disabilities - Volume 13, Issue 4, July–August 1992, Pages 393–417

Timothy R. Vollmer, Henry S. Roane, Joel E. Ringdahl, *EVALUATING TREATMENT CHALLENGES WITH DIFFERENTIAL REINFORCEMENT OF ALTERNATIVE BEHAVIOR*, (1999), JOURNAL OF APPLIED BEHAVIOR ANALYSIS 1999, 32, 9–23 NUMBER 1 (SPRING 1999)

Mervyn K. Wagner, Nelson R. Cauthen, *A comparison of reciprocal inhibition and operant conditioning in the systematic desensitization of a fear of snakes*, (Cauthen), Behaviour Research and Therapy, Volume 6, Issue 2, May 1968, Pages 225–22

H. Richard Waranch, Brian A. Iwata, Marianne K. Wohl, F. Don Nidiffer, *Treatment of a retarded adult's mannequin phobia through in vivo desensitization and shaping approach responses*, (1981), Journal of Behavior Therapy and Experimental Psychiatry, Volume 12, Issue 4, December 1981, Pages 359–362